PEER 24 - Performance Assessment Reviews (1990 to 1992)

1991 PERFORMANCE ASSESSMENT REPORT

INTERA REVIEW

INTERA

M

Review of the

1991 WIPP Assessment

(SAND91-0893)

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INTERA

Our Ref: DAG/jab/SAND-5

27th March 1992

M Marietta Esq.
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USA.

Dear Mel,

Review of the 1991 WIPP Assessment (SAND91-0893)

Enclosed, please find two copies of our review of SAND91-0893, Volumes 1-3. I have read and commented in detail on all the Volumes, giving particular attention to the Chapters highlighted in the contract. In addition, I have incorporated numerous comments from several other Intera staff, notably Peter Robinson (who has re-done some of the CCDFs!). All of the contributors are listed on the cover of the review document.

I have structured the review document into three parts: an introduction, a general overview of comments and, the largest part, a section-by-section technical commentary on the three Volumes. Many suggestions for improvements are made in the enclosed document but I should like to draw your attention in particular to two areas where further input may be especially helpful.

One of the weakest points of the documentation is, in fact, that a concise, readable overview of what you have done is lacking. It may be that you have not had sufficient time to do this or, perhaps, that the support staff working on the project are all too close to the work or too narrow in their outlook. It would be extremely interesting if an independent party were to try to explain to you what you had done, based on the information contained in SAND91-0893 (and possibly some supporting documentation). The draft 1992 assessment documentation could also be taken as the starting point for this overview but it would, in any case, need to be updated regularly. Such an overview would let you know whether what you had intended to present was in fact properly presented. It would also help in the process of completing the concise readable overview that is so desperately needed.

A second area of weakness concerns the scenario development work which reflects a parochial and out-of-date understanding of work in this field. The scenario development is, however, one of the foundations of the performance assessment and you have rightly recognised this by the attention given to it in Volume 1 of SAND91-0893. I feel we should be in a good position to contribute to further work in this field, based on our experience of scenario development work at international level and in support of such exercises for clients in Sweden, Switzerland and the United Kingdom.

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Cont/...

INTERA

Many other recommendations have been put forward in the enclosed document and I await your views on the work we have done.

Finally, I should emphasise here that we never received Volumes 4 and 5 and have, therefore, spent the resources available for the review on Volumes 1 - 3. Please let me know what to do about Volumes 4 and 5.

Also, should you see the need for me to come to Albuquerque in the near future (i.e., around the time of the Las Vegas meeting), please let me know. Possible discussion topics include:

- comments on SAND91-0893, Volumes 1 - 3 and possible follow-up,
- proposed PSACOIN Level 2 exercise and development of draft case specification for the Task Group meeting in May. I should also propose to meet with Jim Sinclair and Peter Robinson (and Brian Thompson, if available) for one day in advance of leaving the UK so that I could represent their views,
- discussion of draft input to NEA Human Intrusion Working Group report. I might also be able to have this completed in advance of leaving the UK,
- other work and associated funding for FY93.

I trust that the enclosed documentation meets your expectations and I should welcome any feedback.

Best regards.

Yours sincerely,

Dr Daniel A Galson
Geosciences Group
Environmental Division

Encl. as stated

IG-3091-1
Version 1

Review of the

1991 WIPP Assessment

(SAND91-0893)

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March 1992

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D.A. Galson et al.

Summary

This document presents a review of Volumes 1-3 of the Sandia report, "Preliminary Comparison with 40 CFR Part 191, Subpart B for the Waste Isolation Pilot Plant, December 1991" (SAND91-0893). The review has been based on the published version of the 1991 WIPP performance assessment, and the results are intended to be used in preparing the 1992 and future-year assessments. As requested by Sandia, this review has considered technical questions pertaining to the performance assessment methodology and its application and results, as well as issues of organization, presentation and flow of information between the various sections, chapters and volumes.

We consider the 1991 assessment documentation a valuable and impressive contribution to the performance assessment literature. The WIPP Program is in the process of coming to terms with most of the contentious issues surrounding the deep geological disposal of long-lived wastes. Many parts of the report are of extremely high quality; we have, however, focused our comments on those parts of the report where we considered improvements could be made.

Our major technical concerns are in the general area of treatment of uncertainty in the assessment, including in particular treatment of scenario uncertainty, data and parameter uncertainty, and model uncertainty. We have also suggested a possible modification to the methodology for generating CCDFs for human intrusion events, and have noted that the treatment of human intrusion, as a particular class of scenarios, is imbalanced in places. The document contains a general overview of our concerns, as well as a detailed section-by-section technical commentary.

With regard to presentation and organization of the report, there is substantial room for improvement, reflecting the difficulty in both completing an assessment and clearly and succinctly documenting it within a twelve-month period. In particular, the report is excessively long, and very much in need of a good summary of the order of 100 pages (or less). More attention needs to be paid to the relevance of the information presented

to the final assessment results. and to the potential audience for the report. Excessive use of mathematics is made throughout the report, and figures are too few in number. are poorly explained or are too complex. In addition, relatively minor errors are rife, particularly in Volume 3.

Review of the 1991 WIPP Assessment (SAND91-0893)

1 Introduction

This document presents a review of the Sandia report, "Preliminary Comparison with 40 CFR Part 191, Subpart B for the Waste Isolation Pilot Plant, December 1991" (SAND91-0893). The Sandia report consists of five volumes:

Volume 1: Methodology and Results

Volume 2: Probability and Consequence Modeling

Volume 3: Reference Data

Volume 4: Uncertainty and Sensitivity Analyses

Volume 5: Dose Calculations

At the time of the review, however, only the first three volumes were available.

The review has been undertaken by Intera Information Technologies for the WIPP Performance Assessment Division at Sandia National Laboratories. The review has been led and coordinated by D.A. Galson. Contributions were also made by P.C Robinson, T.J. McEwen, K.J.Worgan, M.J. Apted and M.D. Impey.

The WIPP Performance Assessment Division will revise and refine the "Preliminary Comparison..." documentation on an annual basis, up until the time that an application for an operating license for the WIPP is made. The "Final Comparison..." is currently scheduled to be completed in about five year's time.

This is the first review of the WIPP assessment documentation carried out by Intera. The review has been based on the published version of the 1991 assessment (Volumes 1-3). It is foreseen that further reviews will be conducted of draft assessment documents on an annual basis, normally in a September timeframe (although future versions of Volume 3 may be available sooner).

Given that the 1991 assessment of compliance with the EPA Standard has already been published, and the undertaking by Sandia to update the assessment on an annual basis, the purpose of this review of the 1991 documentation has been to provide the WIPP Program a set of comments that can be used in preparing the 1992 and later-year assessments. A further objective was to familiarise Intera staff with the WIPP Performance Assessment Program, in order that future requests by Sandia for reviews of assessment documents in draft form could be responded to efficiently and rapidly. It is expected that future reviews will need to be completed in less than one month. This independent review of the Sandia performance assessment work should ultimately provide Sandia and Department of Energy management greater confidence that the assessment work is clearly presented and represents the state-of-the-art in assessment technology at international level.

As requested by Sandia, this review has considered technical questions pertaining to the performance assessment methodology and its application and results, as well as issues of organization, presentation and flow of information between the various sections, chapters and volumes.

All of Volumes 1-3 have been considered, but in varying levels of detail. In particular, Intera was requested to give closer technical attention to Chapters 3 and 4 of Volume 1, Chapters 2, 3, 5, 6 and 7 of Volume 2, and the entirety of Volume 3. In addition, it was found essential to review Chapter 5 (Compliance-Assessment System) of Volume 1 in detail, because the material contained therein provides an important conceptual basis for much of the assessment. However, comments have been made section by section where necessary on most other chapters throughout the three volumes. Finally, somewhat more focus was placed on the near-field part of the assessment than on the far-field; this emphasis reflects the relative importance of the near-field in the WIPP assessment.

This document contains a general overview of concerns in Section 2 and a detailed technical commentary in Section 3.

2 General Overview

The Waste Isolation Pilot Plant is likely to be the world's first deep underground disposal system for long-lived radioactive wastes. Site characterization work, initiated about 15 years ago, has already been extensive, and the performance assessment activities have been underway for several years. The commitment to produce performance assessments on an annual basis prior to licensing reflects the importance given to such work by the WIPP Program. This commitment places heavy demands on the staff of the Sandia and supporting contractors, not least because each assessment requires timely input from a multitude of technical disciplines, and the coherent integration of the work and review of some 100 scientists. In addition, the WIPP Program has expressed its awareness that not only should the work be documented, but that it should be documented in a clear and accessible format, in order to involve as many interested outside observers as possible in the process leading up to licensing of the WIPP. Thus, the task at hand is substantial, and the 1991 assessment is to be viewed as only one step in the ongoing iterative process of data collection/interpretation and performance assessment.

In view of the above, we consider the 1991 assessment documentation a valuable and impressive contribution to the performance assessment literature. The WIPP Program is in the process of coming to terms with most of the contentious issues surrounding the deep geological disposal of long-lived wastes. All of the main issues of concern to such assessments have been clearly identified, including those issues for which time and resources have not yet allowed an adequate treatment.

Much of the report is of extremely high quality. We have, however, focused our comments on those parts of the report where we considered improvements could be made. That we have identified many such areas is unsurprising, given the evolving developments in performance assessment technology and supporting data bases within the WIPP Program, as well as internationally. Our general comments are summarized in this Section, based on the extensive and more detailed comments contained in Section 3.

2.1 Report Organization and Presentation

There is substantial room for improvement, reflecting the difficulty in both completing an assessment and clearly and succinctly documenting it within a twelve-month period. This timescale presents a challenge which has not been undertaken within other national programs, but which the

WIPP Program has committed itself to meet.

In particular, the report is excessively long, and is in desperate need of a good summary of the order of 100 pages (or less). Volume 1 could be developed into a proper summary volume, with much of the detail relegated to Appendices or to Volume 2, which could present the detailed technical basis for the Volume 1 summary. On the other hand, the synopses presented at the end of each chapter in Volume 1 are excellent; much of the information for preparing a good summary report has already been assembled.

Much of the report reads as if it were a "dump" of potentially relevant information available to the author. More attention should be paid to the relevance of the presented information to the final assessment results.

In preparing a summary volume, as well as the more detailed supporting volumes, care should be paid to the intended audience for the report in general, and for the different volumes in particular. It is stated in the preface that the main audience includes "interested parties", but large parts of the report would be inaccessible to the general reviewer.

Some of the material in Volume 1 is poorly organized. For example, a good overview of the site and disposal system should precede the scenario analysis. The approach to performance assessment should be placed later in the volume. Several of the later chapters could be combined. In addition, the numerous references made early in the volume about the lack of releases for the base case scenario are annoying, as this is not demonstrated until much later in the volume.

Excessive use of mathematics is made throughout the report, particularly in Volume 1 (Chapters 3-4) and Volume 2. Much of the mathematics could be relegated to supporting documentation for the assessment. Its appearance in this report serves mainly to distract the reader from the flow of the arguments, and from seeing what has really been done in the assessment.

There are generally too few figures, most of the figures that do exist are poorly explained, and many are much too complex, containing too much detailed information, that obscures the main point to be made. Chapter 4 of Volume 1 (Scenarios for Compliance Assessment) in particular is poorly illustrated.

In addition to this presentational aspect, many of the figures and tables have been carelessly compiled, and errors are rife, particularly throughout Volume 3.

Parts of the report are extremely repetitious, particularly the various discussions concerning development of CCDFs and concerning the content and interpretation of the EPA Standard. In particular, a new section which brings together the analysis and interpretation of the EPA Standard scattered throughout the report should be developed, and could then be referred to whenever needed.

2.2 Treatment of Uncertainty

With all the mathematics contained in the report, an important aspect to the treatment of uncertainty has been forgotten: the need to describe and treat uncertainty in a manner that sheds greater light on the assessment results. Great play is made of the importance of distinguishing between two types of uncertainty, stochastic and subjective. On the one hand, we do not find this distinction meaningful or helpful in the context of the WIPP assessment; on the other hand, other aspects of treating uncertainty have received insufficient attention.

For example, the potential impact of spatial variability and heterogeneity has been largely ignored (a notable exception, however, is the work on transmissivity fields in the Culebra Dolomite). The work on scenario development seems biased, and takes little account of developments in thinking since the late 1970s (outside the U.S.). In addition, the report correctly notes that the crucial issue of conceptual model uncertainty has not yet been examined.

2.3 Scenario Uncertainty

2.3.1 INITIAL IDENTIFICATION

The completeness of the initial set of events and processes compiled as input for the scenario development is unclear. Apparently, no formal site-specific process was followed. Rather, a generic list developed about fifteen years ago and reported in Cranwell et al. (1990) was used, and this was supplemented by three additional potentially disruptive events and processes known to be of concern to the WIPP. The ad hoc procedure followed does not provide confidence that the scenario development was based on a sound initial compilation of potentially disruptive events and processes. For example, in the area of human intrusion, several additional scenarios of potential concern have been identified in this review (see detailed comments). Scenario development is an area that would very much benefit by the expert panel approach used in other areas of the performance assessment.

In addition, the most up-to-date generic list for events and processes of potential importance to disposal system safety is probably that contained in the report of the NEA/PAAG Working Group on scenario development (OECD/NEA, 1992). This work is not referenced at all.

2.3.2 SCREENING

The procedure used to screen the events and processes as input to the scenario development lacks rigour, and appears to be biased in many places, as detailed in Section 3. The intention appears to be to rule out from consideration a priori as many events and processes as possible, without serious consideration as to the possible effects or likelihoods of occurrence. In particular, screening out events and processes on the basis of low consequence, without proper analysis of the potential consequences, should be avoided. This same point is also made by Cranwell et al. (1990).

On the other hand, the performance assessment work to date suggests that the site is fundamentally sound. An effort should be made to be more open-minded and imaginative in the scenario development work. Again, much needed confidence in the scenario screening could be obtained through independent geological input outside of the WIPP performance assessment group.

Although screening is an important aspect of scenario development, an equally important aspect is to explore the range of the possible. The contrast with the input provided for human intrusion (admittedly an important class of scenarios) is notable.

2.4 Parameter Uncertainty

Although we have some concerns over the details of the methodology used to obtain the reference data provided in Volume 3, our general impression of the data gathering exercise is that it is very systematic and comprehensive.

There seems to be a confusion between uncertainty and variability. Repeated measurements of a variable may give different results for a number of reasons (spatial variability, experimental error, lack of control of experimental environment, etc.), and this will lead to an uncertainty as to the true value of this parameter. However, it is only when the parameter varies stochastically that the observations can be taken as a direct indication of uncertainty. In other cases, the varying measurements require careful consideration by experts to determine a reasonable characterisation of uncertainty.

Some of the confusion in terminology may result from the more or less direct translation of reactor PRA methodology and terminology to waste disposal system performance assessment. It is not sensible to apply directly all of the reactor PRA concepts to natural geological systems. For example, large parts of a reactor can be treated as an engineered system comprised of components of more or less constant (but imprecisely known) properties. This same thinking is not easily applied to disposal systems.

In the chapters describing the treatment of individual parameters in Volume 3, it is not always clear whether or not the parameter was sampled and, in many cases, it is not clear how the parameter entered into the performance assessment. An additional point of confusion is that often the performance assessment calculations used different values for parameters than those appearing in the summary tables and boxes. In this case, we presume that the summary values were used solely for the purpose of sensitivity analyses (reported in Volume 4). The distinction should be made clearly.

In addition, the detailed discussions contained for many of the parameters in some cases shed little light on the basis for the median and range used in the assessment. More attention needs to be paid to the clarity of these discussions, and to the link between the discussions and the final summary information presented in the "boxes" and tables.

2.5 Generation of CCDFs

The approach taken for developing CCDFs is clearly explained (Volume 1, Chapter 3), but has not been adequately justified. The methodology seems in part geared toward the analysis of the scenarios arising from the scenario development work presented in Chapter 4. It is concluded there that human intrusion is the only credible scenario that may affect the integrity of the disposal system over the 10,000-year regulatory period.

This area is complex and is full of controversies, many of them philosophical. We are therefore wary of criticising the approach used. Nonetheless, it is worth raising the issues that cause concern.

In essence, the question is why such distinctly different approaches are taken to scenario uncertainties and parameter uncertainties. It is claimed that scenario uncertainties are stochastic in nature, while parameter uncertainties are subjective. In our view, almost all uncertainties are essentially subjective, with the exception being stochastic events (probably including particular human intrusion events).

It is stated that the CCDF over scenarios arises because *a number of different occurences have a real possibility of taking place*, whereas *fixed, but unknown quantities are needed in the estimation of a CCDF*. This seems to suggest that there are many possible futures, but that there is only one possible set of parameter values. We would argue that there are **many possible** futures and **many possible** parameter sets, but that there is only **one actual** future and only **one actual** set of parameter values. Both for futures and parameters the problem is that the actual cannot be distinguished from the possible, and therefore the same methodology is appropriate for both.

We agree with the views expressed by Bonano and Wahi (1990, Chapter 4), and in particular, the construction of a single CCDF including both scenario and parameter uncertainty. It seems that this is the single CCDF referred to in the EPA Standard.

In addition, in our comments on Chapter 3 of Volume 2, we present an alternative methodology for generating the CCDFs for human intrusion. This uses a simulation approach which is easy to describe and implement. Using our approach, we have been able to reproduce efficiently the results given in this chapter of the report.

2.6 Model Uncertainty

We would stress that conceptual model uncertainty may be the most important source of uncertainty in the final assessment. The WIPP Program needs to identify clearly the source of such uncertainties, and develop a position on how such uncertainties will be treated in the final assessment documentation.

2.7 Human Intrusion

The approach taken in treating human intrusion is imbalanced in places. For example, an extremely (and overly) complicated mathematical model is developed to analyse cuttings releases, whereas there is only very limited discussion of current drilling practices in the Delaware Basin. In addition, although an interesting exercise in its own right, we do not consider that the expert panel approach taken to develop models for future societal development and corresponding probabilities for future human intrusion will increase the confidence in this part of the assessment. It would be more conservative to assume that intrusion was going to occur, and to assess the potential consequences of various kinds of intrusion occurring at different times in the future and assuming present-day drilling practices.

The EPA Standard, as promulgated in 1985, implies that human intrusion should be folded into a single CCDF to demonstrate compliance with the containment requirements. The report would nonetheless benefit from a less constrained consideration of the rationale and philosphical basis behind the treatment of human intrusion. We suggest this because consequence estimates for potential future intrusion scenarios, although potentially useful in siting (and possibly designing) repositories, are unlikely to provide a useful quantitative basis for the licensing of deep geological disposal systems, no matter how much modelling is involved and how many experts are called to bear. Human intrusion seems to have taken on undue importance within the WIPP Assessment Program because of the supposed absence of releases by any other means.

2.8 Assessment Timeframe

The containment requirements of the EPA Standard are expressed in terms of a limitation on the probability of exceeding various levels of cumulative release to the accessible environment over a 10,000-year period. The assessment has therefore largely focused on this intitial 10,000-year period, and little information is given about the longer-term performance of the repository system. What information is presented does suggest that the longer-term performance has been considered, and that the site is likely to provide excellent confinement for periods much longer than 10,000 years.

One piece of information presented in Volume 3 does however confuse this picture. There it is shown that the normalized radionuclide activities within the repository, after an initial sharp decline, start to rise steeply with time after about 2000 years, and are still rising steeply at 10,000 years. This begs the question of when does the total activity level off, and at just what level? Also, regulatory timeframe notwithstanding, how does the temporal variation in activity after 10,000 years affect longer-term performance?

Clearer treatment of the post-10,000-year performance of the disposal system would be beneficial. Questions concerning this longer-term performance may well be raised during licensing.

3 Detailed Comments

3.1 Volume 1: Methodology and Results

Preface

The preface seems unnecessarily long. Much of the second half is unnecessary, or would be better off in the main text (where most of it can in any case already be found).

3.1.1 Chapter 1. INTRODUCTION

Section 1.1 40 CFR Part 191, The Standard (1985)

The EPA Standard plays a central role in the assessment documentation, and reference is made throughout the report to the provisions and requirements of the Standard. Often these references include a certain amount of what could be termed regulatory analysis or interpretation. Much of this information in later sections is extremely repetitive, and it would be a good idea to draw together all the key elements of concern to assessing compliance with the Standard in one section or chapter, and simply refer back to this in later chapters. This section could be entitled, for example, "Regulatory Analysis" (and may fit better in Chapter 2). The method of dispersed presentation adopted makes the Standard appear extremely complex, whereas most of the provisions are in fact relatively straightforward.

Section 1.2 Application of Additional Regulations to the WIPP

No comments.

Section 1.3 Organization of the Comparison

No comments per se, but should consider comments on possible reorganization of the documentation provided in comments on sections 1.1 and 1.4 of Volume 1.

Section 1.4 Description of the WIPP Project

The geological parts of Chapter 1, sections 1.4.1 to 1.4.3, are not as helpful as they could be. In addition, much of the information presented in this section is also contained in Chapter 5 and, in part, in more detail in Volume 3, section 2.1.1, where some very relevant information is presented for the first time. It would be particularly useful here if there were cross sections through the WIPP from outside the Delaware Basin in approximately north-south and east-west directions. Figure 1-5 (page 1-16), showing the generalized stratigraphy in the WIPP area, is not sufficient for this purpose, and is too simplified (although note that many other diagrams are overly complex).

A better appreciation and understanding of the scenarios described in Chapter 4 would be possible if more were known about the WIPP area before this chapter were read. This could be achieved either by presenting better geological information in Chapter 1, and/or by placing Chapter 5 before Chapter 4.

Pages 1-17/18. Figure 1-6 contains a shaded line, presumably the southern limit of the Capitan Reef, but there is no explanation in the text or in the figure of what this line is and what, if any, significance it is supposed to have.

Page 1-25, line 16. The reference to figure 1-7 is incorrect. The only relevant figure is figure 1-5, but this figure is partly diagrammatic (i.e., no bedding is shown), and it therefore cannot show the existence of an angular unconformity between the Dewey Lake Beds and the Dockum Group.

Page 1-29, line 14. Care should be taken to avoid mixing between SI units and "English" (i.e., U.S.) units, or conversions to SI should be provided throughout the text. Although conversions are frequently provided, an inconsistent approach has been taken.

3.1.2 Chapter 2. APPLICATION OF SUBPART B TO THE WIPP

See comments under section 1.1 above.

Page 2-2, line 30. We agree strongly with the emphasis placed on development of adequate and clear documentation, as one of the cornerstones to assessing compliance with the EPA Standard. We trust that our review will be of use in this respect!

Section 2.1 Containment Requirements

Page 2-6 to 2-7. It is difficult to understand parts of the discussion of release limits without reference to Table 1 in Appendix A, and a detailed understanding of the EPA Standard. It might be better to reproduce the necessary parts (or all) of the Standard in one place in the main text (e.g., Table 1 can be found later in the main text).

Page 2-7, lines 35-38. This section states that expert judgement will be used, but this is obvious, because expert judgement pervades every aspect of long-term performance assessments. More important to know is how expert judgement will be used (e.g., in some kind of formalised way in specific instances) in the performance assessment process.

Page 2-8, lines 21-23. This states that completeness in scenario development is most appropriately addressed through peer review and probability assignment. We assume that the term completeness refers to the need to ensure that all features, events, and processes, and resultant scenarios, of potential importance to disposal system safety have been identified. Scenario uncertainty is in fact best addressed through the use of a systematic approach to the identification of features, events and processes, and in the development of scenarios, and by a thorough documentation of the scenario development procedure (e.g., see OECD/NEA, 1992). Peer review will mainly check this process, but it will not ensure completeness per se. In addition, it is not clear how probability assignment addresses the issue of completeness.

Page 2-9, Table 2-1. This table and the associated text purport to outline the range of methods to be used in assessing or reducing uncertainty in the WIPP performance assessment. The table is misleading, however, because almost all the methods really serve only to assess uncertainty. Data collection - or making better or fuller use of existing data - and the use of detailed modelling to improve understanding are arguably the only ways to reduce fundamental uncertainties in the performance assessment, regardless of the source of the uncertainty. Note that the fuller use of existing data means the use of both direct and "indirect" information (e.g., the use of hydrochemical data to infer groundwater flow patterns).

Page 2-19, lines 8-10. The text here (and elsewhere in the report) states that models will be validated to the extent possible, and that expert judgement will be relied upon where validation is impossible. While this would seem correct, the approach and wording of such statements should be carefully considered (perhaps with input on the legal side) in view of existing environmental litigation in the United States. There is a legal precedent establishing the need for validation. In particular, the issue of model validation was the basis for the decision in a court case involving the State of Ohio and the Environmental Protection Agency (23 ERC 2091, Sixth Circuit, 1986). The U.S. Court of Appeals ruled that the EPA had acted arbitrarily in using the CRSTER computer code as a basis for establishing limitations on sulphur-dioxide emissions from two electrical utility plants. Specifically, the court criticised the use of a code that had not been validated using site-specific information. The court stated that "...no on-site study had been performed on the CRSTER model...no one had tested the model or cross-checked its predictions against reality at the locations of the company's power plants."

Page 2-19, lines 29-38. These statements suggest a potential misinterpretation of the EPA Standard. It is suggested that should the CCDF exceed the specified limits. then additional qualitative judgements may still lead to a "reasonable expectation" of compliance. Extensive human judgement (almost by definition qualitative) will be used in developing any CCDF, particularly insofar as human intrusion is concerned. If the (presumably best available) quantitative and qualitative judgement used to derive the CCDF do not lead to a reasonable expectation of compliance, additional qualitative judgements are unlikely to prove deciding in a decision on "reasonable expectation". In addition, it seems nonsensical to take the view that if one subjective judgement pushes a CCDF over the regulatory limit, another may be used to argue for compliance in any case. Rather. the conditions leading to a potential violation should be examined in detail. Perhaps some of the assumptions will be seen to have been overly conservative. Also. the wording on lines 35-38 is difficult to understand.

3.1.3 Chapter 3. PERFORMANCE ASSESSMENT OVERVIEW

Section 3.1 Conceptual Model for WIPP Performance Assessment

In this section the approach to generating CCDF curves is described. It describes what is done in reasonably clear terms (with only the necessary amount of mathematical formalism). However, it was not really clear why it is done this way. The methodology seems in part geared toward the analysis of the scenarios arising from the scenario development work presented in Chapter 4. It is concluded there that human intrusion is the only credible scenario that may affect the integrity of the disposal system over the 10,000-year regulatory period. If this is the case, then say so. It may also be easier to understand the approach if the details of the methodology were presented after Chapter 4.

This area is complex and is full of controversies, many of them philosophical. We are therefore wary of criticising the approach used. Nonetheless, it is worth raising the issues that cause concern.

In essence, the question is why such distinctly different approaches are taken to scenario uncertainties and parameter uncertainties. It is claimed that scenario uncertainties are stochastic in nature, while parameter uncertainties are subjective. In our view, almost all uncertainties are essentially subjective, with the exception being stochastic events (probably including particular human intrusion events).

In addition, the words "conceptual model" in the title of this section should be changed (to "approach"?) to avoid confusion with other uses of the term (i.e., as used in models of radionuclide release and transport in the performance assessment).

Page 3-2, line 13. Confusing use of the term conceptual model.

Page 3-10. Figure 3-4. There are numerous examples throughout this Chapter of illustrations of the methodology based on nuclear reactor PRA work (e.g., the paper by Breeding et al., from which five or six examples and figures are taken). It would be preferable if the methodology were illustrated by reference to results from the WIPP performance assessment.

Page 3-13, line 39. It is stated that the CCDF over scenarios arises because *a number of different occurences have a real possibility of taking place*, whereas *fixed, but unknown quantities are needed in the estimation of a CCDF*. This seems to suggest that there are many possible futures, but that there is only one possible set of parameter values. We would argue that there are **many possible** futures and **many possible** parameter sets, but that there is only **one actual** future and only **one actual** set of parameter values. Both for futures and parameters the problem is that the actual cannot be distinguished from the possible, and therefore the same methodology is appropriate for both.

We agree with the views expressed by Bonano and Wahi (1990, Chapter 4), and in particular, the construction of a single CCDF including both scenario and parameter uncertainty. It seems that this is the single CCDF referred to in the EPA regulations.

The discussion in section 3.1.5 is out of place in this report. It is not at all necessary to understand the concept of a Borel algebra in order to follow what has been done in the assessment. The section should be deleted.

Section 3.2 Definition of Scenarios

The definition of the eight scenarios in section 3.2.1 is not as complicated as the notation might lead a reader to suspect. The scenarios can be defined without recourse to set theory or logic diagrams.

The discussion in section 3.2.2 introduces a lot more notation. This is unhelpful to the reader. This issue is reviewed in detail for Volume 2, Chapter 2.

Section 3.3 Determination of Scenario Probabilities

See the comments on Volume 2. Chapter 2.

Section 3.4 Calculation of Scenario Consequences

See the comments on Volume 2. Chapter 3.

Section 3.5 Uncertainty and Sensitivity Analysis

The choice of a Monte Carlo approach here is wholely sensible. The section is however much longer and more detailed than need be to understand the actual methodology chosen. A general treatise on uncertainty and sensitivity techniques is not needed. More helpful would be some representative examples of the techniques actually employed in the WIPP performance assessment, drawn perhaps from Volumes 3 and 4.

Page 3-63, lines 40-41. It is claimed that Importance Sampling is a particular type of stratified sampling. This is not how the phrase is used within the NEA PSAG. Importance Sampling is a scheme which uses different distributions to generate samples than are used to characterise uncertainty, with a view to focusing sampling on important regions of parameter space. This method has been shown to be much more efficient than random sampling or Latin Hypercube Sampling in particular waste disposal assessments (Robinson, Roberts and Sinclair, PSAG meeting, November 1991). Given the expensive nature of running each sample in the WIPP methodology, Importance Sampling should be seriously considered.

Page 3-66, line 43. It is stated that a sample of size 60 was generated from the 45 uncertain variables. No justification for using such a small number of samples is given. It might be a coincidence, but 60 is 4/3 times 45, and the claim has been made that the number of samples needed in Latin Hypercube Sampling is only 4/3 time the number of variables. This claim is not sustainable in general, and if it is true for the particular case of the WIPP assessment models, then this needs to be demonstrated.

To illustrate why 60 samples seems to be too small a number, consider the samples generated for one particular variable. With 60 samples, the expected largest and smallest samples leave one percent of the distribution at each end unrepresented. Since the EPA regulation involves the 0.1 percentile, this is dangerous.

3.1.4 Chapter 4. SCENARIOS FOR COMPLIANCE ASSESSMENT

Section 4.1 Definition of Scenarios

Section 4.1.1 Conceptual basis for scenario development

The introduction to this chapter describes scenarios in terms of the notation developed in Chapter 3. This notation seems unnecesary and merely overcomplicates what is really a relatively simple concept. A comparison with the scenario development carried out by SKI, based on the Sandia approach, will illustrate the difference.

Page 4-4, Figure 4-2. The base case scenario as defined excludes human intrusion, and therefore may be considered to have a relatively low probability over 10.000 years. Its placement in the figure implies a very high probability (greater than 0.9).

Section 4.1.2 Definition of summary scenarios

The completeness of the initial set of events and processes compiled as input for the scenario development is unclear. Apparently, no formal site-specific process was followed. Rather, a generic list developed about fifteen years ago and reported in Cranwell et al. (1990) was used, and this was supplemented by three additional potentially disruptive events and processes known to be of concern to the WIPP. The ad hoc procedure followed does not provide confidence that the scenario development was based on a sound initial compilation of potentially disruptive events and processes. This is an area that would very much benefit by the expert panel approach used in other areas of the performance assessment.

In addition, the most up-to-date generic list for events and processes of potential importance to disposal system safety is probably that contained in the report of the NEA/PAAG Working Group on scenario development (OECD/NEA, 1992). This work is not referenced at all.

For example, in the area of human intrusion, which is especially critical for the WIPP, several additional scenarios of potential concern could be added to the list given in the text:

1. The possibility (see below) of unsealed boreholes or shafts (as opposed to a degraded seal). Experience from the mining industry suggests that this is not an infrequent occurrence.

2. The possibility that undetected older exploration boreholes exist at the site, and have been left unsealed. Again, this is conceivable because the WIPP is located in an area of intensive resource exploration and production.

3. The possibility that excavation and mining occur at depth (e.g., for potash) adjacent to the site (but not within the controlled area, because the EPA Standard suggests that this can be excluded). Such off-site mining may still affect the integrity of the disposal system, either through the establishment of a horizontal access that intersects the waste panels or a contaminated part of MB 139, or via an indirect effect on the geological confinement capacity of the system.

Section 4.1.3 Evaluation of natural events and processes

The procedure used to screen the events and processes as input to the scenario development lacks rigour, and appears to be biased in many places, as detailed below. The section reads as if the intention is to rule out from consideration a priori as many events and processes as possible, without serious consideration as to the possible effects or likelihoods of occurrence. In particular, screening out events and processes on the basis of low consequence, without proper analysis of the potential consequences, should be avoided. This same point is also made by Cranwell et al. (1990).

On the other hand, the performance assessment work to date suggests that the site is fundamentally sound. An effort should be made to be more open-minded and imaginative in the scenario development work. Again, much needed confidence in the scenario screening could be obtained through independent geological input outside of the WIPP performance assessment group.

Although screening is an important aspect of scenario development, an equally important aspect is to explore the range of the possible. The contrast with the input provided for human intrusion (admittedly an important class of scenarios) is notable.

Mass wasting

Page 4-18, lines 43-45. Some words are probably missing. The Pecos River is presumably not more than 90 m lower in elevation than the waste panels (as stated in the report), but rather 90 m lower in elevation than the land surface overlying the waste panels.

Diapirism

Page 4-20, lines 25-31. More care is needed with the wording, here as elsewhere in section 4.1.3. The first part of this text implies that salt diapirism (over the next 10,000 years) is a low-probability event; the following sentence suggests that salt diapirism is not physically reasonable. The two sentences make different and conflicting statements (although both may lead to a similar conclusion).

Seismic activity

Page 4-21. It is stated that there are unlikely to be any changes in the geologic or hydrologic systems due to seismic activity, presumably because no evidence is available to suggest that any such changes have taken place in the past. It is not made clear what evidence would be expected if seismic events had altered. in particular, the hydrologic system at depth. Until relatively recently, with radioactive waste disposal, geothermal research, etc. producing interest in deep groundwater evolution and ages, no one would have collected any data that would be of relevance to substantiating this statement. It is only in the last ten years, with ideas on seismic pumping becoming more prevalent. that much consideration has been given to the effects of seismicity or in situ stress regimes of groundwater flow and evolution.

Ground motion is not of much relevance here, as any significant seismic influences would be related to changes that could have occurred at depth which might have affected groundwater chemistries, or in the extreme, hydraulic conductivities. The argument needs to be placed more in this context, rather than in terms of vibration. though of course these processes are linked.

Finally, at the end of this section, it is stated that future seismic activity will be of no consequence in the performance assessment. While it seems unlikely that seismic activity will be important in the performance assessment, this has not been demonstrated by the arguments provided. In any case, later summaries contained in this chapter are contradictory because they indicate that the effects of seismic activity have been included in the base case (e.g., page 4-86). If so, how has this been done?

Magmatic activity

Page 4-23. We agree with the comments presented in this section of Hunter's (1989) calculation regarding magmatic activity. This is a particularly good example of a misunderstanding of natural processes resulting in erroneous probabilites being calculated. It is essential that such probabilites are estimated by experts with suitable geological knowledge.

Deep Dissolution

Page 4-25. This section contains a discussion of a dissolution structure (Hill A) which is not present on any figure, though there is a Dome A in figures 1-6 and 5-16, which presumably is the same structure. It is unhelpful to refer to specific structures that are not also present in figures. Indeed, there is no need to mention Hill A at all, as it is superfluous to the general argument. This is an example of unnecessary text being presented, whereas better and more figures would put over the concepts in a more cohesive manner.

Shallow dissolution

It would be useful to have a diagram or a cross section which shows the relationship of Nash Draw with the surrounding formations, illustrating the shallow dissolution effects listed in this section.

Page 4-26, lines 28-29. Dissolution of the Rustler Formation by vertical infiltration from the surface has been eliminated because of low consequence, yet the potential consequences have not been analysed in any detail. This should be done for both present-day and pluvial climatic conditions.

Page 27, lines 14-16. Dissolution by flow along the Rustler-Salado contact zone has also been screened out because of low consequences, but here again the qualitative arguments provided are insufficient to justify the screening choice.

In addition, evidence is presented in Chapter 5 that dissolution in the underlying Rustler-Salado contact zone has caused fracturing of the unnamed Lower Member (page 5-14, lines 18-21), and that post-depositional dissolution of salt in the Rustler Formation may have caused fracturing in the Culebra (page 5-16, lines 5-6).

M

Faulting

Page 4-29. Here is another example of a discussion of geological structures with no accompanying figure. A figure that combined the location of the dykes and the faults would provide a much better idea to the reader of what structures were being discussed.

Section 4.1.5 Evaluation of human-induced events and processes

Injection wells

This section is confusing. The argument is rightly made on page 4-36 that injection wells may have the same effect as exploratory drilling, and cannot be eliminated from consideration on the basis of low probability. At the top of page 4-37 (lines 3-8), it is however suggested that injection wells can be eliminated because the driller will "soon" detect the incompatibility of his activities with the area. "Soon" is not defined in Appendix B of the Standard, but seems to be defined here so as to eliminate injection wells from consideration. This is an inappropriate place to define terms appearing in the Standard and, in any case, the same definition has not been used to eliminate exploratory drilling elsewhere in this section.

Finally, at the end of the section (page 4-37, lines 10-15), another argument for elimination of injection wells is given. It is suggested that they can be eliminated because the fluid injection will have no effect on the disposal system, but no analysis of the possible effect on the disposal system of a well undergoing injection (and passing through the repository) is given.

Withdrawal wells

Page 4-37, lines 26-29. Wording could be clearer. The text jumps from water wells to exploratory drilling for resources.

Page 4-38, lines 4-18. Drinking wells are eliminated because of lack of physical reasonableness, but if they are to be eliminated, this could only be on the basis of low probability, because potable water evidently does exist in local areas, and the effect of a more pluvial climate may be to extend the area in which potable water could be obtained. In addition, desalination could be undertaken in future.

Damming of streams and rivers

Page 4-41, lines 29-30. Additional damming of the Pecos River is eliminated because of low consequence, although this has not been demonstrated. Additional damming would probably only make sense in more pluvial conditions. Could the consequence under such conditions be demonstrated by some kind of relatively simple bounding calculation?

Page 4-42, lines 25-26. An extensive discussion of damming of Nash Draw is provided, and then this event is eliminated on the grounds of physical unreasonableness. The discussion and conclusion are nonsensical, however, because damming of Nash Draw would only be likely during more pluvial conditions (which are not discussed at all). It may nonetheless be possible to eliminate this event on other grounds (e.g., low probability?).

Section 4.1.5 Evaluation of repository- and waste-induced events and processes

Caving and subsidence

Pages 4-43 to 4-49. A semi-quantitative treatment is provided for eliminating this category of events, which makes a change from the more qualitative arguments provided for other categories of events. A parallel with the regional dissolution of the Salado Formation is drawn, but the significance is unclear. Salado dissolution is occurring regionally, whereas subsidence above the repository would be a strictly local feature. Local subsidence could conceivably lead, for example, to an increase of leakage between the Culebra and the Magenta, increased dissolution of the Rustler, etc.

In addition, dissolution of the Salado and any associated subsidence will have occurred over periods orders of magnitude longer than the subsidence associated with the repository (and associated local fracturing).

Finally, evidence is in fact presented in Chapter 5 that fracturing in the Culebra has been caused by previous subsidence [associated with post-depositional dissolution of salt in the Rustler Formation, or with stress relief from removal of overburden (page 5-16, lines 5-6)].

Shaft and borehole seal degradation

Page 4-49. Seal degradation has been considered and included in the base case. However, the situation of a totally unsealed borehole has not been considered (e.g., what if ERDA-9 was accidentally left totally unsealed?). It may have to be assumed that one or more boreholes will be left unsealed, either now or in the future. This is a potentially important event has not been considered.

Explosions

Page 4-52. Are explosions in the waste panels expected to occur? The text indicates that they are, but suggests that the 800 psi generated in an explosion will not be problematical, and they have therefore been eliminated because of lack of consequence. The potential consequences have not however been properly evaluated. For example, an explosion of the force indicated might also induce local fracturing in the rock. What would the magnitude of this effect be?

A more sensible approach to the presence of explosive gases would be to attempt to engineer around the problem, so that it could be eliminated on the grounds of physical unreasonableness or low probability. It would be wise to avoid a situation where the public were to be told that the explosions within the waste panels were expected, but that it was thought that the repository had been designed to withstand the shock of these explosions!

Section 4.1.6 Summary of screened events and processes

Page 4-54: It is stated that a pluvial climate is certain to occur in the future, but that damming has been eliminated on grounds of physical unreasonableness (i.e., lack of water supply). However, the consideration of poor water supply only relates to the current climatic conditions.

Page 4-56 , Table 4-2. There is an error in the table. Shaft and borehole seal degradation should be placed on one line only, and the "X" in the "retained for scenario development" column should be deleted.

Section 4.1.7 Developing summary scenarios

Page 4-59, lines 22-24. Page 4-61. Figure 4-5. E3 (water wells) was retained for scenario development (e.g., see table 4-2, page 4-56), but has been eliminated without reason here. Also, why are only the E1, E2 and E1E2 scenarios described and considered further? The argument for eliminating all TS scenarios is unclear.

Page 4-62, line 1-3. It is claimed that the EPA Standard provides a basis for eliminating scenarios having a likelihood of occurrence of less than 1 chance in 10,000 in 10,000 years. This represents a misinterpretation of the EPA Standard. The EPA Standard refers to categories of events and processes (i.e., before their combination into scenarios), and not to the scenarios themselves.

Page 4-62, line 9-14. This statement is unclear and confusing. Is the word "less" on line 11 supposed to be "greater"?

Page 4-63.

1. On line 21, figure 4-4 should read 4-6.

2. The base case summary scenario is here being discussed with very little knowledge of the geology, lithology and structure of the Salado Formation, nor of the engineered barriers. Most of the important detail is contained in Chapter 5; for example, there is no indication here of the separation of MB 139 from the galleries, nor any description of what MB 139 actually is. All explanation related to the DRZ is also contained in Chapter 5, so that the reader cannot assess the reasonableness of the proposed base case scenario in this section. Similar arguments apply to all the remaining parts of the descriptions of the base case scenario.

3. The term DRZ is used in the text and in figure 4-6 (page 4-65) without any explanation of what it means.

4. None of the discussion of the development of gas pressures and migration within and from the waste panels refers to the description of the various processes involved which are presented in section 5.2.5.

5. On line 42. the wording ("some...some") suggests that relatively few waste containers will contain organic material. What fraction of waste containers will contain organic material?

Page 4-69. lines 39-41. In the E2 summary scenario, it is assumed that the borehole plug does not degrade. What happens if the borehole is left unplugged (or if the plug degrades quickly)?

Section 4.1.8 Definition of computational scenarios

No comments.

Section 4.2 Determination of Scenario Probabilities

In section 4.1.2, the event probabilities are presented without justification. Moreover, it is unlikely to be possible to develop credible probability estimates for human intrusion events. The human intrusion event probabilities seem relatively low. It would be more conservative to assume that human intrusion (through exploratory drilling) had a probability of occurrence of close to 1 (which would equally imply that the base case as defined had a very low probability of occurrence). It is especially important to be open-minded on this question early in the assessment process, so that the full range of possibilities is properly explored (e.g., via sensitivity analyses).

In addition, it is unclear why the human intrusion scenarios have such low probabilities of occurrence over 10,000 years, given the guidance in Appendix B of the EPA Standard of 30 boreholes per square kilometre of repository area per 10,000 years (upper limit on frequency of drilling into the repository). It seems to be implied in the subsequent section that the guidance in the Standard would lead to an assumption of about 3 boreholes in 10,000 years.

Also, the period of time to which the probability estimates presented in this section apply should be explicitly stated (it is not stated anywhere).

Section 4.3 Expert Judgement on Inadvertent Human Intrusion

This section gives the impression that the possibility or even the probability of future human intrusion events can be identified. It tends to elevate the whole issue of human intrusion to a pseudo-science, when, in fact, the approach to treating human intrusion assessment must be largely philosophical, regulatory requirements notwithstanding.

3.1.5 Chapter 5. COMPLIANCE-ASSESSMENT SYSTEM

Section 5.1 The Natural Barrier System

Page 5-9, Figure 5-6. There is no indication of what the shaded line is in the northern part of the figure.

Page 5-12, line 9. The difference in hydraulic conductivies between well tests and in situ tests in the Salado Formation is not necessarily due to disturbance. The difference may merely be due to the normal scale effects seen in most rocks.

Page 5-13, Figure 5-7. Is this cross section partly diagrammatic? If not it is incorrect. The change of dip of the Rustler Formation on the eastern margin of Nash Draw appears unnatural and would seem to be an artefact related to the position of borehole WIPP-26. The dip of the Culebra Dolomite changes without there being any equivalent change in dip of the Magenta Dolomite or the Tamarisk Member, and from the structure of the area this seems improbable. In addition, the cross sections implies that the Tamarisk Member suddenly thickens, whereas on page 5-16 it states that dissolution of evaporites within this Member has reduced the separation of the Magenta and Culebra Dolomites, and therefore the opposite is true.

(Figure 2.6-2. Volume 3 shows the true distribution of the strata, though even in this figure the repeated changes in thicknesses of the beds appear unnatural.)

Page 5-16, lines 9-10. Both figures 5-8 and 5-9 should be referred to here for clarity.

Page 5-32, line 13. It is stated here that discharge takes place from the Tamarisk Member. In the description of the Tamarisk Member on page 5-16 it is indicated that in the region of Nash Draw the evaporites have been dissolved from the Member and even the siltstone within the Member has a low transmissivity. It would seem with these transmissivity values that only minimal discharge or recharge (referred to on page 5-33, line 16) would take place. Either the description of the Tamarisk Member on page 5-16 is misleading, or there would appear to be some inconsistencies between its description and the description of potential recharge and discharge zones.

Section 5.2 The Engineered Barrier System

Do engineered barriers "minimise the likelihood (i.e., probability) of radionuclides migrating" or do they minimise the rate of migration given the occurrence of water (either by intrusion or microbially generated) within the waste repository?

Section 5.2.1 The Salado Formation at the repository horizon

At what scale is the stratigraphy of the Salado Formation laterally "consistent"? Are there small, discontinuous stringers of clay and/or anhydrite that occur within the planned repository horizon? Given the approach of assigning probability distributions to parameters affecting source-term (e.g., solubility, rock permeability), what is the estimated probability distribution for small-scale variation in stratigraphy (perhaps a plot of observed frequency versus size of stringer would be helpful).

Section 5.2.2 Repository and seal design

Waste Characterisation

Do the Waste Acceptance Certification requirements say anything about dissolution or leach rate of waste? If so, that should be stated in this section.

Page 5-45, lines 1-2 (and again in Section 5.2.3). The uncertainty in waste inventories and characteristics are cited as potentially large. The reader is left wondering how large is "large". if there is any sensible bound to this estimated uncertainty and. perhaps most importantly, what are the factors that affect this uncertainty. If these issues on waste uncertainty have been addressed, they ought to be cited. If they have not been previously addressed, the text should explain if and when such studies will be undertaken.

Page 5-45, line 27. There are many types of "steels" having various modes and degrees of reaction with water or gases; it would be helpful if these steels for waste boxes and drums were specified with a brief modifier.

Seals

Page 5-46. Figure 5-19. Page 52, Figure 5-20 and Page 5-54, Figure 5-21. These all indicate that the separation of MB 139 from the base of the disposal vaults is greater than is actually the case, and in the case of figures 5-20 and 5-21 greater than the distance to Anhydrite B. This is not correct. Figure 5-19 is particularly misleading in terms of scale because a real vertical scale is implied in the figure illustrating the drift backfill and plug seals. The vertical dimensions implied by the thickness of the disposal vault in this figure compared with the vertical separation of the vault with MB 139 and the thickness of MB 139 are all incorrect. Either this diagram should state that it is only diagrammatic, and therefore should contain no dimensions of any sort, or better still it should be drawn to a true scale.

Page 5-47, lines 10-13. The terms "long-term" and "short-term" are too vague; perhaps cross-reference to section 3.2.1 in Volume 3 would be appropriate.

Page 5-47, lines 31-32. "Over time" is vague, as is reference to "other interbeds", presumably meaning Anhydrite Layers A and B. The text should be more specific.

Backfill

Page 5-47, lines 36-45.

1. The text discusses backfilling of underground workings but does not specifically mention using consolidated materials; is it correct to infer that unconsolidated materials will be used?

2. It is claimed that permeability will be initially reduced (prior to consolidation) of this backfilling but the text does not quantify the extent to which permeability is reduced or how effective this reduction is in limiting water migration into the waste panel.

3. It is also claimed, without supporting reference, that consolidation will occur to limit water inflow but it is not said how long this will take. A plot of permeability (or degree of consolidation) versus time would be extremely helpful. It would seem that brine would be flowing into the waste panel during (and in response to) this consolidation.

4. This paragraph introduces the possibility of clay backfills but leaves the reader wondering if this consideration is an attempt to correct expected poor or unpredictable behaviour of the salt backfill or whether clay is being considered for the positive characteristics of the clay itself that may enhance the near-field performance.

Section 5.2.3 The radionuclide inventory

Page 5-48, line 34. Again, how large is "large"; the text should strive to be more specific or quantitative or bounding, rather than using qualitative, relative modifiers such as this. Also, is there any reference that analyses the factors that affect this "large" uncertainty?

Page 5-49, lines 9 and 11. Please be more specific than "short half-lives" and "low toxicity" (in contrast, the "low activity" term is properly specified).

Page 5-49, lines 9-10. It would help if the text defined the edge of the "repository" from which radionuclides are leaving.

Page 5-49, line 20. While radium-226 activity will be insignificant during the first 10,000 years, to what levels will it increase over 100,000 or 1,000,000 years? It may represent a safety/risk concern over these longer time scales and should not be dismissed solely on the limited time frame of current regulatory guidance.

Section 5.2.4 Radionuclide solubility and the source-term for transport calculations

Solubility is described as "the most important single contributor to variability in total cumulative release". This admission indicates that it would be prudent to include a comprehensive and systematic treatment of this subject within this volume. Supporting references should be (and are) provided in the text. Yet the critical sensitivity of solubility may necessitate that all essential details be explicitly presented in this summary. The current text is too brief for the importance of the topic and too confusing to be helpful to both technical and non-technical readers. Suggested revisions are outlined below.

Page 5-49, lines 25-27. It would be helpful to describe the specific source-term model used in WIPP analyses so that readers could judge for themselves the relative sensitivity of release rates on solubilities and other parameters. A listing of the mathematical formulation for release would be especially instructive.

An explanation should be provided for the selection of a specific type of distribution (e.g., loguniform) and specific range for solubility values, and the reasoning behind assigning such fixed distributions to all radionuclides. Although the WIPP Project is moving beyond this approach, a fuller explanation of how and why such an approach was initially adopted for such an admittedly crucial parameter would be important to the concerned reader.

Page 5-49, line 34. There is not an "absence" of data; rather, a paucity of data exists on radionuclide solubility in brines. For example, Dr Dhan Ral and colleagues at the Pacific Northwest Laboratory have conducted and published several papers on experimental measurements and theoretical models they have developed for the previous salt program for disposal of high-level waste. Other workers have also studied this topic, notably for the German repository program. The assertion that there is an absence of data indicates, at best, a surprising unfamiliarity with the scientific literature; at worst, it shows a predilection toward substituting expert judgement in place of experimental data. Either case severely undercuts the credibility of the expert panel approach adopted by the WIPP Program. Accordingly, it is suggested that the text be corrected and expanded on this key topic of existing experimental data.

In particular, no references could be found in section 5.2.4 or in section 3.3.5 on existing experimental data. The brief mention of existing data on page 5-50, line 38 is entirely inadequate for a parameter which has the *most important uncertainty in the entire performance assessment.*

Page 5-49, line 42. The terminology used here is "expert panel on source term", whereas page 5-49, lines 36-37 and page 5-50, line 37 indicate that it was an expert panel on solubility. Clearly the latter is a small, albeit important, subset of the former. It is crucial to understanding of the approach that the technical subject for the expert panel be clarified and consistently cited.

Page 5-50, lines 9-10. The emphasis on "complementary areas", "breadth" and "balance" seems contradictory to the purpose of invoking a single expert panel on a single issue (i.e., solubility of specific radionuclides under variable Eh and pH conditions). It would seem that this was not a single expert panel but rather four separate one-member panels in four different areas. This separateness of expertise in members of an expert panel elicited for a single purpose is reinforced in lines 34-35, which refers to "each panelist's particular expertise". Readers, especially those who are not specialists in organisation and elicitation of expert panels, will almost certainly be confused by this seeming contradiction.

Page 5-50, lines 12-15. This sentence seems to make a distinction without a difference. The difference between "considering a solubility limit" and "to consider explicitly the individual radionuclide concentrations" should be clarified. Is it suggested that solubility limits will not be reached or that metastable compounds will form or that colloids will be a major contributor for all of these radioelements?

Page 5-50, lines 23-25. The text should note if the communications among panelists were documented and if the panelists communicated with any other "experts" outside the panel during this time. If so, further clarification for the non-specialist is needed to establish the appropriateness of this procedure within the accepted precepts of expert panel elicitation.

Page 5-50, line 39. The single citation of pH as an example of an environmental factor considered is inadequate, again given the acknowledged importance of the solubility values. For example, were pressure solution effects on solubility arising from salt creep considered as a factor? If not, why not?

Page 5-51, lines 1 and 14. Phosphates are well established as important compounds/complexants of actinides. Does the absence of a citation to phosphate imply that such compounds were not considered? The brevity of this treatment of solubility and the expert panel tends to raise more questions than it resolves.

Page 5-51, lines 6-7. This admission is astounding and seemingly in contradiction with the rationale for convening the expert panel. The initial justification for the expert panel was to provide judgement about radionuclide concentrations (page 5-50, lines 12-15) because it was (erroneously) considered that there was an absence of experimental data (page 5-49, line 34). Now the panel was allowed to cease its efforts on this very topic because "available data was insufficient". The reader is left wondering if perhaps these experts might not be so expert after all and whether the WIPP Program has forgotten the basis for convening the panel. This is an unsatisfactory conclusion to this effort.

Section 5.2.5 Performance assessment model for the repository shaft system

Page 5-51, line 40. Will fractures also develop in Anhydrite A and B layers and will these fractures also fill with brine? Figure 5-20 (page 5-32) would seem to suggest this is true.

Page 5-53, line 1. The use of the word "could" indicates that other time durations are possible. What is the effect on performance assessment if closure is complete in 10 years or in 1000 years? Are these possibilities addressed somewhere?

Page 5-53, line 23. Could the activity of other types of microbes generate other gases such as carbon dioxide or methane? If such events have been eliminated, it would be useful to know the basis for their elimination.

Page 5-55, line 34. Rather than merely state that neither pathway resulted in radionuclides reaching the Culebra Dolomite within 50,000 years, it would be helpful if some indication were given of the time interval over which radionuclides did reach this formation and what their subsequent migration was thought to be.

Page 5-55, lines 37-42. It is noted that SUTRA and STAFF2D have been used to model undisturbed performance. Brine flow only is considered, and gas generation is not directly considered in the simulations. The WIPP Performance Assessment Division has already recognised the weakness of the approach adopted, and so no detailed review of this material is provided (see also Volume 2, Chapter 4 comments).

Page 5-56, line 32. How are the rates of these processes determined? Is there an unstated assumption that the limiting step on corrosion is the influx rate of brine or are specific corrosion kinetics considered for metals and, if so, how are available surface areas determined?

Page 5-59, lines 12-21. The relative degree of conservatism for each of the assumptions should be noted. If metastable compounds form containing radionuclides, is the chemical equilibrium assumption likely to be conservative?

Page 5-59, line 43. Is it also assumed that future extraction technologies will be comparable to those of the present?

Page 5-60. The discussion on borehole intrusion into a waste vault is imbalanced, as it appears to miss out several aspects of the argument which are likely to be important. It would seem unlikely that any driller would ignore the effect of the drill bit intersecting a shielded waste container, for example, because this would probably be detectable at the surface in terms of changes in torque, vibration, rate of penetration and perhaps mud returns. Damage to the drill bit would be probable. As some aspects of the borehole intrusion scenario have been considered in great detail, there is a notable lack of discussion of whether intrusion into the waste galleries would be detectable by the drillers, and what the consequences would be of their probable resulting actions.

A discussion of drilling into a SKB-type HLW repository, admittedly using very different drilling techniques, is presented in Charles and McEwen (1991), where more realistic assumptions on drilling intrusion have been applied.

We agree with the comments made by EEG presented in Appendix B, pages B-30 to B-36 concerning human intrusion by drilling. It is important that if so much is to be made of intrusion as the one method by which radionuclides can be brought to man's environment within the 10,000-year period, then totally credible scenarios should be considered.

Section 5.3 CAMCON: Controller for Compliance-Assessment System

This section describes the CAMCON system developed to control data flow and code linkage in the performance assessment calculations. The CAMCON package is extremely complex, and apparently completely unverified. What has been done to provide confidence that the executive system is functioning as intended? For example, it might be of interest to run some of the early NEA/PSACOIN exercises, or to undertake some other PSA code intercomparison activity. In addition, the information in table 1 (pages 5-67 to 5-72) suggests that all but one of the 79 submodels in CAMCON are still unverified. Is this really the case?

3.1.6 Chapter 6. CONTAINMENT REQUIREMENTS

Pages 6-9 to 6-11. Table 6-3. The systematic listing of major assumptions made in obtaining the assessment results provided in this chapter is an important component of the report. These assumptions define many of the conceptual models used in the assessment. This table leads directly to the question of conceptual model uncertainty. It is stated elsewhere that conceptual model uncertainty has not yet been explicitly treated in the assessment work. This will be an important development for future year assessment reports. Although some of the assumptions listed in this table may be adequately considered through bounding calculations or sensitivity analyses. others may relate more directly to the modelling system selected for the assessment. Examples range from the assumptions that no radionuclide transport occurs in colloidal form, to the decision to ignore the contribution of RH-waste to subsurface groundwater releases to the accessible environment, to the decision to model the Culebra dolomite as a two-dimensional confined aquifer, with a single-porosity Darcy flow model and a dual-porosity transport model (for retardation effects).

3.1.7 Chapter 7. INDIVIDUAL PROTECTION REQUIREMENTS

No comments.

3.1.8 Chapter 8. ASSURANCE REQUIREMENTS

Page 8-10, lines 33-35. It is indicated that one objective of active institutional control is to restore the land surface to its original condition. However, this activity would seem to be incompatible with the current plan for passive institutional control to establish a system of permanent markers around the site (page 8-11, lines 9-11), particularly considering the type of large-scale marker systems being recommended by the WIPP Markers Panel.

3.1.9 Chapter 9. GROUNDWATER PROTECTION REQUIREMENTS

No comments.

3.1.10 Chapter 10. COMPARISON TO THE STANDARD

This chapter is little more than a repetition and summary of material provided in the preceding four chapters. Excellent synopses already exist for these chapters. It might be preferable to combine the material in these four chapters into a single chapter, as they are all relatively short.

3.1.11 Chapter 11. STATUS

The main focus of this chapter is to indicate where additional work needs to be done to increase confidence in the assessment results. A very long table is presented, suggesting that a great deal of work still needs to be done, yet the assessment results presented in Chapters 6-10 show either certain compliance or a high degree of confidence. Nearly all of the assessment work is focused on section 191.13 of the Standard (the containment requirements), but even here, with the currently available knowledge, there appears to be many orders of magnitude to spare. The disposal system is robust, and human intrusion may well be the only credible scenario of concern. Because of the apparent contradiction between the assessment results and the presentation in this chapter, there is a need to prioritise the information presented in the table on the basis of the sensitivity analyses already completed and reported in Volume 4 (note however that we have not yet received this important Volume for review).

3.2 Volume 2: Probability and Consequence Modeling

3.2.1 Chapter 1. INTRODUCTION

No comments.

3.2.2 Chapter 2. DRILLING INTRUSION PROBABILITIES

This chapter addresses the issue of generating the scenarios to be used in computing a CCDF. This has two components – the philosophy behind the CCDF construction and the mechanics of generating the scenarios. The comments below refer mainly to the latter. comments on the construction having already been made for Volume 1. Section 3.1.

The overall impression of this chapter is that

- the presentation is unnecessarily mathematical;
- the full generality that is maintained is unnecessary, and contributes to the previous point:
- a simulation approach would have been easier to implement and explain.

The excessive use of mathematics obscures some of the important questions that should be asked about the treatment of human intrusion presented. Most importantly, does the approach lead to greater confidence in the assessment results? The reason for developing the many computational scenarios from the small number of summary scenarios is to obtain a finer resolution of the CCDF. Is this needed if the rougher resolution of the CCDF (e.g.. just based on the small number of summary scenarios) can be used to demonstrate compliance? It seems that sight has been lost of an important objective in evaluating uncertainty and in presenting the results of performance assessments. The approach taken to treat uncertainty should be done so because it leads to a clear presentation of assessment results and associated uncertainties, and not only to ensure mathematical rigour.

After specific comments on each section and on the following chapter, the simulation approach is described and illustrated by regenerating some of the CCDF curves.

Section 2.1 Introduction

This section restates the EPA regulation, and addresses the issue of constructing a single CCDF. Our comments on Volume 1, Section 3.1 also apply here, since much of the material is common.

Page 2-4, Equation 2-4. The notation $\doteq$ is not a familiar one, and is not explained.

Section 2.2 Mathematical Preliminaries

There should be a section prior to this to remind the reader what is being calculated and why. The purpose seems to be to give scenarios with single or multiple intrusions. These will be used in the next chapter to generate CCDFs.

As for this section itself, it is unhelpful to the reader. Although almost certainly mathematically correct and rigorous, it presents ideas which need only appear in a pure mathematics textbook or journal paper. It is impossible to give a detailed review of the whole section; instead some particular examples of these points are made below.

- Pages 2-7 to 2-8. Equations 2-6, 2-7 and 2-8 present three ways of writing mathematically the statement of lines 6 and 7 of page 2-8. For the general reader this is unhelpful and for the mathematical reader it is merely repetitive.
- Page 2-8. Various possible forms of $F(u, v)$ are given. No indication is given as to which is actually used in the assessment. Later in the report it is clear that the simplest and most conventional form (equation 2-9) is in fact used, and this is a sensible choice. Focusing on this case throughout would reduce the need for much of the complexity, and enable the reader to see what is actually being done.
- Pages 2-9 to the middle of 2-11. This material again belongs in a textbook or journal paper, not in an assessment report. Most readers will simply be intimidated by such a presentation.

Section 2.3 Computational Scenario Probabilities for Single Time Intervals

Again the generality is unnecessary: the actual case used should be described.

Section 2.4 Computational Scenario Probabilities for Multiple Time Intervals

Same comment as for section 2.3.

Section 2.5 Computational Scenario Probabilities for Pressurized Brine Pockets

Same comment as for section 2.3.

Section 2.6 Example Results

The decision to use just five time intervals is not well justified. It seems to be based on consideration of combinatorics rather than on the accuracy of calculated results.

The result of all the effort in this chapter is a series of rather daunting tables for input to the next chapter. Given the methodology chosen, these tables are required but need not be presented in such a stark fashion.

3.2.3 Chapter 3. CONSTRUCTION OF COMPLEMENTARY CUMULATIVE DISTRIBUTION FUNCTIONS

Section 3.1 Introduction

This section repeats much of section 2.1 and Volume 1, section 3.1.

Section 3.2 Construction of a CCDF

Again this is too mathematical.

Section 3.3 Computation of Activity Loading Effects

This section dwells too much on the combinatoric problems caused by the methodology that has been chosen. As an example of this, table 3-2 (page 3-10) serves no purpose as far as the reader is concerned.

In a number of places alternative approaches are discussed but it is unclear which has finally been chosen.

Section 3.4 Examples of CCDF Construction

No justification is given for the use of only 60 samples to study parameter uncertainty.

Page 3-17. Equation 3-37 and the preceeding text is confusing, since it seems to contradict table 2-5 (page 2-35) where the relationship between area of brine pockets and area of waste panels was set out.

Page 3-19. Table 3-3 indicates that the consequence changes only slowly with time. This might be used to justify the use of just five time intervals; however no such argument is presented.

Pages 3-20 to 3-25. Tables 3-4 and 3-5 are unhelpful; something with more visual impact would be much better.

Page 3-28. The issue of non-conserved probabilities points to using a different approach. An example of such an approach is given in the next section of this review.

An Alternative Approach using Simulation of Intrusion Events

In this section an alternative simulation methodology is presented for generating the CCDFs for intrusion. The alternative approach is outlined here to illustrate the use of a simpler and more straightforward mathematical and computational approach (than that adopted by Sandia) to the development of a CCDF for human intrusion events. In addition, adoption of the suggested approach would allow the time-dependent consequences of human intrusion events to be clearly illustrated. The suggested approach is easy to describe and implement.

There are an infinite number of possible futures for the repository. In any one future a number of intrusion events will occur and these will have consequences, in terms of releases to the accessible environment, which can be predicted by using the various models available.

The consequences of individual intrusion events or of multiple interacting events (of the E1E2 type) will be time-dependent. The time-dependency can be represented as a piecewise-constant as in the current methodology, or in any convenient form.

The suggested approach generates sequences of intrusions numerically from the basic assumption that this is a Poisson process. If other processes are to be used a similar methodology could be developed.

By generating large numbers of possible futures, and calculating the consequence of each, the CCDF can be directly formed. In the example given below 10^6 futures were generated (taking 8 minutes on a Macintosh computer).

To illustrate the ease with which such an approach can be implemented, the results given in figure 3-4 for cuttings have been reproduced using a small C-program. For each future a sequence of intrusions is generated and the consequence cumulated. When activity loading is considered, the activity level of the penetrated waste is determined randomly according to the given probabilities.

Using this program, and binning the results, the CCDFs are generated. Those shown in figure 1 can be directly compared to figure 3-4 of the WIPP assessment Volume 2, and are found to be virtually identical.

In addition to reproducing the results given, the simulator can be used to look at the possible effect of including time-dependence of the consequence more precisely. Obviously from the five time values given we cannot generate a full time history, and so we have interpolated (and extrapolated for early and late times) from these values. The tables in Appendix B indicate that the normalized releases are dominated by one or two nuclides and so tend to fall off exponentially. Therefore, a linear interpolation and extrapolation scheme has been used for the logarithm of the normalized release. The results with this scheme are shown in figure 2.

The major effect of this change is to smooth the CCDF somewhat. This is a direct result of the elimination of the discontinuities in consequence at 2000-year intervals.

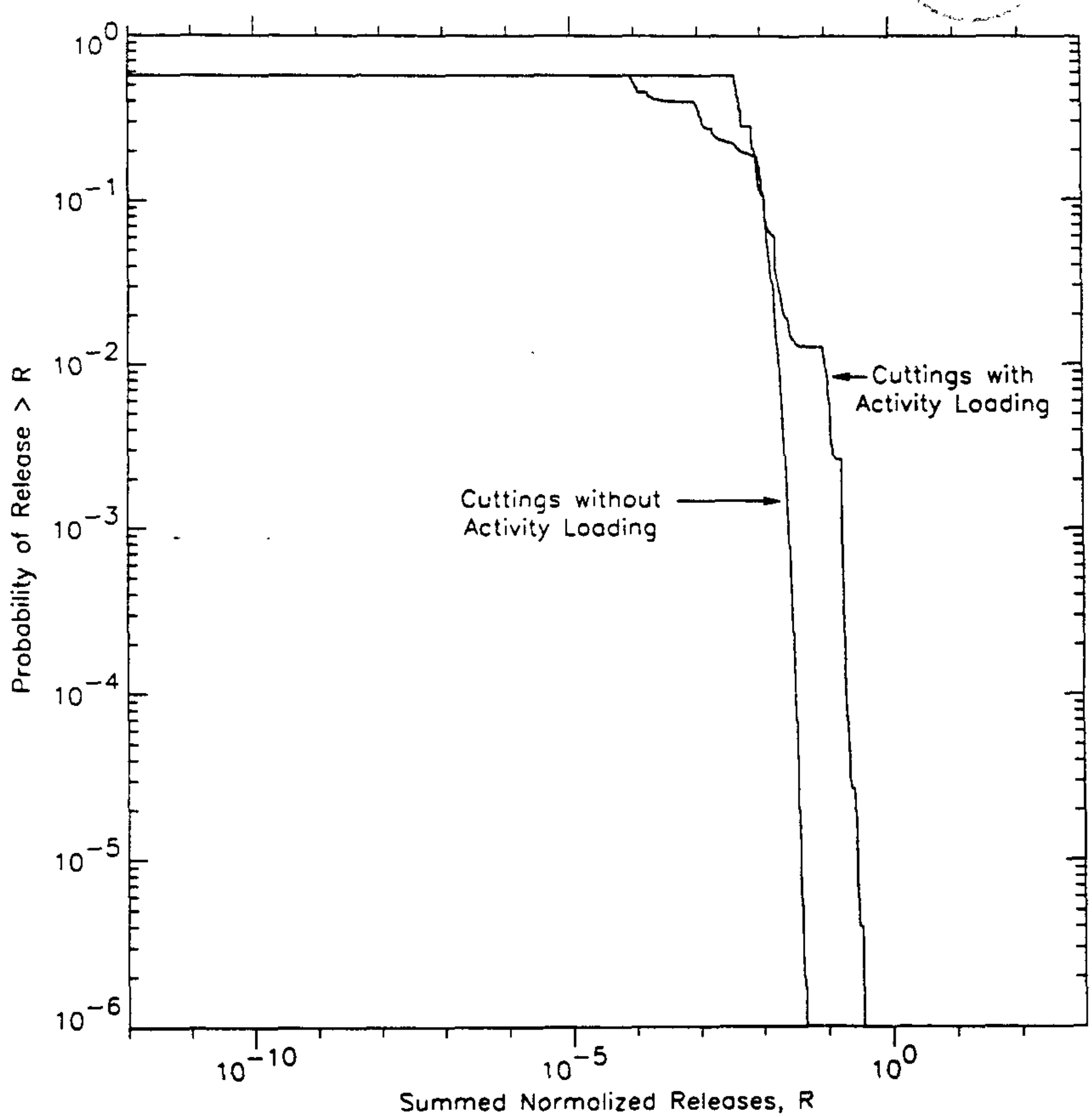

Figure 1: CCDF for cuttings produced by simulation approach.

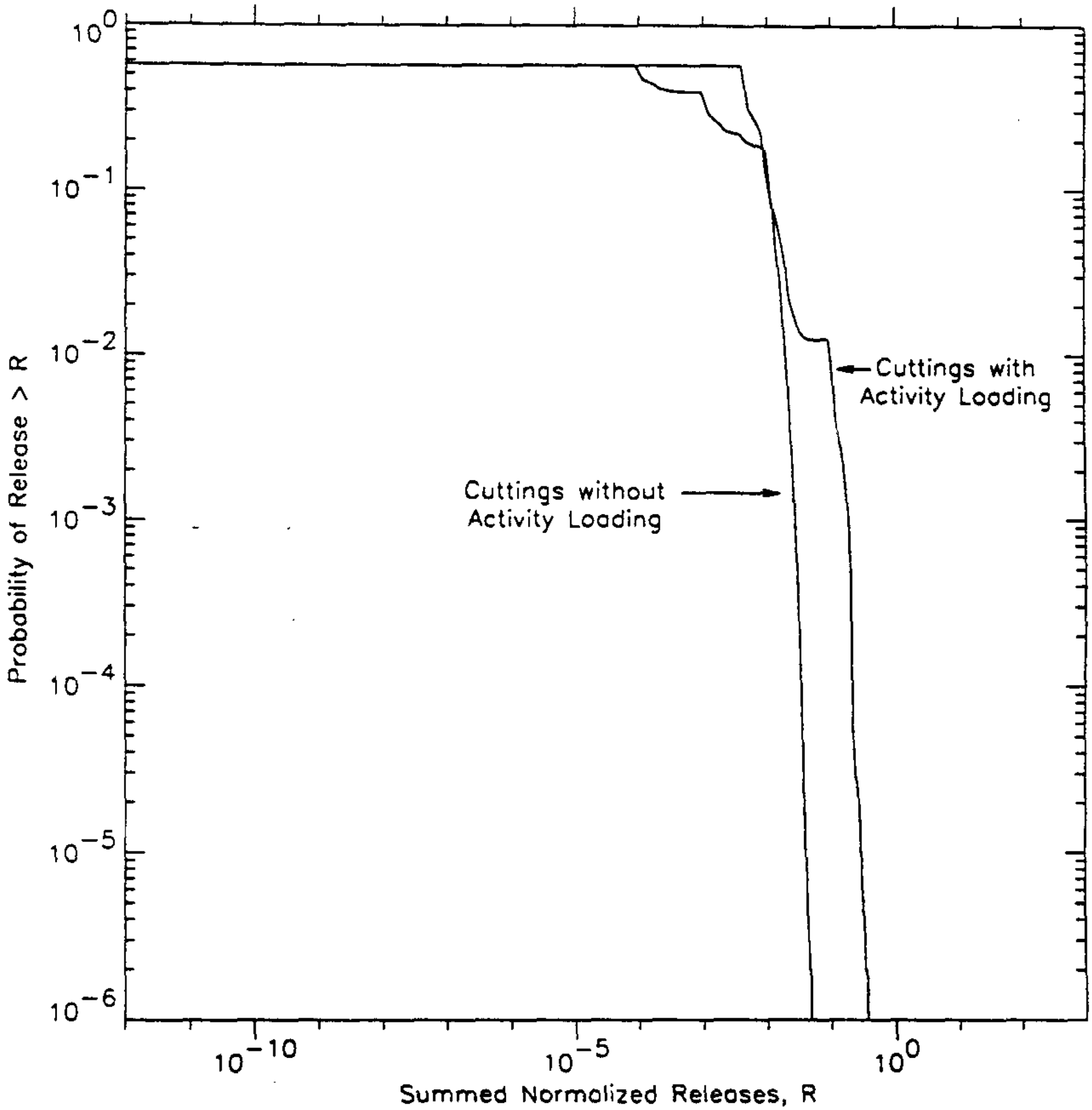

Figure 2: CCDF for cuttings produced by simulation approach with continuously time-varying consequence.

3.2.4 Chapter 4. UNDISTURBED PERFORMANCE OF REPOSITORY/ SHAFT

The 1991 undisturbed performance calculations were made largely on the basis of results from the SUTRA and STAFF2D one-phase-flow codes. We have not reviewed this chapter in any detail, because we understood that a decision has already been made to model the undisturbed performance with the BRAGFLO two-phase-flow code for the 1992 assessment.

3.2.5 Chapter 5. DISTURBED CONDITIONS OF REPOSITORY/SHAFT

Section 5.1 Conceptual Model

Pages 5-1 to 5-2. The initial introduction serves as a reminder of the scenarios modelled and it would help the reader to have a diagram to refer to, either in this chapter or by reference to one in the preceding chapter. It is clearly stated here that scenario E2 includes the possibility of more than one borehole drilled to the same panel, whereas later in the text (section 5.4. page 5-59. lines 5-6). E2 is described as a single intrusion scenario. The rationale for modelling multiple boreholes as a single one is explained for the E1E2 scenario but is not justified for the E2 scenario.

The assumption that multiple wells drilled to the panel are hydraulically isolated is questionable. No explanation is given apart from a vague reference to Chapter 5 of Volume 1. On reading this chapter, the only reference to hydrological isolation of wells refers to wells drilled into the Castile formation, where data suggest that the brine pockets in this formation are isolated. This does not correspond to the E2 scenario, however, in which the wells terminate in the panel. For unmodified waste, the panel has a higher permeability and porosity (Volume 3. section 3.4.7) than that of the Castile formation between brine pockets (Volume 1, Chapter 5, section 5.1.2). More explanation should be provided on this point, as it is not clear that if mutiple wells are modelled as isolated, then conservative results can be guaranteed. Multiple wells will provide additional escape routes for the gas, assisting the depressurisation of the panel and allowing more brine to enter from the far field at an earlier time, than if the wells are considered in isolation. It should be possible to check the performance of mutiple wells for a few example cases using a 3-D reservoir model, such as the TOUGH code used in the verification examples.

Page 5-1, lines 12-14. The use of the term "flow" seems unnecessary. Why not simply use the correct term, cumulative volume, and avoid any possible misunderstanding and the constant repetition of the quotes (which seem to be forgotten about in later sections anyway). The reminder that the volume is accumulated over 10,000 years can then be given once in the text, and also in the figures.

Pages 5-2 to 5-3. The subsections describing the approximations to the various scenarios are unclear. It would be more helpful to deal with each scenario in turn and explain the assumptions made in the calculations for each one, rather than mixing them up in this fashion. Subsection 5.1.2, on the approximation to the E1 scenario is particularly confusing, referring to all three scenarios and also presenting some of the results. The results are presented as justification for the assumptions made (including the apparant interchange of scenarios) but, at this stage of the chapter, they do not enhance the understanding of the modelling procedure.

Page 5-5, Figure 5-2. There seems to be a discrepancy between the results for the single-phase flow calculations performed using PANEL (figure 5-2, page 5-5) and those using BRAGFLO (figure 5-9, page 5-33). The PANEL results indicate that there is a considerable reduction in the cumulative volume of brine for the 1,000-year intrusion vectors, compared with the two-phase flow results shown in figure 5-1 (page 5-4). Results in later sections using BRAGFLO alone indicate the reverse situation, with the single-phase flow calculations giving higher cumulative volumes for the E2 scenario for the 1,000-year intrusion vectors. As the results are calculated using different models there is probably a simple explanation for the discrepancy, but it should be explained (one possibility is that the axes in figure 5-2 are incorrectly labelled by a factor of 10). Again, however, it would be less confusing if results were not introduced at this stage.

Section 5.2 Two-Phase Flow: BRAGFLO

A clear summary of the conceptual basis of the BRAGFLO model is given here and in Appendix A.

Section 5.2.2 Model description

Page 5-8, line 22. The nomenclature here should read k_{rl}, rather than k_{re}.

Pages 5-11 and 5-12. Figures 5-3 and 5-4. These figures present results for 1-D benchmark calculations using BRAGFLO, BOAST and TOUGH. The graphs of repository pressure vs. time and gas saturation vs. time for TOUGH each contain two lines, one labelled *all brine* and the other labelled *gas saturation*, but there is no explanation for this in the text. In the example, the gas generation rate is fixed at 2×10^{-7} kg/s/m^3 and results in a pressure rise from 0.1 to 12 MPa in a time period of 600 years.

Page 5-13. Figure 5-5. In contrast to the 1-D examples, the 2-D example has a lower gas generation rate (1.7×10^{-10} kg/s/m^3 for 525 years and 5.7×10^{-11} kg/s/m^3 for 185 years), but the pressure rises to 45 MPa in 550 years. The high pressure is attributed to the low value of porosity assigned to the panel, but the porosity value used in the 1-D example is not given for comparison. It would be more instructive to compare the 1-D and 2-D results using common parameters, so that the impact of the second dimension can be judged. The 2-D results for BRAGFLO and BOAST are very similar up to the time of intrusion, where the simulations stop. The section should be completed by presenting the TOUGH simulation results, as in the 1-D case. This will have the additional benefit of comparing results beyond the time of intrusion.

Pages 5-14 to 5-16. In section 5.2.2.4. Fundamental Equations, K is used for the absolute permeability, whereas it is defined in the nomenclature (section 5.2.2.1) using k.

Page 5-15, line 21. There is a change in nomenclature from C_{Nw} to C_{Gb}, before it is formally introduced on page 5-16, after equations 5-6 and 5-7.

Page 5-15, line 28. This talks about an *oil* phase of brine and dissolved gas, instead of a wetting phase.

Pages 5-16 to 5-17. Section 5.2.2.5 on wells is clearly presented. It would however be useful if computational times for problems using the simple well model and full spatial representation were included, together with discussion of whether the results for the full representation could be used to calibrate the simple model, in the absence of production history data.

Pages 5-18 to 5-21. The description of updating the time step (section 5.2.2.6) is not as clear, as it implies that the Δz^* are chosen to keep the time step constant. Presumably this occurs only if the maximum changes in the dependent variables Δz_m at any stage in the calculation are equal to the fixed input parameters Δz^*.

Section 5.2.3 Spatial and temporal grids

Pages 5-22 to 5-23. This section discusses the spatial representation of the region but there is no mention of temporal grids, as suggested in the title.

Page 5-23, line 2. Reference to figure 4-1 would be more helpful if it occurred in the first paragraph, so the reader may refer to it straight away.

Section 5.2.5 Results and discussion

Generally the discussion here is easier to understand than that given in the introductory sections of this chapter (see above comments). However, there is no discussion of how results are obtained for multiple intrusions into the panel. The introduction clearly states that multiple intrusions of this kind are included in the computational scenarios, and distribution functions for times of multiple intrusions are given in Chapter 5 of Volume 3. It is not made clear whether the results presented actually include multiple intrusion cases for scenario E2. If they do, it should be explained how such results are derived, as the BRAGFLO model is restricted by the use of cylindrical geometry to modelling single wells.

It was also noted earlier that the discussion of the comparison between single-phase and two-phase flow results is restricted to the BRAGFLO results. There is no explanation of the apparent decrease in cumulative volume obtained for the E1 and E2 scenarios for the 1,000 year intrusion vectors, using the single-phase flow model. PANEL (if indeed this decrease is not simply due to errors in labelling the axes of the figure).

Page 5-30, lines 29-30. The text should read "when gas is *not* generated".

Page 5-31, Figure 5-7. The left axis indicating E2 flows would appear to be a factor of ten low, by comparison with other results presented in this chapter.

Section 5.3 Repository Discharge (PANEL)

The lengthy mathematical description of the relatively simple flow model used in PANEL seems out of place here, particularly as the flow model is not used in the consequence analysis (other than for the results shown in figure 5-2). It would be easier to read if reduced to the level of description given for the waste mobilization and transport model (section 5.3.2), which is brief but clear.

Page 5-49, equation 5-12. $\zeta^{i}_{\omega_j}$ should be $\zeta^{i}_{\ell_j}$.

Page 5-49, line 23. The text refers to p_0 instead of P_0.

Page 5-55, line 37. An additional line is included, by mistake.

Page 5-56, lines 6-8. It would be helpful to include a description of the estimation procedure referred to, for calculation of the final discharge rates to the Culebra Dolomite and the contaminant concentrations.

Pages 5-57 and 5-58. Figure 5-23 and 5-24. The explanation of "Flow" included in both figures 5-23 and 5-24 is not necessary, as the graphs show normalized release rather than cumulative volume. In addition, the caption for figure 5-24 should read E1E2 Releases..., not E2 Releases...

Section 5.4 Summary of Results for Disturbed Performance

Pages 5-59 to 5-60. This section summarises the modelled scenarios once more, and here the description of the E2 scenario is reduced to a *single* intrusion of the waste panel, again with no discussion of how results for the multiple intrusion cases are inferred. It also states that the E1 scenario is assumed identical to the E2 scenario. It would be better to remind the reader why this assumption is made, or simply to say (as in earlier sections) that E2 results, in general, provide an upper limit on the E1 results.

Page 5-59. Numerous examples of poor grammar or lax editing, e.g. -

line 9. "was" should be "were".

line 14. "the" should be deleted.

line 16. "radionuclide" should be plural.

line 33. "was" should be "were".

3.2.6 Chapter 6. DISTURBED GROUNDWATER FLOW AND TRANSPORT

Chapter 6 introduces the conceptual model for disturbed groundwater flow and transport within the Culebra Dolomite. In general, the chapter gives a clear description of the details of the implementation of the model, but it lacks an overview of the links between the three aspects described in sections 6.3, 6.4 and 6.5.

Section 6.1 Conceptual Model

Here the authors clearly describe the requirement to generate realizations of transmissivity honouring point estimates at well locations, but do not make explicit the reasons why a geostatistical approach is an improvement on the zonal technique discussed. A discussion of the effect, on transport, of spatial variability on a range of scales would clarify this.

Section 6.2 Generation of Transmissivity Fields by Geostatistics

The first part of section 6.2.1, on the generation of conditional random fields, could also benefit from clarification. First, there is no discussion of the basic underlying assumption of geostatistics, that the measured values are regarded as realizations of a random field. Second, the description of the variogram is confusing. Third, the statement that each realization has the correct spatial structure of the true field overstates the functionality of the geostatistics approach. There is no reference to the implicit assumption that the measured values and the true field have the same spatial structure. Furthermore, although methods such as turning bands give a specified ensemble spatial structure over a number of realizations, it is not clear whether all individual realizations have the desired spatial structure.

The desirability of assessing conceptual model uncertainty is clearly presented in section 6.2.3. A rather fuller description of the four alternative models would be beneficial.

Section 6.3 Selection of Transmissivity Fields

In this section, the authors discuss the rationale for selecting transmissivity fields, and the calculation of travel times through the selected fields. An indication of the grid dependence of the results, and a discussion of why a 52 x 44 grid was used, would be of interest. The inclusion of all 60 selected transmissivity fields in an overview of the results is unnecessary.

Section 6.4 Fluid Flow Modeling with SECO2D

This section discusses fluid flow modelling with SECO2D. Undue emphasis has been placed on the general capabilities of the code (sections 6.4.1.2 - 6.4.1.7), to the detriment of the discussion of the results of the 1991 calculations. The section could also benefit from a clearer discussion of the links between the 1991 calculations with SECO2D and other aspects of the calculations discussed in this chapter. The transmissivity fields generated using the geostatistics discussed in section 6.3 are only mentioned very briefly. It would also be of interest if a clearer indication of the range of behaviour due to different transmissivity fields was discussed.

Section 6.5 Transport Modeling (STAFF2D)

This section gives a clear, detailed description of local flow and transport modelling. The section introduces a number of assumptions and limitations (section 6.5.2.3). It would be of interest if the authors discussed further the basis for the assumptions. It would also be beneficial if the discussion of the results in section 6.5.2.10 included a more complete description of different scenarios considered (e.g., scenario E2 is introduced without any discussion). This section, as for section 6.4, could also benefit from a fuller discussion of the links between the work discussed and the other aspects of the 1991 calculations. The link between regional and local velocity fields and grids could be made more explicit.

3.2.7 Chapter 7. CUTTINGS REMOVAL DURING DISTURBANCES

Generally, the work reported in this section is excessively detailed for the state of knowledge.

Section 7.1 General Considerations

This subsection begins by restricting attention to a hydrocarbon exploration well using current rotary drilling techniques. While this is clearly a good basis for consideration of the human intrusion scenario, it must be admitted that our ability to predict future human behaviour is minimal. Thus, it cannot be said why a borehole would be drilled, or how it would be drilled. The introduction of sophisticated modelling cannot disguise these basic facts. In order to make a fair assessment of the potential risks arising from such intrusions, it is necessary to estimate both their frequency and scale. The frequency is already estimated (as a probability distribution) directly from expert judgement. The scale of intrusion should be similarly estimated.

Section 7.2 Analysis

Given the comments of the previous section, we consider the analysis presented in sections 7.2.1 and 7.2.2 unnecessary (and have not reviewed it in detail).

In section 7.2.3, the Bateman equations for radioactive decay and ingrowth are stated and solved for the particular case of a 5-member chain. The explicit expansion of the solution as presented is not very helpful, since it is not an efficient way of actually calculating inventories against time. Moreover, it is not necessary to solve the equations in terms of numbers of atoms and then perform messy conversions.

The Bateman equations in terms of activity are simply

$$\frac{dN_i}{dt} = -\lambda_i N_i + \lambda_i N_{i-1}.$$

For given initial inventories $N_i^{(0)}$ [arbitrary units of activity], the solution can be written

$$N_i(t) = \sum_{j=1}^{i} a_{i,j} e^{-\lambda_j t},$$

where the coefficients $a_{i,j}$ are defined by the recurrence relations

$$\begin{aligned} a_{i,i} &= N_i^{(0)} - \sum_{j=1}^{i-1} a_{i,j}, \\ a_{i,j} &= \frac{\lambda_i}{\lambda_i - \lambda_j} a_{i-1,j} \qquad i > j. \end{aligned}$$

This is valid for any length chain and leads directly to an efficient and compact numerical algorithm.

Section 7.3 Code Description

No comments, except that that above solution method should be used for solving the Bateman equations.

Section 7.4 Drilling Parameters

As commented above, the scale of intrusions should be directly estimated.

Section 7.5 Results and Discussion

Why not show the full CCDF?

Pages 7-17 to 7-18. Tables 7.1 and 7.2 both have the 7000-year column incorrectly labelled.

3.3 Volume 3: Reference Data

Although we have some concerns over the details of the methodology used to obtain the reference data, our general impression of the data gathering exercise is that it is very systematic and comprehensive.

On the other hand, in the chapters describing the treatment of individual parameters, it is not always clear whether or not the parameter was sampled and, in many cases, it is not clear how the parameter entered into the performance assessment. An additional point of confusion is that often the performance assessment calculations used different values for parameters than those appearing in the summary tables and boxes. In this case, we presume that the summary values were used solely for the purpose of sensitivity analyses (reported in Volume 4). The distinction should be made clearly.

In addition, the detailed discussions contained for many of the parameters in some cases shed little light on the basis for the median and range used in the assessment. More attention needs to be paid to the clarity of these discussions, and to the link between the discussions and the final summary information presented in the "boxes" and tables.

3.3.1 Chapter 1. INTRODUCTION

Section 1.1 Purpose and Organization of the Report

No comments.

Section 1.2 Conventions

Page 1-2, line 48. Equation 1.2-1 has a missing x after the integral.

Page 1-2, line 55. It is noted that $\bar{x}$ is used to denote both the true mean of a distribution and its sample mean. These two quantities should be clearly distinguished.

Page 1-3, line 11. It is stated that normal distributions are truncated at the 1st and 99th percentiles. Given that the regulation is concerned with the 0.1 percentile of the distribution of consequences, this cannot be justified on mathematical grounds. Presumably the justification is that the normal distribution is only a convenient way of representing some types of uncertainty and so its tails should not be included.

Page 1-3 to 1-4. Section 1.2.3 Constructed Distribution (Empirical). There seems to be a confusion between uncertainty and variability. Repeated measurements of a variable may give different results for a number of reasons (spatial variability, experimental error, lack of control of experimental environment, etc.), and this will lead to an uncertainty as to the true value of this parameter. However, it is only when the parameter varies stochastically that the observations can be taken as a direct indication of uncertainty. In other cases, the varying measurements require careful consideration by experts to determine a reasonable characterisation of uncertainty.

Some of the confusion in terminology may result from the more or less direct translation of reactor PRA methodology and terminology to waste disposal system performance assessment. It is not sensible to apply directly all of the reactor PRA concepts to natural geological systems. For example, large parts of a reactor can be treated as an engineered system comprised of components of more or less constant (but imprecisely known) properties. This same thinking is not easily applied to disposal systems.

Page 1-12, lines 12-18. The neglect of correlations has a potentially significant effect on calculated results and should not be dismissed so lightly. It is often suggested that correlations be included by reordering samples (produced by Latin Hypercube Sampling or straighforward Monte Carlo sampling) to satisfy a specified correlation matrix. This approach is however much less satisfactory than an approach based on identifying the reasons for correlations (in terms of functional relationships) and reparameterising accordingly. In the later parts of this volume, correlations are given as rank-correlation coefficients, presumably indicating an intention to use a reordering method.

Section 1.2.7 to some extent addresses the issue raised above about Empirical distributions. We have not yet been able to review it in detail, but it is at first sight a useful contribution.

Section 1.3 Background on Selecting Parameter Distribution

The method for selecting appropriate parameter distributions is well described.

Page 1-18, lines 33-38. The procedure in step 3 again raises the issue of confusing variability and uncertainty.

Page 1-20, line 49. Some words have been mistakenly repeated.

Section 1.4 Performance-Assessment Methodology

The material in this section is very similar to that contained in Volume 1, Chapter 3 and in Volume 2, Chapter 3. It is not reviewed in detail here.

Page 1-27, line 14. There is a missing symbol between the words "of" and "into", presumably $\mathcal{S}$.

Page 1-27, line 19. The reference to S seems incorrect; this should presumably be replaced by $\mathcal{S}$.

Page 1-29, lines 4-6. The reference to "variable uncertainty" again seems to confuse the distinction between variability and uncertainty. Uncertainty can also arise owing to spatial (and/or temporal) variability of the parameter (e.g., limited spatial characterisation of the disposal system), in which case the uncertainty is more appropriately referred to as "variability". In addition, because of the difficulty of using the expression, "variable variability", we would argue for the substitution of the word "parameter" for "variable" throughout the text.

Section 1.5 Background on WIPP

Page 1-36, lines 28-31. The explanation provided for the repository location is meaningless. It is perhaps not the purpose of this report to explain why the repository has been located where it has been (both horizontally and vertically) but, because it is a logical question to ask, this information would be extremely useful for the reader.

3.3.2 Chapter 2. GEOLOGIC BARRIERS

Section 2.1 Areal Extent of Geologic Barriers

No comments.

Section 2.2 Stratigraphy at the WIPP

Page 2-5, line 12. The existence of 45 siliceous and sulphate units within the Salado Formation is important information regarding the formation, and should be given in Chapter 1 of Volume 1.

Page 2-5, line 13. What has lithostatic and hydrostatic pressure to do with stratigraphy? This information should be presented elsewhere, perhaps under hydrologic parameters.

Page 2-7, Figure 2.2-2. This the first figure in Volumes 1 to 3 where the true spatial relationships between the disposal vaults and the marker beds are displayed. This information should have been presented in simplified form at the beginning of Volume 1. Figure 2.2-3 (page 2-8) is even more useful in this respect; information contained in this figure is needed in order to understand the development of the base case scenario.

Page 2-10. Figure 2.2-5. This figure contains too much information, and little of it is self-explanatory or explained in the caption. How were the various curves and data points shown derived?

Section 2.3 Hydrologic Parameters for Halite and Polyhalite within Salado Formation

Section 2.3.3 Dispersivity

Page 2-25. Figure 2.3-5. Values for transverse dispersivity shown in the figure do not reflect the discussion in the text or in the summary box. It is stated on page 2-21 that transverse dispersivity was assumed to be a factor 10 smaller than longitudinal dispersivity. Also, the bottom axis of the figure has not been defined.

Section 2.3.5 Permeability

Page 2-28. Figure 2.3-7. The figure has been mislabelled. It should be figure 2.3-6.

Page 2-29. The entire discussion on rank correlation between halite and anhydrite permeability in the Salado Formation is nonsensical because there are only two sets of paired data. The correlation coefficient finally settled on (0.8) is justified by a desire not to contradict the conceptual model. This is a backwards argument - the conceptual model should account for the range of parameters, not vica versa.

Page 2-30, line 45. Figure 2.3-6 does not show rank correlations; the referenced figure does not exist in the report.

Page 2-31, line 24. The text states that "Often the PA Division does not model the disturbed zone when it is conservative to do so..." This wording is ambiguous and should be clarified. The text could be read to mean that sometimes, even though it would be conservative to model the disturbed zone, the PA Division does not do so (i.e., a nonconservative choice is made).

Section 2.3.6 Pore pressure at repository level in halite

Page 2-33, Figure 2.3-9. The figure is wrong: the units along the right-hand axis (probability density) are undefined; the bottom axis shows pressure in MPa, and not x 10 Mpa as indicated.

Page 2-34, line 20. It would be useful if a reference were provided to the section of the text that discusses anhydrite pore pressures (i.e., section 2.4.6, pages 2-61 to 2-62).

Section 2.3.8 Specific storage

Page 2-43, line 51. Some words are missing (perhaps just "in" at the end of the line?).

Section 2.4 Hydrologic Parameters for Anhydrite Layers within Salado Formation

2.4.4 Partition coefficients and retardations

Page 2-56, Table 2.4-2. The table is unclear and would be easy to take out of context. It should be indicated that the values provided are maximum partition coefficients for use in sensitivity analyses, and that coefficients of zero were assumed for the actual performance assessment calculations.

Section 2.4.6 Pore pressure at repository level in anhydrite

Page 2-62, line 20. The reference to figure 2.4-6 is incorrect. The referenced figure does not exist in the report.

Page 2-62, lines 27-29. The interpretation of the data provided in figure 2.4-8 is nonsensical. The data do not fit the curves. The modelled asymptotic value of pore pressure (10 MPa) is concluded to be significantly less than many of the measured data. The median value selected for the performance assessment of 13 MPa supports this view.

Section 2.5 Mechanical Parameters for Materials in Salado Formation

No comments because this section of the text is evidently still to be written. Is the implication of no text that the referenced parameters are unimportant to or not used in the performance assessment?

Section 2.6 Parameters for Culebra Dolomite Member of Rustler Formation

Section 2.6.2 Dispersivity

Page 2-78. Figure 2.6-5. The bottom axis of the figure is defined as porosity. It should be longitudinal dispersivity.

Page 2-78. Figure 2.6-6. The figure is unclear. It seems to show distributions used for sensitivity analyses only. not those used in the performance assessment calculations (box on page 2-77). As suggested in the box and at the bottom of page 2-77. these seem to differ slightly. Also, the bottom axis of the figure has not been defined.

Section 2.6.7 Tortuosity

Page 2-94. line 11. The reference to figure 2.6-9 is incorrect. It should be figure 2.6-15.

3.3.3 Chapter 3. ENGINEERED BARRIERS AND SOURCE TERM

Section 3.1 Dimensions of Underground Facility

Page 3-4. Figure 3.1.2. The meaning of the tiny black dot is unclear. Is this the location of the intrusion borehole for transport calculations?

Page 3-10. line 10. This is the first use of the abbreviation SWB, and it should be defined.

Section 3.2 Parameters for Backfill Outside Disposal Region

Section 3.2.1 Description of the reference design for backfill

Page 3-16, line 18. The 200-year period is cited but what are the consequences if the time required for consolidation is much shorter (e.g., 20 years) or much longer (e.g.. 2000 years)? Also, the 200-year figure cited in line 18 seems to contradict the 100-year figure cited in line 31 on this page and in line 4 on page 3-17.

Page 3-17, line 25. As noted in Chapter 5 of Volume 2, the conditions and factors under which it will be determined that a clay backfill will be "necessary" are not well documented. Will clay backfill be considered as a redundant barrier based on its own desirable properties or will it be included because of expected or demonstrated poor performance of consolidated salt? These questions should be answered in at least a brief manner.

Page 3-19, Figure 3.2-3. The scale on the uppermost figure in this group is misleading. as the separation of MB 139 from the waste vault (and its thickness) is much greater than it should be. The provision of precise numbers and a scale implies that the diagram is to scale.

Section 3.2.4 Partition coefficients for salt backfill

Page 3-27. line 38. Table 3.2-2 is solely for "salt and trace amounts of clay", wording that would imply an unintended, coincidental connection between these partition coefficients and those for anhydrite. A possible word change on line 38 to "approximately the same order of magnitude as anhydrite" might make the same point without the unintended connection between clayey salt and anhydrite.

Section 3.3 Parameters for Contaminants Independent of Waste Form

Page 3-30, Figures 3.3-1 and 3.3-2. The left-hand axis of both of these figures is visually daunting because of all the zeros. The axis should be relabelled (e.g.. using exponents) to improve readability. Also, the term "scaled" waste is used. but not defined until later in the chapter. The term is unclear.

Pages 3-31 to 3-38. Table 3.3-1. Is this lengthy table needed? All of the information is repeated in later tables, and without the apparent errors that have crept into this table. There are at least two errors. First, the design inventory for Am-241 in CH-waste is given as four times less than the anticipated inventory (page 3-31). Later in the chapter (figure 3.3-3, tables 3.3-4/5), the anticipated inventory for CH-waste is given as 665,000 Ci. Second, the anticipated inventory of Pu-239 in RH-waste is given as being greater than the design inventory (page 3-35). Later in the chapter (table 3.3-7), the anticipated inventory is given as 1165 Ci.

Section 3.3.1 Inventory of radionuclides in contact-handled waste

Page 3-45. Table 3.3-4. It is unclear what the information presented on the line entitled "System Total" represents, or how the data were derived. A comparison with figure 3.3-5, suggests that this line should probably have been labelled "Total Design Inventory", and that all values are one column removed from their correct position.

Page 3-46. Table 3.3-5 (and Page 3-50. Table 3.3-6). The column entitled "Waste Unit Factor" should be defined. The term is not defined until much later in the chapter (page 3-61).

Section 3.3.2 Inventory of remotely handled waste

Page 3-50. Table 3.3-6. An additional column entitled "Total" should be added to this table, to facilitate comparison with the "PA Calculations Design 1990" column. Total design inventory seems to be about twice as great as that used in the PA calculations, implying that the PA calculations are not conservative. In fact, the inventory value used for the PA calculations (1,700,000 Ci) is also less than the total of stored and projected waste arising shown in table 3.3-7 (2,600,000 Ci).

Section 3.3.3 Radionuclide chains and half-lives

Page 3-53, line 40. Contrary to what is stated in the text, figure 3.3-7 (page 3-59) indicates that the total normalized activity is about 3% of the EPA limit (at 10,000 years after repository closure). The normalized activity of Ra-226 is less than 2% of the EPA limit. The most interesting aspect of the figure, however, is that the normalized activities rise steeply with time after about 2000 years, and are still rising steeply at 10,000 years. This begs the question of when does the total activity level off, and at just what level? Also, regulatory timeframe notwithstanding, how does the temporal variation in activity after 10,000 years affect longer-term performance?

Section 3.3.5 Solubility

See the detailed comments on section 5.2 of Volume 1. In addition, it would be helpful here if the information presented in figure 3.3-8 (page 3-63) was also presented in the pdf and cdf form used for other parameters in this volume, as well as in the "box" form used for other parameters.

Section 3.3.7 Molecular diffusion coefficient

Page 3-69. Figure 3.3-10. The left- and right-hand axes should be reversed for consistency with other such figures presented in this volume.

Sections 3.3.8-3.3.10 Gas production

Page 3-71, line 20; Page 3-72, Figure 3.3-12; Page 3-80, line 20; Page 3-81, Figure 3.3-16. The presentation is unclear. The information presented on relative gas production rates from corrosion and from microbilogical degradation under humid conditions, should say relative to what (i.e., to the inundated rate). Also, why is a relative rate used, as opposed to an absolute rate? The former is calculated from the latter, and the latter would seem to convey more information.

Page 3-73, lines 24-25; Page 3-74, lines 17-18; Page 3-82, lines 13-20; Page 3-85. The units used here are expressed in terms of years, whereas elsewhere in these sections they are expressed in terms of seconds, making the discussion difficult to follow. The use of years on page 3-85 in section 3.3-10 is particularly unfortunate, because this makes cross comparison with the information presented in sections 3.3-8 and 3.3-9 unnecessarily difficult. Perhaps more importantly, it would be useful to present information on total (for the entire repository, or for one waste panel) estimated gas production rates by each of the three mechanisms in these sections, because the units used are different in the three sections.

Section 3.4 Parameters for Unmodified Waste Form Including Containers

Page 3-89, Table 3.4-2, line 24. The word "greater" should presumably be "less".

Section 3.4.1 Composition of CH-TRU contaminated trash (non-radionuclide/non-RCRA inventory)

The tables and boxes in this section should be thoroughly examined to improve their clarity and accuracy. Several examples are given below.

Page 3-91. The information presented in the summary boxes is incorrect. On lines 6-22, all of the values presented are an order of magnitude too great. In addition, the median values do not agree with those provided in table 3.4-6 (page 3-99). In this table, the volume fraction of combustibles is given as 0.403 (not 0.384), and the volume fraction of metals and glass is given as 0.368 (not 0.376). Finally, the reference to table 3.4-10 seems incorrect; table 3.4-6 is the correct table to reference. On lines 32 and 42, the reference to figure 3.1-3 seems incorrect. It is unclear what the source of the data is for the volumes of backfill and air in the repository.

Page 3-103, Table 3.4-7. This table also contains many errors, making it difficult to understand. On line 9, the units should be expressed as Gg (not kg). The summed mass of PVC (on line 38) does not agree with the two values on lines 21 and 28; the source of the error is unclear. On line 36, the total volume of waste is incorrect (too low) by a factor of one million.

Page 3-104, Table 3.4-8. Yet another problematical table. The derivation of the values is unclear and is not explained in the text or in the caption. In addition, on line 33, the total number of SWBs is incorrect (too low) by a factor of one million.

Page 3-105. Table 3.4-9. Yet another example of a table carelessly put together. On line 7, the units of volume are missing (i.e., m^3). On line 29, the reference to table 3.4-3 is incorrect; the correct reference is table 3.4-4.

Page 3-107, lines 13-21. The discussion presented here, on estimation of inventory accessed by intrusion boreholes, and on information presented in figure 3.4-11 is totally unclear and confusing. The logic is absent.

Page 3-111, lines 26-30. The discussion does not distinguish clearly the differences between the information contained in columns 7 and 8 of table 3.4-10 (page 3-106). Neither does the table provide sufficient information to understand clearly how the information contained in these two columns was derived.

Page 3-112, Table 3.4-12, line 25. The data are not in the correct row. Do these data refer to the combination of polyethylene and PVC?

Section 3.4.4 Capillary pressure and relative permeability

Page 3-121, lines 5-7. The median value for the threshold displacement pressure and the lower limit of the range are reversed. In addition, the discussion indicates that a pressure of zero was assumed in the performance assessment calculations. How does this assumption correspond with the summary presentation in the box on this page (the distinction should be noted in the box)?

Section 3.4.5 Drilling erosion parameters

Page 3-128, line 30. The reference to figure 4.2-6 is incorrect. This figure shows the probability of drilling a borehole of specified diameters (not shear strengths). The referenced figure does not exist in the report.

Section 3.4.6 Partition coefficients for clays in salt backfill

Page 3-129. Same comments as given under section 3.2.4 (page 3-27).

Section 3.4.7 Permeability

Page 3-131. There is no discussion of the permeability of sludges in the drums. The mean permeability of a drum cannot be derived without this information.

Page 3-131, line 8. The *exponent* is wrong by ten.

Page 3-132, line 22. The reference to figure 3.4-8 is incorrect; the reference should be to figure 3.4-9.

Section 3.4.8 Porosity

Page 3-135. Despite (or perhaps because of) the ten pages of text that follow in this section, the derivation of porosities in the summary box remains unclear. Were they simply drawn from the paper of Butcher et al. (1991). If so, what is the point of the discussion?

Page 3-135, lines 7-8 (and in summary table 3.4-1, page 3-88). The median and lower range values for porosity of combustibles have been reversed.

Page 3-144, line 9. The porosity estimates referred to are not shown "above"; reference should be made here instead to figure 3.4-10 on page 3-146.

Page 3-144, line 21. The reference to table 3.4-9 is incorrect. The porosity ranges referred to are presumably those from the summary box on page 3-135.

Section 3.5 Parameters for Salt-Packed Waste Form

No comments.

3.3.4 Chapter 4. PARAMETERS OF GLOBAL MATERIALS AND AGENTS ACTING ON DISPOSAL SYSTEM

Section 4.1 Fluid Properties

Section 4.1.1 Salado brine

Page 4-5, lines 22 and 27. Line 22 indicates that the range of the density measurements on six samples of Salado brine was 1224 -1249 kg/m^3, and line 27 gives a range of 1208-1255 kg/m^3 (used in the assessment). Where does the difference arise?

Section 4.1.4 Hydrogen gas

Page 4-15, lines 21-23. Almost the same information has been unnecessarily repeated one line later (on page 4-16, lines 4-5).

Pages 4-16 to 4-20. The discussion on hydrogen gas density is unclear. It does not appear that the information has been used to derive the density values that appear in the box on page 4-15.

Page 4-21, lines 25 and 27. The information on hydrogen viscosity is unnecessarily repeated on these two lines.

Page 4-25, Figure 4.1-6. The right-hand axis (mole fraction for hydrogen solubility in brine) is incorrect: the numbers provided do not agree with those tabulated in the boxes on pages 4-23 and 4-25.

Section 4.1.5 Drilling mud properties

Pages 4-28 to 4-31. It is unclear how the discussion relates to the values of viscosity and yield stress given in the box on page 4-26. Page 4-31, lines 16-23 indicate that the values on page 4-26 were derived from the work of Pace (1990). What is the point of all the discussion?

Page 4-28, line 7. The referenced figure (1.6-2) does not exist in the report.

Page 4-30, Figure 4.1-8. The figure is totally unclear. It contains too much information and little of it is self-explanatory or is properly explained in the text. The caption also provides no useful information.

Page 4-32, Figure 4.1-10. The units of pressure (Pa?) along the bottom axis are missing.

Section 4.2 Human-Intrusion Borehole

Section 4.2.1 Borehole fill properties

Page 4-39, Figure 4.2-3. The figure is incorrect. The units on the bottom axis are wrong by many orders of magnitude. The median value of permeability does not agree with that contained in the box on page 4-35. In addition, the left- and right-hand axes should be reversed for consistency with other figures presented in this volume.

Section 4.2.2 Drilling characteristics

Page 4-43, Figure 4.2-6. The figure seems to be incorrect: the range of diameters given in the figure (approximately 0.29-0.52 m) does not agree with that presented in the box on page 4-42 (0.27-0.44 m).

Page 4-44, lines 29 and 34. The two references to figure 4.2-6 are incorrect.

Section 4.3 Parameters for Castile Formation Brine Reservoir

Page 4-50, Figure 4.3-1. What is the "Disturbed Zone" of Borns et al. (1983)? This should be defined.

Section 4.3.1 Analytic brine reservoir model

Page 4-57, line 49. The value of 20 m^3/Pa is incorrect; the assumed maximum bulk storativity discussed elsewhere is 2 m^3/Pa.

Page 4-59. Figure 4.3-5. The figure is unclear: most of it (e.g., the various Qs) is undefined, either in the text or in the caption.

Section 4.3.2 Numerical brine reservoir model

Page 4-60. It is unclear how the permeabilities of intact and fracture matrix in the boxes were derived. They are not in agreement with the values quoted in summary table 4.2-1 (page 4-33), in contrast to suggestions in the text. Also, on page 4-61, line 2, it is indicated that a permeability of zero was assigned to the intact Castile matrix.

Page 4-61, line 6. Cross references within section 4.3.2 to sections 4.3 and 4.3.2 [sic] are confusing and seem incorrect.

Page 4-64, line 49. This line has been repeated by mistake at the top of page 4-65.

Section 4.4 Climate Variability and Culebra Member Recharge

Page 4-66, Equation 4.4-1. The various parameters in this equation are not defined until page 4-71. The equation can be safely omitted here.

Page 4-77. Equation 4.4-2. Some of the parameters in the equation need to be defined (e.g., the h's).

Page 4-78, lines 13 and 20. The recharge amplitude factor has been defined earlier as "Am" (not "r"). Also, the lower bound is Am=0 (not 1).

3.3.5 Chapter 5. PARAMETERS FOR SCENARIO PROBABILITY MODELS

Section 5.1 Area of Brine Reservoirs

Section 5.1.1 Area of Castile brine reservoir below WIPP disposal area

Page 5-3, line 35. "Anticlinal" is misspelled.

Page 5-3, line 41. The reference to figure 5.1-1 is incorrect. The reference should be to figure 5.1-3.

Page 5-3, line 47. The reference to figure 5.1-2 seems incorrect. The reference should be to figure 5.1-3.

Page 5-3. line 48. The text here provides a value of 45%, whereas the table referred to (5.1-1. page 5-6) suggests that a value of 40% would be more appropriate (at -200 m maximum elevation).

Page 5-4 and 5-5. Figures 5.1-2 and 5.1-3. These figures are meant to be inspected together, but this is extremely difficult because they are presented at different scales and figure 5.1-2 is in terms of depth whereas figure 5.1-3 is in terms of elevation (below sea level). This distinction alone was difficult enough to decipher. In addition, units are lacking on both figures and the axes are undefined.

Page 5-6. Table 5.1-1. The table caption and column headings refer to maximum depths, whereas the values provided seem to be in terms of elevation (below sea level).

Page 5-12. line 9. Grammar. "Were" should be "was".

Page 5-13. lines 25-26. Vital information on the geological controls on brine reservoir locations is provided. This information should be considered much earlier in the discussion.

Section 5.1.2 Location of intrusion

Page 5-15. lines 4-8. There is confusion between the location of the intrusion borehole for transport modelling, and the location of the borehole for input to the source term. For example, the number of boreholes and the activity levels of waste were sampled parameters, yet the single borehole location for transport does not even fall within a disposal area of the repository. This distinction and the reason for it should be clarified throughout the text of all volumes.

In addition, the reference to figure 3.1-2 is unhelpful, as the borehole location is not clearly shown. The only evident indication in this figure of a possible intrusion location is a tiny black dot with defined coordinates. If this is the assumed location, say so; if not, indicate clearly where the intrusion occurs.

Section 5.2 Human-Intrusion Probability (Drilling) Models

Section 5.2.1 Drilling rate function

This section presents the argument for the range of values used in the assessment for lambda, the drilling rate function. The range was assumed to extend from 0 to the EPA Appendix B recommended upper bound, with a uniform distribution. However, the justification for using (nonconservative) values less than the EPA upper bound is lacking. In the absence of information to support a lower value of λ, it would be more conservative to assume a constant drilling rate function equal to the EPA upper bound.

3.3.6 Chapter 6. SUMMARY OF PARAMETERS SAMPLED IN 1991

Page 6-6, line 21. The number 13 is incorrect. It should be 15.

Page 6-6, line 39. It is unclear from the information presented here what has been done in the performance assessment with respect to sampling on solubility limits. The correlation between solubilities of different radionuclides was referred to in Chapter 3, but no evidence for the correlation was presented, nor was sufficient detail provided to justify the summary information provided in this list.

4 References

ERC 2091. Sixth Circuit. 1986. Ohio v. EPA, U.S. Court of Appeals, Sixth Circuit. 23 ERC 2091 - 23 ERC 2097.

Bonano, E.J. and Wahi, K.K.. 1990. Use of performance assessment in assessing compliance with the containment requirements in 40 CFR Part 191. NUREG/CR-5521, SAND90-0127.

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OECD/NEA. 1992. Systematic approaches to scenario development (A report of the NEA Working Group on the identification and selection of scenarios for performance assessment of radioactive waste disposal). OECD/NEA, Paris.

Robinson, P.. Roberts, D. and Sinclair, J.. 1991. Comparative study of sampling methods. Presentation at Twelfth Meeting of NEA/PSAG (Paris, 12th-14th November 1991).

IG3091-1
Addendum 1

REVIEW OF THE 1991 WIPP ASSESSMENT (SAND91-0893)

ADDITIONAL COMMENTS

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31st March 1992

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Addendum 1

Review of the 1991 WIPP Assessment (SAND91-0893) Additional Comments

Volume 3: Reference Data

Section 2.6.10 Partition coefficients and retardations

Partition coefficients are vitally important to the performance assessment, and the underlying basis for selected values should, therefore, be presented clearly and in sufficient detail for an independent reviewer to assess the effectiveness of the selection process. Such a presentation has not been achieved in this section.

The main criticism lies in the absence of any experimental data included in support of recommended values by any of the three experts. Such data, when available, should be directly accessible to the reviewer, who should not have to rely on examining source references. For example, the statement is made on Page 2-102 (2nd paragraph), that the recommended K_d cdfs are "considered to be realistic in light of available data", yet these available data are not provided, either in support of the recommended K_d values, or to illustrate the paucity of data overall. Some background is discussed in Novak's September 4, 1991 memorandum, but this level of detail is insufficient, particularly when recommended cdf values are "subjective estimates only". At least the recommendations which Siegel provides include references and some detail on his selection process. It is ironic, therefore, that his values have been excluded from the "panel's" selected partition coefficients.

The basis for excluding Siegel's recommendations is not clear. It appears from the text (Page 2-108) that exclusion is due to non-agreement of Siegel's values with those of the other two experts, Dosch and Novak, and/or the use of a different methodology for derivation of the cdfs. We agree that, in such circumstances, combination/aggregation of all three sets of data is not recommended; however, the decision that 'the majority should rule' as a means of selecting final values (implied by the text) requires greater justification than is provided here.

It is unclear why Siegel's approach is any less valid than that of the other two experts, especially when the available data are obviously sparse. In addition, the upper bounds on K_d provided for each element by Dosch and Novak suggests that these K_d values reflect sorption processes which include precipitation, in which case the assessment is not valid for the true thermodynamic K_d parameter. Is this what is meant by Novak in his memorandum (September 4, 1991) that the "K_d model . . . may have limited applicability to the WIPP Culebra system" and that the resultant cdfs could be rendered "inadequate"? If not, what do these statements mean? It does suggest at least that Novak has some reservations with the selection process.

Additional comments

1. For Ra and Pb, the cdfs presented in Novak's memorandum (Tables 1 and 2) for rock matrix and fractures do not differ by a factor of 10 in the case of the other elements. However, in tables 2.6 8 and 2.6 9, the factor of 10 difference is maintained. This inconsistency should be corrected.

2. A clear statement (or table) is recommended in this section to define reference conditions, i.e., water composition, pH, Eh conditions (values or ranges of values) for which cdfs are being provided.

3. A clear statement of the sorption processes for which K_d is being estimated (K_d model?) should be included early in this section.

4. Page 2-102, end of 3rd paragraph: the statement "more thorough description of Novak's values is provided in Appendix A of this report" is misleading, as no additional details are provided other than the actual recommended values

5. The way in which the recommendations of Dosch and Novak were "averaged" is not specified and should be discussed.

6. Page A-102 (Novak's September 4 memorandum), 2nd paragraph: some detail or discussion of the basis for selecting chemical analogues is advisable, when discussing sorption processes, possible complexation, and sorbing species.

Conclusion

We agree with the statements that cdfs for K_ds "do not substitute for actual data", and that "additional studies should be performed to quantify the potential for radionuclide retardation". We recommend that efforts be made during experimentation to identify and distinguish precipitation from sorption (isotherm measurements).

In addition, because of the uncertainty in the meaningfulness of both the process for deriving cdfs and the cdfs themselves, we suggest that this area would benefit from the use of independent expertise outside the WIPP performance assessment program.

1992 PERFORMANCE ASSESSMENT REPORT

COMMENTS AND RESPONSES

EPA REVIEW OF VOLUMES 4-5

Department of Energy
Carlsbad Area Office
P. O. Box 3090
Carlsbad, New Mexico 88221

JAN 24 1996

Larry Weinstock
Office of Radiation and Indoor Air
U.S. Environmental Protection Agency
401 M. Street SW
Washington, DC 20460

M

Dear Mr. Weinstock:

Enclosed is the Carlsbad Area Office's (CAO) response to your comments on Volumes 4 and 5 of the 1992 Performance Assessment (PA). Hopefully, these responses will assist you with any questions that may not have been fully answered during the Technical Exchanges. The CAO hopes that you will find these responses helpful in your understanding of the specifics of the 1992 PA.

We would like to extend our gratitude to you for your comments on the 1992 PA. Your comments will assist us in developing a better customer oriented product. The responses to your comments on the 1992 PA may not necessarily reflect the exact approach that the CAO will embark upon in the upcoming performance assessment for the Waste Isolation Pilot Plant Compliance Application, because the CAO is in the process of finalizing a management plan for the structure of the Certification Compliance Application.

If you have any questions regarding this response, please contact George T. Basabilvazo of my staff at (505)-234-7488.

Sincerely,

James A. Mewhinney

Dr. James A. Mewhinney
Compliance Team Leader
Office of Regulatory Compliance

Enclosure

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M. McFadden, CAO
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M. Irwin, SNL, MS #1341

RESPONSES TO COMMENTS FROM THE U.S. ENVIRONMENTAL PROTECTION AGENCY ON VOLUMES 4 AND 5 OF THE PRELIMINARY PERFORMANCE ASSESSMENT FOR THE WASTE ISOLATION PILOT PLANT SAND92-0700/4 AND SAND92-0700/5

EPA Comment 1. Scenarios

While much work has gone into the identification and screening of scenarios, we agree with the document that current treatment of scenarios is incomplete. As presented in the 1992 Performance Report (Volume 2, Figure 4-1) only four scenario combinations were modeled, and more scenario combinations need to be considered and modeled for 10,000 years.

The "base case" or the undisturbed scenario and disruptive scenarios need to include events outside the Land Withdrawal Boundary that may effect repository performance. For example, the performance assessment should consider effects of human initiated activities such as oil, gas, and injection wells around the Land Withdrawal Boundary. It is possible that such activities may increase releases from the repository, even if no borehole penetrates the repository itself. For example, your staff has postulated that an injection well south of the Land Withdrawal Boundary could affect the Culebra. Other potential effects of human-initiated events could include local dissolution of salt, as has happened in part of the Delaware Basin in Texas. In addition, the cumulative effect of multiple intrusions on radioactivity releases due to intrusions and base case releases need to be summed.

The 1992 PA states that effects of potash mining (i.e., subsidence) will be considered in future analyses (scenario TS). We agree that the effects of potash mining should be considered; indeed, all potential effects of the mining should be explored, such as connections between boreholes. This should include the effects of potash mining both within and around the Land Withdrawal Boundary.

We also agree that you need to consider the effects of water withdrawal wells (scenario E3) in the Culebra. The 1992 PA document provides very little information on this scenario, but we would suggest that the water quality is not as an important consideration for the purposes of the containment requirements (191.13) as it is for individual and groundwater protection requirement analyses. This is because it is possible for Culebra water to be used for non-drinking purposes, and this use may (or may not) have an effect on flow in the Culebra. Also, if a well penetrates a contaminated plume, then more radioactivity could be brought to the accessible environment. We do not know how significant the impact of these scenarios will be, but we believe that they should be examined. Additional scenario topics are addressed in the attachment.

Response. *The CAO's current approach to scenario development is described in two documents that have been published more recently than the report on the 1992 PA:*

(1) The Position Paper that was prepared during the Systems Prioritization Method (SPM), entitled: "Scenario Development for Postclosure Performance Assessments of the WIPP: Input to Systems Prioritization and Project Technical Baseline"; and

(2) The draft Compliance Certification Application (DCCA) (DOE, 1995).

These documents present a systematic reevaluation of scenarios since the 1992 PA exercise in order to be as complete as possible. The CAO assumes that future exploratory drilling (within or outside the controlled area) that does not intersect the repository can be eliminated from performance assessments on the basis of low consequence. Thus the effects of a well penetrating a contaminated plume outside the controlled area can be eliminated on the basis of low consequence. Work is underway to evaluate the adequacy of this assumption and additional material in support of low probability and low consequence screening decisions will be included in future drafts of the Position Paper and the final compliance certification package.

EPA Comment 2. BRAGFLO and SANCHO Relationship

Salt creep-closure of the repository rooms and the interactions of the waste and brine are topics that need to be more closely examined. The repository horizon rock mass is deformable and permeable, but the current modeling is not able to take this into account. Separate codes look at deformation and fluid flow. It is our understanding that SANCHO calculates creep closure while BRAGFLO separately calculates fluid flow. SANCHO does not consider the rock mass to be a permeable, porous solid, while BRAGFLO does not consider the rock mass to be deformable. We believe that the physical coupling between fluid flow and deformation needs to be improved because the current method by which data from SANCHO is used in BRAGFLO is suspect enough to cast doubt on the results from the two codes. We suggest that the incompatibility be investigated. You may want to examine the possibility of modeling fluid flow and pressure effects (deformation) simultaneously within one code or in two codes which pass information to one another.

Response. *Sandia National Laboratories (SNL) has completed a detailed study of coupled mechanical deformation-fluid flow codes for performance assessment (PA) since the 1992 PA. This study evaluated the strength of the coupling between brine and/or gas flow into or out of the disposal room, the various methods of coupled analysis and reasonable numerical solutions for PA calculations. Several coupled methods have been implemented for detailed testing: (1)*

the porosity surface approach with the porosity surface calculated by either SANTOS or SPECTROM 32 (2) a coupled flow-mechanical analysis using PHENIX to interface the code for room closure with the code for fluid flow and room pressure at each time step. This approach incorporates the dependence of gas generation on brine availability into the analysis; (3) three-phase flow approximations; and (4) a theoretical model with true coupling of deformation to pore mechanics, including interbed fracturing.

The first two methods have been documented in published reports [Butcher and Mendenhall, 1993; Larsen memorandum in Volume II, Appendix G of the SPM Position Paper "Disposal Room and Cuttings Models White Paper for Systems Prioritization and Technical Baseline"]. The results of the three-phase flow calculations will be published in the near future. The theoretical model is still in development.

All work to date indicates that the porosity surface method is sufficiently accurate for PA calculations and it is therefore being used for all PA calculations.

EPA Comment 3. Culebra Groundwater Model
The Culebra Member of the Rustler Formation has been identified as one of the major potential pathways to the accessible environment. We have concerns and questions about how the Culebra transmissivity field is being modeled and the resulting ground-water travel times.

Alternative conceptual models and computer algorithms can be applied to the existing data to produce different interpretations of the transmissivity fields than those used in the current performance assessment. Indeed, Sandia National Laboratory and their contractors have used different models that have produced different results. The current estimate of ground-water travel times to the boundary of around 15,000 years (with a range of 9,000 years to 32,000 years) appears to be quite high, relatively to previous estimates. Using data from some of the wells in the southern section of the WIPP site, other reasonable approaches could produce travel times that are shorter by several thousand years. Perturbations to the repository/Culebra system (e.g., pressurized brine from the repository or Castile Formation that enters the Culebra at a faster rate than that currently modeled) could produce even faster travel times. Since this is such an important topic, we are considering holding a workshop, in which we would invite your experts and outside experts to further explore this topic. Our comments on this topic are further discussed in the attachment.

Response. *The WIPP project has established a formal program to build confidence in conceptual models and the associated PA analyses by broadening the involvement of outside experts and stakeholders in the PA process and by revising and updating the SNL QA Program for computational activities. WIPP's formal program includes the following activities: (1) involving the international community in hydrologic model development, verification and validation through the INTERVAL programs of the NEA; (2) preparing a series of PA* *calculations (the '91 PA, the '92 PA, the DCCA and the CCA) which show the progression of conceptual models and technical data. The results from these PA analyses have been or will be reviewed by the EPA, stakeholder groups, the NAS and by the international community through the NEA to increase confidence in the PA calculations; (3) involving all of the stakeholders in preparation of the final CCA; and (4) involving regulatory agencies and stakeholder groups in the SPM process.*

Transmissivity fields are calculated from the field data using whole-rock porosity values, resulting in high ground-water travel time predictions (ranging from 9,000 to 32,000 years). In performance assessment, the conservative approach of calculating the transmissivity fields using the much smaller fracture porosities is implemented. This approach results in travel times predictions that are much lower than the range cited, some as low as hundreds of years.

The spatial variations in transmissivity are implemented by numerically generating realizations conditioned on observed head and transmissivity values using the pilot point technique (La Venue and RamaRao, 1992). Multiple realizations are generated and sampled on to address the uncertainty associated with the transmissivity field. This approach has been extensively reviewed.

EPA Comment 4. Inventory Estimates and Waste Analyses
The presentation of the inventory and the types of waste to be disposed at WIPP needs improvement. We have attempted to conduct analyses using the inventory numbers provided in the Appendix of volume 3, but the description of how the inventory was used in the analysis is unclear and incomplete. With the explanation on the inventory given in the Performance Assessment report, it is possible to calculate a range of curie levels between 650,000 curies and 36 million curies. While neither of these activity levels is realistic, the fact that they can be derived from information provided in the PA points to the need for a better discussion. This is a crucial topic because release limits will be based on the information presented in the application. It will be important for DOE to provide clear and accurate documentation of the origin of the inventory estimates. In addition, there should be an analysis and discussion of the potential interactions between the hazardous waste constituents and the radionuclides.

Response. *Since the 1992 PA, the CAO has continued to refine and update the transuranic (TRU) and mixed waste inventories that are destined for disposal at the WIPP. The latest data on the TRU waste disposal inventory are presented in the Waste Isolation Pilot Plant Transuranic Waste Baseline Inventory Report (WTWBIR) [DOE, 1995]. The WTWBIR inventory data are derived from information provided by the TRU waste generator/storage sites. The WTWBIR inventory data are the basis for the PA in the DCCA and for all subsequent PA analyses.*

The release limits given in Volume 3 of the 1992 PA used the waste unit factor of 1.814×10^7 Ci from the 1991 PA (SAND91-0893). The referenced memo by Peterson (Preliminary Contact Handled (CH) Radionuclide and Nonradionuclide Inventories and Remote Handled (RH) Radionuclide Inventory for Use in 1992 Performance Assessment, October, 28, 1992) gives 11.74×10^6 Curies. Because the discrepancy was small, the same number was used for the 1992 PA as for the 1991 PA. This number was then multiplied by an applicable release limit requirement for each radionuclide to yield the waste unit factor that is then used as the normalization factor for the release limits.

In accordance with 40 CFR Part 191, Appendix A, Note 1(e), the waste unit factor includes alpha emitters with half-lives longer than 20 years (Cm-244 with a half-life of 18.11 years is included because it decays to Pu-240). CCDFs, however, are calculated using the entire radionuclide inventory given in the referenced tables and therefore include radionuclides in addition to those in the waste unit factor.

The CAO also has an ongoing program to investigate the interactions between hazardous waste constituents and radionuclides. For example, extensive testing has been performed at the Idaho Nuclear Engineering Laboratory (INEL) to determine the concentrations of gas-phase volatile organic compounds (VOCs) in TRU waste containers. This effort included the measurement of VOCs in 66 drums of TRU waste at INEL and at the Rocky Flats Environmental Technology Site and simulated waste experiments coupled with VOC transport model development to predict drum headspace VOC concentrations [Connolly et al., 1995].

EPA Comment 5. Institutional Controls

The current performance assessment assumes active and passive institutional controls will be in place to deter human intrusion. The performance assessment assumes no intrusion for 100 years after the repository is closed, and credit is taken for a reduction of drilling rate due to passive controls. However, the performance assessment does not provide details on the nature of either active or passive controls. In future analyses, DOE will have to provide detailed information on the active and passive institutional controls it plans to use and justify any credit taken in the quantitative analyses. The Compliance Criteria (40 CFR 194) will further address the topic.

Response. *DOE recognizes the significance of active and passive controls to deter human intrusion. The performance and associated assumptions for institutional controls are discussed in detail in Section 7, Assurance Requirements, of the DCCA. In the future, the CAO will respond to the requirements of 40 CFR 194 through the final Compliance Certification package, which will be submitted in December 1996.*

EPA Comment 6.
Although the document presents a wealth of information regarding radionuclides, in some instances the document ignores related issues pertaining to RCRA-hazardous constituents. Including hazardous constituents in the PA is important because the presence of hazardous constituents could have an effect on the ability of radionuclides to migrate out of the repository. The presence of RCRA-hazardous constituents and their effects on the radionuclides must be addressed.

Response. *The 1992 PA addressed the requirements in 40 CFR 191 for long-term isolation of radionuclides from the accessible environment; it did not consider RCRA. The integration of RCRA requirements into the PA process is discussed in the response to EPA Comment 7.*

EPA Comment 7. Improvements need to be made in modeling gas generation, RCRA-hazardous constituent transport, and radionuclide and RCRA-hazardous constituent concentrations. Furthermore, a comprehensive RCRA-based risk assessment could be performed (using model results) to assess the risk to the public from RCRA-hazardous constituents. Such an assessment, although not required (but of the type requested by the NAS WIPP Panel), would provide additional support to the NMVP.

Response. *The DCCA represents an integrated, comprehensive package that addresses the long-term requirements of RCRA and 40 CFR 191 using a consistent PA methodology. The Project Technical Baseline (PTB) documents the data and models that will be used for evaluating compliance with the long-term requirements of RCRA, 40 CFR 191 and 40 CFR 194. The PA calculations for the DCCA are based on existing or best-estimate data from experimental programs and on the most current estimates of the TRU waste inventory. Computational models and codes used in performance assessment will be the same for the two standards where appropriate; differences in regulatory performance measures will cause computational models to differ in some cases.*

The CAO intends to comply with all requirements of RCRA, 40 CFR 191 and 40 CFR 194. Any decisions about future activities with regard to the NMVP must await review and discussion of the DCCA by the EPA and stakeholders.

EPA Comment 8.
The undisturbed PA scenario to date does not address potentially critical issues such as the impact of anhydrite fracturing, room closure, gas transport, impact of nearby mining, and the lower shaft seal threshold permeability. These issues need to be modelled and analyzed, not only in a segmented fashion, but also in an interrelated fashion concurrent with "worst-case" gas generation scenarios.

For example, the undisturbed scenario is constructed in such a way that it does not evaluate the potential for any mining activity occurring at or near the Land Withdrawal Act (LWA) Boundary. Such mining might impact WIPP (within 10,000 years) while not physically intruding into it. Discussion of the RETSOF commercial salt mine (discussed in detail in the technical comments) suggests how severe the impact could be.

Response. *The undisturbed scenario for the DCCA includes the geological characteristics and fluid properties of the Salado and Non-Salado, including anhydrite fracturing, the rock mechanics of room closure, gas generation processes, and seal permeability. 40 CFR 191 requires probabilistic modeling and specifies reasonably expected not "worst case". However, "worst-case" is included in the distributions used in the probabilistic modeling.*

The DOE believes that disturbances of the disposal system caused by mining activity outside the controlled area need not be evaluated in the undisturbed scenario (see response to Comment 1).

EPA Comment 9.

The undisturbed case scenarios modelled to date exclude potential impacts of mining, at least in part under the assumption that 40 CFR 268.6 excludes consideration of any unnatural or human intrusion occurrences. The 1991 Performance Assessment states "The RCRA applies only to undisturbed performance." (Volume 6, page 2-5; SAND91-0893/6.) These positions have been maintained in the 1992 Assessment, and no developed mining scenarios are included.

While the PA does state that the "effect of subsidence of potash mining will be added in future PAs" (Volume 1, Chapter 4, page 4-2, line 15), the degree of expected emphasis on this scenario varies throughout the PA. For example, a potash mining scenario is identified in the 1992 Summary of Screened Events and Processes (Volume 2, pages 4-6 and 4-7) as being "retained for consideration," but apparently has not been investigated. Volume 1, Chapter 3 (page 3-11) states that consequences of potash mining outside the WIPP boundary "...will be addressed in future analyses when a three-dimensional model for regional groundwater flow is available."

Several factors suggest expansion of this approach. The following paragraphs explain reasons to adopt an emphasis on potential impacts of potash and other resource (e.g., salt) extraction in the final PA.

Since any nearby potash mining would most likely occur in the McNutt zone, above the repository level but still within the Salado formation, it is unlikely that direct "intrusion" would occur. However, the long-term effects of mine subsidence might include significant increases in fracturing, porosity and permeability at distances far from the mined zone, which could reach the WIPP Controlled Area, or even the waste panels themselves. Salt and chlorine production might involve massive room and pillar or other typical shaft and drift operations at or below the repository elevation. These scenarios may be appropriately investigated under either—or even both—"undisturbed" and "human intrusion" conditions. (Undisturbed conditions would include long-term effects of nearby mining, without direct penetration of panels.)

Operators of a mine of this type would be likely to perform horizontal exploratory drilling to determine the extent and characteristics of the resource. This is a direct waste panel intrusion scenario similar to oil and gas drilling.

The 1991 Sandia report mentioned above, "Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant" (SAND90-3063 UC-721) provides reasons to extend the consideration of potential intrusions (or perturbation of "undisturbed" conditions) beyond the oil and gas drilling scenarios evaluated to date. The paper includes estimates of the current market values of mineral resources inside the "controlled area" of the WIPP (a circle of radius 5 km), such as salt, chlorine, potash and magnesium. These values are all greater than the value estimated for oil and gas (combined) within the WIPP controlled area. The value of magnesium is estimated as two thousand times, and potash twenty times, the oil and gas value.

Three potash mines are currently in operation within 10 miles from the WIPP, and the "...nearest economically exploitable potash reserves are 1 km (0.6 mi) from the waste panels..." (1992 PA Volume 1, page 2-4). There are no existing or planned regulations or legislation which would prevent the future expansion of these or other new mines up to the WIPP Land Withdrawal Act boundary, which is at minimum 2414 meters (7916 feet) from the closest (south) edge of the repository.

The Expert Panel report quoted above also noted that the potential impacts of mining at or near the WIPP site were the greatest of any scenario considered, but were discounted on the basis of the interpretation of Appendix B of 40 CFR 191. (Scenarios with impacts greater than oil and gas drilling were excluded from the final conclusions.) This has led to inclusion of statements in the 1992 PA which are directly at odds with the Expert Panel's report, e.g., "...exploratory drilling...has been demonstrated by past analyses...to be the only event likely to lead to radionuclide releases close to or in excess of regulatory limits" (Volume 1, page 4-5). The statements which cite the Expert Panel report (e.g., Volume 1, page 5-4) do not mention the panel's discussions of the probability or potential impacts of mining.

Another reason to reconsider a near-WIPP mine scenario is a specific current example of "worst case" mining impact. A commercial salt mine (RETSOF) near Rochester, New York is a large (11 square miles) room and pillar operation which has supplied much of the east coast with road salt for many years. An unconfined aquifer overlies the mine, which is about 1,100 feet below the ground surface. In mid-March of this year, a portion of the mine collapsed, leading to massive uncontrolled inflow of water (estimated at 14,000 gallons per minute) and widespread subsidence which extends to the ground surface. Efforts to plug fractures which provide water flow paths have so far been futile. If the effort is abandoned, most of the mined zone is expected to collapse due to pillar dissolution. In addition to surface damage (a small town is within the probable 11 square mile collapse area), the predictable results of this event will include dramatic increases in porosity and permeability of the overlying "disturbed rock zone," relatively rapid future dissolution of huge portions of the salt formation, and groundwater contamination.

Economic motive exists for future extraction of salt, chlorine, potash and magnesium resources in the near vicinity of the WIPP, according to the Expert Panel on Human Intrusion, even if oil and gas drilling is more likely to occur in the short term. Mining may involve construction of large diameter injection/withdrawal wells and/or access shafts and drifts, pumping of significant quantities of mine water or extraction solution, and placement of large quantities of removed minerals and water on the ground surface. The Final PA could include evaluation of the effects on the WIPP of such mining.

A final reason to place greater emphasis on the mining scenarios is the ready availability of numerous three-dimensional groundwater flow models. The absence of such a model is the only specific reason given in the 1992 PA (Volume 1, page 3-11) for not including any mining scenarios. The document provides no explanation for the difficulty in finding and applying such a model. Instead, an entirely new 3-D version of the 2-D BRAGFLO model is being written, especially for PA purposes (Volume 5, page 2-4), and the new model "...should be practical by next year" (i.e., 1993). The need for such a new, customized program is unclear.

German investigations of potential radioactive waste disposal sites (in the Gorleben salt dome) have used a 3-D flow and transport model for at least 5 years (Bundrock, G. and H. J. Engelmann, "Groundwater Aspects of High-Level Radioactive Waste Disposal," in *Recent Advances in Groundwater Hydrology*, American Institute of Hydrology, 1989). The International Groundwater Modeling Center in Golden, CO distributes several three-dimensional flow and transport models which are in wide use, such as MODFLOW and SOLUTE.

Response. *The DCCA and the Scenario Position Paper present a systematic reevaluation of scenarios since the 1992 PA. The CAO assumes that future exploratory drilling (within or outside the controlled area) that does not intersect the repository can be eliminated from performance assessments on the basis of low consequence. Work is underway to evaluate the adequacy of this assumption and additional material in support of low probability and low consequence screening decisions will be included in future drafts of the Position Paper and the final compliance certification package.*

DOE believes the reference to the RETSOF mine is inappropriate as no unconfined aquifer capable of massive inflows of waste exist above the WIPP facility.

Promulgation of 40 CFR 194 will provide criteria for certification of the WIPP repository to 40 CFR 191, possibly changing some of the guidance in Appendix C of 40 CFR 191 under "Inadvertent Human Intrusion into Geologic Repositories". The features, events and processes (FEPs) relating to oil and gas resource extraction and potash mining are still retained for evaluation and have not been screened out. These FEPs will be analyzed and included in the final compliance certification package, if appropriate.

EPA Comment 10.
The focus of surface release scenarios is limited to solid "cuttings" (including waste particles and spallings) removed from waste panels penetrated by boreholes (Volume 1, Section 4.1.2 and Volume 2, page 4-13). However, some of the model runs resulted in waste panel pressures much greater than lithostatic (Volume 4, Figures 5.2-1 and 5.2-11) at the time of intrusion. Volume 1 (page 4-6) states that Volume 4 will contain preliminary analyses of the potential for releases by discharge of brine at the surface both during drilling and after plug degradation, but such analyses were not found. Volume 2 (Section 4.2.3.2) and Volume 4 (Section 5.2) include only two types of releases to the accessible environment: cuttings deposited on the surface; and brine flow into the Culebra Formation. The solubility of gas in the brine is assumed to be negligible by the PA (Volume 2, Chapter 7, Page 7-5, line 4). This is inconsistent with existing experimental and theoretical data for gas solubilities in water and brines and may be a key means of gas transport outside the WIPP unit boundary as pressure increases in the repository. Gas mixtures which contain hazardous constituents should also be considered.

Response. *This comment covers several issues which require individual discussion. Repository pressure exceeding lithostatic pressure arose in the 1992 PA because models for fracturing or fracture inflation at high pressure had not been incorporated into the PA models at that time. Models for fracture behavior are under development.*

Volume 1 does indicate that preliminary analyses relating to the potential for release by discharge of brine at the surface are discussed in Volume 4. Actually Volume 4 makes only very brief reference to this possibility. The preliminary analyses were not complete when Volume 4 was compiled and it was not possible to change the already published statement in Volume 1. Investigation of intrusion releases is ongoing, with several different mechanisms under analysis.

The discharge of brine and other release mechanisms at the surface are being investigated in later studies.

The volume of gas that can be dissolved in the brine at the pressures under consideration is far less than the volume of gas that will remain in the gas phase. The assumption of zero solubility is the worst case for purposes of computing repository pressurization as the driver for brine migration out of the repository (for either disturbed or undisturbed conditions). In the 1992 PA, the main emphasis was on whether brine would reach the unit boundary. Development of models for addressing concentrations and transport of hazardous constituents is currently underway.

EPA Comment 11.
The intrusion model is based in part on the assumption that the intruding drilling technology will be "comparable" to current technology (Volume 2, Chapter 2, page 2-51). However, the modeled intrusion scenarios exclude typical casing through water-bearing zones (e.g. the Culebra) and do not account for the immediate, short-term effects of sudden increases in downhole pressure, such as would be encountered by drilling into a waste panel. If the surface casing was not equipped with blowout preventers, large volumes of both gas and brine would likely be expelled. Assuming blowout preventers were in place, continued drilling may require circulation of contaminated brine up the hole along with cuttings. Therefore, the intrusion scenarios should account for some volume of brine from a waste panel reaching the surface (mud pit) along with cuttings.

Response. *The discharge of brine and other release mechanisms at the surface are being investigated in later studies. The current status of models for these discharge and release mechanisms is presented in Section 4 of the SPM Position Paper: "Disposal Room and Cuttings Models White Paper for Systems Prioritization and Technical Baseline".*

EPA Comment 12.
With regard to the undisturbed scenario, the impact of gas transport has not been modelled. EPA believes that additional analyses of gas and brine migration may show a potential for gas migration, and therefore recommends that this modeling effort be included in the proposed no-migration petition that DOE intends to submit next year.

Response. *The current PA analyses use BRAGFLO, a two-phase computer code, to simulate the flow and migration of gas and brine in the Salado formation.*

EPA Comment 13.
A related concern is the absence of modeling for potential transport of colloidal contaminants in the brine and cuttings which escape the waste panel. Although solubility of some hazardous constituents, plutonium and other potential brine contaminants is very low, colloids are known

to provide a mechanism for transport of these elements or compounds (at Hanford, INEL and Los Alamos) at concentrations above solubility limits.

Response. *Experimental programs in actinide solubility and radionuclide transport, including the influence of colloids, are currently being performed by the WIPP Project. A snapshot of these experimental programs, including the colloidal studies, is presented in the Actinide Source Term Position Paper. The results from these studies will be incorporated into the PA for the final compliance certification package.*

EPA Comment 14.
Figure 1.5-1 (Volume 3, Chapter 1, page 1-48) shows the location of the Gnome Project Site, but it is not explained in the text. The PA needs to provide an explanation on the Gnome Project and assess its potential impacts on the WIPP.

The Plowshare Project Gnome Test site is approximately six miles southeast from the WIPP LWA boundary (Volume 3, Figure 1.5-1). The map identification is the only reference to this site found in the PA. According to Benford, et al, in "Ten Thousand Years of Solitude?", a report included in SAND90-3063.UC-721 *Expert Judgment on Inadvertent Human Intrusion into the WIPP*, a fission device was detonated at the site at a depth of 1250 feet. A separate report indicates that the yield was 5 kilotons (TNT equivalent) and the year of the test was 1965.

The site deserves further consideration in the future (final) PA for several reasons. The most obvious reason is the (slight) similarity of the blast cavity to either future mines, or to the WIPP itself. Lessons available from the Gnome site may be applicable to the WIPP in several ways. Since almost no information has been provided about the Gnome site in the PA documents, the related comments are stated in the form of questions:

How large was the cavity created in the Salado by the blast?

Has any follow-up monitoring been performed to determine the current status of the cavity? (For example: is it completely collapsed or porous rubble-filled; creep effects in the last 29 years; disturbed rock zone (DRZ) extent; characteristics of the water/brine which presumably fills it; has ground surface subsidence occurred, etc.)

What are the major differences between the Gnome cavity and possible future mine cavities?

Has any monitoring been performed to determine impacts on groundwater flow and transport of radionuclides or hazardous constituent metals? (Could K values for the Salado and Culebra be verified through such investigations?)

Is there any potential impact on the WIPP, such as increased porosity and permeability in the Salado and overlying formations which could extend from the Gnome site to the WIPP LWA boundary in 10,000 years?

Response. *The Project Gnome Test Site is approximately 6 miles southwest from the WIPP land withdrawal boundary. The fission device was detonated at a depth of 1,184 feet, with a yield of 3 kilotons, and took place December 10, 1961.*

The cavity created in the Salado Formation by the blast produced a sphere with an average diameter of 124 feet.

The cavity did not collapse because it was backfilled with slurried muck from the excavation; therefore there are no means of measuring creep effects. There are no measurable subsidence effects because of the backfill. Further, there is no indication of brine above the rubble-pile. The extent of the DRZ was approximately 350 feet from the detonation point in the upward direction and remained entirely within the salt beds.

The Gnome cavity was formed by internal pressures which forced the rock outward. The DRZ resulting from the Gnome blast was a result of rapid shock wave compression and subsequent relaxation. In projects such as the WIPP, which involve excavation, the DRZ is a result of slow stress release and is not shock-related. Given the differences in mechanism and time scale, it will be very difficult to extrapolate data on the DRZ of the Gnome cavity to the WIPP repository.

The site has been monitored since the test. EPA takes samples once per year and no radioactive contamination has been found, nor have any fission products from the detonation. Tracer tests with non-sorbing isotopes were done before the blast but, because they are non-sorbing, no information is provided on retardation. Sandia continues to make use of the hydrologic information being obtained from the Gnome monitoring wells.

There is no potential for effects from the Project Gnome blast to impact repository performance because the strong shock waves from the Gnome detonation did not extend beyond a few hundred feet and permanent effects did not extend the six miles to the WIPP site. For additional information, see the report that summarizes the reentry, Project Gnome, Carlsbad New Mexico 1961: The Environment Created by a Nuclear Explosion in Salt.

EPA Comment 15.
The modeling performed to date has not taken into account the probable fracturing of anhydrite strata (especially Marker Beds 138 and 139) which may be exposed to high gas and/or brine pressures (perhaps well above lithostatic) during the first several hundred to two thousand

years after closure. This phenomenon has been experimentally demonstrated at the WIPP, and the Agency believes that this modeling is necessary to determine whether repository pressurization has been overstated, or if lateral gas migration has been understated.

Response. *A model for interbed fracturing has been incorporated in the current PA system and will be used in all future analyses. It is not clear whether or not pressure in fractures will change the transport to the accessible boundary, although as soon as pressure in the repository exceeds the threshold pressure in the interbeds, flow into the interbeds begins. Flow is expected to begin between 12.1 MPa and 13.1 MPa.*

EPA Comment 16.
The analyses and model runs for potential flow through the lower shaft seal do not appear to include consideration of the interface of the seal materials with the host rock (salt). The seal itself theoretically performs very well (no gas flows through the seal in 10,000 years), even though gas reaches the bottom of the seal in many cases. However, the interface may be of greater concern, because of natural inhomogeneities, stress relief and repressurization, and other factors such as differences in compressibility and moisture retention, in the surrounding rock as compared to the seal materials.

Within the seal itself, it is stated that the backfill component of the lower shaft seal will achieve final permeabilities comparable to the Salado formation host rock after consolidation due to creep occurs; however, no calculations or modeling is presented directly in the PA to support this assumption (Volume 3, Chapter 3, page 3-14). Specifically, the PA does not provide experimental data to support the assertion that the crushed salt will compact to 95% of initial density within 100 years (Volume 2, Chapter 2, page 2-48, line 19), and that the associated permeability design objectives will be met.

Response. *Experimental data and calculations to support the assertion that the crushed salt will compact to 95 percent of initial density within 100 years have not been included in the 1992 PA report. The experimental data and calculations to support these assertions are found in Nowak, et al. (1987) and Sjaardema, et al. (1987).*

Scoping model calculations for the reconsolidation of crushed salt in WIPP shafts are presented in Nowak, et al. (1987). The scoping model calculations supported an estimate that the reconsolidating crushed salt in the lower third of a WIPP shaft is likely to meet the criterion of acceptability for shaft sealing in less than 200 years.

Further work by Sjaardema, et al. (1987) refined the time required for salt consolidation to 95 percent of intact WIPP salt density through the use of a constitutive model for crushed WIPP salt. The experimental results of modeling the consolidation of crushed salt, to be used as backfill in shaft and drift configurations, is shown in Table 4.1 (page 39) of the report.

EPA Comment 17.
The final extent of the DRZ after 100 years or more of reconsolidation is unknown, because the exact behavior of the salt as it creeps into waste rooms, compacts the waste and backfill, and reseals fractures in the outer DRZ, cannot be precisely predicted. The PA suggests that the final limits of the DRZ, and parameter values characterizing the DRZ, should not be inferred from contemporary measurements (Volume 3, page 2-32). However, the single "disturbed permeability" parameter value used in gas and brine flow calculations was taken from a selection of actual "nonfar-field" measurements (Volume 3, page 2-37) which may not reflect either current or future larger-scale (bulk, i.e., fracture) permeability. This parameter was not sampled or varied in the 1992 BRAGFLO model runs, apparently because it was ranked low in previous sensitivity analyses. The DRZ may deserve further attention, based on the wide range in existing permeability data—as much as 5 orders of magnitude larger than the value used in BRAGFLO—and the DRZ thickness values used in this model.

Response. *Using a contemporary "non far-field" disturbed permeability value for the DRZ in the gas and brine flow calculations, as opposed to a value that would reflect disturbed permeability in the future, provides an idea of the maximum flow quantities expected in the system. As creep closure heals the DRZ, the DRZ permeability should decrease with time.*

There is a wide range of existing permeability data (as much as 5 orders of magnitude larger than the value used in BRAGFLO) and DRZ thickness values used in characterizing the zone. Further work to refine the DRZ characterization has focused on incorporation of fractured interbeds into the new PA model. The current PA uses permeabilities several magnitudes higher than the near-field values, so the analyses use conservative values for the DRZ.

EPA Comment 18.
The PA does not evaluate the impact of Room Closure relative to enhancement of gas pressure and concomitant anhydrite fracturing. In Volume 2, Chapter 7, pages 7-7 and 8, it is implied that the total pore space is constant and as such, room closure (creep) is not taken into account. However, the text also states that room closure is accounted for "in an indirect way" (Volume 2, Chapter 7, page 7-5). Please explain that process.

Response. *The 1992 PA did not address the possibility of fracture formation or inflation of pre-existing fractures in anhydrite layers. Models are being developed for this effect.*

Effects of room closure on gas pressure buildup were addressed. In Volume 2, page 7-8, lines 13 and 14, it is stated that the total pore space in the idealized, <u>*collapsed*</u> *WIPP panel is constant. This is a post-closure condition. Also, the assumptions on pages 7-7 and 7-8 are specifically stated to be for modeling waste mobilization in the E1 and E1E2 scenarios.*

High pressure gas in the repository will be released during drilling of intrusive borehole(s) or very early in the period after plug deterioration. Consequently pressure changes, and hence creep effects, are minor during waste mobilization and transport for these scenarios.

EPA Comment 19.
Consequence assumption number 5 (Volume 2, Chapter 7, page 7-8) states that the pore spaces of the idealized panel are fully saturated with brine at all times. In Chapter 4, page 4-18 of Volume 2, the last paragraph states that assumptions for the E1 borehole show "Brine flows from the waste into the E1 borehole exceed those into the E2 borehole only for those realizations in which total flow is small because the panel was not brine-saturated at the time of intrusion." Please explain the differences regarding brine saturation between the two assumptions.

Response. *The 1992 PA waste mobilization computations for the E2 and E1E2 scenarios concentrated on completely brine saturated conditions because this represents the most severe case for potential release.*

The sentence from Volume 2, page 4-18 quoted in the comment is identified (on page 4-18) as a summary of certain findings from the 1991 PA. Thus it does not contradict the assumptions given for the 1992 PA. The sentence in question is presented as part of a discussion explaining that in '91 it was determined that the E1 scenario always produces releases that are less than or equal to those for E2 except in cases where the release is small. Hence, in '92, separate waste transport calculations were not made for E1 (although specific probability of occurrence calculations were made for E1).

EPA Comment 20.
The document states waste characterization estimates used in the PA have the potential for large uncertainty (Volume 2, Chapter 2, page 2-47). Please provide clarification of waste characterization estimate uncertainties, and measures to reduce them (i.e., how will the waste estimates be reconciled with the projections made from the inventory required by the Federal Facilities Compliance Act?).

Response. *The WIPP TRU waste inventory characterization is largely based on process knowledge. The uncertainties will be reduced by verifying the waste categories using real-time radiography, head-space analysis, and some visual inspection. These verification requirements are defined in the Draft Transuranic Waste Characterization Quality Assurance Plan. The high efficacy of process knowledge in determining waste categories is discussed in the appendices to Chapter 2 of the Background Information Document for Proposed 40 CFR Part 194 (EPA 402-R-95-002).*

EPA Comment 21

Volume 3, Chapter 3 of the document presents the characteristics of the expected WIPP waste inventory. Radionuclides are addressed in detail, but RCRA-hazardous constituents are not provided.

Response. *All relevant information requirements outlined in the EPA's (1992) No-Migration Variance Petition (NMVP) guidance document (EPA-530-R-92-023) will be completed with the Phase II NMVP submittal in June, 1996. This relevant information includes waste code, hazardous properties, physical, chemical and biological characteristics, and waste characterization data. Parameter distributions and assumptions necessary for demonstrating compliance will be documented for the phase II NMVP (June, 1996). Some of this information has already been documented in the Hazardous Constituent Source Term Position Paper, the Phase I NMVP and the WIPP TRU Waste Baseline Inventory Report (WTWBIR).*

EPA Comment 22.

There are inconsistencies concerning discussions on waste inventories in the PA. For example, the RH-TRU inventory is based on 1990 Integrated Data Base figures of 5,300 m^3 over the WIPP design capacity of 6,784 m^3. The PA indicates that the waste generators partially fill the canisters with different volumes of waste and that the actual volume of waste would therefore be lower than the design capacity (Volume 3, Chapter 3). The PA should explain why the lower number was chosen rather than the higher.

The PA states that the inventory of RH-TRU was estimated using an "unknown" slurry mixture from Hanford to provide the isotopic distribution, without explanation (Volume 3, Chapter 3, page 3-28). Please provide the rationale for choosing this waste as representative, and the hazardous constituents and/or characteristics of this waste.

Response. *The DOE is generating a consistent TRU waste inventory for the WIPP, based on information from DOE generator sites. This document, the WTWBIR, defines the TRU waste inventory available for WIPP and provides a description of the physical and source characteristics of the waste, the potential gas-generating waste material parameters, EPA hazardous waste codes, and existing and estimated future inventories. Each site reports the total stored radionuclide inventories (both CH and RH) at the site level and uses these data as the basis for estimating projected radionuclide inventories. The PA for the DCCA and all future PA analyses will be based on the inventory defined in the WTWBIR.*

EPA Comment 23.

A preliminary safety assessment could be prepared for long-term consequences to the public health as a result of the RCRA hazardous constituents of the wastes emplaced in the WIPP (Volume 1, Chapter 1, page 1-2). Although Monte Carlo analysis is part of a widely used risk assessment methodology, the Monte Carlo analysis performed for this PA stops short of performing a RCRA-based risk assessment for the RCRA-hazardous constituents (Volume 1, Chapter 4, page 4-1). As stated earlier, a comprehensive RCRA-based risk assessment. (as outlined by the NAS WIPP Panel) would provide a stronger basis for future public review, hearings, and regulatory decisions.

Response. *The CAO intends to comply with all requirements of RCRA, 40 CFR 191, and 40 CFR 194. Any decisions about future activities with regard to a RCRA-based risk assessment must await review and discussion of the DCCA by the EPA and stakeholders.*

EPA Comment 24.

The document states the logarithmic K_d distributions used in the PA required a number of subjective assumptions derived from an internal expert judgement process (Volume 3, Chapter 2, page 2-92). Please provide the rationale and procedure for using subjective judgement.

Response. *This expert panel was assembled because of the limited data available for the distribution coefficient, K_d, in the Culebra. The expert judgment process included 3 Sandia personnel who participated in individual elicitation sessions to develop the probability distributions regarding radionuclide retardation in the Culebra. The use of logarithmic K_d distributions is considered realistic, based on the evaluations of the expert panel with the data that were available at that time.*

The use of expert panels includes several steps to derive realistic distributions. First, applicable experimental information is evaluated to ascertain potential applications to the development of distribution coefficients for the Culebra. Once these values are determined to be applicable, an elicitation process is conducted. This process includes meeting with other experts in the field as well as other principal investigators and performance assessment personnel. The proposed parameter values are reviewed and the judgments, along with associated rationales, are discussed for implementation into the PA models. The results of the elicitation are then documented and reviewed by the elicitor of the meeting for accuracy.

EPA Comment 25.

The PA does not integrate several key codes into the CAMCON system, namely BRAGFLO, SECOTP2D and CUTTINGS. This is not a computationally efficient approach and is prone to QA breakdowns given the heavy emphasis on manual data entry transfers and analysis. This approach also limits the modeling from the standpoint of the model's ability to handle complexities and the shear number of scenarios that can be modelled.

Response. *The CAMCON system has been expanded to include the BRAGFLO, SECOTP 2D and CUTTINGS codes since the 1992 PA was performed.*

EPA Comment 26.
The second paragraph in Volume 2, Chapter 4, page 4-7 indicates that two scenarios selected for modeling in the screening procedure, TS (potash mining outside the waste panels) and E3 (drilling of water withdrawal wells), were not evaluated. No justification for these omissions was provided.

Response. *Please see the responses to Comments 1 and 9 regarding the current approach to scenario development, including the evaluation of the potential effects of such FEPs as potash mining and water withdrawal wells on repository performance.*

EPA Comment 27.
Gas transport of RCRA-hazardous constituents is not modelled or investigated, although several model runs indicate the possibility of gas migration beyond the LWA boundary. The potential for transport of hazardous constituents needs further attention to address 40 CFR 268.6 compliance concerns. Evaluation of transport mechanisms is necessary before soil concentrations can be estimated at given locations (e.g., the LWA boundary).

Response. *As noted previously, the 1992 PA addressed the requirements in 40 CFR 191 for long-term isolation of radionuclides from the accessible environment; it did not consider RCRA.*

The Hazardous Constituent Source Term Position Paper outlines the conceptual model and assumptions required for modeling gas and liquid phase hazardous constituents. The Salado Formation Fluid Flow and Containment Position Paper discusses gas and liquid phase contaminant transport. Evaluation of all transport mechanisms is an explicit requirement identified in the EPA (1992) NMVP guidance document (EPA-530-R-92-023). All relevant aspects will be considered for the final compliance certification package and NMVP.

EPA Comment 28.
In general, more discussion on Latin Hypercube Sampling (LHS) is needed. For example, the procedure for excluding variables is not defined nor is documentation presented to justify the exclusions. Also, the impact of uncertainties introduced by the LHS needs to be evaluated. The PA does not define a procedure for determining the sampling error of LHS-derived estimates. Also, the current PA LHS procedures treat the uncertain input variables as uncorrelated without adequate justification.

Response. *These issues will be considered for the final CCA.*

EPA Comment 29.
The PA does not include a detailed discussion of Quality Control and Quality Assurance (QC/QA) issues such as the procedures covering experiment execution, computation development and code execution (Volume 1, Chapter 3, page 3-17, line 41). The document makes no distinction between Quality Control (i.e., data validation, checking of calculations) and Quality Assurance (system validation; checking overall model methodology against actual measurements). For example, the PA notes that CAMCON "automatically handles quality assurance during the calculations" (Volume 2, Chapter 1, Page 1-3, line 30), but provides no discussion as to how this is achieved. Also, the PA provides no indication of efforts to assess the quality of parameter data used in modeling. Parameter sheets should be supported with a statement of data quality and/or a statement of confidence level for each parameter. For example, in some cases expert panel judgement and investigator knowledge are used in lieu of actual data (Volume 2, Chapter 2, page 2-39 and Volume 3, Chapter 3, pages 3-1 to 3-7). No procedure is provided for how data from these sources are reviewed from a quality and level of confidence standpoint.

Response. *The WIPP project has established a formal program to build confidence in PA analyses by broadening the involvement of outside experts and stakeholders in the PA process and by revising and updating the SNL QA Program for computational activities.*

The revised QA program includes implementing procedures to control the entire PA process, including: (1) selection of applicable conceptual models to represent processes and events, (2) development of computer codes (software) to represent these conceptual models, (3) development of numerical values for parameters associated with the respective models for both discrete-valued parameters and distributed-value parameters, (4) configuration control of software during the overall development and implementation process, and (5) operational control of the overall analysis process.

The WIPP Project is currently developing documentation for all PA codes. This documentation includes an expanded description of the CAMCON operating system in a report that will be published shortly (Rechard, 1995).

Finally, Sandia has a formal QA procedure for the use of expert judgment (Rechard et al., 1992). Further refinements of this QA procedure can and have been incorporated into WIPP Procedure No. PAP06, Use of Expert Judgment Panel Quality Assurance Procedures.

EPA Comment 30.

Wherever possible, the use of expert judgement and investigator judgement should be replaced with actual data. This is particularly important for those parameters considered important from the standpoint of sensitivity and uncertainty analyses. For example, distribution coefficients (K_d) should be based on experimentally justified data and not based solely on expert panel judgement (Volume 2, Chapter 2, Pages 2-38 to 2-39).

Response. *The CAO agrees with the need to base PA calculations on as strong a site-specific date base as possible. However, because of the generalization processes inherent in probabilistic PA, it must be noted that there is always some level of expert/professional judgment involved in extrapolating any experimentally derived data base over a period of 10,000 years and to different spatial scales.*

The recently completed SPM-2 iteration has served to refocus the experimental programs on those parameters and programs which are important to compliance. The SPM-2 results have highlighted the importance of distribution coefficients and colloids in the NonSalado, resulting in a renewed emphasis on laboratory and field testing programs in these areas.

EPA Comment 31.
The DOE has discussed in verbal presentations an INTRAVAL study of the Culebra model but presents none of INTRAVAL's comments in the PA itself and the impact of these comments on future PAs. Also, DOE has subsequently verbally identified a second INTRAVAL review of the Salado Brine model but has not provided any discussion of INTRAVAL's comments in the PA. Given the PA's heavy reliance on "expert judgement" and "investigator judgement" unbiased, third-party expert input would be useful to assure the overall PA quality is consistent with international efforts.

Response. *The WIPP project has established a formal program to build confidence in PA analyses by broadening the involvement of outside experts and stakeholders in the PA process and by revising and updating the SNL QA Program for computational activities. The QA activities are described in more detail in the response to Comment 29.*

WIPP's formal program to build confidence in PA analyses includes the following activities: (1) involving the international community in hydrologic model development, verification and validation through the INTERVAL programs of the NEA; (2) preparing a series of PA calculations (the '91 PA, the '92 PA, the DCCA and the CCA) which show the progression of conceptual models and technical data. The results from these PA analyses have been or will be reviewed by the EPA, stakeholder groups, the NAS and by the international community through the NEA to increase confidence in the PA calculations; (3) involving all of the stakeholders in preparation of the final CCA; and (4) involving regulatory agencies and stakeholder groups in the SPM process.

EPA Comment 32. Disturbed Performance: Culebra Flow and Transport
Evaluation of the site must address the potential for climatic changes that could alter the long-term waste isolation capability of the site. The 1992 PA climatic modeling results indicate that climatic influences have little effect on the results, but the assumption of no recharge moving through the site should be reconsidered.

The vector plots, presented in Volume 4, Section 6 of the 1992 PA, suggest that almost no groundwater entering the model domain along the "recharge strip" reaches the WIPP Land Withdrawal Boundary because it is routed to the southwestern portion of the modeled area and exits through this fixed head boundary. An alternative approach that should be considered is to assume that the increased rainfall would make the Nash Draw region a recharge area rather than a no-flow boundary. The southwestern side of the modeled area could also be set as a recharge boundary. The effect of these changes would be to allow water from the "climatic recharge boundary" to reach the WIPP Land Withdrawal Boundary, unlike the current situation where almost no water from the recharge boundary is allowed to reach the site. These proposed changes to the model boundaries are supported by the problems encountered during the model calibration activities conducted by LaVenue (LaVenue 92), as well as by the inconclusive evidence as to whether the Malaga Bend and Nash Draw areas are acting as regional recharge or discharge boundaries.

Response. *We recognize the need to improve the modeling of recharge and discharge near the WIPP site under possible wetter climate conditions of the future. A three-dimensional model of flow in the saturated zone of the entire groundwater basin (that is, the area bounded by groundwater divides) is being used for analysis and FEP screening [Corbet and Wallace, 1993]. This model will be used to evaluate the effects of future wetter climates, along with other features, events, and processes affecting recharge.*

EPA Comment 33. Contaminated Brine in Marker Bed 139 (MB-139)
A human intrusion borehole that misses a waste storage room could intercept the anhydrite Marker Bed 139 (MB-139) in an area where it contains contaminated brine.

If contaminated brine is located beneath or in the area surrounding a waste panel that is intercepted by a human intrusion borehole (E1, E2, or E1E2), some of this brine would be brought to the surface with the drilling mud and thereby contribute to the release to the accessible environment. This has not been included in SNL's human intrusion scenarios to date but it should be examined along with the general category of brine being brought to the surface during drilling from waste storage rooms or Castile brine reservoirs. This will require a distribution of values for several parameters, including brine pressure, permeability in MB-139, and the level of contamination in the brine.

The modeling in Volume 4 predicts that the area in MB-139 containing brine that has been in contact with wastes could be large. An estimation of the level of contamination of this brine has not yet been made. A human intrusion borehole away from the waste rooms and into the contaminated portion of MB-139 would be a new scenario since E1, E2, and E1E2 apply only to those boreholes intercepting waste storage rooms or drifts. This borehole would bring additional quantities of radionuclides either to the surface or to the Culebra Aquifer.

Response. *The effect of an intrusive borehole intercepting contaminated brine in MB139 has not yet been addressed in the investigation of human intrusions. Clearly, the results of such a scenario will depend heavily on the permeability, storage capacity, and any retardation effects assigned to the anhydrite layer. Model development for assessing anhydrite behavior is currently underway. In addition, the volume of brine that might be released to MB139 may be modified by various engineered alternatives that are under consideration. Also see the response to Comment 1.*

The commentor also points out that brine brought to the surface during drilling would be a contribution to release in the E1, E2, and E1E2 scenarios. The continuing studies referred to in Comment 10 considered the conditions for release of contaminated brine during drilling. The conditions necessary for releasing significant quantities are rather restrictive. Furthermore, brine released during drilling would, in large part, be a subtraction from the amount of brine (or at least from the amount of contaminants) that would be available for release later in the other scenarios. Consequently, release of brine during drilling was not explicitly incorporated into the 1992 PA.

EPA Comment 34. Modifications to Existing Scenarios: Brine Flows to the Surface During Drilling

The E1E2 scenario should include the potential for contaminated Castile brine to flow to the surface.

Brine is present to some extent in the waste rooms even in the absence of an E1 scenario. Current modeling of the E2 scenario (Volume 4 of the 92 PA) shows that in 20 percent of the realizations, there will be enough brine to provide a long-term flow into the Culebra Aquifer through an intrusion borehole. Thus, there should be sufficient brine and pressure for some flow to the circulating drilling mud and transport to the surface.

All intrusion boreholes intercepting a waste room are assumed to continue through MB-139. Brine in MB-139 immediately below the waste rooms is expected to be contaminated and would also be available for inflow to the drilling mud.

Response. *Please see the responses to Comments 10 and 33.*

EPA Comment 35. Modifications to Existing Scenarios: Plug Degradation
Present assumptions set the intrusion borehole plugs between the Culebra and the surface as perfect barriers against migration of brine to the surface. This assumption maximizes the quantity of brine that will be diverted into the Culebra for transport toward the accessible environment. It is unrealistic to assume these plugs will be any better than ones between the Culebra and the repository. Plug degradation should be modeled.

Response. *We assume that this comment refers to the arbitrary perfect plug that the 1992 PA placed above the Culebra to maximize flow to that unit. Degradation of the arbitrary perfect plug above the Culebra was not modeled in the 1992 PA because the intent in these preliminary analyses was to focus on possible releases through the Culebra and because the assumption that the plug does not degrade was a conservative assumption for the scenario. The 1992 PA assumed that other borehole plugs would degrade into material with properties similar to silty sand. This latter assumption was in keeping with the guidance in Appendix C of 40 CFR 191.*

More recent PA analyses for the DCCA do not use the arbitrary perfect plug and consider flow into units other than the Culebra.

EPA Comment 36. Shaft Seal Failure: Volume 4, page 4-7, line 40
DOE states that radionuclide transport is not modeled for the undisturbed case because releases will not occur into the Culebra. Seal failure is a distinct possibility. Has a failure of the shaft and other seal system been considered as an scenario occurrence?

Response. *Seal failure/degradation has been considered as an occurrence. Seal failure has been retained for further consideration prior to a final screening decision.*

EPA Comment 37. Synergistic Effects From Drilling: Volume 4, Page 2-16, Lines 1-4
DOE assumes that no synergistic effects result from multiple boreholes for disturbed scenarios other than E1E2. This implies that for scenarios E1 and E2, single boreholes into multiple panels is contemplated. The total release then would be determined by adding the releases occurring from each individual borehole. Does this mean that DOE is evaluating the effects of releases from multiple boreholes/panels in the E1 and E2 scenarios, rather than just from one borehole/panel? The PA needs to be clearer on the communication that occurs between panels during multiple intrusions.

Response. *Human intrusion scenarios in the 1992 PA assumed no communication between panels. Multiple intrusions into a single panel were modeled.*

EPA Comment 38. Loading management/Scenarios
There is no analysis of how various loading management schemes could affect potential releases. The assumption that the waste will be homogenized (to the same material properties and activity level everywhere in the repository) is used to simplify the analyses, but it would be useful to analyze scenarios in which it is assumed that the drums have the varying activity levels presented in Volume 3 (A-138). If the activity level in the drums and the number of drums is varied, then the release limits may be violated much more easily than the current PA analyses indicates.

Response. *The "varying activity levels" in Volume 3 refers to estimates of the curie content of drums and standard waste boxes. These estimates were used in the 1992 PA because of uncertainty about the final waste inventory. Cuttings releases were calculated for varying activity levels. Given uncertainty about brine flow through the waste, it was entirely appropriate to assume a homogenized waste form for dissolved concentrations. If future performance assessments demonstrate waste forms to be significant to compliance, then this issue will have to be reevaluated. If necessary, waste homogeneity could be assured through various engineered alternatives.*

EPA Comment 39. Uncertainty of Scenario Probabilities
The examination of the uncertainty in scenario probabilities for risk representation R_1 using a constant drilling rate presented in Section 8.1 of Volume 4 is restricted by the following assumptions:

1. A simple Poisson model with a constant rate parameter.

2. Multiplication of the maximum intrusion rate of 30 boreholes per km^2 per 10,000 years by a uniform random variable between 0 and 1 in the LHS procedure to simulate the sampled drilling rate.

3. Imposition of an intrusion rate of zero for the first 100 years.

4. Imposition of an intrusion rate of zero for the last 8,000 years.

The combined effect of these assumptions leads to a very narrow range of uncertainty for the probability of the "no intrusion" scenario, denoted as S(0,0). A summary of the analysis in Section 8.1, also corroborated by the data in Table 2.5-4, concludes that there is no more than a 50 percent probability that intrusions will occur, given these assumptions.

Probabilities far less than 50 percent may occur in individual sample runs due to assumption two. Presumably, the uniform random number assumption was included in previous PAs as an attempt to ensure that 30 boreholes per km^2 per 10,000 years is the maximum possible intrusion rate for the simulation.

The use of assumption three is inappropriate because the report does not indicate the types of active institutional controls and their ability to reduce drilling.

Removal of assumption four dramatically increases the maximum possible probability of an intrusion to approximately 98 percent, as shown in Table 2.5-1. This would generate a box plot for the S(0,0) scenario in Figure 8.1-1 that is approximately twice as wide, and increase the upper range of uncertainty for the remaining "intrusion" scenarios. To show the full range of uncertainty in this figure, assumption four should be removed and analyzed for the 10,000-year regulatory period (this point is further addressed in EPA's January 1994 comments on the PA). The use of an intrusion rate of zero for the last 8,000 years is unacceptable.

The time-dependent drilling rate model replaces assumption one with a time-dependent Poisson process. Assumptions three and four are retained for this analysis, but assumption two is not. As a result, the range of uncertainty shown for the "no intrusion" scenario in the expert panel, time-dependent drilling rate model in Figure 8.1-1 is extremely narrow when compared to the constant rate model results.

Response. *The 1992 PA was not intended as a compliance application. Rather, its primary purpose was to provide interim guidance to the DOE about potentially important processes and parameters. Varying the intrusion rate allowed for an assessment of the sensitivity of the performance measure to changes in the intrusion rate. In fact, the intrusion rate was shown to be the single most important parameter in the analysis.*

Assumptions 1 and 3 are based directly on interpretations of regulatory guidance. Assumption 4 was simply based on resource and time constraints. We acknowledge that this approach should have been explained better.

EPA Comment 40. Distribution of Uncertain Variables

The definition of the joint distribution of the uncertain variables in the vector $\underline{x}$ Equation 2.1-4 (Volume 4, p. 2-3) should be defined as a multivariate distribution $f(x) = f(x_1, x_2, \ldots, x_k)$ if correlations between these variables are to be considered. The D_k appearing in the equation would then denote the marginal (subjective) variate distribution assigned to each variable. The distribution f(x) used in the analyses was constructed from the D_k under the assumption that most pairs of variables have zero rank correlation, while others have non-zero pairwise rank correlations.

It should be noted in the text that a large amount of uncertainty exists in the method of constructing of the multivariate distribution f(x) from the univariate distributions D_k. Uncertainty in the assignment of correlations has not been addressed adequately in the report. From the discussion, it appears that the marginal distributions D_k were assigned subjectively with no consideration of possible correlations, then the correlations were added later. Is this true? More information on the assignment of subjective distributions to variables with assigned rank correlations is needed.

Response. *Yes, it is true that the correlations were added later based upon the judgement of individual PIs. But it should be noted that very few correlations were used in the 1992 PA (see Table 3-1, Volume 4).*

EPA Comment 41. Disposal-Room Modeling: General Comments

A weakness in the disposal-room material modeling is the lack of a deformable, porous model. Contaminant transport via brine and gas flow from the site is a threat that could only be realized in the presence of porous, fractured rock (i.e., permeable strata). At the same time, pore pressure has a significant effect on room response. SANCHO lacks porous media modeling capability and does not account for the role of effective stress on strata deformation and possible failure. On the other hand BRAGFLO calculates fluid flow but does not account for deformation.

A consequence of the lack of porous media modeling capability is the inability to incorporate starting formation pore fluid pressures into an analysis (and pore pressure changes). Strata failure induced by repository mining cannot therefore be anticipated.

In view of the potential effects of disposal room excavation and subsequent deformation beyond compaction of backfill and waste within a room, the objective of disposal-room modeling needs to be broadened to include effects of deformation on fluid flow as well as the effects of fluid pressure on deformation. This objective cannot be achieved within SANCHO at present.

Response. *The coupling of flow and deformation effects in the disposal room modeling for the 1992 PA was, indeed, limited. Since the 1992 PA, the WIPP Project has investigated four computational approaches to coupling flow and deformation effects in the disposal room response. The WIPP Project also has ongoing work to model the formation of new fractures or the response of existing fractures to the buildup of gas pressure in the rooms. The new approaches for coupling flow and deformation effects in disposal room modeling are discussed in the responses to Comments 2 and 18.*

EPA Comment 42. Starting Conditions
The actual initial site conditions of stratigraphy, preexcavation stress state, rock mass properties, formation pressure and excavation sequence are seemingly ignored in most of the disposal-room analyses, although Butcher and Mendenhall state in SAND93a, pp. 3-1, that a modified stratigraphy and creep law (R-D model) was used in "all subsequent SANCHO calculations."

Response. *A superior capability of the models to match observed deformations at the site has been developed since the 1992 PA. This capability was achieved by introducing more detailed and complete treatment of the stratigraphy and the material properties, particularly creep models and the representation of thin seams.*

EPA Comment 43. Stratigraphy Ignored
The stratigraphy is largely ignored, ostensibly on the basis of prior analyses which suggested rather tentatively that the presence of various strata besides bedded salt did not have a large effect on disposal-room volume change (SAND 93a, Appendix A). However, analysis with stratigraphy showed that the presence of anhydrite beds changed the time for complete closure to 400 or 500 years from about 195 years. Comparisons at 100 years showed room volume changes of 75 percent without stratigraphy compared with 56 percent with inclusion of stratigraphy. In addition, thin seams modeled as slide lines had a pronounced effect on room deformation, in keeping with mining experience.

Response. *Since the 1992 PA, a more detailed representation of the stratigraphy, particularly the weak interbeds close to the repository level, has been introduced along with revision of the halite creep model, revised material properties, and representation of transient creep.*

EPA Comment 44. Disturbed Rock Zone
The DRZ concept is used explicitly in BRAGFLO where the extent of the DRZ is an input parameter. Since the DRZ is not actually calculated in any objective fashion, how is it determined and what are the consequences of the DRZ extent for BRAGFLO?

If the DRZ is important to performance assessment, then it should be an outcome of numerical modeling of mechanical and hydrologic responses which may be coupled both ways, each affecting the other. A specific equivalent material properties procedure for averaging the heterogeneous DRZ properties to an equivalent uniform zone is needed unless spatially dependent properties are used.

Response. *The DRZ in the underground facility has been characterized by three approaches: visual observation, geophysical methods, and in situ hydrologic testing (Borns, 1985; Bechtel National, 1985; USDOE, 1988). Geophysical studies have utilized seismic refraction, seismic* *tomography, surface wave analysis, electromagnetic (EM) methods, and direct current (DC) methods (Borns and Stormont, 1989; Borns et al., 1990; Holcomb, 1988; Jung et al., 1991; Pfeifer et al., 1989). These studies, when taken in conjunction with the in situ hydrologic tests, define a DRZ extending to a depth of 1 to 5 meters throughout the underground facility (Munson et al., 1995).*

The air intake shaft (AIS) at WIPP has been well instrumented (Munson et al., 1992a) to assist in the characterization of the DRZ around the shafts. Further DRZ characterization in the form of permeability, as well as brine and gas flow testing in the AIS, is planned.

The DRZ extent is important because it provides a potential pathway for brine and gas migration. The BRAGFLO model uses Darcian flow to model brine and gas flow through porous and permeable media. BRAGFLO will be influenced by both the extent (thickness) and physical properties, such as porosity and permeability, within the DRZ (SAND92-0700/2; page 7-2).

Porosity and fractures within the DRZ will influence fluid flow and may provide gas storage volume. DRZ porosity is at the expense of porosity in the room. The issue is how much of it is squeezed out by closure, and whether it can reopen by gas pressurization. While porosity and fracture within the DRZ will influence fluid flow and other factors such as whether communication paths to anhydrite interbeds exist, these parameters are not expected to have much effect on the mechanical part of closure.

No simplified model of the DRZ is presently in closure codes and implementation of such a model would be a major effort. The reader is referred to the Position Paper "Rock Mechanics: Creep, Fracture, and Disturbed Rock Zone (DRZ)" for further discussion of this topic.

The present assessment of the DRZ's role in PA is that it is not important because most of the enhanced porosity is eliminated by closure by the time any substantial gas pressurization of the repository occurs; i.e., the DRZ is assumed to close up rapidly, and it is not an important part of the gas storage volume within and immediately adjacent to the waste (Butcher et al., 1995).

EPA Comment 45. Unclear Disposal-Room Modeling Features

The documentation on room modeling is extensive, but a great deal of information is relegated to memoranda, some of which are included in extensive appendices. These are helpful, but generally contain references to earlier work. The tabulation of parameters in Volume 3 of the 1992 PA was useful but not always complete in the sense that one could not determine whether a parameter of interest was determined by experiment or simply estimated on the basis of experience. For example, the importance of anhydrite bed response has become apparent, so the mechanical parameters that characterize anhydrite behavior are of interest. These are given in a memorandum in an appendix to Volume 3 of the 1992 PA (SAND 92c, pp. A-109, A-124) along with a reference to the source of the data, but no information is given as to whether they were measured or otherwise determined. The same is true of slide-line mechanical properties. Slide-lines are apparently cohesionless, but it is not clear whether separation of the surfaces in contact is allowed. A simple diagram of the stratigraphy used and the corresponding rock properties should accompany each analysis of disposal-room behavior.

Response. *The reports for the 1992 PA were intended to communicate with a broad audience and we have followed the common practice in technical documents of using references to avoid overburdening the report with technical details. Nonetheless, efforts are being made to improve the presentation of data and guidelines have been established for referencing memoranda.*

EPA Comment 46. Gas Pressure in SANCHO
How is gas pressure actually used in SANCHO and related codes which do not have pore pressure capability? Presumably, the gas pressure is applied as a normal traction on the current disposal room free surface. However, what happens when contact is made between room surface and room contents (backfill and waste)?

Response. *SANCHO has been replaced with a related code named SANTOS. In both codes, the available void volume is computed as the difference between the room volume and the solid volume of waste and backfill in the room at each time step. Gas pressure is calculated from this void volume and the gas generation rate. This pressure is then applied as a normal traction on the disposal room surfaces. In SANTOS, a contact surface routine is used to reconfigure the disposal room surfaces and provide for stress redistribution when contact is made between the room surface and room contents (backfill and waste).*

EPA Comment 47. Porosity Surface
The porosity surface calculation in SANCHO and its incorporation into BRAGFLO needs a better explanation.

Response. *A detailed discussion of room closure modeling, including the porosity surface approach, is presented in the Disposal Room Position Paper (Butcher et al., 1995). Briefly, the SANCHO continuum mechanics code was used to make an extensive series of computations of waste (effective) porosity as a function of time and gas generation rate (which determines pressure history). These results were used to compile a "map" of waste porosity as a function of time and pore pressure (the "porosity surface"), which was then used in the porous media flow code, BRAGFLO, to determine waste porosity as a function of these variables. With the porosity surface approach, the effect of room closure is coupled to repository pressure and porosity in the BRAGFLO computation.*

EPA Comment 48. Modeling of Backfill
Another related question concerns the treatment of backfill and the inevitable ~~air~~ gap present between room roof and top of the backfill. Is this gap represented by "air" elements or how are the room contents—air gap, backfill, solid waste—represented in SANCHO? The mesh diagrams suggest air elements, but are they compressed to flatness before contact between roof and backfill is made?

Response. *For the SANCHO calculations, any air or gas in the disposal room is represented as a pressure on the boundaries of the halite or backfill; there are no zones or elements that represent the air (or gas). Within the SANCHO model, a contact surface (permitting sliding or separation, but transmitting compressive stress when contact exists) was used between the mesh in the intact halite and the mesh in the backfill. The initial condition was a separation corresponding to the height of the air gap. Gas pressure was applied as a normal traction on the halite and backfill surfaces. The meshes were free to come in contact and then separate again at later time if gas pressure was sufficiently high.*

EPA Comment 49. Fluid Pressure
Fluid pressure in formation void space, in pores and fractures, needs to be included in the constitutive equations describing (porous) solid deformation. Even in the purely elastic range, fluid pressure needs to be properly taken into account. Pore and fracture fluid pressure is essential to the determination of failure in non-salt beds; it is the effective stresses that should be used in a Druck-Prager yield condition, for example.

Response. *DOE is aware of the importance of fluid pressure in porous material response. Porosity in the halite is quite low. The mechanical properties for halite are drawn from measurements on bulk samples with pore fluid. Recent modeling with improved representation of stratigraphy, including formation specific material properties, and with an improved creep law that incorporates transient creep, has been successful in closely matching observed deformation in the excavated cavities at the site. Development of appropriate models for simulating failure in the non-salt beds is continuing.*

EPA Comment 50. Salt Mine Closure, Subsidence Analogs
Is there evidence that salt mines containing rooms and pillars of similar size close in 100 years or so? If so, it would be helpful to tabulate such observations in support of laboratory creep-rate data and calculated disposal-room closure times.

A survey of subsidence and the potential for caving in mines nearby, or mines in similar geologic settings, would also be helpful. Subsidence analyses would seem in order in any event. Such analyses should address the question of interbed fracturing, especially in the Rustler formation.

The effect of overburden removal from the generic, two-dimensional mesh is of interest. Although the effect on room closure may be small, it should examined to make sure. Any opportunity to remove untested assumptions, explicit or tacit, should be considered.

Test Case

Are there any mines in similar geologic settings where the model could be applied?

Response. *DOE reviewed data from various salt mines early in the WIPP Project. The difficulty with these data is that every case has its own unique features of local stratigraphy, hydrologic regime, and mine design, often with limited documentation. The time scale calculated for WIPP is not inconsistent with other sites in broad terms, but there are no easily comparable cases. At this point in time, a good base of high quality, precisely documented data have been accumulated for the excavations at the WIPP site, and these observations are viewed as the best data set for model validation.*

Consideration is currently being given to issues such as subsidence and the testing of as many modeling assumptions as possible.

EPA Comment 51. SANCHO/BRAGFLO Room Porosity Values (in Appendix B)

Room porosity values for BRAGFLO and SANCHO are different numbers. Appendix B to Volume 4 of the PA (performance assessment) describes the derivation of the relationship between the two porosities. This relationship is in error because it is based on the assumption of a constant volume of solids. In fact, iron and cellulosic wastes are consumed relatively fast in chemical reactions. These reactions are largely responsible for gas generation that, in turn, affects room closure and fluid flow about waste-filled disposal rooms. Whether this inconsistency is important not is unknown.

Response. *For the 1992 PA, the volume of waste solids (exclusive of pore volume) was held constant. Note, that the "consumption" of iron, which is a major part of the gas generation, involves conversion of the iron to various oxidized compounds; hydrogen is released, but the solids remain. Additionally, the chemical reactions referred to are dependent on brine availability. In a large number of realizations, the brine inflow is insufficient to "consume" these wastes.*

EPA Comment 52. Permeability and Porosity

Permeability of disposal rooms containing waste and backfill is presumably related to porosity, although it is not certain how permeability of disposal rooms is determined. While an average porosity for the heterogeneous room contents is useful for gas and brine storage considerations, it is not evident what permeability value of room flow control should be assigned.

The values that are obtained from SANCHO are total porosities, whereas, BRAGFLO uses effective porosities as input parameters. The importance of this is that effective porosities will always be lower than total porosities and will provide less storage within the matrix. If effective porosities had been used pressures would have reached higher levels and the migration potential would have been increased.

Response. *Permeabilities are discussed in Volume 3 of the 1992 PA (see also the response to Comment 130). Permeability and porosity were not coupled in the computations for the 1992 PA; permeability was held constant. Determining exactly what permeability behavior should be attributed to the waste after the material has been compressed and the waste is partially or completely degraded is obviously a difficult problem. However, sensitivity analysis has shown that results are not particularly sensitive to this variable. This is not surprising because the room is surrounded by regions having quite low permeability and flow path lengths much greater than the dimensions of the room. These regions therefore control the total transport process.*

Since the waste and backfill are initially unconsolidated material and retain substantial porosity even after compaction, total and effective porosity are expected to be very close.

EPA Comment 53. Specific Comments on Disposal-Room Modeling

Volume 4, Page 4-17, Lines 31 to 34:

This section states that additional SANCHO simulations are necessary to more adequately describe the deformation of halite when both brine and gas are considered. Such additional simulations would be advisable.

Volume 4, Page 4-23, Line 44:

Here it is stated that BRAGFLO uses a modified rock compressibility, but no rationale or justification is given. Please clarify why a modified rock compressibility is warranted.

Volume 4, Page 4-26, Lines 4 to 7:

This section states that the same sampled values of relative permeability parameters are used for halite, anhydrite, the transition zone, and the DRZ. Conceptually, this does not seem to be appropriate. Page 4-24, footnote 3 states that relative permeability is a function of saturation of the phase of interest. The degree of saturation of a media would depend on the media or rock type, type and amount of fractures and effective and intrinsic porosity. Such parameters would seem to have different values for the halite, anhydrite, transition zone and DRZ. Page 7-2 of Volume 2 states that relative permeability is the ratio of the permeability of the rock with the fluid at a given saturation to the permeability of the material when it is 100 percent saturated with the fluid. Again, such parameters would seem to have different values for different materials having different permeabilities. Please explain.

Volume 4, Page 4-26, Lines 10 and 11:
The parameter values for relative permeability are stated to be based on the surrogate materials identified in Section 2.3.1 of Volume 3 of the 1992 PA. Section 2.3.1 states that a low permeability sand was used to determine the relative permeability parameters of the halite and the anhydrite. Does this include the halite and anhydrite which comprise the transition zone and the DRZ? If so, please clarify the appropriateness of applying the same surrogate material to both the undisturbed media and the disturbed media The undisturbed halite and anhydrite should by definition have a much different effective porosity and permeability.

Response. *The responses to these four specific comments are as follows:*

(1) Since the 1992 PA was published, SANCHO was used to make an extensive series of computations of waste (effective) porosity as a function of time and gas generation rate (which determines pressure history). These results were used to compile a "map" of waste porosity as a function of time and pore pressure (the "porosity surface"), which was then used in the porous media flow code, BRAGFLO, to determine waste porosity as a function of these variables. A detailed discussion of room closure modeling, including the porosity surface approach, is presented in the Disposal Room Position Paper (Butcher et al., 1995).

(2) The reference to use of modified rock compressibility in BRAGFLO simply indicates that the code is formulated with compressibility defined as the relative change in porosity produced by a change in pressure (one over porosity times the partial derivative of porosity with respect to pressure, Equation 4.2-7 on page 4-23 of Volume 4), rather than the more common definition of compressibility (the partial derivative of porosity with respect to pressure) given by Equation 4.2-6 on the same page. The reference to this as a "modified" rock compressibility is not an ideal choice of terminology. No modification is made to the measured properties of the rock. The point is simply that the "compressibility" value input to BRAGFLO is not the same as the value that would be used in a code formulated with compressibility defined by Equation 4.2-6.

(3) Lines 4 to 7 on page 4-26 in Volume 4 are intended to indicate that the same relative permeability description is used for the halite, anhydrite, transition zone and DRZ for any single realization. There were no cases in which some of these regions were described using Brooks-Corey, while others were van Genuchten-Parker. The intrinsic permeability was different for each region and saturation was allowed to vary independently. The parameters for the relative permeability model were sampled independently for each material, but, for lack of more specific information at the time of the 1992 PA, the same ranges were sampled for all 4 regions.

(4) This does mean that, as indicated on lines 10 and 11 of page 4-26, the relative permeability characteristics of a low permeability sand were applied to undisturbed halite and anhydrite and to the transition zone and DRZ. However, as is stated on pages 4-8 through

4-11 of Volume 4, the halite, anhydrite, and DRZ intrinsic permeabilities and porosities were sampled over very different ranges. Please see EPA Comment 138.

EPA Comment 54. Disposal Room Modeling

Volume 5, Page 2-9, Table 2-1:

This table provides a list of the assumptions used in BRAGFLO for the Undisturbed Performance analyses. The information provided in the table appears to be a shortened version of the more detailed information provided in Volume 4, Chapter 4. No information is provided, however, regarding the assumptions used in SANCHO. As stated on page 2-1, of this volume, SANCHO is used to simulate halite creep and its modelling results are incorporated into BRAGFLO. Some of the assumptions of SANCHO, however, appear to be incompatible or inconsistent with the assumptions used in BRAGFLO. In addition, similar to BRAGFLO, the impact of the SANCHO assumptions are difficult to quantify. To not include the SANCHO assumptions in this table seems to negate their importance on the Undisturbed Performance analyses.

Response. *The recently published Position Papers and the DCCA contain expanded discussions of the assumptions and limitations for the current PA models. The need for more complete explanation of the impacts of modeling assumptions on compliance is noted for future code documentation and the final compliance certification package.*

EPA Comment 55.

Volume 5, Page 2-9, Line 25:

Please specify which "selected regions" are adjusted with respect to permeability and porosity.

Response. *Regions that are backfilled with crushed salt for sealing purposes had permeability and porosity adjusted to reflect the creep compaction of the seal.*

EPA Comment 56.

Volume 5, Page 4-18, Line 20:

Should this line reference Cell 4 rather than Cell 6?

Response. *The line in question does refer to Cell 4, not Cell 6.*

EPA Comment 57.

Volume 5, Page 4-50, Lines 1-3:

This sentence states that conditional on conceptual models and parameter distributions, creep closure modeling may not be necessary. The phrase "...conditional on conceptual models and parameter distributions..." should be clarified. Information regarding the incorporation of SANCHO results into BRAGFLO (provided in Volume 4, Pages 4-15 through 4-18) should be referenced in this volume (either here or in Section 4.1). The application of SANCHO simulations into the BRAGFLO simulations is an initial attempt and, as stated on Page 4-17 of Volume 4, strictly applied only to the case where the pore space is occupied by gas, rather than where pore space is occupied by both gas and brine. Given the discussion of the inconsistencies between the SANCHO and BRAGFLO assumptions, it is not timely to make the statement that creep closure modelling may not be necessary.

Response. *The sentence in question appears in a section of text introducing the discussion of results from sensitivity analyses using a major excursion in closure characteristics (waste panel porosity set to the post-closure condition from time zero). The sensitivity analysis shows that even this very large (indeed, unrealistically large) excursion in the effects of creep closure had minimal impact on the results from the 1992 PA models for undisturbed performance. Both transport and closure modeling are being revised and expanded since the 1992 PA. The statement in the 1992 PA was intended to apply only to the status of the modeling as it existed at that time.*

EPA Comment 58. DRZ Zone Not Consistent in Scenarios
Volume 4, page 5-4, Figure 5.1-2 and Figure 5.1-3:
There should be a DRZ between the waste panel and the halite in the center of the panel model. The undisturbed case has the DRZ in this location and it seems it should also be in the disturbed case. Line 15, page 5-3 states that "a disturbed rock zone (DRZ) surrounding the waste-storage area" is not shown in the figure as described.

Response. *The portion of the DRZ at the radial boundary of the waste panel, between the repository and the halite, was inadvertently omitted from the referenced figure.*

EPA Comment 59. Conserving Void Volume
Volume 4, pages 4-13 to 4-23
The porosity conversion outline in Equation 4.2-2 and amplified in Appendix B seems unreasonable. What does it mean to conserve void volume while adjusting the porosity value in volume?

Response. *In both SANCHO and BRAGFLO, the porosity of a mesh element is the ratio of the pore volume to the total volume of the element. However, the mesh element volumes in SANCHO and BRAGFLO are not always the same. Even if the initial meshes are created so that the volume of elements are initially equal, the mesh for SANCHO deforms with time while the BRAGFLO mesh does not. The porosity for the cell in BRAGFLO that contains the* *repository is calculated so that BRAGFLO has the same total repository void volume (as a function of time and pressure) as that found in SANCHO.*

EPA Comment 60. Creep Closure Effects
Volume 4, pages 5-18 & 5-33
It is our understanding (see earlier comments) that BRAGFLO does not actually account for creep closure but only implements the effects of creep closure passed by SANCHO which does account for creep closure. This effect in SANCHO is passed to BRAGFLO by an adjustment of the porosity values calculated. As stated on page 4-18, line 7 the ultimate effect of this implementation is unknown? Could this effect the implementation of the creep closure to make it a "pseudo-closure" in BRAGFLO that is incorrect?

The present implementation of creep closure appears to be so incomplete that the conclusion, "Overall, dynamically modeling creep closure results in only minor differences compared with using a fixed porosity," is the only conclusion DOE/SANDIA can make. Could the "minor differences" be a result of the limited ability of the '92 code? It seems inappropriate to draw such conclusions from an admittedly faulty model (page 4-18).

Response. *It is not really correct to say that BRAGFLO does not "account" for creep closure. BRAGFLO does not compute closure. Creep closure is accounted for in BRAGFLO by using a time and pore pressure dependent repository porosity, which is drawn from the results of calculations made with SANCHO. This is a real closure, not a "pseudo-closure". Although it may not be identical to the closure that would be computed by a fully coupled flow and deformation code, at least it is a reasonable approximation in terms of transient behavior.*

As discussed in the response to EPA Comment 57, sensitivity analysis with a fixed porosity, which produced a large excursion in the transient repository pressure, showed only minor differences in release volumes. It is certainly possible that this result stems from limitations in the ability of the 1992 PA codes, but it is unlikely that it is the limited coupling of the closure computation that is primarily responsible.

EPA Comment 61, Performance Assessment Representations Used in 1992
There appear to be some small discrepancies in the numbers presented throughout Tables 2.5-1 through 2.5-4. In Table 2.5-2, for example, using 3.78×10^{-4} for the constant drilling intrusion rate, the probability of zero intrusions in 9,900 years is calculated as $\exp\{(-9900)(0.126)(30)/10000\} = 0.0237019$ using a simple hand-held calculator. Tables 2.5-2 (column with footnote b, line 17) and 2.5-4 (column with footnote b, line 18) report 0.02378, which is a small difference, but it seems too large to be accounted for by rounding error alone. Similar comments apply throughout these tables.

In Tables 2.5-1 and 2.5-3, using a time-integrated intrusion rate of 0.99 per km^2 per 10,000 years as shown in Figure 2.5-1 (1 at 10,000 years minus .01 at 100 years), the probability of zero intrusions may be calculated as exp{(-9900)(0.126)(0.99/10000)} = 0.883828. The tables report 0.8703. These discrepancies may be removed by one of the following steps: report fewer digits in these tables; refine the calculation procedure to eliminate this annoying discrepancy; or explain the calculation procedure better, if the values in the tables are accurate.

Response. *Thank you for bringing these discrepancies to our attention. In the future, the tables will be checked for consistency and/or fewer significant figures will be reported to eliminate this problem.*

EPA Comment 62.

Use of Median Values for Variables Not Sampled

In the evaluation of sensitivity of the model to variations of a given input parameter, the other variables in the model are held to constant values as the parameter in question is varied. WIPP procedures call for holding the other variables at their median value rather than at the expected value. Please explain the reasoning for this decision.

Sampling Error for Estimated CCDFs

The following statement appears on page 3-21: "...the use of a Latin hypercube sample of size 70 to assess the effects of subjective uncertainty has *no effect on the estimation* of the 0.1 and 0.001 exceedence probabilities in the individual CCDFs used in comparison with the EPA release limits." (Italics added.) Although the italicized phrase is ambiguous, this assertion appears to be based on the faulty premise that individual CCDFs should be compared to the release limits.

The LHS sampling is very important for comparison of PA results to the EPA release limits. While the separation of stochastic and subjective uncertainty is a noteworthy achievement of the WIPP PA process, the point of LHS sampling is to determine the uncertainty inherent in any comparison made with the EPA release limits. Individual CCDFs developed using LHS are less important than the summary CCDFs derived from them. A summary CCDF, such as the mean CCDF, will probably be used to compare results of the PA to the Standard. LHS is an important tool for assessing the degree of uncertainty in the estimated mean CCDF resulting from the sampling. Procedures for estimating the confidence interval for the mean CCDF from the LHS results should be established.

Response.

Use of Median Values for Variables Not Sampled

The median value is precisely defined given the probability distribution. The expected value is less clearly established for many variables, especially at the time of the 1992 PA. The decision *was therefore made to use median values in order to provide a systematic and consistent approach.*

Sampling Error for Estimating CCDFs
The PA for the compliance certification package will include activities to estimate the uncertainty and confidence interval that are relevant to comparison of CCDFs with the EPA release limits. However, in accordance with the direction given above and in 40 CFR 191, a mean CCDF will be used to compare results of the PA to the Standard.

EPA Comment 63.
Comments on Table 3-1 Variables
Comments on uncertain variables are limited to only those few on which information was readily available. No attempt has been made to address certain generic questions regarding these variables such as: (1) are some of these unnecessary; (2) should additional ones be added; and (3) is there a problem of co-variance that is being ignored (i.e., two or more variables that are really correlated but are being sampled here as if they were uncorrelated). These three items eventually need to be thoroughly evaluated. Also, the distribution of values for each of these variables needs to be evaluated from both WIPP and broader scientific literature.

BHPERM
Borehole permeability has been identified by SNL as one of the more sensitive parameters for performance assessment. The range of values used (1×10^{-14} m^2 to 1×10^{-11} m^2) is said to represent silty sand as defined in Table 2-2 of a book by Freeze and Cherry. The range of values actually shown in Freeze and Cherry is $\sim 7 \times 10^{-14}$ m^2 to 7×10^{-11} m^2.

The assumption that any abandoned borehole is filled with silty sand is a very unconservative assumption and does not appear to be justified from either the site conceptual model or from the site data. Although the 1985 EPA Guidance indicates that it is not necessary to assume that the borehole is completely open, but rather that it may be filled with soil or gravel, the DOE takes the least conservative assumption regarding the EPA guidance by adopting borehole permeabilities as those typical of a silty sand. The intrusion that may disturb the repository will, at first, be essentially open and over time will eventually be filled or sealed by collapse of the borehole. The DOE does not present any rationale as to why it was assumed that the borehole should have such a low permeability.

BPPRES
The range given for the initial pressure in a brine pocket in the Castile Formation is 13 to 21 MPa, with a median of 17. The range given in two places in Volume 3 is 11 to 21 MPa with a median of 12.6. The values used in Volume 3 are supported by the data presented in

DOE 83 and appear to be preferable. The reason for the discrepancy between Volumes 3 and 4 is not known.

BPSTOR

It is not clear how the range (0.02–2.0 m^3/Pa) and median (0.2 m^3/Pa) for the bulk storativity were developed from the reference (DOE 83). The overall flow data for WIPP-12 suggest a value of 0.04 m^3/Pa, and a value for the small fracture portion of the system of about 0.07 m^3/Pa (page H-54). Is this a variable that needs to be sampled?

BRSAT

Initial fluid (brine) saturation of the waste was assumed to range from 0.0 to 0.14 with a median of 0.07. These values were obtained purely by PA Investigator Judgement. Yet this parameter is listed as important to compliance with 40 CFR 191 and (in Volume 5) as very important to compliance with 40 CFR 268.6. Also, the Experimental Program Plan does not mention plans to study this parameter. There is not adequate information for this important parameter. In the absence of data, it would seem appropriate to determine and use a plausible value that would result in the greatest consequence.

DBDIAM

It is noted that drill bit diameter is the only one of the 49 variables sampled that is not listed in Table 9-3 as being important for 40 CFR 191B. Is this an oversight?

FKDxx and MKDxx—DISTRIBUTION COEFFICIENTS

All of the fracture and matrix distribution coefficients for the individual elements are listed in Chapter 9 as important or very important to PA. For the 1992 PA, those values were determined based on expert judgement by SNL scientists. These parameters are too important to be determined in this manner. Laboratory studies are underway and the data obtained should be used in future performance assessments.

SOLxx—Radionuclide Solubilities

The values of plutonium solubilities, developed by SNL expert opinion were considerably less than those from ongoing SNL-supported studies at Lawrence Berkeley Laboratory. Future performance assessments should not rely on expert judgment if data is available.

Response.

a. Comments on Table 3-1 Variables

(1) Are some of the variables unnecessary?

The goal of the PA analyses was to be as complete as possible in identification of variables, even though some of the variables may be less important than others in term of model sensitivity.

(2) Should additional variables be added?

As alternative conceptual models are evaluated and incorporated in the PA analyses, additional variables may need to be added. The current variable identification is consistent with the needs of the current models. Future calculations may require different input parameters as more sophisticated models are developed and tested.

(3) Is there a problem of covariance, in that covariance is being ignored (i.e., two or more variables that are really correlated, but are being sampled here as if they were uncorrelated)? The effects of not including variable correlations are being investigated.

b. BHPERM

Volume 3, Section 4.2.1, indicates that the selected value of borehole fill-material permeability was based on the investigator's judgment that the borehole fill material would be a "silty sand" (as identified by Freeze and Cherry). For the purpose of the initial sensitivity studies, the choice of a relatively low-permeability material (silty sand, for example, instead of a gravel) is not unreasonable, since a silty sand is approximately mid-range on a scale of potential unconsolidated, granular materials expected to degrade from the borehole wall or be placed in the borehole upon borehole abandonment.

The permeability of silty sand is also felt to be conservative given the range of expected borehole fill materials. The borehole fill is expected to be a granular and uniform material. Its physical characteristics will be determined by grain size and uniformity of the source material from which it is derived. If a concrete plug is placed in the borehole, it will degrade to a material similar to silty sand after hundreds of years. If the borehole is unfilled, the borehole is closed by salt creep. This would result in a lower permeability than a silty sand. A range of borehole fill-material permeability values will be modeled in future PA analyses.

The description of initial borehole plug permeability on page 4-4, Section 4.2.1, Volume 3 also indicates that plug permeabilities depend strongly on the host rock in which the plug is emplaced. Because most experimental studies of plug-borehole interaction extend for only hundreds of days or less, data are limited. Any PA calculations that start from initial conditions assume permeabilities of 10^{-15} m^2 for plugs in the Castile Formation and 10^{-18} m2 in the Salado and Rustler Formations. These values are more conservative than the silty-sand permeability used in the PA modeling.

c. BPPRES

The correct distribution is identified in Volume 3, Section 4.3.1. The data in Volume 3 are based on WIPP-12 and other borehole measurements. The higher pressure value (from Volume 4, Table 3-1) was used in the PA and represents a more conservative value in terms of potential releases. The data range included in Volume 4, Table 3-1, however, appears to be incorrect, an error which was missed during the consistency checks on the data in the PA report.

d. BPSTOR

The brine pocket storativity is used to determine the amount of brine that is potentially available for upward flow in an intruding borehole. It is a necessary parameter in evaluating the disturbed scenario. It is sampled because the 1992 PA evaluated a range of possible values for the CCDF development.

e. BRSAT

The brine saturation in a hypothetical brine pocket below the repository is an important parameter for the disturbed scenario. Since the 1992 PA, data have been obtained through additional research involving literature reviews and experimentation. Estimates of the range of initial brine saturation have been reduced to between 0.004 and 0.052 based on this more recent EEG data (Howarth, 1994). The new range of values is used in the 1995 Draft Compliance Certification Application (DCCA, 6-115).

f. DBDIAM

The drill bit diameter is one of the 49 variables sampled in the 1992 PA. It was an oversight that this variable is not included in Table 9-3.

g. FKDxx and MKDxx—DISTRIBUTION COEFFICIENTS

Improved values of fracture and matrix distribution coefficients for the individual radionuclides will be included in future PAs as data become available from the ongoing laboratory studies.

h. SOLxx—Radionuclide Solubilities

Improved values of actinide solubilities will be included in future PAs as data become available from the ongoing laboratory studies.

EPA Comment 64. Initial and Boundary Conditions

The initial conditions which are assumed for the scenarios will significantly affect the modeling results. However, the modeling that was described in the 1992 PA to obtain the initial conditions (e.g., repository pressures and brine saturations) provides only limited rationale for the assumptions. The approach needs to be better described in general. Furthermore, it appears that none of the considerable uncertainty which is associated with the initial conditions has been quantified. As much emphasis should be placed on obtaining the initial conditions for brine saturation and pressure distributions as any other aspect of the modeling. While this refers to BRAGFLO, initial and boundary conditions for all models should be thoroughly evaluated before they are chosen, and fully discussed in future reports.

Mechanical modeling of disposal room and rock mass motion with SANCHO apparently uses room gas pressure as a boundary condition. This varies in time as consequence of gas generation which is also variable. Rock mass and room contents are deformable, porous media through which seepage of gas and brine occurs. As a consequence, there is a difference in total stress, effective stress and fluid pressure and corresponding differences in boundary conditions. Also, effective stress should be used in the constitutive equations. However, these distinctions are not made in SANCHO nor does BRAGFLO accommodate deformation. The consequences are unknown, but may be worth examining.

At the end of 50 years of operation, the DRZ is considered to be "dewatered" and only partially saturated with brine; the fluid pressure is reset to atmospheric pressure. These assumptions and decisions should be evaluated in light of actual calculations. While the fluid pressure in the DRZ may decrease from ambient towards the excavation, it is not certain that desaturation follows.

The regional boundary conditions will have an effect on releases to the accessible environment. Currently, there is a great deal of uncertainty associated with regional boundary conditions, yet the effect of this uncertainty of the boundary conditions on the modeling results has not been evaluated.

Response.
a) The basis for the definition of PA model initial conditions is provided in the source documents cited in the 1992 PA reports. A summary of results from these source documents and the rationale that leads to the selection of the parameter distributions used as the model initial conditions will be included in future PAs or in related reports.

The boundary conditions depend on the specific model or code. For mechanical models which calculate the rock deformation, the boundary conditions include either the stress or velocity conditions applied to the external and/or interior boundaries of the model. For hydrological models, the conditions include either the hydraulic potential or the fluid flux at the external and/or internal boundaries. Boundary conditions also need to be consistently applied between models for regional-scale, local-scale, repository-scale, panel-scale, and room-scale phenomena. The CAMCON system assists in defining and documenting the appropriate interfaces between the various PA models, particularly in terms of data transfer for boundary-conditions. See also the responses to EPA Comments 25 and 117.

b) The pressure-time history in a disposal room is appropriately modeled as an internal pressure boundary condition in the SANCHO code. SANCHO computes disposal room closure (and expansion when room pressure exceeds lithostatic pressure) in response to the total stress field in the rock. The validity of this approach is demonstrated by the excellent agreement between SANCHO calculations and in situ measurements of room closure at the WIPP facility. The results of SANCHO calculations are not used directly in the PA analyses, but are summarized as a porosity surface for the BRAGFLO code. This porosity surface provides BRAGFLO with the capability to account for the effects of room closure and gas pressure on porosity and permeability of the waste. See also the resolution of EPA Comment 18.

There is a difference in total stress, effective stress, and fluid pressure and corresponding differences in boundary conditions. SANCHO calculates the total stress field, NOT the effective stress (total stress minus fluid pressure). SANCHO, therefore, does not include effective stress in the definition of the boundary conditions or in its constitutive equations.

BRAGFLO does not calculate deformation, but the effects of deformational changes are passed from SANCHO. The BRAGFLO fluid flow boundary conditions are developed from SANCHO input, in particular the internal (room) boundary conditions for fluid pressure (or hydraulic potential) and the initial conditions for porosity.

c) BRAGFLO calculations for the 1992 PA and all subsequent PAs indicate that the disturbed rock zone (DRZ) will be unsaturated. As indicated in Volume 2, Section 2.3.5, page 2-55, brine seepage from the Salado Formation will have filled fractures in anhydrite interbeds above and below the emplacement horizon [Lappin et al., 1989, and Rechard et al., 1990b]. Brine will also flow into the excavated disposal rooms/panels. The computational and experimental data indicate that the DRZ will indeed be desaturated.

d) The region boundary conditions will have an effect on releases to the accessible environment. Volume 2, Section 2.2.3.6, page 2-30 describes the process used to incorporate the regional potentiometric conditions in the repository-scale models. The uncertainties of regional-scale boundary conditions and how they are incorporated into local- and repository-scale models are currently being studied with three-dimensional, regional models that are designed to enhance the conceptual understanding of the site.

EPA Comment 65. Results and Discussion (Undisturbed Performance)
A major point in the discussion of results is the rise in gas pressure that in some cases exceeds lithostatic pressure. Pressure in excess of lithostatic could conceivably lift the overburden, arch the surface, and stress intervening strata in tension (or open vertical joints). Such an outcome is the opposite of subsidence. Neither are considered in PA uncertainties.

Whether the pressure increases at a sufficient rate for fracture depends on treatment of the rock mass as a porous or non-porous medium. In fact, the rock mass is a porous, permeable medium, so it may be the effective stress that needs to be considered in fracture analysis, not the total stress.

Response. *The effect of disposal room/panel gas pressures when above the lithostatic pressure is currently under study. The opening of fractures by gas pressurization is considered to occur primarily within the interbeds because they contain evidence of preexisting fracturing and therefore have very low tensile strength. A discussion of the interbed fracture flow model can be found in the Position Paper on Salado Formation Fluid Flow and Transport. See also the resolution to EPA Comment 15.*

EPA Comment 66. Radionuclide Inventory Available for Removal: Cuttings Release
The values in expression 7.3 -2 on page 7-13 are taken from the 1991 PA report. The values from Table 7.3-3 would be more appropriate. The scale factor is 39.32 and is the same at all time periods because of the manner in which the table was calculated.

Response. *The numbers used in Equation 7.3-2 should have been updated. Care will be taken not to repeat the mistake in later documents.*

EPA Comment 67. Decay of Radionuclide Levels
The method used for decaying the projected activity levels in Table 7.3-3 is not correct for the WIPP inventory for reasons explained below.

All activity levels in the Table are decayed at the same rate for the entire inventory[1]. These overall decay rates are correct for the inventory being used. However, the waste containers with the high initial radioactivity levels do not contain the average mixture of radionuclides. This is apparent by comparing the isotopic mix of radionuclides at the Savannah River Site (SRS), the origin of most of the high-curie containers. At SRS, 96.9 percent of the Curies are from 238Pu (half-life of 87.7 years) and only 2.8 percent are from 239Pu and 240Pu (half lives of 24,100 years and 6,560 years). Related values for the entire inventory are 72.6 percent 238Pu and 10.4 percent 239Pu and 240Pu. Because of these differences in half-life, the high activity containers will decay at a much faster rate than the inventory as a whole.

The decay values for the Activity Levels in Table 7.3-3 should be recalculated using the actual radionuclide compositions that exist for each of the initial activity levels chosen.

Response. *The "average for CH waste" decay shown in Table 7.3-3 of V.4 is the __total__ activity of the radionuclides shown in Table 7.3-1. The decay chains were simplified to include only long-lived alpha-emitters, and decay was calculated using the activity form of the Bateman equations (see Volume 2 of the 1992 PA report).*

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[1] Inspection of Volume 4 Table C-6 in Appendix C shows a constant proportion of contribution from each radionuclide across all sampled vectors. This lack of variation in activity levels within the repository at the selected time of intrusion indicates that the waste characteristics have not been modeled adequately. Development of an alternative approach to modeling activity levels should be considered. As a last resort, such variations could be captured by adding one or more uncertain activity variables to the LHS procedure.

This comment assumes that loading will be such that waste from one generating site (e.g., SRS) will be concentrated in one area. This situation is not reflected in Table 7.3-3. The activity levels assume a normal distribution of activity throughout the repository, and reflect the relative amounts of lower specific activity and higher specific activity in the waste. They are not "actual" activity levels because the loading pattern is not pre-determined. The activities shown in Table 7.3-1 will be modified and amended as the WTWBIR is developed.

EPA Comment 68. Discussion of Activity Levels Procedure

It is not obvious what the overall effect of using recalculated decay activities will be. Obviously, the activities at later time periods in Activity Level Four will be less if a more rapidly decaying radionuclide composition is used and this will reduce calculated release from Activity Level Four. However, at lower activity levels the reverse is true and these are the much more probable occurrences. For example, consider the radionuclide components for 66,030 stored INEL drums (page A-10 in DOE/WIPP 91-058) which have an average of 5.027 Ci/drum total and 2.119 Ci/drum of alpha-emitting transuranics. These drums will have about 2.4 times the fraction of the initial activity remaining at 1,000 years to 7,250 years as is indicated for Activity Levels One and Two in Table 7.3-3. These two activity levels have a probability of occurrence about 125 times that of Level Four.

Other ways in which the Activity Levels could be addressed should be considered. It is useful to compare the curies that might be brought to the surface at 125 years with values calculated by SNL. For a 0.1 m^2 borehole (the size used in the 1992 PA), the Table 7.3-3 value would be 79.9 Ci. Drilling through one drum that contained 1,100 Ci 238Pu at time 0.0 would bring 166 Ci to the surface at 125 years. If three such drums were intercepted by this drill bit (this would be a low probability occurrence but would be possible and permissible under current WIPP procedures for emplacing wastes) the total would be 498 Ci. For drums containing 1,000 Ci of 241Am, the activity brought to the surface at 125 years would be 332 Ci for one drum and 996 Ci for 3 drums.

A borehole through an RH-TRU canister that initially contained 23 Ci/l of 137Cs and 137mBa (the maximum permitted at WIPP) would bring 43 Ci of 137Cs to the surface at 125 years.

The procedure used assumes that the various activity levels are placed randomly in the repository which may or may not be the best assumption. Note that if it is assumed that a three-drum high stack in the repository would never contain more than one drum with greater than 100 curies, the 5,159 drums in this activity level (four) would have a surface area of 1,326 m^2, or 0.0105 of the total waste surface area (rather than 0.0060).

The New Mexico Environmental Evaluation Group (EEG) has considered intrusion scenarios where a borehole intrudes into a waste room location that has a concentration of average activity drums from SRS and Los Alamos National Laboratory (LANL). The basis for this assumption is that when seven-packs come into the repository together, they will be emplaced together. Thus, different locations will have somewhat higher or lower average concentrations.

In the absence of load management, the possibility and consequences of waste emplacement where average concentrations within a room or panel vary significantly from the average needs to be evaluated (Chat 88).

More information on the distribution of radioactivity levels within the various activity levels would be helpful. The alpha curies in Activity Level Four are equivalent to 217 Ci/drum (at time zero) with the calculation used. Is this too low? Too high? Or about right?

Because alternative procedures for developing activity levels have not been evaluated and presented, it cannot be concluded at this time that the procedure used, even after correcting the decay values, is the most appropriate one.

Response. *The DOE has developed a detailed radionuclide inventory in the WTWBIR and Revision 11 of the IDB. The radionuclide inventories in both documents are being developed from the same data call to ensure consistency. These documents present the CH and RH radionuclide inventories by isotope, site, and total for the WIPP. This new inventory will be used by Performance Assessment to model releases under various scenarios.*

Any increased precision in determining what drums (or rooms) would be breached by a cuttings scenario implies a load management scheme, which does not exist. Load management in the repository is not planned at this time. If the human intrusion scenarios indicate that load management is a necessary activity, DOE will conduct further evaluations to determine appropriate load management options.

Since completion of the 92 PA, the estimates of the waste inventory and activity levels for WIPP have been updated and revised several times; therefore, it may not be worthwhile to go into specific calculations. However, with regard to the question posed by the reviewer on activity level per drum, the estimate of 217 Ci per drum for the RH waste at time zero appears to be correct.

EPA Comment 69. Total Release to Accessible Environment: LHS Sampling Error
No information on sampling error for LHS-derived estimates are given in the current PA. Summary curves, such as the mean or percentile CCDFs, are generated by averaging the results of 70 LHS trials. It is expected that the estimated mean CCDF will have a relatively wide uncertainty region due to sampling variation from a relatively small sample size. After analysis of sampling variations, it may be found necessary to reduce the magnitude of sampling error by increasing the LHS sample size. Even if the increased sample size did not

dramatically affect the estimated mean, the estimated confidence intervals could be reduced substantially by increasing the sample size.

Response. *The 1992 PA was one of a series of annual PA updates from 1990 through 1992. Its purpose was to evaluate and improve the existing PA models and computational strategies, but it was never intended to demonstrate compliance. Given this purpose, the use of 70 LHS trials was very appropriate. The specific number of LHS trials for the final compliance calculations will be determined on technical merit and through consultation with the EPA and the stakeholders.*

EPA Comment 70. Discussion
This chapter reflects on the findings of the Uncertainty and Sensitivity Analyses in Volume 4 and on the implications for future studies and demonstration of compliance with 40 CFR 191. The current PA is based on the assumption of Darcian flow within the Salado. However, other conceptual models exist for flow and transport within the Salado at the WIPP in which flow is non-Darcian. It would have been very helpful if Chapter 9 included a discussion which addressed how some of the results may have changed if alternative conceptual models had been adopted.

The numerous references to what will be modeled in future performance assessments should be summarized in Chapter 9 (e.g., interbed fracturing p. 4-8).

Response. *The WIPP Project has investigated numerous conceptual models since the 1992 PA was performed. For example, the WIPP Project is currently considering three alternate conceptual models for fluid flow in the Salado: a far-field (Darcy) flow model, a redistribution model for the DRZ and a clay consolidation model. These three models have been discussed in detail with the EPA through technical exchange meetings and with stakeholders through the SPM process. However, it would have been inappropriate to speculate in the 1992 PA report on "how the results may have changed" until the studies and analyses of alternate conceptual models were completed.*

The status of the current PA models is documented in the Position Papers for the SPM and in the DCCA. The DOE has no plans to modify the 1992 PA report with discussions of changes for future PA analyses.

EPA Comment 71.
Five parameters are said to be important when analyzing the repository/shaft barrier effect: (1) drilling intensity; (2) intrusion borehole permeability; (3) Salado (marker bed) permeabilities; (4) radionuclide solubilities; and (5) drill-bit diameter. Sandia observes that only solubility can be affected by actions taken in the repository. Only Salado (marker bed)

permeabilities can be reduced in uncertainty by continued in-situ investigation. The other three parameters are determined by regulatory guidance. Comments on the SNL discussion of each of these parameters are given below.

Drilling Intensity
It is correct that drilling intensity will probably be decided by regulatory guidance. However, these are references to taking credit for passive institutional control, which also will likely be influenced by regulatory guidance. It is implied in more than one place in Volume 4 that the only reduction in drilling intensity being used in the 1992 PA is due to passive institutional controls. This is incorrect. Actually, the major reduction is due to use of a much lower raw drilling intensity that was developed by expert panels. In addition, the PA has not provided any plans for passive controls and so no credit for reduced drilling should have been taken.

Salado (Marker Bed) Permeabilities
It is agreed that this is an important parameter that needs continued study to reduce uncertainty. The statement is made (bottom of page 9-4) that this is not a parameter that can be changed by action taken within the repository. While this is true for the permeability value itself, there are engineering alternatives that could be employed to reduce the void space in the repository and thereby reduce the amount of brine that flows through the wastes.

Drill Bit Diameter
The entire drilling area needs further evaluation. Drill bit diameter and erosion are the best understood mechanisms at present. There are plans to study spalling phenomena. There is also a need to model the quantities of pressurized brine from waste rooms, Marker Bed-139, and Castile brine reservoirs that will flow to the surface during typical drilling operations. Also, activity levels in cuttings need to be re-evaluated. This study should include the effects of a variable inventory and "non-average" waste emplacement in the repository.

Some documentation should be referenced or provided regarding the specific comments or major contributions that the various experts listed in the front of the document have made regarding the 1992 PA. This would help the EPA to access the level of independent review that the studies and activities described in the PA have received.

Response.
Drilling Intensity
The major reduction in drilling intensity for the 1992 PA is due to the guidance from the Futures Panel and the Markers Panel.

The charge given to the Futures Panel was broader than a limited consideration of current oil and gas drilling rates and included identification of possible future societies and how they may intrude over the next 10,000 years. Extending current drilling rates for the next 10,000 years and using them in performance assessment calculations assumes that drilling for oil and gas *will continue at the same rate as today. One justification for using drilling rates lower than EPA's maximum is that the world may move away from a petroleum-based economy within hundreds of years.*

The extent to which passive institutional controls may deter inadvertent human intrusion, and thus reduce the intrusion rates used in the calculations, is based on the durability of the markers and their ability to communicate correctly over long periods of time.

The 1992 PA presented calculations and results for intrusion probabilities estimated for cases with and without passive markers. Complete modeling of repository performance must include the effects of the entire system. Determining the effects of the Passive Institutional Controls is important in consequence analysis.

Work is continuing, through PA efforts, to strengthen the DOE position on quantitative results from passive institutional controls for inclusion in disposal system performance predictions.

Salado (Marker Bed) Permeabilities
Westinghouse Isolation Division is currently studying engineered alternatives, including various engineered backfill materials, that will reduce the void space in the disposal rooms and help to isolate the waste from brine inflows.

Drill Bit Diameter
The 1992 PA was only a preliminary analysis and it was recognized that additional work was needed. Some of the points raised in this comment, such as the issue of pressurized brine reaching the surface, have already been dealt with elsewhere in our responses (see, e.g., response to EPA Comment 10). Efforts are currently underway to gain a better understanding of releases from spallings and cuttings. The assumption in the 1992 PA that all waste was homogenized is still in use in current PA analyses.

Documentation
The DOE will consider the suggestion that the authors of major contributions be identified in the PA documentation.

EPA Comment 72.
Volume 4, page 2-18
The last paragraph on this page provides the rationale for selecting only one intrusion time (i.e., computational cost), but does not provide adequate rationale for why 1,000 years was selected. An E1E2-type scenario simulated at 101-500 years would have resulted in higher cumulative releases. The gas generation reactions would not be brine limited and there would already be a pathway to the Culebra.

Response. *The 1992 PA was only a preliminary analysis and it was recognized that additional work was needed in many areas. With regard to the intrusion time, subsequent PA calculations have expanded the number and range of intrusion times. For example, the PA for the DCCA considers intrusions occurring at 100, 125, 175, 350, 1000, 3000, 7250 and 10,000 years after decommissioning (see Section 6.3, Determination of Scenario Probabilities in the DCCA).*

EPA Comment 73.
<u>Volume 4, page 2-18</u>
Where are the referenced "Table 3-4 of Volume 3" and "Table 3-5 of Volume 3"?

Response. *Tables 3-4 and 3-5 are found on pages 3-20 to 3-25 of Volume 2 of SAND91-0893, denoted as reference WIPP PA Division (1991b) .*

EPA Comment 74.
<u>Volume 4, page 3-1</u>

(a) Table 3.1. To obtain the mass flux which was input into PANEL, the BRAGFLO simulations included the Culebra. However, it is unclear which transmissivity fields were used and where the two phase flow parameters assigned to the Culebra can be found.

(b) There is a discrepancy between the range that is provided for matrix porosity in Volume 3 (p. 2-83) and that which is presented in this table. The range should be .028–.303, with a median of .145 rather than .139.

(c) The bulk storage range for the Culebra is missing from the Table and is required for the transient simulations.

(d) The clay effective porosity (CULCLYP) appears to be unreasonably high. As pointed out in Volume 3 (p. A-130), a lower porosity would probably be more realistic (18 percent) for the actinides. Unrealistically high porosities would overestimate retardation by matrix diffusion.

Response.
a) Two-phase flow parameters for the BRAGFLO code are described in Volume 3, Section 1.4.1. Single-phase (liquid) fluid flow parameters (including the transmissivity field) for the Culebra are currently developed for the SECO2D code (see Volume 3, Sections 1.4.5 and 2.6). Two-phase flow processes are modeled by BRAGFLO at the repository-scale, including the Salado, but excluding the Culebra. Two-phase flow for the Culebra is not included in the 1992 PA, but will be included in future PA modeling.

b) The noted discrepancy for the Culebra matrix porosity (Volume 3, page 2-83, Figure 2.6-6, Culebra porosity distribution and Volume 4, page 3-3, Culebra matrix porosity (CULPOR) range and median) is an oversight in the data consistency checking of 1992 PA reports. The distribution presented in Volume 3 is the correct definition of the matrix porosity for the Culebra.

c) The storage coefficient for the Culebra is included in Table 2.6-1, page 2-76 of Volume 3. The storage coefficient is used with the transmissivity and other hydrologic parameters in the SECO2D code to compute ground-water velocity and discharge. The storage coefficient is required for the transient groundwater flow simulations, however, the variables included in Table 3-1 are ONLY those variables sampled for 1992 PA. Storativity is not included in this sampling, and therefore is not included in Table 3.1 of Volume 4.

d) The porosity of the clay-lined fractures in the Culebra (CULCLYP) range from 0.05 to 0.5 (dimensionless), median 0.275. A value of 0.18 is referenced in Volume 3, page A-130. The data inconsistencies will be removed in future PA reports. Effective fracture porosities can be very high when a fracture is open and not clay lined. The clay lining may significantly reduce the effective porosity. Future PA reports will provide additional discussion on the expected values.

EPA Comment 75
Volume 4, page 3-1

Where are the parameters for the seals?

Response. *The seal parameters used in the 1992 PA are listed in Volume 4 of the 1992 PA report at the end of Table 3-1 on pages 3-5 and 3-7. These variables relate to the parameters listed in Volume 3 of the 1992 PA report on page 6-3, Table 6.0-2, "Distributions of Sample Parameters in December 1992 WIPP Performance Assessment for Engineered Barriers."*

EPA Comment 76
Volume 4, page 4-17
An additional shortcoming is that the output from SANCHO is in the form of total porosities which were not transformed into effective porosities as required input for BRAGFLO. The effect of this will be that BRAGFLO will overestimate storage and underestimate the amount of pressure buildup and distance that brine may potentially migrate.

Volume 4, page 4-19
Apparently gas-generation is active for 1,050 years. However, when the gas generation is at a maximum, there would be the least potential for brine intrusion. Therefore, a single intrusion at this time would result in minimized releases to the environment in an E2 scenario.

Response.
Volume 4, page 4-17
Effective porosity is the amount of interconnected pore space through which fluids can pass, expressed as a percentage of bulk volume. Part of the total porosity will be occupied by static fluid being held to the matrix grains by surface tension, so effective porosity will be less than total porosity. Isolated pore space (containing fluids or not) within the matrix is part of the total porosity, but is not part of effective porosity.

The difference in BRAGFLO and SANCHO porosity formulation and application is recognized. As discussed in the 1992 PA report, the formulation of the porosity interface between the two codes was being evaluated for the first time; the difficulties are outlined in the report. Future PA reports will address the differences between the use of total porosity in SANCHO and the effective porosity used in BRAGFLO. For the 1992 PA, the effective porosity and total porosity are assumed to be the same. As noted on page 4-17, an improved way of dealing with these inconsistencies is planned for future performance assessments.

Volume 4, page 4-19
The comment is correct.

EPA Comment 77.
Volume 4, page 4-24
In the manipulation of the specific storage equation to solve for rock compressibility, what is the basis for the values for specific storage of the anhydrite and halite? These are fairly critical values as they will strongly influence the pressures attained. It is odd that a distribution was not assigned and there is no mention of these parameters in Table 3.1. Furthermore, how was the alpha for the waste itself determined?

Response. *The basis for the specific storage values for anhydrite and halite used in the manipulation of the specific storage equation to solve for compressibility (Volume 4, page 4-24) is found in Volume 3 of the 1992 PA, pages 2-44 for halite and 2-49 for anhydrite. The ranges for specific storage for both rock types are recorded as follows:*

SPECIFIC STORAGE (m^{-1})

Rock Type	*Range*	*Median*
Halite	*$2.8x10^{-8}$ to $1.4x10^{-6}$*	*$9.5x10^{-8}$*
Anhydrite	*$9.7x10^{-8}$ to $1x10^{-6}$*	*$1.4x10^{-7}$*

The specific storage values for anhydrite and halite listed in Volume 4, page 4-24 represent the high end values for each range for each rock type.

Although specific storage can affect formation pressure response, pressure response in the formation is in itself not a critical criteria for long term performance of the WIPP. The sensitivity of room pressure, gas migration, brine migration, gas mass outflow, and brine mass outflow to formation specific storage have more affect.

The alpha for the waste is not used. Compaction has been investigated through both a volumetric plasticity (crushable foam) model and the Nonlinear Elastic Waste model (Butcher, et al., 1995).

EPA Comment 78.
Volume 4, page 4-23
Generally, the van Genuchten-Parker formulae provide a better model for relative permeability and capillary pressure. Typically the Brooks-Corey formulation is only used because of its numerical efficiency. The method by which the van Genuchten approach treats the air entry pressures suggests that it is not only more conservative but also more realistic. Further information justifying the selection of one formulation over the other should be provided. In addition, the use of the combination of formulae seems to be arbitrary.

Response. *A comparison of the van Genuchten-Parker and Brooks-Corey formulations, as well as the rationale for selection of a two-phase flow model for the latest PA analyses, is discussed in Sections 5.6 and 5.7 of the SPM Position Paper on Salado Flow and Transport (Howarth et al., 1995) and in Webb (SAND93-3912).*

EPA Comment 79.
Volume 4, page 4-28
The means by which the initial conditions were determined for the simulations needs to be better justified. Essentially all of the results will be significantly affected by the assumptions that are made in estimating the initial conditions. The description that is presented in the PA should be as detailed for this phase of the modeling as it is for the remainder of the analysis. For example, the rationale for treating excavated regions as an atmospheric pressure initial

condition rather than as a boundary condition is not presented. It seems that in reality the excavation will serve more as a boundary than as an initial condition. If the excavation were treated as a boundary condition, far more depressurization and brine desaturation would occur. Furthermore, if the depressurization reaches the no flow boundary at the base of the model, the depressurization will be over-predicted. This needs to be checked and discussed.

Response. *The selection of initial and boundary conditions for the SANCHO and BRAGFLO codes is discussed in detail in the response to EPA Comment 64.*

EPA Comment 80.

Volume 4, page 4-30

Why was a distribution not placed on the Culebra permeability when it is set from zero back to 2.1 x 10^{-14} m^2? Where did this number come from?

Volume 4, page 4-31

The very high permeability that is referenced as being assigned to the excavated region to simulate cavities is equivalent to that of a gravel. Were higher permeabilities not assigned because of convergence problems? If this is the case, what overall effect does this assumption (i.e., regarding the permeability of the open rooms being relatively low as compared to a cavity) have on the model results?

Response.

Volume 4, Page 4-30

The purpose of Table 4.3-1 on page 4-30 is to illustrate the procedure for running a single PA realization for the undisturbed case. The permeability value of 2.1 x 10^{-14} m^2 is simply the choice made by the analyst for this particular calculation (as exemplified by the values in the table).

Single values for variables, such as the Culebra permeability, are assigned for each PA realization. These single values are selected by sampling from the appropriate parameter distribution. It would be inappropriate to assign a distribution of values here because the table is intended as a realization for a particular PA calculation.

Volume 4, Page 4-31

Two options can be used to simulate internal cavities in a permeable medium: (1) definition of an internal model boundary across which fluid could flow; or (2) definition of a very high-permeable material into which fluid would flow. In the analysis described on page 4-31, the high-permeable material was used successfully with no convergence problems. Further increases in permeability of the excavated zone will have no impact on the PA results because once the excavation permeability is more than three orders of magnitude greater than the *permeability of the surrounding Halite, the Halite permeability will control brine flows into and out of the disposal rooms.*

EPA Comment 81

Volume 4, page 4-46

Given the assumptions provided in the text (e.g. radial flow), the determination made regarding the farthest distance that brine may have moved in MB139 does not appear to be correct. The formula, $V = (\pi)(r^2)(h)$(effective porosity), where $V = 1800\ m^3$; $h = 0.85$ meter; porosity = 0.001 yields a value of r of 821 meters. The far end of this grid block would be 2,500 meters or 100 meters beyond the repository boundary. Furthermore, these calculations do not account for the gas which may follow behind the brine and move it considerably greater distances. The effects of gas and the brine should be added to make this determination.

Response. *Since the 1992 PA, the methods of calculating brine outflow have been modified. The recently published Position Paper: Non-Salado Flow and Transport (Axness, C.,1994) contains a discussion of the brine flow calculations.*

EPA Comment 82.

Volume 4, page 4-46

The second illustration which provides percent gas saturations also appears to be incorrect. Using the above formula and correcting for gas saturations, a radial migration distance of 806 meters was obtained. The far end of this grid block would also be 2,500 meters.

Response. *Since the 1992 PA, the methods of calculating brine outflow have been modified. The recently published Position Paper: Non-Salado Flow and Transport (Axness, C.,1994) contains a discussion of the brine flow calculations.*

EPA Comment 83.

Volume 4, page 4-48

The analysis and objectives of the creep closure effects needs to be better explained. The text states "...the panel porosity was initially 66 percent and dropped as creep progressed, leveling off at 12 percent to 21 percent. In the fixed-porosity calculations, the waste panel porosity was initially 19 percent which is the median final-state porosity of the wastes. (See Table 3.4.-1 in Volume 3 of this report)." In Volume 3 it is indicated that 19 percent was the value of porosity used for the 1991 PA. Therefore, it is probably not the median of the 1992 analysis where porosity values ranged from 12 to 21 percent. It appears that the fixed porosity analysis was actually part of the 1991 PA, but this is, not clear in the text. Furthermore, the 12 to 21 percent range is a minimum as stated on page 4-33. The range of pore pressures that are shown in Figure 4.4-2 for creep closure simulations is approximately 12 to 34 percent. It appears that both approaches used porosities that were very close. Therefore, it is not surprising that the text concludes "The overall effect of modeling creep closure dynamically was minor." Why not, for the fixed porosity simulation, set the porosity at the low end of the porosity range for the creep model and perform a conservative bounding analysis?

Response. *Performance assessment studies have shown that the permeability of the waste has to be within three orders of magnitude of the permeability of the host rock to have any influence on the time for brine to flow through the facility. That is, the flow through the high permeability element (the waste) is for all practical purposes instantaneous when the waste is much more permeable than the host rock. This can be understood by considering two volumes (elements) in parallel. If the difference in permeability is greater than a factor of 1000, all of the flow is concentrated in the high permeability element and this flow occurs instantaneously, for all practical purposes, relative to parallel flow through the low permeability element.*

EPA Comment 84.
Volume 4, page 5-7
DOE needs to discuss why it decided it was not important to statistically sample the hydraulic conductivity (2.24 x 10-7 m/s) of the Culebra. This will be a critical factor in estimating source term flux. A single value of hydraulic conductivity will yield a single permeability value which will have a significant impact on how much of any release migrates into the Culebra.

Response. *BRAGFLO calculates the radionuclide source-term for SECO calculations of transport in the Culebra. The parameter variations in the Culebra are accomplished by sampling on the transmissivity fields used by SECO and on the fracture spacing.*

EPA Comment 85.
Volume 4, page 5-9
Specific storage is a critical parameter. Why has it not been statistically sampled?

Response. *Specific storage is important in the physical response of certain parts of the system; however, previous research and a recent publication (Freeze, et. Al, 1995) have shown that variation of specific storage in Performance Assessment does not affect regulatory performance measures related to brine migration, repository pressurization, and gas migration. The processess are dominated by other physical phenomena, such as waste collapse and fracturing.*

Reference: G.A.Freeze, K.W. Larson, and P.B Davies, 1995, *Coupled Multiphase Flow and Closure analysis of Repository Response to Waste-Generated Gas at Waste Isolation Pilot Plant (WIPP)*, SAND93-1986, Albuquerque, NM : Sandia National Laboratories

EPA Comment 86

Volume 4, page 5-9

The van Genuchten model should be strongly considered for the borehole material to ensure a conservative approach.

Response. *The van Genuchten model will be considered in future PA analyses. See also resolution of Comment 92PA EPA-78.*

However, it should be pointed out that it may not be appropriate to assume that the use of van Genuchten model will always ensure a conservative approach. The differences between the two model formulations is most significant at the extremes of very low saturation of brine or of gas. The van Genuchten model predicts easier gas penetration into pore space that is initially 100 percent brine saturated. This can have a significant impact on calculations of the migration of gas out of the repository panels into the surrounding, very low permeability, small pore volume, halite. For something like borehole fill material, with significantly greater permeability and pore size and where most important release mechanism may be brine transport to the surface, application of the van Genuchten model may not be at all conservative; or it may be that there will be little difference between the results achieved with the two models because the saturation levels are in the intermediate range where the predictions of the models do not differ greatly. The decision to use one model or the other or to employ a sampling strategy will have to be based on examination of how well each model is able to match measured or literature derived characteristics of appropriate fill material, issues of conservatism, and also, whether the choice of model, in this particular situation, makes significant difference to the calculated outcomes.

EPA Comment 87

Volume 4, page 5-10

The rationale for Castile brine pressure and storage needs to be discussed. The pressure range (12.6-21 MPa) appears to have been selected because it falls between lithostatic and hydrostatic pressures. This pressure range, therefore assumes that the deepest brine reservoir that will be breached is at 900 meters. This assumption needs more discussion.

Volume 4, page 5-11

The second paragraph in Section 5.1.3 describes what would normally be considered as a boundary rather than an initial condition. The reasons for not setting the excavations at atmospheric pressure for the entire 20 years needs to be discussed.

The initial conditions are so critical to the analysis that there needs to be substantially more discussion and justification as to how the pressure and saturation distributions were reached. For example, it appears that no statistical sampling was performed on any of the rock properties from which the initial conditions were obtained. As much emphasis should be placed on the determination of initial conditions as for the rest of the modeling exercise.

The importance of the initial conditions is illustrated by Figure 4.4-3 which indicates that once the brine within the waste is consumed during the gas generation reactions, the saturation within the waste never increases (i.e. there is only very limited brine flow into the waste panels). Therefore, the assumed initial conditions will simply shift, in time, the peak saturations and pressures which are the critical aspects of the analysis. High initial saturations and pressures will not only shift the peaks to the earlier times, but also to higher magnitudes. Lower initial saturations and pressures will shift the peaks to the later times and lower their magnitudes. Therefore, the uncertainty associated with the assumed initial conditions will be propagated through the entire modeling analysis. Apparently, no attempts have been made to quantify the affect that this uncertainty will have on the modeling analysis.

Response.

Volume 4, page 5-10

Two types of data are considered to be best suited for determining initial reservoir pressure. The first is the earliest buildup data recorded after encountering the brine reservoir, and the second is the longest buildup data recorded. A varying number of wells drilled in and around the WIPP site were used to estimate Castile brine reservoir pressures and volumes. Further discussion of these parameters can be found in Reeves et al., 1989. The pressure range (12.6 MPa to 21 MPa) was estimated from several wellhead measurements at WIPP-12 and other boreholes that encountered pressurized Castile brine. The initial range was between 7.0 and 17.4 MPa. However, because the range of pressures includes measurements in wells completed at various elevations, a correction for differences in elevation was required. This calculated range is similar to the maximum and minimum possible range of 11 and 21 MPa, assuming hydrostatic and lithostatic pressures at the elevation of the WIPP-12 brine reservoir. The pressure range therefore assumes a maximum depth of approximately 900 meters below ground surface based on these calculations and assumptions. Further discussion of the Castile brine reservoirs can be found in Lappin et al., 1989, Volume 3 of 1992 PA (Pages 4-10 to 4-17), and in DOE document Brine Reservoirs in the Castile Formation TME 3153.

Volume 4, page 5-11

Pressure in the repository during the operational period is unchanging due to ventilation; this is a boundary condition. However, BRAGFLO cannot accommodate fixed pressures internal to the fluid flow domain. Therefore, the initial pressure is set for the start-up period, which is short, and is allowed to change due to flow, etc. The magnitude of changes is slight, however, and the atmospheric pressure initial condition is essentially an atmospheric pressure boundary condition for a short period of time.

The commentor should also note that the complexity of the modeled system is such that "insight" based on experience with simpler models is often incorrect.

EPA Comment 88.

Volume 4, page 5-15

The statement "In only two vectors did the pressure exceed lithostatic (14.8 MPa), probably a result of rapid gas-generation rates and high initial brine content in the waste" is misleading. It appears that the reason only two vectors exceeded lithostatic pressure is that the simulated intrusion occurred before the other twenty or so vectors could also exceed lithostatic pressure as indicated in Figure 5.2.-1.

It is not clear from the text how the intrusion was simulated (i.e., initial or a time-dependent boundary condition).

The statement "Once the borehole is filled with brine, the pressure in the waste reaches hydrostatic relative to the Culebra pressure, and then levels off," raises an important point in that the Culebra pressure has been held constant despite the effects of the intrusion. What is likely to actually happen is that the pressure in the Culebra will also locally rise, will drop the potential between the two formations, and reduce the brine flow from the Salado to the Culebra to levels below which has currently been predicted given the constant permeability used in the Culebra.

Response.

It is true that a later intrusion may penetrate a higher pressure repository.

The intrusion is modelled as a time-dependent permeability change in the code.

The statement regarding local pressure rise in the Culebra that reduces potential between the two formations is probably correct because of the formulation of the model. What is suggested probably did occur, but at so low a magnitude that it was not observed from the plots generated.

EPA Comment 89

Volume 4, page 5-16

At the top of the page it states that "The only parameter that distinguishes these two from the other 68 is that they have the highest sampled anhydrite permeabilities, which would have provided good communication to the higher far field pressures." Again, the distance to these far field pressures will be based on the initial conditions.

Response. *The initial depressurized zone around the repository, developed over 20 years, has little impact on the distance to high far-field pressures over 10,000 years. High k anhydrite is* *an effective conduit for flow and associated pressure change, and with 10,000 years, a 20-year perturbation at the onset is essentially meaningless.*

EPA Comment 90

Volume 4, page 5-33

At the top of the second paragraph the text states "...reach similarly unrealistically high values, up to 38 MPa." What is the basis for estimating if a pressure value is unrealistic?

Response. *The "unrealistic values, up to 38 MPa" are approximately 2.5 times lithostatic pressure and are considered to be in excess of the overlying rock strength under tension. Lithostatic pressure at the repository level is 14.8 MPa (SAND92-0700/4 page 4-33) and decreases as one goes up towards ground surface. Pressures within the repository in excess of lithostatic pressure change the stress conditions in the overlying rock from compression to tension. Sedimentary rocks at the WIPP site are inherently weak when placed under tension. Repository pressures in excess of lithostatic pressure, such as up to 38 MPa, cannot be maintained because tensile failure of the host rock, probably through fracturing, will tend to relieve the repository pressure.*

EPA Comment 91

Volume 4, page 6-1

The first bullet on the page leads the reader to the conclusion that the dual porosity model provided the best fit to the data, whereas in actuality other models (e.g., networks of fractures) fit the data equally as well (Jones 92).

Response. *The data from ongoing laboratory and field experiments will assist in defining the proper conceptual model for fluid flow in the Culebra.*

EPA Comment 92

Volume 4, page 6-1

The second bullet also raises a concern regarding the calibration to fresh water head. If the flow varies across the thickness of the Culebra, the densities may also vary. Furthermore, the velocities could be considerably greater through these sections of the Culebra than those velocities which are currently assumed.

Response. *A three-dimensional numerical representation of the groundwater basin model, using the SECO3D code, is currently being developed and may be used to investigate long-term regional flow of groundwater in strata above the Salado Formation. Over the modeling periods of concern, flow deviations due to density effects are expected to be minimal, as discussed in the Non-Salado Position Paper (Axness et al., 1995).*

EPA Comment 93

Volume 4, page 6-2

Regarding the fourth bullet, future climate change could also affect density gradients as well as transmissivity through active dissolution of the dolomite.

Response. *Future climate change affects the Culebra by changing the recharge rates. It is expected that, even as recharge rates might change, the chemistry of the groundwater reaching the Culebra will be similar to the chemistry of present day recharging groundwater. Thus, density gradients and dolomite dissolution rates are not expected to change as a result of climate change.*

EPA Comment 94

Volume 4, page 6-2

The last bullet on the page also needs to be further addressed. In addition, the three-dimensional model which is currently being developed should evaluate the hydraulic relationship among the two units as well as the potential for greater density driven flow and transport.

Response. *Further discussion of the last bullet is included in SAND 89-7069 (Reeves, et al., 1989). The importance of density driven flow was addressed in Davies, 1989, USGS open file report 88-490, who found that within the Land Withdrawal Boundary, density-driven flow is not important. The hydraulic connection between Salado and Culebra in the absence of a borehole to the repository or brine reservoir is assumed to be slight due to the low permeability of the Salado. However, the connectivity is modelled in BRAGFLO.*

EPA Comment 95

Volume 4, page 6-4

The local grid boundary conditions which are mentioned in the last sentence of Section 6.2.2 should be presented for each of the 70 analyses in Volume 3.

The regional control for the determination of the 70 transmissivity fields is very poor. Furthermore, the uncertainties, discussed in LaVenue (LaVen 92) regarding the transmissivity fields, need further attention. Specifically, the differences between GRASP-INV and the 1990 calibrated fields illustrated in Figure 4-5 indicate that the GRASP-INV calibration yielded significantly lower transmissivities in the regions immediately south of the WIPP repository along the most likely travel path of contaminant releases. LaVenue indicates that the travel time determined in the 1990 study was approximately 14,000 years, whereas, the travel time with the GRASP-INV code is approximately 30,000 years. The higher travel time was attributed to the lower transmissivity north of the H-3 borehole south of the WIPP Site.

Furthermore, considerable uncertainties with respect to the regional boundaries have been propagated through the analysis, apparently without any attempts to quantify the effect on the analysis results.

Response. *The local grid boundary conditions (regional head solutions) would be better placed in Volume 3, section 2.6.3.*

Details are provided in La Venue (SAND92-7306, page 61) for the model results, including the apparent discrepancy in transmissivities in the region of the H-3 borehole. The section explains that if the differences in the observed and calculated heads at the H-1 borehole were reduced, the likely result would be increased transmissivity north of H-3, allowing for a travel time closer to 14,000 years. Because there may not be enough pilot points to adequately modify the transmissivity field, additional pilot points may be needed to produce a correlation that is closer to empirical findings. In addition, the discussion on pages 103 to 107 of LaVenue (SAND92-7306) discusses the variability in calculated values. Uncertainty in the models originates from identifiable contributing parameters that are analyzed for closeness of fit to observed data. These models are then used for a best-case model.

EPA Comment 96

Volume 4, page 6-9

The statement in Section 6.3, "all other hydrologic parameters were held constant, at values described in Volume 3 of this report," introduces some confusion as to exactly how the local boundaries were assigned for each simulation. Each of the local flow and transport simulations would have required unique and different local boundaries obtained from the respective regional realization.

The last sentence on this page which states "heads in the strip were prescribed as a function of a sinusoidal climate function applied to the initial calibrated heads derived from the steady-state solution for each transmissivity field," is misleading and suggests that the heads in this region have been calibrated. In reality the heads along the boundary where the climate function has been superimposed are actually obtained from a best guess approximation. The uncertainty of the results of the transmissivity field calculations associated with this boundary approximation have not been assessed.

Response. *The statement in Section 6.3 refers to hydrologic properties of the rock, such as porosity, transmissivity, etc. The boundary conditions for the local model were consistent with the specific regional flow field for each realization.*

The efforts to divide the groundwater model into two separate grids significantly reduces but does not eliminate the effects of boundary conditions on performance assessment calculations.

EPA Comment 97
Volume 4, page 6-11
All of the climatic change vector plots should have been included in Volume 3.

The vector plots in Figure 6.5-1 suggest that, even though this analysis was selected to illustrate climatic effects because it had the largest sampled climatic impact factor, there is almost no ground water entering the model domain along this recharge boundary. This is probably due to the low transmissivity in this region for this particular realization. The last sentence in Section 6.5 on Page 6-19 states "...and subsurface releases of radionuclides are not sensitive to climatic variation of heads along the modeled "recharge strip." First, there is an extremely large degree of uncertainty associated with predicting what the regional boundaries will be as the climate changes. This is acknowledged in the first sentence in the second paragraph on Page 6-11 which states "The effect of climatic changes on regional boundary conditions cannot be modeled directly because of uncertainty in the location of present and future recharge and uncertainty in the hydrologic properties affecting the flow path from the recharge area to the regional domain boundary." Although this statement recognizes the uncertainties, it also illustrates a major problem with the approach that has been taken to model potential climatic changes.

Figure 6.5-2 indicates that regardless of how high the heads are set in the recharge region to simulate the climatic changes, there will be very little effect on the flow field in the vicinity of the WIPP site. The arrows (i.e., specific discharge) indicate that almost no water is entering the system. This is due largely to the low transmissivities. Furthermore, the water that does enter the system is drained away to the southwest due to boundary condition specifications.

It is difficult to determine whether any increase in gradients caused by the climatic change would affect the contaminant release. In the case of cumulative releases, if all of the contaminants reach the boundary regardless of climate change, both scenarios will have the same results. However, differences in peak arrival times would be observed under different gradients.

A more conservative and possibly more realistic approach would be to assume that the increased rainfall will cause the Nash Draw region to become a recharge area rather than a no flow boundary. The southwestern side of the modeled area could also be a recharge boundary. The effect of these would be to allow water to reach the WIPP Land Withdrawal Boundary, unlike the current situation. These proposed changes to the model boundaries relate to the discussion on Page 6-26 which indicates that the boundary conditions specified along the southern and western boundaries are not consistent with the observed heads and that "If the specified heads are increased along the southern boundary to fit H-7 and USGS-1, the southern boundary converts from a discharge boundary to a recharge boundary." Furthermore, the statement "however, the Pecos River, and the Malaga Bend region in particular, has been determined to behave as a discharge region for regional flux from the Rustler (Mercer, 1983)," does not recognize the uncertainty that other investigators have regarding these areas (LaVen 92). Furthermore, the relevance of the statement at the bottom of page 6-26 that "this may indicate a ground-water divide occurs between the H-9 borehole and the H-8 borehole south of the model domain," needs to be better explained.

Response. *In future reports, the DOE may include additional groundwater (specific-discharge) vector plots for anticipated future climate conditions and regional hydrological boundary conditions if these plots will enhance understanding of the results for potential climatic change scenarios.*

Any increase in gradients will affect contaminant release. However, it is difficult to determine a climate change scenario that will significantly change gradients. The assumptions for the 1992 PA disregard the energy loss (head loss) by groundwater recharging the system as it passes through overlying strata; this is extremely conservative. Additionally, the reason there is little flow around the WIPP boundary is not due to uncertainty in boundary conditions in the regional domain, but rather because the transmissivity of the Culebra is reduced there.

Recharge from Nash Draw is probably unrealistic. Nash Draw is a topographic low that serves either as a discharge point or as a groundwater divide or both. Due to the effect of topography or hydraulic head and established regional gradients, there is more hydraulic head in Culebra and Magenta groundwater than will be in Nash Draw. More rainfall in Nash Draw will result in a larger Laguna Grande de la Sal.

EPA Comment 98

Volume 4, page 6-19

It is unclear whether the climatic change boundaries were used for these flow and transport simulations. If they were not, why were transient rather than steady-state heads used in the analysis since source-term flux was not assumed to change the ambient flow field? Furthermore, if the initial conditions were assumed to be transient because of the climatic change, the local boundary conditions obtained from the regional modeling would have also had to have been assumed to be transient. This does not appear to be the case.

The following discussion recited from Section 4.2 (LaVen 92) indicates that the prescribed-pressure model boundaries used for the determination of transmissivity fields are not only poorly understood, but they were also not systematically investigated in the uncertainty analysis.

"Several iterations were made to the boundary conditions prior to beginning the calibration exercise. The iterations were necessary due to the difficulty in matching the H-7, USGS-1, and H-9 observed heads while properly fitting the heads in the rest of the domain. The difficulty arises from the existence of the no-flow region along the southern boundary to fit

H-7 and USGS-1, and the southern boundary converts from a discharge to a recharge boundary. Recently, discussions on the Culebra have lead toward considering this possibility. One problem, however, stems from the fact that the Pècos River, and the Malaga Bend region in particular, has been hypothesized to behave as a discharge region for regional flux from the Rustler (Mercer 83). While no absolute conclusions may be made yet concerning the direction of groundwater flow in the region south of the WIPP site, the results determined in this study have indicated that there is an inconsistency between the assumption the groundwater flows southward throughout the model domain and the observed heads in the area. Thus, a compromise between the fits at the southern boreholes and the rest of the model area was necessarily implemented through the boundary conditions."

The large degree of uncertainty in understanding the system boundary conditions has serious implications regarding not only the determination of the alternative transmissivity fields, but also the entire conceptualization of the system. If the model boundaries vary significantly from what is actually occurring in the field, then the solution of the model domain (i.e., transmissivity field) for the 70 simulations would be in error by some unknown and untested quantity. Furthermore, even if the conceptual model was better defined, the current approach that has been taken to obtain the 70 transmissivity fields does not evaluate the sensitivity of the boundary conditions to the overall model results.

Response. *The climate change boundaries were used for radionuclide transport. It is not understood what the commentor's sentence "if initial conditions were assumed to be transient. . ." means since initial conditions necessarily cannot be transient. While there is admitted uncertainty regarding the adequacy of the boundary conditions of the regional model in the 1992 PA, there are two facts to bear in mind: 1) The uncertainty in the southern section of the regional grid may not impact radionuclide travel times in the disposal system; and (2) the regional system is currently being analyzed on a much larger scale with three-dimensional models (Axness, C, et al, 1995). The intent of this modeling is to provide an assessment of groundwater flow on a basin-wide scale to which approximations made in Performance Assessment can be compared.*

EPA Comment 99

Volume 4, page 6-28

Where is the source for the matrix effective porosity value of 16 percent come from?

Response. *As noted on page 6-26 of Volume 4, the purpose of these analyses was to characterize the transmissivity fields, not to predict radionuclide transport or to provide input to a CCDF for a compliance application. The matrix porosity of the Culebra is described in Sections 2.6.1 and 2.6.2 of Volume 3 and has a median of 0.139. The 16 percent value is the analyst's choice in this particular analysis.*

EPA Comment 100
Volume 4, page 7-13
Equation 7.3-2 appears to be in error. Based on information in Table 7.3-3, the correct formulation should be:

$$SF_{44} = 138.67/3.527 = 39.32$$

The text defines AL_i as the average activity level. According to Volume 3, Table 3.3-1, waste inventories are median values. Is AL_i actually a median activity level?

Response. *The release of activity level 4 at 3,000 years can be obtained by multiplying the average activity level release at 3,000 years by the following scaling factor:*

$$SF_{54} = 138.67/3.527 = 39.32$$

The average activity level was derived by dividing the total activity (based on the memorandum by Peterson in Appendix A of Volume 3 and compensating for decay) by the total surface area of CH waste. AL_i is not a "median" level but the total activity/m² at the designated time, based on the radionuclides shown in Table 7.3-1. AL_i is thus the average activity per square meter.

EPA Comment 101
Volume 4, page 8-30

Why has the relatively short half-life of Am-241 and decay to Np-237 not reduced the release of this nuclide to the accessible environment from that which is released to the Culebra?

Response. *Americium-241 should be reduced proportional to the amount of travel time in the Culebra. However, the postulated travel times of 10^2 to 10^3 years would reduce Am-241 ($t½$ = 432 years) by about a factor of 3 at most.*

The statement in Volume 4, page 8-30, to the effect that releases to the accessible environment are essentially identical to releases to the Culebra was not intended to imply that decay of short-lived isotopes does not occur during the transport in the Culebra. The plot on page 8-31 shows that calculated releases to the Culebra vary over a range of nearly four orders of magnitude. Because the plot on page 8-31 is on a log-log scale covering quite a few decades, it is necessary to examine the figure closely to see that the calculated releases to the accessible environment are indeed less than the releases to the Culebra. For the vectors that featured the longest residence times in the Culebra, decay of the short half isotopes (which make up only a part of the total activity level) reduced the released activity level by as much as a factor of 2 or 3. However, there were only a very few such vectors, and compared to the four order of *magnitude spread in calculated releases to the Culebra even a factor of 3 reduction in the release to the accessible environment may look negligible.*

EPA Comment 102

Volume 4, page 8-30

The inclusion of clay thickness in the Chemical Retardation, Clay-Lined Fractures, No Matrix Diffusion scenario is questionable. The sorption is assumed to be linear and non-reversible. Therefore, as long as clay is present, a retardation factor will be applied regardless of the thickness. Since there is no matrix diffusion, there is no means by which the radionuclides can reach the matrix to have a distribution coefficient applied for the dolomite which is different from that of the clay.

In addition, the assumption that clay-lined fractures (e.g., with corrensite) are common is very questionable - and is questioned by researchers even within Sandia. The presence of clay-lined fractures should be reconsidered.

Volume 4, page 8-44

In this section, there is no mention of varying clay thicknesses which should have been used in these simulations due to different diffusion rates and distribution coefficients for the clay lining and the dolomite matrix.

Response. *The WIPP Project is currently performing laboratory and field experiments to determine the presence and potential impact of clay deposits on radionuclide transport and retardation in the Culebra.*

EPA Comment 103.

Volume 4, page A-16

Pinch nodes should be added to the capability of SECO to avoid having to perform regional and local in a disjointed fashion.

Response. *Comment noted. Future PA modeling will use the CAMCON methodology to couple regional and local models.*

EPA Comment 104

Appendix C

The listings of LHS sampled variables in Table C-2 are useful for a serious reader interested in recalculating certain portions of the analysis. For the casual reader, a reference to the cumulative distribution plots of these sample variables provided in Figure 3-1 should be included. Scatterplots of one variable versus the other should be provided for all uncertain variables which were assumed to exhibit pairwise correlation.

Table C 1 is garbled. The vectors are not ordered, and many vectors appear in the Table more than once with different discharges.

Response. *The graphical representation of the LHS sampling has not been included in Appendix C due to the number of scatter plots used in PA. However, Chapter 8 of Volume 4 includes a significant number of scatterplots which may assist in understanding the correlation of data.*

Table C-1 of Appendix C is a summary of the 49 variables discussed in Chapter 3 of this document. In this chapter, each variable is briefly summarized including identification of the distributions, vectors, and other relative references. After further review of Table C-1, we have been unable to identify repeated vectors or inconsistencies with the summaries in Chapter 3.

EPA Comment 105

Appendix C

Table C-5 is unclear. Why are three sets of 70 vectors shown in the table? Sub-headers such as in Tables C-6 and C-7 should be used to explain the different sets of vectors, if three sets are appropriate.

Response. *The presentation of data in the compliance certification package will be clarified as much as possible.*

EPA Comment 106

Hazardous Waste and the PA: Throughout PA

The 1992 PA does not include an evaluation of the influence of hazardous constituents in its analysis. What effect will these materials have on the transport and activity of radionuclides? Are there any synergistic interactions that can effect compliance? What experiments have been done to determine these issues? What are the plans for the future?

Response. *The commentor is correct: the 1992 PA does not include an evaluation of the influence of hazardous constituents in its analysis. However, it is expected that these materials will have no effect on the transport and activity of radionuclides. In theory, there could be an occasional alpha radiolysis of organic compounds, but it is believed that the compounds would then be reduced to non-RCRA compounds. The WIPP Project has not conducted specific experiments to address the above issues because there is an extensive literature on this topic. There are no plans to conduct experiments in this area in the future.*

EPA Comment 107

Probability Calculations: Volume 4, page 2-20, lines 5 & 18

Please include examples of how the probabilities are calculated for Tables 2.5-1 to 2.5-4.

Response. *A detailed description of the methodology for computing probabilities can be found in (Helton and Iuzzolino, 1993).*

EPA Comment 108

$2 \pi r$ Relationship: Volume 4, page 4-7, line 6

This paragraph needs to be explained more clearly.

Response. *The $2 \pi r$ relationship will be explained more clearly in future PA documentation.*

EPA Comment 109

More Unjustified Conclusions: Volume 4, page 5-18, line 22

Once again a conclusion is presented without justification or supporting data or calculations. However, if the porosity is as low as can be expected, 0.001, this brine would travel only 935m radially from the panel", this may be a simple question but how the 935m distance is calculated should be shown. Besides the range of MB139 porosity is from .001 to .03. What would the travel distance be for porosity = .03?

Unrealistic Initial Conditions: Volume 4, page 5-18, 5-33, 4-48

Using the assumption of an initial porosity of 19% to compare non-creep closure to creep closure results intuitively seems unreasonable. The results of unrealistically high peak pressures seems to invalidate the assumptions.

Response. *As shown in Figure 5.1-2 on page 5-4 of Volume 4, the radius of the waste panel was taken to be about 61 m and MB139 as modeled as being 0.85 m thick. A released volume of 2500 m^3 in a layer 0.85 m thick with a porosity of 0.001 will occupy the annular volume from 61 m out to a radius of about 970 m, which implies migration of about 910 m. The 935-m value was not quite right. A porosity of 0.03 would imply about 150 m migration radially beyond the waste panel.*

The fixed porosity case was run for the purpose of investigating the sensitivity of the PA results to the creep closure calculation. Using the fixed 19% porosity created a large excursion in repository pore pressure for the sensitivity analysis. The fact that assuming a constant 19%

porosity would not be realistic does not affect the process of assessing the sensitivity of the model.

EPA Comment 110

"Excellent Communication": Volume 4, page 5-28, line 22

Is "excellent communication another way of saying that brine will be flowing into the far field?

Response. *Yes, excellent communications refers to brine flow to the far field.*

EPA Comment 111

"Reference Conditions": Volume 4, page 5-31, line 14

What are "reference conditions? Are they standard temperature and pressure?

Response. *The reference conditions used in the pressure calculation are not Standard Temperature and Pressure (STP). Rather, the pressure is calculated using the calculated WIPP void volume and the (constant) WIPP temperature, 27 degrees Centigrade. The volume is derived from the time-dependent porosity surface calculation.*

EPA Comment 112

Four Vectors: Volume 4, page 5-31, line 27

What are the four vectors in "Only in four vectors was there any net outward flow of brine..."? These are the kind of results EPA needs to see in detail not in a summary statement.

Response. *This information will be included in the final compliance certification application.*

EPA Comment 113

Minimum Porosity: Volume 4, page 5-31, line 39

If the brine will travel in the MB139 no more than 500m with a porosity of 0.001, how far will brine travel with a porosity of 0.03 (the maximum allowable)?

Response. *The brine will travel 150 meters with an effective porosity of 0.03 (the maximum allowable). The overall radial flow distance will decrease because materials with higher effective porosities will entrain more brine. Note that a travel distance of 500 m is well within the 2,400 meter boundary limit.*

EPA Comment 114

Current Creep Closure Model: Volume 4, page 5-36, line 12

Once again a conclusion is drawn that may not be completely defendable and justified. It appears that the present creep closure model may not adequately model the effect.

Response. *Since the 1992 PA report was published, the MDCF creep model has been extensively validated against in situ data for room closure. The code does model the effect on transient behavior (such as waste pressures). The Rock Mechanics Position Paper (Munsen et al., 1995) presents details of the creep model and the validation calculations.*

EPA Comment 115

Vertically Integrated Model: Volume 4, chapter 6, page 6-2, line 2

Please clearly define a "vertically integrated two-dimensional model"? What advantages and disadvantages does it have? How realistic is it? Does it account for the results of test?

Response. *In this case, "vertical integration" means that the parameters in the model do not vary in the vertical direction; i.e., the hydrological properties are constant with aquifer elevation. Since the groundwater flow to the accessible environment is expected to be predominantly lateral (not vertical) through the relatively thin Culebra Dolomite Member, the horizontal hydrological parameter components are of primary interest. As noted in Chapter 6, most of the well test data that exist are for the lateral/radial properties of the Culebra aquifer. A "vertically integrated two-dimensional model," in this case, is simply a model that includes no vertical variation in properties values. This is consistent with aquifer tests in the Culebra which typically measure aquifer properties across the entire vertical extent of the aquifer at a point.*

EPA Comment 116

Potential Fluid Discharge into the Culebra: Volume 4, page 6-2, line 41

What is the basis for the assumption that fluid injected will have no effect on Culebra fluid density?

Response. *The basis for the assumption of minimal effect on groundwater flow from fluid injection into the Culebra is deduced from calculations of disturbance to natural flow fields found in SAND89-7069, pages 3-11 through 15. These calculations demonstrate that stream lines of flow become relatively straight and parallel a short distance from the borehole, showing that the velocities are about equal to the natural flow field. Likewise, this suggests little effect on Culebra fluid densities since magnitude and direction of flow are minimally changed.*

Additionally, Davies (1989), USGS open file report 88-490, studied natural variation in Culebra groundwater density and found density differences within the Land Withdrawal Boundary to be of low importance with respect to overall groundwater flow directions.

EPA Comment 117

Regional and Local Domains: Volume 4, page 6-4

The regional and local grids are significantly different, the size of the grid blocks (elements) are quite different. How are values at these grid boundaries passed from the local to the regional grids? Are they interpolated or "fit" in any way? What assumptions are used in these calculations? What effect do these assumptions have on the transmissivity fields. It is stated on page 6-11, line 15 that SECO-FLOW interpolates boundary condition for the local grid. Please explain how this is done and does it introduced any unusual effects in the results? Has this been tested in any way?

Response. *The information at the boundary of the local grid is obtained from the numerical solution for the regional grid by second-order accurate linear interpolation in space and time of heads. The property data (e.g., transmissivity) is obtained by conservative interpolation using the Dukowitz-Knupp algorithm. Thus the local grid boundary conditions are always Dirichlet (specified dependent variable) values of head (time-dependent values for the time-dependent regional grid problem). The result is that Darcy velocities (specific discharges) are preserved.*

The only "assumption" involved is that a coarse grid solution over a much larger area provides better (less constrained) boundary conditions for the local grid than the usual practice of setting no-flow boundary conditions. Virtually anything would be defensible as an improvement over the usual practice. The method used here has obvious advantages.

This method has been tested and exercised in many calculations. A revealing test involves over-plotting head contours in the same region of space obtained by both the regional and local grid solutions. When the grid resolution in both regional and local grids are comparable, the contour plots virtually overlay, as expected. When the local grid resolution is much higher, as intended in the concept and as used in the 1992 PA calculations, there is some difference. Most importantly, the head contour lines in the local grid solution vary smoothly and align with the regional grid solution at the boundary of the local grid, indicating no distortion from the interpolation procedure (again, as expected from the theory).

It is worth noting that transport calculations in the 1992 PA calculations (and in later WIPP PA calculations) do not extend across the local grid boundaries, i.e., the regulatory boundaries are inside the local grid. Thus, there is no question about the boundary conditions for the transport equations. This level of detail in the algorithm description was not included in the 1992 PA calculations for the obvious reason that it is inappropriate for an already *massive document on preliminary results. Examples of the overplots have been shown to EPA personnel during one of their visits to SNL.*

EPA Comment 118

SECO-FLOW Regional to Local Grid Interpolation: Volume 4, page 6-11, line 15

You state that SECO-FLOW uses interpolation to establish boundary conditions for the local grid from the regional grid elements because the local grid elements do not exactly overlay the regional grid elements. What type of interpolation is used, linear? Does this process introduce any distortion into this pass of data? Has this effect, if any, been evaluated?

Response. *Please see the response to Comment 117.*

EPA Comment 119

Inconsistency in Hydrostatic Heads: Volume 4, page 6-26, line 13

The inconsistencies between the boundary conditions and the observed heads may be significant. What has been done to rectify this difficulties?

Response. *Please see the response to Comment 97.*

EPA Comment 120

Transmissivity: Volume 4, Chapter 6

The main explanations for high transmissivities in general at the WIPP site are: the lack of halite and reduced overburden that create fractures (Section 2.1.4 in LaVenue et al., SAND89-7068/1, Ground-Water Flow Modeling of the Culebra Dolomite; pages IV-47, IV-48 in Brinster, SAND89-7147, Preliminary Geohydrologic Conceptual Model of the Los Medaños Region Near the WIPP for the Purpose of Performance Assessment). These explanations are most valid west of the WIPP, but they fail to explain the high transmissivities in the southeast where the overburden is thicker and the halite is more intact. What is thought to be the reason for the high transmissivity in the southeast portion of the WIPP site, and could the high transmissivity extend further west of well H-11 to directly south of the site?

Response. *In reference to SAND89-7147, Preliminary Geohydrologic Conceptual Model of the Los Medaños Region Near the WIPP for the Purpose of Performance Assessment, the author states that transmissivity values taken from wells in the southeastern portion of the WIPP site are not reported to be high in terms of transmissivity values for the WIPP site. Referring to Figure III-16, page II-29 of SAND89-7147, distribution of halite in the Rustler Formation shows transmissivity values for wells in and around the WIPP site. Transmissivity values for wells southeast and south of the WIPP site have lower values than those to the west. It is concluded that the transmissivity values of well H-11 are a result of local effects.*

It does not appear that high transmissivity can extend further west of well H-11 to directly south of the site. Transmissivity values of wells directly south of the WIPP site show values approximately two orders of magnitude lower than well H-11 (such as the value of 3.0 x 10^{-7} Cabin Baby-1 versus the value of 2.9 x 10^{-5} for H-11). Further discussion of well H-11 can be found in SAND89-7147, pages IV-55 to IV-68.

EPA Comment 121

Transmissivity: Volume 4, Chapter 6

On Page 6-26 of Volume 4 there is a discussion of groundwater travel times (Section 6.8.3). The text states "The purpose of the groundwater-travel-time calculations described here is to characterize the transmissivity fields, not to predict the transport of radionuclides."

The approach that was taken in the PA uses GRASP-INV to solve the groundwater inverse problem as described by LaVenue and RamaRao (1992). In this publication the authors' state:

"In the earlier modeling efforts for the Culebra Dolomite aquifer (Haug et al., 1988, LaVenue et al., 1990), kriging was employed to address the spatial variability in transmissivity. In an effort in which only one calibrated field is to be produced, kriging becomes an obvious choice. Kriging provides an optimal estimate of the transmissivity at a point, i.e., the mean value. Simulated transmissivity values reproduce the fluctuation patterns in transmissivity, which may lead to extreme values in travel times. Thus simulated fields are useful to resolve the residual uncertainty not addressed by kriging."

This suggests that the approach that was used to create transmissivity fields for the PA would result in a greater range [extreme values] of possible travel times. It does not appear, however, that the transmissivity fields yield extreme travel time values or adequately describe the probable distribution of travel times.

The travel time results presented in Figure 6.8-6 indicate that the travel times range from approximately 9,000 years to 32,000 years. The histogram of travel times, from the ensemble of transient calibrated fields, does not appear to be consistent with the actual field data or with LaVenue's calibrated model (LaVenue et. al., 1990).

LaVenue et. al. (1990) performed a detailed model calibration that yielded an excellent correlation between observed and simulated events. Therefore, something appears intuitively wrong when LaVenue (1990) predicted travel times for the transient calibration of 14,000 years, and 75% of the travel times in the 1992 PA were greater than 14,000 years, while only 10 percent were less. Furthermore, the shortest travel time predicted in the PA was approximately 9,000 years, a difference of 5,000 years from the calibrated value, whereas, the longest travel time is approximately 32,000, or two and a half times greater than the calibrated model would have predicted. There does not appear to be a good reason as to why the longer travel times are over represented. To further represent this point calculations have been included.

Response. *As stated in Volume 4 on page 6-26, line 43, "These travel times were calculated assuming advection of groundwater through a single-porosity medium without fracture flow..." Performance assessment calculates groundwater travel times assuming fracture flow conditions with porosities on the order of 1/100 of the values used for creating the calibrated transmissivities. This is a very conservative use of transmissivity fields which results in radionuclide travel times approximately 1/100 of those discussed in this section.*

EPA Comment 122

Transmissivity: Volume 4, Chapter 6

Figure 6.8-8 indicates that the majority of travel paths run southeast from their starting point near H-3 and run towards DOE-1 before taking a more southerly direction. The hydraulic properties of the majority of these paths would, therefore, best be described by hydraulic property data collected from wells H-1, H-3, DOE-1 and H-11. The transmissivity data, collected from these wells and tabulated in LaVenue et. al. 1990 are presented in Table 1.

The geometric mean of hydraulic conductivity measured from these wells is 22 m/y. The overall gradient of 0.0034 may be obtained from wells H-1 and H-11 (923.3 m -913.1 m -10.2 m), which are approximately 3000 meters apart.

The bend in the travel paths suggest a somewhat longer distance than a straight line approximation, and for most of the paths, 3250 m would be a reasonable estimate. Assuming an effective porosity of 16 percent, as was done in the 1992 PA, and applying Darcy's law, a groundwater velocity of 0.47 m/y was calculated which results in a travel time of approximately 6,900 years. If the shortest possible travel path was assumed the travel time would have been 5300 years. All of these assumptions are reasonable and yet, the shortest travel time predicted by the 1992 PA was 9000 years.

Table 1. Hydraulic Property Data for the Culebra Dolomite

	Transmissivity		Hydraulic Conductivity
Well No.	m^2/sec	m^2/yr	m/yr
H-1	9.4×10^{-7}	29.64	3.85
H-3	2.5×10^{-6}	78.84	10.24
DOE-1	1.2×10^{-5}	378.40	49.14
H-11	3.1×10^{-5}	97.76	12.70

Response. *The shortest travel time predicted by 1992 PA was likely less than 100 years due to the use of a fractured media assumption for radionuclide transport.*

EPA Comment 123

Flowpath: Volume 4, Chapter 6.8

The southern and southeast quadrants are shown to have high transmissivities in Figures 6.8-2 and 6.8-3. Figures 6.8-8 and 6.8-11 show travel paths that veer southeast before turning south. Given that the flow model has numerous "calibration points" and the location of highly transmissive zones in the transmissivity figures, it would appear that a reasonable alternative conceptual model could include a dominant travel path that goes directly south from the repository site to the boundary, thus reducing the travel time needed to reach the boundary. Has this alternative model been considered in DOE's analysis?

Response. *The alternative model proposed by the commentor (i.e., flow directly south from the repository) is included through the stochastic variation of input parameters for the more general SECO-2D computational model. The flow paths shown in Figures 6.8-8 and 6.8-11 indicate a predominant southeast and south flow direction from the repository. These paths correspond to the 70 transmissivity field realizations performed as a part of the analysis described in Section 6.8. The flow paths are based on SECO-2D calculations performed with different transmissivity fields and on the hydraulic head measurements made in surrounding observation wells (i.e., calibration points). Depending on the realizations performed, some of* *the flow paths are directed due-south from the repository. These flow paths, as well as all other computed flow paths, are used to evaluate the groundwater travel times shown in Figure 6.8-9. There is no need for a special model within the probabilistic framework of the PA.*

EPA Comment 124

Spalling form Borehole Wall: Volume 4, page 7-1, line 18

What is the progress of the work to include spalling of the borehole wall? What knowledge was gained from the laboratory work?

Spallation needs to be incorporated into the modeling.

Variables Not Defined: Volume 4, page 7-9, line 53

Variables Q and R_i are not defined.

Assumptions Not in PA: Volume 4, page 7-10, line 32

Modeling assumptions and approaches should be included in the PA/Application. Information of this importance should be included not referenced.

Response. *Preliminary models for spalling and erosion of the borehole wall have been developed and are being revised and improved. Laboratory testing has been ongoing since 1994. The current status of the spallation model is described in the Disposal Room Position Paper (Butcher et al., 1995).*

The omission of definitions for Q and R_i and the comment regarding modeling assumptions will be taken into consideration so that a more complete presentation can be made in future documents.

EPA Comment 125

Volume 4, Page 1-2, Line 21

Uncertainty and sensitivity analysis also identify and direct experimental areas which can reduce uncertainty in PA results.

Volume 4, Page 1-3, Lines 9 & 10

This paragraph states that radionuclide transport was not modeled in the undisturbed scenario, because other results indicated that brine which was in contact with the waste did not reach the accessible environment during the 10,000-year regulatory time-frame. The decision not to model radionuclide transport seems logical provided that the incorporation of the radionuclides (and/or other waste constituents) will not increase the brine transport rate, e.g., mobilization of metals by leachate containing solvents.

Response. *The DOE is currently using uncertainty and sensitivity analyses to identify experiments that can reduce the uncertainties in PA. The 1992 PA and SPM-2 have been used to identify those experiments needed to reduce the uncertainty for demonstration of compliance.*

The concentrations and chemical reactions of the radionuclides will not increase brine transport to the unit boundary. Hence the use of brine travel time to limit radionuclide transport seems appropriate.

92PA EPA Comment 126

Volume 4, Page 3-1, Lines 32 to 35

Section 2.3.1 does not provide enough information regarding this variable. Please provide some additional clarification.

Volume 4, Page 3-2, Lines 17 to 19

Section 3.4.3 does not provide any discussion regarding this variable and the basis for determination of the range and median. Since the values were determined completely by investigator judgement (with no additional data sources), it would be helpful to know how the PA investigator determined the values.

Response. *The compliance certification package will contain an appendix, probably called PARameters, that will define all variables for the PA.*

With reference to the comment concerning Volume 4, page 3-1, line 32-35, the variable BCFLG is a pointer variable of characteristic curves for capillary pressure and permeability. The parameters used for these calculations are arbitrary with the initial ranges selected for the purpose of being able to run sensitivity studies. The ranges of the parameters for the model which supports generation of this variable were estimated using natural-analog data from materials which contain the same characteristics (permeability and porosity) as anhydrite, halite, and the waste room.

EPA Comment 127

Volume 4, Page 3-3, Line 8

Please clarify how the Culebra matrix porosity is used in BRAGFLO.

Response. *The Culebra matrix porosity was not used in BRAGFLO for the 1992 PA. Table 3-1 (CULPOR) should be changed by eliminating the reference to BRAGFLO to reflect this fact.*

EPA Comment 128

Volume 4, Page 3-4, Lines 19 to 33

Supporting information regarding the selection of range and median values for these variables should be provided in Volume 3, rather than referenced.

Volume 4, Page 3-6, Lines 28 to 35

Supporting information regarding the selection of range and median values for these variables should be provided in Volume 3, rather than referenced.

Response. *The compliance certification package will contain appropriate information regarding the selection of range and median value for PA variables.*

EPA Comment 129

Volume 4, Page 4-4, Lines 13 to 17

Please note that Figure 4.1.2 also shows the Unnamed Member. Given that the basal interval of the Unnamed Member contains siltstones and sandstones having a sufficient transmissivity to allow groundwater flow (Page 2-16, Volume 2, 1992 PA), it does appear that the Unnamed Member should be considered. Please clarify why the Rustler/Salado Residuum has not been included. Page 2-12 of Volume 2 of the 1992 PA states that in the shafts excavated at WIPP the residuum shows evidence of channeling, filling, fossils and bioturbation indicating some past dissolution occurred prior to deposition of the Rustler Formation (Holt & Powers, 1988). In addition, it is EPA's understanding that DOE has stated that vertical flow from the Residuum may be upward and into the Culebra Member.

Response. *Transmissivity values of the Culebra Dolomite range from 10^{-7} to 10^{-3} m^2/s. By contrast, the transmissivity values of the unnamed lower member is generally less than 6×10^{-10} m^2/s.*

The difference between the hydraulic head potentials in the units indicates drainage is very slow. Water levels take months to years to stabilize in wells completed in low-transmissivity zones such as the unnamed member. At the site, the relative head potentials between the Magenta and Forty-niner Members indicate that there is no modern vertical recharge from the Dewey Lake Red Beds into any portion of the Rustler Formation below the Forty-niner member. West of the site however, the decreasing difference between the Magenta and Culebra heads may indicate a combination of westward and downward drainage of the Magenta Dolomite.

From a hydrostratigraphic point of view, the bottom several feet of the unnamed member, consisting of anhydrite/gypsum, polyhalite and halite, represents a confining bed indistinguishable hydraulically from the underlying Salado Formation. The lower siltstone unit of the unnamed member (the transition zone and bioturbated clastic interval of Holt and

Powers {1988}) can be considered to be the lowermost Rustler water-producing zone, and the overlying halite and anhydrite/gypsum units act as another confining bed. The top unit of the unnamed member is composed of siltstone, mudstone and claystone. The PA for the final CCA will model the five members of the Rustler discretely.

EPA Comment 130
Volume 4, Page 4-8, Lines 19 and 23
Please provide the basis or reference for the assigned permeability values given for DRZ and waste. Please clarify if all the permeability values/ranges given in Lines 8 through 23 are intrinsic permeability.

Response. *Tests have been performed on simulated unprocessed waste with compaction of the material to full lithostatic pressure. Permeabilities were found for compacted combustible wastes, metals and glass, and sludges. Mean permeability was estimated based on a weighted volume average for these waste categories. The calculation of the range and median for the permeability of the waste can be found on page 3-130 in Volume 3 of the 91 PA document, SAND90-0893.*

Information on permeabilities for disturbed halite and anhydrite can be found in Volume 3, pages 2-36 and 2-61 of SAND92-0700. The value given for the DRZ on page 4-8 of Volume 4 is the same as that for disturbed anhydrite on page 2-61 of Volume 3. This value is an arbitrary (but conservative) choice made by SNL in the absence of specific information at the time of the 1992 PA. Values in the Table are for intrinsic permeability.

EPA Comment 131
Volume 4, Page 4-8, Line 37
Please correct the referenced section to Section 5.1.2.1.

Response. *Reference section will be corrected. Thank you.*

EPA Comment 132
Volume 4, Page 4-11, Lines 1 to 25
Please provide references for all values shown. In addition, clarify if the porosity values given here are total, effective, matrix, and/or fracture porosity.

Response. *The porosity values given on the cited page are total porosity values for the undisturbed case. In the BRAGFLO model, total porosity equals effective porosity, so the values are also the effective porosities. Matrix and fracture porosity components are not delineated on the cited page. In this analysis, the porosity values are time-independent.*

The porosity values for the undisturbed case are described in additional detail in Volume 4, Table 3-1 and Volume 3. In Volume 3, Salado (halite) porosity is described and source documents referenced in Section 2.3.7. In Volume 3, the Salado anhydrite interbed porosity is described and source documents referenced in Section 2.4.4.

EPA Comment 133
<u>Volume 4, Page 5-1, Line 25</u>
Should this sentence reference Figure 5.1-1 rather than Figure 4.1-1?

Response. *Yes.*

EPA Comment 134
<u>Volume 4, Page 5-3, Lines 8 to 16</u>
Please clarify why Figure 5.1-2 does not also show the Unnamed Member of the Culebra as does Figure 4.1.2. Again, as stated in an earlier comment regarding the use of BRAGFLO to model the undisturbed conditions, it does appear appropriate to consider inclusion of the Unnamed Member in the modeling grid. Please clarify why the Rustler/Salado Residuum has not been included. Please see the comment concerning Volume 4, Page 4-4. Lines 13-17.

Response. *Figure 5.1-2 does not show the Unnamed Member of the Culebra because the 1992 PA did not directly model this stratum. The rationale for eliminating the Unnamed Member from the model is discussed in the response to Comment 129. The PA for the final CCA will model the five members of the Rustler as discrete units.*

EPA Comment 135
<u>Volume 4, Page 5-5, Lines 20, 24, and 26</u>
Please provide the basis or reference for the assigned permeability values given for DRZ, waste and Castile brine reservoir. Please clarify if all the permeability values/ranges given in Lines 13 through 26 are intrinsic permeability.

Response. *Permeabilities for the DRZ, waste and Castile brine reservoir are addressed in Volume 3, Section 3.2, Volume 3, Section 3.4, and Volume 3, Section 4.3 respectively. The permeability values/ranges given in Lines 13 through 26 are intrinsic permeabilities.*

EPA Comment 136
Volume 4, Page 5-7, Lines 1 to 4
This section states that BRAGFLO uses intrinsic permeability rather than hydraulic conductivity for Culebra Member. Is the use of intrinsic permeability in BRAGFLO exclusive to the Culebra Member or is it used for the other strata as well?

Response. *The use of intrinsic permeability in BRAGFLO is not exclusive to the Culebra Member. In BRAGFLO, the intrinsic permeability and fluid viscosity are represented discretely in the groundwater flux equation. For additional information, refer to the DCCA, Appendix BRAGFLO: Two-Phase Flow.*

EPA Comment 137
Volume 4, Page 5-7, Lines 35 to 42
Please provide references for all values shown. In addition, please clarify if the porosity values given here are total, effective, matrix, and/or fracture porosity.

Response. *The porosity values given on the cited page are total porosity values for the disturbed case; this is similar to the values for the undisturbed case (see Comment 132). In the BRAGFLO model, total porosity equals effective porosity, so the values also are the effective porosities. Matrix and fracture porosity components are not delineated on the cited page.*

The porosity values for the disturbed case are described in additional detail in Volume 4, Table 3-1 and Volume 3. In Volume 3, Salado (halite) porosity is described and source documents referenced in Section 2.3.7. In Volume 3, the Salado anhydrite interbed porosity is described and source documents referenced in Section 2.4.4.

EPA Comment 138
Volume 4, Page 5-9, Lines 30 & 31
This section states that the relative permeability parameters are varied and are the same for all materials except the waste and the DRZ. How can the parameters be varied and the same? In addition, please note that the comment made addressing Page 4-26, Lines 4 to 7 would also apply here.

Response. *The wording of the section leads to a confusing statement. The statement may be said more accurately as: "Relative permeability parameters are varied in an identical way for all materials..." That is, the same method of variance was used for relative permeability parameters for all materials.*

EPA Comment 139

Volume 4, Page 6-2, Lines 5 to 15

This paragraph introduces the assumption that vertical flow is not occurring (or is occurring too slowly to be significant) between the Culebra and the overlying (Tamarisk) and underlying (Unnamed) members of the Rustler Formation as well as between the Culebra and the Rustler-Salado Residuum. The information provided on page 2-34 of Volume 2 is not detailed enough to substantiate this assumption. Please provide a more detailed discussion regarding supporting information for this assumption. In addition, please specify and discuss the laboratory and/or field data which will be used to support the regional hydrologic modelling proposed here.

Response. *The following is a summary of several descriptions included in the 1992 PA reports:*

Reference is made to Haug et al., 1987 and Davies, 1989 in Volume 2 (page 2-30) for additional information on the no vertical flow assumption. Brinster (1991) and Beauheim (1987a) present analyses of vertical hydraulic gradients on a well-by-well basis. These analyses suggest that, if flow occurs, the direction of flow between the Magenta and the Culebra is downward through the WIPP area. Directly above the repository, flow may be upward from the Rustler-Salado residuum to the Culebra Dolomite. Elsewhere in the region, both upward and downward flow directions exist between the two units.

The Salado Formation has very low permeabilities. Table 2.4.2 of Volume 3 (page 2-59) provides a summary of measurements of Salado anhydrite interbeds. Figures 2.4-2 and 2.4-3 provide permeability distribution data used in the 1991 and 1992 PAs. Permeability of the anhydrite layers is on the order of 10^{-19} to 10^{-21} m^2. These permeabilities will result in very limited flow. This is further substantiated by the very limited seepage conditions which occur in the Salado Formation today, as observed directly in the WIPP underground facility.

b) As noted in Volume 4 on page 6-2, the validity of the assumption that leakage between the Culebra and the over-and underlying-units can be neglected is uncertain. The WIPP Project is currently performing three-dimensional, regional hydrological modeling to evaluate the importance of vertical fluxes between the Culebra and adjacent strata. In addition, single and multiwell conservative tracer tests are planned to evaluate the effects of heterogeneity, anisotropy, and layering on transport (Sattler, A.R., 1995).

EPA Comment 140

Volume 4, Page 9-4, Lines 10 to 44

This discussion states that regression analyses indicates that Salado halite and anhydrite permeabilities are considered important parameters with respect to the total projected releases from WIPP. The discussion further states that the uncertainty in the permeability values can be decreased with continued in-situ investigation. SAND93-1197 indicates that the experimental values will not be available until December 1996. In light of the accelerated compliance schedule, has the experimental schedule been revised to allow an earlier incorporation of the measurements into the PA calculations?

In addition, since this experimental program is initially focusing only on the anhydrite interbreeds, has a determination been made as to whether or not to expand the program to encompass both impure and pure halite?

Response. *Following the SPM-2 iteration, the schedules of all high priority experimental activities have been revised to allow input of new data to the PA models by March, 1996. These high priority experimental activities include laboratory testing and in situ field testing for hydrological and transport properties of the Culebra. However, further permeability testing for both pure and impure halite is not planned because repository performance appears insensitive to the uncertainty in Salado permeabilities.*

EPA Comment 141

Volume 4, Page 9-8 and 9-9, Table 9-3

Capillary pressure and relative permeability parameters (Brooks-Corey/van Genuchten-Parker parameters) are listed in this Table as being less important parameters with respect to compliance for the disturbed performance scenarios. SAND93-1197, however, indicates that threshold displacement pressure, one of the capillary pressure/relative permeability parameters, may be an important parameter. Page 10 of that report states that the uncertainty in the threshold displacement pressure values for the Salado rock is large and may prevent a clear prediction of repository behavior in both the undisturbed and disturbed scenarios. Please clarify.

Response. *The referenced report (Howarth, 1993) concerns the Salado Two-Phase Flow Laboratory Program. One of the objectives of this program is to quantify the Salado anhydrite rock and flow parameters that describe the ability of the Salado to transmit and store brine and gas as a function of the initial conditions and time-dependent material damage.*

The flow of waste-generated gas from the repository is predicted to be controlled by three physical properties of the Salado: (1) pore fluid pressure, (2) threshold displacement pressure, and (3) gas-brine relative permeability. The permeabilities of both halite and anhydrite were very important parameters in the 1992 PA as shown in Table 9-3 (WIPP 1992 PA, Vol. 4). Since neither threshold pressure nor relative permeability for the Salado had been measured before the 1992 PA, the Brooks and Corey and van Gnuchten-Parker correlations were used to model these parameters. These two models are based on capillary relationships from a sandstone core from which wetting phase relative permeability is derived.

Whereas it is true that there is uncertainty associated with not having measured two-phase properties, PA has developed an approach for addressing this uncertainty based on sampling the van Genuchten-Parker and Brooks-Corey sets of equations and their associated parameters. This approach provides a rational means of predicting repository behavior in both the undisturbed and disturbed scenarios. In effect, the importance of reducing uncertainty for performance assessment (as established by ranking the sensitivty of compliance against other parameters) is not necessarily the same as stated for the scientific studies described in SAND93-1197.

EPA Comment 142

<u>Volume 1, Page 1-1, Lines 35 to 45</u>
This section should be updated to discuss the Experimental Program Plan, rather than the Test Phase Plan.

<u>Volume 1, Page 1-2, Lines 1 to 7</u>
This section should be updated to discuss the Experimental Program Plan, rather than the Test Phase Plan.

<u>Volume 1, Page 1-2, Lines 9 through 11</u>
This paragraph states that the results of laboratory and field studies conducted during the Site Characterization Phase form the basis for the data used in the PAs. It is EPA's understanding that the Site Characterization Phase was not conducted under the same level of quality assurance currently employed by DOE today. It is also EPA's understanding that DOE recently has evaluated the quality of the data collected during the Site Characterization Phase. Since so much of the PA is based on this data, it would be helpful if a discussion of the quality evaluation results were included here.

Response. *Following the SPM-2 iteration, the scientific activities for the WIPP Project have been refocused on those high priority activities that are required for a successful compliance certification application. The Experimental Program Plan, which was written before the SPM process began, is now obsolete and will not be reissued by the CAO. Rather, individual Test Plans for the eight high-priority scientific activities have been or are being developed by SNL.*

The CAO is aware that the Site Characterization Phase was not conducted under the same level of quality assurance currently employed by DOE today. A QA process has therefore been established to evaluate and requalify these old data using independent technical review (ITR) teams. The results of the reevaluations by the ITRs will be discussed in the CCA.

EPA Comment 143

Volume 1, Page 1-8, Lines 30 to 36

DOE may wish to note here or in subsequent chapters of the PA (e.g., Volume 3, Chapter 3, Page 3-68) that additional generator/storage facilities may be sending TRU and mixed TRU waste to WIPP for disposal. For example, Volume 2 of the April 1993 Interim Mixed Waste Inventory Report states that DOE also plans to ship to WIPP wastes generated by the Energy Technology Engineering Center located in California (waste volume currently in storage reported as 2.32 m^3) and the Argonne National Laboratory-West located in Idaho (waste volume currently in storage reported as 0.59 m^3 and waste volume to be generated reported as 2.15 m^3).

Response. *Updated versions of the PA will include waste inventories defined by the WIPP Transuranic Waste Baseline Inventory Report (WTWBIR). This report is an aggregate representation of all the currently stored, projected and scaled inventories scheduled to be shipped to WIPP during the disposal period. Several smaller generators may also ship wastes to WIPP, assuming all acceptance criteria are attained. However, most of the smaller generators will ship their wastes to the larger facilities for certification and transport to WIPP in the TRUPACT II containers.*

EPA Comment 144

Volume 3

It would be extremely helpful if either the PA or the EPP had identified the experimental studies which are intended to reduce the uncertainty of the parameters identified in Volume 3.

As stated before in the January 1994 comments, the process of identifying each input parameter and their values, each variable input parameters and their distribution should be addressed in detail.

Response. *Following the SPM-2 iteration, the scientific activities for the WIPP Project have been refocused on eight high priority activities that are required for a successful compliance certification application. In general, these eight activities will also reduce the uncertainty in the critical parameters and in the CCDFs themselves.*

The CCA will provide complete documentation of input parameters, including their values and/or distributions and the sources for the data. Appendix PARamters in the DCCA is the first draft of this documentation for the WIPP Project.

EPA Comment 145

Volume 3, Page 1-21, Lines 3 to 5

This section states that most of the 49 sampled parameters were assumed to be independent random variables even though some was known to be dependent upon others. What efforts have been made since the drafting of the 1992 PA or will be made in the future, to correlate appropriate parameters (e.g., porosity, permeability, transmissivity, storativity, hydraulic conductivity)? If such correlation is not planned, please discuss to what extent this lack of correlation of known dependent parameters will have on the credibility of the current PA modelling results.

Response. *Where correlations were known in the 1992 WIPP PA, they were described in Table 3-1, Volume 4. However, there were very few correlations. Where correlations in sampled parameters are known, they will be reflected in the next PA iteration. It is thought that the lack of correlation between known dependent parameters will have the effect of making any performance assessment overly conservative because dependent variables will be modeled as independent variables.*

EPA Comment 146

Volume 3, Page 2-3, Figure 2.1-3

This map is intended to show topography, well location and modeling domains. Yet, the scale is not large enough to provide such information in an usable fashion. A topographic map as described in 40 CFR Section 70.14(b)(19) of EPA's RCRA regulations would be more useful for discerning topography and well locations.

Response. *We believe the scale used in this map is appropriate because of the largeness of the domain discussed in this volume.*

Similar information can be found in Figure 3 in SAND88-7002 (Figure 3.1); Figure IV-14 in SAND89-7147; and Figure 3 in SAND93-2266. The first two maps listed above may require some updates to reflect the subsurface mine/unit and any proposed wells/borings.

An appendix containing all borings within the WIPP site boundary is found in the DCCA, March 1995. DOE has identified over 118 holes for this appendix. The CCA will contain additional maps as required.

EPA Comment 147

Volume 3, Page 2-6, Figure 2.2-2

The title of this figure is "Reference local stratigraphy near repository." It is recommended that the word "local" be deleted from the title. The intent of this figure and its derivation is explained in Volume 3, but is not explained prior to the figure's presentation in earlier volumes of the PA (Volume 1, Page 2-10, Figure 2-5 and Volume 2, Page 2-43, Figure 2-20a). In addition, it is assumed that the elevations are in reference to meters above mean sea level. Also, it should be noted that "Halite" is missing from the legend and that a well location map would be useful in interpreting this figure.

Response. *The word "local" was used because the stratigraphy is that which surrounds room D in the WIPP underground. The reviewer's assumption is correct that the elevations stated are meters above mean sea level. The legend for halite was inadvertently omitted; it will be included in future versions of this figure. Since the stratigraphy is local (around room D in the WIPP facility), a well location map would not be necessary because there are no wells drilled from the surface in the immediate area of room D.*

EPA Comment 148

Volume 3, Page 2-12, Lines 3 to 27

This section states that the capillary pressure and relative permeability parameters (i.e., residual wetting (brine) phase saturation) have not been experimentally determined for the Salado halite (permeability of 10^{-19} to 10^{-24} m^2) or anhydrite (permeability of 10^{-16} to 10^{-21} m^2). Instead, measurements from a low permeability sand (permeability of 10^{-16} to 10^{-19} m^2) have been used since such measurements have not been made for other material with a lower permeability. Since sand has a relatively high porosity, it is assumed that the sand referenced here, is actually a cemented sandstone or perhaps a quartzite. Please state the possible implications or effects of using an analog having a higher permeability and different physical and chemical properties than halite and anhydrite. SAND93-1197 notes that the analog values were developed from only one core sample and that the validity of these values for use with respect to Salado rock has not been experimentally justified. Such information should be provided in the PA. This is particularly important in view of the statement made on page 10 of SAND93-1197 that the uncertainty in the threshold displacement pressure values for the Salado rock is large and may prevent a clear prediction of repository behavior in both the undisturbed and disturbed scenarios.

Response. *The possible implication of using an analog having a higher permeability and different physical and chemical properties than halite and anhydrite is that the calculations might represent a condition that is not realistic for WIPP. DOE has allowed for this possibility by attaching a wide range of uncertainty to the tight gas sand analogue measurements. This uncertainty is manifest in the PA approach by using both equation sampling (Brokes-Corey versus van Genuchten-Parker) and parameter sampling (S_{t} S_{gr}, λ, P_{t}).*

EPA Comment 149

Volume 3, Page 2-12, Lines 3 to 27

It is EPA's understanding that the Salado Two-Phase Flow Laboratory Program was established to provide measurements of threshold pressure, relative permeability, capillary pressure, rock compressibility, total and effective porosity, intrinsic permeability and core damage assessment for the Salado anhydrite. Such measurements will decrease the level of uncertainty of the parameters discussed here in Volume 3. SAND93-1197, however, states that the data results and reports will not be available until December 1996. In light of the accelerated compliance schedule, has this laboratory program schedule been revised to allow an earlier incorporation of the measurements into the PA calculations? In addition, since this laboratory program is initially focusing only on the anhydrite interbeds, has a determination been made as to whether or not to expand the program to encompass both impure and pure halite?

Response. *Following the SPM-2 iteration, the schedules of all high priority experimental activities have been revised to allow input of new data to the PA models by March, 1996. These high priority experimental activities include laboratory testing and in situ field testing for hydrological and transport properties of the Culebra. However, further testing under the Salado Two-Phase Laboratory Program is not planned because repository performance appears insensitive to the uncertainties in hydrological parameters in the Salado anhydrite and halite.*

EPA Comment 150

Volume 3, Page 2-12, Lines 3 to 27

It also would be helpful, if the PA referenced the experiment proposed to reduce uncertainty for particular parameters. This would alert the reviewer that DOE anticipates reducing the uncertainty associated with particular parameters and would enhance credibility of future modelling results. For example, the parameters provided in this section of Volume 3 have not been measured for the Salado halite and anhydrite, necessitating the use of natural analogs. The EPP notes that three of these parameters are proposed to be measured for future PA calculations. These parameters are threshold pressure, relative permeability and porosity. SAND93-1197, discusses, in greater detail, these same parameters and 4 additional parameters which will be addressed in the Salado Two-Phase Flow Laboratory Program. Providing the reviewer the information that the parameters will be measured at a future data will elicit less criticisms regarding the use of analogs.

Response. *The CAO has recently identified eight scientific activities that are critical for demonstrating compliance. The selection of these activities is based on programmatic and schedule considerations, input from the WIPP scientific advisor (SNL) and the results of the SPM-2 iteration. The EPA and stakeholder groups have had extensive input into this selection process, primarily through technical interchanges and review of the Position Papers during the*

SPM process. The relevant Position Papers, which describe the experimental programs in general terms, are supported by detailed Test Plans that have been or are being prepared for individual experiments. As noted in the response to Comment 149, further testing under the Salado Two-Phase Laboratory Program is not planned because repository performance appears insensitive to the uncertainties in hydrological parameters in the Salado anhydrite and halite

In addition to the Position Papers, the CAMCON data base and code interrelationships (described in Volume 2, Section 3.1, page 1-3) provides the data management relationships between the PA models and the reference material properties databases. PA/QA is extending the data management interfaces to the source documents which contained the initial data for the material properties and to tests, experiments, and other analyses that are planned to reduce the uncertainty of the stated parameter values (described in the PA reports).

EPA Comment 151

Volume 3, Page 3-29, Lines 17 to 33

Pursuant to the Integrated Data Base, 70 radionuclides have been identified as known components of CH TRU waste. Of these 70 radionuclides, 23 are considered to be the primary radionuclides in the CH TRU waste inventory. These 23 radionuclides are being used in calculating the cuttings radionuclide releases resulting from human intrusion and in calculating the radionuclide concentrations within the repository prior to transport into the Culebra. Only 9 of the 23 radionuclides are being used to calculate transport within the Culebra. There should have been a more complete discussion on the rationale behind this. Is the decrease in the number of radionuclides being considered in the calculations due to radioactive decay and assumptions regarding the length of time it will take for the release to reach the Culebra?

Response. *WIPP calculations use eight radionuclides for calculating release values, not nine as indicated in the comment. These eight radionuclides are selected on the basis of their contribution to the total radioactive material inventory that is available for transport. The primary selection parameters are the quantity of the material in the waste and its half-life ($t_{1/2}$). The release quantities for the eight radionuclides are adjusted through decay calculations using the PANEL code.*

In general, a radionuclide with a $t_{1/2}$ of less than 20 years is not included in the analysis. However, if there is a large quantity of radionuclide or if there may be significant in-growth due to radioactive decay of other radionuclides, it may be included (such as Curium with a $t_{1/2}$ of less than 20 years). The change in the amount of radionuclides over time is also factored into this selection process.

EPA Comment 152
Volume 3, Page 4-8, Line 44
Please clarify why 0.0889 was selected.

Response. *This value (0.0889 meters, or 3.5 inches) was chosen to reflect uncertainty about the diameter of future oil and gas boreholes. It corresponds to the difference in diameter today between standard gas holes at the WIPP horizon (14 inches) and the largest diameters in the region (17.5 inches), generally used at higher horizons.*

EPA Comment 153
Assurance Requirements
The language in 40 CFR part 191 says that active institutional controls are not to be considered for more than 100 years. It does not state that active controls can automatically be considered for 100 years. There is an implied burden on the applicant to justify the effectiveness of such controls for any period of time up to 100 years. DOE needs to describe in some detail its plans and commitments for providing active institutional controls which will be effective in precluding inadvertent human intrusion during the 100-year period after disposal.

In the 1992 PA, SNL states that credit for 100 years of active institutional controls is taken by assuming that no intrusions occur during that period (for example, SAND 93a, p. 2-21 or SAND 92a, p. 5-18). However, many of the diagrams presented in Appendix D, SAND 92c show non-zero intrusion rates. DOE needs to clarify how the period of assumed active institutional controls was actually treated.

Response. *DOE's plans and commitments for institutional controls are outside the limits of the 1992 PA. Further information on these plans and commitments for institutional controls can be found in the response to Comment 5.*

The 1992 PA assumes no intrusions during the 100-year institutional control period.

EPA Comment 154
Inventory and Area of Repository: Volumes 3 and 4 and the February 1994 Presentation
According to the February, 1994 presentation by Harold Iuzzuloni the WIPP inventory as modeled is assumed to be homogenized with an activity per square meter of about 37.89. This number comes from 4.226 million Curies divided by 0.115 km^2, the area of the repository. $(4.226 \times 10^6$ Ci$)/.111520 \times 10^6$ $m^2) = 37.9$ Ci/m^2. However, in Volume 4, the area of the repository used for drilling rate calculations is 0.126 km^2. $(4.226 \times 10^6$ Ci$)/.126 \times 10^6$ $m^2) =$ 33.5 Ci/m^2.) The only place areas are listed is Table 7.3-3 on page 7-15 of volume. Even in the table, it is not easy to understand the reasoning for the different areas, making it difficult to reproduce analyses in the PA. Unclear information such as this could be grounds for considering an application incomplete.

Response. *The activity per square meter is based on the floor space of the repository (0.115 km^2). The area of the repository for drilling rate calculations equals the area of the floor space plus the area for the RH-TRU waste canisters, which are emplaced in the walls of the emplacement rooms and access tunnels.*

Inventory

Volume 3, p. A-138

Given the information in Table 2 on page A-138 and the limited discussion on its development (and use), it is not possible to reconcile Table 2 with the 4,225,000 curie inventory in Table 1 on A-137. Using the numbers provided in the table (p. A-138), the projected total curies is between 650,000 and 36 million curies, with an average drum equivalent curie content of between 0.8 and 6 curies. One set of our calculations is close to the 4.2 million curies used in the PA analyses, but it was still off by 5% at the closest. A discrepancy in the inventory and its use has implications in potential releases from the repository, especially releases to the surface from drilling. The uncertainty in the inventory and its distribution within the repository should be more closely analyzed. EPA should be able to reproduce DOE's calculations, and so it will be necessary for DOE to provide a clear discussion and a traceable set of calculations. For example, the 8.22 box to drum ration was not with the inventory information in Peterson's memo in Appendix A.

Estimates of Currently Stored and Future Projected TRU Waste
Calculated As Listed in Table 2, A-138

Total Drums	Lowpt. Ci/drum	Weighted Avg.	Total Drums	Midpt. Ci/drum	Weighted Avg.	Total Drums	Hipt. Ci/drum
207,371	0	0	207,371	0.25	0.07	207,371	0.5
55,380	0.5	0.04	55,380	0.75	0.05	55,380	1
294,292	1	0.38	294,292	5	1.92	294,292	10
91,048	10	1.19	91,048	15	1.78	91,048	20
95,744	20	2.50	95,744	60	7.50	95,744	100
21,928	100	2.86	21,928	550	15.75	21,928	1000
Total Drums	Average Activity (Curies)	Total Activity (Curies)	Total Drums	Average Activity (Curies)	Total Activity (Curies)	Total Drums	Average Activity (Curies)
765,813	7.0	5,340,620	765,813	27.1	20,736,231	765,813	47.6

Estimates of Currently Stored and Future Projected TRU Waste
Curie Activity in Boxes Were Divided by 8.22 to Get DrumEquivalent

Total Drums	Lowpt. Ci/drum	Weighted Avg.	Total Drums	Midpt. Ci/drum	Weighted Avg.	Total Drums	Hipt. Ci/drum
207,371	0/8.22 = 0	0.0	207,371	0.25/8.22 = 0.01	0.01	207,371	0.5/8.22 = 0.06
55,380	0.5/8.22 = 0.06	0.0	55,380	0.75/8.22 = 0.01	0.01	55,380	1/8.22 = 0.12
294,292	1/8.22 = 0.12	0.05	294,292	5/8.22 = 0.61	0.23	294,292	10/8.22 = 1.22
91,048	10/8.22 = 1.22	0.14	91,048	15/8.22 = 1.82	0.22	91,048	20/8.22 = 2.43
95,744	20/8.22 = 2.43	0.30	95,744	60/8.22 = 7.3	0.91	95,744	100/8.22 = 12.17
21,928	100/8.22 = 12.17	0.35	21,928	550/8.22 = 66.91	1.92	21,928	1000/8.22 = 65
Total Drums	Average Activity (Curies)	Total Activity (Curies)	Total Drums	Average Activity (Curies)	Total Activity (Curies)	Total Drums	Average Activity (Curies)
765,813	0.8	649,710	765,813	3.3	2,522,656	765,813	5.8

92PA EPA Comment 155

Inventory: Volume 1, page 2-16, ¶1

States that "...all analyses [involving the inventory] will be based on current projections of a design volume inventory, estimated at about 532,500 drums and 33,500 boxes of CH-TRU waste." At 1 box = 8.22 drum equivalents, this totals 807,870 drum equivalents, yet the PA analysis apparently uses 765,813 as described in volume 3 page A-138 and the 11/12/92 note that Harold Iuzzolino used in his presentation at the February, 1994 technical exchange.

In a previous PA (SAND90-2347) SNL has estimated that the capacity of the WIPP is as much as 863,000 drum equivalents and about 10,000,000 curies (and nearly 12 million curies in the 1991 PA). The current PA uses an estimate of 765,813 drum equivalents and 4,226,000 curries. What is expected to be the maximum number of drums and activity level? These numbers should be made final for a draft application. These may be especially important in determining how large the repository will be because different activity levels may require different loading management approaches. This is in turn could affect the area of the repository.

Response. *The WIPP facility is designed to receive up to 6.2 million cubic feet (175,600 cubic meters) of contact-handled transuranic (CHTRU) waste and 250,00 cubic feet (7,080 cubic meters) of remote-handled transuranic (RHTRU) waste. However, the WIPP Land Withdrawal Act (LWA) limits the volume of CHTRU and RHTRU waste to be emplaced at the WIPP to 6.2 million cubic feet (175,600 cubic meters), of which only 250,000 cubic feet (7,143 cubic meters) can be RHTRU waste. The LWA further restricts RHTRU waste to a maximum activity of 23 curies per liter, not to exceed a total of 5.1 million curies [LWA §7, Public Law 102-579]. Page A-138 of the 1992 PA (SAND92-0700 v.3) gives a total estimate of 517,182 drums and 28,207 waste boxes. The estimated total curie content in the 1992 PA is 8.206×10^6 CHTRU (p. A137) and 3.54×10^6 RHTRU, for a total of 11.746×10^6 curies. As the memo (pp. A135-A136) explains, these values were obtained from the 1991 Integrated Data Base (IDB) inputs.*

This inventory estimate has now (1995) been superseded. Current anticipated TRU waste inventories are derived from the Waste Isolation Pilot Plant Transuranic Waste Baseline Inventory Report (WTWBIR). To maintain a consistent volume for performance assessment, the data in the WTWBIR are scaled to the full volume of the repository. The latest available revision of the WTWBIR will be used in all future performance assessment.

The normal disposal room configuration is waste in drums stacked in units of seven, three drums high, surrounded by backfill in waste storage (disposal) rooms 4 m (13 ft) high, 10 m (22 ft) wide, and 91.4 m (300 ft) long. For computational purposes, the absolute maximum (perfect) packing of 6,804 drums within each room is assumed, even though it is unlikely in practice that so many drums can actually be emplaced within a room. After the eight panels *(seven rooms each) are filled, waste will be emplaced in the four access drifts. Waste will also be placed in accessways to the storage panels in the same mode as in the storage room.*

To date there has not been a need to resolve effects introduced by emplacement of different types of waste in different regions of the room. The waste is assumed to be a homogeneous mixture throughout the repository.

Comment 156

Communication: All volumes

The discussion in the text of the modeling that was done needs to better reflect what actually has been modeled. For example, throughout the PA the E1E2 scenario is described as a two borehole scenario. However in chapter 5, Volume 4, there a several diagrams of the model geometry, and none of them has more than one borehole. The difference between the E1 and E1E2 scenario is the assumptions that used in each scenario. This was briefly described in the 1991 PA, but it should have been explicitly stated in the 1992 PA.

Response. *The text describing the modeling assumptions for the E1 and E1E2 scenarios appears in Section 4.2.4 of Volume 2 of the 1992 PA. These assumptions will be cross-referenced or explicitly stated, as you suggest, in the CCA.*

EPA Comment 157

Referencing: Future documentation

It has been stated by DOE and Sandia that the 1992 PA and earlier PA reports were "snapshots" in time. Information is added or changed in each report, resulting in a dynamic set of interpretations, with some being abandoned. For this reason, future performance assessment reports should not reference previous performance assessments.

Response. *The final CCA will be a standalone document that fully describes the PA models and calculations for compliance certification purposes. Some information, such as code manuals and QA documentation, will obviously be referenced, but it is the CAO's intent to prepare as complete a compliance certification package as possible.*

EPA Comment 158

Volume 5, page 1-1

Undisturbed performance is also of interest for the Containment Requirements (191.13) of 40 CFR 191 (line 30).

Response. *Your observation is correct. Omission of a reference to the containment requirements was simply an oversight.*

EPA Comment 159

Volume 5, page 2-7

The modeling of formation dewatering during the disposal stage should more reasonably represent the condition when a panel is sealed. However, using a 50-year disposal period seems excessive. After disposal operations are underway it should be possible to excavate a panel in one to two years, fill it with wastes in three or four years, and backfill plus seal in another one or two years. This would total only five to eight years. The observed problems of room stability when rooms remain open for extended periods should encourage a policy of minimizing time between excavating and sealing of a panel. Even recognizing that Panel 1 and the north/south access drifts will be open for longer periods, the use of an average time of 10 or 15 years would be more reasonable (lines 26-33).

Volume 5, page 2-8

The discussion of the manner in which the initial brine saturation range was chosen is not very reassuring. This is stated on page 4-1 to be a very important parameter. Yet, no data exists and the Experimental Program Plan apparently will not obtain data. The reduction from the 1991 PA values, which included the maximum value, appears to have been done for modeling convenience. Data should be obtained on this parameter. If this is not done, then bounding analyses could be used to identify the value leading to the greatest consequences (lines 25-38).

Volume 5, page 2-9

The development of a model for pressure dependent fracturing of anhydrite interbeds is important and needs to be accomplished as soon as possible. Also, the degree of correlation between permeabilities and porosities needs to be determined.

Response.

A shorter 5-year, start-up period is now used.

Admittedly, the range of values chosen for initial brine saturation in the 1992 PA was "somewhat arbitrary" [Vol. 5, page 2-8, line 35]. However, the estimated range of initial brine saturation has since been reduced to between 0.004 and 0.052 based on recent EG&G/INEL data and transportation restriction on the amount of free liquid the waste can contain.

A model for interbed fracture has been developed and implemented in PA. Although work is underway, there have been no new developments on ascertaining the correlation, if any, between permeability and porosity in the interbed alteration model. The model implemented in BRAGFLO since the 1992 PA for pressure-dependent alteration of the anhydrite interbeds contains a pressure at which alternation begins. At pressures above the alteration pressure, permeability increases by the magnitude of porosity raised to a power (the so-called power-law *assumption). Concerns about the power-law assumption for correlating permeability and porosity resulted in a suggestion for an alternative approach (Memo form K. Larson and P. Davies to M. Tierney, October 11, 1993, found at Appendix D, Salado Position Paper, March 17, 1995). This alternative approach correlates permeability changes directly with fracture aperture, and for that reason has been termed the Aperture Model. Although not implemented in performance assessment, the Aperture Model illustrates the uncertainty that exists in attempting to correlate permeability with porosity.*

92PA EPA Comment 160

Volume 5, page 4-1, last paragraph

In the statement, "brine saturation in the waste rises steeply during the first 100–300 years as creep closure reduces the pore volume of the waste more rapidly...," the assumption and the numbers that are being used should be supported.

Volume 5, page 4-2, last paragraph

Based on what data or assumptions were used to come-up with the numbers in this paragraph.

Response. *The statement on page 4-1 in Volume 5 of the 1992 PA regarding brine saturation in the waste was intended as a general introductory remark and is based on computer runs that are reflected in Figures 4-2 and 4-3. This introductory statement is amplified on page 4-4, lines 18-33. Model assumptions for the disposal room are set out on page 2-12 in Table 2-1 of Volume 4.*

Based on this and other comments, the DOE will identify the sources of data for input parameters and to document the logic for derivation of parameter values in the final CCA in a straightforward and clear fashion.

EPA Comment 161

Volume 5, page 4-10

a) It is not clear how the total iron content was calculated.

Volume 5, page 4-14

b) Does the DOE design prevent fluids from bypassing the seals via the DRZ?

c) Drift seals as modeled do not prevent fluids from bypassing the seals by way of the DRZ. This result should also be true of panel seals and has implications for the E1E2 scenario where it is presently assumed that boreholes must be in the same panel to be hydraulically connected (lines 11–12).

d) The reference to plug flow at the bottom of the page is misleading since it suggests that this is how the brine will move. There is no reason to believe that such flow will take place. It is more likely that flow will occur by displacing portions of the brine already present in the pores rather than all of it. Furthermore, paths may be selective (i.e., MB-139) and brine will only move through a small percentage of the units and, therefore, not nearly all of the brine at a uniform radial distance would have to be displaced.

Volume 5, page 4-18

e) The results of the realizations have little meaning without the input parameters that went into each realization. In the future, identify where this information is provided.

Response.

a) The total iron content for the 1992 PA was the same as for the 1991 PA. SAND91-0893/3 Page 3-140 identifies iron from both the waste (Table 3.4-12 instead of Table 3.4-11) and the metal containers (Table 3.4-10 instead of Table 3.4-8).

b) No. Panel and drift systems do not impede fluid flow in BRAGFLO.

c) Panel and drift seals do not impede fluid flow in BRAGFLO.

d) Plug flow in the marker beds is currently one of several possible flow behaviors modelled in PA.

e) The final CCA will include all parameters for all realizations.

EPA Comment 162

BRSAT: Volume 5, page 5-7

BRSAT, Initial brine saturation in waste: Does this refer to the starting time of saturation?

"The variable selected in the analysis is BRSAT, the initial brine saturation in the waste, which has a positive regression coefficient and can account for 49% of the variability in gas generation by inundated corrosion." What is the range of parameter values that account for the 49% of the variability?

Response. *BRSAT refers to the initial water content of the waste at the time of repository closure. The initial water content is assumed to be a combination of liquid in the waste and brine in the backfill. For the 1992 PA, BRSAT parameters ranged in value from 0 to 0.14 with a median value of 0.07 [Table 3-1, Vol. 5; see also paragraph 3.4.3, Vol. 3].] This is the range of parameter values that account for the 49 percent variability in gas generated by inundated corrosion. Recent data has reduced the values and uncertainty in this parameter. See response to EPA comment #159.*

EPA Comment 163
Volume 5, page 5-9
The first paragraph needs rewording.

Response. *DOE will ensure that the final CCA is worded in as clear a fashion as possible.*

92PA EPA Comment 164
Volume 5, page 5-23, last paragraph
What is meant by last sentence? Based on what is written here, it is not clear that brine influx has minor impact on gas generation.

Response. *The intent of this discussion was to illustrate that corrosion of metals may be sensitive to other factors (other than brine inflow from the interbeds). However, it would be correct to say that anhydrite permeability does have an effect on corrosion rates and a higher permeability would have a higher brine influx which provides more water for corrosion.*

EPA Comment 165
Sensitivity Analysis Process: Volume 5, chapter 5
A more thorough explanation of the three analysis techniques; scatterplots, stepwise regression analysis, and partial correlations analysis may be very useful to assist the reviewer. We generally know how these techniques are used and applied, but how was each applied to the PA data? For all of the cases/runs what variable(s) varied and what parameters held constant? Was this procedure used in all cases, such as each major segment: "Brine Flow" or "Distance Gas Flows Out Anhydrite Layers" for example. It would be useful to show complete examples in an appendix of each technique.

Response. *DOE will show complete examples with greater discussion in the compliance certification application.*

EPA Comment 166
Volume 5, page 6-2
The most important parameter listed is the initial brine saturation. What is not listed and was not tested, but is probably of equal importance, are the initial conditions for pressure and saturations in the surrounding rocks.

Response. *Initial pressure in surrounding rocks was in fact tested and was found to be less important than initial brine content of waste. Initial pressure was set between 12 and 13 MPa at MB-139, and adjusted by a hydrostatic gradient to other units accordingly. Saturation is not varied.*

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1992 PERFORMANCE ASSESSMENT REPORT

DR. PATE-CORNELL REVIEW

IV-D-121

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September 13, 1995

SEP 1 1995
EPA AIR DOCKET

Environmental Protection Agency
Docket No. A-92-56, Air Docket
Room M-1500 (6102)
401 M Street, S.W.
Washington, D.C. 20460

To The Docket:

Enclosed is a report prepared for this office by Prof. Elisabeth Paté-Cornell, entitled Conservatism of the Performance Assessment and Decision Criteria for WIPP. Prof. Paté-Cornell is Professor of Industrial Engineering and Engineering Management at Stanford University and is currently President of the Society for Risk Analysis. She has written and lectured extensively on probabilistic risk assessment and has testified in Congress on proposed legislation on the subject.

The report constitutes further comments on the proposed compliance criteria, 40 CFR Part 194, and should be examined fully and carefully. Briefly, Prof. Paté-Cornell concludes as follows:

1. Generally, the 1992 performance assessment ("PA") in fact constitutes a conditional risk assessment, predicated on certain fundamental assumptions (made by EPA) as to the linkage between radionuclide releases and health effects and other assumptions (made by DOE or its contractor, Sandia) as to the probability and consequences of various release events. Whether the EPA assumptions or the DOE assumptions are conservative as judged by the outcome of a full probabilistic risk assessment of the WIPP repository is not known.

2. In inquiring whether the PA curve deemed determinative of compliance meets Prof. Paté-Cornell's standard--that high fractiles of the future frequency of exceedence of potential loss levels should be required to meet the performance criteria with a high level of confidence--it is important to know (a) what fractile of the current CCDF distribution the suggested curve--the mean--corresponds to, and (b) what fractile would the mean correspond to if some of the assumptions of the PA were instead treated probabilistically?

3. In pursuing the same inquiry, it is also important to know where the mean would fall if methods other than the use of expert opinion were used to obtain probability distributions for input variables.

4. Concerning the specific issue of the selection of experts for purposes of expert judgment elicitation, such persons should be required to meet a test of recognition by their peers in the scientific community.

5. Concerning elicitation of expert opinions on parameter values, the process must include the elements of (a) clarity of question, (b) identification of desired central value--probably the mean--and (c) the description of the thought process leading to the estimate.

6. Concerning the selection of variable parameters for PA, the test should be whether the variation of an input value across the possible range could change the final decision.

7. DOE should justify its decision to treat variables whose distribution is critical to the results through expert opinion rather than through experiments or measurement where feasible.

8. Concerning elicitation of expert opinions as to distributions of variable parameters, the process must include (a) construction of a probability distribution for a set of possible hypotheses, (b) identification of the appropriate distribution model for an identified model variable, and (c) given such model, identification of the distribution for the value of the variable.

9. Concerning aggregation of expert opinions of multiple experts, the process must include methods to reduce the range of disagreement, such as requirements that all experts (a) agree on the substance of the question, (b) consider and account for all available data, and (c) articulate the relationship between the data and their judgment as to the probability of the various models. Further, to aggregate different opinions, it is preferable to employ an interactive process wherein the experts (1) discuss the data, (2) explain their models, (3) discuss the probability of each of the models, (4) assess such probabilities, and (5) generate a composite distribution. Aggregation of multiple opinions must be performed systematically as to all expert elicitations. The task of quantifying the uncertainty of alternative assumptions cannot be ignored.

10. The rationality of the mean as a relevant characteristic of a probability distribution does not apply to collective decisions (such as governmental decisions), in which the administrator is concerned not only with the probability distribution of the levels of release but also with the health and safety of the most exposed members of the public, which involves

the choice of a threshold based on prudence. The mean may or may not reflect that threshold, depending on the fractile it represents and the practicality of so demonstrating.

11. A full uncertainty analysis includes (1) structuring alternative hypotheses into realizations so that probability distributions can be assigned to them, (2) aggregation of expert probabilities for each set of assumptions, (3) identification of the models and parameter values (with probabilistic treatment) which correspond to each hypothesis, including interdependencies, (4) propagation of uncertainties for each fundamental hypothesis, and (5) aggregating the results of conditional analyses according to the probabilities of the underlying hypotheses.

12. The full uncertainty analysis of WIPP has not been done and would be extremely difficult. In this situation, it is sensible to apply a test of reasonable expectations to the results of a conditional risk analysis based on fixed hypotheses, provided (1) that the hypotheses are globally conservative and (2) that the mean curves generally correspond to high fractiles of the CCDF families. In such situation the combination of hypotheses and means may provide "reasonable assurance." It must be demonstrated that the global model (health effects plus PA assumptions) is conservative and that a full uncertainty analysis achieves "reasonable assurance." It is appropriate for EPA to find a "reasonable expectation" only if its assumptions as to health effects (including its cancer risk model) provide the additional level of safety consistent with the NRC language of "reasonable assurance."

13. Such demonstration involves identifying the major hypotheses from EPA and DOE and assessing, by analysis of their probabilities and outcomes, their effect on the placement of the current mean curves.

14. EPA cannot simply frame a conditional risk analysis based on certain assumptions and then claim without checking that the conditional means resulting from this analysis necessarily support "reasonable expectation" of human safety. The effects of the hypotheses as to health effects and release models on the mean curves must be assessed. EPA must show that the combination of "reasonable expectation" for the PA and conservatism (if it is so) of the health effect model provides "reasonable assurance" of actual safety.

15. DOE, for its part, must identify the major hypotheses in its PA and show the effects of those hypotheses on the family of release curves. As an example, one can take the five or six most important assumptions of the PA (such as the hypotheses about the frequency, means, and effects of drilling; borehole diameters; groundwater flow model; solubility model; engineered barrier model), generate a set of reasonable alternatives, and show that

the mean curves generated with proper probabilistic analysis of the alternatives show compliance and do not move the mean curves toward lower fractiles of the CCDF families.

16. Depending on how far the current means are (assuming full probabilistic treatment of hypotheses) from a reassuring (but not sacred) 95% fractile, it may be appropriate to ask for additional analysis or a change in risk management strategy.

17. The test of 95% confidence to account for sampling error should be sufficient.

18. It is essential to deal with correlations among variable parameters.

* * *

We have undertaken to draft proposed regulatory language for §§194.26 and 194.34 following the analyses by Prof. Paté-Cornell, and it is attached to our comments, filed today. We request that the Agency consider and adopt the proposed regulatory language.

Very truly yours,

Lindsay A. Lovejoy Jr. (by AB)

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Portola Valley, July 5th, 1995

Mr Lindsay Lovejoy
Assistant Attorney General
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Santa Fe, New Mexico 87504

Dear Lindsay:

You will find enclosed here my final report to the Attorney General of New Mexico entitled: "Conservatism of the performance assessment and decision criteria for WIPP". I enjoyed my interaction with you in this work and I hope that we will have the opportunity to continue.

Sincerely yours,

~~M. Elisabeth~~ Paté-Cornell

CONSERVATISM OF THE PERFORMANCE ASSESSMENT AND DECISION CRITERIA FOR WIPP

Report to Office of the Attorney General of New Mexico

by

Elisabeth Paté-Cornell

July, 1995

CONSERVATISM OF THE PERFORMANCE ASSESSMENT AND DECISION CRITERIA FOR WIPP

by

M. Elisabeth Paté-Cornell

July, 1995

1. BACKGROUND AND PROBLEMS

WIPP has been basically constructed and is scheduled to start operating in 1998. At this time, the remaining operational decisions concern the potential need for engineered barriers, the management of the facility in the future, and the timing of the start of operations. EPA has been required by Congress to certify that WIPP will comply with Federal regulations for the storage of high-level wastes. A "performance assessment"(PA) has been done by Sandia National Laboratories for the Department of Energy which is in charge of the design, construction, and operation of the facility.

This performance analysis is in essence, a *conditional* probabilistic analysis based on *mixed methods* involving both a set of fundamental assumptions provided by EPA and a probabilistic release analysis conditional on these assumptions. (There is no probabilistic risk analysis *per se* because the consequences have been determined by EPA through a single-estimate method, presumably using a conservative model.)

1. The set of fundamental assumptions that have been adopted by the EPA concern mostly the linkage between health effects and radionucleide releases. The figures presented in Table 1, Appendix A of 40 CFR 191 are based on a number of hypotheses that I could not all identify. They involve, for instance, the assumption that the different isotopes are released to a large stream of water. EPA's assumptions also affect the framing of the risk analysis problem. For example, EPA has set the requirements "in terms of cumulative

releases of radionucleides at the accessible environment, either at the ground surface or anywhere at depth, 5 kms horizontally from emplaced wastes, over 10,000 years" (Lee). These hypotheses can generally be assumed to be conservative with respect to health effects, but it may not be the case, and their effects on the overall result has to be checked.

2. Sandia's performance assessment is a conditional analysis of the radionucleide release given the hypotheses and constraints set by EPA. It includes an uncertainty analysis within this framework. This analysis is restricted to uncertainties associated with the distributions of the variables of the conditional release model (such as λ, the mean number of human intrusions in 10,000 years per km^2). This uncertainty analysis does not involve the fundamental assumptions originally set by EPA in the containment requirements: these are taken for granted. Therefore, it does not reflect the uncertainties about the outcome of interest: the health effects of the potential release. A second set of assumptions were made by Sandia in the performance analysis. For instance, some of these distribution models were fixed, such as a Poisson model for human intrusions and a uniform distribution for its mean λ. The value of the parameter(s) of these distribution were either based on past data or on expert opinions (here λ=U[0,30]). The propagation of these uncertainties through the analysis has been performed using simulation (Monte Carlo or Latine Hypercube sampling methods) to obtain a description of the uncertainties about the release levels given the uncertainties about the inputs of the analysis (e.g., solubility factors).

The results of the performance assessment are thus families of risk curves that represent, for each release level, a discretization of the conditional probability of exceeding this value in a specified time window (10,000 years in most cases) given the analytical hypotheses specified by EPA. Note again that Sandia does not address directly the uncertainties about the health effects.

I do not know, given the way the performance analysis was done, whether these conditional results and their implications for health effects are conservative or not. In other terms, if instead of using the EPA assumptions plus additional assumptions of their own about the shape of the variable distributions, Sandia had done a complete uncertainty analysis (i.e., had assessed probabilities for these assumptions), would the curves obtained by this full uncertainty analysis about the release be above or under the current conditional curves? (I recognize, of course, that the uncertainty analysis has to stop somewhere). I can only *presume* that, in the EPA's generic studies that led to the release criteria, the accumulation of hypotheses that are generally intended to be conservative in the first place, lead to conservative results in specific analyses such as that of WIPP.

The question is thus whether the EPA hypotheses are in fact conservative with respect to the WIPP site. One of them, as mentioned above, concerns the release of radionucleides to the environment through a large stream, part of which will provide drinking water to the population. Whether this large stream assumption is conservative or not given that WIPP is in the desert, I do not know. Also, the argument was made that the assessment of cancer risk that led to this table was based on japanese epidemiologic data and that they have been found to be unconservative in later studies (EPA, background info, 1993, p 6-5).

It is important to note that the results of this kind of conditional PRA are not directly comparable to the results that one would have obtained if the EPA and Sandia's assumptions had been incorporated and weighted along with alternative assumptions in a fully probabilistic risk analysis of the health effects. Restricting the scope to release levels alone and to the hypotheses that led to Sandia's current results, Figure 1 shows a schematic representation of the full uncertainty analysis of release (for one single Hypothesis 1), and the restriction of Sandia's analysis to one particular realization of Hypothesis 1.

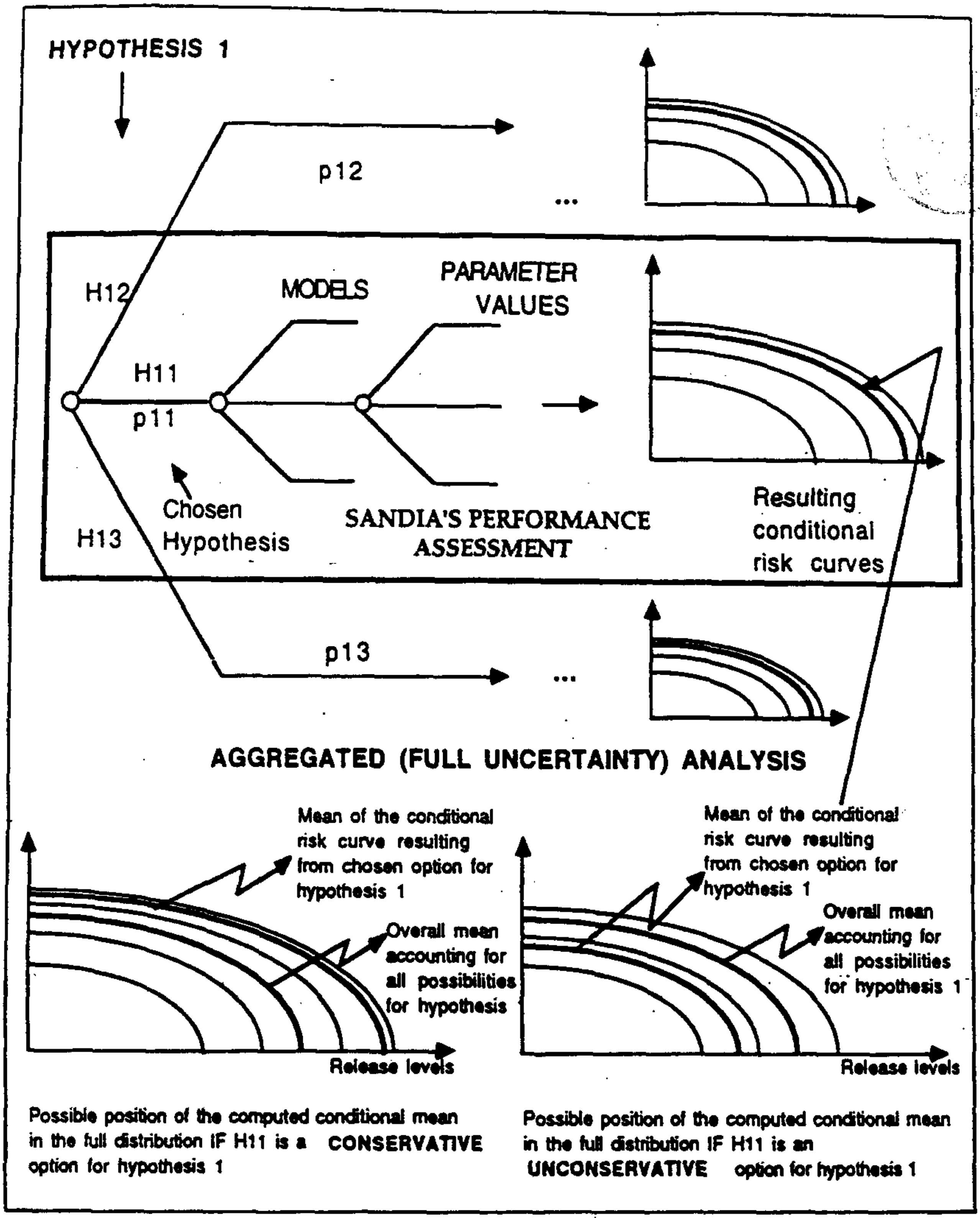

Figure 1: Conditional risk curves (CCDFs), and position of the conditional mean in a full uncertainty analysis.

This would have required using probabilities for the different possible realizations of each hypothesis instead of adopting what is probably (but not necessarily) conservative assumptions. Clearly, a full analysis of this type is complex. It would also involve much larger uncertainties because it would require assessing the probabilities of additional poorly known phenomena. Yet, it may be possible once the conditional analysis is done for the chosen case, to assess (even coarsely) the probable effects of alternative key assumptions on the final results. Therefore, the conservatism of the final safety levels achieved under the proposed criteria (e.g., specifying that the risk curve corresponding to the *mean* must meet the EPA release requirement) has to be examined in the light of the conservatism of the EPA (and later, Sandia's) assumptions.

EPA's compliance criteria involve several components: the assumptions behind the containment criteria, the criteria themselves, the CCDF characteristics (fractiles or moments) to be used to show compliance (second order), and the confidence level that the compliance criteria are met (third order). There are infinite combinations of such choices that lead to the same level of safety. EPA first made its own (single point, presumably conservative) analysis of the link between health effects and release levels. Then, they chose the mean CCDF for its robustness in the face of large uncertainties for the 10,000-year horizon, a set of analytical assumptions for the release model, and a high level of confidence for the mean. The first question is whether the choice of this combination is both prudent and practical. The second question is whether it provides a sufficient level of safety in terms of health effects.

In a 1986 paper, in the Journal of Nuclear Engineering and Design, I wrote an article entitled "Probability and Uncertainty in Nuclear Safety Decisions". In that article, I argue that both qualitative and quantitative safety goals are useful tools, and that *high fractiles* of the future frequency of exceedence of potential loss levels should be required to meet the performance criteria with a *high level of confidence* (hence the accumulation of two layers of conservatism). I assumed in that discussion that the PRA results came out of a full uncertainty analysis *on the outcomes* (here: health effects). I did not specify that a designated fractile should systematically be used (95% is one possibility). I believe that such a goal should remain flexible, depending on the case, alternative risk management options, and the difficulty of showing compliance (uncertainties, time horizon, etc.).

Therefore, I think that you want to know which one(s) of the *real* risk curves (that would result from a full uncertainty analysis as shown in Figure 1) are tangent to the compliance criteria curve, and possibly, what level of health safety do they represent. Two questions thus arise:

° What fractiles of the current distributions (conditional on Sandia *and* EPA hypotheses) do means correspond to in the *current* analysis? The means are shown in Sandia's results but, in each case, they do not correspond to one single CCDF curve on the whole range of release levels. Although the mean (predictably) appears to be in the high fractiles, I cannot tell which ones.

° Where would these means be in the full (marginal) distributions if some of the assumptions of the release model were treated probabilistically? In other words, how do the current assumptions affect the position of the current mean in the family of CCDFs?

The mean has several advantages in many PRA cases. First, it is compatible with economic efficiency criteria. Second, in the face of large uncertainties, the mean is relatively robust compared to specified fractiles (for example, it can be estimated with smaller sample size). Third, in PRAs as performed so far for nuclear power plants, the means are often among the high fractiles of the risk curves (e.g., 70%, 80% or 90%). This is true because many of the distributions that represent uncertainties in the results are skewed right. The position of the mean does reflect the level of uncertainty. Typically, the distance between the mean and the median is one measure of the uncertainty: the higher the level of uncertainty, the higher the fractile corresponding to the mean. Therefore, altogether, for studies involving a very high level of uncertainty, the advantage of the mean is that it is a robust estimate that generally corresponds to high fractiles.

In a recent paper entitled "A Perspective on the 1992 Performance Assessment for the Waste Isolation Pilot Plant" (EEG, 1995), William Lee argues that the analysis has been incomplete. I concur with him, to the degree that the Sandia conditional performance analysis does not allow me to estimate the conservatism of some of the basic hypotheses in the WIPP case. Eventually, the issues are: (1) To what extent will additional uncertainty analysis change the PA results? (2) Is it likely to make a difference in policy decisions given the release criteria as set? And (3) what combination of change in performance criteria and performance analysis would result in a change of risk mitigation measures? Lee also argues that expert opinions play a critical role in the PA results, that they may not have been encoded with sufficient care, and that they may not be appropriate given that experimental data could be reasonably obtained instead (e.g., for solubility). I tend to agree with Lee on this last point. Some parameters can be better estimated. The decision to gather more data depends on the "value of information" and on the difference that experimentation would make in the final decision. I believe, however, that the use of panels to assess what might happen in the distant future is unavoidable and appropriate in a probabilistic framework. Clearly, the results are subjective probabilities. There is nothing wrong with that if the encoding is well done: they are the only ones available for this kind of exercise.

At this point, the concern of the Office of the Attorney General of New Mexico is that the combination of the release criteria as set by EPA, the (conditional) performance analysis as done by Sandia for the DoE, and the compliance requirements proposed by EPA may not provide sufficient conservatism to ensure the long-term safety of the citizens of the state of New Mexico. The central issues are thus:

i. What was the level of conservatism used by EPA in its model linking health effects and release levels?

ii. What is the actual conservatism of Sandia's PA (conditional risk analysis) given the combination of EPA hypotheses and Sandia's choice of distribution models and parameter values for the input variables? In particular: What are the potential problems and possible effects on the PA results of the procedures that were used to obtain probability distributions for the input variables including the choice of distribution models and parameter values, based on expert opinions?

iii. If the proposed compliance criteria are adopted and the case is judged based on the combination of the existing analyses and these criteria, what would be the actual level of safety and with what level of confidence?

iv. Does the Office of the Attorney General of New Mexico want to require (or can require) that EPA issue fractile-based compliance criteria *on the release level* as a general numerical standard?

v. Does the Office of the Attorney General of New Mexico want to require (or can require) that EPA provide a full probabilistic version of its model of the link between radionucleide release and health effects so that one can perform a probabilistic risk analysis (complete with uncertainty analysis) of *the final health effects*?

[Since a large part of the problem relies on the treatment of uncertainties in risk analysis in general and in the Sandia study in particular, you will find in appendix of this report a discussion of this problem based on a report that I recently wrote for the Electric Power Research Institute].

Probabilities are understood in two different ways by different people. For classical statisticians, probability means frequency in very large samples. For the Bayesians, probability is a degree of belief and is updated in a systematic way given each new piece of information.

Uncertainties themselves are also of two different types. The first type is randomness in samples (or aleatory uncertainty to which you refer as stochastic). It can be treated by statistical methods and the frequentists' definition of probability. The second type reflects the limits of fundamental knowledge and can be called epistemic uncertainty (you refer to it as subjective). It cannot be addressed by the frequentist approach to probability. For this second case, one needs Bayesian probability and expert opinions, with the understanding that there are numerous problems associated (1) with the encoding and the validity of this type of information and (2) with the aggregation of expert opinions.

Because of the unavoidable subjectivity of Bayesian probability and expert opinions, some government agencies (such as the EPA) have used, since the late seventies, "plausible upper bounds" of the risks, for instance, for dose-response relationships for carcinogens. These plausible upper bounds are single numbers meant to provide conservative estimates based on an accumulation of worst-case assumptions. This approach, however, has led in the past to regulations that the present Congress found unacceptably costly.

Currently discussed (or recently voted) legislations such as HR 1022 require a "soft" cost-benefit analysis approach to regulation, and therefore, an estimate of central values and a description of uncertainties in addition to plausible upper bounds. EPA, as well as many other governement agencies, is in a state of transition in its approach to risk assessment as they are trying to adapt their methods to this new sensitivity to consistency in rule making. There are several problems with a full probabilistic quantification of health risks. First, the methods of risk analysis are not yet fully developed, especially in environmental and health risk assessments. This is true, for instance, for cancer risk assessment, for which there is no full probabilistic analysis method. This is why I think that it would be very difficult for EPA, at this time, to provide a probabilistic version of the model that they used to set the release criteria for WIPP, especially over 10,000 years. This model would be necessary for Sandia to do a full probabilistic risk analysis of health effects of radionucleide release at WIPP which would be dominated by cancer risks. Second, there is not enough consensus in the scientific community to base risk acceptance and degree of confidence on analytical results alone. Furthermore, the acceptability of a particular risk level depends on many more factors than its computed magnitude.

As I shall discuss further, the issue of aggregation of expert opinions is still unresolved, i.e., there is no consensus about how to do it. Therefore, the agencies (such as the EPA) tend to focus their initial efforts about uncertainty analysis on the development of methods for the quantification of *randomness* in parameter values. As far as fundamental (epistemic) uncertainties, they are not generally ready to incorporate them in a probabilistic risk analysis and still tend to base their risk assessment on specified hypotheses that are generally conservative. Therefore, for the moment, the results tend to be *conditional risk analyses* of the type performed by Sandia for WIPP and not the type of full PRA that is state-of-the-art in the nuclear power industry.

As a result, the curves that are produced in that way are difficult to interpret. This point is at the core of the problem that you have with the WIPP analysis. Yet, Sandia had no choice: the hypotheses had been set for them by the EPA in the generic studies that led to the release criteria. The methods are still in flux and criteria that one may want to adopt for complete probabilistic risk analysis in which *all* uncertainties have been quantified are not the same as criteria appropriate for mixed methods. In any case, showing that expert data have been gathered in a way that is as objective as possible, then properly aggregated is going to be both difficult and necessary.

2. QUESTIONS FROM THE OFFICE OF THE ATTORNEY GENERAL OF NEW MEXICO

It is against this background that I will address the five questions posed to me by the Office of the Attorney General of New Mexico.

Question 1:

How should expert judgment elicitations be conducted?

I interpret this question as: elicitation of "best estimates" and elicitation of probability distributions for either a spectrum of hypotheses or for the numerical value of an uncertain parameter.

Question 2 is made of two independent parts:

How should the judgments of multiple experts be combined?

How should the results (and the uncertainties) be incorporated in the regulatory agency's decision making? (I will answer this second question as part of question five).

Question 3: How should variable parameters be selected?

I interpret this question as: in which case should a parameter value be represented by a "best estimate" (i.e., some central value of the distribution to be determined), and when should the uncertainties about a parameter be represented by a full probability distribution?

Question 4: How should probability distribution functions be developed?

Question 5: When a family of risk curves has been generated (by propagating uncertainties about models and parameter values through the risk analysis model), how should compliance with the containment requirements be determined? i.e., what fractile or other characteristic of the CCDF family should be required to meet the criteria and with what level of confidence?

I shall address these questions *from the point of the view of the risk analyst.* I will leave to the Office of the Attorney General of New Mexico the task to translate the scientific answer into regulatory language, which is out of my domain of expertise. It is also important to note that some issues are objective (the discussion of the soundness and the practicality of a methodology), while others are purely subjective and reflect a desired degree of *prudence.* There is no scientific basis for the latter and one can only approach it from the point of view of consistency and practicality.

Q1. HOW SHOULD EXPERT JUDGMENT ELICITATION BE CONDUCTED?
I will separate the question into two parts: choice of the experts, and elicitation of individual expert opinions. Note that the encoding of a distribution for a single expert is fairly standard. The difficulty is in the aggregation of the opinions of several experts to obtain a composite distribution.

Q1.1 Choice of experts and definition of expertise:

The choice of the experts should be limited to people who have demonstrated scientific competence in the field, and have been recognized by a substantial fraction of the corresponding peer scientific group as part of the scientific expert community for this particular domain. The notion of expertise includes knowledge and understanding of the generally admited theories and of the available base of evidence, and capability to reason about the different hypotheses *given the evidence* (i.e., mastering the scientific method of reasoning about existing data). The demonstration of such competence may have been achieved in different ways: publication in the refereed literature, reasoned support of one or several hypotheses, and contribution to research, development, or practice in the field of interest.

In particular, the sole role of advocate, on political grounds alone, of one view or another is not sufficient to constitute expertise. A scientific understanding of the current evidence base and of the spectrum of possible hypotheses is an essential component of the definition of expertise. Obviously, this definition is not black and white. There is a spectrum of expertise levels based on experience and the ability of an individual to reason scientifically from the evidence base. Therefore, there remains an unavoidable subjective element in the definition of the degree of expertise.

Other issues, such as "no conflict of interest", seem to have been adequatly addressed by EPA and in this respect, EPA language (40 CFR 194.26) generally appears reasonable.

Q1.2 Elicitation of expert opinions for a single point estimate

Clarity test

First of all, questions to the experts must be phrased in such a way that a hypothetical individual who would know the variables with certainty could immediately answer with a single number. This requires that the input variable is clearly defined and that there is no ambiguity such that given perfect information, different values could be given in good faith (in the literature, this is called "the clarity test" Ref. Ron Howard).

Best estimate

Second, if the objective is to elicit a "best estimate", one must understand the thought process by which the expert is going to come up with this figure. Suppose that there are several possible models for this best estimate and several parameter values for each of these models. A simple way for the expert to find a best estimate is to take the most likely model, and for this model the maximum likelihood estimate of the parameter value(s). Note that if the expert does that, the result is unlikely to be equal (or even close) to the mean, and one cannot use this figure as such. Indeed in some cases, e.g., for a remote risk (low probability, high consequences), the most likely mechanism may well be "nothing happens", which does not require any further treatment of parameter values. This process may thus yield an "unconservative" answer which could be inappropriate because of the possibility of severe consequences.

Central values

Central values of the distribution are generally what one wants from the expert.

° The mode (the maximum of a probability density function) is not very helpful because it cannot be easily combined in a risk analysis with other variables (treated either deterministically or by a probability distribution). In other terms, one does not know what the results mean at the end of computations where distributions, means and modes have been mixed.

° The median is more helpful because the experts can think about it relatively easily (variable X is as likely to be larger as smaller than the revealed value). It is not easy, however, to include it in the analysis (i.e., to combine it logically with other variables) except for lognormal distributions of which it is a natural characteristic.

° The mean is the most robust of the central values. But for an expert to come up with a mean sometimes requires a more sophisticated thought process: what are the different possible underlying models for that variable, what is the spectrum of parameter values for each model, and given these, what is the mean that one gets after combining models and parameter values. For skewed distributions, the mean may be driven by extreme values and correspond to high fractiles; in that case, it may not be easy for the expert to assess it directly without analytical support.

Note that for variables for which there is little uncertainty, the mean, the mode, and the median are close enough and the distinction does not matter much.

O1.3 Elicitation of expert opinions for a distribution

Encoding the probabilities of fundamental hypotheses

Assume first that the issue is to assign a probability distribution to a spectrum of possible hypotheses. The first step is to structure them as a set of exhaustive, mutually exclusive possibilities. The second step is to get the expert to elicit a probability distribution for this structured set of hypotheses. At that stage, most experts require first some training and explanation about probabilities, and for what the figures actually mean. To do the actual encoding, one of the best tools is a "wheel of chance". The expert is asked to divide the wheel into "pie portions" whose relative angles represent the relative probabilities of the different hypotheses. Therefore, when the wheel is spinned, for any hypothetical "lottery", the expert is indifferent between playing the lottery with the wheel, and with the true nature of the phenomenon of interest (as if it were to be revealed). The result thus represents the expert's degree of belief in each hypothesis.

This method is adequate for relatively large probabilities. Very small ones must generally be either decomposed into a sequence of conditional variables whose probablities can be more easily assessed, or based on revealed models.

Encoding a distribution for a variable of the model

To encode the distribution for a model variable X, one generally needs: (1) to identify the appropriate distribution model for X (e.g., normal), and (2) given this model, to encode a distribution for the value of its parameter(s) (e.g., the mean and standard deviation of X). The probability distribution for a parameter value can be obtained in two ways: a non-parametric approach based on the wheel of chance described above (e.g., interval by interval), or by a specified probability distribution (e.g., Normal) for which the expert assesses secondary parameter values (e.g., mean and standard deviation for the mean of X).

It is clear that this process of embedded uncertainty analyses has to stop somewhere. A general rule is to stop when additional information is unlikely to influence the final choice given the decision criteria.

Q2. AGGREGATION OF EXPERT OPINIONS

How should the judgments of multiple experts be combined?

One of the greatest challenges of risk analysis is the treatment of expert opinions when they disagree. Note again, that there is no standard, widely accepted procedure to do it at this time. First, one must understand why the experts disagree. One can then proceed to obtain a family of risk curves that represent, for each value on the consequence axis, a composite distribution reflecting the spectrum of opinions.

Q2.1. Sources of disagreement

They can include semantic misunderstandings, differences in experience and evidence base, fundamentally different mental models to treat the evidence base, and disagreement about parameter values (Ref. Bonduelle). Note that, of course, some of the experts may also want to influence the decision to fit their own value system, and may for instance, choose to ignore part of the evidence in their assessment of probabilities.

° Semantic disagreement is often overlooked. Therefore, one should first check that the variables are precisely defined and understood in the same way by all the experts.

° Bases of evidence can differ entirely from expert to expert. First, different experts may have observed the same phenomenon but in different settings. In addition, someone who has seen only "real-world" data (e.g., epidemiological data) may have gathered information that differ significantly from laboratory results. This is why, in the processing of real-world data, all relevant confounding factors must be taken into account. For laboratory experiments, it is their adaptability to the case *in situ* that has to be questioned. In all instances, the experts should not be allowed to arbitrarily truncate the evidence base to fit their views of what should be done.

° Disagreements about models and parameter values are the most difficult to resolve. The first thing to do is to examine the relationship between the probability of the different models and the complete set of data and evidence (e.g., by Bayesian methods). The second is to decide what approach is required by the level of complexity and the importance of the variable in the final decision.

Q2.2 Different approaches to aggregation of expert opinions

There are three classical approaches to this problem.

° The iterative approach: for example, the Delphi technique, in which the experts are required to elicit independently their probabilistic opinions. These opinions are gathered and sent back to the experts who then have the opportunity to revise their assessments in the light of the colleagues'estimates. The process generally converges quickly, but perhaps towards the wrong figures and for the wrong reasons. One of the major problems is that the experts do not have the opportunity to argue about their models, to exchange their evidence bases, and to discuss the probability of each theory given the evidence.

° The analytical approach:

An example of this type of approach is the Bayesian treatment of the opinion of each expert by a "super expert" (presumably the decision maker). The super expert is supposed to compute the probability of different values conditional on the opinion of each expert treated as different pieces of evidence, with possible dependences (Ref. Morris, Winkler). The problem with this approach is the role of the "super expert" who acts as an aggregator, adding one more layer of subjectivity to the process. Besides, it is often politically difficult to attribute different likelihood functions to the opinions of different experts. A simplified version of this procedure is to simply weight the opinions of the different experts, often with equal weights as if they were independent. Unfortunately, in such a case, the result is a direct product of the choice of the group of experts, without a real chance for them to interact and debate the problem. Hence my preference for the third approach:

° Interactive procedures

In an interactive procedure, the experts meet (1) to share the evidence and discuss the existing data, (2) to explain their models and their reasonings of how they conceived the model given the data, (3) to structure the set of models so that they can begin to talk about the probability of each of them, (4) to assess (individually) the probabilities of the different models, and (5) to participate actively and directly in a debate leading to the generation of the composite distribution.

It is important to note that *there is currently no standard procedure for the aggregation of expert opinions*, and that this exercise will remain subjective in nature. I believe that the key to success (matching the evidence and the distributions, and respect of the internal consistency of the probabilistic logic) is to focus on the probabilities of the models and assumptions as opposed to weighting the experts. Having said that, I have to recognize that the two are frequently linked and that the problem often involves personalities and conflicts as well as a scientific issues.

One promising such procedure has been designed and implemented by the Seismic Hazard Assessment Committee (SHAC) chaired by Robert Budnitz. The work of this committee is now in the publication process. Basically, the committee asked the experts to play successive roles in the aggregation process (from proponent of their own model, to technical integrator of the spectrum of opinions). The result of this work is similar, in its form, to the family of risk curves that Sandia has obtained for potential release levels at WIPP. For a given site, the SHAC committee modeled first the different sources of seismic activity, then the propagation of energy from the source to the site. They obtained a family of risk curves representing a discretization of the frequency of exceedence of different peak ground acceleration levels at the choosen site (Ref. SHAC). Note that this analysis integrates uncertainties about both the source model and the attenuation model (as opposed to the use of conservative hypotheses). These curves are similar to the CCDFs generated by Sandia for WIPP.

How the results (and the uncertainties) should be incorporated in the regulator's decision making is discussed in details in my answer to the fifth question. This answer is based on the assuption that the aggregation of expert opinions will be done systematically for all fundamental assumptions, and that the resulting distributions will be integrated in the risk analysis. Otherwise (e.g., if the disagreement is simply represented by a set of consequence distribution, one per expert), I do not know how to recommend to a decision maker to systematically treat a collection of results, or the results of a conditional risk analysis based on unweighted assumptions. It becomes a matter of faith in the conservatism of the assumptions.

Q3. SELECTION OF INPUT VARIABLES THAT REQUIRE PROBABILISTIC TREATMENT: SENSITIVITY ANALYSIS

It is not necessary in many risk analysis problems to put a probability distribution on all variables. In the decision analysis cycle (Ref. Howard), the first step is to develop models by a deterministic analysis of the link between the consequences and the input variables. Second, a sensitivity analysis for each variable reveals whether or not the variation of an input value across the possible range can change by itself the final decision. Third, the probabilistic analysis is performed: for the variables that do not require full treatment of uncertainty, the mean value is encoded and included in the model. For the variables that do require a probability distribution, this distribution is encoded as described above. The uncertainties are then "propagated" through the analysis by different methods (closed-form solutions, relevant moments, logic/event trees, or simulation, for example, using Monte Carlo or Latin Hypercube sampling).

Q4. DEVELOPMENT OF PROBABILITY DISTRIBUTIONS

Incorporating *all* uncertainties is a risk analysis is indeed a challenge. Therefore, it is important to proceed first to the sensitivity analysis discussed above so as not to lose sight of the ultimate goal (to support a specific decision).

The development of probability distributions is currently a hot topic within the EPA and the environmental/health risk analysis community. [Note, however, that for many years, it has been done systematically for industrial facilities such as nuclear power plants]. Because of the controversial nature of the treatment of epistemic uncertainties by Bayesian probabilities, the solution is often to do only what I consider a partial uncertainty analysis, focusing on randomness in statistical samples and on distributions for the variables explicitly included in the model. The default solution is thus to focus on randomness and on *some* epistemic uncertainties.

There is seldom any attempt to quantify systematically the epistemic uncertainties (about partially known fundamental phenomena) because it requires quantifying explicitly the probabilities of alternative assumptions and, in order to do that, proceeding to an aggregation of expert opinions. For example, in a recent expert-based study of global climate change, Granger Morgan chose to simply present the range of results for each of the different experts without any attempt to come up with a composite distribution. I personally believe that one cannot escape this full uncertainty analysis (i.e., to include the probabilities of alternative hypotheses). Otherwise, the problem is exactly the one that you are facing with WIPP: how to judge of the degree of conservatism of a conditional risk analysis without looking at the conservatism of the hypotheses.

The structure of a full uncertainty analysis is thus the following:

1. Structuring of the different hypotheses into sets of alternative realizations so that probability distributions can be

attributed to these sets of assumptions.

2. Encoding and aggregation of expert probabilities for each set of assumptions.

3. For each fundamental hypothesis, identification of the subsequent models and parameter values (probabilistic treatment). Conditional risk analyses of the type performed by Sandia, but one for each possibility (e.g., each H1i in Figure 1) in a complete set of assumptions, including a measure of possible dependencies through conditional probabilities.

4. Propagation of all relevant uncertainties for each hypothesis (the results are the sets of risk curves shown in Figure 1 for each realization of a given hypothesis).

5. Summing of the results of the conditional analyses weighted by the probabilities of the fundamental underlying assumptions (one then obtains an overall set of risk curves like those presented at the bottom of Figure 1).

(Alternatively, the overall set of risk curves can be obtained directly through the use of a logic tree).

Again, there are different methods for the propagation of uncertainties through each model: closed-form solutions (which is sometimes possible, for example, to treat lognormal distributions and products of variables), computation of the relevant moments, use of logic (event) trees that layout all possible combinations of hypotheses, models, and parameter values, or full simulation (by various methods including Monte Carlo and Latin Hypercube sampling)

Q5. COMPLIANCE CRITERIA GIVEN A FAMILY OF RISK CURVES

How this full uncertainty analysis is used by the decision maker (DM) is a function of his or her own preferences (including risk attitude). Therefore, it is by nature subjective. The consistency of the process, however, can be treated somehow objectively.

For individual decisions, these preferences are represented by a utility function that allows representing risk aversion by putting higher weights (than linear functions would) on the possibility of higher losses. Note that by virtue of the axioms of rationality for individual decisions, it is the *mean future frequency* that is the relevant characteristic of the probability distribution for the future frequency of the potential loss levels (in the WIPP case: the release level as an intermediate descriptor, but more importantly, the helth effects).

This rationality paradigm does not apply to collective decisions, except if one assumes that one elected decision maker (administrator) has been given complete power to make these decisions according to his or her utility function (which, presumably, would have to be revealed if it were to be used in an analytical model). This is impractical because it does not fit our political process and because there are many attributes to each decision that would require some adaptation of any revealed preferences.

The administrator is not only concerned about the probability distribution of the levels of release and about the economic costs of release (for which mean future frequencies would theoretically suffice), but also about the health and safety of the most exposed individuals in the public. The choice of a threshold and the way one demonstrates that it has not been exceeded should reflect directly a concern for prudence. The mean may or may not do that depending on the fractile(s) that it represents in the family of risk curves, and the practicality of demonstrating by analytical means that the goal has been achieved.

I would like, at this point, to go back to what I wrote in my 1986 paper:

> " The next question is to ensure that the goals have been satisfied with "reasonable certainty". A common procedure is to use "conservative estimates" at every step which means to overestimate the probabilities of initiating events, failures, accidents, etc. The overestimation of the

final result, however, is impossible to assess. It is a wrong approach that may lead to absurd figures and quite possibly to suboptimal decisions, thus defeating the pupose of conservatism itself. This is why the analysis of uncertainties and their explicit treatment in the final decision are critical.

Once this analysis has been done, safety decisions must be made to ensure that with a high probability (e.g., 0.95) the plant is in compliance with a the maximum acceptable individual risk constraint and with the maximum allowable frequency of failure. There is no compelling theoretical reason to use one fractile or a mean value rather than another criterion. In a framework involving numerical safety goals, this certainty level must be specified by the U.S. NRC along with the safety goal"

The example that I was using was safety of nuclear reactors for which the time horizon is relatively short and the uncertainties can be approached systematically. Therefore, the Probabilistic Risk Analyses that are performed for these plants do not involve the types of uncertainties faced with WIPP. Hence the possibility of "reasonable certainty" (which the USNRC calls "reasonable assurance"). In the case of WIPP, part of the analysis (the EPA linkage of release and health effects) is non-probabilistic and presumably, based on conservative modeling. Therefore, given the time frame and the level of uncertainties (e.g., about the future of civilizations in the next 10,000 years), the chosen approach has been different: to start with a set of preliminary results and framing hypotheses, then do a conditional performance analysis based on a mixed method (probabilistic and pre-set health effects estimates). First, one cannot judge directly which fractile(s) the mean curves of the future release levels would actually represent if Sandia had included in the analysis (1) the presumably conservative hypotheses that EPA had specified (complete with alternative assumptions and their probabilities), and (2) the uncertainties attached to the hypotheses that they generated themselves. Second, one cannot derive from this analysis a probabilistic distribution for the health effects. The problem is that a full risk analysis of this type would be extremely difficult given the state of the art, and that the uncertainties over the next 10,000 years would be so large that the results may not be very informative.

In this highly uncertain, long-term case, I believe that the approach based on some fixed hypotheses, *then* on "reasonable expectations" for the conditional risk results is generally sensible provided (1) that the hypotheses are globally conservative (health effects given release as well as assumptions in the release computation) and (2) that the mean curves for the release of the different radionucleides generally correspond to high fractiles of the risk curve families (CCDFs). If that is the case, the combination of *hypotheses* and *means* may indeed provide the level of "reasonable assurance" that you wish and that is consistent with the USNRC requirements for much shorter life facilities. To check that the overall analysis is "globally conservative" you need to verify that the global model (Health Effects + Performance Assessment) yields conservative results and in particular that the hypothetical health risk results that would have come out of a fully integrated analysis meet the level of "reasonable assurance" that you want to see. This requires that the combination of the health effect model and the Sandia hypotheses provides a higher level of safety than the one demonstrated by the position of the PA mean curves in the PA alone.

Therefore, you may want to examine the effects of hypotheses on the position of the *current means* in the family of CCDFs (fractiles) for release accounting for the EPA/DoE hypotheses as shown in Figure 1. Of course, you do not want to ask Sandia to redo the whole uncertainty analysis, but to give you a feeling for the final degree of conservatism of the release results after this accumulation of assumptions. This involves listing the main hypotheses (both from EPA and from the Sandia PA) and assessing (even coarsely) their cumulative effects on the position of conditional (current) means in the CCDFs families. If it is the case that the EPA/DoE assumptions are generally *conservative*, it is likely that what are now mean curves in the current conditional performance analysis (Sandia's PA) would correspond to higher fractiles of distributions that would account probabilistically for all hypotheses (Figure 1, bottom left). If the set of assumptions turns out to be altogether *unconservative*, introducing alternative assumptions will tend to make the current means go down in the families of risk curves towards lower fractiles (Figure 1, bottom right).

When you receive this information about the probabilities and the effects of alternatives to the main hypotheses on the position of the mean curves, you want to examine whether the *final* levels of fractiles that would correspond to the *current means* meet the level of conservatism that you want. You may also want to go one step further and look closely at the health effects themselves and at the conservatism of the EPA model of cancer risk. I do not believe that at that stage it would be realistic to require EPA to proceed to a full probabilistic risk assessment (they do not have the methods as far as I know). Yet, you can argue that their "reasonable expectations" are reasonable only if their hypotheses and health effects model provide the additional level of safety that is consistent with the NRC language of "reasonable assurance". In other terms, first their current means for the release of the different radionucleides have to provide at least as much safety as the overall "expected value" of the release that one would from a probabilistic analysis of the hypotheses. Second, the EPA health effect model should provide an additional layer of safety that convinces you that you are indeed in the high fractiles of a hypothetical full risk analysis.

Should you push EPA to specify a fractile level applicable across the board to all cases? I don't believe so, simply because each problem has to be replaced in its context (uncertainties, existence of alternatives, economic and political context, etc.). I believe, however, that examining carefully the range of fractiles corresponding to the mean in the consequence distribution is a reasonable way to address the question of uncertainties. In the WIPP case, the choice of the mean conditional on a set of hypotheses was based on the long-term nature of the project, the fact that the computation of the mean is more robust than that of specified fractiles, and that the means (given the uncertainties) are likely to be among the high-fractiles anyway. And in any case, requiring the EPA to make a general statement about a "high level of confidence" in the final health effects analysis including all uncertainties would be helpful.

Regulatory language

I think that you can require that EPA be more rigorous in its implementation of the "reasonable expectation" language. They cannot just set hypotheses and models (as those leading to Figure 1, Appendix A of 40CFR191), frame the conditional risk analysis for the applicant, then claim without checking that the conditional means (even with infinite sampling size) resulting from this analysis necessarily support "reasonable expectation" of human safety. Whereas it may be unreasonable (and perhaps, even hazardous given how uncertain the results would be) to leave the choice of hypotheses and model framing to the applicant, it is not unreasonable to require that the effects of these hypotheses on the mean curves be assessed (i.e., simply to check how they displace the mean curve: up or down). In the WIPP case, I would focus on the hypotheses of the intrusion model (frequency, means and effects of drilling) which are the most likely to significantly affect the release results. I would also examine very closely the EPA health effect model.

I would want EPA to show that, in the end, the combination of "reasonable expectation" for the performance assessment and of the conservatism (if it is the case) of the health effect model that they have used to set the release criteria provides "reasonable assurance" of actual safety (i.e., for the ultimate health effects). Because EPA has done the health effects modeling, they are in a good position to show the conservatism of their own results and of the final health risks when these results are combined with those of the performance assessment.

Therefore, you want to require EPA:
(1) to fully reveal the models that they have used to come up with the release standards,
(2) to list all the major assumptions that they have made (those that are likely to affect the risk analysis results), (3) then, to ask the applicants to show that the combination of these models, hypotheses and their own performance analysis supports the requirement that the current conditional mean is indeed "above" the marginal (overall) mean, and that altogether, the assumptions are in fact "conservative".

By comparison, the uncertainties that result from the sampling are probably (1) cheap to reduce and (2) not very significant compared to effects of the basic hypotheses. Therefore, you may choose either to accept their 95% confidence language, or to require a third level of confidence in the analysis. I do not think that it will make much difference.

3. ADDITIONAL COMMENTS ON ISSUES RAISED IN YOUR LETTER

<u>3.1 Level of confidence in the fractiles (or mean) given the sampling size</u>

This issue is easy to resolve because it is cheap to require additional computer runs if you do not think that the level of confidence achieved is what you want. Of course, the tail of the distribution will not be often reached in the simulation by definition of high consequence/low probability modeling. You may want to press EPA to specify the confidence level in this process (third order treatment of uncertainty, i.e., one level further than what I describe as Level 5 in Figure 2 of the Appendix). But you have to realize that the results will be somehow artificial given the variety of the sources of uncertainties. So, I would not focus so much on the uncertainties due to sampling size because they are

probably "in the noise", as I would on the uncertainties about the fundamental hypotheses.

3.2 Encoding of expert opinions

I agree that you may want EPA to specify better their encoding procedures. Anyway, in the case of WIPP, you want to find out how Sandia exactly did it (especially for parameter values).

3.3 Use of the mean

I generally agree with EPA that the mean *does* convey "a sense of the whole ensemble of the CCDF's generated". It represents an aggregated description of the risk by a single probability distribution (Level 4 of Figure 2) without displaying the higher level of uncertainties (Level 5 in Figure 2). I do not believe, as you do, that the applicant can vary the number of realizations and dilute at will the effects of *any particular CCDF*. What is true, however, is that with a small number of realizations (in the simulation) one may not reach the tail of the distribution. You want Sandia to specify case by case what level of assurance the mean represents (it varies, of course, along the release axis).

3.4 Additional comments

a. Specific guidance for the form of probability distribution functions seems to me impractical.

b. Need to deal with correlations: I agree, this is essential.

c. Appropriateness of the mean: in the case of WIPP, I think that the coupling of EPA assumptions (*if* they are globally conservative) and mean release level (which is likely to be among the high fractiles given the uncertainties) should provide the level of safety that you want. This is what you want Sandia to demonstrate.

d. Calling explicitly for a 95% fractile with 99% confidence would require a full probabilistic treatment of all EPA/DoE hypotheses regarding the release, introducing still more uncertainties in the analysis and probably producing highly questionable results. [I would not suggest this kind of fractile *on top* of the EPA hypotheses.] Again, I would start by checking what the current mean represents (roughly) in the full picture. To call for the 95% fractile of the *real risk curves (i.e., the health effects)*, would require a whole new risk analysis including both the release model and the health effect model. It is obviously not the direction that was chosen *a priori*.

f. Of course, the process of sampling of 50 parameters, even with an infinite sampling size would dilute the effects of the extremes. It is the nature of probability: the extremes are much less likely than the central range of the distribution. But you want sufficient sample size to have confidence that you have given the extremes their proper weight.

h. Reducing uncertainties can be done in many different ways. Increasing the sampling size of course is one of them; but again, these uncertainties are probably minor compared with the uncertainties involved in the fundamental assumptions.

j. No, it is not easy to identify the various percentiles of crossed curves. Indeed, any mean curve will represent different percentiles in different release ranges.

4. CONCLUSIONS:

I believe that the case of WIPP as it stands now raises issues that are different from those that I addressed in my 1986 article regarding nuclear power plants. But the fundamental concern is the same: reaching an acceptable level of safety with reasonable certainty (or assurance). In the 1986 article, I proposed to do it using high fractiles of the risk curves (which is often where the means are anyway) based on full PRAs including the treatment of all identified and relevant uncertainties (as determined by sensitivity analysis). For WIPP, we do not have risk curves (in the sense of full probability distributions for the consequences,i.e., the health effects). Because of the 10,000-year time horizon, the uncertainties in the case of WIPP are such that this kind of analysis may be a futile exercise. Instead, EPA has chosen to make some assumptions in its performance criteria and to require a conditional performance analysis given these assumptions. Then, EPA specified the use of the conditional means as the basis for the compliance criteria.

In order to verify that the conditional means (conditional on specified health models and hypotheses) provide indeed "reasonable expectation" of safety once the effects of the hypotheses on expected values are carefully considered, you want to ask Sandia to provide additional information about what these conditional means really represent for future release and what they imply for human safety. In particular, you want to question assumptions regarding engineered barriers and the hypotheses that have been made to support the currently planned storage system. This is where you may be able to show that some of the assumptions are unconservative and that the real mean curves are below the conditional ones. Therefore, you may be able to conclude that the current analysis based on conditional means does not meet, on the whole, the "reasonable expectation" standard. I would not focus much on the effect of the sampling size (although it probably does not cost much) because increasing it may not provide large variations of the position of the mean in the overall CCDF family. The hypotheses about the frequency, the means and the effects of drilling are more likely to provide significant variations.

To summarize my conclusions:

4.1 I do not know where the current means stand in terms of fractiles on the distribution of release curves presented by Sandia.
=> You may consider asking Sandia to specify which fractiles are involved in the mean release curves that are presented in their final PA report (these fractiles will vary along the release axis; but Sandia may be able to bracket them).

4.2 I cannot judge the degree of conservatism of the Performance Assessment results because I do not know the effects of the EPA and DoE hypotheses on the release curves.
=> Ask Sandia to list the major hypotheses that have been taken for granted in their PA and to give you an idea (if not a full analysis) of the effects of these hypotheses on the results (i.e., the family of release curves). For example take the five or six most important assumptions of the PA (e.g., the Poisson model of human intrusions,

the diameter of the bore holes, the water flow model, the solubility factor of the main nucleides, etc). Ask Sandia to generate a set of reasonable alternatives to these hypothese and to show you that the mean curves that would be generated with proper probabilistic analysis of these alternative assumptions actually meet the criteria (and that they do not pull the means towards lower fractiles of the risk curve families).

4.3 The expert opinion procedures of encoding could be made more rigorous.
=> You may want to ask Sandia to identify the variables whose distributions are critical for the results (could make WIPP violate the performance criteria), to justify their decision to treat them through expert opinions (as opposed to experiments or measurements when feasible), to better justify their findings by describing exactly how they have encoded and aggregated expert opinions, or to redo the encoding and aggregation of these judgments if you conclude that some of the variables have nor been properly treated.

4.4 The uncertainties about WIPP are such that full probabilistic treatment of all assumptions is likely to introduce large additional uncertainties in the results if they were to be systematically treated through probabilities.
=> You may want to find out what is the level of release risk obtained given the combination of EPA and DoE assumptions and the results of the corresponding conditional risk analysis, judge whether it is reasonable, and if it is not, ask EPA to reveal how it is going to inject additional levels of prudence in its decision. Depending on how far the current means are (assuming full probabilistic treatment of hypotheses) from a reassuring (but not sacred) 95% fractile, you may want to ask for additional analysis or for a change of risk management strategy.

4.5 If you really want to estimate the long-term health risks associated with the possibility of release, you need a probabilistic version of the EPA health effect model and a true risk analysis involving both release and health effects. I doubt that this is feasible. But:

=> You may want to ask EPA to better justify what they have done to obtain Table 1 of 40CFR191 and DoE to show that the overall risk results (their model plus the PA) provide "reasonable assurance" of safety.

5. APPENDIX

[What follows on this topic is based on a report that I recently wrote for the Electric Power Research Institute].

Six levels of treatment of uncertainties in risk analysis:

The form under which one would like uncertainty analysis to be done depends in large part on the use that one intends to make of the results, i.e., what criteria will apply in the decision making. All decisions do not need full treatment of uncertainties. Different degrees of sophistication in the assessment of the risks can be envisioned depending on the management rule that one intends to apply. Six different levels in the treatment of uncertainty (see Figure 2) can be identified.

Level 0 simply involves the detection of a potential hazard without attempt to assess the risk in any way. It is sufficient, in theory, to support strict zero-risk policies, or to make risk management decisions when the costs are low.

Level 1 is the "worst-case" approach. It does not involve any notion of probability. It is based on the accumulation of worst-case assumptions and yields, in theory, the maximum loss level. In practice, however, whatever the worst-case scenario that has been constructed, it is often possible to imagine still more unlikely circumstances that could worsen the result. It is therefore necessary to truncate the loss distribution.

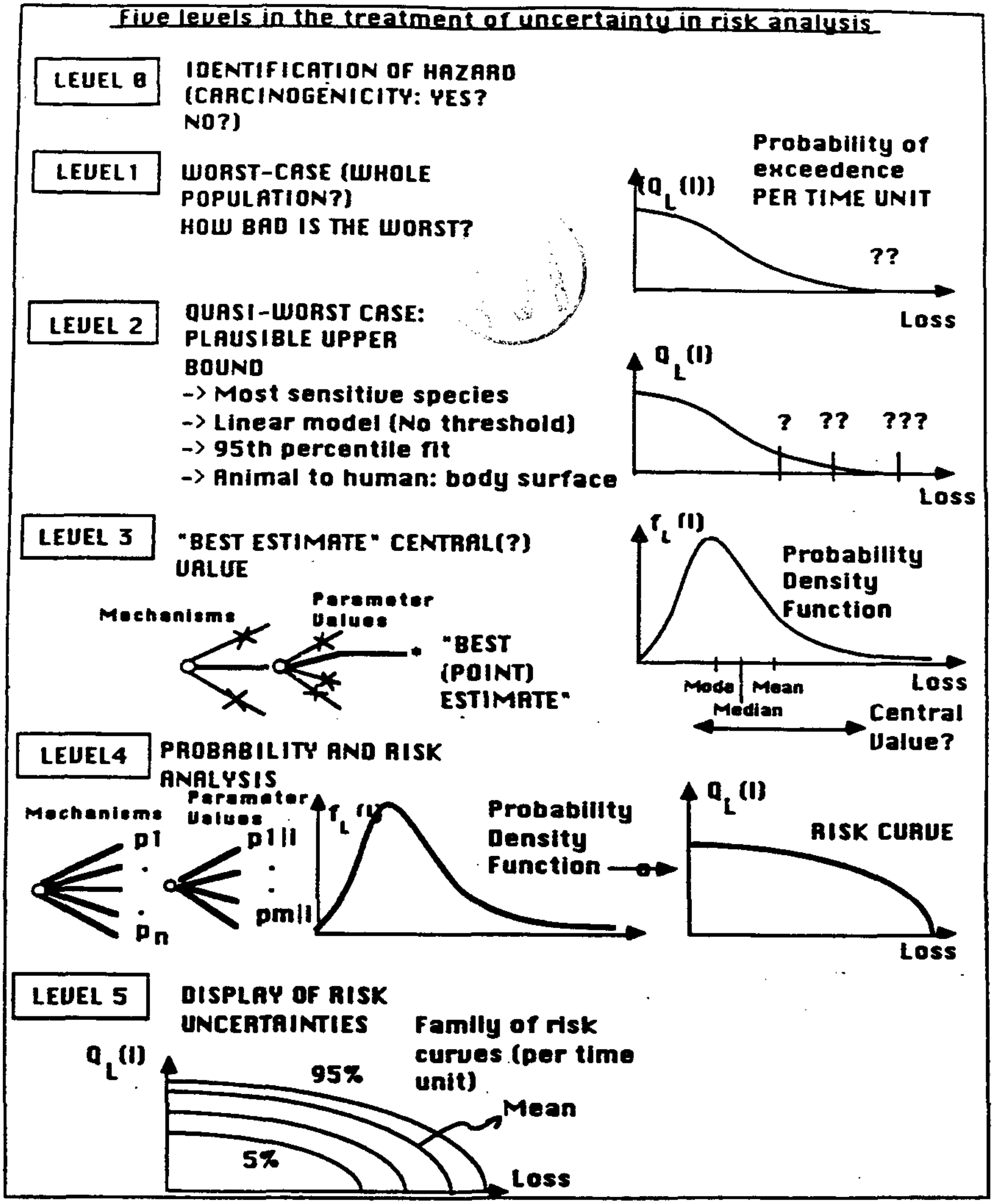

Figure 2: Six levels of treatment of uncertainties in risk analysis (Paté-Cornell, EPRI report, 1995).

Level 2 involves "plausible upper bounds" (or the "quasi-worst case"). This analysis represents an attempt to obtain an evaluation of the worst possible conditions that can be "reasonably" expected (1) when there is some uncertainty as to what the worst case might be, or (2) when the worst case is so unlikely that it is meaningless. Examples of these approaches include the Maximum Credible Earthquake or the Maximum Probable Flood used by the U.S. Army Corps of Engineers in the construction and management of dams.

This popular approach, however, presents major shortcomings. First, there is no way to judge the "conservatism" of these point estimates (the residual risk is unknown). Second, this approach does not allow a meaningful comparison of risks. Ranking among these presumably extreme values may not be related to the ranking of the mean values of the potential losses and there is no reason to believe that priorities set on the basis of plausible upper bounds will ensure maximum risk reduction for the money spent.
analysis.

Level 3 relies on "best estimates" and/or on a search for a central value (e.g., the mean, the median or the mode) of the loss distribution. Generally speaking, the advantage of central values is to provide a reasonable balance to plausible upper bounds. The disadvantage is that the risk is still characterized by a single point estimate and that the uncertainties disappear from the results.

Level 4 relies on probabilistic risk assessment (PRA), also known in engineering as quantitative risk assessment (QRA), or probabilistic safety assessment (PSA). It permits representation of a risk, not by a single point estimate, but by a complete distribution of the potential losses to represent the uncertainties involved. Still, the effects of all uncertainties being aggregated into one risk curve, it is impossible to extract from this information the dispersion due, for example, to expert disagreements about competing models for a fundamental hypothesis.

Level 5 allows display of uncertainties about fundamental mechanisms. This can be done in several ways. One approach is to ask each expert to provide an assessment of the risk based on their favorite model and on their evaluation of the distribution of parameter values, and to display this set of risk curves (one for each expert) without attempting to aggregate the results or to assess the probabilities of the fundamental assumptions on which they rely. The problem is that one popular hypothesis may be favored by a large proportion of experts for a combination of scientific and other reasons. Therefore, if a composite distribution is needed, one must sooner or later address squarely the issue of the relative probabilities of the different hypotheses and proceed to an aggregation of expert opinions. It is important at that stage to depoliticize the process if needed, and to put weights on models (given the evidence available) and not on the experts.

Therefore, in order to reach its logical conclusion, Level 5 requires a full probabilistic treatment of epistemic uncertainties. The result is a *family* of risk curves. These curves provide, for each value of the potential losses, a discretization of the probability distribution of the future frequency of exceedence of this loss value. Both aleatory and epistemic uncertainties are propagated through the analysis, for example, by Monte Carlo simulation or other simulation models such as the Latine Hypercube approach.

WIPP Performance assessment is a mixed case of some aspects of level 2 (plausible upper bounds; conservative hypotheses) and level 5 (full uncertainty analysis).

6. ANNOTATED BIBLIOGRAPHY

A. 40CFR191

1. Russo, September 1991: Updated Uncertainty Analysis of EPA River Mode Pathways Model Used for 40CFR Part 191: Table 1 of 1985 40CFR191 Analysis of Curie release corresponding to 90% level of certainty that effects will be less than or equal to 10 fatal

cancers/10,000 years was completed by an uncertainty analysis in which the probability distributions characterizing uncertainty about model input parameters were based on discussions held with radiological assessment experts taking into account theoretical considerations, variability in published data, and insightful judgment [How was this done?]

2. EPA 520/3-80-006. Population Risk

p. 150: "The expected frequency of human intrusion into a repository ranges from a drilling event every 400 years for granite to a drilling event every 50 years for salt and shale (ADL 79d)." [This is one of the assumptions whose effect on the results should be checked]

3. Federal Register 1985. 40CFR191.

191-13 Containment Requirement:

"Reasonable expectation" language. PA need not provide complete assurance...etc."

4. Response to comments; EPA 520/1-85-0242. p2-5: The median is insufficient. [I agree]. p.2-12: EPA states that the standards, as they are written, will allow demonstrating compliance in a way that will not be "unreasonably difficult or expensive". [Fine].

5. Report of the Review of 40CFR191 by a subcommittee of the SAB.1984. "The subcommittee supports the general form of the proposed standard, including the use of a social objective as an upper bound of acceptable health (cancer and genetic) effects. [The question is: how conservative is the societal risk target *given the assessment method*. Could be very conservative or not. I don't know].

6. Working draft of final 40CFR191: 11/1/83

191.16: Guidance for implementation

"determination of compliance should be based upon "best estimate" predictions (e.g., the mean of the appropriate distribution results)." [Best estimates is generally not a good term to use without specification in regulatory language because it is too vague].

7. Working Draft of final 40CFR191: 2/1/84

"Instead the implementing agency may determine compliance based upon the part of the range of predictions that falls within one standard deviation of the mean..." [That was an idea but it was not implemented].

8. Working Draft of final 40CFR191: 4/23/84
Mean + one standard deviation => 85% for Normal distribution [Many distributions are not normal but skewed right].

9. Working Draft of final 40CFR191: 3/21/85
191.13: Containment requirements Uncertainties are too large hiven the time frame. => reasonable expectation language [Intended to be: the mean; actually here: conditional means].
Further: compliance with 191.13: Integrate all uncertainties into one risk curve [i.e., the mean risk curve. This is the level four of Figure 1. It is generally sufficient to support the choices of the risk averse decision maker in rational individual decision making].

10. Working Draft of final 40CFR191: 6/15/85
191.13: same language about "reasonable expectation".

11. Working Draft of final 40CFR191: 7/5/85
Uncertainties, and long term => reasonable expectation

12. Report of Meeting with extra-agency personnel concerning EPA Docket Number R-82-3. (with NRC staff personnel). "Subparagraph 191.16a requires that the standards be implemented in terms of the upper 85% confidence level of the simulated cumulative release. In view of the very very large number of judgmental factors that will have entered into the calculation, the use of the specified confidence level as a basis for deciding compliance is highly susceptible to mischief during the licensing process[...]; in view of uncertainties involved, confidence levels must be adressedd in terms of qualitative (e.g., reasonable assurance terms) rather than quantitatively."
Further: "Confidence level: DoE is concerned that the mention of an 85% confidence level will become the required level for all analyses; this would be contrary to EPA's intention."
Further: Guidance for implementation:
Suggestion again that mean + one standard deviation becomes the standard (=>≈85% for Normal distribution).
Further: "Paragraph 191.16.c unclear and calls for a precision level that may not be possible to demonstrate analytically."

13. EPA background information behind 40CFR191 1985 EPA 520/1-85-023.
p.6-3 Problem of uncertainties in EPA modeling of radiation risk estimate (the risk per unit dose are likely to be low)
p 6-13 the risk estimates are not unduly conservative [Important to check and to assess the effect on individual safety].
14. EBASCO study: Uncertainty and sensitivity analysis for the exposure pathway models used in 40CFR191.
p. xiii: levels of certainty for each radionucleide in Table 1.tryin to evaluate the level of conservatisms [Again, effects on conservatism of Sandia's result?].
15. Background information: EPA 93. EPA cancer risks are based on NAS study. Further (p.6-5): dose-response function was based on japanese epidemiology after the Hiroshima bomb. Perhaps unconservative following subsequent studies (p.6-93). p.6-31: estimates of cancer risks are NOT conservative.
16. EPA environmental pathway model, 1986
p.S9: Releases to a river [could be very conservative: WIPP is in the desert; but assumption was based on large rivers; how about small ones?].
17. Analysis of Uncertainties. Envirosphere, June 10, 1983.(problem of the original ore body release and river mode exposure pathway). Another set of uncertainty assessments for Table 2 of 40CFR191.

B. 40CFR194

1. Federal Register
2. 40CFR194 Proposed Rules: Criteria for the Certification and determination of the Waste Isolation Plant's compliance.
p.81: Expert Judgment "should be used provided that it does not substiotute for data that could be obtained through data collection or implementation." [An apparently reasonable set of requirements. No conflict of interest. At least five experts. Not all from DoE].
p.113: results of performance assessment:Risk curves. Monte Carlo of Latin Hypercube. Not the median. Requirement that the number of risk curves be large enough so that the maximum CCDF generated exceeds the 99th percentile of the population of CCDFs with at least 0.95

probability. [Looks conservative. To be checked]

p.114: the criterion itself: "demonstrate that there is at least a 95% level of statistical confidence that the mean of the population of CCDFs meets the requirements of section 13(a) of 40CFR19" [The mean is the most robust measure under the circumstances (smaller sample size required) and it may already be in the 80 to 95% fractile].

3. Background information: EPA 402-R-95002

p.3-7: Disposal systems shall be designed to provide reasonable expectation based upon performance assessments that cumulative releases of radionucleides to the accessible environment for 10,000 years after disposal from all significant processes and events that may affect the disposal system shall:

(1) Have a likelihood of less than one chance in ten of exceeding the quantitties calculated according to table 1 (Appendix A) and

(2) Have a likelihood of less than one chance in 1,000 of exceeding ten times the quantities calculated according to Table 1. Table 1 defines a set of permissble releases ("normalized release" for each isotope). [The question is; what were all the hypotheses underlying Table 1].

4. Compliance criteria: March 21, 1995

p.55: results of performance assessments.

5. EEG Comments. April 28, 1995.

p.5: the WIPP site does not meet the there stated criteria of 40 CFR 191.149 (because it is in a resource rich area)=> unconservative assumption. On the other hand, (p.6)EPA claims that the hypotehse are favorable because of the favorable characteristics of the WIPP (located in the desert). [net result??]

p.11: Engineered barriers:

Argument for engineered barriers: unconservative assumptions regarding human intrusion in a resource rich environment. Also: benefits will be small because it would only delay the arrival of actinides in the environment.

1992 PERFORMANCE ASSESSMENT REPORT

COMMENTS AND RESPONSES

EPA/EEG/NMAG/NMED

DOE/WIPP-95-2053

Responses to Comments on the 1992 Preliminary Performance Assessment Report for the Waste Isolation Pilot Plant

June 1995

United States Department of Energy
Waste Isolation Pilot Plant

Carlsbad Area Office
Carlsbad, New Mexico

Table of Contents

Introduction

This document contains the Department of Energy's (DOE) responses to comments made by the Environmental Protection Agency (EPA), the Environmental Evaluation Group (EEG), the New Mexico Environment Department (NMED), and the New Mexico Attorney General (NMAG) on the "Preliminary Performance Assessment for the Waste Isolation Pilot Plant, December 1992", Volumes 1-3. Each comment and response is listed along with the reference materials. The appendixes contain supportive figures and memos referenced in the document. The subject document is referenced as the 1992 PA (Performance Assessment).

Comment CL1

Cover Letter, Format and Content

"We reviewed this document with the idea that it should contain all of the information needed to allow us to review, understand and evaluate DOE's approaches, and to demonstrate that the approaches were sufficiently justified to support a request for certification of compliance."

Response

The DOE appreciates the perspective taken by the EPA for this review since it provides the DOE with significant insight regarding what should be included in a compliance application. In addition, the EPA's perspective will help identify areas where the two agencies may disagree regarding implementation. It is important to seek resolution to these prior to the preparation of a final application.

The DOE will use EPA's comments and suggestions as a guide on formulating aspects of its compliance program. In addition, the DOE will establish, as a priority, the resolution of any issues or disagreements that have resulted from these comments.

Comment CL2

M

Cover Letter, Format and Content

"Although the PA provides a large amount of information, it lacks a sufficient description of the analyses that are discussed. The current PA is not a "stand alone" document that uses references as supporting information. Rather, references are often provided as the proof of the validity of DOE's reasoning, with insufficient information presented in the PA to enable the reader to follow that reasoning. ... While we understand that references and accompanying documents will be needed, we feel that the PA (in all its volumes) needs to tell a complete story. In our view, the PA should start with the basic information and, step by step, build up to a demonstration of compliance."

Response

The DOE agrees that the PA is not a "stand alone" document for compliance purposes. In fact, PA is only a tool used to determine compliance with quantitative limits and to understand uncertainty. Numerous other topics are to be included in the compliance application as indicated by the Format and Content Guide issued in May 1994.

This not withstanding, the DOE appreciates the broad perspective used by the EPA in its review as discussed above. The DOE will use this broad review as a guide in preparing the final application.

Comment CL3

Cover Letter, Format and Content

"For example, it would be helpful if the PA presented a listing of scenarios considered, and showed the analysis of probability and consequence for each separate scenario. The presentation of separate analyses would help clarify how scenarios are combined to create a final set of CCDFs."

Response

This material will be included in the compliance application as appropriate. The DOE notes, however, that separate analysis of both probability and consequences may not be appropriate for all scenarios considered. Some scenarios may be shown to be of sufficiently low probability that consequence analyses are unnecessary. Other scenarios may be shown to be of sufficiently low consequence that probability analyses are not required. In other cases, different scenarios may have sufficiently similar consequences that results of a single consequence analysis may be used in conjunction with different probabilities.

Comment CL4

Cover Letter, Format and Content Guide

The outline in the November 1993 Format and Content Guide prepared by the DOE is a significant improvement over the current PA organization.

Response

Comment noted.

Comment CL5

Cover Letter, Access to Information

"We are concerned that much of the information referenced or used as integral part of the PA analyses is not available for review. The current PA cannot be thoroughly reviewed because the supporting information is not accessible. This includes some references, computer codes, their documentation, laboratory and field data, and the data generated from computer runs, such as the results of the Latin Hypercube Sampling that is used as input for the computer runs. The computer codes and their documentation should be available for public review."

Response

The DOE is currently pursuing access to computer codes and training in their use with the EPA and stakeholders. Documentation of codes is being completed, and will be complete before PA analyses are used in a compliance application.

All source material cited in the 1992 PA, including primary documentation for laboratory and field data, is on file at the WMT library at Sandia National Laboratories. For a compliance application, reference materials will be dealt with in a significantly different manner in order to assure timely access to information by the EPA.

Comment CL6

Cover Letter, Access to Information

"In addition, if they are incompatible with EPA's computer system, they should be remotely accessible on DOE's computers to allow independent examination. I recognize that the codes and their documentation are not in final form; however, our review of the codes will take a long time, so we need the copies used to prepare a performance assessment along with the results of that assessment. If we do not get the codes until the final application, it will significantly slow our review of the application."

Response

The DOE is as concerned as the EPA over the length of time that EPA may require to review DOE's codes. Consequently, the DOE has made code availability a priority. The EPA and the DOE are successfully resolving this issue of access and training.

Computational efficiency is a complex topic and would be a welcome topic for the DOE/EPA technical exchange meetings. Specific recommendations concerning resource allocation, e.g., model development versus collection of experimental data, are to be an integral part of the systems prioritization methodology (SPM) effort. The SPM will have "outside scrutiny" inherent in the process design.

Comment CL7

Cover Letter, Repromulgation of 40 CFR 191

(a) "We realize that 40 CFR 191 had not been repromulgated when the analyses for this version of the PA were being conducted, thus consistency with the rule was impossible. However, the rule is now final, and changes in the PA will need to be made in order to reflect the new 40 CFR 191. The main areas where changes are necessary are in the ground-water and individual protection requirements, and the use of the committed effective dose. The definitions relating to ground water have been changed to reflect EPA's policy of protecting underground sources of drinking water. DOE will need to identify the potential aquifers and their water quality (i.e., total dissolved solids)."

(b) "In addition, the undisturbed performance time frame calculations need to reflect the 10,000 year requirement. With the increase of the time frame from 1,000 years to 10,000 years, DOE may need to include scenarios that were previously omitted."

Response

All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated into compliance documentation.

(a) The DOE has taken the position that a decision on when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application) will be based on the expectation of releases. Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

(b) The DOE agrees that calculations for a compliance application must be performed for a 10,000 year compliance period as stated in 40 CFR 191.

Comment CL8

Cover Letter, Regulatory Issues

"The PA also needs to address both 191 and RCRA compliance."

Response

The DOE has implemented a PA program which examines two-phase flow. This allows the determination of releases via both liquid and gas pathways. For the compliance determination, appropriate transport calculations will be made using these PA models. These calculations will include contaminant transport of interest to each of the regulations.

Comment CL9

Cover Letter, Regulatory Issues (Guidance to 40 CFR 191)

"The Guidance for 40 CFR 191 is generic in its application and it is non-binding to the implementing agency. EPA is evaluating the Guidance's suitability for use at the WIPP site. However, deviations from the Guidance should be clearly explained. The compliance criteria (40 CFR 194) will provide further clarification on this topic."

Response

The DOE agrees that any deviation from the guidance in 40 CFR 191 must be carefully and clearly documented. The DOE realizes that EPA's guidance, while non-binding, is not arbitrary and is provided to assist the implementing agency in meeting EPA's overall goal of protecting human health and the environment. Since EPA's guidance is established as the result of the technical bases developed during rulemaking, the DOE realizes that any significant deviation from this guidance must have equally in-depth technical justification.

Comment CL10

Cover Letter, Regulatory Issues

"The future applicability of the Guidance notwithstanding, DOE did not correctly follow the Guidance in this PA. If DOE was going to follow the Guidance, the PA should have used a constant drilling rate of 30 Boreholes/km^2 per 10,000 years for comparison with the containment requirement. In addition, the PA incorporates credit for passive institutional controls without proper justification."

Response

The maximum rate for human intrusion considered by DOE yielded an expected number of boreholes equal to 30 per square kilometer. Smaller rate constants were also considered to allow the evaluation of the sensitivity of disposal-system performance to uncertainty about future drilling rates. Determining such sensitivities is important to capturing the most significant parameters for the compliance calculations. These sensitivity analyses indicated that system performance is highly sensitive to the drilling rate and that it may be a very important factor in the compliance decision.

The DOE agrees that any assumptions made regarding the guidance need adequate justification as discussed previously. This applies to the credit taken for passive controls, drilling rates, and other factors.

Comment CL11

Cover Letter, Use of Expert Panels and Peer Review

"There should be documentation of the process used to obtain expert opinion, and the process should follow written procedures."

Response

A formal QA procedure for the use of expert judgment by Sandia National Laboratories was published in 1992 (Rechard et al., 1992). Further refinements of this QA procedure can and have been incorporated into WIPP Procedure No. PAP06, Use of Expert Judgment Panel Quality Assurance Procedures. Compliance documentation will contain documentation in accordance with this or any other formal procedure used by the DOE and its contractors to elicit expert judgment.

Comment CL12

Cover Letter, Use of Expert Panels and Peer Review

"Specifically, we do not agree with the approach taken by DOE to estimate a reduction of the drilling rate from speculations on the use of markers. The markers aren't yet designed; therefore, the panel was asked to provide advise about the effectiveness of the markers at WIPP on the basis of incomplete information. Nor did the panel include all the necessary expertise, e.g., no petroleum engineers or drilling experts were included on the panel. The information from the marker panel was then apparently provided as input to a computer program that produces results in an unclear manner. EPA's compliance criteria will contain additional guidance on the use of expert panels."

Response

This issue has been raised numerous times by the EEG and the DOE has been sensitized to assuring proper care is taken in both establishing the scope of an expert panel and in using the outcome.

With regard to the specific comment on the process used by the DOE, the following is provided:

The process used by the Markers Panel was to first develop design guidelines for long-term communicative markers based on the contributions from individuals in disparate related fields such as materials science, archaeology, and communications. Based on the design guidelines, the two teams comprising the Markers Panel each developed a conceptual design for a system of markers. Estimates of efficacy of the markers system over time were based on the conceptual design. Implicit in the deliberations was the assumption that sufficient testing was undertaken to determine, for example, the appropriate design of the foundation for stone markers to withstand possible fluctuations in surface level and still remain stable. A second assumption in the effort was to evaluate what was possible for a marker system (as a first approximation) with no cost constraints. Cost constraints may come into play regarding the definition of "practicable" in 40 CFR 191. There is much evidence from the fields related to marker design that suggest avenues to pursue to improve long-term survivability and communication.

A petroleum engineer was not included on the Markers Panel, because the thrust was geared to long-term survivability of a marker system and continued interpretability. A petroleum engineer's skills are not such as to contribute to this effort.

M

Comment CL13

Cover Letter, Use of Expert Panels and Peer Review

"In future performance assessments and interim documents, it would be helpful for DOE to identify: 1) the areas where no data exist; 2) where expert panels and expert judgement will be used in lieu of data; and 3) whether the expert judgement will be replaced with data by the time of the final application. We strongly recommend that DOE use data where it is possible to obtain it, instead of relying on expert judgement."

Response

The DOE agrees that the use of expert panels in lieu of data must be carefully documented and justified. The DOE, however, does not agree with the concluding statement in this comment since it is written so broadly. Instead, a level of "practicability" must be applied when designing tests in lieu of expert judgment. This is particularly true when tests may require unrealistically long time frames or represent unreasonable costs.

Comment CL14

Cover letter, Models

"The development and implementation of conceptual, computational, and computer models is one of the most important technical aspects of the performance assessment topics. Therefore, it is imperative that we and the public have a good understanding about the modeling process and the models themselves. In the current PA, a good discussion of conceptual models and their alternatives are provided in only a few instances, such as the porosity model for the Culebra Dolomite. The conceptual models for the potash mining scenario are absent."

Response

The DOE agrees that the development, documentation, and implementation of conceptual, computational, and computer models is critical to a defensible performance assessment. Documentation of models is facilitated by Sandia's formal software Quality Assurance Procedures. The mandatory guidelines and requirements contained in these procedures ensure traceability and verification of computational and computer models, as well as documentation of the underlying conceptual models.

The evaluation of computational-model uncertainty involves evaluation of various conceptual models against relevant repository performance metrics. Current WIPP PA accomplishes this in two ways:

1. An "all-other-things-being-equal" method, in which alternative conceptual models for one component of the system are individually evaluated over the LHS sampling of imprecisely known parameters, while maintaining the variability of parameters not included in the tested sub-model constant.

2. Inclusion of the alternative conceptual models within the sampling of imprecisely known parameters.

Examples of method (1) are inclusion of: a) multiple transport and flow models of the Culebra dolomite (single-porosity vs. dual-porosity vs. fracture-flow only; chemical retardation vs. no chemical retardation); and b) multiple repository and Salado long-term responses (with gas generation vs. without gas generation; with room consolidation vs. without room consolidation; with a representation of fracturing in the Salado vs. with no Salado fracturing).

The example of method (2) to date is inclusion of different two-phase-characteristic curves, e.g. the Brooks/Corey and Van Genuchten/Parker sub-models, and sampling on each.

The DOE believes it is important to evaluate the defensibility of all conceptual models continually, both by examining their supporting experimental data and evidence and by examining whether or not the different models have significantly different impacts on expected performance. Ultimately, the defensibility of a performance assessment depends on the belief of the regulators and major stakeholders that a reasonable conceptual model has been used, and that there is sufficient evidence to support its use. In order to make this evaluation, this history of the development and screening of alternative conceptual models used in the performance assessment must be thoroughly documented.

Comment CL15

Cover Letter, Models

"The next performance assessment iteration should contain a detailed description of all conceptual models chosen and the alternative conceptual models that are or have been under consideration. For those conceptual models no longer under consideration, DOE should justify why they were discarded."

Response

During the development of compliance documentation, a conceptual model screening process that has the goal of examining all conceptual models put forward by Sandia, WID, DOE, and stakeholders will be used to arrive at a prediction of reasonably expected system performance. The screening process will include the reasoning by which the model is accepted or rejected for use in the performance assessment. The compliance documentation will provide the full description of the screening process and its application.

Comment CL16

Cover Letter, Models

"Before DOE submits an application for certification of compliance, there should be general agreement between EPA and DOE on the conceptual models that will be used by DOE."

Response

The DOE agrees that ongoing dialog regarding conceptual models will be very useful.

Comment CL17

Cover Letter, Models

"The development of the computer codes will take time, especially since many of the computer codes are 'state of the art.' Because of the sophistication of the modeling, the peer review and quality assurance of the code will also take time--the more complex the code, the more time it is likely to take. They will also take more time to review. We recommend that DOE takes the necessary time to ensure that the peer review and quality assurance is implemented in a thorough manner, especially where there is uncertainty in the conceptual models used in the codes."

Response

The DOE agrees and has instituted a thorough review of the quality of the codes and data used to implement conceptual models.

Comment CL18

Cover Letter, Quality Assurance

"...the PA does not seem to address data quality objectives or other related issues."

Response

This was not a rigorous objective of the 92 PA. The DOE has recently initiated a quality verification activity to assure data and code quality for compliance determinations.

Comment CL19

Cover Letter, Quality Assurance

(Restatement) EPA is concerned about the implementation of QA for the "old data", such as site characterization or completed laboratory studies.

Response

In 1993, the DOE began an extensive review of early and completed work. The assessment is still in progress. Deficiencies identified in the assessment shall be documented and appropriate corrective action taken. Before the final compliance application is brought to EPA, data, analyses, and resulting conclusions shall be screened against appropriate QA requirements.

Comment T001

Page 1: I. Technical Comments, A. General, Access to Information

Area in document General

"EPA should have access to source codes, code documentation, on-line help files and the executable image. It is recognized that at an early stage of development, a code is a working draft and should not be subjected to a critical outside review. However, if the computer code, references, or other information is adequate for use in the PA, then it is appropriate to have it accessible to EPA and to other interested parties."

Response

The DOE is currently pursuing access to computer codes and training in their use with the EPA and stakeholders. Documentation of codes is being completed, and will be complete before PA analyses are used in a compliance application.

Comment T002

Page 1: I. Technical Comments, A. General, Resource Allocation

Area in document General

"Decisions regarding resource allocation (e.g., model development versus collection of experimental data) should also be subjected to outside scrutiny. In the case of computer resources, DOE should have its computer codes reviewed for their computational efficiency, because of the potential for the algorithms themselves to be unnecessarily resource limiting to the PA effort."

Response

EPA's comment is noted and will be considered by the DOE.

Comment T003

Page 1: I. Technical Comments, A. General, Limited Resources

Area in document Volume 2, Chapter 7, page 7-5, Line 17

"Direct solution of 'fully coupled equation' is said to be unrealistic using present resources. Why is this true? What resources staff, money, or computer capacity would be required? Has DOE tried to use more efficient algorithms and computer program applications? "

Response

The basis for this statement is discussed in Butcher and Mendenhall (1993, page 7-3 middle paragraph). An example of typical computer capacity requirements is given in the same reference, page 6-5, third paragraph. In regard to algorithms and computer applications, the codes used for these analyses have evolved over the past 30 years and represent the most advanced state-of-the-art technology.

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T004

Page 2: I. Technical Comments, A. General, Room Modeling

Area in document General

(a) "Has DOE developed a field theory for two-phase flow in a deformable porous media with fractures that can undergo large deformation?"

(b) "Can DOE provide justification for separating (over the various time-and-space scales) the two-phase flow, mechanical rock response, and gas generation models?"

Response

(a) While a single field theory for two-phase flow in a deformable porous media with fractures that can undergo large deformation may be theoretically feasible, it is considered to be technically infeasible because it would be too unwieldy to use in the global context of PA (also see response to previous comment).

(b) The justification for separation is discussed in Chapter 7 of Butcher and Mendenhall (1993).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T005

Page 2: I. Technical Comments, A. General, Poor Referencing of Information: Shaft Consolidation Example

Area in document Volume 3, Chapter #, Page 3-35, Line 7, A137, A140

It is stated that backfill in the lower is parts of the shafts will become consolidated due to salt creep, with a final permeability comparable to that of the host rock of the Salado formation. However, no calculations or modeling results are presented in the PA report to justify this assumption. The 1992 PA cites the 1991 PA, which in turn cites two other reports without discussion how the values were derived. This is but one case out of many in which the reviewer must peruse a succession of documents to find the source of cited data.

Response

The Project is currently investigating the permeability likely to be achieved by the crushed salt components placed in the shafts. The most current published information is summarized in Van Sambeek et al. (1993). Current technical efforts are focused on evaluating the effects of backstress, placement technique, and parameter variability on our ability to achieve an acceptably low permeability in each of the shafts; effective emplacement of the crushed salt components is an important part of the proposed Large-Scale Seal Tests Program.

Reference

Van Sambeek, L.L., D.D. Luo, M.S. Lin, W. Ostrowski, and D. Oyenuga. 1993. *Seal Design Alternatives Study*. SAND92-7340. Albuquerque, NM: Sandia National Laboratories.

Comment T006

Page 2-3: I. Technical Comments, A. General, Radionuclide Inventories

Area in document General, Volume 3

"The source of the radionuclide inventories is the memo from Andrew Peterson, which appears on page A-135 of volume 3. The inventories for the various generator sites are inconsistent: some include the short-lived daughter products of longer-lived parents, while others do not. For example, Y-90 is in secular equilibrium with Sr-90 in the CH waste at Hanford, while it is absent at INEL. ... Furthermore, INEL list[s] different activities of the two nuclides in its RH wastes. The Peterson memo sums the reported activities, showing significantly different totals for the two nuclides. Of greater import, Hanford lists a large CH waste inventory of PU-241, but nothing for its daughter product, Am-241. In fact, ten years after it is generated (for example), each curie of Pu-241 will be in equilibrium with 12.6 mCi of Am-241."

"Steps should be taken to insure that all generator sites use a consistent methodology for estimating their inventories. Absent such a practice, Sandia should obtain enough information to enable it to evaluate the data and make the necessary corrections."

Response

The Project is evaluating the sensitivity of compliance to this issue. Additional detail will be included in Project Technical Baseline report.

The radionuclide inventory used in the 1992 PA was a hypothetical "design" inventory based on the numbers given in the Peterson memo; inconsistencies in the way generator sites quoted the inventories were ignored in forming the design inventory.

The DOE has recently published the Baseline Inventory Report (BIR) which is aimed at achieving the consistency goal referred to in this comment. The BIR focuses on those parameters believed to be the most important to performance assessment.

Comment T007

Page 3: I. Technical Comments, A. General, Inventory and Release Limits

Area in document Volume 3, Chapter 3, Page 3-35, Line 7; A-137,140

"Why are the 1991 release limits presented instead of the 1992 limits used? The inventory in 1991 was 11.87 million Ci of waste. The 1992 PA inventory is listed as 4.227 million Ci (in tables on pages A-137 and A-140). This is more than "slightly different," than the 1991 release limits as stated in volume 3 of the PA, and it effects the release limit. What is the reason for this discrepancy? What numbers were used for the analyses?"

Response

Possible reasons for this apparent discrepancy (i.e., editorial error, error in interpretation, etc.) are being investigated. The release limits used in the 1992 PA analyses were based on the 1992 PA inventory. Future compliance documents will base inventory information on the BIR which combines information from numerous sources.

Comment T008

Page 3: I. Technical Comments, A. General, Inventory and Release Limits

Area in document Volume 3, Chapter 3, Page 3-35, Line 7; A-137, A-140

"What is the estimate of uncertainty in the waste inventory and the estimate of the release limits? What is being done to decrease this uncertainty? Will bounding values be used?"

Response

Uncertainties in all waste characteristics (e.g., the composition of the waste as well as its radionuclide inventory) are presently unknown. The radionuclide inventory used in the 1992 PA was a fixed, hypothetical "design" inventory based on estimates given by Peterson (see response to preceding Comment T006); uncertainty was arbitrarily added to certain waste characteristics also estimated by Peterson (volumes of cellulosics and corrodible metallics) in order to test the sensitivity of performance measures to variations in these characteristics.

Comment T009

Page 3: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-39, Line 9

"EPA strongly agrees with the State of New Mexico that distribution coefficients (K_d's) be based on 'experimentally justified data' and not based solely on expert panel judgment."

Response

The DOE has planned an experimental program to provide these data if needed.

Comment T010

Page 3: I. Technical Comments, A. General, Uncertainty

Area in document Volume 1, Chapter 3, Page 3-13, Line 44

"Please provide a detailed explanation of all methods used to reduce uncertainty and methods used to evaluate uncertainty."

Response

The line referred to in the text references Table 3-1 on the following pages of Volume 1. This table contains approximately 48 references and internal cross-references to examples of techniques used to assess or reduce uncertainty. The reviewer is referred to the table for more detail than the text provides and to the cited documents for additional detail. The DOE will include the information in these references in the final compliance application to a level deemed appropriate.

Comment T011

Page 3: I. Technical Comments, A. General, Uncertainty

Area in document Volume 2, Chapter 3, Page 3-22, Line 1

"Please provide more discussion on how uncertainty is 'propagated through a model'."

Response

The referenced line of text refers to the propagation of a sample through a model. That step of the analysis is briefly explained in the previous section on the previous page, and is explained in detail in later chapters of Volume 2 and in Volumes 4 and 5. Propagation of the sample through the model simply refers to the calculation of consequences for each Latin hypercube sample, using each of the consequence models in the system.

As discussed in general terms in Section 3.5 of Volume 2, and in extensive detail in the references cited therein, the WIPP PA has selected a Monte Carlo methodology to allow estimation of the uncertainty in model outcomes that results from uncertainty in input parameters.

Comment T012

Page 3: I. Technical Comments, A. General, Grout Seal in MB-139

Area in document Volume 1, Chapter 4, Page 4-4 (Figure 4-2a)

How does the "Grout Seal" get into Marker Bed 139?

Response

Effective placement of grout into Marker Bed 139 has been part of the technology development activities performed at the WIPP facility. Specifically, as indicated in the test plan for the Small-Scale Seal Performance Test-Series F (Ahrens, 1992), this underground test at the WIPP was "intended to demonstrate equipment and techniques for producing, injecting, and evaluating microfine cementitious grout." The grouting was completed in March, 1993, and the final report is currently being prepared.

Reference

Ahrens, Ernst H. 1992. *Test Plan - Sealing of the Disturbed Rock Zone (DRZ), Including Marker Bed 139 (MB139) and the Overlying Halite, Below the Repository Horizon, at the Waste Isolation Pilot Plant - Small-Scale Seal Performance Test - Series F.* Albuquerque, NM: Sandia National Laboratories.

Comment T013

Page 4: I. Technical Comments, A. General, Grout Seal in MB-139

Area in document Volume 1, Chapter 4, Page 4-4 (Figure 4-2a)

How will the seal location be selected?

Response

Tentative locations for the seal components have been identified in the reference seal design report (Nowak et al., 1990) and the logic for the locations is identified. In general, locations were selected on the basis of the sealing strategy (combination of long- and short-term components with some desirable redundancy) and needed function (e.g., limit water flow into the shaft) of a particular component. Locations have been slightly modified in a recent update of the reference seal design; documentation of the updated design is in progress. Additional information related to the intended seal locations will be included in design reports on the various components that will be primary references for compliance documents. At the time of actual placement of the seals, it is likely that location-specific factors such as degree of fracturing or observed water inflow will influence the final placement.

Reference

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant.* SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Comment T014

Page 4: I. Technical Comments, A. General, Bell Canyon Formation Characteristics

Area of document Volume 2, Chapter 2, Page 2-10, Lines 20-22

"If the Bell Canyon aquifers can possibly act as a source of groundwater into the repository due to exploration activity, then it would be prudent to know more about the hydrostatic head gradient of the formation. If there is data on this topic, it was not presented in the PA."

Response

The regional potentiometric surface of the Bell Canyon Formation is presented in Figure 9 of Mercer (1983) and extrapolated static bottomhole pressures in the Bell Canyon in three boreholes (AEC-7, AEC-8, and ERDA-10) tested by the WIPP project are given in Table 4 of the same reference. Pressure and hydraulic head data from the Bell Canyon in two additional boreholes (Cabin Baby-1 and DOE-2) tested by the WIPP project are given in Beauheim et al. (1983) and Beauheim (1986). Data from all five holes indicate that Bell Canyon heads are sufficient to drive brine to the level of the repository in an open borehole; whether flow would be upwards or downwards in this borehole would depend on the pressure conditions existing in the repository at the time.

References

Mercer, J.W. 1983. *Geohydrology of the Proposed Waste Isolation Pilot Plant Site, Los Medanos Area, Southeastern New Mexico.* U.S. Geological Survey Water-Resources Investigations Report 83-4016. Albuquerque, NM: U.S. Geological Survey.

Beauheim, R.L., B.W. Hassinger, and J.A. Klaiber. 1983. *Basic Data Report for Borehole Cabin Baby-1 Deepening and Hydrologic Testing, Waste Isolation Pilot Plant (WIPP) Project, Southeastern New Mexico.* WTSD-TME-020. Albuquerque, NM: U.S. Department of Energy.

Beauheim, R.L. 1986. *Hydraulic-Test Interpretations for Well DOE-2 at the Waste Isolation Pilot Plant (WIPP) Site.* SAND86-1364. Albuquerque, NM: Sandia National Laboratories.

Comment T015

Page 4: I. Technical Comments, A. General, Area of Drifts, Waste Panels

Area in document Volume 3, Chapter 5

"Please clarify the size of the area of the drifts, waste panels and the repository as a whole. The PA uses different numbers for area: 0.5 sq. km and 109,354 sq. meters. What is used in estimating the number of boreholes?"

Response

The areas of the drifts, waste panels, and other features of the repository are given in Table 3.1-1 (which is also keyed to Figure 3.1-2) on page 3-4 of Volume 3. The total excavated area of the disposal region is 111,520 meters^2 but the total area of the disposal region (including pillars and room separators) is 0.5069 kilometers^2. The area used in the 1992 PA to compute the drilling intensity into the repository includes the 111,520 meters^2 of area for CH TRU waste and 14,480 meters^2 hypothetically occupied by RH waste emplaced in the walls of the waste-emplacement panels (total target area of 126,000 meters^2).

Comment T016

Page 4: I. Technical Comments, A. General, Crushed Salt

Area in document Volume 2, Chapter 2, Page 2-48, Line 19

What process was used to prove that crushed salt will compact to 95% of initial density within 100 years?

Response

Creep modeling activities, supported by laboratory measurements on crushed salt and host rock salt, have led to the belief that sufficient deformation will be attained to achieve compaction to about 95%. The most recent modeling efforts are summarized in Van Sambeek et al. (1993). The timing for when the degree of compaction reaches about 95% is directly dependent upon numerous factors such as the steady-state creep rate of the host rock, the initial or emplacement density of the crushed salt, the backstress exerted on the formation by the crushed salt, moisture content of the crushed salt, etc.

Reference

Van Sambeek, L.L., D.D. Luo, M.S. Lin, W. Ostrowski, and D. Oyenuga. 1993. *Seal Design Alternatives Study*. SAND92-7340. Albuquerque, NM: Sandia National Laboratories.

Comment T017

Page 4: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54

"Colloids could potentially have a large impact on the migration or retardation of the radionuclides; they could have a noticeable effect on solubility and sorption of the radionuclides. When will data on colloid formation and transport be collected?"

Response

A laboratory program to determine important information about colloid formation and transport in Salado and Rustler brines is in progress. Some qualitative information from this program has already been transferred to PA for inclusion in future calculations; other information will be provided for the compliance analysis.

Two colloid laboratory programs address the two major types of radiocolloids. Actinide intrinsic colloids, which form by condensation reactions from dissolved radionuclides, are being investigated by a series of screen experiments. Potential carrier colloids, which are ordinarily non-radioactive particles that may act as a substrate for sorption, are being investigated separately, by a series of screening experiments that focuses on evaluating their stability in brines. Results from those two laboratory programs will be incorporated into a model that describes the concentrations of colloid-borne actinides in the disposal room environment. Predictions made with the model will be compared with results from the Source-Term Test Program (STTP) being conducted as part of the Actinide Source-Term Program (see Phillips and Molecke, 1993).

Reference

Phillips, M.L.F., and M.A. Molecke. 1993. *Technical Requirements for the Actinide Source-Term Waste Test Program.* SAND91-2111. Albuquerque, NM: Sandia National Laboratories.

Comment T018

Page 5: I. Technical Comments. A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54, Line 29

"When will it [colloid formation and transport] be modeled in future PAs?"

Response

Transport of colloids will be considered for inclusion in the SPM. Any decision on how to incorporate it in PA for a compliance application will depend on the outcome.

Comment T019

Page 5: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54, Line 29

"How does the lack of information on colloids affect the geochemical and hydrology models developed or under development?"

Response

Colloids may impact current PA modeling in two places: by affecting total concentrations of radionuclides transported in disposal-room brine, and by affecting transport of radionuclides in the Culebra.

Disposal-room actinide concentrations are presently based on values for solubility limits derived from an expert panel (Trauth et al., 1992). The "solubility" panel recognized that suspended forms could contribute to the total concentrations, but concluded that they lacked the information to make any estimate of what that contribution could be.

The distribution coefficients used to describe actinide sorption are also based on expert panel judgment (Trauth et al., 1992), and also do not include colloidal effects. The 1992 PA reported releases into the Culebra and releases transported in fractures only without any sorption. These calculations do not consider the effects of colloids in the disposal room. With regard to colloid-facilitated radionuclide transport in the Culebra, it has been argued that colloids may increase transport rates relative to dissolved species, because colloids may have little retardation and may be preferentially transported in the center of channels where velocities are greater.

Reference

Trauth, K.M., S.C. Hora, R.P. Rechard, and D.R. Anderson. 1992. *The Use of Expert Judgment to Quantify Uncertainty in Solubility and Sorption Parameters for Waste Isolation Pilot Plant Performance Assessment*. SAND92-0479. Albuquerque, NM: Sandia National Laboratories.

Comment T020

Page 5: I. Technical Comments, A. General, Room Closure -vs- Constant Pore Space

Area in document Volume 2, Chapter 7, Page 7-8, Line 13

"If the 'total pore space ... is constant' then room closure (creep compression is not taken into account. However, it is stated on page (7-5) Line 25 room closure is accounted for 'in an indirect way'. This appears to be inconsistent. If you have closure the pore space must decrease. The model appears to be deficient on this point."

Response

The statements referring to constant volume and constant porosity in Volume 2 page 7-7, lines 20 to 24 refer to how the repository was conceptualized prior to 1992. In 1992 the porosity and volume in a disposal room varied in time according to the SANCHO predicted consolidation results, as described in Volume 2, page 7-5.

Comment T021

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"How are values transferred between the regional and the local grids? What is the data/information loss across these boundaries?"

Response

The data between regional and the local grids is transferred by interpolation of heads using either bilinear or integral-preserving (Dukowitz) interpolation. For a locally linear variation of head in the regional grid, no information is lost, neither on heads, gradients, nor fluxes. The motivation for the regional and local grid domain decomposition is to provide improved far-field boundary conditions for the local grid. The compatibility of the interpolation procedure is demonstrated by overlaid contour plots of head, gradients, and fluxes obtained from both regional and local grids.

Comment T022

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"What source (e.g., data and investigator judgment) is used to establish the initial boundary conditions in DOE's modeling efforts?"

Response

The specification of boundary conditions is discussed on page 7-16 in general terms. The sources of boundary condition information for the regional groundwater flow domain are described in more detail in Volume 4, page 6-9 through 6-11.

Comment T023

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"How are the initial boundary conditions peer reviewed?"

Response

There was no formal review limited only to model boundary conditions. Boundary conditions are discussed on page 7-16 only in general terms. Specific boundary conditions for the regional groundwater flow domain are in Volume 4 of the 1992 PA, pages 6-9 through 6-11. Boundary conditions received the same peer review as other aspects of the 1992 PA: internal reviews were performed prior to publication by coauthors and coworkers, by formal SNL technical reviewers, by the Performance Assessment Peer Review Panel, and by SNL and DOE management.

Comment T024

Page 5: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13; Volume 3, Chapter 1, Page 1-7; Volume 3, Chapter 2, Page 2-11

"The PA discusses categories of distributions for different parameter types: continuous, discrete, constructed based on experiments, constructed based on expert judgement, and miscellaneous categories."

"The process used to select a distribution for each parameter needs to be discussed. How is a distribution chosen for a particular set of parameters?"

Response

For more detailed discussions of the ways in which distributions of uncertain parameters were constructed in the 1990, 1991 and 1992 PAs, see Tierney (1990, in particular, Figure E-1) and Tierney (1994).

References

Tierney, M.S. 1990. *Constructing Probability Distributions of Uncertain Variables in the Models of the Performance of the Waste Isolation Pilot Plant (WIPP).* SAND90-2510. Albuquerque, NM: Sandia National Laboratories.

Tierney, M.S. 1994. "Using Data and Information to Form Distributions of Model Parameters in Stochastic Simulations of Performance of the Waste Isolation Pilot Plant (WIPP)", *Proceedings of PSAM-II, San Diego, California, U.S.A., March 20-25, 1994.* 051-9 to 051-16.

Comment T025

Page 5: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13, Volume 3, Chapter 1, Page 1-7, Volume 3, Chapter 2, Page 2-11

"How representative are these distributions of actual data?"

Response

Of the distributions of the 49 parameters sampled for human intrusion analyses in the 1992 PA (Volume 3): five were histograms of actual field measurements; three were distributions inferred from actual measurements (e.g., Culebra transmissivities); 18 were constructed by formal elicitation of expert opinion, which may indirectly be linked to data; and the remainder (23) were constructed on the basis of informal expert judgment using the five-step procedure described in Figure E-1 of Tierney (1990). It is not known at this time how well subjectively determined distributions reflect the true uncertainty in a model parameter or how well these distributions represent actual WIPP-specific conditions.

Reference

Tierney, M.S. 1990. *Constructing Probability Distributions of Uncertain Variables in the Models of the Performance of the Waste Isolation Pilot Plant (WIPP).* SAND90-2510. Albuquerque, NM: Sandia National Laboratories.

Comment T026

Page 5-6: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13, Volume 3, Chapter 1, Page 1-7, Volume 3, Chapter 2, Page 2-11

"The constructed distribution type should differentiate between values derived from measurements and those derived from expert judgement."

"How were the input parameters chosen? How many are there, and how many are variable? Which ones are important?"

Response

In theory, expert judgment is founded in measurements and other data collection activities albeit not necessarily WIPP specific. Consequently, to use the general rule indicated here may not be reasonable. Instead, some rationale should be provided for the combination of the two sources of parameter values.

Input parameters are dictated by the nature of the mathematical models used in the PA (see Section 1.4 of Volume 3). There were nearly 400 input parameters in the 1992 PA; 49 of them were treated as uncertain (variable) for the purposes of sensitivity/uncertainty analyses for human intrusion analyses (see sections 6.1 and 6.2 of Volume 3). The most sensitive (important?) parameters in the 1992 PA are described in Table 9-3 of Volume 4, and Table 6-1 of Volume 5.

Comment T027

Page 6: I. Technical Comments, A. General, Assignment of Probability Distributions

Area in document Volume 3, Chapter 1, Page 1-18, Line 37

"Very general procedures are described for assigning probability distributions, but these procedures are incomplete and do not answer critical questions."

"Please clarify the review process used for assigning probability distributions? How do you determine confidence in the probabilities?"

"What are the constructed distributions and which does DOE expect will be replaced by data? (Volume 2, page 6-4)."

Response

The "replacement of constructed distributions by data" is an activity that will be considered for inclusion in the SPM, i.e., the addition of experimental data where required to support a compliance application.

The review process used to assign probability distributions in the 1992 PA is briefly described in Section 1.3.1 of Volume 3; evidence of the implementation of this process is shown in the many memos of Appendix A of the latter document. All parameters used in the 1992 PA were classified as "X" among the three quality-assurance categories of ascending confidence, X, C, and A. To be classified as "C", a parameter would have to have documentation of the line of reasoning that established its distribution and the sources of any data used in constructing the distribution. An "A" class parameter must also have received a documented peer review.

Constructed distributions are explained on page 1-10 of Volume 3. This category of distributions is characterized by direct use of data to form an empirical cumulative distribution function; the "data" may be measurements of real quantities or the set of percentiles obtained from an elicitation of subjective opinion. Ideally, all constructed distributions would be based on real measurements; however, the Project must focus on sensitive parameters.

Additional detail on the construction and use of distributions will be included in the final compliance application.

Comment T028

Page 6; I. Technical Comments, A. General, Heterogeneous Reservoirs

Area in document Volume 3, Chapter 1, Page 1-21 - Line 60

"What is meant by 'reservoirs' in the context of the BRAGFLO model?"

Response

The term reservoir is used in the context of formation, host rock, and porous media, etc., not in the context of brine reservoir per se. The predecessor to BRAGFLO was a multi-phase flow code used in the petroleum field -- thus the use of the term 'oil reservoir' or 'reservoir' model.

Comment T029

Page 6: I. Technical Comments, A. General, Brine Reservoirs

Area in document Volume 3, Chapter 1, Page 1-30, Line 15

"Why is the 'sample intensity function' multiplied by the 'fraction of the repository area that is underlain by brine reservoirs'?"

Response

This question arises in the context of the brief description of the model for computing computational scenario probabilities given in Section 1.4.2 of Volume 3. A more thorough treatment of the same subject, models used to compute human-intrusion probabilities for different summary scenarios, is given in Section 5.2 of Volume 2; the answer to the present question is given by lines 10 thru 28 of page 5-4, including Equation (5-13), in the latter reference.

The cited text is simply an example being given wherein the fraction of repository area underlain by brine reservoirs is of interest for a particular intrusion event, E1 (E1 is an event in which one or more boreholes pass through a waste panel and into a brine reservoir).

Comment T030

Page 6: I. Technical Comments, A. General, Viscosity

Area in document Volume 3, Chapter 1, Page 1-34, Line 9

"DOE states that 'viscosity measurements for an oil-based, 1.7 - kg/m^3 mud'. Why would an oil-based mud be used to drill through the Salado Salt Beds instead of a high-salt water-based mud?"

Response

A high-salt, water based mud is assumed to be the drilling mud used when drilling through the Salado. The Oldroyd model requires a value for the ratio of the initial viscosity (at zero shear rate) to the plastic viscosity, to fully define the model in the low shear regime. This ratio was not available for a high-salt, water-based mud in 1992 so a ratio based on an oil based mud was chosen. Since high shear rates occur at the borehole wall the value chosen for the ratio was expected to have little impact on the final model diameter. The Project is evaluating the sensitivity of eroded diameter to this issue.

Comment T031

Page 7: I. Technical Comments, A. General, "Dual Porosity" Model

Area in document Volume 3, Chapter 1, Page 3-39, Line 1

"The way the 'dual porosity' model is described generates confusion. Does the model really allow diffusion through the rock matrix?"

Response

Yes, the PA's dual porosity model allows diffusion through the rock matrix. In this transport model fluid only flows (advects) along fractures. In this way, solutes (i.e., dissolved actinides) are advectively transported in the fracture void volume and diffuse into the much larger matrix void volume. The SECOTP transport code numerically simulates the diffusion process with a mass transfer term. This term incorporates the free water molecular diffusion of each solute, the tortuosity of the matrix, and the solute concentration gradient between the fractures and the matrix.

References

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra*

Dolomite at the Waste Isolation Pilot Plant Site. SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Beauheim, R.L., and P.B. Davies. 1992. *Experimental Plan for Tracer Testing in the Culebra Dolomite at the WIPP Site.* Revision A. Albuquerque, NM: Sandia National Laboratories.

Comment T032

Page 7: I. Technical Comments, A. General, "Dual Porosity" Model

Area in document Volume 3, Chapter 1, Page 1-39, Line 1

"How does the flow model (SECO2D) compare with laboratory results? Is it verifiable?"

Response

Results from the SECO2D code have not been compared to laboratory results. This code is designed to simulate flow that occurs at a scale that is larger than what could be represented in a laboratory experiment. The SECO2D flow code solves the partial differential flow equation for heads in a 2D, confined, heterogeneous aquifer that obeys Darcy's Law. The flow code has been benchmarked, tested and verified for freedom from coding errors, order of convergence, and discretization consistency (Roache et al., 1990).

Reference

Roache, P., P.M. Knupp, S. Steinberg, and R.L. Blaine. 1990. "Experience with Benchmark Test Cases for Groundwater Flow," *Forum on Benchmark Test Cases for Computational Fluid Dynamics, ASME Fluid Engineering Division Spring Conference, Toronto, Ontario, Canada., June 4-7, 1990.*

Comment T033

Page 7: I. Technical Comments, A. General, Base of Anhydrite III

Area in document Volume 3, Chapter 2, Page 2-4, Line 14

"If the base of the Anhydrite III is so important it would seem more accurate, to create a regional contour map of the base of the Anhydrite III. The North-South geologic cross-section may not account for all unknowns."

Response

The point is well taken. The exact elevation of the base of Anhydrite III beneath the WIPP is not well constrained. Well data are not available for the region immediately beneath the panels. Other methods, including the construction of regional contour maps on the base of the unit could have been used to estimate its elevation. Regional dips are small, however, and the uncertainty introduced locally by a limited stratigraphic data base may be small compared to the uncertainty in the interpretation of the depth to the conducting layer and the interpretation of the conducting layer as brine (see page 5-2 and following text in Volume 3 of the 1992 PA).

Comment T034

Page 7: I. Technical Comments, A. General, Top of Bell Canyon

Area in document Volume 3, Chapter 2, Page 2-10

"A more accurate value for the Top of Bell Canyon can be found by the method outlined in the comment for the Base of Anhydrite III (above)."

Response

The point is valid. The exact elevation of the top of the Bell Canyon Formation beneath the WIPP is not well constrained. Well data are not available for the region beneath the panels. Other methods, including the construction of regional contour maps on the base of the unit, could have been used to estimate its elevation. As seen in well and seismic data, however, regional dips are small, and the uncertainty introduced by limited stratigraphic control may be small compared to the uncertainty in the interpretation of the depth to the conducting layer and the interpretation of the conducting layer as brine (see page 5-2 and following text in Volume 3 of the 1992 PA).

Comment T035

Page 7: I. Technical Comments, A. General, Natural-Analog Data

Area in document Vol 3, Chapter 2, Page 2-12

"It would seem using parameters of sandstones and substituting them for salt is inappropriate because the stress characteristics, the permeability and the porosities are quite different. The performance of salt does not compare to sandstone."

Response

Two-phase characteristics of salt (capillary pressure, relative permeability) have not been measured experimentally for WIPP-specific materials. Very little research has been done on the two-phase properties of very low permeability rock. A search failed to produce data and/or curves that are directly applicable to WIPP. Therefore, an approximate analog approach was taken, based on the lowest permeability rock for which capillary pressure and relative permeability data have actually been measured. A tight gas sand core (Sample MWX 67-35) from the multi-well experiment (Morrow et al., 1986) was selected as the best analog material. This sample is a fine-grained sandstone with bedding and 12 percent porosity. The dominant pore geometry consists of intergranular cracks between abutting quartz grains and solution pores partially filled with dolomite. The permeability of this sample to brine is 43 microdarcies ($\sim 43. \times 10^{-18}$ m^2) at 3.4 MPa confining pressure and 24 microdarcies ($\sim 24. \times 10^{-18}$ m^2) at 34 MPa confining pressure. Based on these results, and a study of threshold pressure (Davies, 1991), two-phase flow in pure or impure halite units is not anticipated. Two-phase flow is confined to the various anhydrite marker beds within the Salado Formation, making the selection of two-phase properties in the halite units (except for threshold pressure) unimportant.

References

Morrow, N.R., J.S. Ward, and K.R. Brower. 1986. "Rock Matrix and Fracture Analysis of Flow in Western Tight Gas Sands," *1985 Annual Report, New Mexico Institute of Mining and Technology*. DOE/MC/21179-2032.

Davies, P.B. 1991. *Evaluation of the Role of Threshold Pressure in Controlling Flow of Waste-Generated Gas into Bedded Salt of the Waste Isolation Pilot Plant*. SAND90-3246. Albuquerque, NM: Sandia National Laboratories.

Comment T036

Page 8: I. Technical Comments, A. General, Lack of Halite and Polyhalite Chemical Interaction

Area in document Volume 3, Chapter 2, Page 2-28, Line 42.

"What justification or data is there to support the comment that 'halite and polyhalite ... are assumed ... not to interact chemically with any contaminants'? This is a very important assumption."

Response

This statement is misleading. The statement should say that the salts have limited sorption potential, and therefore do not provide a significant retardation mechanism. A conservative assumption of 0 for the partition coefficient is used.

Comment T037

Page 8: I. Technical Comments, A. General, Radionuclides for Transport Modeling

Area in document Volume 3, Chapter 3, Page 3-29, Line 31

"Why were only nine radionuclides considered in the 1992 PA transport calculations (and solubility estimates) for CH-TRU waste? This needs to be more clearly explained."

Response

The answer can be found in Section 7.3 of Volume 4 of the 1992 PA. Figure 7.3-1 (page 7-12 of Volume 4) shows plots of radionuclide inventory through time in normalized EPA units. With the exception of Pu-238, only those radionuclides that exist at 1,000 years or later in activities greater than 10^{-2} EPA units were included in transport modeling. The reason for excluding the others is straightforward: they cannot contribute to exceeding regulatory limits even if their entire inventory is released. Pu-238 was omitted from groundwater transport calculations in error and will be included in future analyses—it is a major factor in total inventory before 1,000 years, but rapidly drops out of the inventory after 1,000 years. However, for the fracture-only transport model travel times are sufficiently short that some Pu-238 could reach the accessible environment boundary. The effect on fracture-only transport releases could be significant for intrusions occurring before 1,000 years.

Note that the use of the cutoff of 10^{-2} EPA units is cautious; the total release limit is 1 EPA unit.

Note also that a total of 23 radionuclides were included in cuttings releases, allowing for the full consideration of short-lived species in the cuttings releases.

See Appendix D of Volume 4 for memoranda describing the use of this reasoning in designing actinide and transport experimental programs.

Comment T038

I. Technical Comments, A. General, Excavated Area Underlain by Brine Reservoir

Area in document Volume 3, Chapter 5, Pages 5-2 to 5-11

(a) "What is the accuracy of Transient Electromagnetic Methods? The depth precision 'may be $\pm$ 75 m,' but what about the accuracy of the process itself to 'see' fluids? Electromagnetic (EM) methods tend to be gross estimators at best. Why was the data not extended to the ERDA-9 borehole to calibrate the measurements?"

(b) "Has DOE investigated the use of a High-Frequency Three-Dimensional (3D) Common Depth-Point Seismic Survey over the WIPP disposal panels with extended coverage of one-half mile around the panels? The survey should include the ERDA-9 borehole as a reality check. If these pressurized brine reservoirs are associated with anticlinal structures in the upper anhydrite layer then a 3D Survey will clearly, with high confidence, define even a small closure at this depth."

Response

We believe that the geophysical studies are complete and adequately documented. At present, we believe that the resistivity characterization of possible brine distribution beneath the waste panels has provided information to a level of detail exceeding that provided by on-site/near-site stratigraphic data presently available. However, we believe that the assumption that any conductor identified within the Castile is due to the presence of brine is reasonably conservative for purposes of PA.

The Project has investigated and resolved these issues as follows:

1. The validation (accuracy) of the transient electromagnetic method to detect brine was done at the same time as the panel survey by running the same survey over the WIPP-12 area (brine reservoir present known from drilling) and the DOE-1 area (brine reservoirs absent known from drilling) (Reference SAND87-7144, p. 14, Fig. 3-8). Additional validation work with several methods was done prior to the panel survey using the known brine reservoir at WIPP-12 and an artificial target placed in the underground WIPP facility. ERDA-9 was not drilled deep enough into the Castile for most brine occurrences. However, the results of the dual-induction log of hole ERDA-9 was used directly to constrain the resistivity of brine-free Salado/Castile halites and anhydrites.

2. Seismic methods were not the method of choice for brine reservoir delineation after the Project's experience with the original seismic reflection lines for several reasons: a) While seismic studies delineate deformed areas within the Castile (called deformation zone, DZ, in Borns and others, 1993), not all anticlines contain brine reservoirs. Drillholes, such as WIPP-12, in areas of moderately deformed Castile, as well as

drillholes in severely deformed Castile both encounter brine and; b) Lateral velocity structures produce false anticlines. In 1982, drillhole DOE-1 was drilled into an apparent anticline that was based on the seismic surveys. This structure was nonexistent, and the original interpretation was due to the lateral velocity variations; c) it has proven difficult to propagate high frequency signals in the portion of the basin; d) brine reservoirs are characterized by a low fracture porosity (1%), which does not result in a change in velocity much above background variation in velocity. The contrast between the resistivity of a brine reservoir (1 ohm-m) and the background anhydrite (100 - 1000 ohm-m) is significant. Several early Project conclusions were that (1) seismic methods alone are not sufficient to answer whether brine reservoirs are under the site and (2) gravity methods were not effective in mapping deformation.

Considerable development and review went into the selection of methods. Specifically for the brine reservoirs in the early 1980s, WIPP began to study methods based on the measurement of electrical conductivity or resistivity (for example, Controlled Source Audio Magneto Telluric--CSAMT and charged body--mis-a-la-masse) (Elliot, 1982). The basic assumption is that the brine-charged fractures of a brine reservoir will represent a significant conductivity contrast within the Castile. CSAMT was the method most extensively tried at WIPP during this period (Bartel and others, 1983) to delineate the brine reservoirs in the Castile. The EEG sponsored review of the Sandia program for delineating brine reservoirs and the CSAMT method by J. Waite (U or A) and Peter Hoekstra (Geophysicon). This review suggested that WIPP consider other electromagnetic methods specifically the transient electromagnetic method (TEM) a.k.a. time domain electromagnetic methods (TDEM). In 1985, SNL conducted validation surveys in an area around a known brine occurrence (WIPP 12) using several methods (CSAMT, TEM, and Frequency Domain Electromagnetic methods [FEM]) with the Colorado School of Mines and Phoenix Geophysics. Some of the surveys also delineated an artificial target placed in the WIPP underground. Analyses of survey results concur with EEG review recommendations that transient (or time-domain) electromagnetic methods are best suited for delineation of brine reservoirs at the WIPP site.

Comment T039

Page 8: I. Technical Comments, A. General, Organic Containers

Area in document Volume 3, Chapter 3, Page 3-9

"The text on page 3-9 [of Volume 3] states that containers are 55 gallon drums or SWBs, yet on page 4-11 [of Volume 2] it states that some 'waste containers' will be composed of organic material. Please explain this discrepancy."

Response

This is an editorial error: The words "and some waste containers" should be deleted in lines 11-12 on page 4-11 (Volume 2) .

Comment T040

Page 9: I. Technical Comments, A. General Comments, Ideal Gas Law

Area in document Volume 2, Chapter 2, Page 2-34

"How valid is the use of the ideal gas law at lithostatic or hydrostatic pressures? How sensitive a parameter is it? Assumptions of this nature should be explained and justified."

Response

A set of comparative calculations were made using the ideal gas law and several non-ideal equations of state. These calculations showed very little variation in the calculated properties (< 10%). This amount of uncertainty has very little effect on PA calculations, since other sources of uncertainty are much more important.

Comment T041

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"The screening process described in detail in the 1991 documentation should be included in the EPA PA."

Response

The 1992 PA was not intended to be interpreted as a compliance application. Additional material will be included as needed in the compliance application.

Comment T042

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"The possibility of nuclear criticality should be investigated further before it is screened out."

Response

The DOE has initiated further evaluation of nuclear criticality.

Comment T043

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"How will the changes in the repromulgated 40 CFR 191 effect the scenarios that are included?"

Response

This question is still under evaluation. No changes in scenarios specific to the repromulgation of 191.15 and Subpart C are identified at this time. Clearly, the time interval that must be considered for 191.15 and Subpart C has changed. However, 10,000 yr undisturbed performance was already included in the scenario development process as the base case for 191.13.

Comment T044

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"How are scenario uncertainties propagated through the analyses; if scenario uncertainties were included in the screening process, would more scenarios be included?"

Response

As discussed in Chapter 4 of Volume 2, Section 4.2, scenarios are constructed from the events and processes that may affect the system in the future. Uncertainties about these events and processes are explicitly considered in the screening process. Those events and processes that survive the screening process (based on the criteria discussed in the previous section of Chapter 4 of Volume 2) are used to construct the scenarios for consideration. Uncertainty about the occurrence of those events is reflected in the estimation of the probability of their occurrence, which in turn is reflected in the estimation of scenario probabilities.

Comment T045

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, 4-19

"DOE says that the 'effect of subsidence of potash mining will be added in future PAs'. How is the WIPP site influenced by potash mining and its associated water use? When will the effect of subsidence due to potash mining be added?"

"The mining scenario should consider mining in the potash zone in the controlled area, but above the repository."

Response

The text in question has been paraphrased, rather than quoted exactly, in the comment. The relevant phrase occurs on line 21, and states "...the impact of subsidence events will be examined in future analyses." A decision to add consequence modeling of subsidence effects to the full PA cannot be made until these analyses are available. As noted elsewhere in this volume, "consequences of such potash mining ... will be addressed in future analyses when a three-dimensional model for regional groundwater flow is available" (Volume 1, page 3-11, lines 16-19; see also page 6-3, lines 19-22). A three-dimensional flow model is now operational and ready for preliminary analyses.

As discussed in the event and process screening text in Volume 1 of the 1991 PA (SAND91-0893/1, page 4-35, lines 7-13), subsidence over mines has the potential to affect regional groundwater flow both by creating catchment basins at the surface (changing recharge) and by fracturing hydrostratigraphic units (altering hydraulic conductivity).

The final point here, that mining should be considered within the controlled area, is a point of regulatory interpretation. The wording of Appendix C of 40 CFR 191 indicates that systematic exploitation can be effectively deferred by controls. Mining is such a systematic process and does not occur inadvertently and intermittently.

Comment T046

Page 10: I. Technical Comments, A. General, Probabilities

Area in document Volume 1, Chapter 4, Page 4-2

"How are the probabilities used in the cutoff comparison found, calculated, etc. This explanation should be within the PA."

Response

As the text on page 4-2 of Volume 1 notes, this information is summarized in Chapter 4 of Volume 2 and described in detail in Volume 1 of the 1991 PA (SAND91-0893).

Comment T047

Page 10: I. Technical Comments, A. General, Probabilities

Area in document Volume 1, Chapter 4, Page 4-2

"What are the expected probabilities for each scenario? They should be stated on the same page as the event tree or on the event tree."

Response

The event tree displays summary scenarios, as described in more detail in Section 3.2.2 of Volume 2. These summary scenarios are further subdivided into computational scenarios on the basis of time and number of intrusions, as discussed in detail in Chapter 2 of Volume 4 of the 1992 PA. Probabilities estimated using the Poisson model for intrusion are assigned to computational scenarios, rather than to the summary scenarios, and therefore cannot be displayed on Figure 4-1 as requested. Probabilities for selected computational scenarios are given in table form in Chapter 2 of Volume 4 for specific values of the Poisson rate constant.

Comment T048

Page 10: I. Technical Comments, A. General, Base Case Explanation

Area in document Volume 1, Chapter 4, Page 4-2, Line 40

"All of the events placed in the 'base case' need to be described in detail with an explanation of how the event probability was developed and provide justification for placing these scenarios in the base case."

Response

As discussed in more detail in Volume 2 and in Volume 1 of the 1991 PA (SAND91-0893), all events placed in the base case were assigned a probability of 1. In compliance documentation, a rationale for the assignment of probabilities will be given.

Comment T049

Page 10: I. Technical Comments, A. General, Nearby Boreholes as Communication Pathways

Area in document Volume 1, Page 4-4; Volume 2, Page 2-16

"Have you considered transport to and through boreholes that don't hit the repository, but which could increase the transport of radionuclides?"

"Would current and future boreholes alter the vertical flow regime between units? (Volume 2, page 2-30)"

"Is it possible for the high drilling density around the WIPP site to cause salt dissolution much like in oil and gas fields in Pecos County, Texas? Should this scenario be considered?"

Response

The issue of the consequences of "near misses" potentially affects the compliance analysis and will be reexamined and evaluated. Rates of brine flow from the repository to a "near miss" were examined quantitatively in 1991 in response to comments by the EEG on the 1990 PA. Results of these analyses are reported in pages B-18 through B-26 of Volume 1 of the 1991 PA (SAND91-0893/1). For the assumptions of these analyses (including no lateral development of the DRZ), flow rates were shown to decrease more than two orders of magnitude 0.25 m from the waste. Flow was decreased further at greater distances. Based on these analyses, PA concluded that radionuclide releases up a borehole that directly penetrated the waste would be greater than those from a "near miss."

The Project is currently investigating the issue of existing and future boreholes that could alter the vertical flow regime. Three-dimensional regional flow modeling is in progress to permit evaluation of possible consequences.

The Project has already investigated and resolved the issue of dissolution of salt by oil field drilling and work is complete. Dissolution of salt by oil field drilling has been considered quantitatively in the past by the WIPP Project (see Christensen et al., 1983), although not on the scale proposed here. Previous work has indicated that dissolution by freshwater flowing through a single well does not pose a threat to the WIPP. Possible effects of high-density drilling outside the controlled area can be evaluated for inclusion in the system prioritization. High-density drilling within the controlled area constitutes an intrusion scenario more severe than "inadvertent and intermittent...exploratory drilling."

Reference

Christensen, C.L., C.W. Gulick, and S.J. Lambert. 1983. *Sealing Concepts for the Waste Isolation Pilot Plant (WIPP) Site*. SAND81-2195. Albuquerque, NM: Sandia National Laboratories.

Comment T050

Page 10: Section: I. Technical Comments, A. General, Thermal Effect of Pu-238

Area in document General

"Has the thermal effect of Pu-238 been taken into account in PA Analyses? What would its effect be?"

Response

It is assumed that the comment refers to the thermal load of radioactive waste. Justification for ignoring radioactive induced thermal effects is given in Butcher and Mendenhall (1993, Section 3.7, page 3-26).

In addition, the Performance Assessment Department looked into this issue and concluded a maximum temperature rise of 2° C in the repository falling to 1° C after 80 years (Volume 1, page 4-50 of the 1991 PA [SAND91-0893/1]).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B*. SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T051

Page 11: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"...at issue is the behavior of the clay (bentonite) units "sandwiched" between concrete plugs."

Response

In the reference seal design report (Nowak et al., 1990), the reasons for using swelling clay components are summarized and references to more detailed discussions are provided. In response to the specific questions regarding the clay, it is believed that the clay units may become fully saturated. Clay is not likely to intrude into all exposed fractures and voids although it is obvious that flow through many of these will be limited by the clay. It is intended that the swelling pressure will be controlled (via the initial density of the material) to preclude significant, deleterious fracturing due to the swelling.

Reference

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant*. SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Comment T052

Page 11: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

(Paraphrase) Numerous specific questions are offered related to the removal of shaft liners. The essence of the questions is captured by: Is it necessary to remove the liner? Can this be done safely? What are the consequences?

Response

The question of whether or not to remove the shaft liners (or portions of them) remains an active question within the WIPP sealing program. It is clear that if the liners need to be removed in order to assure effective seals, they can and will be removed. Safe removal of portions of shaft liners has been accomplished in the Carlsbad area in nearby potash mine shafts. Only small amounts of water inflow have been encountered during construction operations and drilling operations conducted in the WIPP shafts. Design and performance considerations for the WIPP shaft seals generally stress reliance upon the components placed within the Salado formation below the current shaft liner and key; if the final designs maintain this reliance, it may not be necessary to remove any significant portions of the existing liners. Detailed design descriptions to be included in compliance documentation will describe whether or not removal is intended and (if needed) procedures for the safe removal of this material.

M

Comment T053

Page 11-12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"Compaction details have not been provided which may further impact on the method in which the backfill is emplaced and the effectiveness of the backfill as a seal."

Response

This is an ongoing area of significant effort. While the Small-Scale Seal Test Program in Test Series C and D have provided (see summary in Finley and Tillerson [1992]) an indication that an initial relative density of about 80% can be achieved, additional demonstrations are planned to determine if a relative density of at least 85% can be achieved. These additional demonstrations are part of the planned Large-Scale Seal Tests proposed for the WIPP. Results of the demonstrations are intended to be part of the compliance documentation.

Reference

Finley, R.E., and J.R. Tillerson. 1992. *WIPP Small Scale Seal Performance Tests - Status and Impacts*. SAND91-2247. Albuquerque, NM: Sandia National Laboratories.

Comment T054

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"Small amounts of water are considered beneficial in the consolidation of the salt backfill (St 88), but the effects of channelling on individual blocks of salt is uncertain. Channelling of water from upper aquifers seems more likely than a uniform dispersion throughout the backfill. This potential problem needs to be addressed in the PA.

Response

At the present time, efforts are focusing on evaluating methods for in-place compaction of crushed salt. The use of salt blocks is at present considered a backup technology. If in-place compaction of the crushed salt is successful, there will be no reason to do further evaluation of the behavior of blocks. On the other hand, if the use of precompacted blocks of crushed salt becomes the preferred technology, evaluations of phenomena such as this will be completed.

Comment T055

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"...to insure conformance with the circular shaft walls, blocks must be specially milled and placed. No estimate to the amount of time required to fill each shaft by carefully placing blocks has been provided. Such information is necessary to establish the practicality of the approach."

Response

If blocks are to be used in the shafts as part of the sealing approach, there is no question but that the practicality of their emplacement must be established. In the Small-Scale Seal Performance tests, the feasibility of making and emplacing blocks was demonstrated. At the present time however, efforts are focusing on evaluating methods for in-place compaction of crushed salt. If this technology can be demonstrated to achieve the desired degree of compaction, it offers advantages related to cost and safety. Demonstration tests are currently being planned which will evaluate the degree of compaction that can be achieved. If in-place compaction of the crushed salt is successful, there will be no reason to further evaluate the behavior of blocks. On the other hand, if the use of precompacted blocks of crushed salt becomes the preferred technology, evaluations of the practicality of full-scale placement of blocks will be initiated. Because of the planned sequence of evaluations, the use of salt blocks is at present considered a backup technology and very little resources are being expended to evaluate phenomena specific to this technology.

Comment T056

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 1, Page 5-20

"It is stated that the repository will use bentonite and crushed-salt backfill as a barrier in waste emplacement panels. Isn't it more correct to say that SNL has recommended this backfill but that DOE has made no commitment to use it?"

Response

The EPA suggested statement is correct. SAND90-3074 discusses the scientific aspects of crushed salt/bentonite backfill but no determination has yet been made. Backfill will be considered in the SPM.

Reference

Butcher, B. M., 1991, *The Advantages of Salt/Bentonite Backfill for the WIPP Disposal Rooms*, SAND90-3074, Albuquerque, NM: Sandia National Laboratories.

Comment T057

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Page 2-48

(Restatement) What data are there to support the assumed long-term permeabilities in the shaft seals?

Response

Numerous laboratory studies have been completed which document the permeability of compacted, crushed salt samples. These data are summarized in Figure 4 of Hansen et al. (1993). The relationship is shown between the permeability of the crushed salt samples and the relative/fractional density of the sample. This paper also references the numerous reports in which the data were first documented. In addition, the small-scale seals and the thermal-structural interaction tests are providing some useful information regarding the transfer of the lab studies and analysis results to field situations.

Reference

Hansen, F.D., G.D. Callahan, and L.L. Van Sambeek. 1993. "Reconsolidation of Salt as Applied to Permanent Seals for the Waste Isolation Pilot Plant," *3rd Conference on the Mechanical Behavior of Salt, September 14-16, 1993, Ecole Polytechnique, 91129 Palaiseau, Cedex-France.*

Comment T058

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Page 2-48

"What is the likelihood that the panel and shaft seals will be able to prevent migration through MB-139, both under the excavation and away from the excavation in the Disturbed Rock Zone?"

Response

The shaft seals are not designed to prevent lateral migration through MB-139: flow up the shafts from MB-139 would be very limited by the combination of short and long term components included in the shaft system.

The sealing concepts for the WIPP panel seals (see Nowak et al. [1990]) include provisions for grouting of the Marker Beds or the DRZ in the halite as necessary to limit flow. Effective placement of grout into Marker Bed 139 has been part of the technology development activities performed under the direction of Sandia National Laboratories. Specifically, as indicated in the test plan for the Small-Scale Seal Performance Test-Series F (Ahrens, 1992), this underground test at the WIPP was "intended to demonstrate equipment and techniques for producing, injecting, and evaluating microfine cementitious grout." The grouting was completed in March, 1993, and the final report is currently being prepared.

From the standpoint of long-term performance of the disposal system, the sealing of the marker bed in the immediate vicinity of the waste rooms is of little consequence since virgin conditions will exist within the near vicinity of the excavation. It is these virgin conditions that provide the robust natural barrier to contaminant transport. The project is currently evaluating the advantages of sealing these units, however, from an operational standpoint.

References

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant.* SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Ahrens, Ernst H. 1992. *Test Plan - Sealing of the Disturbed Rock Zone (DRZ), Including Marker Bed 139 (MB139) and the Overlying Halite, Below the Repository Horizon, at the Waste Isolation Pilot Plant - Small-Scale Seal Performance Test - Series F.* Albuquerque, NM: Sandia National Laboratories.

Comment T059

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"It is correctly stated that the current backfill design (which is used in the 1992 PA calculations) is "pure, unconsolidated crushed salt with a relatively high permeability that provides little resistance to fluid flow." Salt and bentonite backfill have been studied and said to be available if needed. However, the PA has not yet reported any analyses to indicate the benefits this mixture might provide."

Response

The backfill condition stated here was a PA assumption for the 1992 calculations. In actuality, the backfill is expected to rapidly consolidate to a dense, low permeable state as described in Butcher (1991, Figure 4-4, page 28).

Optimization analyses have not been performed. Preliminary sensitivity analyses showing the potential benefits of reducing porosity and permeability within the waste-disposal area were performed using the 1989 PA modeling system and reported in Bertram-Howery and Swift (1990).

References

Butcher, B.M. 1991. *The Advantages of a Salt/Bentonite Backfill for Waste Isolation Pilot Plant Disposal Rooms.* SAND90-3074. Albuquerque, NM: Sandia National Laboratories.

Bertram-Howery, S.G., and P.N. Swift. 1990. *Status Report: Potential for Long-Term Isolation by the Waste Isolation Pilot Plant Disposal System.* SAND90-0616. Albuquerque, NM: Sandia National Laboratories.

M

Comment T060

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"Neither have analyses been performed to determine the benefit of reducing the initial void space in the backfill."

Response

Analyses have not been performed, but backfill consolidation is estimated to occur so rapidly that reduction of the initial void space would correspond to accelerating closure by no more than 10 years. Crushed-salt porosities are estimated to decrease to less than 10% within 40 years. For this reason, control of initial emplacement density is considered unimportant, as discussed in Butcher and Mendenhall (1993, page 42).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B*. SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T061

Page 12-13: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"The design of backfill is part of the engineered alternatives (engineered barriers) issue and evaluations of relative benefits should be included."

Response

Crushed salt backfill with additives is an engineered alternative that DOE will evaluate for consideration in the SPM. These will also be part of the engineered alternatives benefit/detriment study being performed by the DOE.

Comment T062

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 6-1, Line 9

"The statement that the PA Department has a high level of confidence, etc., is perhaps premature."

Response

The 1992 PA was not intended as a compliance application. The sentence in question does not say that the PA Department is confident that compliance has been demonstrated. Rather, the statement was made that PA Department is confident that "the WIPP will be able to comply with the quantitative requirements of the Standard...."

Comment T063

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 6-2, Lines 20-28

"More documentation is needed before it can be concluded 'that no radionuclides will reach the accessible environment from the undisturbed repository for 10,000 years.'"

Response

The 1992 PA was not intended to be a compliance application. More documentation will be provided in the compliance application. The Format and Content Guide provides a snapshot of the comprehensive nature of the application.

Comment T064

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Chapter 6, Page 6-2, Line 37-44

"Any conclusions about meeting the Groundwater Protection Requirements are premature since these requirements have been changed in the repromulgated standard."

Response

Conclusions for the 1992 PA were based on 40 CFR 191 as it existed at that time. All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated in future performance assessments. The specific conclusion about the ability to meet the Groundwater Protection Requirements depended only on the reported results (i.e., no releases to the accessible environment).

As was stated in the 1992 PA (Volume 1, Chapter 3, p. 3-23, l. 25-31), "One of the products of scenario development for the Containment Requirements is a base-case scenario for the WIPP that describes undisturbed conditions. The undisturbed performance of the repository is its design-basis behavior, including variations in that behavior resulting from uncertainties in the 10,000-year performance of natural and engineered barriers and excluding human intrusion and unlikely natural events, as defined in §191.12(p)." Thus, the 10,000 year issue has already been addressed for the undisturbed case, i.e., Groundwater Protection Requirements and Individual Protection Requirements. Future performance assessment calculations, if conducted similarly to those in 1992, will not cause a change in the conclusions since nothing has yet to be shown to be released from the disposal system for undisturbed performance. The results of any future performance assessment calculations conducted using different conceptual models, probability distributions, etc., will of course be evaluated for compliance with the Groundwater Protection Requirements.

The concentration limits for the Groundwater Protection Requirements have not changed between the 1985 and 1993 versions of 40 CFR 191. While not called such, the limits in the 1985 version were the Maximum Contaminant Levels (MCLs) from 40 CFR 141 (5 pCi/l for Ra-226 and Ra-228; 15 pCi/l for gross alpha particle activity; 4 mrem/yr for beta particles and photon radioactivity). Those MCLs have not been changed since, and are now officially incorporated into 40 CFR 191. The definition of the groundwater that is to be protected did change between the 1985 (special sources of groundwater) and 1993 (underground sources of drinking water) versions of 40 CFR 191. The revised definition may cause programmatic changes if DOE is required to identify all potential underground sources of drinking water. As stated in a paper recently presented at the Waste Management '94 conference (Trauth et al., 1994), we propose to determine when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application). Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

Reference

Trauth, K.M., S.G, Bertram, and B. Bower. 1994. "Considerations for Guidance for Radioactive Waste Disposal Arising from Rules Under 40 CFR 191 and 40 CFR 194," *Proceedings of Waste Management '94 Conference, Tucson, AZ, February 27-March 3, 1994.*

Comment T065

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(1) the determination of the extent that expert judgment should be used in PA and development of an acceptable procedure to incorporate this expert judgment into distributions in the various parameters;"

Response

The referenced page does not exist. The answer is given assuming the question refers to page 6-3 of Volume 1.

The concern is appropriate and the DOE looks forward to discussions with the EPA in this matter.

Comment T066

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(2) the inclusion of the scenarios recommended elsewhere in these comments;"

Response

The referenced page does not exist. The following response is given assuming the question refers to page 6-3 of Volume 1.

Scenario development is an ongoing process and the suggested events will be considered for inclusion.

Comment T067

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(3) the use of plausible radionuclide inventories (including radionuclide composition and specific activity) and their uncertainty in the performance assessment. (To date, the inventory has not been treated as an uncertain variable, even though the PA states that uncertainty in this inventory is large [Volume 2, page 2-51])."

Response

The referenced page does not exist. The following response is given assuming the question refers to page 6-3 of Volume 1.

Because 40 CFR 191 sets limits on the probability of radionuclide releases that have been normalized to the total transuranic inventory in the system, performance is not likely to be strongly sensitive to uncertainty in the radionuclide. This observation has not been tested by formal sensitivity analyses, but can be partially supported by comparison of the 1991 and 1992 preliminary PAs, which used different inventories. Uncertainty in the radionuclide inventory will be considered in future evaluations.

Comment T068

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 2 Page 3-18, (Line 5)

"Is temperature a variable in BRAGFLO?"

Response

Temperature is not treated as an unknown variable -- the repository is assumed to be isothermal. Temperature is used to calculate the physical properties of the fluid so temperature is a parameter, i.e., a value assumed constant for the purposes of computation.

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T069

Page 14: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 2, Chapter 2, Page 2-38, Line 12

"How will the 'location and amount of future' groundwater flow be dealt with in future PAs? How will DOE deal with such vast uncertainties?"

Response

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of compliance to this issue.

Comment T070

Page 14: I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids

Area in document Volume 2, Chapter 2, Page 2-34

(a) "What is the explanation for the wells with the low TDS?"

(b) "What is the TDS level in all the potential aquifers?"

Response

(a) Hypotheses concerning the possible lack of consistency between inferred north-to south flow in the Culebra and hydrochemical facies are discussed on page 2-36, lines 11 - 20. Also see response to the comment from the EPA document, page 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

(b) For this response, "potential aquifers" is assumed to mean Potential Underground Sources of Drinking Water, as defined in Subpart C to 40 CFR Part 191, in strata above the Salado Formation. The principal stratigraphic units known to be able to produce sufficient amounts of water are the Culebra and Magenta dolomites. The TDS of waters in these units is summarized on page 2-34, lines 24 - 31 in Volume 2 of the 1992 PA. Measured TDS values from drill holes in the vicinity of WIPP are given in Table 2-2 of Siegel et al. (1991). Furthermore, TDS values are measured routinely as part of the WIPP Groundwater Quality and Sampling Program.

As stated in a paper recently presented at the Waste Management '94 conference (Trauth et al., 1994), we propose to determine when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application). Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

References

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.G, Bertram, and B. Bower. 1994. "Considerations for Guidance for Radioactive Waste Disposal Arising from Rules Under 40 CFR 191 and 40 CFR 194," *Proceedings of Waste Management '94 Conference, Tucson, AZ, February 27-March 3, 1994.*

Comment T071

Page 14: I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids

Area in document Volume 2, Chapter 2, Page 2-34

"How much uncertainty reduction in aquifer characteristics would there be if DOE drilled more test wells near the controlled area?"

Response

The Project requests clarification of this question to insure that the response is appropriate. We are unclear whether the reviewer is concerned about: 1) the errors of estimation of total dissolved solids at unmeasured locations near the controlled area, or 2) the effect of the uncertainty in total dissolved solids on estimates of transmissivity or other aquifer properties.

The Project believes that there will always be uncertainty in aquifer characteristics, and that the acceptable level of uncertainty is related to the effect of that uncertainty on regulatory performance measures.

Comment T072

Page 14: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation

Area of document General

"Current and historical head measurements of the wells penetrating post-Salado strata would be helpful in interpreting the significance of seasonal or annual fluctuations in the reported potentiometric surface."

Response

Hydrographs for all wells at the WIPP site are presented in Hydrologic Data Reports #1-8 (Hydro Geo Chem, Inc., 1985; Intera Technologies, Inc. and Hydro Geo Chem, Inc., 1985a,b; Saulnier et al., 1987; Stensrud et al., 1988a,b; Stensrud et al., 1990), Richey (1987), and Cauffman et al. (1990). No fluctuations related to seasonal or annual cycles have ever been identified nor, given the depth and degree of confinement of Rustler Formation units at the WIPP site, are any expected to occur.

References

Hydro Geo Chem, Inc. 1985. *WIPP Hydrology Program Waste Isolation Pilot Plant, SENM Hydrologic Data Report #1.* SAND85-7206. Albuquerque, NM: Sandia National Laboratories.

Intera Technologies, Inc., and Hydro Geo Chem, Inc. 1985a. *WIPP Hydrology Program Waste Isolation Pilot Plant, Southeastern New Mexico Hydrologic Data Report #2.*

SAND85-7263. Albuquerque, NM: Sandia National Laboratories.
Intera Technologies, Inc., and Hydro Geo Chem, Inc. 1985b. *WIPP Hydrology Program Waste Isolation Pilot Plant, Southeastern New Mexico Hydrologic Data Report #3.*

SAND86-7109. Albuquerque, NM: Sandia National Laboratories.

Saulnier, G.J., Jr., G.A. Freeze, and W.A. Stensrud. 1987. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #4.* SAND86-7166. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, A.M. LaVenue, J.B. Palmer, and G.J. Saulnier, Jr. 1987. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #5.* SAND87-7125. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, T.L. Cauffman, J.B. Palmer, and G.J. Saulnier, Jr. 1988a. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #6.* SAND87-7166. Albuquerque, NM: Sandia National Laboratories. *WIPP*

Stensrud, W.A., M.A. Bame, K.D. Lantz, J.B. Palmer, and G.J. Saulnier, Jr. 1988b. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #7.* SAND88-7014. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, J.B. Palmer, and G.J. Saulnier, Jr. 1990. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #8.* SAND89-7056. Albuquerque, NM: Sandia National Laboratories.

Richey, S.F. 1987. *Water-Level Data from Wells in the Vicinity of the Waste Isolation Pilot Plant, Southeastern New Mexico.* Open-File Rpt 87-120. Albuquerque, NM: USGS.

Cauffman, T.L., A.M. LaVenue, and J.P. McCord. 1990. *Ground-Water Flow Modeling of the Culebra Dolomite, Volume II: Data Base.* SAND89-7068/2. Albuquerque, NM: Sandia National Laboratories.

Comment T073

Page 14-15: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater geochemistry

Area in document Volume 2, Chapter 2, Page 2-34

"Inferred north to south groundwater flow directions in the Culebra Dolomite (based on the potentiometric surface in the Culebra Dolomite) are inconsistent with the distribution of geochemical facies in the Culebra groundwaters (salinities decrease from north to south).

Attempts are made to explain this apparent discrepancy, but alternative working hypotheses should be explored, including the possibility that the potentiometric surface is inaccurate."

Response

The comment notes that several hypotheses have been advanced to explain the apparent discrepancy, but that additional (alternative) hypotheses should be explored. This issue affects the compliance analysis in that it relates to alternative conceptual models of groundwater flow in the Culebra. Additional work to evaluate alternative hypotheses concerning Culebra geochemistry will be considered for inclusion in the systems prioritization.

The Project will record the issue that the potentiometric surface might be inaccurate. Pending the outcome of the systems prioritization, the Project may consider this issue. It should be noted that, because of variations in fluid density, the potentiometric surface is not the best indicator of flow directions. Instead, calculated velocity fields should be used. These depend on measured values of pressure, fluid density, and transmissivity, as well as the assumptions of the model used to calculate them.

See also responses to comments in the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids); p. 18 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields); and p. 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

Comment T074

Page 15: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 2, Chapter 2, Page 2-34

"Points of groundwater recharge and discharge into post-Salado strata are very poorly constrained. Further study is needed to document these important aspects of the hydrology across the WIPP site."

Response

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of this issue to compliance.

Comment T075

Page 15: I. Technical Comments, C. General Geology and Hydrology, Hydrologic Parameters

Area in document General

(a) "It is not clear why only horizontal fractures were used in PA models for the Culebra Dolomite member (Volume 3, p. 2-85), as vertical fractures are more likely to have greater connectivity in most subsurface environments."

(b) "More data need to be acquired for all relevant stratigraphic units at the WIPP site (i.e., Castile through Dewey Lake Redbeds."

Response

(a) When modeling a formation as a dual-porosity continuum, the actual orientations of fractures are unimportant. The modeled fractures are not used to provide connections between points. The important fracture parameters to be captured in the models are the fracture porosity, which will control the flow velocity, and the surface area of the fractures, which will control the amount of matrix diffusion that occurs. As long as the fracture porosity and surface area are kept constant, it does not matter if the fractures are modeled as single sets of horizontal or vertical fractures or as three orthogonal sets of fractures. Single sets of horizontal fractures are the simplest to model, so that is what PA has used.

(b) The need for additional hydrologic data for various stratigraphic units may be considered for inclusion in the SP.

Comment T076

Page 16: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

(Summary) This paragraph follows the introductory comment that many problems associated with the transmissivity fields are related to calibration to fresh-water heads. The second paragraph points out that neglecting spatial variations in density of Culebra groundwaters could potentially cause significant errors in the calibrated transmissivity fields, as well as predicted flow directions.

Response

The transmissivity fields are actually calibrated to pressure rather than fresh-water head (Volume 2 of the 1992 PA, Section 7.5, p. 7-10, l. 14-17). However, given that the elevations of the measuring points are known, it really does not matter which parameter, pressure or fresh-water head, is used for calibration. The important point is that the flow portion of the code (SWIFT II) used for the calibration solves differential equations formulated in terms of pressure. Variations in density are fully accounted for in the code. In these calibrations, it is assumed that the density of water varies with position but is fixed in time. The evolution of the chemistry of Culebra waters is not sufficiently well understood to determine if the assumption that the density distribution does not change over long periods of time is valid.

Comment T077

Page 16: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

"...Simulations which are based on equivalent freshwater head may produce erroneous velocity magnitudes and flow directions in this critical area."

Response

See preceding response. Transmissivity fields calibration accounts for variable density. The SECO-FLOW calculations in the 1992 PA used the calibrated transmissivity fields but assumed constant fluid density. Modifications to SECO-FLOW now permit variable-density flow calculations in PA.

Comment T078

Page 16: I. Technical Comments, General Geology and Hydrology, Groundwater Transmissivity Fields

Area of document General

"The means by which the aquifer test results were incorporated as known values into the calibration of the transmissivity fields is unclear. However, the aquifer test analysis should have considered density effects on pumping responses in the monitoring wells."

Response

Transmissivities derived from single-well hydraulic tests or from interference (pumping) test responses over distances less than 50 m formed the data base for kriging of the transmissivity field. The measured transmissivities were preserved in the kriged transmissivity fields, within the estimated error bounds of the measurements. When the model domain was discretized into grid blocks, however, average values of the kriged field were calculated for and assigned to each grid block. Therefore, the average value assigned to a particular grid block need not coincide with the transmissivity determined at an individual well lying within that grid block. The process of defining transmissivity fields using aquifer-test results is discussed in LaVenue et al. (1990).

The aquifer-test analyses did take density effects into account. All analyses were done in terms of pressure changes, not water-level changes, providing results in the form of permeability-thickness products, not transmissivities. Transmissivities were then calculated based on the brine density at each location. Aquifer-test analysis procedures are discussed in Beauheim (1989).

References

LaVenue, A.M., T.L. Cauffman, and J.F. Pickens. 1990. *Ground-Water Flow Modeling of the Culebra Dolomite. Volume I: Model Calibration.* SAND89-7068/1. Albuquerque, NM: Sandia National Laboratories.

Beauheim, R.L. 1989. *Interpretation of H-11b4 Hydraulic Tests and the H-11 Multipad Pumping Test of the Culebra Dolomite at the Waste Isolation Pilot Plant (WIPP) Site.* SAND89-0536. Albuquerque, NM: Sandia National Laboratories.

Comment T079

Page 16-17: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity

Area in document **Volume 2, Chapter 2, Page 2-34**

"Transmissivity values obtained from the tests should also have been converted to hydraulic conductivities due to the assumption of a uniform thickness over the area. If transmissivity values were used as calibration points directly, they would have been in error by a factor of the effective thickness versus the assumed model thickness of 7.7 meters."

Response

The hydraulic tests within the Culebra were interpreted using the full thickness of the Culebra because the wells are fully screened across the Culebra. Given the uncertainty of the effective thickness across the site, the average thickness of the Culebra was used in the numerical model. The thickness of 7.7 m is smaller than the actual thickness in the southwestern portion of the model area where the transmissivities are the largest. However, the small difference in the conductivity that the uniform thickness assumption would make would have no appreciable difference in the model results since the transmissivity field is significantly changed through the process of calibrating the model to the measured steady-state and transient pressures.

Comment T080

Page 17: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity, Grid Sensitivity

Area in document Volume 2, Chapter 2, Page 2-34

"A sensitivity analysis on the finite-difference grid that was used to generate the 70 transmissivity fields should be performed. ...A finer grid may lead to significantly different transmissivity fields and should be evaluated."

Response

The Project has recently begun a local scale modeling effort with a much finer grid to investigate the transmissivity distribution within the near field (i.e., within the WIPP site). However, the numerical grid used in the 1992 PA flow modeling was designed to represent the regional groundwater flow surrounding the WIPP site and the transient events which have been conducted within the WIPP-site boundary. The 50 x 57 x 1 grid used in the model has larger grid blocks (e.g., 1,000 m to 2,500 m) away from the WIPP site and smaller grid blocks (e.g., 75 m to 250 m) within the WIPP-site boundary where the transient tests have been conducted. The grid resolution is believed acceptable given the objectives of the modeling study (i.e., to determine plausible regional transmissivity distributions within the Culebra).

Different grids (either coarser or finer) may lead to different transmissivity fields because the inverse procedure identifies effective or average transmissivity values at the scale of the grid. Differences in transmissivity values using grids with different resolution may reflect a scaling property of transmissivity, rather than indicating non-convergence of the inverse problem. Convergence of the travel time distribution is important, but that convergence of transmissivity estimates themselves may not be expected, and is not essential by itself.

Comment T081

Page 18: I. Technical Comments, C. General Geology and Hydrology, Culebra transmissivity, Boundary condition uncertainty

Area in document Volume 2, Chapter 2, Page 2-34

"The uncertainty with which the system boundary conditions are understood has serious implications ... the current approach that has been taken to obtain the 70 transmissivity fields does not evaluate the sensitivity of the overall model results to the boundary conditions."

Response

The Project is currently investigating this issue through the development of a three-dimensional model to assess vertical recharge into the Culebra. In addition, elicitation and examination of other conceptual models is an important part of the SP. The boundary conditions used in the 1992 Culebra flow model were estimated from regional water-level measurements and by specifying Nash Draw as a no-flow boundary condition. The south-western boundary condition has some uncertainty due to the variation in water-level measurements in this area.

Note that the horizontal boundary conditions may not produce significant changes to transmissivities within the WIPP-site boundary given the conceptual model used in 1992. This is because of the significant influence that the transient pumping tests have had upon the Culebra. These tests have stressed the Culebra to the extent that the effect of boundary conditions is small.

Comment T082

Page 18: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

(Summary) This paragraph contains two points:

> If the area along the Pecos River south of the WIPP site acts as recharge area and groundwater flow is to the east, the low ionic strength water could be dissolving dolomite, thereby creating secondary permeability.
>
> It is unclear why more emphasis in the performance assessment has not been placed on integrating the geochemical data with the hydrogeological data to form a cohesive conceptual model(s).

Response

It is possible that regions of higher transmissivity are due to dolomite dissolution. However a few points should be clarified. There is no indication that flow is presently toward the east from the Pecos River. The possibility that eastward flow occurred in the late Pleistocene has been proposed by Lambert and Carter (1987) and Lambert (1991). While dolomite dissolution might play a role, the distribution of fracture density and the degree to which gypsum and halite presently fill fractures have been proposed as more important controls on the transmissivity distribution (Holt and Powers, 1988). Circulation of low ionic strength water would likely dissolve gypsum and halite from the fractures.

The Project has placed a strong emphasis on integrating geochemical and hydrogeological data. The Siegel et al. (1991) report is an example. Additional work to integrate geochemical and hydrogeological data will be considered for inclusion in the systems prioritization.

See also responses to comments in the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids); p. 14-15 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater Geochemistry); and p. 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

References

Lambert, S.J., and J.A. Carter. 1987. *Uranium-Isotope Systematics in Groundwaters of the Rustler Formation, Northern Delaware Basin, Southeastern New Mexico. I: Principles and Preliminary Results*. SAND87-0388. Albuquerque, NM: Sandia National Laboratories.

Lambert, S.J. 1991. "Fossil Meteoric Groundwaters in the Delaware Basin of Southeastern New Mexico," *Stable Isotope Geochemistry: A Tribute to Samuel Epstein*. Eds. H.P. Taylor, Jr., J.R. O'Neil, and I.R. Kaplan. Special Publication No. 3. SAND89-2660. San Antonio, TX: Geochemical Society. 135-156.

Holt, R.M., and D.W. Powers. 1988. *Facies Variability and Post-Depositional Alteration Within the Rustler Formation in the Vicinity of the Waste Isolation Pilot Plant, Southeastern New Mexico*. DOE/WIPP-88-004. Carlsbad, NM: Westinghouse Electric Corporation.

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico*. SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T083

Page 18-19: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity, Recharge Uncertainty

Area in document Volume 2, Chapter 2, Page 2-34

"In addition to LaVenue's suggestion (SAND 92-7306) that recharge to the Culebra may be occurring in the vicinity of the Pecos River, at least one other alternative conceptual model has been proposed which also involves vertical recharge to the Culebra. This alternative model considers significant vertical recharge to the Culebra over the entire southern region of the modeled area (SAND 88-0196). In either case, if vertical recharge occurs, the 70 transmissivity fields calibrated to the aquifer tests and equivalent fresh-water heads would be lower (i.e. slower velocities) than those which would be calculated with the present model. Vertical recharge should be evaluated in the sensitivity analysis."

Response

The Project is currently investigating this issue through the development of a three-dimensional model to assess vertical recharge into the Culebra. Future modeling studies may include these estimates in the calibration process. It should be recognized that the exclusion of vertical recharge in the region upgradient of the WIPP site leads to higher transmissivity estimates and higher groundwater velocities as noted by the EPA reviewer. This is conservative from a groundwater travel time viewpoint.

Comment T084

Page 19: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences

Area in document Volume 2, Chapter 2, Page 2-36, Lines 11-20

"What is being done to examine the differences between the north-to south flow in the Culebra and the hydrogeochemical facies data? How does this discrepancy impact the confidence of the flow modeling?"

Response

It should be emphasized that it is not certain that a discrepancy between flow directions and hydrochemical facies actually exists. The apparent discrepancy might instead be due an incomplete understanding of the hydrogeochemical system.

Several hypotheses have been advanced to explain the apparent discrepancy, as referenced in lines 11-20 in Volume 2 of the 1992 PA. Also, lines 32-35 on page 2-33 and lines 1-3 on page 2-36 reference the strong correlation between the region of high molality sodium-chloride water and the presence of halite in adjacent strata (see Figure 1-13 in Siegel et al., 1991). It is anticipated that a three-dimensional computer model of regional groundwater flow that is being developed (EPP study 5.1.1.2, p. 5-8) will provide information that could be used to evaluate and/or modify the existing hypotheses. Additional work to integrate hypotheses concerning Culebra geochemistry with the results of the regional model will be considered in the systems prioritization.

This apparent discrepancy affects confidence in the flow modeling in that it suggests the need to consider alternative conceptual models of groundwater flow in the Culebra. Consideration of alternative conceptual models is an objective of the SP.

See also responses to comments from the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology); p. 14-15 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater geochemistry); p. 15 (I. Technical Comments, C. General Geology and Hydrology); and p. 18 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields).

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T085

Page 19: I. Technical Comments, C. General Geology and Hydrology, Fracture Density

Area of document Volume 2, Chapter 2, Page 2-19

"Since good information on Culebra fracture density is lacking, please explain why you do not use the higher fracture density where it can be observed."

Response

As the comment states, good information on Culebra fracture density is lacking. It is not clear what is meant by "use the higher fracture density where it can be observed" because fracture density can't be observed in the subsurface except in shafts (Holt and Powers, 1990). Also, all fractures, whether observed or not, are not hydraulically significant. Through tracer tests, we seek to determine the fracture density that is important for transport through the Culebra (see Jones et al., 1992). The interpreted "effective" fracture density is always less than observed fracture densities. Were we to use observed fracture densities in our models, much more physical retardation would occur as a result of matrix diffusion than our tracer tests show to be realistic.

References

Holt, R.M., and D.W. Powers. 1990. *Geologic Mapping of the Air Intake Shaft at the Waste Isolation Pilot Plant.* DOE/WIPP-90-051. Carlsbad, NM: Westinghouse Electric Corporation.

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra Dolomite at the Waste Isolation Pilot Plant Site.* SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Comment T086

Page 19: I. Technical Comments, C. General Geology and Hydrology, Vertical Fractures

Area of document Volume 2, Chapter 2, Page 2-19

"In volume 3 (1-39) it states that vertical fractures in the Culebra are not used in the calculations. Why? How are vertical fractures handled or what assumptions are used?"

Response

When modeling a formation as a double-porosity continuum, the actual orientations of fractures are unimportant. The modeled fractures are not used to provide connections between points. The important fracture parameters to be captured in the models are the fracture porosity, which will control the flow velocity, and the surface area of the fractures, which will control the amount of matrix diffusion that occurs. As long as the fracture porosity and surface area are kept constant, it does not matter if the fractures are modeled as single sets of horizontal or vertical fractures or as three orthogonal sets of fractures. Single sets of horizontal fractures are the simplest to model, so that is what PA has used.

Comment T087

Page 20: I. Technical Comments, C. General Geology and Hydrology, Effects of the Magenta Dolomite in Transport Calculations

Area in document Volume 2, Chapter 2, Page 2-23 to 2-24

"Most of the focus is on the Culebra for transport because it has the highest transmissivity. What is the effect on the release when the Magenta and Culebra Dolomites are combined in the calculations?"

Response

The Project is using a three-dimensional computer model of regional groundwater flow (EPP Study 5.1.1.2, p. 5-8) to evaluate the sensitivity of releases into other hydrologic units to compliance.

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of this issue to compliance.

Comment T088

Page 20: I. Technical Comments, C. General Geology and Hydrology, Use of Crushed Culebra Rock

Area in document Volume 2, Chapter 2, Page 2-40/41

"Please expand the discussion on [*how*] the tests involving crushed Culebra rock will help determine K_ds? Is this to simulate the effect of clays? If many of the fractures are clay lined, are any tests being conducted on the clay?"

Response

Early laboratory investigations of chemical retardation in the Rustler Formation were conducted using traditional batch sorption experiments with crushed rock. The early experiments were largely intended to be scoping experiments to ascertain whether chemical retardation was significant and they were conducted under specific experimental conditions. Results of those experiments are expected to be valuable, however, in providing independent checks on results from the present experimental approaches (see, for example, Lynch and Dosch [1980] and the review by Novak [1992]). Currently, mechanistic adsorption experiments coupled with surface complexation modeling are in progress on very carefully prepared ground mineral constituents of the Culebra, dolomite and corrensite, the primary clay mineral constituent. The resulting surface complexation model will predict values for K_d as a function of mineralogy, fluid composition, and adsorbates. On the basis of published information in peer-reviewed journals (see also Siegel et al., 1990), we have strong evidence that results from mechanistic experiments will be representative of the phenomena occurring in the intact Culebra rock. To confirm this, comparisons will be made with the column experiments and sorption experiments with thin slabs or ground samples of Culebra rock.

References

Lynch, A.W., and R.G. Dosch. 1980. *Sorption Coefficients for Radionuclides on Samples from the Water-Bearing Magenta and Culebra Members of the Rustler Formation.* SAND80-1064. Albuquerque, NM: Sandia National Laboratories.

Novak, C.F. 1992. *An Evaluation of Radionuclide Batch Sorption Data on Culebra Dolomite for Aqueous Compositions Relevant to the Human Intrusion Scenario for the Waste Isolation Pilot Plant.* SAND91-1299. Albuquerque, NM: Sandia National Laboratories.

Siegel, M.D., J.O. Leckie, S.W. Park, S.L. Phillips, and T. Sewards. 1990. "Studies of Radionuclide Sorption by Clays in the Culebra Dolomite at the WIPP Site, Southeastern New Mexico," *Waste Management '90, Waste Processing, Transportation, Storage and Disposal, Technical Programs and Public Education, Tucson, AZ, February 25-March 1, 1990.* Ed. R.G. Post. SAND89-2387. Tucson, AZ: University of Arizona. Vol. 2, 893-900.

Comment T089

Page 20: I. Technical Comments, B. Additional General Comments, Groundwater Age Discussion

Area in document Volume 2., Chapter 2, Page 2-37, Lines 22-32

"The section on the isotopic and tritium data is confusing. DOE states that tritium levels indicate minimal contributions from the atmosphere since 1950. If the waters have even 'minimal' contributions, then that would indicate the waters are not that old. Please clarify what is meant. In addition, what is being done to resolve uncertainty about groundwater age?"

Response

For a discussion of the isotopic data, including tritium, see Chapter 5 of Siegel et al. (1991) and the references cited therein. The statement quoted from Volume 2 of the 1992 PA is consistent with the observed data. Meaningful nonzero tritium measurements suggest either contamination during drilling or sampling or some degree of mixing with modern surface water in the hydrostratigraphic unit. For the WIPP, these measurements indicate that the amount of mixing has been either zero or extremely small. As stated in the text in Volume 2, this in turn suggests that groundwater travel time (i.e., the mean travel time) from the surface to the sampled units is long. The presence of some "younger" water from the surface does not imply that all water is "young," nor does it imply that recharge is rapid.

Effects on disposal-system performance about the rates of vertical flow, and therefore about groundwater age, are being examined through regional 3D modeling.

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T090

Page 21: I. Technical Comments, C. General Geology and Hydrology, Porosity of Anhydrite Interbeds

Area in document Volume 2, Chapter 2, Page 2-42, Lines 15-21

"Are the porosities of the anhydrite interbeds matrix porosity, fracture porosity or both? Will DOE assume that anhydrite fracturing continues to the accessible environment?"

Response

Porosities for the anhydrite reference in the cited area of Volume 2 are presented in Volume 3, Chapter 2, p. 2-65 of the 1992 PA. The porosities given are estimates, not measurements, and are intended to represent the total porosity of the anhydrite. No distinction between fracture and matrix porosity is necessary because PA treats the anhydrite as a porous, not fractured, medium. A laboratory program is underway to measure anhydrite porosity, both in an unloaded state and as a function of stress (Howarth, 1994).

PA assumes that the fracturing naturally present in the anhydrites continues to the accessible environment. The 1992 PA did not explicitly include induced fracturing outside of the disturbed rock zone around the repository. It is planned that future PAs will include a relationship between pressure in the fractures and fracture porosity and permeability. Whether or not the pressure in the fractures, and hence the porosity and permeability, changes at the accessible environment boundary will depend on the conditions in the model.

Reference

Howarth, S.M. 1994. *Test Plan: Two-Phase Flow Laboratory Program for the Waste Isolation Pilot Plant (WIPP).* Albuquerque, NM: Sandia National Laboratories.

Comment T091

Page 21: I. Technical Comments, C. General Geology and Hydrology, Average Undisturbed Pore Pressure in Anhydrite

Area in document Volume 3, Chapter 2, Page 2-63

"It is unclear how the average undisturbed pore pressure in the anhydrite was developed. Table 2.3-2 (p 2-33) does not have any measurements greater than 9.5 MPa, yet the mean and median pressures are between 12 and 13 MPa."

Response

Table 2.3-2 shows only halite pore pressure data. Anhydrite data are presented in Table 2.4-2 on p. 2-59. The three values between 12.4 and 12.6 MPa are considered to provide the best representation of anhydrite pore pressures undisturbed by the excavations.

Comment T092

Page 21: I. Technical Comments, C. General Geology and Hydrology, Culebra Matrix Porosity

Area in document Volume 3, Chapter 2, Page 2-83

"If only intact rock was measured for porosity and there was a large amount of core lost in porous (vuggy) and/or fractured portions of the Culebra, is the matrix porosity used in the models: 1) adjusted upward to reflect the higher bulk matrix porosity; or 2) is the data indicating the vugs and fracturing considered as part of the fractured porosity? It is not clear if the Culebra matrix porosity values include these features or not."

Response

The matrix porosity used in the models has not been adjusted to compensate for unsampled, presumably higher porosity, portions of the Culebra. The data include vugs and fractures only to the extent that they were present in the core samples tested. Both fractures and vugs could be included in the "fracture" porosities determined from tracer tests (e.g., Jones et al., 1992), which represent what might be called the effective flow porosity of the Culebra. The matrix porosity might, in turn, be called the diffusion porosity. If matrix porosity is being underestimated, the potential for matrix diffusion is also being underestimated. See also response to comment in EPA's document, p. 25 (I. Technical Comments, D. Additional Comments on General Geology and Hydrology).

Reference

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra Dolomite at the Waste Isolation Pilot Plant Site.* SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Comment T093

Page 22: I. Technical Comments, C. General Geology and Hydrology, Data on pH and Eh

Area in document Volume 3, Chapter 3, Page 3-41

"What are the current data or expected values of the pH and Eh in the Culebra under existing conditions? If the data exist, they could not be found in the PA."

Response

Ranges for pH conditions in the Culebra Dolomite have been well defined and range between about 6.5 and 8.0 (see Siegel, 1991, Chapter 2). The pH of the Culebra is expected to be narrowly constrained because of the tremendous buffering capacity of carbonate minerals. Any artificially induced perturbation in pH would rapidly be eliminated by dissolution/precipitation reactions.

Ranges for Eh in Culebra Dolomite groundwaters were investigated by Myers et al. (see Siegel, 1991, Chapter 6 and Appendix 6A). Myers et al. attempted to characterize Eh by evaluating four redox couples (As, N, I, and Se) with measurements with a platinum electrode (refer to Table 6-4 and Figure 6-2 of Siegel, 1991). Unfortunately, many of the measurements for individual redox species were below the analytical detection limits. Consequently, Myers et al. were only able to bound the Eh conditions and were not able to decisively quantify values. Myers et al. did speculate that groundwaters south of the site boundary are more oxidizing relative to groundwaters to the north (see Figure 6-1 and discussion on p. 6-22 of Siegel, 1991).

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T094

Page 22: I. Technical Comments. D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-6 (Figure 2-3)

"The generalized stratigraphy of the Delaware Basin is inaccurate on several accounts.

- Castile formation onlaps the terminal platform margin of the Capitan Limestone and extends further onto inner parts of the Capitan platform. Figure 2-3 shows the top of the Castile formation being located stratigraphically lower than the top of the Capitan Limestone. Figure 2-5 on p. 2-8 more accurately depicts the relationship of the Capitan and Castile formations.

- The Dewey Lake Red Beds are Ochoan in age and should be included in this stage, along with the Castile, Salado, and Rustler formations."

Response

Both points in the comment are correct as stated by the EPA reviewer. Editorial corrections in future documents are appropriate.

Comment T095

Page 22: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-10 (Section 2.2.2.2)

"No mention is made of the wide variety of depositional facies that actually comprise the Capitan Limestone. A potentially important lithofacies, at least with regard to the hydrologic characteristics of the Capitan Limestone, is the forereef or foreslope facies, which consists of poorly sorted carbonate clasts shed from the high relief Capitan margin. This facies is poorly mapped, may have very different flow characteristics than for other facies in the Capitan Limestone, and tongues of this facies may extend close to or beneath the WIPP site."

Response

The first sentence of the comment is plainly correct. We are unaware of evidence for forereef deposits extending 10 to 15 km into the basin for very different flow characteristics within these deposits. We would be willing to discuss this topic with the EPA during technical exchange meetings.

Comment T096

Page 22-23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-10, Line 35

"'Lateral variations in depositional environments (in the Culebra Dolomite) were small within the mapped region...' What is the evidence for this statement? Detailed lithologic columns for the Culebra Dolomite with lithologies and sedimentary structures should be shown."

Response

Additional detail in the level of referencing will be included as appropriate in the PTB/draft compliance application. It is not clear that inclusion of stratigraphic columns is relevant to compliance, except along possible radionuclide-release paths.

Page 5-11 of Holt and Powers (1988) states: "The bulk of the Culebra is microlaminated to thinly laminated. The strata may be flat to wavy to locally contorted and discontinuous. Portions of the Culebra appear macroscopically devoid of depositional fabric. The dolomite is mottled in some zones. With the exception of the upper and lower contact zones, there is very little variation of depositional sedimentary features throughout most of the Culebra." Additional detail from the same page includes: "The lowermost foot (30 cm) of the Culebra locally is thinly laminated to laminated, with alternating light and dark brown laminae." "The uppermost few inches to 1 ft (30 cm) of the Culebra often differs radically from the underlying dolomite. The gamma ray signature of this zone is unique and is present throughout the Delaware Basin." We agree that the detail of referencing needs to be increased; generalized referencing in important areas, is inappropriate.

A large number of detailed correlations, cross-sections, and stratigraphic columns of the Rustler Formation are contained in Holt and Powers (1988).

use of conservative hypotheses). These curves are similar to the CCDFs generated by Sandia for WIPP.

How the results (and the uncertainties) should be incorporated in the regulator's decision making is discussed in details in my answer to the fifth question. This answer is based on the assuption that the aggregation of expert opinions will be done systematically for all fundamental assumptions, and that the resulting distributions will be integrated in the risk analysis. Otherwise (e.g., if the disagreement is simply represented by a set of consequence distribution, one per expert), I do not know how to recommend to a decision maker to systematically treat a collection of results, or the results of a conditional risk analysis based on unweighted assumptions. It becomes a matter of faith in the conservatism of the assumptions.

Q3. SELECTION OF INPUT VARIABLES THAT REQUIRE PROBABILISTIC TREATMENT: SENSITIVITY ANALYSIS

It is not necessary in many risk analysis problems to put a probability distribution on all variables. In the decision analysis cycle (Ref. Howard), the first step is to develop models by a deterministic analysis of the link between the consequences and the input variables. Second, a sensitivity analysis for each variable reveals whether or not the variation of an input value across the possible range can change by itself the final decision. Third, the probabilistic analysis is performed: for the variables that do not require full treatment of uncertainty, the mean value is encoded and included in the model. For the variables that do require a probability distribution, this distribution is encoded as described above. The uncertainties are then "propagated" through the analysis by different methods (closed-form solutions, relevant moments, logic/event trees, or simulation, for example, using Monte Carlo or Latin Hypercube sampling).

Q4. DEVELOPMENT OF PROBABILITY DISTRIBUTIONS

Incorporating *all* uncertainties is a risk analysis is indeed a challenge. Therefore, it is important to proceed first to the sensitivity analysis discussed above so as not to lose sight of the ultimate goal (to support a specific decision).

The development of probability distributions is currently a hot topic within the EPA and the environmental/health risk analysis community. [Note, however, that for many years, it has been done systematically for industrial facilities such as nuclear power plants]. Because of the controversial nature of the treatment of epistemic uncertainties by Bayesian probabilities, the solution is often to do only what I consider a partial uncertainty analysis, focusing on randomness in statistical samples and on distributions for the variables explicitly included in the model. The default solution is thus to focus on randomness and on *some* epistemic uncertainties.

There is seldom any attempt to quantify systematically the epistemic uncertainties (about partially known fundamental phenomena) because it requires quantifying explicitly the probabilities of alternative assumptions and, in order to do that, proceeding to an aggregation of expert opinions. For example, in a recent expert-based study of global climate change, Granger Morgan chose to simply present the range of results for each of the different experts without any attempt to come up with a composite distribution. I personally believe that one cannot escape this full uncertainty analysis (i.e., to include the probabilities of alternative hypotheses). Otherwise, the problem is exactly the one that you are facing with WIPP: how to judge of the degree of conservatism of a conditional risk analysis without looking at the conservatism of the hypotheses.

The structure of a full uncertainty analysis is thus the following:

1. Structuring of the different hypotheses into sets of alternative realizations so that probability distributions can be

attributed to these sets of assumptions.

2. Encoding and aggregation of expert probabilities for each set of assumptions.

3. For each fundamental hypothesis, identification of the subsequent models and parameter values (probabilistic treatment). Conditional risk analyses of the type performed by Sandia, but one for each possibility (e.g., each H1i in Figure 1) in a complete set of assumptions, including a measure of possible dependencies through conditional probabilities.

4. Propagation of all relevant uncertainties for each hypothesis (the results are the sets of risk curves shown in Figure 1 for each realization of a given hypothesis).

5. Summing of the results of the conditional analyses weighted by the probabilities of the fundamental underlying assumptions (one then obtains an overall set of risk curves like those presented at the bottom of Figure 1).

(Alternatively, the overall set of risk curves can be obtained directly through the use of a logic tree).

Again, there are different methods for the propagation of uncertainties through each model: closed-form solutions (which is sometimes possible, for example, to treat lognormal distributions and products of variables), computation of the relevant moments, use of logic (event) trees that layout all possible combinations of hypotheses, models, and parameter values, or full simulation (by various methods including Monte Carlo and Latin Hypercube sampling)

Q5. COMPLIANCE CRITERIA GIVEN A FAMILY OF RISK CURVES

How this full uncertainty analysis is used by the decision maker (DM) is a function of his or her own preferences (including risk attitude). Therefore, it is by nature subjective. The consistency of the process, however, can be treated somehow objectively.

For individual decisions, these preferences are represented by a utility function that allows representing risk aversion by putting higher weights (than linear functions would) on the possibility of higher losses. Note that by virtue of the axioms of rationality for individual decisions, it is the *mean future frequency* that is the relevant characteristic of the probability distribution for the future frequency of the potential loss levels (in the WIPP case: the release level as an intermediate descriptor, but more importantly, the helth effects).

This rationality paradigm does not apply to collective decisions, except if one assumes that one elected decision maker (administrator) has been given complete power to make these decisions according to his or her utility function (which, presumably, would have to be revealed if it were to be used in an analytical model). This is impractical because it does not fit our political process and because there are many attributes to each decision that would require some adaptation of any revealed preferences.

The administrator is not only concerned about the probability distribution of the levels of release and about the economic costs of release (for which mean future frequencies would theoretically suffice), but also about the health and safety of the most exposed individuals in the public. The choice of a threshold and the way one demonstrates that it has not been exceeded should reflect directly a concern for prudence. The mean may or may not do that depending on the fractile(s) that it represents in the family of risk curves, and the practicality of demonstrating by analytical means that the goal has been achieved.

I would like, at this point, to go back to what I wrote in my 1986 paper:

> " The next question is to ensure that the goals have been satisfied with "reasonable certainty". A common procedure is to use "conservative estimates" at every step which means to overestimate the probabilities of initiating events, failures, accidents, etc. The overestimation of the

> final result, however, is impossible to assess. It is a wrong approach that may lead to absurd figures and quite possibly to suboptimal decisions, thus defeating the pupose of conservatism itself. This is why the analysis of uncertainties and their explicit treatment in the final decision are critical.
>
> Once this analysis has been done, safety decisions must be made to ensure that with a high probability (e.g., 0.95) the plant is in compliance with a the maximum acceptable individual risk constraint and with the maximum allowable frequency of failure. There is no compelling theoretical reason to use one fractile or a mean value rather than another criterion. In a framework involving numerical safety goals, this certainty level must be specified by the U.S. NRC along with the safety goal"

The example that I was using was safety of nuclear reactors for which the time horizon is relatively short and the uncertainties can be approached systematically. Therefore, the Probabilistic Risk Analyses that are performed for these plants do not involve the types of uncertainties faced with WIPP. Hence the possibility of "reasonable certainty" (which the USNRC calls "reasonable assurance"). In the case of WIPP, part of the analysis (the EPA linkage of release and health effects) is non-probabilistic and presumably, based on conservative modeling. Therefore, given the time frame and the level of uncertainties (e.g., about the future of civilizations in the next 10,000 years), the chosen approach has been different: to start with a set of preliminary results and framing hypotheses, then do a conditional performance analysis based on a mixed method (probabilistic and pre-set health effects estimates). First, one cannot judge directly which fractile(s) the mean curves of the future release levels would actually represent if Sandia had included in the analysis (1) the presumably conservative hypotheses that EPA had specified (complete with alternative assumptions and their probabilities), and (2) the uncertainties attached to the hypotheses that they generated themselves. Second, one cannot derive from this analysis a probabilistic distribution for the health effects. The problem is that a full risk analysis of this type would be extremely difficult given the state of the art, and that the uncertainties over the next 10,000 years would be so large that the results may not be very informative.

In this highly uncertain, long-term case, I believe that the approach based on some fixed hypotheses, *then* on "reasonable expectations" for the conditional risk results is generally sensible provided (1) that the hypotheses are globally conservative (health effects given release as well as assumptions in the release computation) and (2) that the mean curves for the release of the different radionucleides generally correspond to high fractiles of the risk curve families (CCDFs). If that is the case, the combination of *hypotheses* and *means* may indeed provide the level of "reasonable assurance" that you wish and that is consistent with the USNRC requirements for much shorter life facilities. To check that the overall analysis is "globally conservative" you need to verify that the global model (Health Effects + Performance Assessment) yields conservative results and in particular that the hypothetical health risk results that would have come out of a fully integrated analysis meet the level of "reasonable assurance" that you want to see. This requires that the combination of the health effect model and the Sandia hypotheses provides a higher level of safety than the one demonstrated by the position of the PA mean curves in the PA alone.

Therefore, you may want to examine the effects of hypotheses on the position of the *current means* in the family of CCDFs (fractiles) for release accounting for the EPA/DoE hypotheses as shown in Figure 1. Of course, you do not want to ask Sandia to redo the whole uncertainty analysis, but to give you a feeling for the final degree of conservatism of the release results after this accumulation of assumptions. This involves listing the main hypotheses (both from EPA and from the Sandia PA) and assessing (even coarsely) their cumulative effects on the position of conditional (current) means in the CCDFs families. If it is the case that the EPA/DoE assumptions are generally *conservative*, it is likely that what are now mean curves in the current conditional performance analysis (Sandia's PA) would correspond to higher fractiles of distributions that would account probabilistically for all hypotheses (Figure 1, bottom left). If the set of assumptions turns out to be altogether *unconservative*, introducing alternative assumptions will tend to make the current means go down in the families of risk curves towards lower fractiles (Figure 1, bottom right).

When you receive this information about the probabilities and the effects of alternatives to the main hypotheses on the position of the mean curves, you want to examine whether the *final* levels of fractiles that would correspond to the *current means* meet the level of conservatism that you want. You may also want to go one step further and look closely at the health effects themselves and at the conservatism of the EPA model of cancer risk. I do not believe that at that stage it would be realistic to require EPA to proceed to a full probabilistic risk assessment (they do not have the methods as far as I know). Yet, you can argue that their "reasonable expectations" are reasonable only if their hypotheses and health effects model provide the additional level of safety that is consistent with the NRC language of "reasonable assurance". In other terms, first their current means for the release of the different radionucleides have to provide at least as much safety as the overall "expected value" of the release that one would from a probabilistic analysis of the hypotheses. Second, the EPA health effect model should provide an additional layer of safety that convinces you that you are indeed in the high fractiles of a hypothetical full risk analysis.

Should you push EPA to specify a fractile level applicable across the board to all cases? I don't believe so, simply because each problem has to be replaced in its context (uncertainties, existence of alternatives, economic and political context, etc.). I believe, however, that examining carefully the range of fractiles corresponding to the mean in the consequence distribution is a reasonable way to address the question of uncertainties. In the WIPP case, the choice of the mean conditional on a set of hypotheses was based on the long-term nature of the project, the fact that the computation of the mean is more robust than that of specified fractiles, and that the means (given the uncertainties) are likely to be among the high-fractiles anyway. And in any case, requiring the EPA to make a general statement about a "high level of confidence" in the final health effects analysis including all uncertainties would be helpful.

<u>Regulatory language</u>

I think that you can require that EPA be more rigorous in its implementation of the "reasonable expectation" language. They cannot just set hypotheses and models (as those leading to Figure 1, Appendix A of 40CFR191), frame the conditional risk analysis for the applicant, then claim without checking that the conditional means (even with infinite sampling size) resulting from this analysis necessarily support "reasonable expectation" of human safety. Whereas it may be unreasonable (and perhaps, even hazardous given how uncertain the results would be) to leave the choice of hypotheses and model framing to the applicant, it is not unreasonable to require that the effects of these hypotheses on the mean curves be assessed (i.e., simply to check how they displace the mean curve: up or down). In the WIPP case, I would focus on the hypotheses of the intrusion model (frequency, means and effects of drilling) which are the most likely to significantly affect the release results. I would also examine very closely the EPA health effect model.

I would want EPA to show that, in the end, the combination of "reasonable expectation" for the performance assessment and of the conservatism (if it is the case) of the health effect model that they have used to set the release criteria provides "reasonable assurance" of actual safety (i.e., for the ultimate health effects). Because EPA has done the health effects modeling, they are in a good position to show the conservatism of their own results and of the final health risks when these results are combined with those of the performance assessment.

Therefore, you want to require EPA:
(1) to fully reveal the models that they have used to come up with the release standards,
(2) to list all the major assumptions that they have made (those that are likely to affect the risk analysis results), (3) then, to ask the applicants to show that the combination of these models, hypotheses and their own performance analysis supports the requirement that the current conditional mean is indeed "above" the marginal (overall) mean, and that altogether, the assumptions are in fact "conservative".

By comparison, the uncertainties that result from the sampling are probably (1) cheap to reduce and (2) not very significant compared to effects of the basic hypotheses. Therefore, you may choose either to accept their 95% confidence language, or to require a third level of confidence in the analysis. I do not think that it will make much difference.

3. ADDITIONAL COMMENTS ON ISSUES RAISED IN YOUR LETTER

3.1 Level of confidence in the fractiles (or mean) given the sampling size

This issue is easy to resolve because it is cheap to require additional computer runs if you do not think that the level of confidence achieved is what you want. Of course, the tail of the distribution will not be often reached in the simulation by definition of high consequence/low probability modeling. You may want to press EPA to specify the confidence level in this process (third order treatment of uncertainty, i.e., one level further than what I describe as Level 5 in Figure 2 of the Appendix). But you have to realize that the results will be somehow artificial given the variety of the sources of uncertainties. So, I would not focus so much on the uncertainties due to sampling size because they are

probably "in the noise", as I would on the uncertainties about the fundamental hypotheses.

3.2 Encoding of expert opinions

I agree that you may want EPA to specify better their encoding procedures. Anyway, in the case of WIPP, you want to find out how Sandia exactly did it (especially for parameter values).

3.3 Use of the mean

I generally agree with EPA that the mean *does* convey "a sense of the whole ensemble of the CCDF's generated". It represents an aggregated description of the risk by a single probability distribution (Level 4 of Figure 2) without displaying the higher level of uncertainties (Level 5 in Figure 2). I do not believe, as you do, that the applicant can vary the number of realizations and dilute at will the effects of *any particular CCDF*. What is true, however, is that with a small number of realizations (in the simulation) one may not reach the tail of the distribution. You want Sandia to specify case by case what level of assurance the mean represents (it varies, of course, along the release axis).

3.4 Additional comments

a. Specific guidance for the form of probability distribution functions seems to me impractical.

b. Need to deal with correlations: I agree, this is essential.

c. Appropriateness of the mean: in the case of WIPP, I think that the coupling of EPA assumptions (*if* they are globally conservative) and mean release level (which is likely to be among the high fractiles given the uncertainties) should provide the level of safety that you want. This is what you want Sandia to demonstrate.

d. Calling explicitly for a 95% fractile with 99% confidence would require a full probabilistic treatment of all EPA/DoE hypotheses regarding the release, introducing still more uncertainties in the analysis and probably producing highly questionable results. [I would not suggest this kind of fractile *on top* of the EPA hypotheses.] Again, I would start by checking what the current mean represents (roughly) in the full picture. To call for the 95% fractile of the *real risk curves (i.e., the health effects)*, would require a whole new risk analysis including both the release model and the health effect model. It is obviously not the direction that was chosen *a priori*.

f. Of course, the process of sampling of 50 parameters, even with an infinite sampling size would dilute the effects of the extremes. It is the nature of probability: the extremes are much less likely than the central range of the distribution. But you want sufficient sample size to have confidence that you have given the extremes their proper weight.

h. Reducing uncertainties can be done in many different ways. Increasing the sampling size of course is one of them; but again, these uncertainties are probably minor compared with the uncertainties involved in the fundamental assumptions.

j. No, it is not easy to identify the various percentiles of crossed curves. Indeed, any mean curve will represent different percentiles in different release ranges.

4. CONCLUSIONS:

I believe that the case of WIPP as it stands now raises issues that are different from those that I addressed in my 1986 article regarding nuclear power plants. But the fundamental concern is the same: reaching an acceptable level of safety with reasonable certainty (or assurance). In the 1986 article, I proposed to do it using high fractiles of the risk curves (which is often where the means are anyway) based on full PRAs including the treatment of all identified and relevant uncertainties (as determined by sensitivity analysis). For WIPP, we do not have risk curves (in the sense of full probability distributions for the consequences,i.e., the health effects). Because of the 10,000-year time horizon, the uncertainties in the case of WIPP are such that this kind of analysis may be a futile exercise. Instead, EPA has chosen to make some assumptions in its performance criteria and to require a conditional performance analysis given these assumptions. Then, EPA specified the use of the conditional means as the basis for the compliance criteria.

In order to verify that the conditional means (conditional on specified health models and hypotheses) provide indeed "reasonable expectation" of safety once the effects of the hypotheses on expected values are carefully considered, you want to ask Sandia to provide additional information about what these conditional means really represent for future release and what they imply for human safety. In particular, you want to question assumptions regarding engineered barriers and the hypotheses that have been made to support the currently planned storage system. This is where you may be able to show that some of the assumptions are unconservative and that the real mean curves are below the conditional ones. Therefore, you may be able to conclude that the current analysis based on conditional means does not meet, on the whole, the "reasonable expectation" standard. I would not focus much on the effect of the sampling size (although it probably does not cost much) because increasing it may not provide large variations of the position of the mean in the overall CCDF family. The hypotheses about the frequency, the means and the effects of drilling are more likely to provide significant variations.

To summarize my conclusions:

4.1 I do not know where the current means stand in terms of fractiles on the distribution of release curves presented by Sandia.
=> You may consider asking Sandia to specify which fractiles are involved in the mean release curves that are presented in their final PA report (these fractiles will vary along the release axis; but Sandia may be able to bracket them).

4.2 I cannot judge the degree of conservatism of the Performance Assessment results because I do not know the effects of the EPA and DoE hypotheses on the release curves.
=> Ask Sandia to list the major hypotheses that have been taken for granted in their PA and to give you an idea (if not a full analysis) of the effects of these hypotheses on the results (i.e., the family of release curves). For example take the five or six most important assumptions of the PA (e.g., the Poisson model of human intrusions,

the diameter of the bore holes, the water flow model, the solubility factor of the main nucleides, etc). Ask Sandia to generate a set of reasonable alternatives to these hypothese and to show you that the mean curves that would be generated with proper probabilistic analysis of these alternative assumptions actually meet the criteria (and that they do not pull the means towards lower fractiles of the risk curve families).

4.3 The expert opinion procedures of encoding could be made more rigorous.
=> You may want to ask Sandia to identify the variables whose distributions are critical for the results (could make WIPP violate the performance criteria), to justify their decision to treat them through expert opinions (as opposed to experiments or measurements when feasible), to better justify their findings by describing exactly how they have encoded and aggregated expert opinions, or to redo the encoding and aggregation of these judgments if you conclude that some of the variables have nor been properly treated.

4.4 The uncertainties about WIPP are such that full probabilistic treatment of all assumptions is likely to introduce large additional uncertainties in the results if they were to be systematically treated through probabilities.
=> You may want to find out what is the level of release risk obtained given the combination of EPA and DoE assumptions and the results of the corresponding conditional risk analysis, judge whether it is reasonable, and if it is not, ask EPA to reveal how it is going to inject additional levels of prudence in its decision. Depending on how far the current means are (assuming full probabilistic treatment of hypotheses) from a reassuring (but not sacred) 95% fractile, you may want to ask for additional analysis or for a change of risk management strategy.

4.5 If you really want to estimate the long-term health risks associated with the possibility of release, you need a probabilistic version of the EPA health effect model and a true risk analysis involving both release and health effects. I doubt that this is feasible. But:

=> You may want to ask EPA to better justify what they have done to obtain Table 1 of 40CFR191 and DoE to show that the overall risk results (their model plus the PA) provide "reasonable assurance" of safety.

5. APPENDIX

[What follows on this topic is based on a report that I recently wrote for the Electric Power Research Institute].

Six levels of treatment of uncertainties in risk analysis:

The form under which one would like uncertainty analysis to be done depends in large part on the use that one intends to make of the results, i.e., what criteria will apply in the decision making. All decisions do not need full treatment of uncertainties. Different degrees of sophistication in the assessment of the risks can be envisioned depending on the management rule that one intends to apply. Six different levels in the treatment of uncertainty (see Figure 2) can be identified.

Level 0 simply involves the detection of a potential hazard without attempt to assess the risk in any way. It is sufficient, in theory, to support strict zero-risk policies, or to make risk management decisions when the costs are low.

Level 1 is the "worst-case" approach. It does not involve any notion of probability. It is based on the accumulation of worst-case assumptions and yields, in theory, the maximum loss level. In practice, however, whatever the worst-case *scenario* that has been constructed, it is often possible to imagine still more unlikely circumstances that could worsen the result. It is therefore necessary to truncate the loss distribution.

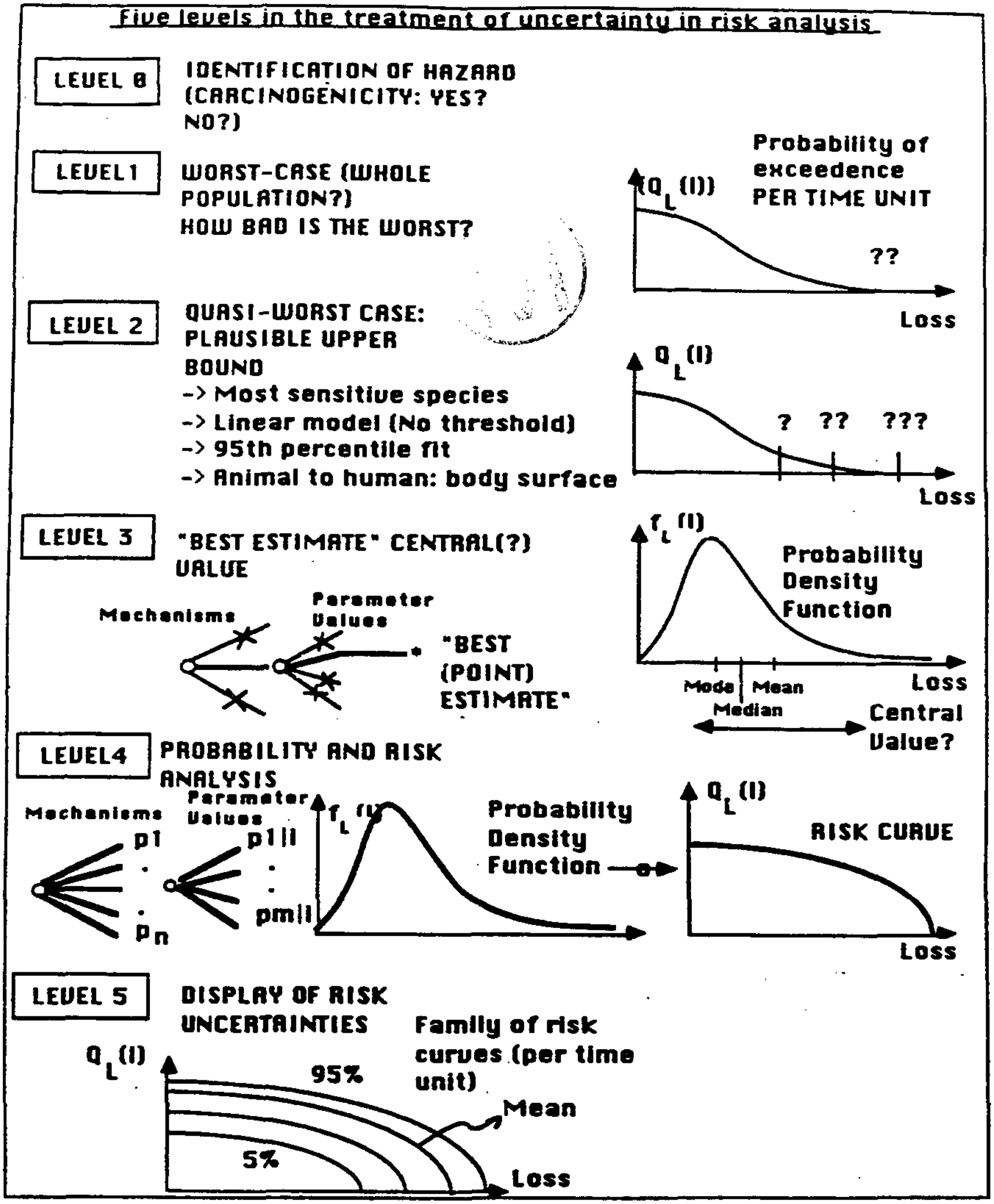

Figure 2: Six levels of treatment of uncertainties in risk analysis (Paté-Cornell, EPRI report, 1995).

Level 2 involves "plausible upper bounds" (or the "quasi-worst case"). This analysis represents an attempt to obtain an evaluation of the worst possible conditions that can be "reasonably" expected (1) when there is some uncertainty as to what the worst case might be, or (2) when the worst case is so unlikely that it is meaningless. Examples of these approaches include the Maximum Credible Earthquake or the Maximum Probable Flood used by the U.S. Army Corps of Engineers in the construction and management of dams.

This popular approach, however, presents major shortcomings. First, there is no way to judge the "conservatism" of these point estimates (the residual risk is unknown). Second, this approach does not allow a meaningful comparison of risks. Ranking among these presumably extreme values may not be related to the ranking of the mean values of the potential losses and there is no reason to believe that priorities set on the basis of plausible upper bounds will ensure maximum risk reduction for the money spent.
analysis.

Level 3 relies on "best estimates" and/or on a search for a central value (e.g., the mean, the median or the mode) of the loss distribution. Generally speaking, the advantage of central values is to provide a reasonable balance to plausible upper bounds. The disadvantage is that the risk is still characterized by a single point estimate and that the uncertainties disappear from the results.

Level 4 relies on probabilistic risk assessment (PRA), also known in engineering as quantitative risk assessment (QRA), or probabilistic safety assessment (PSA). It permits representation of a risk, not by a single point estimate, but by a complete distribution of the potential losses to represent the uncertainties involved. Still, the effects of all uncertainties being aggregated into one risk curve, it is impossible to extract from this information the dispersion due, for example, to expert disagreements about competing models for a fundamental hypothesis.

Level 5 allows display of uncertainties about fundamental mechanisms. This can be done in several ways. One approach is to ask each expert to provide an assessment of the risk based on their favorite model and on their evaluation of the distribution of parameter values, and to display this set of risk curves (one for each expert) without attempting to aggregate the results or to assess the probabilities of the fundamental assumptions on which they rely. The problem is that one popular hypothesis may be favored by a large proportion of experts for a combination of scientific and other reasons. Therefore, if a composite distribution is needed, one must sooner or later address squarely the issue of the relative probabilities of the different hypotheses and proceed to an aggregation of expert opinions. It is important at that stage to depoliticize the process if needed, and to put weights on models (given the evidence available) and not on the experts.

Therefore, in order to reach its logical conclusion, Level 5 requires a full probabilistic treatment of epistemic uncertainties. The result is a *family* of risk curves. These curves provide, for each value of the potential losses, a discretization of the probability distribution of the future frequency of exceedence of this loss value. Both aleatory and epistemic uncertainties are propagated through the analysis, for example, by Monte Carlo simulation or other simulation models such as the Latine Hypercube approach.

WIPP Performance assessment is a mixed case of some aspects of level 2 (plausible upper bounds; conservative hypotheses) and level 5 (full uncertainty analysis).

6. ANNOTATED BIBLIOGRAPHY

A. 40CFR191

1. Russo, September 1991: Updated Uncertainty Analysis of EPA River Mode Pathways Model Used for 40CFR Part 191: Table 1 of 1985 40CFR191 Analysis of Curie release corresponding to 90% level of certainty that effects will be less than or equal to 10 fatal

cancers/10,000 years was completed by an uncertainty analysis in which the probability distributions characterizing uncertainty about model input parameters were based on discussions held with radiological assessment experts taking into account theoretical considerations, variability in published data, and insightful judgment [How was this done?]

2. EPA 520/3-80-006. Population Risk

p. 150: "The expected frequency of human intrusion into a repository ranges from a drilling event every 400 years for granite to a drilling event every 50 years for salt and shale (ADL 79d)." [This is one of the assumptions whose effect on the results should be checked]

3. Federal Register 1985. 40CFR191.

191-13 Containment Requirement:

"Reasonable expectation" language. PA need not provide complete assurance...etc."

4. Response to comments; EPA 520/1-85-0242. p2-5: The median is insufficient. [I agree]. p.2-12: EPA states that the standards, as they are written, will allow demonstrating compliance in a way that will not be "unreasonably difficult or expensive". [Fine].

5. Report of the Review of 40CFR191 by a subcommittee of the SAB.1984. "The subcommittee supports the general form of the proposed standard, including the use of a social objective as an upper bound of acceptable health (cancer and genetic) effects. [The question is: how conservative is the societal risk target *given the assessment method*. Could be very conservative or not. I don't know].

6. Working draft of final 40CFR191: 11/1/83

191.16: Guidance for implementation

"determination of compliance should be based upon "best estimate" predictions (e.g., the mean of the appropriate distribution results)." [Best estimates is generally not a good term to use without specification in regulatory language because it is too vague].

7. Working Draft of final 40CFR191: 2/1/84

"Instead the implementing agency may determine compliance based upon the part of the range of predictions that falls within one standard deviation of the mean..." [That was an idea but it was not implemented].

8. Working Draft of final 40CFR191: 4/23/84
Mean + one standard deviation => 85% for Normal distribution [Many distributions are not normal but skewed right].

9. Working Draft of final 40CFR191: 3/21/85
191.13: Containment requirements Uncertainties are too large hiven the time frame. => reasonable expectation language [Intended to be: the mean; actually here: conditional means].
Further: compliance with 191.13: Integrate all uncertainties into one risk curve [i.e., the mean risk curve. This is the level four of Figure 1. It is generally sufficient to support the choices of the risk averse decision maker in rational individual decision making].

10. Working Draft of final 40CFR191: 6/15/85
191.13: same language about "reasonable expectation".

11. Working Draft of final 40CFR191: 7/5/85
Uncertainties, and long term => reasonable expectation

12. Report of Meeting with extra-agency personnel concerning EPA Docket Number R-82-3. (with NRC staff personnel). "Subparagraph 191.16a requires that the standards be implemented in terms of the upper 85% confidence level of the simulated cumulative release. In view of the very very large number of judgmental factors that will have entered into the calculation, the use of the specified confidence level as a basis for deciding compliance is highly susceptible to mischief during the licensing process[...]; in view of uncertainties involved, confidence levels must be adressedd in terms of qualitative (e.g., reasonable assurance terms) rather than quantitatively."
Further: "Confidence level: DoE is concerned that the mention of an 85% confidence level will become the required level for all analyses; this would be contrary to EPA's intention."
Further: Guidance for implementation:
Suggestion again that mean + one standard deviation becomes the standard (=>≈85% for Normal distribution).
Further: "Paragraph 191.16.c unclear and calls for a precision level that may not be possible to demonstrate analytically."

13. EPA background information behind 40CFR191 1985
EPA 520/1-85-023.
p.6-3 Problem of uncertainties in EPA modeling of radiation risk estimate (the risk per unit dose are likely to be low)
p 6-13 the risk estimates are not unduly conservative [Important to check and to assess the effect on individual safety].
14. EBASCO study: Uncertainty and sensitivity analysis for the exposure pathway models used in 40CFR191.
p. xiii: levels of certainty for each radionucleide in Table 1.tryin to evaluate the level of conservatisms [Again, effects on conservatism of Sandia's result?].
15. Background information: EPA 93. EPA cancer risks are based on NAS study. Further (p.6-5): dose-response function was based on japanese epidemiology after the Hiroshima bomb. Perhaps unconservative following subsequent studies (p.6-93). p.6-31: estimates of cancer risks are NOT conservative.
16. EPA environmental pathway model, 1986
p.S9: Releases to a river [could be very conservative: WIPP is in the desert; but assumption was based on large rivers; how about small ones?].
17. Analysis of Uncertainties. Envirosphere, June 10, 1983.(problem of the original ore body release and river mode exposure pathway). Another set of uncertainty assessments for Table 2 of 40CFR191.

B. 40CFR194

1. Federal Register
2. 40CFR194 Proposed Rules: Criteria for the Certification and determination of the Waste Isolation Plant's compliance.
p.81: Expert Judgment "should be used provided that it does not substiotute for data that could be obtained through data collection or implementation." [An apparently reasonable set of requirements. No conflict of interest. At least five experts. Not all from DoE].
p.113: results of performance assessment:Risk curves. Monte Carlo of Latin Hypercube. Not the median. Requirement that the number of risk curves be large enough so that the maximum CCDF generated exceeds the 99th percentile of the population of CCDFs with at least 0.95

probability. [Looks conservative. To be checked]

p.114: the criterion itself: "demonstrate that there is at least a 95% level of statistical confidence that the mean of the population of CCDFs meets the requirements of section 13(a) of 40CFR19" [The mean is the most robust measure under the circumstances (smaller sample size required) and it may already be in the 80 to 95% fractile].

3. Background information: EPA 402-R-95002

p.3-7: Disposal systems shall be designed to provide reasonable expectation based upon performance assessments that cumulative releases of radionucleides to the accessible environment for 10,000 years after disposal from all significant processes and events that may affect the disposal system shall:

(1) Have a likelihood of less than one chance in ten of exceeding the quantitties calculated according to table 1 (Appendix A) and

(2) Have a likelihood of less than one chance in 1,000 of exceeding ten times the quantities calculated according to Table 1.

Table 1 defines a set of permissble releases ("normalized release" for each isotope). [The question is; what were all the hypotheses underlying Table 1].

4. Compliance criteria: March 21, 1995

p.55: results of performance assessments.

5. EEG Comments. April 28, 1995.

p.5: the WIPP site does not meet the there stated criteria of 40 CFR 191.149 (because it is in a resource rich area)=> unconservative assumption. On the other hand, (p.6)EPA claims that the hypotehse are favorable because of the favorable characteristics of the WIPP (located in the desert). [net result??]

p.11: Engineered barriers:

Argument for engineered barriers: unconservative assumptions regarding human intrusion in a resource rich environment. Also: benefits will be small because it would only delay the arrival of actinides in the environment.

1992 PERFORMANCE ASSESSMENT REPORT

COMMENTS AND RESPONSES

EPA/EEG/NMAG/NMED

DOE/WIPP-95-2053

Responses to Comments on the 1992 Preliminary Performance Assessment Report for the Waste Isolation Pilot Plant

June 1995

United States Department of Energy
Waste Isolation Pilot Plant

Carlsbad Area Office
Carlsbad, New Mexico

Table of Contents

Introduction

This document contains the Department of Energy's (DOE) responses to comments made by the Environmental Protection Agency (EPA), the Environmental Evaluation Group (EEG), the New Mexico Environment Department (NMED), and the New Mexico Attorney General (NMAG) on the "Preliminary Performance Assessment for the Waste Isolation Pilot Plant, December 1992", Volumes 1-3. Each comment and response is listed along with the reference materials. The appendixes contain supportive figures and memos referenced in the document. The subject document is referenced as the 1992 PA (Performance Assessment).

Comment CL1

Cover Letter, Format and Content

"We reviewed this document with the idea that it should contain all of the information needed to allow us to review, understand and evaluate DOE's approaches, and to demonstrate that the approaches were sufficiently justified to support a request for certification of compliance."

Response

The DOE appreciates the perspective taken by the EPA for this review since it provides the DOE with significant insight regarding what should be included in a compliance application. In addition, the EPA's perspective will help identify areas where the two agencies may disagree regarding implementation. It is important to seek resolution to these prior to the preparation of a final application.

The DOE will use EPA's comments and suggestions as a guide on formulating aspects of its compliance program. In addition, the DOE will establish, as a priority, the resolution of any issues or disagreements that have resulted from these comments.

M

Comment CL2

Cover Letter, Format and Content

"Although the PA provides a large amount of information, it lacks a sufficient description of the analyses that are discussed. The current PA is not a "stand alone" document that uses references as supporting information. Rather, references are often provided as the proof of the validity of DOE's reasoning, with insufficient information presented in the PA to enable the reader to follow that reasoning. ... While we understand that references and accompanying documents will be needed, we feel that the PA (in all its volumes) needs to tell a complete story. In our view, the PA should start with the basic information and, step by step, build up to a demonstration of compliance."

Response

The DOE agrees that the PA is not a "stand alone" document for compliance purposes. In fact, PA is only a tool used to determine compliance with quantitative limits and to understand uncertainty. Numerous other topics are to be included in the compliance application as indicated by the Format and Content Guide issued in May 1994.

This not withstanding, the DOE appreciates the broad perspective used by the EPA in its review as discussed above. The DOE will use this broad review as a guide in preparing the final application.

Comment CL3

Cover Letter, Format and Content

"For example, it would be helpful if the PA presented a listing of scenarios considered, and showed the analysis of probability and consequence for each separate scenario. The presentation of separate analyses would help clarify how scenarios are combined to create a final set of CCDFs."

Response

This material will be included in the compliance application as appropriate. The DOE notes, however, that separate analysis of both probability and consequences may not be appropriate for all scenarios considered. Some scenarios may be shown to be of sufficiently low probability that consequence analyses are unnecessary. Other scenarios may be shown to be of sufficiently low consequence that probability analyses are not required. In other cases, different scenarios may have sufficiently similar consequences that results of a single consequence analysis may be used in conjunction with different probabilities.

Comment CL4

Cover Letter, Format and Content Guide

The outline in the November 1993 Format and Content Guide prepared by the DOE is a significant improvement over the current PA organization.

Response

Comment noted.

Comment CL5

Cover Letter, Access to Information

"We are concerned that much of the information referenced or used as integral part of the PA analyses is not available for review. The current PA cannot be thoroughly reviewed because the supporting information is not accessible. This includes some references, computer codes, their documentation, laboratory and field data, and the data generated from computer runs, such as the results of the Latin Hypercube Sampling that is used as input for the computer runs. The computer codes and their documentation should be available for public review."

Response

The DOE is currently pursuing access to computer codes and training in their use with the EPA and stakeholders. Documentation of codes is being completed, and will be complete before PA analyses are used in a compliance application.

All source material cited in the 1992 PA, including primary documentation for laboratory and field data, is on file at the WMT library at Sandia National Laboratories. For a compliance application, reference materials will be dealt with in a significantly different manner in order to assure timely access to information by the EPA.

Comment CL6

Cover Letter, Access to Information

"In addition, if they are incompatible with EPA's computer system, they should be remotely accessible on DOE's computers to allow independent examination. I recognize that the codes and their documentation are not in final form; however, our review of the codes will take a long time, so we need the copies used to prepare a performance assessment along with the results of that assessment. If we do not get the codes until the final application, it will significantly slow our review of the application."

Response

The DOE is as concerned as the EPA over the length of time that EPA may require to review DOE's codes. Consequently, the DOE has made code availability a priority. The EPA and the DOE are successfully resolving this issue of access and training.

Computational efficiency is a complex topic and would be a welcome topic for the DOE/EPA technical exchange meetings. Specific recommendations concerning resource allocation, e.g., model development versus collection of experimental data, are to be an integral part of the systems prioritization methodology (SPM) effort. The SPM will have "outside scrutiny" inherent in the process design.

Comment CL7

Cover Letter, Repromulgation of 40 CFR 191

(a) "We realize that 40 CFR 191 had not been repromulgated when the analyses for this version of the PA were being conducted, thus consistency with the rule was impossible. However, the rule is now final, and changes in the PA will need to be made in order to reflect the new 40 CFR 191. The main areas where changes are necessary are in the ground-water and individual protection requirements, and the use of the committed effective dose. The definitions relating to ground water have been changed to reflect EPA's policy of protecting underground sources of drinking water. DOE will need to identify the potential aquifers and their water quality (i.e., total dissolved solids)."

(b) "In addition, the undisturbed performance time frame calculations need to reflect the 10,000 year requirement. With the increase of the time frame from 1,000 years to 10,000 years, DOE may need to include scenarios that were previously omitted."

Response

All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated into compliance documentation.

(a) The DOE has taken the position that a decision on when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application) will be based on the expectation of releases. Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

(b) The DOE agrees that calculations for a compliance application must be performed for a 10,000 year compliance period as stated in 40 CFR 191.

Comment CL8

Cover Letter, Regulatory Issues

"The PA also needs to address both 191 and RCRA compliance."

Response

The DOE has implemented a PA program which examines two-phase flow. This allows the determination of releases via both liquid and gas pathways. For the compliance determination, appropriate transport calculations will be made using these PA models. These calculations will include contaminant transport of interest to each of the regulations.

Comment CL9

Cover Letter, Regulatory Issues (Guidance to 40 CFR 191)

"The Guidance for 40 CFR 191 is generic in its application and it is non-binding to the implementing agency. EPA is evaluating the Guidance's suitability for use at the WIPP site. However, deviations from the Guidance should be clearly explained. The compliance criteria (40 CFR 194) will provide further clarification on this topic."

Response

The DOE agrees that any deviation from the guidance in 40 CFR 191 must be carefully and clearly documented. The DOE realizes that EPA's guidance, while non-binding, is not arbitrary and is provided to assist the implementing agency in meeting EPA's overall goal of protecting human health and the environment. Since EPA's guidance is established as the result of the technical bases developed during rulemaking, the DOE realizes that any significant deviation from this guidance must have equally in-depth technical justification.

Comment CL10

Cover Letter, Regulatory Issues

"The future applicability of the Guidance notwithstanding, DOE did not correctly follow the Guidance in this PA. If DOE was going to follow the Guidance, the PA should have used a constant drilling rate of 30 Boreholes/km^2 per 10,000 years for comparison with the containment requirement. In addition, the PA incorporates credit for passive institutional controls without proper justification."

Response

The maximum rate for human intrusion considered by DOE yielded an expected number of boreholes equal to 30 per square kilometer. Smaller rate constants were also considered to allow the evaluation of the sensitivity of disposal-system performance to uncertainty about future drilling rates. Determining such sensitivities is important to capturing the most significant parameters for the compliance calculations. These sensitivity analyses indicated that system performance is highly sensitive to the drilling rate and that it may be a very important factor in the compliance decision.

The DOE agrees that any assumptions made regarding the guidance need adequate justification as discussed previously. This applies to the credit taken for passive controls, drilling rates, and other factors.

Comment CL11

Cover Letter, Use of Expert Panels and Peer Review

"There should be documentation of the process used to obtain expert opinion, and the process should follow written procedures."

Response

A formal QA procedure for the use of expert judgment by Sandia National Laboratories was published in 1992 (Rechard et al., 1992). Further refinements of this QA procedure can and have been incorporated into WIPP Procedure No. PAP06, Use of Expert Judgment Panel Quality Assurance Procedures. Compliance documentation will contain documentation in accordance with this or any other formal procedure used by the DOE and its contractors to elicit expert judgment.

Comment CL12

Cover Letter, Use of Expert Panels and Peer Review

"Specifically, we do not agree with the approach taken by DOE to estimate a reduction of the drilling rate from speculations on the use of markers. The markers aren't yet designed; therefore, the panel was asked to provide advise about the effectiveness of the markers at WIPP on the basis of incomplete information. Nor did the panel include all the necessary expertise, e.g., no petroleum engineers or drilling experts were included on the panel. The information from the marker panel was then apparently provided as input to a computer program that produces results in an unclear manner. EPA's compliance criteria will contain additional guidance on the use of expert panels."

Response

This issue has been raised numerous times by the EEG and the DOE has been sensitized to assuring proper care is taken in both establishing the scope of an expert panel and in using the outcome.

With regard to the specific comment on the process used by the DOE, the following is provided:

The process used by the Markers Panel was to first develop design guidelines for long-term communicative markers based on the contributions from individuals in disparate related fields such as materials science, archaeology, and communications. Based on the design guidelines, the two teams comprising the Markers Panel each developed a conceptual design for a system of markers. Estimates of efficacy of the markers system over time were based on the conceptual design. Implicit in the deliberations was the assumption that sufficient testing was undertaken to determine, for example, the appropriate design of the foundation for stone markers to withstand possible fluctuations in surface level and still remain stable. A second assumption in the effort was to evaluate what was possible for a marker system (as a first approximation) with no cost constraints. Cost constraints may come into play regarding the definition of "practicable" in 40 CFR 191. There is much evidence from the fields related to marker design that suggest avenues to pursue to improve long-term survivability and communication.

A petroleum engineer was not included on the Markers Panel, because the thrust was geared to long-term survivability of a marker system and continued interpretability. A petroleum engineer's skills are not such as to contribute to this effort.

Comment CL13

Cover Letter, Use of Expert Panels and Peer Review

"In future performance assessments and interim documents, it would be helpful for DOE to identify: 1) the areas where no data exist; 2) where expert panels and expert judgement will be used in lieu of data; and 3) whether the expert judgement will be replaced with data by the time of the final application. We strongly recommend that DOE use data where it is possible to obtain it, instead of relying on expert judgement."

Response

The DOE agrees that the use of expert panels in lieu of data must be carefully documented and justified. The DOE, however, does not agree with the concluding statement in this comment since it is written so broadly. Instead, a level of "practicability" must be applied when designing tests in lieu of expert judgment. This is particularly true when tests may require unrealistically long time frames or represent unreasonable costs.

Comment CL14

Cover letter, Models

"The development and implementation of conceptual, computational, and computer models is one of the most important technical aspects of the performance assessment topics. Therefore, it is imperative that we and the public have a good understanding about the modeling process and the models themselves. In the current PA, a good discussion of conceptual models and their alternatives are provided in only a few instances, such as the porosity model for the Culebra Dolomite. The conceptual models for the potash mining scenario are absent."

Response

The DOE agrees that the development, documentation, and implementation of conceptual, computational, and computer models is critical to a defensible performance assessment. Documentation of models is facilitated by Sandia's formal software Quality Assurance Procedures. The mandatory guidelines and requirements contained in these procedures ensure traceability and verification of computational and computer models, as well as documentation of the underlying conceptual models.

The evaluation of computational-model uncertainty involves evaluation of various conceptual models against relevant repository performance metrics. Current WIPP PA accomplishes this in two ways:

1. An "all-other-things-being-equal" method, in which alternative conceptual models for one component of the system are individually evaluated over the LHS sampling of imprecisely known parameters, while maintaining the variability of parameters not included in the tested sub-model constant.

2. Inclusion of the alternative conceptual models within the sampling of imprecisely known parameters.

Examples of method (1) are inclusion of: a) multiple transport and flow models of the Culebra dolomite (single-porosity vs. dual-porosity vs. fracture-flow only; chemical retardation vs. no chemical retardation); and b) multiple repository and Salado long-term responses (with gas generation vs. without gas generation; with room consolidation vs. without room consolidation; with a representation of fracturing in the Salado vs. with no Salado fracturing).

The example of method (2) to date is inclusion of different two-phase-characteristic curves, e.g. the Brooks/Corey and Van Genuchten/Parker sub-models, and sampling on each.

The DOE believes it is important to evaluate the defensibility of all conceptual models continually, both by examining their supporting experimental data and evidence and by examining whether or not the different models have significantly different impacts on expected performance. Ultimately, the defensibility of a performance assessment depends on the belief of the regulators and major stakeholders that a reasonable conceptual model has been used, and that there is sufficient evidence to support its use. In order to make this evaluation, this history of the development and screening of alternative conceptual models used in the performance assessment must be thoroughly documented.

Comment CL15

Cover Letter, Models

"The next performance assessment iteration should contain a detailed description of all conceptual models chosen and the alternative conceptual models that are or have been under consideration. For those conceptual models no longer under consideration, DOE should justify why they were discarded."

Response

During the development of compliance documentation, a conceptual model screening process that has the goal of examining all conceptual models put forward by Sandia, WID, DOE, and stakeholders will be used to arrive at a prediction of reasonably expected system performance. The screening process will include the reasoning by which the model is accepted or rejected for use in the performance assessment. The compliance documentation will provide the full description of the screening process and its application.

Comment CL16

Cover Letter, Models

"Before DOE submits an application for certification of compliance, there should be general agreement between EPA and DOE on the conceptual models that will be used by DOE."

Response

The DOE agrees that ongoing dialog regarding conceptual models will be very useful.

Comment CL17

Cover Letter, Models

"The development of the computer codes will take time, especially since many of the computer codes are 'state of the art.' Because of the sophistication of the modeling, the peer review and quality assurance of the code will also take time--the more complex the code, the more time it is likely to take. They will also take more time to review. We recommend that DOE takes the necessary time to ensure that the peer review and quality assurance is implemented in a thorough manner, especially where there is uncertainty in the conceptual models used in the codes."

Response

The DOE agrees and has instituted a thorough review of the quality of the codes and data used to implement conceptual models.

Comment CL18

Cover Letter, Quality Assurance

"...the PA does not seem to address data quality objectives or other related issues."

Response

This was not a rigorous objective of the 92 PA. The DOE has recently initiated a quality verification activity to assure data and code quality for compliance determinations.

Comment CL19

Cover Letter, Quality Assurance

(Restatement) EPA is concerned about the implementation of QA for the "old data", such as site characterization or completed laboratory studies.

Response

In 1993, the DOE began an extensive review of early and completed work. The assessment is still in progress. Deficiencies identified in the assessment shall be documented and appropriate corrective action taken. Before the final compliance application is brought to EPA, data, analyses, and resulting conclusions shall be screened against appropriate QA requirements.

Comment T001

Page 1: I. Technical Comments, A. General, Access to Information

Area in document General

"EPA should have access to source codes, code documentation, on-line help files and the executable image. It is recognized that at an early stage of development, a code is a working draft and should not be subjected to a critical outside review. However, if the computer code, references, or other information is adequate for use in the PA, then it is appropriate to have it accessible to EPA and to other interested parties."

Response

The DOE is currently pursuing access to computer codes and training in their use with the EPA and stakeholders. Documentation of codes is being completed, and will be complete before PA analyses are used in a compliance application.

Comment T002

Page 1: I. Technical Comments, A. General, Resource Allocation

Area in document General

"Decisions regarding resource allocation (e.g., model development versus collection of experimental data) should also be subjected to outside scrutiny. In the case of computer resources, DOE should have its computer codes reviewed for their computational efficiency, because of the potential for the algorithms themselves to be unnecessarily resource limiting to the PA effort."

Response

EPA's comment is noted and will be considered by the DOE.

Comment T003

Page 1: I. Technical Comments, A. General, Limited Resources

Area in document Volume 2, Chapter 7, page 7-5, Line 17

"Direct solution of 'fully coupled equation' is said to be unrealistic using present resources. Why is this true? What resources staff, money, or computer capacity would be required? Has DOE tried to use more efficient algorithms and computer program applications? "

Response

The basis for this statement is discussed in Butcher and Mendenhall (1993, page 7-3 middle paragraph). An example of typical computer capacity requirements is given in the same reference, page 6-5, third paragraph. In regard to algorithms and computer applications, the codes used for these analyses have evolved over the past 30 years and represent the most advanced state-of-the-art technology.

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B*. SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T004

Page 2: I. Technical Comments, A. General, Room Modeling

Area in document General

(a) "Has DOE developed a field theory for two-phase flow in a deformable porous media with fractures that can undergo large deformation?"

(b) "Can DOE provide justification for separating (over the various time-and-space scales) the two-phase flow, mechanical rock response, and gas generation models?"

Response

(a) While a single field theory for two-phase flow in a deformable porous media with fractures that can undergo large deformation may be theoretically feasible, it is considered to be technically infeasible because it would be too unwieldy to use in the global context of PA (also see response to previous comment).

(b) The justification for separation is discussed in Chapter 7 of Butcher and Mendenhall (1993).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T005

Page 2: I. Technical Comments, A. General, Poor Referencing of Information: Shaft Consolidation Example

Area in document Volume 3, Chapter #, Page 3-35, Line 7, A137, A140

It is stated that backfill in the lower is parts of the shafts will become consolidated due to salt creep, with a final permeability comparable to that of the host rock of the Salado formation. However, no calculations or modeling results are presented in the PA report to justify this assumption. The 1992 PA cites the 1991 PA, which in turn cites two other reports without discussion how the values were derived. This is but one case out of many in which the reviewer must peruse a succession of documents to find the source of cited data.

Response

The Project is currently investigating the permeability likely to be achieved by the crushed salt components placed in the shafts. The most current published information is summarized in Van Sambeek et al. (1993). Current technical efforts are focused on evaluating the effects of backstress, placement technique, and parameter variability on our ability to achieve an acceptably low permeability in each of the shafts; effective emplacement of the crushed salt components is an important part of the proposed Large-Scale Seal Tests Program.

Reference

Van Sambeek, L.L., D.D. Luo, M.S. Lin, W. Ostrowski, and D. Oyenuga. 1993. *Seal Design Alternatives Study*. SAND92-7340. Albuquerque, NM: Sandia National Laboratories.

Comment T006

Page 2-3: I. Technical Comments, A. General, Radionuclide Inventories

Area in document General, Volume 3

"The source of the radionuclide inventories is the memo from Andrew Peterson, which appears on page A-135 of volume 3. The inventories for the various generator sites are inconsistent: some include the short-lived daughter products of longer-lived parents, while others do not. For example, Y-90 is in secular equilibrium with Sr-90 in the CH waste at Hanford, while it is absent at INEL. ... Furthermore, INEL list[s] different activities of the two nuclides in its RH wastes. The Peterson memo sums the reported activities, showing significantly different totals for the two nuclides. Of greater import, Hanford lists a large CH waste inventory of PU-241, but nothing for its daughter product, Am-241. In fact, ten years after it is generated (for example), each curie of Pu-241 will be in equilibrium with 12.6 mCi of Am-241."

"Steps should be taken to insure that all generator sites use a consistent methodology for estimating their inventories. Absent such a practice, Sandia should obtain enough information to enable it to evaluate the data and make the necessary corrections."

Response

The Project is evaluating the sensitivity of compliance to this issue. Additional detail will be included in Project Technical Baseline report.

The radionuclide inventory used in the 1992 PA was a hypothetical "design" inventory based on the numbers given in the Peterson memo; inconsistencies in the way generator sites quoted the inventories were ignored in forming the design inventory.

The DOE has recently published the Baseline Inventory Report (BIR) which is aimed at achieving the consistency goal referred to in this comment. The BIR focuses on those parameters believed to be the most important to performance assessment.

Comment T007

Page 3: I. Technical Comments, A. General, Inventory and Release Limits

Area in document Volume 3, Chapter 3, Page 3-35, Line 7; A-137,140

"Why are the 1991 release limits presented instead of the 1992 limits used? The inventory in 1991 was 11.87 million Ci of waste. The 1992 PA inventory is listed as 4.227 million Ci (in tables on pages A-137 and A-140). This is more than "slightly different," than the 1991 release limits as stated in volume 3 of the PA, and it effects the release limit. What is the reason for this discrepancy? What numbers were used for the analyses?"

Response

Possible reasons for this apparent discrepancy (i.e., editorial error, error in interpretation, etc.) are being investigated. The release limits used in the 1992 PA analyses were based on the 1992 PA inventory. Future compliance documents will base inventory information on the BIR which combines information from numerous sources.

Comment T008

Page 3: I. Technical Comments, A. General, Inventory and Release Limits

Area in document Volume 3, Chapter 3, Page 3-35, Line 7; A-137, A-140

"What is the estimate of uncertainty in the waste inventory and the estimate of the release limits? What is being done to decrease this uncertainty? Will bounding values be used?"

Response

Uncertainties in all waste characteristics (e.g., the composition of the waste as well as its radionuclide inventory) are presently unknown. The radionuclide inventory used in the 1992 PA was a fixed, hypothetical "design" inventory based on estimates given by Peterson (see response to preceding Comment T006); uncertainty was arbitrarily added to certain waste characteristics also estimated by Peterson (volumes of cellulosics and corrodible metallics) in order to test the sensitivity of performance measures to variations in these characteristics.

Comment T009

Page 3: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-39, Line 9

"EPA strongly agrees with the State of New Mexico that distribution coefficients (K_d's) be based on 'experimentally justified data' and not based solely on expert panel judgment."

Response

The DOE has planned an experimental program to provide these data if needed.

Comment T010

Page 3: I. Technical Comments, A. General, Uncertainty

Area in document Volume 1, Chapter 3, Page 3-13, Line 44

"Please provide a detailed explanation of all methods used to reduce uncertainty and methods used to evaluate uncertainty."

Response

The line referred to in the text references Table 3-1 on the following pages of Volume 1. This table contains approximately 48 references and internal cross-references to examples of techniques used to assess or reduce uncertainty. The reviewer is referred to the table for more detail than the text provides and to the cited documents for additional detail. The DOE will include the information in these references in the final compliance application to a level deemed appropriate.

Comment T011

Page 3: I. Technical Comments, A. General, Uncertainty

Area in document Volume 2, Chapter 3, Page 3-22, Line 1

"Please provide more discussion on how uncertainty is 'propagated through a model'."

Response

The referenced line of text refers to the propagation of a sample through a model. That step of the analysis is briefly explained in the previous section on the previous page, and is explained in detail in later chapters of Volume 2 and in Volumes 4 and 5. Propagation of the sample through the model simply refers to the calculation of consequences for each Latin hypercube sample, using each of the consequence models in the system.

As discussed in general terms in Section 3.5 of Volume 2, and in extensive detail in the references cited therein, the WIPP PA has selected a Monte Carlo methodology to allow estimation of the uncertainty in model outcomes that results from uncertainty in input parameters.

Comment T012

Page 3: I. Technical Comments, A. General, Grout Seal in MB-139

Area in document Volume 1, Chapter 4, Page 4-4 (Figure 4-2a)

How does the "Grout Seal" get into Marker Bed 139?

Response

Effective placement of grout into Marker Bed 139 has been part of the technology development activities performed at the WIPP facility. Specifically, as indicated in the test plan for the Small-Scale Seal Performance Test-Series F (Ahrens, 1992), this underground test at the WIPP was "intended to demonstrate equipment and techniques for producing, injecting, and evaluating microfine cementitious grout." The grouting was completed in March, 1993, and the final report is currently being prepared.

Reference

Ahrens, Ernst H. 1992. *Test Plan - Sealing of the Disturbed Rock Zone (DRZ), Including Marker Bed 139 (MB139) and the Overlying Halite, Below the Repository Horizon, at the Waste Isolation Pilot Plant - Small-Scale Seal Performance Test - Series F.* Albuquerque, NM: Sandia National Laboratories.

Comment T013

Page 4: I. Technical Comments, A. General, Grout Seal in MB-139

Area in document Volume 1, Chapter 4, Page 4-4 (Figure 4-2a)

How will the seal location be selected?

Response

Tentative locations for the seal components have been identified in the reference seal design report (Nowak et al., 1990) and the logic for the locations is identified. In general, locations were selected on the basis of the sealing strategy (combination of long- and short-term components with some desirable redundancy) and needed function (e.g., limit water flow into the shaft) of a particular component. Locations have been slightly modified in a recent update of the reference seal design; documentation of the updated design is in progress. Additional information related to the intended seal locations will be included in design reports on the various components that will be primary references for compliance documents. At the time of actual placement of the seals, it is likely that location-specific factors such as degree of fracturing or observed water inflow will influence the final placement.

Reference

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant*. SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Comment T014

Page 4: I. Technical Comments, A. General, Bell Canyon Formation Characteristics

Area of document Volume 2, Chapter 2, Page 2-10, Lines 20-22

"If the Bell Canyon aquifers can possibly act as a source of groundwater into the repository due to exploration activity, then it would be prudent to know more about the hydrostatic head gradient of the formation. If there is data on this topic, it was not presented in the PA."

Response

The regional potentiometric surface of the Bell Canyon Formation is presented in Figure 9 of Mercer (1983) and extrapolated static bottomhole pressures in the Bell Canyon in three boreholes (AEC-7, AEC-8, and ERDA-10) tested by the WIPP project are given in Table 4 of the same reference. Pressure and hydraulic head data from the Bell Canyon in two additional boreholes (Cabin Baby-1 and DOE-2) tested by the WIPP project are given in Beauheim et al. (1983) and Beauheim (1986). Data from all five holes indicate that Bell Canyon heads are sufficient to drive brine to the level of the repository in an open borehole; whether flow would be upwards or downwards in this borehole would depend on the pressure conditions existing in the repository at the time.

References

Mercer, J.W. 1983. *Geohydrology of the Proposed Waste Isolation Pilot Plant Site, Los Medanos Area, Southeastern New Mexico.* U.S. Geological Survey Water-Resources Investigations Report 83-4016. Albuquerque, NM: U.S. Geological Survey.

Beauheim, R.L., B.W. Hassinger, and J.A. Klaiber. 1983. *Basic Data Report for Borehole Cabin Baby-1 Deepening and Hydrologic Testing, Waste Isolation Pilot Plant (WIPP) Project, Southeastern New Mexico.* WTSD-TME-020. Albuquerque, NM: U.S. Department of Energy.

Beauheim, R.L. 1986. *Hydraulic-Test Interpretations for Well DOE-2 at the Waste Isolation Pilot Plant (WIPP) Site.* SAND86-1364. Albuquerque, NM: Sandia National Laboratories.

Comment T015

Page 4: I. Technical Comments, A. General, Area of Drifts, Waste Panels

Area in document Volume 3, Chapter 5

"Please clarify the size of the area of the drifts, waste panels and the repository as a whole. The PA uses different numbers for area: 0.5 sq. km and 109,354 sq. meters. What is used in estimating the number of boreholes?"

Response

The areas of the drifts, waste panels, and other features of the repository are given in Table 3.1-1 (which is also keyed to Figure 3.1-2) on page 3-4 of Volume 3. The total excavated area of the disposal region is 111,520 meters^2 but the total area of the disposal region (including pillars and room separators) is 0.5069 kilometers^2. The area used in the 1992 PA to compute the drilling intensity into the repository includes the 111,520 meters^2 of area for CH TRU waste and 14,480 meters^2 hypothetically occupied by RH waste emplaced in the walls of the waste-emplacement panels (total target area of 126,000 meters^2).

Comment T016

Page 4: I. Technical Comments, A. General, Crushed Salt

Area in document Volume 2, Chapter 2, Page 2-48, Line 19

What process was used to prove that crushed salt will compact to 95% of initial density within 100 years?

Response

Creep modeling activities, supported by laboratory measurements on crushed salt and host rock salt, have led to the belief that sufficient deformation will be attained to achieve compaction to about 95%. The most recent modeling efforts are summarized in Van Sambeek et al. (1993). The timing for when the degree of compaction reaches about 95% is directly dependent upon numerous factors such as the steady-state creep rate of the host rock, the initial or emplacement density of the crushed salt, the backstress exerted on the formation by the crushed salt, moisture content of the crushed salt, etc.

Reference

Van Sambeek, L.L., D.D. Luo, M.S. Lin, W. Ostrowski, and D. Oyenuga. 1993. *Seal Design Alternatives Study*. SAND92-7340. Albuquerque, NM: Sandia National Laboratories.

Comment T017

Page 4: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54

"Colloids could potentially have a large impact on the migration or retardation of the radionuclides; they could have a noticeable effect on solubility and sorption of the radionuclides. When will data on colloid formation and transport be collected?"

Response

A laboratory program to determine important information about colloid formation and transport in Salado and Rustler brines is in progress. Some qualitative information from this program has already been transferred to PA for inclusion in future calculations; other information will be provided for the compliance analysis.

Two colloid laboratory programs address the two major types of radiocolloids. Actinide intrinsic colloids, which form by condensation reactions from dissolved radionuclides, are being investigated by a series of screen experiments. Potential carrier colloids, which are ordinarily non-radioactive particles that may act as a substrate for sorption, are being investigated separately, by a series of screening experiments that focuses on evaluating their stability in brines. Results from those two laboratory programs will be incorporated into a model that describes the concentrations of colloid-borne actinides in the disposal room environment. Predictions made with the model will be compared with results from the Source-Term Test Program (STTP) being conducted as part of the Actinide Source-Term Program (see Phillips and Molecke, 1993).

Reference

Phillips, M.L.F., and M.A. Molecke. 1993. *Technical Requirements for the Actinide Source-Term Waste Test Program.* SAND91-2111. Albuquerque, NM: Sandia National Laboratories.

Comment T018

Page 5: I. Technical Comments. A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54, Line 29

"When will it [colloid formation and transport] be modeled in future PAs?"

Response

Transport of colloids will be considered for inclusion in the SPM. Any decision on how to incorporate it in PA for a compliance application will depend on the outcome.

Comment T019

Page 5: I. Technical Comments, A. General, Colloid Transport

Area in document Volume 2, Chapter 2, Page 2-54, Line 29

"How does the lack of information on colloids affect the geochemical and hydrology models developed or under development?"

Response

Colloids may impact current PA modeling in two places: by affecting total concentrations of radionuclides transported in disposal-room brine, and by affecting transport of radionuclides in the Culebra.

Disposal-room actinide concentrations are presently based on values for solubility limits derived from an expert panel (Trauth et al., 1992). The "solubility" panel recognized that suspended forms could contribute to the total concentrations, but concluded that they lacked the information to make any estimate of what that contribution could be.

The distribution coefficients used to describe actinide sorption are also based on expert panel judgment (Trauth et al., 1992), and also do not include colloidal effects. The 1992 PA reported releases into the Culebra and releases transported in fractures only without any sorption. These calculations do not consider the effects of colloids in the disposal room. With regard to colloid-facilitated radionuclide transport in the Culebra, it has been argued that colloids may increase transport rates relative to dissolved species, because colloids may have little retardation and may be preferentially transported in the center of channels where velocities are greater.

Reference

Trauth, K.M., S.C. Hora, R.P. Rechard, and D.R. Anderson. 1992. *The Use of Expert Judgment to Quantify Uncertainty in Solubility and Sorption Parameters for Waste Isolation Pilot Plant Performance Assessment.* SAND92-0479. Albuquerque, NM: Sandia National Laboratories.

Comment T020

Page 5: I. Technical Comments, A. General, Room Closure -vs- Constant Pore Space

Area in document Volume 2, Chapter 7, Page 7-8, Line 13

"If the 'total pore space ... is constant' then room closure (creep compression is not taken into account. However, it is stated on page (7-5) Line 25 room closure is accounted for 'in an indirect way'. This appears to be inconsistent. If you have closure the pore space must decrease. The model appears to be deficient on this point."

Response

The statements referring to constant volume and constant porosity in Volume 2 page 7-7, lines 20 to 24 refer to how the repository was conceptualized prior to 1992. In 1992 the porosity and volume in a disposal room varied in time according to the SANCHO predicted consolidation results, as described in Volume 2, page 7-5.

Comment T021

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"How are values transferred between the regional and the local grids? What is the data/information loss across these boundaries?"

Response

The data between regional and the local grids is transferred by interpolation of heads using either bilinear or integral-preserving (Dukowitz) interpolation. For a locally linear variation of head in the regional grid, no information is lost, neither on heads, gradients, nor fluxes. The motivation for the regional and local grid domain decomposition is to provide improved far-field boundary conditions for the local grid. The compatibility of the interpolation procedure is demonstrated by overlaid contour plots of head, gradients, and fluxes obtained from both regional and local grids.

Comment T022

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"What source (e.g., data and investigator judgment) is used to establish the initial boundary conditions in DOE's modeling efforts?"

Response

The specification of boundary conditions is discussed on page 7-16 in general terms. The sources of boundary condition information for the regional groundwater flow domain are described in more detail in Volume 4, page 6-9 through 6-11.

Comment T023

Page 5: I. Technical Comments, A. General, Boundary Conditions

Area in document Volume 2, Chapter 7, Page 7-16

"How are the initial boundary conditions peer reviewed?"

Response

There was no formal review limited only to model boundary conditions. Boundary conditions are discussed on page 7-16 only in general terms. Specific boundary conditions for the regional groundwater flow domain are in Volume 4 of the 1992 PA, pages 6-9 through 6-11. Boundary conditions received the same peer review as other aspects of the 1992 PA: internal reviews were performed prior to publication by coauthors and coworkers, by formal SNL technical reviewers, by the Performance Assessment Peer Review Panel, and by SNL and DOE management.

Comment T024

Page 5: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13; Volume 3, Chapter 1, Page 1-7; Volume 3, Chapter 2, Page 2-11

"The PA discusses categories of distributions for different parameter types: continuous, discrete, constructed based on experiments, constructed based on expert judgement, and miscellaneous categories."

"The process used to select a distribution for each parameter needs to be discussed. How is a distribution chosen for a particular set of parameters?"

Response

For more detailed discussions of the ways in which distributions of uncertain parameters were constructed in the 1990, 1991 and 1992 PAs, see Tierney (1990, in particular, Figure E-1) and Tierney (1994).

References

Tierney, M.S. 1990. *Constructing Probability Distributions of Uncertain Variables in the Models of the Performance of the Waste Isolation Pilot Plant (WIPP).* SAND90-2510. Albuquerque, NM: Sandia National Laboratories.

Tierney, M.S. 1994. "Using Data and Information to Form Distributions of Model Parameters in Stochastic Simulations of Performance of the Waste Isolation Pilot Plant (WIPP)", *Proceedings of PSAM-II, San Diego, California, U.S.A., March 20-25, 1994.* 051-9 to 051-16.

Comment T025

Page 5: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13, Volume 3, Chapter 1, Page 1-7, Volume 3, Chapter 2, Page 2-11

"How representative are these distributions of actual data?"

Response

Of the distributions of the 49 parameters sampled for human intrusion analyses in the 1992 PA (Volume 3): five were histograms of actual field measurements; three were distributions inferred from actual measurements (e.g., Culebra transmissivities); 18 were constructed by formal elicitation of expert opinion, which may indirectly be linked to data; and the remainder (23) were constructed on the basis of informal expert judgment using the five-step procedure described in Figure E-1 of Tierney (1990). It is not known at this time how well subjectively determined distributions reflect the true uncertainty in a model parameter or how well these distributions represent actual WIPP-specific conditions.

Reference

Tierney, M.S. 1990. *Constructing Probability Distributions of Uncertain Variables in the Models of the Performance of the Waste Isolation Pilot Plant (WIPP).* SAND90-2510. Albuquerque, NM: Sandia National Laboratories.

Comment T026

Page 5-6: I. Technical Comments, A. General, Categories of Distributions and Parameter Selection

Area in document Volume 1, Chapter 4, Page 4-13, Volume 3, Chapter 1, Page 1-7, Volume 3, Chapter 2, Page 2-11

"The constructed distribution type should differentiate between values derived from measurements and those derived from expert judgement."

"How were the input parameters chosen? How many are there, and how many are variable? Which ones are important?"

Response

In theory, expert judgment is founded in measurements and other data collection activities albeit not necessarily WIPP specific. Consequently, to use the general rule indicated here may not be reasonable. Instead, some rationale should be provided for the combination of the two sources of parameter values.

Input parameters are dictated by the nature of the mathematical models used in the PA (see Section 1.4 of Volume 3). There were nearly 400 input parameters in the 1992 PA; 49 of them were treated as uncertain (variable) for the purposes of sensitivity/uncertainty analyses for human intrusion analyses (see sections 6.1 and 6.2 of Volume 3). The most sensitive (important?) parameters in the 1992 PA are described in Table 9-3 of Volume 4, and Table 6-1 of Volume 5.

Comment T027

Page 6: I. Technical Comments, A. General, Assignment of Probability Distributions

Area in document Volume 3, Chapter 1, Page 1-18, Line 37

"Very general procedures are described for assigning probability distributions, but these procedures are incomplete and do not answer critical questions."

"Please clarify the review process used for assigning probability distributions? How do you determine confidence in the probabilities?"

"What are the constructed distributions and which does DOE expect will be replaced by data? (Volume 2, page 6-4)."

Response

The "replacement of constructed distributions by data" is an activity that will be considered for inclusion in the SPM, i.e., the addition of experimental data where required to support a compliance application.

The review process used to assign probability distributions in the 1992 PA is briefly described in Section 1.3.1 of Volume 3; evidence of the implementation of this process is shown in the many memos of Appendix A of the latter document. All parameters used in the 1992 PA were classified as "X" among the three quality-assurance categories of ascending confidence, X, C, and A. To be classified as "C", a parameter would have to have documentation of the line of reasoning that established its distribution and the sources of any data used in constructing the distribution. An "A" class parameter must also have received a documented peer review.

Constructed distributions are explained on page 1-10 of Volume 3. This category of distributions is characterized by direct use of data to form an empirical cumulative distribution function; the "data" may be measurements of real quantities or the set of percentiles obtained from an elicitation of subjective opinion. Ideally, all constructed distributions would be based on real measurements; however, the Project must focus on sensitive parameters.

Additional detail on the construction and use of distributions will be included in the final compliance application.

Comment T028

Page 6; I. Technical Comments, A. General, Heterogeneous Reservoirs

Area in document Volume 3, Chapter 1, Page 1-21 - Line 60

"What is meant by 'reservoirs' in the context of the BRAGFLO model?"

Response

The term reservoir is used in the context of formation, host rock, and porous media, etc., not in the context of brine reservoir per se. The predecessor to BRAGFLO was a multi-phase flow code used in the petroleum field -- thus the use of the term 'oil reservoir' or 'reservoir' model.

Comment T029

Page 6: I. Technical Comments, A. General, Brine Reservoirs

Area in document Volume 3, Chapter 1, Page 1-30, Line 15

"Why is the 'sample intensity function' multiplied by the 'fraction of the repository area that is underlain by brine reservoirs'?"

Response

This question arises in the context of the brief description of the model for computing computational scenario probabilities given in Section 1.4.2 of Volume 3. A more thorough treatment of the same subject, models used to compute human-intrusion probabilities for different summary scenarios, is given in Section 5.2 of Volume 2; the answer to the present question is given by lines 10 thru 28 of page 5-4, including Equation (5-13), in the latter reference.

The cited text is simply an example being given wherein the fraction of repository area underlain by brine reservoirs is of interest for a particular intrusion event, E1 (E1 is an event in which one or more boreholes pass through a waste panel and into a brine reservoir).

Comment T030

Page 6: I. Technical Comments, A. General, Viscosity

Area in document Volume 3, Chapter 1, Page 1-34, Line 9

"DOE states that 'viscosity measurements for an oil-based, 1.7 - kg/m^3 mud'. Why would an oil-based mud be used to drill through the Salado Salt Beds instead of a high-salt water-based mud?"

Response

A high-salt, water based mud is assumed to be the drilling mud used when drilling through the Salado. The Oldroyd model requires a value for the ratio of the initial viscosity (at zero shear rate) to the plastic viscosity, to fully define the model in the low shear regime. This ratio was not available for a high-salt, water-based mud in 1992 so a ratio based on an oil based mud was chosen. Since high shear rates occur at the borehole wall the value chosen for the ratio was expected to have little impact on the final model diameter. The Project is evaluating the sensitivity of eroded diameter to this issue.

Comment T031

Page 7: I. Technical Comments, A. General, "Dual Porosity" Model

Area in document Volume 3, Chapter 1, Page 3-39, Line 1

"The way the 'dual porosity' model is described generates confusion. Does the model really allow diffusion through the rock matrix?"

Response

Yes, the PA's dual porosity model allows diffusion through the rock matrix. In this transport model fluid only flows (advects) along fractures. In this way, solutes (i.e., dissolved actinides) are advectively transported in the fracture void volume and diffuse into the much larger matrix void volume. The SECOTP transport code numerically simulates the diffusion process with a mass transfer term. This term incorporates the free water molecular diffusion of each solute, the tortuosity of the matrix, and the solute concentration gradient between the fractures and the matrix.

References

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra*

Dolomite at the Waste Isolation Pilot Plant Site. SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Beauheim, R.L., and P.B. Davies. 1992. *Experimental Plan for Tracer Testing in the Culebra Dolomite at the WIPP Site.* Revision A. Albuquerque, NM: Sandia National Laboratories.

Comment T032

Page 7: I. Technical Comments, A. General, "Dual Porosity" Model

Area in document Volume 3, Chapter 1, Page 1-39, Line 1

"How does the flow model (SECO2D) compare with laboratory results? Is it verifiable?"

Response

Results from the SECO2D code have not been compared to laboratory results. This code is designed to simulate flow that occurs at a scale that is larger than what could be represented in a laboratory experiment. The SECO2D flow code solves the partial differential flow equation for heads in a 2D, confined, heterogeneous aquifer that obeys Darcy's Law. The flow code has been benchmarked, tested and verified for freedom from coding errors, order of convergence, and discretization consistency (Roache et al., 1990).

Reference

Roache, P., P.M. Knupp, S. Steinberg, and R.L. Blaine. 1990. "Experience with Benchmark Test Cases for Groundwater Flow," *Forum on Benchmark Test Cases for Computational Fluid Dynamics, ASME Fluid Engineering Division Spring Conference, Toronto, Ontario, Canada., June 4-7, 1990.*

Comment T033

Page 7: I. Technical Comments, A. General, Base of Anhydrite III

Area in document Volume 3, Chapter 2, Page 2-4, Line 14

"If the base of the Anhydrite III is so important it would seem more accurate, to create a regional contour map of the base of the Anhydrite III. The North-South geologic cross-section may not account for all unknowns."

Response

The point is well taken. The exact elevation of the base of Anhydrite III beneath the WIPP is not well constrained. Well data are not available for the region immediately beneath the panels. Other methods, including the construction of regional contour maps on the base of the unit could have been used to estimate its elevation. Regional dips are small, however, and the uncertainty introduced locally by a limited stratigraphic data base may be small compared to the uncertainty in the interpretation of the depth to the conducting layer and the interpretation of the conducting layer as brine (see page 5-2 and following text in Volume 3 of the 1992 PA).

Comment T034

Page 7: I. Technical Comments, A. General, Top of Bell Canyon

Area in document Volume 3, Chapter 2, Page 2-10

"A more accurate value for the Top of Bell Canyon can be found by the method outlined in the comment for the Base of Anhydrite III (above)."

Response

The point is valid. The exact elevation of the top of the Bell Canyon Formation beneath the WIPP is not well constrained. Well data are not available for the region beneath the panels. Other methods, including the construction of regional contour maps on the base of the unit, could have been used to estimate its elevation. As seen in well and seismic data, however, regional dips are small, and the uncertainty introduced by limited stratigraphic control may be small compared to the uncertainty in the interpretation of the depth to the conducting layer and the interpretation of the conducting layer as brine (see page 5-2 and following text in Volume 3 of the 1992 PA).

Comment T035

Page 7: I. Technical Comments, A. General, Natural-Analog Data

Area in document Vol 3, Chapter 2, Page 2-12

"It would seem using parameters of sandstones and substituting them for salt is inappropriate because the stress characteristics, the permeability and the porosities are quite different. The performance of salt does not compare to sandstone."

Response

Two-phase characteristics of salt (capillary pressure, relative permeability) have not been measured experimentally for WIPP-specific materials. Very little research has been done on the two-phase properties of very low permeability rock. A search failed to produce data and/or curves that are directly applicable to WIPP. Therefore, an approximate analog approach was taken, based on the lowest permeability rock for which capillary pressure and relative permeability data have actually been measured. A tight gas sand core (Sample MWX 67-35) from the multi-well experiment (Morrow et al., 1986) was selected as the best analog material. This sample is a fine-grained sandstone with bedding and 12 percent porosity. The dominant pore geometry consists of intergranular cracks between abutting quartz grains and solution pores partially filled with dolomite. The permeability of this sample to brine is 43 microdarcies ($\sim 43. \times 10^{-18}$ m^2) at 3.4 MPa confining pressure and 24 microdarcies ($\sim 24. \times 10^{-18}$ m^2) at 34 MPa confining pressure. Based on these results, and a study of threshold pressure (Davies, 1991), two-phase flow in pure or impure halite units is not anticipated. Two-phase flow is confined to the various anhydrite marker beds within the Salado Formation, making the selection of two-phase properties in the halite units (except for threshold pressure) unimportant.

References

Morrow, N.R., J.S. Ward, and K.R. Brower. 1986. "Rock Matrix and Fracture Analysis of Flow in Western Tight Gas Sands," *1985 Annual Report, New Mexico Institute of Mining and Technology*. DOE/MC/21179-2032.

Davies, P.B. 1991. *Evaluation of the Role of Threshold Pressure in Controlling Flow of Waste-Generated Gas into Bedded Salt of the Waste Isolation Pilot Plant*. SAND90-3246. Albuquerque, NM: Sandia National Laboratories.

Comment T036

Page 8: I. Technical Comments, A. General, Lack of Halite and Polyhalite Chemical Interaction

Area in document Volume 3, Chapter 2, Page 2-28, Line 42.

"What justification or data is there to support the comment that 'halite and polyhalite ... are assumed ... not to interact chemically with any contaminants'? This is a very important assumption."

Response

This statement is misleading. The statement should say that the salts have limited sorption potential, and therefore do not provide a significant retardation mechanism. A conservative assumption of 0 for the partition coefficient is used.

Comment T037

Page 8: I. Technical Comments, A. General, Radionuclides for Transport Modeling

Area in document Volume 3, Chapter 3, Page 3-29, Line 31

"Why were only nine radionuclides considered in the 1992 PA transport calculations (and solubility estimates) for CH-TRU waste? This needs to be more clearly explained."

Response

The answer can be found in Section 7.3 of Volume 4 of the 1992 PA. Figure 7.3-1 (page 7-12 of Volume 4) shows plots of radionuclide inventory through time in normalized EPA units. With the exception of Pu-238, only those radionuclides that exist at 1,000 years or later in activities greater than 10^{-2} EPA units were included in transport modeling. The reason for excluding the others is straightforward: they cannot contribute to exceeding regulatory limits even if their entire inventory is released. Pu-238 was omitted from groundwater transport calculations in error and will be included in future analyses—it is a major factor in total inventory before 1,000 years, but rapidly drops out of the inventory after 1,000 years. However, for the fracture-only transport model travel times are sufficiently short that some Pu-238 could reach the accessible environment boundary. The effect on fracture-only transport releases could be significant for intrusions occurring before 1,000 years.

Note that the use of the cutoff of 10^{-2} EPA units is cautious; the total release limit is 1 EPA unit.

Note also that a total of 23 radionuclides were included in cuttings releases, allowing for the full consideration of short-lived species in the cuttings releases.

See Appendix D of Volume 4 for memoranda describing the use of this reasoning in designing actinide and transport experimental programs.

Comment T038

I. Technical Comments, A. General, Excavated Area Underlain by Brine Reservoir

Area in document Volume 3, Chapter 5, Pages 5-2 to 5-11

(a) "What is the accuracy of Transient Electromagnetic Methods? The depth precision 'may be ± 75 m,' but what about the accuracy of the process itself to 'see' fluids? Electromagnetic (EM) methods tend to be gross estimators at best. Why was the data not extended to the ERDA-9 borehole to calibrate the measurements?"

(b) "Has DOE investigated the use of a High-Frequency Three-Dimensional (3D) Common Depth-Point Seismic Survey over the WIPP disposal panels with extended coverage of one-half mile around the panels? The survey should include the ERDA-9 borehole as a reality check. If these pressurized brine reservoirs are associated with anticlinal structures in the upper anhydrite layer then a 3D Survey will clearly, with high confidence, define even a small closure at this depth."

Response

We believe that the geophysical studies are complete and adequately documented. At present, we believe that the resistivity characterization of possible brine distribution beneath the waste panels has provided information to a level of detail exceeding that provided by on-site/near-site stratigraphic data presently available. However, we believe that the assumption that any conductor identified within the Castile is due to the presence of brine is reasonably conservative for purposes of PA.

The Project has investigated and resolved these issues as follows:

1. The validation (accuracy) of the transient electromagnetic method to detect brine was done at the same time as the panel survey by running the same survey over the WIPP-12 area (brine reservoir present known from drilling) and the DOE-1 area (brine reservoirs absent known from drilling) (Reference SAND87-7144, p. 14, Fig. 3-8). Additional validation work with several methods was done prior to the panel survey using the known brine reservoir at WIPP-12 and an artificial target placed in the underground WIPP facility. ERDA-9 was not drilled deep enough into the Castile for most brine occurrences. However, the results of the dual-induction log of hole ERDA-9 was used directly to constrain the resistivity of brine-free Salado/Castile halites and anhydrites.

2. Seismic methods were not the method of choice for brine reservoir delineation after the Project's experience with the original seismic reflection lines for several reasons: a) While seismic studies delineate deformed areas within the Castile (called deformation zone, DZ, in Borns and others, 1993), not all anticlines contain brine reservoirs. Drillholes, such as WIPP-12, in areas of moderately deformed Castile, as well as

drillholes in severely deformed Castile both encounter brine and; b) Lateral velocity structures produce false anticlines. In 1982, drillhole DOE-1 was drilled into an apparent anticline that was based on the seismic surveys. This structure was nonexistent, and the original interpretation was due to the lateral velocity variations; c) it has proven difficult to propagate high frequency signals in the portion of the basin; d) brine reservoirs are characterized by a low fracture porosity (1%), which does not result in a change in velocity much above background variation in velocity. The contrast between the resistivity of a brine reservoir (1 ohm-m) and the background anhydrite (100 - 1000 ohm-m) is significant. Several early Project conclusions were that (1) seismic methods alone are not sufficient to answer whether brine reservoirs are under the site and (2) gravity methods were not effective in mapping deformation.

Considerable development and review went into the selection of methods. Specifically for the brine reservoirs in the early 1980s, WIPP began to study methods based on the measurement of electrical conductivity or resistivity (for example, Controlled Source Audio Magneto Telluric--CSAMT and charged body--mis-a-la-masse) (Elliot, 1982). The basic assumption is that the brine-charged fractures of a brine reservoir will represent a significant conductivity contrast within the Castile. CSAMT was the method most extensively tried at WIPP during this period (Bartel and others, 1983) to delineate the brine reservoirs in the Castile. The EEG sponsored review of the Sandia program for delineating brine reservoirs and the CSAMT method by J. Waite (U or A) and Peter Hoekstra (Geophysicon). This review suggested that WIPP consider other electromagnetic methods specifically the transient electromagnetic method (TEM) a.k.a. time domain electromagnetic methods (TDEM). In 1985, SNL conducted validation surveys in an area around a known brine occurrence (WIPP 12) using several methods (CSAMT, TEM, and Frequency Domain Electromagnetic methods [FEM]) with the Colorado School of Mines and Phoenix Geophysics. Some of the surveys also delineated an artificial target placed in the WIPP underground. Analyses of survey results concur with EEG review recommendations that transient (or time-domain) electromagnetic methods are best suited for delineation of brine reservoirs at the WIPP site.

<u>Comment</u> T039

Page 8: I. Technical Comments, A. General, Organic Containers

<u>Area in document</u> Volume 3, Chapter 3, Page 3-9

"The text on page 3-9 [of Volume 3] states that containers are 55 gallon drums or SWBs, yet on page 4-11 [of Volume 2] it states that some 'waste containers' will be composed of organic material. Please explain this discrepancy."

Response

This is an editorial error: The words "and some waste containers" should be deleted in lines 11-12 on page 4-11 (Volume 2) .

Comment T040

Page 9: I. Technical Comments, A. General Comments, Ideal Gas Law

Area in document Volume 2, Chapter 2, Page 2-34

"How valid is the use of the ideal gas law at lithostatic or hydrostatic pressures? How sensitive a parameter is it? Assumptions of this nature should be explained and justified."

Response

A set of comparative calculations were made using the ideal gas law and several non-ideal equations of state. These calculations showed very little variation in the calculated properties (< 10%). This amount of uncertainty has very little effect on PA calculations, since other sources of uncertainty are much more important.

Comment T041

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"The screening process described in detail in the 1991 documentation should be included in the EPA PA."

Response

The 1992 PA was not intended to be interpreted as a compliance application. Additional material will be included as needed in the compliance application.

Comment T042

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"The possibility of nuclear criticality should be investigated further before it is screened out."

Response

The DOE has initiated further evaluation of nuclear criticality.

Comment T043

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"How will the changes in the repromulgated 40 CFR 191 effect the scenarios that are included?"

Response

This question is still under evaluation. No changes in scenarios specific to the repromulgation of 191.15 and Subpart C are identified at this time. Clearly, the time interval that must be considered for 191.15 and Subpart C has changed. However, 10,000 yr undisturbed performance was already included in the scenario development process as the base case for 191.13.

Comment T044

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, Line 13

"How are scenario uncertainties propagated through the analyses; if scenario uncertainties were included in the screening process, would more scenarios be included?"

Response

As discussed in Chapter 4 of Volume 2, Section 4.2, scenarios are constructed from the events and processes that may affect the system in the future. Uncertainties about these events and processes are explicitly considered in the screening process. Those events and processes that survive the screening process (based on the criteria discussed in the previous section of Chapter 4 of Volume 2) are used to construct the scenarios for consideration. Uncertainty about the occurrence of those events is reflected in the estimation of the probability of their occurrence, which in turn is reflected in the estimation of scenario probabilities.

Comment T045

Page 9: I. Technical Comments, A. General, Screening Process

Area in document Volume 1, Chapter 4, Page 4-2, 4-19

"DOE says that the 'effect of subsidence of potash mining will be added in future PAs'. How is the WIPP site influenced by potash mining and its associated water use? When will the effect of subsidence due to potash mining be added?"

"The mining scenario should consider mining in the potash zone in the controlled area, but above the repository."

Response

The text in question has been paraphrased, rather than quoted exactly, in the comment. The relevant phrase occurs on line 21, and states "...the impact of subsidence events will be examined in future analyses." A decision to add consequence modeling of subsidence effects to the full PA cannot be made until these analyses are available. As noted elsewhere in this volume, "consequences of such potash mining ... will be addressed in future analyses when a three-dimensional model for regional groundwater flow is available" (Volume 1, page 3-11, lines 16-19; see also page 6-3, lines 19-22). A three-dimensional flow model is now operational and ready for preliminary analyses.

As discussed in the event and process screening text in Volume 1 of the 1991 PA (SAND91-0893/1, page 4-35, lines 7-13), subsidence over mines has the potential to affect regional groundwater flow both by creating catchment basins at the surface (changing recharge) and by fracturing hydrostratigraphic units (altering hydraulic conductivity).

The final point here, that mining should be considered within the controlled area, is a point of regulatory interpretation. The wording of Appendix C of 40 CFR 191 indicates that systematic exploitation can be effectively deferred by controls. Mining is such a systematic process and does not occur inadvertently and intermittently.

Comment T046

Page 10: I. Technical Comments, A. General, Probabilities

Area in document Volume 1, Chapter 4, Page 4-2

"How are the probabilities used in the cutoff comparison found, calculated, etc. This explanation should be within the PA."

Response

As the text on page 4-2 of Volume 1 notes, this information is summarized in Chapter 4 of Volume 2 and described in detail in Volume 1 of the 1991 PA (SAND91-0893).

Comment T047

Page 10: I. Technical Comments, A. General, Probabilities

Area in document Volume 1, Chapter 4, Page 4-2

"What are the expected probabilities for each scenario? They should be stated on the same page as the event tree or on the event tree."

Response

The event tree displays summary scenarios, as described in more detail in Section 3.2.2 of Volume 2. These summary scenarios are further subdivided into computational scenarios on the basis of time and number of intrusions, as discussed in detail in Chapter 2 of Volume 4 of the 1992 PA. Probabilities estimated using the Poisson model for intrusion are assigned to computational scenarios, rather than to the summary scenarios, and therefore cannot be displayed on Figure 4-1 as requested. Probabilities for selected computational scenarios are given in table form in Chapter 2 of Volume 4 for specific values of the Poisson rate constant.

Comment T048

Page 10: I. Technical Comments, A. General, Base Case Explanation

Area in document Volume 1, Chapter 4, Page 4-2, Line 40

"All of the events placed in the 'base case' need to be described in detail with an explanation of how the event probability was developed and provide justification for placing these scenarios in the base case."

Response

As discussed in more detail in Volume 2 and in Volume 1 of the 1991 PA (SAND91-0893), all events placed in the base case were assigned a probability of 1. In compliance documentation, a rationale for the assignment of probabilities will be given.

Comment T049

Page 10: I. Technical Comments, A. General, Nearby Boreholes as Communication Pathways

Area in document Volume 1, Page 4-4; Volume 2, Page 2-16

"Have you considered transport to and through boreholes that don't hit the repository, but which could increase the transport of radionuclides?"

"Would current and future boreholes alter the vertical flow regime between units? (Volume 2, page 2-30)"

"Is it possible for the high drilling density around the WIPP site to cause salt dissolution much like in oil and gas fields in Pecos County, Texas? Should this scenario be considered?"

Response

The issue of the consequences of "near misses" potentially affects the compliance analysis and will be reexamined and evaluated. Rates of brine flow from the repository to a "near miss" were examined quantitatively in 1991 in response to comments by the EEG on the 1990 PA. Results of these analyses are reported in pages B-18 through B-26 of Volume 1 of the 1991 PA (SAND91-0893/1). For the assumptions of these analyses (including no lateral development of the DRZ), flow rates were shown to decrease more than two orders of magnitude 0.25 m from the waste. Flow was decreased further at greater distances. Based on these analyses, PA concluded that radionuclide releases up a borehole that directly penetrated the waste would be greater than those from a "near miss."

The Project is currently investigating the issue of existing and future boreholes that could alter the vertical flow regime. Three-dimensional regional flow modeling is in progress to permit evaluation of possible consequences.

The Project has already investigated and resolved the issue of dissolution of salt by oil field drilling and work is complete. Dissolution of salt by oil field drilling has been considered quantitatively in the past by the WIPP Project (see Christensen et al., 1983), although not on the scale proposed here. Previous work has indicated that dissolution by freshwater flowing through a single well does not pose a threat to the WIPP. Possible effects of high-density drilling outside the controlled area can be evaluated for inclusion in the system prioritization. High-density drilling within the controlled area constitutes an intrusion scenario more severe than "inadvertent and intermittent...exploratory drilling."

Reference

Christensen, C.L., C.W. Gulick, and S.J. Lambert. 1983. *Sealing Concepts for the Waste Isolation Pilot Plant (WIPP) Site*. SAND81-2195. Albuquerque, NM: Sandia National Laboratories.

Comment T050

Page 10: Section: I. Technical Comments, A. General, Thermal Effect of Pu-238

Area in document General

"Has the thermal effect of Pu-238 been taken into account in PA Analyses? What would its effect be?"

Response

It is assumed that the comment refers to the thermal load of radioactive waste. Justification for ignoring radioactive induced thermal effects is given in Butcher and Mendenhall (1993, Section 3.7, page 3-26).

In addition, the Performance Assessment Department looked into this issue and concluded a maximum temperature rise of 2° C in the repository falling to 1° C after 80 years (Volume 1, page 4-50 of the 1991 PA [SAND91-0893/1]).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B*. SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T051

Page 11: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"...at issue is the behavior of the clay (bentonite) units "sandwiched" between concrete plugs."

Response

In the reference seal design report (Nowak et al., 1990), the reasons for using swelling clay components are summarized and references to more detailed discussions are provided. In response to the specific questions regarding the clay, it is believed that the clay units may become fully saturated. Clay is not likely to intrude into all exposed fractures and voids although it is obvious that flow through many of these will be limited by the clay. It is intended that the swelling pressure will be controlled (via the initial density of the material) to preclude significant, deleterious fracturing due to the swelling.

Reference

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant*. SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Comment T052

Page 11: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

(Paraphrase) Numerous specific questions are offered related to the removal of shaft liners. The essence of the questions is captured by: Is it necessary to remove the liner? Can this be done safely? What are the consequences?

Response

The question of whether or not to remove the shaft liners (or portions of them) remains an active question within the WIPP sealing program. It is clear that if the liners need to be removed in order to assure effective seals, they can and will be removed. Safe removal of portions of shaft liners has been accomplished in the Carlsbad area in nearby potash mine shafts. Only small amounts of water inflow have been encountered during construction operations and drilling operations conducted in the WIPP shafts. Design and performance considerations for the WIPP shaft seals generally stress reliance upon the components placed within the Salado formation below the current shaft liner and key; if the final designs maintain this reliance, it may not be necessary to remove any significant portions of the existing liners. Detailed design descriptions to be included in compliance documentation will describe whether or not removal is intended and (if needed) procedures for the safe removal of this material.

Comment T053

Page 11-12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"Compaction details have not been provided which may further impact on the method in which the backfill is emplaced and the effectiveness of the backfill as a seal."

Response

This is an ongoing area of significant effort. While the Small-Scale Seal Test Program in Test Series C and D have provided (see summary in Finley and Tillerson [1992]) an indication that an initial relative density of about 80% can be achieved, additional demonstrations are planned to determine if a relative density of at least 85% can be achieved. These additional demonstrations are part of the planned Large-Scale Seal Tests proposed for the WIPP. Results of the demonstrations are intended to be part of the compliance documentation.

Reference

Finley, R.E., and J.R. Tillerson. 1992. *WIPP Small Scale Seal Performance Tests - Status and Impacts*. SAND91-2247. Albuquerque, NM: Sandia National Laboratories.

Comment T054

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"Small amounts of water are considered beneficial in the consolidation of the salt backfill (St 88), but the effects of channelling on individual blocks of salt is uncertain. Channelling of water from upper aquifers seems more likely than a uniform dispersion throughout the backfill. This potential problem needs to be addressed in the PA.

Response

At the present time, efforts are focusing on evaluating methods for in-place compaction of crushed salt. The use of salt blocks is at present considered a backup technology. If in-place compaction of the crushed salt is successful, there will be no reason to do further evaluation of the behavior of blocks. On the other hand, if the use of precompacted blocks of crushed salt becomes the preferred technology, evaluations of phenomena such as this will be completed.

Comment T055

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document General

"...to insure conformance with the circular shaft walls, blocks must be specially milled and placed. No estimate to the amount of time required to fill each shaft by carefully placing blocks has been provided. Such information is necessary to establish the practicality of the approach."

Response

If blocks are to be used in the shafts as part of the sealing approach, there is no question but that the practicality of their emplacement must be established. In the Small-Scale Seal Performance tests, the feasibility of making and emplacing blocks was demonstrated. At the present time however, efforts are focusing on evaluating methods for in-place compaction of crushed salt. If this technology can be demonstrated to achieve the desired degree of compaction, it offers advantages related to cost and safety. Demonstration tests are currently being planned which will evaluate the degree of compaction that can be achieved. If in-place compaction of the crushed salt is successful, there will be no reason to further evaluate the behavior of blocks. On the other hand, if the use of precompacted blocks of crushed salt becomes the preferred technology, evaluations of the practicality of full-scale placement of blocks will be initiated. Because of the planned sequence of evaluations, the use of salt blocks is at present considered a backup technology and very little resources are being expended to evaluate phenomena specific to this technology.

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Comment T056

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 1, Page 5-20

"It is stated that the repository will use bentonite and crushed-salt backfill as a barrier in waste emplacement panels. Isn't it more correct to say that SNL has recommended this backfill but that DOE has made no commitment to use it?"

Response

The EPA suggested statement is correct. SAND90-3074 discusses the scientific aspects of crushed salt/bentonite backfill but no determination has yet been made. Backfill will be considered in the SPM.

Reference

Butcher, B. M., 1991, *The Advantages of Salt/Bentonite Backfill for the WIPP Disposal Rooms*, SAND90-3074, Albuquerque, NM: Sandia National Laboratories.

Comment T057

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Page 2-48

(Restatement) What data are there to support the assumed long-term permeabilities in the shaft seals?

Response

Numerous laboratory studies have been completed which document the permeability of compacted, crushed salt samples. These data are summarized in Figure 4 of Hansen et al. (1993). The relationship is shown between the permeability of the crushed salt samples and the relative/fractional density of the sample. This paper also references the numerous reports in which the data were first documented. In addition, the small-scale seals and the thermal-structural interaction tests are providing some useful information regarding the transfer of the lab studies and analysis results to field situations.

Reference

Hansen, F.D., G.D. Callahan, and L.L. Van Sambeek. 1993. "Reconsolidation of Salt as Applied to Permanent Seals for the Waste Isolation Pilot Plant," *3rd Conference on the Mechanical Behavior of Salt, September 14-16, 1993, Ecole Polytechnique, 91129 Palaiseau, Cedex-France.*

Comment T058

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Page 2-48

"What is the likelihood that the panel and shaft seals will be able to prevent migration through MB-139, both under the excavation and away from the excavation in the Disturbed Rock Zone?"

Response

The shaft seals are not designed to prevent lateral migration through MB-139: flow up the shafts from MB-139 would be very limited by the combination of short and long term components included in the shaft system.

The sealing concepts for the WIPP panel seals (see Nowak et al. [1990]) include provisions for grouting of the Marker Beds or the DRZ in the halite as necessary to limit flow. Effective placement of grout into Marker Bed 139 has been part of the technology development activities performed under the direction of Sandia National Laboratories. Specifically, as indicated in the test plan for the Small-Scale Seal Performance Test-Series F (Ahrens, 1992), this underground test at the WIPP was "intended to demonstrate equipment and techniques for producing, injecting, and evaluating microfine cementitious grout." The grouting was completed in March, 1993, and the final report is currently being prepared.

From the standpoint of long-term performance of the disposal system, the sealing of the marker bed in the immediate vicinity of the waste rooms is of little consequence since virgin conditions will exist within the near vicinity of the excavation. It is these virgin conditions that provide the robust natural barrier to contaminant transport. The project is currently evaluating the advantages of sealing these units, however, from an operational standpoint.

References

Nowak, E.J., J.R. Tillerson, and T.M. Torres. 1990. *Initial Reference Seal System Design: Waste Isolation Pilot Plant.* SAND90-0355. Albuquerque, NM: Sandia National Laboratories.

Ahrens, Ernst H. 1992. *Test Plan - Sealing of the Disturbed Rock Zone (DRZ), Including Marker Bed 139 (MB139) and the Overlying Halite, Below the Repository Horizon, at the Waste Isolation Pilot Plant - Small-Scale Seal Performance Test - Series F.* Albuquerque, NM: Sandia National Laboratories.

Comment T059

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"It is correctly stated that the current backfill design (which is used in the 1992 PA calculations) is "pure, unconsolidated crushed salt with a relatively high permeability that provides little resistance to fluid flow." Salt and bentonite backfill have been studied and said to be available if needed. However, the PA has not yet reported any analyses to indicate the benefits this mixture might provide."

Response

The backfill condition stated here was a PA assumption for the 1992 calculations. In actuality, the backfill is expected to rapidly consolidate to a dense, low permeable state as described in Butcher (1991, Figure 4-4, page 28).

Optimization analyses have not been performed. Preliminary sensitivity analyses showing the potential benefits of reducing porosity and permeability within the waste-disposal area were performed using the 1989 PA modeling system and reported in Bertram-Howery and Swift (1990).

References

Butcher, B.M. 1991. *The Advantages of a Salt/Bentonite Backfill for Waste Isolation Pilot Plant Disposal Rooms.* SAND90-3074. Albuquerque, NM: Sandia National Laboratories.

Bertram-Howery, S.G., and P.N. Swift. 1990. *Status Report: Potential for Long-Term Isolation by the Waste Isolation Pilot Plant Disposal System.* SAND90-0616. Albuquerque, NM: Sandia National Laboratories.

Comment T060

Page 12: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"Neither have analyses been performed to determine the benefit of reducing the initial void space in the backfill."

Response

Analyses have not been performed, but backfill consolidation is estimated to occur so rapidly that reduction of the initial void space would correspond to accelerating closure by no more than 10 years. Crushed-salt porosities are estimated to decrease to less than 10% within 40 years. For this reason, control of initial emplacement density is considered unimportant, as discussed in Butcher and Mendenhall (1993, page 42).

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B*. SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T061

Page 12-13: I. Technical Comments, A. General, Shaft Seals and Salt Backfill

Area in document Volume 2, Pages 2-48 and 2-50

"The design of backfill is part of the engineered alternatives (engineered barriers) issue and evaluations of relative benefits should be included."

Response

Crushed salt backfill with additives is an engineered alternative that DOE will evaluate for consideration in the SPM. These will also be part of the engineered alternatives benefit/detriment study being performed by the DOE.

Comment T062

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 6-1, Line 9

"The statement that the PA Department has a high level of confidence, etc., is perhaps premature."

Response

The 1992 PA was not intended as a compliance application. The sentence in question does not say that the PA Department is confident that compliance has been demonstrated. Rather, the statement was made that PA Department is confident that "the WIPP will be able to comply with the quantitative requirements of the Standard...."

Comment T063

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 6-2, Lines 20-28

"More documentation is needed before it can be concluded 'that no radionuclides will reach the accessible environment from the undisturbed repository for 10,000 years.'"

Response

The 1992 PA was not intended to be a compliance application. More documentation will be provided in the compliance application. The Format and Content Guide provides a snapshot of the comprehensive nature of the application.

Comment T064

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Chapter 6, Page 6-2, Line 37-44

"Any conclusions about meeting the Groundwater Protection Requirements are premature since these requirements have been changed in the repromulgated standard."

Response

Conclusions for the 1992 PA were based on 40 CFR 191 as it existed at that time. All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated in future performance assessments. The specific conclusion about the ability to meet the Groundwater Protection Requirements depended only on the reported results (i.e., no releases to the accessible environment).

As was stated in the 1992 PA (Volume 1, Chapter 3, p. 3-23, l. 25-31), "One of the products of scenario development for the Containment Requirements is a base-case scenario for the WIPP that describes undisturbed conditions. The undisturbed performance of the repository is its design-basis behavior, including variations in that behavior resulting from uncertainties in the 10,000-year performance of natural and engineered barriers and excluding human intrusion and unlikely natural events, as defined in §191.12(p)." Thus, the 10,000 year issue has already been addressed for the undisturbed case, i.e., Groundwater Protection Requirements and Individual Protection Requirements. Future performance assessment calculations, if conducted similarly to those in 1992, will not cause a change in the conclusions since nothing has yet to be shown to be released from the disposal system for undisturbed performance. The results of any future performance assessment calculations conducted using different conceptual models, probability distributions, etc., will of course be evaluated for compliance with the Groundwater Protection Requirements.

The concentration limits for the Groundwater Protection Requirements have not changed between the 1985 and 1993 versions of 40 CFR 191. While not called such, the limits in the 1985 version were the Maximum Contaminant Levels (MCLs) from 40 CFR 141 (5 pCi/l for Ra-226 and Ra-228; 15 pCi/l for gross alpha particle activity; 4 mrem/yr for beta particles and photon radioactivity). Those MCLs have not been changed since, and are now officially incorporated into 40 CFR 191. The definition of the groundwater that is to be protected did change between the 1985 (special sources of groundwater) and 1993 (underground sources of drinking water) versions of 40 CFR 191. The revised definition may cause programmatic changes if DOE is required to identify all potential underground sources of drinking water. As stated in a paper recently presented at the Waste Management '94 conference (Trauth et al., 1994), we propose to determine when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application). Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

Reference

Trauth, K.M., S.G, Bertram, and B. Bower. 1994. "Considerations for Guidance for Radioactive Waste Disposal Arising from Rules Under 40 CFR 191 and 40 CFR 194," *Proceedings of Waste Management '94 Conference, Tucson, AZ, February 27-March 3, 1994.*

Comment T065

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(1) the determination of the extent that expert judgment should be used in PA and development of an acceptable procedure to incorporate this expert judgment into distributions in the various parameters;"

Response

The referenced page does not exist. The answer is given assuming the question refers to page 6-3 of Volume 1.

The concern is appropriate and the DOE looks forward to discussions with the EPA in this matter.

Comment T066

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(2) the inclusion of the scenarios recommended elsewhere in these comments;"

Response

The referenced page does not exist. The following response is given assuming the question refers to page 6-3 of Volume 1.

Scenario development is an ongoing process and the suggested events will be considered for inclusion.

Comment T067

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 1, Page 8-2 (first paragraph)

"The following needs in performance assessment should be added to those mentioned:

(3) the use of plausible radionuclide inventories (including radionuclide composition and specific activity) and their uncertainty in the performance assessment. (To date, the inventory has not been treated as an uncertain variable, even though the PA states that uncertainty in this inventory is large [Volume 2, page 2-51])."

Response

The referenced page does not exist. The following response is given assuming the question refers to page 6-3 of Volume 1.

Because 40 CFR 191 sets limits on the probability of radionuclide releases that have been normalized to the total transuranic inventory in the system, performance is not likely to be strongly sensitive to uncertainty in the radionuclide. This observation has not been tested by formal sensitivity analyses, but can be partially supported by comparison of the 1991 and 1992 preliminary PAs, which used different inventories. Uncertainty in the radionuclide inventory will be considered in future evaluations.

Comment T068

Page 13: I. Technical Comments, B. Additional General Comments

Area in document Volume 2 Page 3-18, (Line 5)

"Is temperature a variable in BRAGFLO?"

Response

Temperature is not treated as an unknown variable -- the repository is assumed to be isothermal. Temperature is used to calculate the physical properties of the fluid so temperature is a parameter, i.e., a value assumed constant for the purposes of computation.

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T069

Page 14: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 2, Chapter 2, Page 2-38, Line 12

"How will the 'location and amount of future' groundwater flow be dealt with in future PAs? How will DOE deal with such vast uncertainties?"

Response

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of compliance to this issue.

Comment T070

Page 14: I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids

Area in document Volume 2, Chapter 2, Page 2-34

(a) "What is the explanation for the wells with the low TDS?"

(b) "What is the TDS level in all the potential aquifers?"

Response

(a) Hypotheses concerning the possible lack of consistency between inferred north-to south flow in the Culebra and hydrochemical facies are discussed on page 2-36, lines 11 - 20. Also see response to the comment from the EPA document, page 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

(b) For this response, "potential aquifers" is assumed to mean Potential Underground Sources of Drinking Water, as defined in Subpart C to 40 CFR Part 191, in strata above the Salado Formation. The principal stratigraphic units known to be able to produce sufficient amounts of water are the Culebra and Magenta dolomites. The TDS of waters in these units is summarized on page 2-34, lines 24 - 31 in Volume 2 of the 1992 PA. Measured TDS values from drill holes in the vicinity of WIPP are given in Table 2-2 of Siegel et al. (1991). Furthermore, TDS values are measured routinely as part of the WIPP Groundwater Quality and Sampling Program.

As stated in a paper recently presented at the Waste Management '94 conference (Trauth et al., 1994), we propose to determine when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application). Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

References

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.G, Bertram, and B. Bower. 1994. "Considerations for Guidance for Radioactive Waste Disposal Arising from Rules Under 40 CFR 191 and 40 CFR 194," *Proceedings of Waste Management '94 Conference, Tucson, AZ, February 27-March 3, 1994.*

Comment T071

Page 14: I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids

Area in document **Volume 2, Chapter 2, Page 2-34**

"How much uncertainty reduction in aquifer characteristics would there be if DOE drilled more test wells near the controlled area?"

Response

The Project requests clarification of this question to insure that the response is appropriate. We are unclear whether the reviewer is concerned about: 1) the errors of estimation of total dissolved solids at unmeasured locations near the controlled area, or 2) the effect of the uncertainty in total dissolved solids on estimates of transmissivity or other aquifer properties.

The Project believes that there will always be uncertainty in aquifer characteristics, and that the acceptable level of uncertainty is related to the effect of that uncertainty on regulatory performance measures.

Comment T072

Page 14: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation

Area of document General

"Current and historical head measurements of the wells penetrating post-Salado strata would be helpful in interpreting the significance of seasonal or annual fluctuations in the reported potentiometric surface."

Response

Hydrographs for all wells at the WIPP site are presented in Hydrologic Data Reports #1-8 (Hydro Geo Chem, Inc., 1985; Intera Technologies, Inc. and Hydro Geo Chem, Inc., 1985a,b; Saulnier et al., 1987; Stensrud et al., 1988a,b; Stensrud et al., 1990), Richey (1987), and Cauffman et al. (1990). No fluctuations related to seasonal or annual cycles have ever been identified nor, given the depth and degree of confinement of Rustler Formation units at the WIPP site, are any expected to occur.

References

Hydro Geo Chem, Inc. 1985. *WIPP Hydrology Program Waste Isolation Pilot Plant, SENM Hydrologic Data Report #1.* SAND85-7206. Albuquerque, NM: Sandia National Laboratories.

Intera Technologies, Inc., and Hydro Geo Chem, Inc. 1985a. *WIPP Hydrology Program Waste Isolation Pilot Plant, Southeastern New Mexico Hydrologic Data Report #2.*

SAND85-7263. Albuquerque, NM: Sandia National Laboratories.
Intera Technologies, Inc., and Hydro Geo Chem, Inc. 1985b. *WIPP Hydrology Program Waste Isolation Pilot Plant, Southeastern New Mexico Hydrologic Data Report #3.*

SAND86-7109. Albuquerque, NM: Sandia National Laboratories.

Saulnier, G.J., Jr., G.A. Freeze, and W.A. Stensrud. 1987. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #4*. SAND86-7166. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, A.M. LaVenue, J.B. Palmer, and G.J. Saulnier, Jr. 1987. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #5*. SAND87-7125. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, T.L. Cauffman, J.B. Palmer, and G.J. Saulnier, Jr. 1988a. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #6*. SAND87-7166. Albuquerque, NM: Sandia National Laboratories. *WIPP*

Stensrud, W.A., M.A. Bame, K.D. Lantz, J.B. Palmer, and G.J. Saulnier, Jr. 1988b. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #7*. SAND88-7014. Albuquerque, NM: Sandia National Laboratories.

Stensrud, W.A., M.A. Bame, K.D. Lantz, J.B. Palmer, and G.J. Saulnier, Jr. 1990. *WIPP Hydrology Program, Waste Isolation Pilot Plant, Southeastern New Mexico, Hydrologic Data Report #8*. SAND89-7056. Albuquerque, NM: Sandia National Laboratories.

Richey, S.F. 1987. *Water-Level Data from Wells in the Vicinity of the Waste Isolation Pilot Plant, Southeastern New Mexico*. Open-File Rpt 87-120. Albuquerque, NM: USGS.

Cauffman, T.L., A.M. LaVenue, and J.P. McCord. 1990. *Ground-Water Flow Modeling of the Culebra Dolomite, Volume II: Data Base*. SAND89-7068/2. Albuquerque, NM: Sandia National Laboratories.

Comment T073

Page 14-15: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater geochemistry

Area in document Volume 2, Chapter 2, Page 2-34

"Inferred north to south groundwater flow directions in the Culebra Dolomite (based on the potentiometric surface in the Culebra Dolomite) are inconsistent with the distribution of geochemical facies in the Culebra groundwaters (salinities decrease from north to south).

Attempts are made to explain this apparent discrepancy, but alternative working hypotheses should be explored, including the possibility that the potentiometric surface is inaccurate."

Response

The comment notes that several hypotheses have been advanced to explain the apparent discrepancy, but that additional (alternative) hypotheses should be explored. This issue affects the compliance analysis in that it relates to alternative conceptual models of groundwater flow in the Culebra. Additional work to evaluate alternative hypotheses concerning Culebra geochemistry will be considered for inclusion in the systems prioritization.

The Project will record the issue that the potentiometric surface might be inaccurate. Pending the outcome of the systems prioritization, the Project may consider this issue. It should be noted that, because of variations in fluid density, the potentiometric surface is not the best indicator of flow directions. Instead, calculated velocity fields should be used. These depend on measured values of pressure, fluid density, and transmissivity, as well as the assumptions of the model used to calculate them.

See also responses to comments in the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids); p. 18 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields); and p. 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

Comment T074

Page 15: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 2, Chapter 2, Page 2-34

"Points of groundwater recharge and discharge into post-Salado strata are very poorly constrained. Further study is needed to document these important aspects of the hydrology across the WIPP site."

Response

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of this issue to compliance.

Comment T075

Page 15: I. Technical Comments, C. General Geology and Hydrology, Hydrologic Parameters

Area in document General

(a) "It is not clear why only horizontal fractures were used in PA models for the Culebra Dolomite member (Volume 3, p. 2-85), as vertical fractures are more likely to have greater connectivity in most subsurface environments."

(b) "More data need to be acquired for all relevant stratigraphic units at the WIPP site (i.e., Castile through Dewey Lake Redbeds."

Response

(a) When modeling a formation as a dual-porosity continuum, the actual orientations of fractures are unimportant. The modeled fractures are not used to provide connections between points. The important fracture parameters to be captured in the models are the fracture porosity, which will control the flow velocity, and the surface area of the fractures, which will control the amount of matrix diffusion that occurs. As long as the fracture porosity and surface area are kept constant, it does not matter if the fractures are modeled as single sets of horizontal or vertical fractures or as three orthogonal sets of fractures. Single sets of horizontal fractures are the simplest to model, so that is what PA has used.

(b) The need for additional hydrologic data for various stratigraphic units may be considered for inclusion in the SP.

Comment T076

Page 16: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

(Summary) This paragraph follows the introductory comment that many problems associated with the transmissivity fields are related to calibration to fresh-water heads. The second paragraph points out that neglecting spatial variations in density of Culebra groundwaters could potentially cause significant errors in the calibrated transmissivity fields, as well as predicted flow directions.

Response

The transmissivity fields are actually calibrated to pressure rather than fresh-water head (Volume 2 of the 1992 PA, Section 7.5, p. 7-10, l. 14-17). However, given that the elevations of the measuring points are known, it really does not matter which parameter, pressure or fresh-water head, is used for calibration. The important point is that the flow portion of the code (SWIFT II) used for the calibration solves differential equations formulated in terms of pressure. Variations in density are fully accounted for in the code. In these calibrations, it is assumed that the density of water varies with position but is fixed in time. The evolution of the chemistry of Culebra waters is not sufficiently well understood to determine if the assumption that the density distribution does not change over long periods of time is valid.

Comment T077

Page 16: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

"...Simulations which are based on equivalent freshwater head may produce erroneous velocity magnitudes and flow directions in this critical area."

Response

See preceding response. Transmissivity fields calibration accounts for variable density. The SECO-FLOW calculations in the 1992 PA used the calibrated transmissivity fields but assumed constant fluid density. Modifications to SECO-FLOW now permit variable-density flow calculations in PA.

Comment T078

Page 16: I. Technical Comments, General Geology and Hydrology, Groundwater Transmissivity Fields

Area of document General

"The means by which the aquifer test results were incorporated as known values into the calibration of the transmissivity fields is unclear. However, the aquifer test analysis should have considered density effects on pumping responses in the monitoring wells."

Response

Transmissivities derived from single-well hydraulic tests or from interference (pumping) test responses over distances less than 50 m formed the data base for kriging of the transmissivity field. The measured transmissivities were preserved in the kriged transmissivity fields, within the estimated error bounds of the measurements. When the model domain was discretized into grid blocks, however, average values of the kriged field were calculated for and assigned to each grid block. Therefore, the average value assigned to a particular grid block need not coincide with the transmissivity determined at an individual well lying within that grid block. The process of defining transmissivity fields using aquifer-test results is discussed in LaVenue et al. (1990).

The aquifer-test analyses did take density effects into account. All analyses were done in terms of pressure changes, not water-level changes, providing results in the form of permeability-thickness products, not transmissivities. Transmissivities were then calculated based on the brine density at each location. Aquifer-test analysis procedures are discussed in Beauheim (1989).

References

LaVenue, A.M., T.L. Cauffman, and J.F. Pickens. 1990. *Ground-Water Flow Modeling of the Culebra Dolomite. Volume I: Model Calibration.* SAND89-7068/1. Albuquerque, NM: Sandia National Laboratories.

Beauheim, R.L. 1989. *Interpretation of H-11b4 Hydraulic Tests and the H-11 Multipad Pumping Test of the Culebra Dolomite at the Waste Isolation Pilot Plant (WIPP) Site.* SAND89-0536. Albuquerque, NM: Sandia National Laboratories.

Comment T079

Page 16-17: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity

Area in document **Volume 2, Chapter 2, Page 2-34**

"Transmissivity values obtained from the tests should also have been converted to hydraulic conductivities due to the assumption of a uniform thickness over the area. If transmissivity values were used as calibration points directly, they would have been in error by a factor of the effective thickness versus the assumed model thickness of 7.7 meters."

Response

The hydraulic tests within the Culebra were interpreted using the full thickness of the Culebra because the wells are fully screened across the Culebra. Given the uncertainty of the effective thickness across the site, the average thickness of the Culebra was used in the numerical model. The thickness of 7.7 m is smaller than the actual thickness in the southwestern portion of the model area where the transmissivities are the largest. However, the small difference in the conductivity that the uniform thickness assumption would make would have no appreciable difference in the model results since the transmissivity field is significantly changed through the process of calibrating the model to the measured steady-state and transient pressures.

Comment T080

Page 17: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity, Grid Sensitivity

Area in document Volume 2, Chapter 2, Page 2-34

"A sensitivity analysis on the finite-difference grid that was used to generate the 70 transmissivity fields should be performed. ...A finer grid may lead to significantly different transmissivity fields and should be evaluated."

Response

The Project has recently begun a local scale modeling effort with a much finer grid to investigate the transmissivity distribution within the near field (i.e., within the WIPP site). However, the numerical grid used in the 1992 PA flow modeling was designed to represent the regional groundwater flow surrounding the WIPP site and the transient events which have been conducted within the WIPP-site boundary. The 50 x 57 x 1 grid used in the model has larger grid blocks (e.g., 1,000 m to 2,500 m) away from the WIPP site and smaller grid blocks (e.g., 75 m to 250 m) within the WIPP-site boundary where the transient tests have been conducted. The grid resolution is believed acceptable given the objectives of the modeling study (i.e., to determine plausible regional transmissivity distributions within the Culebra).

Different grids (either coarser or finer) may lead to different transmissivity fields because the inverse procedure identifies effective or average transmissivity values at the scale of the grid. Differences in transmissivity values using grids with different resolution may reflect a scaling property of transmissivity, rather than indicating non-convergence of the inverse problem. Convergence of the travel time distribution is important, but that convergence of transmissivity estimates themselves may not be expected, and is not essential by itself.

Comment T081

Page 18: I. Technical Comments, C. General Geology and Hydrology, Culebra transmissivity, Boundary condition uncertainty

Area in document Volume 2, Chapter 2, Page 2-34

"The uncertainty with which the system boundary conditions are understood has serious implications ... the current approach that has been taken to obtain the 70 transmissivity fields does not evaluate the sensitivity of the overall model results to the boundary conditions."

Response

The Project is currently investigating this issue through the development of a three-dimensional model to assess vertical recharge into the Culebra. In addition, elicitation and examination of other conceptual models is an important part of the SP. The boundary conditions used in the 1992 Culebra flow model were estimated from regional water-level measurements and by specifying Nash Draw as a no-flow boundary condition. The south-western boundary condition has some uncertainty due to the variation in water-level measurements in this area.

Note that the horizontal boundary conditions may not produce significant changes to transmissivities within the WIPP-site boundary given the conceptual model used in 1992. This is because of the significant influence that the transient pumping tests have had upon the Culebra. These tests have stressed the Culebra to the extent that the effect of boundary conditions is small.

Comment T082

Page 18: I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields

Area in document Volume 2, Section 7.5 and Appendix D

(Summary) This paragraph contains two points:

> If the area along the Pecos River south of the WIPP site acts as recharge area and groundwater flow is to the east, the low ionic strength water could be dissolving dolomite, thereby creating secondary permeability.
>
> It is unclear why more emphasis in the performance assessment has not been placed on integrating the geochemical data with the hydrogeological data to form a cohesive conceptual model(s).

Response

It is possible that regions of higher transmissivity are due to dolomite dissolution. However a few points should be clarified. There is no indication that flow is presently toward the east from the Pecos River. The possibility that eastward flow occurred in the late Pleistocene has been proposed by Lambert and Carter (1987) and Lambert (1991). While dolomite dissolution might play a role, the distribution of fracture density and the degree to which gypsum and halite presently fill fractures have been proposed as more important controls on the transmissivity distribution (Holt and Powers, 1988). Circulation of low ionic strength water would likely dissolve gypsum and halite from the fractures.

The Project has placed a strong emphasis on integrating geochemical and hydrogeological data. The Siegel et al. (1991) report is an example. Additional work to integrate geochemical and hydrogeological data will be considered for inclusion in the systems prioritization.

See also responses to comments in the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology, Low Values of Total Dissolved Solids); p. 14-15 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater Geochemistry); and p. 19 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences).

References

Lambert, S.J., and J.A. Carter. 1987. *Uranium-Isotope Systematics in Groundwaters of the Rustler Formation, Northern Delaware Basin, Southeastern New Mexico. I: Principles and Preliminary Results*. SAND87-0388. Albuquerque, NM: Sandia National Laboratories.

Lambert, S.J. 1991. "Fossil Meteoric Groundwaters in the Delaware Basin of Southeastern New Mexico," *Stable Isotope Geochemistry: A Tribute to Samuel Epstein*. Eds. H.P. Taylor, Jr., J.R. O'Neil, and I.R. Kaplan. Special Publication No. 3. SAND89-2660. San Antonio, TX: Geochemical Society. 135-156.

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Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico*. SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T083

Page 18-19: I. Technical Comments, C. General Geology and Hydrology, Culebra Transmissivity, Recharge Uncertainty

Area in document Volume 2, Chapter 2, Page 2-34

"In addition to LaVenue's suggestion (SAND 92-7306) that recharge to the Culebra may be occurring in the vicinity of the Pecos River, at least one other alternative conceptual model has been proposed which also involves vertical recharge to the Culebra. This alternative model considers significant vertical recharge to the Culebra over the entire southern region of the modeled area (SAND 88-0196). In either case, if vertical recharge occurs, the 70 transmissivity fields calibrated to the aquifer tests and equivalent fresh-water heads would be lower (i.e. slower velocities) than those which would be calculated with the present model. Vertical recharge should be evaluated in the sensitivity analysis."

Response

The Project is currently investigating this issue through the development of a three-dimensional model to assess vertical recharge into the Culebra. Future modeling studies may include these estimates in the calibration process. It should be recognized that the exclusion of vertical recharge in the region upgradient of the WIPP site leads to higher transmissivity estimates and higher groundwater velocities as noted by the EPA reviewer. This is conservative from a groundwater travel time viewpoint.

Comment T084

Page 19: I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow and Hydrogeochemical Facies Differences

Area in document Volume 2, Chapter 2, Page 2-36, Lines 11-20

"What is being done to examine the differences between the north-to south flow in the Culebra and the hydrogeochemical facies data? How does this discrepancy impact the confidence of the flow modeling?"

Response

It should be emphasized that it is not certain that a discrepancy between flow directions and hydrochemical facies actually exists. The apparent discrepancy might instead be due an incomplete understanding of the hydrogeochemical system.

Several hypotheses have been advanced to explain the apparent discrepancy, as referenced in lines 11-20 in Volume 2 of the 1992 PA. Also, lines 32-35 on page 2-33 and lines 1-3 on page 2-36 reference the strong correlation between the region of high molality sodium-chloride water and the presence of halite in adjacent strata (see Figure 1-13 in Siegel et al., 1991). It is anticipated that a three-dimensional computer model of regional groundwater flow that is being developed (EPP study 5.1.1.2, p. 5-8) will provide information that could be used to evaluate and/or modify the existing hypotheses. Additional work to integrate hypotheses concerning Culebra geochemistry with the results of the regional model will be considered in the systems prioritization.

This apparent discrepancy affects confidence in the flow modeling in that it suggests the need to consider alternative conceptual models of groundwater flow in the Culebra. Consideration of alternative conceptual models is an objective of the SP.

See also responses to comments from the EPA document, p. 14 (I. Technical Comments, C. General Geology and Hydrology); p. 14-15 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Flow above the Salado Formation, Groundwater geochemistry); p. 15 (I. Technical Comments, C. General Geology and Hydrology); and p. 18 (I. Technical Comments, C. General Geology and Hydrology, Groundwater Transmissivity Fields).

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T085

Page 19: I. Technical Comments, C. General Geology and Hydrology, Fracture Density

Area of document Volume 2, Chapter 2, Page 2-19

"Since good information on Culebra fracture density is lacking, please explain why you do not use the higher fracture density where it can be observed."

Response

As the comment states, good information on Culebra fracture density is lacking. It is not clear what is meant by "use the higher fracture density where it can be observed" because fracture density can't be observed in the subsurface except in shafts (Holt and Powers, 1990). Also, all fractures, whether observed or not, are not hydraulically significant. Through tracer tests, we seek to determine the fracture density that is important for transport through the Culebra (see Jones et al., 1992). The interpreted "effective" fracture density is always less than observed fracture densities. Were we to use observed fracture densities in our models, much more physical retardation would occur as a result of matrix diffusion than our tracer tests show to be realistic.

References

Holt, R.M., and D.W. Powers. 1990. *Geologic Mapping of the Air Intake Shaft at the Waste Isolation Pilot Plant.* DOE/WIPP-90-051. Carlsbad, NM: Westinghouse Electric Corporation.

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra Dolomite at the Waste Isolation Pilot Plant Site.* SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Comment T086

Page 19: I. Technical Comments, C. General Geology and Hydrology, Vertical Fractures

Area of document Volume 2, Chapter 2, Page 2-19

"In volume 3 (1-39) it states that vertical fractures in the Culebra are not used in the calculations. Why? How are vertical fractures handled or what assumptions are used?"

Response

When modeling a formation as a double-porosity continuum, the actual orientations of fractures are unimportant. The modeled fractures are not used to provide connections between points. The important fracture parameters to be captured in the models are the fracture porosity, which will control the flow velocity, and the surface area of the fractures, which will control the amount of matrix diffusion that occurs. As long as the fracture porosity and surface area are kept constant, it does not matter if the fractures are modeled as single sets of horizontal or vertical fractures or as three orthogonal sets of fractures. Single sets of horizontal fractures are the simplest to model, so that is what PA has used.

Comment T087

Page 20: I. Technical Comments, C. General Geology and Hydrology, Effects of the Magenta Dolomite in Transport Calculations

Area in document Volume 2, Chapter 2, Page 2-23 to 2-24

"Most of the focus is on the Culebra for transport because it has the highest transmissivity. What is the effect on the release when the Magenta and Culebra Dolomites are combined in the calculations?"

Response

The Project is using a three-dimensional computer model of regional groundwater flow (EPP Study 5.1.1.2, p. 5-8) to evaluate the sensitivity of releases into other hydrologic units to compliance.

A three-dimensional computer model of regional groundwater flow is being developed. This model uses a free surface/seepage face as the upper boundary. This model is designed to simulate areas of discharge and recharge, and patterns of groundwater flow for assumed spatial and temporal distributions of maximum potential infiltration to the water table. The Project is using this model to evaluate the sensitivity of this issue to compliance.

Comment T088

Page 20: I. Technical Comments, C. General Geology and Hydrology, Use of Crushed Culebra Rock

Area in document Volume 2, Chapter 2, Page 2-40/41

"Please expand the discussion on [*how*] the tests involving crushed Culebra rock will help determine K_ds? Is this to simulate the effect of clays? If many of the fractures are clay lined, are any tests being conducted on the clay?"

Response

Early laboratory investigations of chemical retardation in the Rustler Formation were conducted using traditional batch sorption experiments with crushed rock. The early experiments were largely intended to be scoping experiments to ascertain whether chemical retardation was significant and they were conducted under specific experimental conditions. Results of those experiments are expected to be valuable, however, in providing independent checks on results from the present experimental approaches (see, for example, Lynch and Dosch [1980] and the review by Novak [1992]). Currently, mechanistic adsorption experiments coupled with surface complexation modeling are in progress on very carefully prepared ground mineral constituents of the Culebra, dolomite and corrensite, the primary clay mineral constituent. The resulting surface complexation model will predict values for K_d as a function of mineralogy, fluid composition, and adsorbates. On the basis of published information in peer-reviewed journals (see also Siegel et al., 1990), we have strong evidence that results from mechanistic experiments will be representative of the phenomena occurring in the intact Culebra rock. To confirm this, comparisons will be made with the column experiments and sorption experiments with thin slabs or ground samples of Culebra rock.

References

Lynch, A.W., and R.G. Dosch. 1980. *Sorption Coefficients for Radionuclides on Samples from the Water-Bearing Magenta and Culebra Members of the Rustler Formation.* SAND80-1064. Albuquerque, NM: Sandia National Laboratories.

Novak, C.F. 1992. *An Evaluation of Radionuclide Batch Sorption Data on Culebra Dolomite for Aqueous Compositions Relevant to the Human Intrusion Scenario for the Waste Isolation Pilot Plant.* SAND91-1299. Albuquerque, NM: Sandia National Laboratories.

Siegel, M.D., J.O. Leckie, S.W. Park, S.L. Phillips, and T. Sewards. 1990. "Studies of Radionuclide Sorption by Clays in the Culebra Dolomite at the WIPP Site, Southeastern New Mexico," *Waste Management '90, Waste Processing, Transportation, Storage and Disposal, Technical Programs and Public Education, Tucson, AZ, February 25-March 1, 1990.* Ed. R.G. Post. SAND89-2387. Tucson, AZ: University of Arizona. Vol. 2, 893-900.

Comment T089

Page 20: I. Technical Comments, B. Additional General Comments, Groundwater Age Discussion

Area in document Volume 2., Chapter 2, Page 2-37, Lines 22-32

"The section on the isotopic and tritium data is confusing. DOE states that tritium levels indicate minimal contributions from the atmosphere since 1950. If the waters have even 'minimal' contributions, then that would indicate the waters are not that old. Please clarify what is meant. In addition, what is being done to resolve uncertainty about groundwater age?"

Response

For a discussion of the isotopic data, including tritium, see Chapter 5 of Siegel et al. (1991) and the references cited therein. The statement quoted from Volume 2 of the 1992 PA is consistent with the observed data. Meaningful nonzero tritium measurements suggest either contamination during drilling or sampling or some degree of mixing with modern surface water in the hydrostratigraphic unit. For the WIPP, these measurements indicate that the amount of mixing has been either zero or extremely small. As stated in the text in Volume 2, this in turn suggests that groundwater travel time (i.e., the mean travel time) from the surface to the sampled units is long. The presence of some "younger" water from the surface does not imply that all water is "young," nor does it imply that recharge is rapid.

Effects on disposal-system performance about the rates of vertical flow, and therefore about groundwater age, are being examined through regional 3D modeling.

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico*. SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T090

Page 21: I. Technical Comments, C. General Geology and Hydrology, Porosity of Anhydrite Interbeds

Area in document **Volume 2, Chapter 2, Page 2-42, Lines 15-21**

"Are the porosities of the anhydrite interbeds matrix porosity, fracture porosity or both? Will DOE assume that anhydrite fracturing continues to the accessible environment?"

Response

Porosities for the anhydrite reference in the cited area of Volume 2 are presented in Volume 3, Chapter 2, p. 2-65 of the 1992 PA. The porosities given are estimates, not measurements, and are intended to represent the total porosity of the anhydrite. No distinction between fracture and matrix porosity is necessary because PA treats the anhydrite as a porous, not fractured, medium. A laboratory program is underway to measure anhydrite porosity, both in an unloaded state and as a function of stress (Howarth, 1994).

PA assumes that the fracturing naturally present in the anhydrites continues to the accessible environment. The 1992 PA did not explicitly include induced fracturing outside of the disturbed rock zone around the repository. It is planned that future PAs will include a relationship between pressure in the fractures and fracture porosity and permeability. Whether or not the pressure in the fractures, and hence the porosity and permeability, changes at the accessible environment boundary will depend on the conditions in the model.

Reference

Howarth, S.M. 1994. *Test Plan: Two-Phase Flow Laboratory Program for the Waste Isolation Pilot Plant (WIPP).* Albuquerque, NM: Sandia National Laboratories.

Comment T091

Page 21: I. Technical Comments, C. General Geology and Hydrology, Average Undisturbed Pore Pressure in Anhydrite

Area in document Volume 3, Chapter 2, Page 2-63

"It is unclear how the average undisturbed pore pressure in the anhydrite was developed. Table 2.3-2 (p 2-33) does not have any measurements greater than 9.5 MPa, yet the mean and median pressures are between 12 and 13 MPa."

Response

Table 2.3-2 shows only halite pore pressure data. Anhydrite data are presented in Table 2.4-2 on p. 2-59. The three values between 12.4 and 12.6 MPa are considered to provide the best representation of anhydrite pore pressures undisturbed by the excavations.

Comment T092

Page 21: I. Technical Comments, C. General Geology and Hydrology, Culebra Matrix Porosity

Area in document Volume 3, Chapter 2, Page 2-83

"If only intact rock was measured for porosity and there was a large amount of core lost in porous (vuggy) and/or fractured portions of the Culebra, is the matrix porosity used in the models: 1) adjusted upward to reflect the higher bulk matrix porosity; or 2) is the data indicating the vugs and fracturing considered as part of the fractured porosity? It is not clear if the Culebra matrix porosity values include these features or not."

Response

The matrix porosity used in the models has not been adjusted to compensate for unsampled, presumably higher porosity, portions of the Culebra. The data include vugs and fractures only to the extent that they were present in the core samples tested. Both fractures and vugs could be included in the "fracture" porosities determined from tracer tests (e.g., Jones et al., 1992), which represent what might be called the effective flow porosity of the Culebra. The matrix porosity might, in turn, be called the diffusion porosity. If matrix porosity is being underestimated, the potential for matrix diffusion is also being underestimated. See also response to comment in EPA's document, p. 25 (I. Technical Comments, D. Additional Comments on General Geology and Hydrology).

Reference

Jones, T.L., V.A. Kelley, J.F. Pickens, D.T. Upton, R.L. Beauheim, and P.B. Davies. 1992. *Integration of Interpretation Results of Tracer Tests Performed in the Culebra Dolomite at the Waste Isolation Pilot Plant Site*. SAND92-1579. Albuquerque, NM: Sandia National Laboratories.

Comment T093

Page 22: I. Technical Comments, C. General Geology and Hydrology, Data on pH and Eh

Area in document Volume 3, Chapter 3, Page 3-41

"What are the current data or expected values of the pH and Eh in the Culebra under existing conditions? If the data exist, they could not be found in the PA."

Response

Ranges for pH conditions in the Culebra Dolomite have been well defined and range between about 6.5 and 8.0 (see Siegel, 1991, Chapter 2). The pH of the Culebra is expected to be narrowly constrained because of the tremendous buffering capacity of carbonate minerals. Any artificially induced perturbation in pH would rapidly be eliminated by dissolution/precipitation reactions.

Ranges for Eh in Culebra Dolomite groundwaters were investigated by Myers et al. (see Siegel, 1991, Chapter 6 and Appendix 6A). Myers et al. attempted to characterize Eh by evaluating four redox couples (As, N, I, and Se) with measurements with a platinum electrode (refer to Table 6-4 and Figure 6-2 of Siegel, 1991). Unfortunately, many of the measurements for individual redox species were below the analytical detection limits. Consequently, Myers et al. were only able to bound the Eh conditions and were not able to decisively quantify values. Myers et al. did speculate that groundwaters south of the site boundary are more oxidizing relative to groundwaters to the north (see Figure 6-1 and discussion on p. 6-22 of Siegel, 1991).

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T094

Page 22: I. Technical Comments. D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-6 (Figure 2-3)

"The generalized stratigraphy of the Delaware Basin is inaccurate on several accounts.

- Castile formation onlaps the terminal platform margin of the Capitan Limestone and extends further onto inner parts of the Capitan platform. Figure 2-3 shows the top of the Castile formation being located stratigraphically lower than the top of the Capitan Limestone. Figure 2-5 on p. 2-8 more accurately depicts the relationship of the Capitan and Castile formations.

- The Dewey Lake Red Beds are Ochoan in age and should be included in this stage, along with the Castile, Salado, and Rustler formations."

Response

Both points in the comment are correct as stated by the EPA reviewer. Editorial corrections in future documents are appropriate.

Comment T095

Page 22: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-10 (Section 2.2.2.2)

"No mention is made of the wide variety of depositional facies that actually comprise the Capitan Limestone. A potentially important lithofacies, at least with regard to the hydrologic characteristics of the Capitan Limestone, is the forereef or foreslope facies, which consists of poorly sorted carbonate clasts shed from the high relief Capitan margin. This facies is poorly mapped, may have very different flow characteristics than for other facies in the Capitan Limestone, and tongues of this facies may extend close to or beneath the WIPP site."

Response

The first sentence of the comment is plainly correct. We are unaware of evidence for forereef deposits extending 10 to 15 km into the basin for very different flow characteristics within these deposits. We would be willing to discuss this topic with the EPA during technical exchange meetings.

Comment T096

Page 22-23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-10, Line 35

"'Lateral variations in depositional environments (in the Culebra Dolomite) were small within the mapped region...' What is the evidence for this statement? Detailed lithologic columns for the Culebra Dolomite with lithologies and sedimentary structures should be shown."

Response

Additional detail in the level of referencing will be included as appropriate in the PTB/draft compliance application. It is not clear that inclusion of stratigraphic columns is relevant to compliance, except along possible radionuclide-release paths.

Page 5-11 of Holt and Powers (1988) states: "The bulk of the Culebra is microlaminated to thinly laminated. The strata may be flat to wavy to locally contorted and discontinuous. Portions of the Culebra appear macroscopically devoid of depositional fabric. The dolomite is mottled in some zones. With the exception of the upper and lower contact zones, there is very little variation of depositional sedimentary features throughout most of the Culebra." Additional detail from the same page includes: "The lowermost foot (30 cm) of the Culebra locally is thinly laminated to laminated, with alternating light and dark brown laminae." "The uppermost few inches to 1 ft (30 cm) of the Culebra often differs radically from the underlying dolomite. The gamma ray signature of this zone is unique and is present throughout the Delaware Basin." We agree that the detail of referencing needs to be increased; generalized referencing in important areas, is inappropriate.

A large number of detailed correlations, cross-sections, and stratigraphic columns of the Rustler Formation are contained in Holt and Powers (1988).

Reference

Holt, R.M., and D.W. Powers. 1988. *Facies Variability and Post-Depositional Alteration Within the Rustler Formation in the Vicinity of the Waste Isolation Pilot Plant, Southeastern New Mexico.* DOE/WIPP-88-004. Carlsbad, NM: Westinghouse Electric Corporation.

Comment T097

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology, General

Area of document Volume 2, Chapter 2, Page 2-19

"Use of tools like Schlumberger's Formation MicroScanner may help to characterize *in situ* fractures in most stratigraphic units at the WIPP."

Response

New geophysical tools such as FMS are being evaluated for their potential use during tests associated with new tracer tests in the Culebra dolomite (Beauheim and Saulnier, 1994).

Comment T098

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-24, Line 27

"'These rocks are absent...' Which rocks are absent: the Dewey Lake Red Beds? or the Dockum Group?"

Response

The Dockum Group (which includes the Santa Rosa sandstone). We recognize that the subject sentence had an unclear antecedent.

Comment T099

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 2-39, Line 26

"Will the effect of gases on K_ds for radionuclides be considered in experiments?"

Response

Gases produced in the WIPP disposal room may include hydrogen, methane, carbon dioxide, hydrogen sulfide, nitrogen, and ammonia from corrosion and microbial processes (see Brush, 1990). The primary gasses produced, however, are likely to be hydrogen, carbon dioxide, and methane. Quantification of the gases produced by corrosion and microbial degradation is currently being investigated. The disposal room is expected to become anoxic in a relatively short period of time. In the event of a human intrusion, an influx of radionuclides into the Culebra Dolomite would be accomplished by some of those disposal-room gases.

The effect is expected to be negligible. In order for gases to affect sorption processes (includes adsorption and ion exchange), the dissolved gas must interact with either the dissolved radionuclides or with species on mineral surfaces. Methane is not recognized as a strong complexant for dissolved ions, nor is it likely to interact with mineral surface species. The effect of hydrogen is indirect, in that it affects pH, which plays a strong role in controlling the reactivity of mineral surface species and the extent of complexation in solution. As mentioned in the response to an earlier comment, however, pH is constrained by the presence of vast quantities of carbonate minerals in the Culebra Dolomite. An influx of hydrogen from the disposal room cannot significantly perturb the natural equilibrium in the Culebra, unless massive quantities are introduced in a short time. Like hydrogen, the concentration of carbon dioxide is strongly tied to pH, which is constrained in the Culebra Dolomite. The range of pCO_2 is limited to between about $10^{-3.5}$ atm (atmospheric pCO_2) and about $10^{-2.5}$ atm (10x atmospheric) based on equilibrium calculations (see Siegel et al, 1991, Chapter 2). However, both pH and pCO_2 are experimental parameters in the mechanistic adsorption experiments being conducted on dolomite and corrensite.

Reference

Siegel, M.D., S.J. Lambert, and K.L. Robinson, eds. 1991. *Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Comment T100

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 4-4, Lines 17-19

"The wording suggests that the effects of pluvial periods (which would increase recharge and thereby increase the hydraulic gradient and shorten transport time to the accessible environment), although incorporated into the base case scenario, are not used in transport calculations in the disturbed scenario. The reasons for the differing treatment should be explained."

Response

The sentence in question is badly worded and is misleading. All events and processes included in the base case are also included in all disturbed-performance scenarios. The intended point was that because they are included in all scenarios, they are not used in the process of distinguishing between the disturbed-performance scenarios.

Note that the parenthetical observation in the comment, although logical, is in part unsupported speculation. Increased precipitation would very likely lead to an increase in recharge (dependent on other factors such as temperature, topography, and plant cover). However, increased recharge need not lead to an increase in hydraulic gradient and shorter transport times. Three-dimensional modeling of the regional flow system is in progress to provide the basis for observations of this sort.

Comment T101

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 4-7, Lines 18-20, and Figure 4-1

(Paraphrase) The EPA notes that the water quality in the Culebra may be adequate for dust control, oil field injection, and fire control. "A requirement that the water be potable for human or stock use (which is implied by this statement) is not necessarily a prerequisite for all water-well development."

Response

This suggestion will be considered for inclusion in the systems prioritization.

The statement does not imply a requirement of potability; the E3 event is withdrawal for any use (see the reference on page 4-4, line 12 of Volume 2).

Comment T102

Page 23: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 2, Page 7-5, Line 4

"The solubility of gas in brine is assumed to be negligible. This requires justification. Abundant experimental and theoretical data exist for gas solubilities in water and brines and should be used to form a basis for this claim. Transport of gas out of the repository as dissolved species may turn out to be the most important means of gas transport in the WIPP environment, particularly as pressures increase from atmospheric to near lithostatic as the salt collapses on the waste panels."

Response

Information on the solubility of gases in NaCl brines has been evaluated and published by Cygan (1991). The transport of gas out of the repository as dissolved gas is not currently modeled in PA. The PA model (BRAGFLO) has the capability to consider gas solubility in brine, but preliminary evaluation suggests this to be a minor secondary effect. A more systematic analysis needs to be performed and is planned to better evaluate the assumption of negligible gas solubility. The issue will be considered for inclusion in the SP.

Reference

Cygan, R.T. 1991. *The Solubility of Gases in NaCl Brine and a Critical Evaluation of Available Data.* SAND90-2848. Albuquerque, NM: Sandia National Laboratories.

Comment T103

Page 17: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 2, Chapter 7, Page 7-1

"The last sentence on this page indicates that the storage coefficients and the Culebra thickness were treated as constants (as opposed to functions of position) in the 1992 series of calculations. How was the sensitivity of the storage coefficient evaluated in the transient transmissivity calculations described in Section 4.3 of LaVenue (SAND 92-7306)?"

Response

The storage coefficient used in the model was taken from averaging the $\log_{10}$ of the measurements taken from the Culebra tests. The sensitivity of the model results to the storage coefficient has not been investigated thus far. We believe that assigning a fixed value for storage coefficient leads to smaller travel times than would result from calibrating on storage coefficient as well as transmissivity, based on the following argument:

> Calibration of the response of wells H-15 and DOE-1 to the H-11 pumping test provides critical information on aquifer properties along potential Culebra flow paths. The drawdown at well H-15 is not well matched using transmissivity fields calibrated from steady-state data alone (LaVenue et al., 1990 SAND89-7068/1 Figure 5.6a). Simulated drawdown at H-15 might be increased by increasing transmissivity between H-11 and H-15, or by lowering the storage coefficient. The effect of adjusting only the transmissivity can be seen in Figure 4-32 of LaVenue 1992 (SAND92-7306) which shows a significant reduction in travel time due to including transient data in the calibration. If storage coefficients were also adjusted in the calibration, a smaller increase in transmissivity would be required to match the critical drawdown observations during the H-11 pumping test. The resulting travel times would therefore be larger than the travel times produced by adjusting transmissivity alone. We therefore believe that excluding storage coefficients from the calibration produces a conservative estimate for travel time.

References

LaVenue, A.M., T.L. Cauffman, and J.F. Pickens. 1990. *Ground-Water Flow Modeling of the Culebra Dolomite. Volume I: Model Calibration.* SAND89-7068/1. Albuquerque, NM: Sandia National Laboratories.

LaVenue, A.M., and B.S. RamaRao. 1992. *A Modeling Approach To Address Spatial Variability within the Culebra Dolomite Transmissivity Field.* SAND92-7306. Albuquerque, NM: Sandia National Laboratories.

Comment T104

Page 24: I. Technical Comments, D. Additional Comment on General Geology and Hydrology

Area in document Volume 3, Pages 1-36

"The nature of the post-closure repository will probably resemble a fracture network more closely than a porous media, the equation for calculating retardation factors in PANEL should perhaps be based on surface area as is described in Section 7.6.2.2 of [Volume 2]."

Response

Comment acknowledged; the discussion of the waste-mobilization model (PANEL) in Section 1.4.4 of Volume 3 is a general discussion, allowing for non-zero K_ds. In actual calculations with PANEL, the K_ds have so far been set to zero, thus simulating the retardation in a fracture network.

Comment T105

Page 24: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 1-47, Lines 30 - 32; Volume 3, Page 1-47, Lines 36-37

(lines 30 - 32) "'Geologic history of the Delaware Basin ...began...during the Ordovician period...' This is inaccurate because at least part of the Bliss Formation and lower Ellenburger Group are Cambrian in age."

(lines 36-37) "'...the Central Basin Platform uplifted during the Pennsylvanian Period..' Subtle uplift of the Central Basin Platform probably began as early as the Mississippian (WU 86) and continued on into the Wolfcampian (early Permian) (Wu 86, Ya 93, YaDo 92, YaDo 93)."

Response

Additional detail will be included in the PTB report, as appropriate. Regardless of the impact on compliance or lack of same, we must ensure that what is stated in the PTB report and/or compliance submittal is technically correct, to the extent possible. Some level of disagreement in some areas of geologic work will always be present.

We recognize that there is some uncertainty concerning the ages of the Bliss and Ellenburger formations, and that the terminology regarding the beginning of the history of the "Delaware Basin" is open to different interpretations. We also recognize that there is some uncertainty regarding the timing of uplift of the Central Basin Platform. We were previously unaware of the reference (Wu86), and appreciate the information.

In general, the point we would like to raise is that some level of uncertainty and/or disagreement about some aspects of geologic history in a region as large as the Delaware Basin is unavoidable, and should be acceptable, unless it has a clear bearing on regulatory compliance. It is not clear that the level of disagreement indicated in this comment has a direct role in compliance evaluation. However, we recognize that, certainly in areas having a direct role in compliance, no information which is demonstrably incorrect should be stated, no matter how "minor."

Comment T106

Page 24: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-12

"Relative permeabilities are somewhat dependent on the nature of the two phases present. For which liquid and gas compositions have the relative permeabilities curves been measured experimentally and/or calculated?"

Response

The Project is currently investigating the issue as discussed in the EPP, Section 5.1.3, Salado Hydrologic Properties Activity.

Relative permeability curves have not been measured experimentally for WIPP-specific materials, although laboratory studies are planned (Howarth, 1993). The absence of such measurements is recognized as a significant source of uncertainty in present analyses. Current relative permeability calculations rely on models developed by Brooks and Corey (1964) and by van Genuchten (1980) and Parker et al. (1987). Brooks and Corey developed their model from relative permeability data using oil and air as the fluids. Van Genuchten used some of the Brooks and Corey data with oil and air as the fluids as well as some other data with unspecified fluids. The Parker et al. (1987) extension to van Genuchten for the nonwetting phase relative permeability did not include any data-model comparisons in their development.

References

Howarth, S.M. 1993. *Conceptual Plan: Two-Phase Flow Laboratory Program for the Waste Isolation Pilot Plant.* SAND93-1197. Albuquerque, NM: Sandia National Laboratories.

Brooks, R.H., and A.T. Corey. 1964. "Hydraulic Properties of Porous Media," *Hydrology Papers No. 3.* Colorado State University.

van Genuchten, M.Th. 1980. "A Closed-form Equation for Predicting the Hydraulic Conductivity of Unsaturated Soils," *Soil Sci. Soc. Am. J.* Vol. 44, 892-898.

Parker, J.C., R.J. Lenhard, and T. Kuppusamy. 1987. "A Parametric Model for Constitutive Properties Regarding Multiphase Flow in Porous Media," *Water Resour. Res.* Vol. 23, No. 4, 618-624.

Comment T107

Page 24: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-43

"The 'disturbed porosity' in halite and polyhalite within the Salado formation is, at best, an estimate. *In situ* data may be necessary to refine estimates for this parameter."

Response

The Project acknowledges that this parameter is estimated. Currently there are no plans to collect *in situ* data.

Comment T108

Page 24: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-61

"The 'disturbed permeability' in anhydrite layers within the Salado formation is only an estimate. *In situ* data may be necessary to refine estimates for this parameter."

Response

The Project acknowledges that this parameter is estimated. Currently there are no plans to collect *in situ* data.

Comment T109

Page 24-25: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3 Page 2-72, Line 23

"'The cause of fracturing (in the Culebra Dolomite), however, is unresolved.' A better understanding of the origin of fractures in the Culebra Dolomite would help constrain the models on the flow of groundwater and brine."

Response

The reviewer's comment is valid. However, the sentence referred to in Volume 3 is not particularly informative. Considerably more information is known about Culebra fracturing than is implied here. See, for example, the discussion in Volume 2 of the 1992 PA (p. 2-16, line 33 through p. 2-23, line 13) and the references cited therein for more information on the relationship between fractures and hydrologic properties of the Culebra. Questions remain about the precise origin of fractures in the Culebra (and in most geologic environments). Present work emphasizes the relationship between the present condition of fractures (rather than their origin) and hydrologic properties. Regional 3-D groundwater flow modeling in progress (EPP 5.1.1.2) uses the present spatial distribution of fracturing in the Rustler Formation and Dewey Lake Red Beds as a basis for characterizing hydraulic conductivity.

Comment T110

Page 25: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-77 (Figure 2.6-2)

"No well control points or total depths are indicated on this cross section."

Response

The point is well taken. Control points are given for ERDA-9, although in a way that is nonstandard for most geologists. The caption should have noted that this is a schematic cross-section.

Comment T111

Page 25: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 3, Page 2-77

"Clay filling in fractures through the Culebra Dolomite is poorly characterized. Fracture aperture, clay (and other phases) mineralogy, clay volume, and petrographic relationships are not fully documented. These may affect transport of radionuclides through permeable fracture systems in the Culebra Dolomite."

Response

A significant amount of effort has been given to investigating the sedimentology and petrology of the Culebra (see Powers et al. [1978]; Ferrall and Gibbons [1980]; Sewards et al. [1991]; Siegel et al. [1990]; Krumhansl et al. [1990]; Sewards et al. [1992]; Sewards [1991]; Holt and Powers [1986]; Holt and Powers [1988]; Holt and Powers [1990]; Chaturvedi [1987]; Lowenstein [1987]; U.S. DOE [1984]). A concise and accurate summary of this work can be found in Volume 2 of the 1992 PA, p. 2-19 and 2-23. As is stated there, "...clay fracture-linings may play an important role in the chemical retardation of radionuclides during potential transport...." A significant amount of information exists, and is being assimilated in conjunction with additional, more specific information that has not yet been published. This work will be published at a future date. The sensitivity of compliance to this issue will be considered for inclusion in the systems prioritization.

References

Powers, D.W., S.J. Lambert, S-E. Shaffer, L.R. Hill, and W.D. Weart, eds. 1978. *Geological Characterization Report Waste Isolation Pilot Plant (WIPP) Site, Southeastern New Mexico.* SAND78-1596. Albuquerque, NM: Sandia National Laboratories.

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Sewards, T. 1991. *Characterization of Fracture Surfaces in Dolomite Rock, Culebra Dolomite Member, Rustler Formation.* SAND90-7019. Albuquerque, NM: Sandia National Laboratories.

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Comment T112

Page 25: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 3, Page 2-81

"How was "clay porosity" measured?"

Response

Clay porosity was not measured. It was estimated by WIPP investigators (see 1992 PA, Volume 3, p. A-130).

Comment T113

Page 25: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Chapter 2, Page 2-83, Lines 9-12

"'For many of the wells, a large amount of core was lost in porous (vuggy) and/or fractured portions of the Culebra Dolomite Member. Thus only intact matrix porosity is reported here.' Vuggy and/or highly fractured zones may control flow paths in the Culebra Dolomite and measurements of matrix porosity may be inconsequential."

Response

The Project agrees that vuggy and/or highly fractured zones probably control flow paths in the Culebra, but this does not make matrix porosity irrelevant. Matrix porosity is still important because it represents the potential storage reservoir available for diffusion of radionuclides from the fractures. This diffusion will act to retard transport of radionuclides to the accessible environment. In addition to the physical retardation provided directly by matrix diffusion, sorption occurring following diffusion into the matrix porosity will provide a chemical retardation mechanism. See also response to comment in EPA document, p. 21 (I. Technical Comments, C. General Geology and Hydrology, Culebra Matrix Porosity).

Comment T114

Page 25: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-85, Lines 4-5

"Why did the PA models use only horizontal fracture sets? This seems unrealistic and could lead to very inaccurate model results."

Response

Comment acknowledged. The models in question (SECO2D and SECO/TP) are briefly described in Sections 1.4.5 and 1.4.6 of Volume 3 of the 1992 PA, and in more detail in Sections 7.5, 7.6 and Appendix C of Volume 2.

When modeling a formation as a dual-porosity continuum, the actual orientations of fractures are unimportant. The modeled fractures are not used to provide connections between points. The important fracture parameters to be captured in the models are the fracture porosity, which will control the flow velocity, and the surface area of the fractures, which will control the amount of matrix diffusion that occurs. As long as the fracture porosity and surface area are kept constant, it does not matter if the fractures are modeled as single sets of horizontal or vertical fractures or as three orthogonal sets of fractures. Single sets of horizontal fractures are the simplest to model, so that is what PA has used.

Comment T115

Page 25: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 2-92/101

"Partition coefficients of radionuclides are very poorly constrained and further experimental work is necessary."

Response

An experimental program designed to address retardation is planned. This program includes

- Mechanistic adsorption studies (surface complexation modeling) on primary mineral constituents of the Culebra,
- Column experiments with intact Culebra core, and, if needed and justified
- Field sorbing multi-well tracer test.

The model describing actinide sorption to be developed from these experiments will provide data to PA.

Comment T116

Page 25-26: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 3-17

"In the Waste Retrieval Plan (DOE 93), the depth of the repository is 2,150 feet below the surface, which is the depth at the floor or sill. In the performance assessment (Volume 3), the surface elevation is given as 1,023.3 meters and the repository level at 379.0 meters or a depth of 644.3 meters, which equates to 2,114 feet -- a difference of 36 feet. Other differences include the thickness of the Magenta member of the Rustler Formation, listed as 25 feet thick in Volume 3 and ranging from 22 to 35 feet thick in reference No 90; and the thickness of the Culebra member, listed as 23 feet in Volume 3, and ranging from 22 to 29 feet in reference No. 90."

Response

We recognize that some Project-internal contradictions in quoted elevations/depths within the WIPP repository have arisen through time. As part of the plugging and sealing program at Sandia, the Project has initiated the effort of standardizing the reference depths, especially in the WIPP shafts. It should also be noted, however, that there is some variability in both depth and elevation within the repository, due to the slight dip of both the Salado Formation and the land surface.

Table 2.6-1 (pg. 2-76) of Volume 3 lists a thickness range for the Culebra of from 5.5 to 11.3 m. However, this thickness range was not sampled on for these calculations. Instead, as noted on page 2-72 of Volume 3, the "PA department has chosen 7.7 m as a reference thickness." A similar situation exists in the case of the Magenta dolomite, although it is not considered as a potential radionuclide-release pathway in these calculations.

Comment T117

Page 26: I. Technical Comments, C. General Geology and Hydrology

Area in document Volume 3, Page 3-37

"It is unclear how the range for the free-liquid diffusion coefficients in Table 3.3-4 were determined. Typically, free-liquid diffusion coefficients are determined in low ionic strength solutions which precludes competition among ions. However, diffusion coefficients for the radionuclides at WIPP should be determined under expected salinities to ensure that diffusion is not overestimated."

Response

The source of the free-liquid diffusion coefficients in Table 3.3-4 is Table E-7 of Lappin et al. (1989). A discussion of the uncertainties in these numbers is given in Section E.2.4.2 of the same document. Please refer also to: Dykhuizen and Casey (1989).

References

Lappin, A.R., R.L. Hunter, D.P. Garber, and P.B. Davies, eds. 1989. *Systems Analysis, Long-Term Radionuclide Transport, and Dose Assessments, Waste Isolation Pilot Plant (WIPP), Southeastern New Mexico; March 1989.* SAND89-0462. Albuquerque, NM: Sandia National Laboratories.

Dykhuizen, R.C., and W.H. Casey. 1989. *An Analysis of Solute Diffusion in the Culebra Dolomite.* SAND89-0750. Albuquerque, NM: Sandia National Laboratories.

Comment T118

Page 26: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 3-37

"The last sentence in the first paragraph which states "Because of the improbability of developing interconnected vertical fractures in plastic halite, this pathway is not modeled in performance assessment." needs to be supported and a reference included."

Response

Initial justification for not including vertical crack development is provided in Section 5.2.1.1, pages 5-23 to 5-27 in Butcher and Mendenhall (1993). This question raises issues that will be considered for inclusion in the system prioritization.

Reference

Butcher, B.M., and F. T. Mendenhall. 1993. *A Summary of the Models Used for the Mechanical Response of Disposal Rooms in the Waste Isolation Pilot Plant with Regard to Compliance with 40 CFR 191, Subpart B.* SAND92-0427. Albuquerque, NM: Sandia National Laboratories.

Comment T119

Page 26: I. Technical Comments, D. Additional Comments on General Geology and Hydrology

Area in document Volume 3, Page 5-3 (Figure 5.1-2) and Volume 3, Page 5-4 (Figure 5.1-3)

"Volume 3, page 5-3 (Figure 5.1-2). No contour interval is given for this map. It may be obvious, but the contour interval should still be indicated."

"Volume 3, page 5-4 (Figure 5.1-3). No contour interval is given for this map. It may be obvious, but this contour interval should still be indicated."

Response

Comment accepted. In Figure 5.1-2, the contour interval is 100 m. In Figure 5.1-3, the contour interval is 50 m, with the even 100-m intervals being indicated by a heavier line.

Comment T120

Page 26: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Time of intrusion for Modeling Purposes

Area in document Volume 1, Chapter 4, Page 4-8, Line 11

"The PA should model subsurface radionuclide releases at more than the one intrusion at 1000 years. What support does DOE have for not including additional releases? Have different time periods been modeled and put into a sensitivity analysis?"

Response

The sampling of intrusion times may be needed for a complete analysis that would be used for compliance application. The 1992 PA was not intended as a compliance application. The decision to limit subsurface release calculations to a single time of intrusion was based solely on resource limitations in 1992.

The 1991 PA included analysis of subsurface releases from 5 times of intrusion (1,000, 3,000, 5,000, 7,000, and 9,000 yr). Comparison of figures 4.1-2 (lower right frame) and 5.1-4 (upper right frame) of Volume 4 of the 1991 PA (SAND91-0893/4) shows the effect of later times of intrusion on dual-porosity subsurface releases. The 1990 PA sampled the time of intrusion (rather than including it in the probability model), and results of a formal sensitivity analysis including time of intrusion are published in Helton et al. (1991).

Reference

Helton, J.C., J.W. Garner, R.D. McCurley, and D.K. Rudeen. 1991. *Sensitivity Analysis Techniques and Results for Performance Assessment at the Waste Isolation Pilot Plant.* SAND90-7103. Albuquerque, NM: Sandia National Laboratories.

Comment T121

Page 27: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Human Intrusions in 10,000 Years

Area in document Volume 3, Chapter 2, Page 2-3 (Figure 2.1.3)

"On Figure 2.1-3, are these DOE or commercial wells?"

Response

They include both. Note that this is not an exhaustive listing of wells. As the caption indicates, it is a map of wells that provided information about general stratigraphy. Note also that the total depth of these wells is not given. Many are wells drilled for potash exploration, and do not penetrate the repository horizon. These wells should not be included in an attempt to estimate the probability of penetrating the repository.

Comment T122

Page 27-28: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Human Intrusions in 10,000 Years

Area in document Volume 3, Chapter 2, Page 2-3 (Figure 2.1.3)

a) "Assuming that the wells are commercial wells, a simple calculation of human intrusion based on the boreholes shown on Figure 2.1-3 derives a value of 28 boreholes/km^2 per 10,000 years. These results are based on 459 boreholes drilled on 13356.6 km^2. It was assumed that these boreholes were drilled during 50 years."

b) "A study of oil and gas drilling will discover a cyclic nature. Early exploration will generally be shallow drilling with high density drilling for easy oil prospects, then this phase will subside. After a time of hiatus, the next phase will progress to deeper oil and gas prospects with additional increased density drilling. This phase may include enhanced recovery from old shallow production such as steam or water flood techniques, this may also increase drilling density. Then with advancements in technology, after another hiatus, enhanced recovery techniques will progress and deeper more aggressive prospects will be drilled."

c) "The number of boreholes drilled per area may increase with time, potentially to very high densities. The oil fields of Pennsylvania, which are more than one hundred years old, may provide a way to gain an understanding of this cycle of drilling."

Drilling Rate Model

d) "The expert panel results lead to a significantly lower number of boreholes per square kilometer in 10,000 years than suggested in the Appendix B Guidance to 40 CFR 191. The probability of zero intrusions over 10,000 years is increased from 2.4 percent for a constant drilling rate of 3.28×10^{-4} per year (Volume 4, Table 2.5-2) to 87 percent if the time-dependent drilling-rate function with the highest cumulative number of intrusions is used (Volume 4, Table 2.5-1). The use of "expert" opinion to reduce the drilling rate by this amount appears to belie the original intention of including the guideline of 30 boreholes per square kilometer in 10,000 years. Other expert panels could be commissioned to devise additional future scenarios leading to lower or higher drilling rates, a situation which the guideline intended to avoid."

e) "As mentioned above, the maximum value for the drilling-rate parameter used in both the 1991 and 1992 PAs is 3.28 x 10^{-4}/year. This is equivalent to a disposal area of approximately 0.11 km^2. Yet the 1991 and 1992 PA used 0.5 km^2 as an approximate disposal area. In addition, the 1992 PA (Volume 1, page 5-3) notes that of the seventy sample vectors of inputs used in the 1991 study, the maximum number of boreholes obtained by the sampling procedure as implemented was equivalent to a drilling rate of only 20 boreholes/ km^2 in 10,000 years. This discrepancy was noted in the comments received from EEG (Volume 1, p. B-19, Comment 22) and has not been addressed adequately in the current PA."

Response

Performance assessments conducted for the WIPP have made every effort to be consistent with the provisions of 40 CFR 191 and with the guidance in Appendix C (previously Appendix B) and in the Supplementary Information published in the Federal Register with 40 CFR 191.

The reviewer's observations in parts b) and c) about exploration practices are useful.

a) We were not able to calculate 28 boreholes/km^2 per 10,000 years from the numbers provided. Using your value of 459 boreholes with an area of around 4000 km^2 for Figure 2.1-3, yields a value of approximately 22-23 boreholes/km^2 per 10,000 years. However, many of the wells in Figure 2.1-3 would not be included according to Appendix C and the supplemental information (also see response to previous comment).

An evaluation of drilling rates for areas underlain by bedded salt indicates a value of approximately 20 boreholes/km^2 per 10,000 years for New Mexico (Cranwell et al., 1990, Appendix C).

b) The discussion of the cyclic nature not just of exploration, but of development appears to be in conflict with guidance in Appendix C (previously Appendix B) that only exploratory boreholes are to be considered, not exploratory and development boreholes:

> "Therefore, inadvertent and intermittent intrusion by exploratory drilling for resources (other than any provided ;by the disposal system itself) can be the most severe intrusion scenario assumed by the implementing agencies."

Discussions of drilling practices and rates should carefully distinguish between exploratory wells and development wells.

The discussion of initial shallow drilling highlights a second important point. Shallow wells that do not interfere with the performance of the repository would not constitute human intrusion. These shallow wells should not be included in the calculation/consideration of boreholes/km^2 per 10,000 years.

d) There may be some misunderstanding about the use of expert judgment and the "original intent" of the guidance in Appendix C. referring to the guidance about both the "chance and consequences of intrusion" contained in the Appendix, the Supplementary Information to the 1985 Standard States:

> "The implementing agencies are free to use other assumptions if they develop information considered adequate to support those judgments" (50 FR 38080b).

Clearly, the EPA did not intend at that time to require the DOE to include 30 boreholes per square kilometer per 10,000 yr. The reviewer's comment here may imply an extension of the existing regulatory requirements.

Specifically, the following response to the reviewer's comments that "The expert panel results lead to a significantly lower number of boreholes per square kilometer in 10,000 years than suggested in the Appendix B Guidance to 40 CFR 191," and "The use of 'expert' opinion to reduce the drilling rate by this amount appears to belie the original intention of including the guideline of 30 boreholes per square kilometer in 10,000 years" is based on previously published EPA guidance.

Guidance from the EPA in Appendix C (previously Appendix B) indicates that the DOE, as the implementing agency:

> "*...should consider the effects of each particular disposal system's site, design, and* passive institutional controls in judging the likelihood and; consequences of such inadvertent exploratory drilling."

There is additional explanatory text in the Supplementary Information published in the Federal Register with 40 CFR 191 that indicates an appreciation that the value of 30 probably would and should be reduced:

> "However, assessing the ways and the reasons that people might explore underground in the future--and evaluating the effectiveness of passive controls to deter such exploration near a repository--will entail informed judgement and speculation. It will not be possible to develop a 'correct' estimate of the probability of such intrusion. The Agency believes that performance assessments should consider the possibilities of such intrusion, but that limits should be placed on the severity of the assumptions used to make the assessments. Appendix B to the final rule describes a set of parameters about the likelihood and consequences of inadvertent intrusion that the Agency assumed were the <u>most pessimistic that would</u>

be reasonable in making performance assessments [emphasis added]. The implementing agencies may adopt these assumptions or develop similar ones of their own." (50 FR 38077a)

Making assumptions allowing the effect of markers to lower the probabilities is consistent with text in the *Supplementary Information:*

"Therefore, determining compliance with the standards involves performance assessments that consider the probabilities and consequences of a variety of disruptive events, including potential human intrusion. Not allowing passive institutional controls to be taken into account to some degree when estimating the consequences of inadvertent human intrusion could lead to less protective geologic media being selected for repository sites." (50 FR 38080b,c)

The reviewer's comment that "Other expert panels could be commissioned to devise additional future scenarios leading to lower or higher drilling rates, a situation which the guideline intended to avoid" is inconsistent with EPA guidance. Guidance in the Supplementary Information quoted above clearly states that "informed judgment and speculation" would be involved and indicates that the impact of passive institutional controls should be taken into account when considering the likelihood of inadvertent human intrusion. In addition, the guidance clearly states that in fact there is no 'correct' value and apparently one was expected.

e) *The points raised in the final paragraph call attention to errors and a lack of clarity in the* PA documentation. The maximum rate constants were not the same in 1991 and 1992 (compare Table 2.3-1 in the 1991 V.2 and Table 2.5.2 in the 1992 V.4), although the difference is not significant. Both rate constants yield expected values of 30 boreholes/km^2 per 10,000 years. They differ only because the target area of the waste was adjusted from 0.109 km^2 in 1991 to 0.126 km^2 in 1992, reflecting the inclusion of the area occupied by horizontal RH-TRU waste emplacement. References to 0.5 km^2 in both the 1991 and 1992 PAs are errors. That area includes the enclosed volume (the pillars), and was not used in probability modeling. The statement that a maximum of 10 intrusions occurred in the 1992 PA is correct. However, those intrusions occurred into 0.126 km^2 rather than into 0.5 km^2. This corresponds to 79 intrusions in 1 km^2, more than twice the number suggested in the EPA guidance.

Comment T123

Page 28: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments

Area in document Volume 1, Page 4-6

"In the E1 and E2 scenarios, any plugs between the repository and the Culebra are assumed to fail immediately..." Figure 4.2-2 on page 4-6 of [Volume 3] shows permeability remaining constant for 75 years at 10^{-18} m^2 and increasing to 10^{-12} m^2 at 150 years after intrusion. Which values were used in the calculation?"

Response

The 1992 PA assumed that the borehole permeability immediately following intrusion was that of a silty sand, lognormally distributed with a range of 10^{-14} to 10^{-11} m^2 (see page 4-3 of Volume 3 of the 1992 PA). Figure 4.2-2 was included to provide background information.

Comment T124

Page 28: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments

Area in document Volume 1, Page 4-9, Lines 12-14. "It is not clear how these values for number of intrusions are related to the 70 realizations shown in Appendix D of Volume 3 where the largest number of intrusions in 10,000 years was slightly greater than one per square kilometer."

Area in document Volume 1, Page 5-3, Lines 35-37. "It is stated here that "the largest number of intrusions in the 0.5 km^2 of the waste-disposal area was 10..." The largest number shown for the 70 realizations shown in Volume 3, Appendix D, was slightly greater than 1 for one square kilometer. Why don't these numbers agree? In addition why does 0.5 km^2 appear to be used when the waste area has a footprint of about 0.11 km^2 (Volume 3, Table 3.1-1)? (As an aside it should be noted that the shifting frame of reference from holes per km^2 to holes per 0.5 km^2 creates further confusion in understanding an already complex subject.)"

Response

The discussion in the 1992 PA was less than clear.

First, all references to 0.5 km^2 were incorrect. The target area for the waste used in the 1992 PA was 0.126 km^2.

Second, the curves labeled "intrusions" in Appendix D of Volume 3 should more properly have been labeled "integrated intrusion rate". The integrated rate constant does not yield the number of intrusions that are included in the PA. Rather, it yields the expected value of the Poisson equation. (See section 5.2 of Volume 2). The assumption that drilling is random in time and space (i.e., a Poisson process) means that there is no absolute upper bound on the total number of intrusions. For any nonzero rate constant, there is always some finite probability that one more intrusion will occur within the time interval. The rate constant defines the expected value of the function, not its limit. In the 1992 PA, the total number of intrusions considered in consequence analyses were limited to either those which occurred with a probability greater than 10^{-6} in 10,000 years or to the number 10, whichever occurred first. Note that 10 intrusions in 0.126 km^2 corresponds to 79 intrusions in 1 km^2.

Solving the following simplified Poisson equation

$$p[\, E_n(\Delta t)\,] = \frac{[\,\lambda(\Delta t)\,]^n}{n!} e^{-\lambda \Delta t} \qquad (Eq.\ 1)$$

for $= 3.78 \times 10^{-4}$/yr and n = 10 indicates that 10 intrusions occur in 10,000 yr with a probability of 3.75×10^{-3}. As shown in Tables 2.5-2 and 2.5-4 of Volume 4 of the 1992 PA (pages 2-24 and 2-30), the assumption of 100 years of institutional control reduces this probability to 3.50×10^{-3}. Consideration of only one time interval (0-2,000 yr) reduces this probability to 4.87×10^{-9}.

Inspection of Tables 2.5-2 and 2.5-4 of Volume 4 indicates that the maximum number of intrusions occurring in the time interval (0,2000 yr) with a probability greater than 10^{-6} is 7. Thus, the largest number of intrusions included in subsurface releases in 1992 was 7. Cuttings releases calculated for all time intervals contained up to 10 intrusions.

Comment T125

Page 29: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments

Area in document **Volume 2 Page 4-4, Lines 30-35**

"Exploratory water-well drilling into the Culebra Aquifer is reasonable at the WIPP site and has potential consequences. Also, exploratory drilling for potash could penetrate close enough to the repository level to have consequences. To date, drilling considerations have centered only on oil and gas exploratory drilling and other possibilities have largely been ignored."

Response

Scenario development is not a closed process. These suggestions will be considered for inclusion in the systems prioritization. Direct intrusion into the waste by drilling for either potash in the McNutt zone of the Salado or water in the Culebra is not a threat because both units are well above the repository horizon.

Comment T126

Page 29: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments

Area in document Volume 2, Page 4-7, Lines 8-9

"Is there a reference for the conclusion that explosions have no effect on the long-term performance of the repository?"

Response

Yes. See Chapter 4 of Volume 1 of the 1991 PA (SAND91-0893). The appropriate text is on page 4-52, lines 5-26.

Comment T127

Page 29: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Drilling Rate Model

Area in document Volume 2, Page 7-26, Line 19

"The equations presented for calculating the pressure drop for turbulent flow appear accurate. ... Muds having high salt contents seldom have high gelation properties."

Response

We agree with these observations. Activities relating to them fall under ongoing cuttings release model development and laboratory studies.

Comment T128

Page 29: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Drilling Rate Model

Area in document Volume 2, Page 7-27, Line 23

"The possibility of high pressure gas being created by the waste material and eventually flowing into the wellbore is an important factor. Should such events occur, they would indeed contribute to the contamination problem by compromising the stability of the borehole wall and contributing to the quantity of waste material that reaches the surface."

Response

We refer to the process described in the comment as cuttings removal by spall, and currently consider its definition our top priority with regard to cuttings release.

Comment T129

Page 29: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Drilling Rate Model

Area in document Volume 2, Page 7-28, Line 15

"The scenario presented, in which the driller gradually works his way through such a formation is questionable, and would depend on the properties of the compacted waste material. The waste material may respond as a very compacted, solidified rock and not be detected at the surface except for its radioactive properties, or it may respond as "a Montmorillonite clay suspension" with all solids suspended in a highly pressurized slurry which flows into the wellbore. This latter scenario would be very troublesome because it would not be possible to shut the well in using normal well control procedures, without fracturing other formations in the uncased portion of the hole."

Response

We agree with these observations. However, the prospect of a montmorillonite clay suspension is considered very unlikely. Activities relating to these comments fall under ongoing cuttings release model development and laboratory studies.

Comment T130

Page 30: I. Technical Comments, E. Drilling Rate and Intrusion Related Comments, Drilling Rate Model

Area in document Volume 3, Page 3-57 (Table 3.4-1)

"Drilling erosion parameters are given here. Throughout the three volumes there is considerable discussion about the theory of drilling and the various factors that determine the effective borehole diameter in waste storage rooms. However, no values for the range and average of the effective borehole diameter or the volume of cuttings being brought to the surface was found anywhere. This type of presentation makes it very difficult for a reviewer who does not actually do the calculations with the SNL codes to evaluate the reasonableness of the computations."

Response

This was indeed an oversight in Volumes 1, 2, and 3 of the 1992 PA and will be corrected in future documentation. Typically for the 1992 PA the final eroded diameter was 2-3 times the sampled drill bit diameter.

Comment T131

Page 30: I. Technical Comments, F. Source Term, Waste Related Issues

Area in document General

"The performance assessment analysis is only as good as the data on which it is based. Using the PA to identify critical or key parameters to evaluate the overall integrity of the repository is appropriate. The PA should serve as the driver to clearly identify those parameters on which activities such as waste characterization and experimental testing need to focus. For example, assumed parameter ranges, values, or repository conditions identified in the performance assessment should be tested with experiments to the extent possible. For instance, the actinide source-term has been identified as a key component to the evaluate [sic] the total performance of the repository. The source-term test program must use a waste source reflective of a comprehensive review of waste process knowledge and characterization. This requires input from the waste characterization program. A source-term test using representative or bounding waste sets the stage to study synergistic affects waste [sic] that could support or nullify the current values used in the PA. A test performed with non representative waste does not."

Response

Non-radioactive constituents of the wastes are expected to alter the brine composition (for example, dissolution of portions of cement and/or the generation of CO_2 gas from the microbial degradation of combustibles). The brine composition controls radionuclide solubilities. The Source-Term Test Program, designed to assess the appropriateness of the Actinide Source-Term Model, uses waste forms that were selected to have significant potential impacts on the brine composition, and hence on the actinide solubilities (and potential colloid formation). The DOE invites further discussion on these topics.

Comment T132

Page 31: I. Technical Comments, F. Source Term, Waste Related Issues

Area in document General

"In addition to the experimental testing program, the PA can also identify the important parameters necessary for waste characterization. Data generated from waste experiments aimed at gas generation, solubility, viscometric functions, and evaluation of chelating agents and colloid formation, needs to be verified by identifying the appropriate waste streams."

Response

Non-radioactive constituents of the wastes are expected to alter the brine composition (for example, dissolution of portions of cement and/or the generation of CO_2 gas from the microbial degradation of combustibles). The brine composition controls radionuclide solubilities. The Source-Term Test Program, designed to assess the appropriateness of the Actinide Source-Term Model, uses waste forms that were selected to have significant potential impacts on the brine composition, and hence on the actinide solubilities (and potential colloid formation). The DOE invites further discussion on these topics.

Comment T133

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Page 31: I. Technical Comments, F. Source Term, Waste Related Issues

Area in document Volume 2, Page 7-27, Line 31

"It is mentioned that the waste produced as a result of spalling is very dependent on the constitutive nature of the compacted composite waste. DOE should address in the PA its approach for determining how assumed values or ranges of factors such as viscometric functions of the waste will be supported by information obtained by characterizing waste. This would determine the complete response of such compacted waste materials, should it be over pressured with gas. Such an occurrence is similar to what happens in normal oil and gas drilling when "unconsolidated sandstone" formations are penetrated. In such cases, if the wellbore pressure is less than that of the fluid pressure in the unconsolidated sand, immediate flow occurs, the well is blocked and prompt remedial action must be taken. Other formations which exhibit similar behavior are thick salt beds drilled at great depths, and "gumbo shale" drilled at shallow depths."

Response

We agree with these observations and they are included in our thinking in regard to direction of the ongoing cuttings release model development and laboratory studies.

Comment T134

Page 31: I. Technical Comments, F. Source Term, Uncharacterized RH-TRU Waste

Area in document Volume 2, Page 2-51, Line 19

"There is very little information about the RH-TRU waste inventory."

Response

From a Performance Assessment (PA) perspective, large quantities of data are not required for RH TRU. The waste streams are not significantly different from CH TRU except for the fuel examination samples. There is , of course, another difference - the radioactive component, which is stated relative to PA: otherwise it is only an operational concern.

Comment T135

Page 31: I. Technical Comments, F. Source Term, Uncharacterized RH-TRU waste

Area in document Volume 2, Chapter 2, Page 2-51, Line 19

"What does 'uncharacterized waste' mean in regards to RH-TRU waste?"

Response

The category "uncharacterized waste" is mentioned in the 1991 IDB (U.S. DOE, 1991); it applies to wastes that are suspected to be TRU contaminated materials but whose other characteristics are presently unknown.

Reference

U.S. DOE (Department of Energy). 1991. *Integrated Data Base for 1991: U.S. Spent Fuel and Radioactive Waste Inventories, Projections, and Characteristics.* DOE/RW-006, Rev. 7. Oak Ridge, TN: Oak Ridge National Laboratory.

Comment T136

Page 32: I. Technical Comments, F. Source Term, Solubility

Area in document Volume 2, Chapter 2, Page 2-52

"What data are available on solubilities of the target elements (& their various species)?"

Response

Additional detail will be included in Project Technical Baseline.

Some data exist for solubilities of target elements in specific brine compositions, but these data cannot be generalized to cover all expected changes in brine compositions due to waste interactions with brines (including gas generation). The solubility model under development is designed to calculate the variation of actinide solubilities as a function of brine composition, which in turn will allow solubility analyses to be related to the types of waste in the repository. Large amounts of data have been generated in this program, and these data are being used to develop a numerical model. The Project is working to assimilate existing information and gather the new information necessary to complete the solubility model. The relative importance of the experimentally based actinide concentration information compared with other information needed for the performance assessment will be considered for inclusion in the systems prioritization analysis.

Comment T137

Page 32: I. Technical Comments, F. Source Term, Solubility

Area in document Volume 2, Chapter 2, Page 2-52

"Will the source term expert panel be superseded by actual data on solubilities and colloids? When?"

Response

The objective of the ongoing Actinide Source-Term Program is to provide model predictions of actinide concentrations in WIPP brines to the performance assessment process, where the model output includes both solubilities and colloids and the model is based on experimental data. The current plan is to use this experimentally based actinide concentrations model to support the compliance application. The relative importance of the experimentally based actinide concentration information compared with other information needed for the performance assessment included in the SP analysis.

Comment T138

Page 32: I. Technical Comments, F. Source Term, Quantity and General Form of Waste

Area in document Volume 3, Chapter 3, Page 3-9, 10.

"Please identify the quantity of radionuclides and the general form that they are in, e.g., sludge, on rags, etc. How would this effect the PA?"

Response

The quantities of radionuclides assumed in the 1992 PA are listed in Table 3.3-1 of Volume 3 of the 1992 PA; the assumed physical compositions of both CH-TRU and RH-TRU wastes are stated in Section 3.4 (see especially Table 3.4-1) of that report.

The 1992 series of sensitivity and uncertainty analyses did not investigate the effects of uncertainty in radioactivity content of the waste. The effects of uncertainty of physical compositions (varying volume fractions of cellulosics, metallics and sludges) were investigated and results of these investigations may be found in Table 9-3 of Volume 4 of the 1992 PA.

Comment T139

Page 32: I. Technical Comments, F. Source Term, Quantity and General Form of Waste

Area in document Volume 1, Page 5-4

"How sensitive are the cuttings release estimates to the waste inventory and waste form?"

Response

Formal sensitivity analyses have not been performed using an uncertain radionuclide inventory. In general, integrated normalized radionuclide releases are relatively insensitive to changes in the inventory because they are normalized to the total regulated curie content of the system. This effect can be observed informally by comparing cuttings releases from the 1991 and 1992 PAs, as shown in Figure 4.1-2 of Volume 4 of the 1991 PA (SAND91-0893/4) and Figure 8.2-3 of Volume 4 of the 1992 PA. Despite differences between 1991 and 1992 in both the total number of curies considered and the relative abundance of specific radionuclides, and the inclusion of early-time intrusions in 1992, cuttings-only CCDFs for the two PAs lie within an order of magnitude of each other.

Waste form may affect cuttings releases by influencing the amount of waste eroded by circulating drilling fluid or spalled into the borehole. The 1990 PA examined sensitivity of cuttings releases to a change in effective waste shear strength from 1 Pa to 5 Pa, and observed little effect (Bertram-Howery et al., 1990, Figure VI-5). Subsequent PAs have used a fixed value of 1 Pa for waste shear strength. Other values of waste shear strength have not been examined in full PAs. Sensitivity of eroded borehole diameter to changes in borehole roughness and waste shear strength has been reported by Berglund (1992, Figures 2-5 and 2-6). Modeling and experimental work in progress (EPP 5.4) will provide additional information about the bounding cuttings releases. More detail will be provided in the PTB report.

References

Bertram-Howery, S.G., M.G. Marietta, R.P. Rechard, P.N. Swift, D.R. Anderson, B.L. Baker, J.E. Bean, Jr., W. Beyeler, K.F. Brinster, R.V. Guzowski, J.C. Helton, R.D. McCurley, D.K. Rudeen, J.D. Schreiber, and P. Vaughn. 1990. *Preliminary Comparison with 40 CFR 191, Subpart B for the Waste Isolation Pilot Plant, December, 1990.* SAND90-2347. Albuquerque, NM: Sandia National Laboratories.

Berglund, J.W. 1992. *Mechanisms Governing the Direct Removal of Wastes from the Waste Isolation Pilot Plant Repository Caused by Exploratory Drilling.* SAND92-7295. Albuquerque, NM: Sandia National Laboratories.

M

Comment T140

Page 32: I. Technical Comments, E. Drilling and Intrusion Related Comments, Waste Brought to Surface

Area in document Volume 2, Chapter 4, Page 4-13

"How much waste (i.e., # of barrels and associated radiation) is assumed to be brought to the surface? What is the average and maximum curie content of the drums in these analyses? How do these values compare to the known inventory?"

Response

The volume of waste brought to the surface due to a drilling intrusion is not assumed but calculated based on several drilling parameters and the effective shear strength for erosion of the compacted decomposed waste. The computation is accomplished in the code CUTTINGS. In 1992 calculations only the drillbit diameter was sampled and all the remaining drilling parameters were set at their median values. The maximum volume computed to be released to the surface in the 1992 analyses was approximately 3.3 m^3 of uncompacted waste. The CUTTINGS code computes the curie content of the volume removed based on the best estimate of the inventory uniformly distributed over the disposal area of the repository decayed to the time of intrusion.

Transportation and WAC requirements were not explicitly considered in the formulation of the activity levels for CCDF construction, and the activities reported at later times for the highest level exceed the 200 fissile-gram-equivalent requirement of the WAC. The decision to include "nontransportable" waste in the 1991 PA was deliberate, and was based on uncertainty about future transportation requirements. (Recall that the 1991 and 1992 PAs were preliminary, and were not intended to be interpreted as compliance applications.) The effect also occurs in the 1992 PA.

The effect of the "nontransportable" waste on CCDFs is minor, and results in an overestimation of the lower-probability/higher-consequence cuttings releases. Figure 3-4 in Volume 2 of the 1991 PA (SAND91-0893/2) shows cuttings-only CCDFs calculated for a single realization using average-activity cuttings and cuttings of multiple activity levels.

Comment T141

Page 32: I. Technical Comments, G. Source Term

Area in document General

"Changes in DOE's mission are acknowledged in the 1992 Integrated Data Base (IDB 92), but only one generator (RFP) has included the effect of their waste projections and revised them accordingly. Since the initial projections of waste type, isotopic composition, production rate, and volume were based on assumptions regarding DOE's continued production of nuclear weapons and their associated support functions, these projections need to be reassessed in light of DOE's changed mission. This is not adequately addressed in the 1992 PA.

Response

The conclusion is accurate and the DOE acknowledges that additional data are required. The DOE has assigned a task to update this type of information and is planned for publication as the WIPP transuranic Waste Baseline inventory Report, Revision 1, in December, 1994. Sandia will receive the source information contained in this document by the end of November, 1994, to use in their ongoing PA analyses.

Comment T142

Page 33: I. Technical Comments. G. Source Term

Area in document Volume 1, Page 2-16

"The uncertainties associated with the source term need to be resolved. For example the PA states that 'Many of these chemicals [RCRA constituents], if present in significant quantities, could affect the ability of radionuclides to migrate out of the repository by influencing rates of degradation of the organics, microbial activity, and gas generation. The effects of these processes are being studied.' The status of these studies should be reported in the next PA."

Response

The term "chemicals" in the quoted text referred to all chemicals in the waste, and not merely the RCRA-regulated constituents. In fact, with the exception of some metals, all indications are that for most wastes, the RCRA-regulated chemicals are in insignificant quantities (Table 2-1, DOE/WIPP 89-003).

This information will be reported in future compliance applications and other documents when available.

Reference

U.S. DOE (Department of Energy). 1990. *No-Migration Variance Petition for the Waste Isolation Pilot Plant.* DOE/WIPP-003, Appendix B. Carlsbad, NM. Waste Isolation Pilot Plant.

Comment T143

Page 33: I. Technical Comments. G. Source Term

Area in document Volume 2, Page 2-47

"Using a 'scale-up' of masses estimated from expanded waste characterization information is only as accurate as the degree to which the projected waste agrees with what is produced currently. As indicated elsewhere in this report, this is very uncertain.

Response

The comment is correct. The data were entered in 1992 using available records. The DOE has initiated a task to improve the waste inventory data and has called for new projections from every site based on current site missions. This report will be issued in December, 1994, as the WIPP Transuranic Waste Baseline Inventory Report, Revision 1. A subsequent update to that document is scheduled for 1995. The 1994 data will be used for any new assessments conducted.

Comment T144

Page 33: I. Technical Comments. G. Source Term

Area in document Volume 2, Page 2-50

The PA uses the design volume for CH-TRU wastes and the RH-TRU maximum curie limit for calculations. The Integrated Data Base (IDB) in its current form cannot provide any more than a rough estimate of waste volumes, types, and isotopic compositions. Some of the sources of uncertainty in the IDB with the potential to affect the PA are as follows:

a) The rational for classifying certain RH-TRU wastes currently in interim storage at Hanford (HANF) and the Savannah River Site (SRS) is unclear. These wastes are irradiated fuel components that the sites manage as RH-TRU, apparently for administrative reasons. Based on the nature and origin of these materials they appear to better meet the definition of high-level waste as defined in the Nuclear Waste Policy Act. Should these be reclassified as other than RH-TRU, they would not go to WIPP, which could affect performance assessment by reducing the RH component of the source term for modeling purposes.

b) The accumulation of data to produce the estimates in the IDB is disconnected from those activities that can provide the best quality information regarding radionuclide inventory, i.e., waste characterization. Additionally, there are several sources of uncertainty within DOE's waste characterization program. TRU waste generators tend to take a conservative approach when categorizing wastes, leading to classification of many "suspect" wastes as TRU. It appears that as much as 37% of these wastes could be reclassified as low-level wastes, based on their radioassay.

c) The IDB contains many internal inconsistencies, i.e., Section 3.42, Table 3.13 vs. Table 3.16. etc. Site-specific radionuclide inventories are based on the information obtained from "data calls" made to the generators. These calls take the form of requests for information on current and projected radionuclide concentrations and waste volumes. Due to the site-specific differences in waste characterization, radionuclide mix and data reporting, and the need to fit diverse data into a single format, much of this information is of limited utility.

d) The 1992 IDB lists the changes relative to the 1991 IDB. Of these, the volumes of TRU waste increased 35% for SRS, 30% for HANF, and "dramatic" for LANL; projections of future wastes from RFP were reduced and small changes in current inventories were reported for all other generators except INEL, which reported no significant changes. It is difficult to conduct reasonable performance assessments when a defining characteristic of the source term, radionuclide inventory, is subject to this amount of annual change.

Response

a) Part of the weapon's program was the sectioning of the fuel rods for metalographic examination. The fuel was irradiated specifically for the production of plutonium. These slices of fuel were polished in hot cells at Hanford and Savannah River, as well as at other laboratories. The grit, polishing material, phenolic sample holders, and small fuel samples were then discarded as RH TRU waste, because of the preponderance of plutonium contamination. Most likely these will not be reclassified as high level waste by the DOE at the sites mentioned, therefore, they are maintained in the RH TRU waste inventory. Even if they were to be reclassified, there are large inventories of other wastes that are suspect RH TRU. Those will be characterized, and if RH TRU, would fill the voided capacity.

b) The estimates made in the IDB were the results of many years of accumulation of TRU wastes. Instrumentation used for classification could not always discern the lower limits applied at that time. As these wastes are retrieved, they will be assayed with modern instrumentation and if classified as low level waste, will be removed from the CH TRU inventory. There remains a large quantity of unknown waste that will come from the DOE facility decontamination and decommissioning activities, some of which will be classified as

CH TRU. In any event, there is a sufficient quantity of waste to be generated to meet the WIPP stated volume capacity.

c) The PA used the available data and the IDB happened to be the best available . The DOE has developed an updated waste inventory, based on the Federal Facilities Compliance Act mandated Mixed Waste Inventory Report and later versions of the IDB. This WIPP Transuranic Waste Baseline Report (BIR) has been generated and is in draft for its first revision, which is due to be published in December, 1994. Sandia will receive the updated data at the end of November, 1994, for their use. The data contained in this report is more uniform than the data in the previous IDBs and future data calls will be used as the same input for both the BIR and the IDB.

d) The commentor is absolutely correct. As stated, immediately above, the DOE recognized the need to improve this situation and it is being corrected with the BIR. This is not a short term effort, however. It will take time to get all the databases into a compatible form. The 1995 data call will be only one, which will be used to supply data to the other databases. The PA will be able to use updated data beginning in December, 1994.

Comment T145

Page 34: I. Technical Comments, G. Source Term

Area in document Volume 2, Pages 2-50 and 2-51

"The first two paragraphs [of Section 2.3.3 of Volume 2] correctly note the uncertainty in the future radionuclide inventory caused partially by anticipated changes in weapons production. This inventory change is likely to also change the radionuclide composition and this could affect the location of the CCDF curve. Changes in the waste form mix could also alter the fraction that escapes to the accessible environment."

Response

This comment is acknowledged.

Comment T146

Page 34-35: I. Technical Comments, G. Source Term

Area in document Volume 2, Page 2-51, Line 20-23

"The number of curies of RH-TRU in Peterson's memo is incorrect because his Table 4 does not contain all the short-lived radioactive daughters of Cs-137, Sr-90, Ru-106, and Ce-144.

The WIPP Land Withdrawal Act (PL 102-579, Section 7) limitation of 5.1 million curies in RH-TRU waste includes all the short-lived radioactive daughter products. The total in Table 4 should be 3.97 million Curies. Also, the TRU waste unit factor from Table 4 should actually be 3.91 E+04 because Cm-244 has a half-life of < 20 years and should not be included."

Response

This comment is acknowledged. Peterson's memo was prepared using the only documented material available at the time (e.g., DOE, 1991) and all of that material predated the passage of the WIPP Land Withdrawal Act (October 1992). The EPA Standard, 40 CFR Part 191, Subpart B, was the only standard guiding selection of radionuclides that should be counted among the WIPP inventory in the 1992 PA.

Reference

U.S. DOE (Department of Energy). 1991. *Integrated Data Base for 1991: U.S. Spent Fuel and Radioactive Waste Inventories, Projections, and Characteristics*. DOE/RW-006, Rev. 7. Oak Ridge, TN: Oak Ridge National Laboratory.

Comment T147

Page 35: I. Technical Comments, G. Source Term

Area in document Volume 2, Page A-3, Line 24

"It was stated previously, on page 7-5, line 4, that the amount of gas in the brine is negligible, but here it states that gas exists in the brine. Why? Is the amount of gas in the brine considered in the flow equations? Is the change in gas solubility with changing pressure considered?"

Response

The only significant effect of gas dissolved in brine recognized to date is the effect of gases such as CO_2 and H_2S on brine pH. Brine pH is extremely important to corrosion and gas generation calculations and potentially to radionuclide solubilities. The effect of dissolved gases on brine physical properties is considered to be minor, although future model calculations will address this question. Solubility data for gas dissolved in brine are summarized by Cygan (1991).

The two-phase flow equations solved by BRAGFLO and described in Volume 2, Appendix A, of the 1992 PA contain terms which account for the effect on flow of dissolving gas in brine and its gas solubility dependence on pressure. In the case of H_2, the dominant gas phase component, this effect is of a secondary nature and the term in the equation was assumed to be zero in the 1992 calculations. If significant amounts of CO_2 are present, which is likely if biodegradation occurs, then more significant amounts of dissolved gas may occur. The Project is currently investigating this effect on brine and gas flow through numerical modeling sensitivity studies.

Reference

Cygan, R.T. 1991. *The Solubility of Gases in NaCl Brine and a Critical Evaluation of Available Data.* SAND90-2848. Albuquerque, NM: Sandia National Laboratories.

Comment T148

Page 35: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-20, Line 22

"The estimated inventory also includes over 600,000 Ci each of Strontium-90 and Cesium-137 which have half-lives greater than 20 years."

Response

Inventories of Sr-90 and Cs-137 are listed in Table 3.3-1 of Volume 3. Indeed, the inventory of each of these radioisotopes is over 600,000 Ci.

Comment T149

Page 35: I. Technical Comments, G. Source Term

Area in document Volume 3 Page 3-20, Line 34

"The volume limit on RH-TRU waste is 250,000 ft^3 (about 7,080 m^3) not 250,000 m^3."

Response

Comment noted.

Comment T150

Page 35: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-21

"DOE has often used detailed data from generators and draft reports (that have not been made available to reviewers) to develop their inventory (e.g., Peterson in Appendix A cites Draft Report DOE/WIPP 91-058). It is very important that all of this input data be made accessible to EPA.

Response

All relevant information and data sources used in the demonstration of compliance will be made available to the EPA.

Comment T151

Page 35-36: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-28

"The statements made on this page about RH-TRU being of less long-term concern than CH-TRU are correct for the current understanding of the RH-TRU inventory. However, the inventory is much more uncertain than that for the CH-TRU. Little attention has been given to the behavior of an RH-TRU canister after it is placed in the wall of a CH-TRU storage room and creep closure begins. The RH-TRU canister is simply three 55-gallon drums of untreated waste placed inside a metal pipe that is sealed at each end. The waste is expected to have gas generation characteristics similar to CH-TRU waste of the same composition. The RH-TRU waste would have a surface area about 13% of the CH-TRU and this area needs to be included when calculating the number of drilling intrusions. The statements about RH-TRU will need to be verified by a more detailed analysis in the future."

Response

The gas generation potential associated with the RH-TRU waste, containers, and plugs is included in the current 1994 data base. It was not considered for the 1992 PA "snapshot".

The repository footprint used to determine the number of drilling intrusions was increased 15.5% (from 1.09×10^5 m^2 in 1991 to 1.26×10^5 m^2 in 1992) to account for emplacement of RH waste in the side walls. This larger footprint value was used in the 1992 PA calculations (Volume 3 of the 1991 PA [SAND91-0893/3], p. 5-17, line 45; Volume 4 of the 1992 PA, p. 2-20, line 35).

Comment T152

Page 36: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-36 to 3-43

"It is clear that the radionuclide solubilities estimated by various workers show a very large range, and that these solubilities are dependent on the pH and Eh of the solutions. As noted in the PA, better understanding of the radionuclide solubilities under conditions similar to those expected to exist at WIPP is essential to reducing the overall uncertainty in the PA."

Response

The objective of the ongoing Actinide Source-Term Program is to provide model predictions of actinide concentrations in WIPP brines to the performance assessment process. Where the model output includes both solubilities and colloids, the model is suitable for post-closure conditions expected to exist at WIPP, and the model is based on experimental data. The relative importance of the experimentally based actinide concentration information compared with other information needed for the performance assessment will be considered for inclusion in the systems prioritization analysis.

Comment T153

Page 36: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-55, 1st paragraph

(Restatement) The commentor notes two typographical errors on page 3-55 of Sandia WIPP Project, 1992.

Response

Thank you for pointing out these typographical errors.

Comment T154

Page 36: I. Technical Comments, G. Source Term

Area in document Volume 3, Page 3-55, 2nd paragraph

"RH-TRU waste to be emplaced at WIPP is limited by volume to 7.08 x 10^3 m^3 and by activity to 5.1x10^6 Ci."

Response

Agreed.

Comment T155

Page 36: II. Format and Content, Stand Alone Document

Area in document Throughout

"The performance assessment should be a stand alone document as much as possible. Key referenced information should be included, and references should be minimized. In order to help develop the next PA, DOE may want to develop an 'example section' for external comment."

Response

The DOE has prepared the 1994 Compliance Status Report (CSR), in part, to solicit external comment on the appropriate format and content of a compliance application. It would be helpful for EPA to revisit this comment when reviewing the CSR and indicate whether the CSR outlines an appropriate approach for a compliance application. Note that the PA is incorporated into the CSR as an integral part of compliance documentation as apposed to a stand-alone document. The usefulness of the comment as it applies to a future compliance application is acknowledged.

Comment T156

Page 37: II. Format and Content, Including Examples

Area in document Volume 2, Page 3-1, Page 6-1, Throughout

"The PA should include calculations (e.g., associated with CCDFs and Latin Hypercube Sampling) with actual data to illustrate how the mathematics were implemented."

Response

This was done in Chapter 3 of Volume 2 of the 1991 PA (SAND91-0893/2) for sample element 46. LHS input vectors and selected performance measures are reported for all sample elements in the 1992 PA in appendices to Volumes 4 and 5 of the 1992 report.

Comment T157

Page 37: II. Format and Content, CCDF Development

Area in document Volume 1, Chapter 3, Page 3-18, Line 19

"The development and construction of CCDFs should be explained in more detail, with examples actually used, in the PA to the EPA. How are computational results converted into the CCDF display format: What uncertainties are introduced?"

Response

This material can be included in the compliance application. Chapter 3 of Volume 2 of the 1991 PA (SAND91-0893/2) contains a useful discussion of this subject.

Comment T158

Page 37-39: II. Format and Content, Latin Hypercube Sampling

Area in document Volume 1, Chapter 4, Page 4-9, Line 39; Page 4-14, Line 9

"The discussion on Latin Hypercube Sampling (LHS) is incomplete. There should be an example of how it was used in the performance assessment. Does LHS introduce additional uncertainty into the PA?"

"Please provide evidence that 4/3 times the number of uncertain variables is sufficient for Latin Hypercube Sampling procedures. The reference quoted in the PA provides no further information on the specific criteria."

Measures of Sampling Error for Estimated Curves

"The lack of information on sampling error for LHS-derived estimates is a serious omission in the current PA methodology. When summary curves, such as the mean or percentile CCDFs, are generated from the set of LHS CCDFs, a procedure for determining the sampling error of the estimated mean and percentile curves should be established. One procedure suggested in the PA is to use multiple LHS samples. An alternative procedure for estimating sampling error based on resampling methods within a single LHS sample may be more efficient."

"It is to be expected that estimates of the mean and extreme percentiles will have rather wide regions of sampling variation for a sample size of 70. After such analysis, it may be found necessary to reduce the magnitude of sampling error by increasing the sample size."

"The sampling error may also vary with the level of the normalized release. Information on such variation would be required to determine the level of confidence for concluding that the selected summary curve satisfies requirements (1) and (2) of Section 191.13(a)."

Correlation between LHS variables

"Current LHS procedures treat the uncertain input variables as uncorrelated. Although this is a generally accepted statistical practice, some justification for this assumption is necessary. Several variables may be expected to exhibit correlation, such as permeability and porosity. Others may also be identified. Parameters with strong correlations should transformed to a more orthogonal parameterization for the LHS procedures. For example, if the parameters $X > 0$ and $Y > 0$ are strongly correlated, then a new parameterization defined as X and the ratio X/Y will often be less correlated."

List of Variables not included in LHS Procedures.

"The selection procedure by which variables were excluded from the LHS procedures is unclear. Starting with a list of all input variables, and the models affected by each, documentation should be provided of the reasoning by which each variable was excluded from the LHS procedures. What evidence can be presented that these excluded variables are 'better known' than those selected for assignment of LHS distributions? The sensitivity of model results to these variables and estimates of the precision of each variable should be included in a discussion of the rationale for their selection or omission."

Response

More complete discussions of LHS can be found in Chapter 3 of Volume 2 of the 1992 PA, in Chapter 3 of Volume 1 of the 1991 PA (pages 3-62 and following), and in references cited in those places.

The Project is currently investigating the issues of the "four thirds" rule, the effect of sample size on CCDFs, correlations between LHS variables, and documentation of selecting variables for sampling.

There is no specific evidence that the "four thirds" rule is sufficient for analyses of this type. The rule has been used for convenience, and demonstration of its acceptability remains to be done.

The reviewer's comments on the lack of information about the effect of the sample size on the location of the summary CCDFs are useful.

Correlations between LHS variables will be included when a defensible basis for such correlations is available. Until data are available to defend correlations, it is appropriate to sample variables independently.

The reviewer's comment on the need for documentation on the method for selecting variables for sampling is useful. Such documentation can be included in a compliance application. Some clarification of how "variable" is defined may be useful: there are many "constants" used in modeling.

Comment T159

Page 39: II. Format and Content, List of Variables not Included in LHS Procedures

Area in document Volume 1, Pages 5-9 to 5-15.

"These CCDF plots for different conceptual models and the supporting discussion are very useful and lead to several observations. The points noted below assume the plots are correct although it will be necessary in the future to thoroughly review the models, the probabilities, and the calculations."

"(1) The PA department's "most realistic conceptual model" indicates that releases are less than 1% of the amounts allowed by the Standard. Yet some of the values being used are unproved; e.g., the use of plutonium solubility values that are two to three orders-of-magnitude less than those being found in SNL laboratory reports (SAND92-1579). The very important drilling rate parameter is less than 1% of the maximum suggested in the Guidance to the 40 CFR 191 Standard. The K_d=0 assumption is consistent with the agreement between DOE and the State of New Mexico unless valid experimental values are obtained.

(2) The maximum curve plotted has releases that are 30-40% of the Standard. While it seems unlikely that both the K_d and single porosity values will turn out to be as pessimistic as indicated here, several potentially negative phenomena are not incorporated. These include: (a) various scenarios bringing brine to the surface; (b) greater solubility values for transuranics in the waste storage room and Culebra Aquifer; (c) the formation and transport of colloids in the Culebra Aquifer or to the surface; (d) uncertainties in the inventory; (e) uncertainties in the scenario probabilities; and (f) use of the maximum drilling rate of 30 boreholes/km^2 over 10,000 years."

Response

All but one of the reviewer's points here are essentially correct and well taken. The 1992 PA was not intended to be a compliance application. Most of these caveats are noted in the text of the 1992 PA. (See, for example, page 6-3 of Volume 1 of the 1992 PA for comments about additional work needed in the areas of actinide solubility, K_ds, and brine flows to the surface.) Because 40 CFR 191 sets limits on the probability of radionuclide releases that have been normalized to the total transuranic inventory in the system, performance is not likely to be strongly sensitive to uncertainty in the radionuclide. This observation has not been tested by formal sensitivity analyses, but can be partially supported by comparison of the 1991 and 1992 preliminary PAs, which used different inventories.

One point warrants further discussion. The reviewer's implied requests for the use of the maximum intrusion rate and a consideration of the effect of uncertainties in scenario probabilities are inconsistent. The 1991 and 1992 PAs used a sampled intrusion rate constant, rather than a fixed value, specifically to allow consideration of the uncertainty in scenario probability. If the guidance in the Standard is to be interpreted as specifying both the most severe intrusion scenario to be considered and its probability, then there is little remaining uncertainty in scenario probabilities.

Comment T160

Page 40: II. Format and Content, Estimated Dose

Area in document Volume 1, Chapter 3, Page 3-23, Line 23

(a) "EPA needs to have the complete detailed method used to estimate dose included in the PA."

(b) "In addition, future assessments will need to use the committed effective dose as required in the new 40 CFR 191."

Response

(a) The complete, detailed method used to calculate committed effective dose can be included in a compliance application. The GENII-S code is capable of calculating the committed effective dose. There was no detail on this code provided in the 1992 PA because no radionuclide releases occurred from the undisturbed repository and therefore doses were zero. The detail requested will be provided in the Project Technical Baseline (PTB) report.

(b) All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated in future performance assessments.

Comment T161

II. Format and Content, Definitions

Area in document Activity load categories (Volume 1. Page 5-6, Line 29) Gauge Borehole (Volume 2, Chapter 7, Page 7-25, Line 3) Dimensions of the controlled area (Volume 1., p 3-5) Salt String (Volume 2., Chapter 7, Page 7-24, Figure 7-6)

"Please explain 'Salt String'. Oil and gas operators in the Delaware Basin 'set' an intermediate string of approximately nine inch casing at around 4000 feet depth. This string of casing appears to be at the lower limit of the salt beds. Is this casing the same as 'salt string'?"

CH-TRU vs RH-TRU (General)

"How does DOE define radioactive waste (as RH-TRU or CH-TRU) if the surface dose exposure is < 200 mrem/hr because of internal lead shielding?"

"Since a performance assessment is such an interdisciplinary activity, it would be useful to have a glossary of terms in addition to the list of acronyms."

Response

The State of New Mexico Oil Conservation Division (OCD) provides a detailed definition of the "Salt Protection String" required in the Potash Area of Eddy and Lea Counties (including the WIPP) in Order R-111-P:

> "A salt protection string of new or used oil field casing in good condition shall be set not less than one hundred (100) feet nor more than six hundred (600) feet below the base of the salt section; provided that such string shall not be set below the top of the highest known oil or gas zone." (OCD Order R-111-P, page 7; two additional pages of text follow specifying procedures for installation of the salt string).

The distinction between CH-TRU and RH-TRU waste for transport to the WIPP is described in the WIPP Waste Acceptance Criteria (U.S. DOE, 1991) on page 3-45, and is based on TRUPACT-II Requirements.

> "The external dose rates on the loaded TRUPACT-II placed on the trailer are limited to 200 mrem/hr at the surface of the TRUPACT-II and 10 mrem/hr at two meters. Dose rates on the TRUPACT-II must comply with 10 CFR 71.47. Drums or SWBs shall not exceed the 200 mrem/hr surface reading or 10 mrem/hr at two meters. Shielded waste containers are allowed for As Low As Reasonably Achievable (ALARA) purposes only and must comply with Section 12.0 of Appendix 1.3.7 of the TRUPACT-II SARP."

The TRUPACT-II SAR (page 1.3.7-55) further states:

"Occasionally, drums of TRU waste that meet the radiation level (surface dose rate) requirements require ALARA/dose reduction shielding to meet DOE site requirements....If the measured radiation levels are below the specified levels, but do not meet the site criteria, shielding may be added to the drum."

A glossary of terms can be included with the compliance application if necessary.

Reference

U.S. DOE (Department of Energy). 1991. *Waste Acceptance Criteria for the Waste Isolation Pilot Plant.* WIPP-DOE-069, Rev. 4. Carlsbad, NM: Westinghouse Electric Corporation.

Comment T162

Page 41: II. Format and Content, Use of Bounding Analyses in PA (General)

Area in document **General**

"The PA should include results from bounding analysis in areas such as the evaluation of alternative conceptual models."

Response

While there may be cases when bounding analyses are appropriate, they, as a general rule, are discouraged for compliance determinations. The reason for this is clear, the Standard encourages reasonableness in expectation. Bounding analyses may be useful for parameters that are of little consequence or alternative conceptual models that are clearly bounded by the conceptual model being pursued.

Comment T163

Page 41: II. Format and Content, Misnumbered Equation

Area in document **Volume 2, Appendix A, Page A-14, Line 1**

"Should Equation (A-11) be (A-10)?"

Response

Yes.

Comment T164

Page 41: II. Format and Content; Potential WIPP Resources

Area in document Volume 1, Page 2-4

"There should be an update on the potential resources at and around WIPP. These should be portrayed graphically, so that it is clear where the expected resources are located."

Response

The Project has initiated a re-evaluation of resources based on available data and current market conditions. This information will be part of the Project Technical Baseline and will be used in the compliance application.

Comment T165

Page 41: II. Format and Content, Additional Format and Content Comments

Area in document Volume 1, Page 4-6

"The statement is made that Volume 4 of the 1992 PA documentation will contain preliminary analyses of the potential for the releases of radionuclides dissolved in brine at the ground surface. No analyses or discussion of this scenario was found in Volume 4."

Response

The analyses were not performed because of time and resource constraints. They will be considered for inclusion in the systems prioritization. The decision to omit the analysis was made after publication of Volume 1.

Comment 166

Page 41: II. Format and Content, Additional Format and Content Comments

Area in document Volume 1, Page 5-17, Lines 25-27

"It states here that Volume 4 will contain results of the 1992 preliminary performance assessment for informal comparison with the Individual Protection Requirements. Volume 4 does not contain these results."

Response

Volume 4 does not highlight the results of the undisturbed performance analyses in its Abstract or Conclusion. The results are reported in considerable detail, however, in Chapter 4 of Volume 4, "Undisturbed Performance (Repository/Shaft)." The conclusion is clearly stated on page 4-53, lines 1-3: "Neither in the previous [1991] analyses nor in the 1992 PA was there any release of contaminated brine to the accessible environment in the undisturbed scenario."

Comment T167

Page 42: III. Models, Conceptual Models

Area in document Throughout; Volume 1, Chapter 6, Page 6-1, 6-2

"Conceptual models should be fully explained and justified (soon) so that EPA and the public understand the conceptual models that DOE plans to use in its compliance assessment package. Future assessments should include the reasons why discarded conceptual models are no longer being considered."

Response

Alternative conceptual model screening will be part of the SP.

Comment T168

Page 42: III. Models, Conceptual Models

Area in document Throughout; Volume 1, Chapter 6, Page 6-1, 6-2

"The PA uses 'three conceptual modes for radionuclide transport in the Culebra and two approaches to estimating the probability of inadvertent human intrusion into the WIPP by exploratory drilling.' (The 1,000 year period used for the comparison limit needs to be revised to a 10,000 year period.)"

"What other alternate conceptual models are to be included in the future? The PA should contain results using alternative conceptual models where there is a significant difference between the alternatives, and the models that DOE prefers."

Response

The SP will examine other alternative conceptual models. The Project agrees that consideration of alternative conceptual models is an important part of the PA. It is not possible now to list alternatives that will be identified and considered in the future.

(Regarding the parenthetical comment, the Project agrees that intrusions need to be considered in the full 10,000 yr time interval. Note that the integrated releases displayed in the 1992 PA do consider a 10,000 yr period for transport. Only the time of intrusion is limited to 1000 yr.)

Comment T169

Page 42: III. Models, Computer Generated Maps

Area in document Volume 2, Chapter 2, Figures 2-9, 2-14, 2-16 to 2-17

"The grid spacing selected for the computer generated maps could be improved. For example, on the southern half of the map in figure 2-18 the computer generated "goose eggs" around the sparse well data look questionable. A more realistic map would continue the regional character and shape established in the northern portion of the map."

Response

The point is well taken: the "goose eggs" are probably not realistic. However, subjective contouring of sparse data could create the impression of more information than is actually available. A more suitable approach might be to simply display the data points without contours.

Note that the maps are shown for display purposes only. These contours are not used in quantitative consequence analysis.

Comment T170

Page 43-44: III. Models, Code Linkage and Data Flow

Area of document Volume 2, Section 1.3, Page 1-3 through 1-5

"The discussion of code linkage and data flow in Section 1.3 of Volume 2 is incomplete. The secondary data base, which contains all the information on the conceptual model that is utilized by the WIPP performance assessment analyses, is discussed in one brief paragraph. No information is presented on the structure of the files in this data base, how access to the data is controlled, QA procedures, etc. The computational data base CAMDAT, which is at the heart of code linkage in the PA analyses, receives an even briefer discussion--there is no explanation of what is meant by the "zig-zag" connection nor how it is achieved."

"Contrary to the assertion in the report that CAMDAT is fully described in the CAMCON user's manual (Re 92), that manual still fails to present a comprehensive discussion of the structure of the data base and its role in code linkage and data flow, although it presents a brief conceptual discussion of CAMDAT which somewhat expands the previous discussion. It is necessary to turn to a third reference, a separate report on this topic (Re 89), to find such information. This last report does, in fact, present a thorough discussion of the structure and function of the two aforementioned data bases. This material should be included in the PA report."

"The overall plan for code linkage and data flow described in the PA report and the supporting documents cited above, if fully implemented, constitutes a consistent and tractable methodology for assessing the performance of the repository at this stage of the analysis."

"One shortcoming of the 1992 PA was the failure to integrate several key codes into the CAMCON system, namely BRAGFLO, SECOTP2D and CUTTINGS. Since 70 simulations of three distinct scenarios were analyzed, this posed a large burden on the individual analysts, requiring hundreds of manual data transfers with concomitant chances of errors and breakdowns of the QA process. Integrating these codes should be a high priority."

"Given the fact that substantial manual data transfer is currently necessary, it is unclear how uncertainty and parameter sensitivities are being carried through from one model to the next. For example, once the sensitivity and uncertainty analysis is performed on the source-term model, how are the distribution of release rates, etc. being input into the flow and transport models such as SECO and BRAGFLO? Furthermore, it is not clear how sensitivities to various uncertainties associated with the model boundary conditions for each of the models are being accommodated within the analysis."

"Two areas where improvements could have major impacts on future PAs are better code integration and expanded computational facilities. Despite its current drawbacks, the CAMCON system is a practical approach to linking disparate codes which have been and continue to be developed by various code designers working with specialists in different disciplines. It seems to be a suitable methodology to use at the present state of model development. However, in the long run, CAMCON is not a substitute for a single, fully integrated model. As described in the cited references, it is not computationally efficient. Each pre- and post-processor to the major codes involves additional computational steps. Furthermore, running each code *ab novo* for each realization involves many repetitive calculations, whereas a fully integrated model should be able to repeat only those calculations required by a changed parameter, in some cases perhaps scaling the results of a previous calculation. More than inconveniencing the analysts, this lack of efficiency severely limits the scope of the analysis, limiting the number of Monte Carlo simulations, the complexities of the models which can be employed, and the number of scenarios which are modeled."

Response

CAMCON and CAMDAT are an integral part of the code linkage and data communication process which provide the computational structure of WIPP performance assessment. It is important that sufficient detail be presented in a compliance application or supporting information that the regulator can easily understand the computational structure and methodology used. The level of detail in describing CAMCON and CAMDAT in Section 1.3 of Volume 2 is inadequate and will be expanded for the application.

The Project is currently investigating the issue of software QA. Computational codes such as BRAGFLO, SECO TRANSPORT 2D, and CUTTINGS were not included within the CAMCON system primarily because these codes were still in the development and transition stages. One of the highest priority tasks for PA in 1994 is in the area of software QA. BRAGFLO currently receives all of its input from CAMDAT via a pre-processor and writes all of its output to CAMDAT via a post-processor.

Manual data transfer was not done in the 1992 PA. This was an unfortunate choice of words which incorrectly suggests that the data entry and data transcribing process was inconvenient,

Inefficient, and possibly error prone. Actually, what was meant by "manual data transfer" was "electronic data transfer outside of the CAMCON executive controller." This involved the direct reading of output of one model by another to obtain the latter's input.

The issue of using a single numerical model was considered in the past and dismissed. In a modeling environment as large as that of the WIPP PA, and characterized by diverse and distinct phenomena occurring in well-defined regions within the repository and natural barrier systems, it is not only inefficient but undesirable to develop a single all-encompassing numerical model. It is inefficient because phenomena important in one region would be carried along as extra baggage in other regions. Sensitivity analyses often require intermediate results which focus only on the performance of a small portion of the disposal system, and should not require running the entire modeling system. It is more efficient to split the larger picture into smaller, more manageable, portions and model each portion's particulars as rigorously as necessary. The amount of computational effort expended in linking the models together is negligible compared to the effort of simulating the physics. The CAMCON approach is computationally efficient when placed in the context of the PA modeling scope and demands.

Comment T171

Page 44-45: III. Models, Cuttings Model

Area in document General

"Most of the technical assumptions and data quality appear to be reasonable and accurate."

"It is important to reiterate the comment that the spalling mechanism is difficult to handle and is not well understood. This does not mean the issue should not be vigorously addressed. Using the 'Unconsolidated Sand' analogy, as discussed in the SAND92-7295 report, is a good approach."

"Assumptions made for the calculation of erosion including effective shear strength for erosion of waste material and volume of material discharged to the surface need further technical support. The plugging and abandoning procedures and the data for drilling in the Delaware basin seem to be in order."

"One obvious weakness observed in the CUTTINGS model is the lack of physical property data for the waste material. The predictions for drilling, erosion, and spalling, are highly dependent on the material properties. These properties may need to be developed through a joint effort between people working on performance assessment and waste characterization. A very soft, highly pressurized material may present a unique 'blow-out' potential."

Response

The Project is currently investigating the issue.

We agree with these observations and they are included in our thinking in regard to direction of the ongoing cuttings release model development and laboratory studies (see EPP 3.2.4.3, 5.4.3.1, 5.4.3.2).

Comment T172

Page 45: II. Format and Content, Cuttings Model

Area in document General

"A second weakness observed is in the rheology model which used only two data points to develop a model which describes the entire rheogram."

Response

The observation is correct. The data used to characterize the high salt drilling mud is consistent with a Bingham model which requires only two parameters, the plastic viscosity and yield point. To fully define the Oldroyd model the ratio of the initial viscosity (at zero shear rate) to the plastic viscosity is also necessary.

A high-salt, water based mud is assumed to be the drilling mud used when drilling through the Salado. The Oldroyd model requires a value for the ratio of the initial viscosity (at zero shear rate) to the plastic viscosity, to fully define the model in the low shear regime. This ratio was not available for a high-salt, water-based mud in 1992 so a ratio based on an oil based mud was chosen. Since high shear rates occur at the borehole wall the value chosen for the ratio was expected to have little impact on the final model diameter. The Project is evaluating the sensitivity of eroded diameter to this issue.

Comment T173

Page 45: III. Models, Cuttings Model

Area in document Volume 2, Figure 7.6, Page 7-24

"Figure 7.6 located on page 7-24 of Volume 2 shows the drill pipe being rotated an a counter clockwise direction, which is inconsistent with standard drilling processes. Although this misrepresentation will not affect the actual calculations for waste removal, if does present an unrealistic condition which could be construed as lack of understanding of drilling technology. Should the drill pipe be rotate as shown, the entire drilling assembly would come apart because of the tool joint would unscrew as torque is applied to the bit."

Response

Thank you for pointing out this error. The rotation direction in the figure has been corrected.

Comment T174

Page 45: III. Models, Cuttings Model

Area in document General

"In Volume 2 of SAND 91-0893 it is stated, on page 7-16, that when modeling erosion, turbulence existed as the hole washed out from 0.4445 m to 0.994 m. It is difficult to imagine flow through a 0.994 m wellbore having a Reynolds number of 4319. This calculation should be checked."

Response

The computation was rechecked and found to be correct.

Comment T175

Page 45-46: III. Models, Cuttings Model

Area in document General

"In further refining the CUTTINGS model, the following should be considered:

- Include uncertainties associated with the range of waste inventory instead of basing source term on drum equivalent.

- Additional analyses are needed on the practicability of drilling exploratory wells in the Delaware basin using slim holes

- The physical properties including viscometric properties of compacted slurries of waste material are needed. Evaluations possibly tests should be performed under drained and undrained conditions.

- Spallation predictions are an important topic requiring additional work.

- A more detailed rheogram, based on multi-speed viscometer data, should be used for calculating shear stresses at the wellbore wall, and for calculating the occurrence of turbulence for a multiphase non-newtonian fluid of cuttings and drilling mud.

- Local concentrations of radionuclides near the waste panel should be added to the cuttings volume"

Response

These comments raise issues that will be considered for inclusion in the SP.

Comment T176

Page 46-47: III. Models, BRAGFLO Changes

Area in document Volume 2, Appendix A, Page A-19, Line 10Volume 2, Chapter 7, Page 7-3 to 7-5

"When you say 'time constraints' do you mean you don't have enough time to accomplish the changes to BRAGFLO, or do you mean that the code itself takes so long to run? It would seem the addition of general behavior would enhance versatility and the value of BRAGFLO. Will there be time in the future to generalize this aspect of BRAGFLO?. . ."

". . . However, based on the presentation of boundary condition formulation on Page A-17 of volume 2, it does not appear that the well boundary conditions are properly formulated."

Response

Clarification of the 1992 PA, Volume 2, Page A-19, lines 9-10:

> "To program the integration and summary calculations to be completely general to enable it to perform on any mesh is not feasible under the PA time constraints."

The time constraints referred to are not computing or run time for BRAGFLO, but rather analysts' and code developers' time. This includes code that might make BRAGFLO more versatile or easier to use for users outside PA. Tools already existed for performing the integrations and summary calculations outside BRAGFLO within the CAMCON system. Although BRAGFLO had not been adapted to use these tools in the 1992 version, the code has since been upgraded to use them. BRAGFLO can now print out "history variables," which are results printed out at every time step, thereby enabling integrations to be performed in post-processing to the same degree of accuracy as they could be done internally to BRAGFLO. Algebra is then used to perform integrations or other summary calculations. This capability frees BRAGFLO from having to be hard-wired to a particular mesh.

Clarification of the BRAGFLO boundary condition treatment:

In BRAGFLO, relative flows of each phase across the boundary in a grid block containing a constant pressure well are proportional to their relative saturation in the well block. In this way, the model does calculate these percentages.

Comment T177

Page 47: III. Models, BRAGFLO Changes

Area in document Volume 2, Appendix A, Page A-19, Line 10; Volume 2, Chapter 7, Page 7-3 to 7-5

a) "The temporal and spatial discretization methods used in BRAGFLO are very inefficient and would lead to very long simulation times. Specifically, the finite-difference spatial discretization scheme should be reexamined; numerous pre-processing packages are available to facilitate the finite element grid construction, thus eliminating any advantage of the finite-difference techniques. This is particularly true when a telescoping mesh procedure is used in the WIPP modeling to refine the model domain. A finite-element mesh would have allowed the same descretization without sacrificing the ability to evaluate the sensitivity as the solution to regional model boundary variations."

b) "It is stated on page A-5 of volume 2 that 'The BRAGFLO flow model simultaneously solves five equations.' This approach is very inefficient and by substituting the constraints into the balance equations only two unknowns would have to be simultaneously solved. Furthermore, adaptive implicit procedures exist which would allow only one unknown to be solved at nodes where only one phase (i.e., gas or brine) exists."

Response

a) The temporal discretization method used in BRAGFLO is adaptive and therefore efficient. The spatial discretization and solution method is driven by the need for robustness. The particular 2-phase problems solved for the WIPP, with large pressure gradients and property discontinuities (of several orders of magnitude) is very demanding of numerical methods.

b) BRAGFLO actually only solves two equations per grid block. The three constraints are used to eliminate unknowns in the differential equations. The unknowns solved for are brine pressure and gas saturation. Adaptive implicit methods, adaptive in the sense of switching dependent variables, are difficult to implement in a robust form when the dependent variable at a node/element switches within a time step, i.e., when one phase disappears or reappears.

Comment T178

Page 47-48: III. Models, BRAGFLO Changes

Area in document Volume 2, Appendix A, Page A-19, Line 10; Volume 2, Chapter 7, Page 7-3/5

"The PA assumes that the properties of the gas phase are approximated by those of hydrogen (Volume 2, p. 7-3). The PA further states that using hydrogen serves as a 'worst case' analog because hydrogen is much less viscous than the other gases expected to exist in the repository (CO_2, etc.) and, as a result, would more likely be transported to the accessible environment. This assumption is valid, but the ease with which hydrogen escapes from the repository may in fact alter the ease with which brine moves out of the repository. In two-phase flow modeling, it is important to note that each phase present affects the relative permeability of the other phase, and by removing hydrogen from the repository the predicted movement of brine may be affected. As CO_2 may be more representative of the fluid flow properties of gases present, why not use CO_2 in the model or at least compare the behavior of the two gases?"

Response

Hydrogen produced by corrosion is expected to be the most abundant gaseous component in the WIPP environment. Approximately 2/3 of the gas to be generated is believed to be H_2 and the remainder to be CO_2. Assuming less mobile properties for the gas is not expected to increase the migration of either the gas or brine phase. Calculations are planned to evaluate this effect and the current PA assumptions.

Comment T179

M

Page 48: III. Models, BRAGFLO Changes

Area in document Volume 2, Appendix A, Page A-19, Line 10; Volume 2, Chapter 7, Page 7-3/5

"The model also assumes that none of the gas is transported as a dissolved component in the brine (Volume 2, p. 7-5). Considerable data exist on gas solubilities in water and salt solutions at pressure-temperature conditions similar to those expected to exist at WIPP (SAND90-2848 ...), and these should be built into the model, rather than assuming no solubility of gases in the brine."

Response

The Project is evaluating the sensitivity of compliance to this issue by means of performance assessment and other modeling sensitivity studies.

Information on the solubility of gases in NaCl brines has been evaluated and published by Cygan (1991). The transport of gas out of the repository as dissolved gas is not currently modeled in PA. The PA model (BRAGFLO) has the capability to consider gas solubility in brine, but preliminary evaluation suggests this to be a minor secondary effect. A more systematic analysis needs to be performed and is planned to better evaluate the assumption of negligible gas solubility.

Reference

Cygan, R.T. 1991. *The Solubility of Gases in NaCl Brine and a Critical Evaluation of Available Data.* SAND90-2848. Albuquerque, NM: Sandia National Laboratories.

Comment T180

Page 48: III. Models, BRAGFLO Changes

Area in document Volume 2, Appendix A, Page A-19, Line 10; Volume 2, Chapter 7, Page 7-3 to 7-5

"The algorithm that describes flow of brine and gas from the repository is only concerned with the conservation of mass. Presumably, flow is assumed to be isothermal, since conservation of energy and momentum are not required. Cooling (or heating) of the brine-gas mixture as it flows into or out of the WIPP repository might have a significant affect on the transport of hazardous materials, particularly, VOCs, to the accessible environment. Are there plans to incorporate energy and momentum conservation into the flow equations?"

Response

The Project has already investigated and resolved the issue of thermally driven repository processes and work is complete. With respect to analyses primarily concerned with horizontal pathways the assumption of isothermal flow is considered to be an appropriate representation. However, for vertical migration, the effect of the geothermal gradient on migration of radionuclides and VOCs has not been addressed.

This issue affects the compliance analysis as follows: For issues associated with vertical migration there may be a need for analyses to determine the effect of the geothermal gradient on migration of radionuclides and RCRA constituents. The issue will be considered for inclusion in the systems prioritization.

M

Comment T181

Page 48-49: IV. Regulatory Issues, Include 40 CFR 191 Changes in Future Assessments

Area in document Throughout

(a) (Restatement) Consider changes to the Groundwater Protection Requirements.

(b) "Future assessments should identify the potential aquifers, the water quality (i.e., total dissolved solids), and potential underground sources of drinking water."

Response

(a) All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated in future performance assessments.

(b) As stated in a paper recently presented at the Waste Management '94 conference (Trauth et al., 1994), we propose to determine when and if underground sources of drinking water should be identified and characterized (i.e., when such a characterization will provide pertinent information for a compliance application). Briefly, identification and characterization of USDWs should not be required if no radionuclide releases to the accessible environment are predicted for 10,000 years or if 10,000 year peak predicted releases to the accessible environment are less than or equal to the applicable Maximum Contaminant Levels (MCLs). USDWs along the pathway should be identified and characterized if peak predicted releases to the accessible environment for 10,000 years are greater than the MCLs.

Reference

Trauth, K.M., S.G, Bertram, and B. Bower. 1994. "Considerations for Guidance for Radioactive Waste Disposal Arising from Rules Under 40 CFR 191 and 40 CFR 194," *Proceedings of Waste Management '94 Conference, Tucson, AZ, February 27-March 3, 1994.*

Comment T182

Page 49: IV. Regulatory Issues, Use of Passive Institutional Controls

Area in document Volume 1, Page 3-11; Throughout

"The Guidance does state that passive institutional controls could reduce the likelihood and consequences of inadvertent drilling intrusion, but DOE should present the calculation and their results before taking credit for the passive controls."

Response

The 1992 PA did this. Results are presented in Chapter 5 for intrusion probabilities estimated both with and without the effects of passive markers.

Comment T183

Page 49: IV. Regulatory Issues, Justification of Drilling Rate

Area in document Volume 1, Chapter 3, Page 3-11, 3-12

"While the 40 CFR 191 Guidance states 'that the likelihood of such inadvertent and intermittent drilling need not be taken to be greater than 30 boreholes per square kilometer of repository area per 10,000 years...,' DOE still needs to justify its rationale for accepting or not accepting the Guidance drilling rate. The drilling rate used in the performance assessment is dramatically different than that discussed in the Guidance. The compliance criteria will further address this topic."

Response

The drilling rates used in PA will be reevaluated when the compliance criteria are available. Past PAs have deliberately used an uncertain rate (i.e., they have sampled values of the Poisson rate constant from a range which results in an expected value of 30/km^2/10,000 yr at its maximum) to examine system sensitivity to the drilling rate. These preliminary PA analyses have demonstrated that performance is extremely sensitive to future drilling rates.

Comment T184

Page 49-50: IV. Regulatory, Engineering Measures

Area in document Volume 1, Chapter 5, Page 5-20, Line 12; Volume 2, Chapter 2, Page 2-50

"The 'additional engineering measures for the WIPP' should be modeled in the PA to evaluate the effect of these measures on the institutional control even during construction of WIPP (EEG 92)."

Response

Engineered alternatives will be evaluated for inclusion in the systems prioritization.

Comment T185

Page 50: IV. Regulatory Issues, Undisturbed Performance

Area in document Volume 1, Chapter 5, Page 5-21, Line 9

(a) "The new Subpart C requires the disposal system be designed for 10,000 years of undisturbed performance."

(b) "All possible groundwater uses should be considered and evaluated."

Response

(a) All aspects of 40 CFR 191, including those portions repromulgated in December 1993, will be incorporated in future performance assessments.

(b) The text in question refers to the consideration of "special sources of groundwater" as defined in the 1985 version of 40 CFR 191. The regulation now requires consideration of underground sources of drinking water in the accessible environment. Future documentation will reflect that requirement.

Comment T186

Page 50: IV. Regulatory Issues, Incompleteness of Modeling System and Database

Area in document Volume 1, Chapter 6, Page 6-1, Line 12

"What parts of the modeling system and database are still incomplete? What is being done to fill the data gaps? Statements like this should not be presented in the PA without explanation. This problem occurs throughout the PA."

Response

The complete sentence referred to begins: "As summarized in the following discussion, however, the modeling system and data base are still incomplete..." The discussion following on Page 6-3 provides a very brief summary of the major parts of the system that remain incomplete. It was not possible to compress this information into a single sentence. Instead, the caveat was supplied with the reference to the following text for additional information.

M

Comment T187

Page 50-51: IV. Regulatory Issues, Incompleteness of Modeling System and Database

Area in document Volume 1, Chapter 6, Page 3-11

(a) "Active institutional control has been assumed by SNL in prior preliminary comparisons to eliminate any possibility of inadvertent human intrusion during the first 100 years after WIPP decommissioning. However, in the 1992 PA this possibility is included. This is a preferred interpretation because a 100-year period of institutional control is not required by the Standard or the LWA and DOE has not committed to such action."

(b) "There have been lapses in institutional control even during construction of WIPP."

Response

(a) Hora's memo in (Volume 3, Appendix A, p. A-69 to A-99) contains guidance from the Futures Panel on the timing of intrusions, including the possibility of intrusions in the first 100 years after closure of the repository. In the performance assessment calculations, intrusions in the first 100 years were precluded based on Guidance to 40 CFR 191 that active institutional controls may be able to "prevent or limit potential releases of waste from a disposal system" (50 FR 38080a) for some time after closure, not to exceed 100 years. This is clearly stated in Volume 1, Chapter 5, p. 5-18, l. 20-22 that in the 1992 PA "no intrusions are assumed to occur during the first 100 years after decommissioning." Details of the DOE's active institutional control will be included in the compliance application.

(b) The reference to lapses could not be located in EEG 92. It was assumed that the reference was to the JR-13 well. Neither the actions surrounding the JR-13 well (which was drilled in compliance with the regulations in effect at the time) nor the written record suggest that institutional memory was lost. Proper control of the activities taking place in the area of the WIPP was maintained throughout the construction of the WIPP.

Comment T188

Page 51: IV. Regulatory Issues, Incompleteness of Modeling System and Data Base

Area in document Volume 1, Page 3-21

"It is stated that 'Thus, the EPA assumes that satisfying the numeric requirements is sufficient to demonstrate compliance with 191.13(a) but not mandatory.' This statement is incorrect. EPA did not make such an assumption. EPA's statements about 40 CFR 191 appear to have been taken out of context and misinterpreted in the PA report."

Response

EPA is requested to clarify the meaning of the text quoted on pages 3-20, lines 24-33 and 39-49, and page 3-21, lines 1-3. This text is from 40 CFR 191.13(a) and from Appendix C to 40 CFR 191.

Comment T189

Page 51: IV. Regulatory Issues, Incompleteness of Modeling System and Database

Area in document Volume 1, Chapter 5, Page 5-17, Line 39-42

(Restatement) "...in the final PA suggests assurance requirements will be used quantitatively and only if needed." EPA states that this is inconsistent with the 1985 Standard Guidance and Preamble that assurance requirements are a qualitative measure only.

Response

Guidance in what is now Appendix C (identical to the text that was Appendix B in the 1985 promulgation of 40 CFR 191) states: "The implementing agencies should consider the effects of each particular disposal system's site, design, and passive institutional controls in judging the likelihood and consequences of such inadvertent exploratory drilling." EPA has stated that the impact of passive institutional controls should be considered in a quantitative fashion. The Supplementary Information to the 1985 promulgation of 40 CFR 191 included:

"Not allowing passive institutional controls to be taken into account to some degree when estimating the consequences of inadvertent human intrusion could lead to less protective media being selected for repository sites." (50 FR 38080b,c)

and

"The Agency also assumed that passive institutional controls should reduce the chance of inadvertent intrusion compared to the likelihood if no markers and records were in place. Specific judgments about the chances and consequences of intrusion should be made by the implementing agencies when more information about particular disposal sites and passive control systems is available." (50 FR 38080b)

M

Comment T190

Page 51: IV. Regulatory Issues, Incompleteness of Modeling System and Database

Area in document Volume 1, Chapter 5, Page 5-18

(a) "The Standards do not automatically grant a 100-year period free from intrusions. The statement on page 3-11 is a more accurate interpretation."

(b) "The assumptions used in future PAs should be consistent with the length and degree of active institutional control to which DOE eventually commits and considerations of failure in preventing all human intrusions."

Response

(a) Text in the Supplementary Information for the 1985 version of 40 CFR 191 included:

"The proposed rule limited reliance on 'active institutional controls' (such as controlling access to a disposal site, performing maintenance operations, or cleaning up releases) to a reasonable period of time after disposal," (50 FR 38079c-38080a)

In the final rule, the "reasonable period of time" was changed to no more than 100 years. PA calculations use what was considered to be a "reasonable period of time."

The text on page 3-11 was not clear in stating that in 1992, as in previous years, no intrusions were allowed during the first 100 years. Text on page 5-18, lines 21-22 is clearer.

(b) Comment acknowledged. The DOE's active control program will be described in the compliance application.

Comment T191

Page 51: IV. Regulatory Issues, Incompleteness of Modeling System and Database

Area in document Volume 1, Chapter 5, Page 5-19, Line 39

(a) "It is stated here that, with respect to drilling, DOE has control of the area within the land-withdrawal boundary from the surface to a depth of 6,000 feet. This is not accurate."

(b) "The WIPP Land Withdrawal Act provides no lower limit to the control of the subsurface, except for those two leases which underlie Section 31 and for which the 6,000 foot limit currently applies."

Response

(a) Text immediately preceding this text quotes from the Land Withdrawal Act about the withdrawal of the land from entry appropriation and disposal (Section 3(a)(1)). It should also quote the exception (Section 4(b)(4)) for the rights under Federal Oil and Gas Leases No. NMNM 02953 and No. NMNM 02953C.

(b) It is correct that the Land Withdrawal Act places no lower limit on the withdrawal of land from use (with the above exceptions). EPA needs to clarify what is considered inaccurate in the statement (i.e., surface to 6,000' or below 6,000').

Comment T192

Page 51: IV. Regulatory Issues

Area in document Volume 2, Page 4-4, Lines 26-27

"The regulatory requirements which preclude the possibility of inadvertent explosions in waste storage rooms from gases created by degradation of waste should be cited."

Response

The sentence referred to is poorly worded, and does not clearly state the intended position.

The reasoning behind the sentence can be found in the expanded description of the event and process screening procedure in Volume 1 of the 1991 PA (SAND91-0893/1), page 4-31, lines 3-12. The reference should have been only to inadvertent explosions that are not a spontaneous result of the behavior of the disposal system (e.g., accidental nuclear bombing of the site). The PA interpreted the guidance to 40 CFR 191 indicating that "exploratory drilling...can be the most severe intrusion scenario..." to allow the exclusion of accidental explosions.

Explosions of waste-generated gas are considered on page 4-52 of Volume 1 of the 1991 PA (SAND91-0893/1). Quantitative analysis of possible peak pressures resulting from a waste-gas explosion indicate no potential to affect performance of the panel seals.

Comment T193

Page 51-52: IV. Regulatory Issues, Incompleteness of Modeling System and Data Base

Area in document Volume 3, Page 1-51, Lines 11-13

"The statement about the extent of the WIPP control area is confusing. It took two years for Congress to pass the LWA which designated the specific area to be withdrawn for WIPP."

Response

The statement was overtaken by events when the LWA was passed (October 30, 1992). This volume was already in publication, and the text wasn't updated. The controlled area is the area withdrawn.

Comment T194

Page 52: V. Quality Assurance, Including Quality Assurance

Area in document Volume 1, Chapter 3, Page 3-17, Line 41

"Quality assurance (QA) aspects of experiments, computation development, code development, and code execution must be combined and included as part of the PA documentation."

Response

The PA reports are considered snapshots, and never were intended to be stand alone documents upon which to base compliance. The amount of material suggested above is significant. Is EPA suggesting that all QA supporting documentation be included directly in the compliance application or in the docket?

QA documentation is currently contained in the Project's records center.

Comment T195

Page 52: V. Quality Assurance, CAMCON Control Code

Area in document Volume 2, Chapter 1, Page 1-3, Line 30

"How does CAMCON 'automatically handle quality assurance during the calculations'?"

Response

No system can completely automatically handle all aspects of quality assurance. CAMCON provides an analyst with a system to aid in QA. CAMCON does this through database management, directory structure, and an on-line help file system.

Comment T196

Page 52: V. Quality Assurance, Data Quality Objectives

Area in document Volume 1, Chapter 3, Page 3-17, Line 41; Volume 2, Chapter 1; Volume 3 data

(a) "How does the PA require that the data is good enough for the purpose for which it is used?"

(b) "What data quality objectives are required of the data in the database?"

(c) "Are the data being used for the purpose(s) that they were collected for?"

Response

The Project is currently investigating the issue.

(a) The 1990 - 1992 series of preliminary PA studies drew upon a wide variety of sources of data and information, but mainly from the work of SNL principal investigators (PIs).

(b) Data quality objectives for experimentally derived data and information are described in each PI's Test Plan. But these objectives do not directly address the quality of entries in PA's database of model parameters. QA procedures for construction of model parameters are being developed and early versions of these procedures are described in Rechard et al. (1992).

If the term "data quality objectives" used by the reviewers means the specific DQO processes outlined in EPA QA documents, then the following should also be considered: The WIPP Project believes that the DQO process outlined in EPA documents is not directly applicable to data used in forming the parameters of the complex numerical models that simulate WIPP performance. The EPA's DQO process appears to apply to situations in which measured quantities (the "data") are used directly to determine compliance with standards, e.g., water-quality standards.

The WIPP performance assessment is very different from these situations in that field and laboratory data are used as indirect inputs to a large numerical model that generates compliance-related quantities, e.g., the CCDF required in 40 CFR Part 191, Subpart B. Data priorities, but not data quality, can be determined through sensitivity analyses with these large numerical models of the WIPP. In general, existing WIPP QA procedures for assuring the quality of the (field data to model parameter) process strongly emphasize peer review and careful documentation.

(c) Much of the data used by the Project was generated prior to 1985 for the purpose of establishing baseline conditions (site descriptions, hydrology, etc.) in the WIPP Environmental Impact Statement. While quality assurance has always been applicable to the WIPP, the more stringent data requirements of a compliance determination mandated in 40 CFR Part 191 became apparent later, and only within the past five years were data-gathering programs begun to specifically address the needs for data that are used chiefly in a determination of compliance.

Reference

Rechard, R.P., K.M. Trauth, and R.V. Guzowski. 1992. *Quality Assurance procedures for Parameter Selection and Use of Expert Judgment Panels Supporting Performance Assessments of the Waste Isolation Pilot Plant*. SAND91-0429. Albuquerque, NM: Sandia National Laboratories.

Comment T197

Page 53: V. Quality Assurance, Parameter Sheet Format and Data Quality Indicators

Area in document Volume 3, Chapter 1, Page 1-12

"The quality of the parameters used in the PA are of critical importance. The parameters presented on the parameter sheet are not graded as to quality or level of confidence. Parameter sheets should have a statement of data quality or a statement of the level of confidence for each parameter."

Response

The point is well taken. The form will be reviewed.

Comment T198

Page 53: VI. Use of Expert Panels and Review, Expert Panels and Expert Judgment

Area in document Volume 2, Chapter 2, Page 2-39; Throughout

(a) "Expert panel judgement and investigator knowledge may be necessary in lieu of actual data parameters. However, a procedure should be developed to insure such information is of the highest level of confidence. DOE should consult with EPA as it develops expert panel elicitation procedures."

(b) "The use of expert judgement should be replaced with data as soon as data become available, especially for data or parameters that are considered to be important from the sensitivity and uncertainty analyses. DOE should clearly identify areas where expert judgement is or will be used and when it expects that it will be replaced by actual data."

Response

(a) A formal QA procedure for the use of expert judgment was published in 1992 (Rechard et al., 1992). Further refinements of this QA procedure can and have been incorporated into WIPP Procedure No. PAP06, Use of Expert Judgment Panel Quality Assurance Procedures.

Specific uses of expert judgment are documented in Hora et al. (1991), Trauth et al. (1993), and Trauth et al. (1992). A further SAND report is being prepared by the expert panel addressing solubility to provide additional information on the process and results.

With respect to "substituting estimates for actual data," even when additional WIPP-specific data are collected, judgment may still be required to reconcile WIPP and non-WIPP data, to reconcile possible conflicting data, and to interpret the data for the current probabilistic application.

(b) It is appropriate to describe how experimental data are used in the current application. However, it may be more useful to discuss the type of data available rather than those limited cases where no data at all exist. Judgments are based on the available data (however abundant or sparse, under whatever experimental conditions they were collected). For example, all data may not be WIPP-specific. All data may not have been collected for the current application (to answer the current questions). In addition data may exist, yet there may also exist methods by which to collect additional data to improve the understanding (e.g., batch K_d data vs. column experiments). Even when additional WIPP-specific data are collected, judgment may still be required to reconcile WIPP and non-WIPP data, to reconcile possible conflicting data, and to interpret the data for the current probabilistic application.

References

Rechard, R.P., K.M. Trauth, and R.V. Guzowski. 1992. *Quality Assurance Procedures for Parameter Selection and Use of Expert Judgment Panels Supporting Performance Assessments of the Waste Isolation Pilot Plant.* SAND91-0429. Albuquerque, NM: Sandia National Laboratories.

WIPP PA Department. *Use of Expert Judgment Panel Quality Assurance Procedures.* PAP-06. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, R.P. Rechard, and D.R. Anderson. 1992. *The Use of Expert Judgment to Quantify Uncertainty in Solubility and Sorption Parameters for Waste Isolation Pilot Plant Performance Assessment.* SAND92-0479. Albuquerque, NM: Sandia National Laboratories.

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Comment T199

Page 53-54: VI. Use of Expert Panels and Review,

Area in document Volume 3, Chapter 1, Page 1-27, Line 62-64; Volume 3, Appendix A, Page A-71 to A-99

(a) "Since extreme care needs to be taken when substituting estimates for actual data, DOE should consult with EPA and the public on the use of expert panels. There should be documentation of the process used to obtain expert opinion, and the process should follow written guidelines in a clearly defined process."

(b) "Specifically, we do not agree with the approach taken by DOE in using the marker panel to estimate a reduction of the drilling rate from the use of markers. ...Nor did the panel include all the necessary expertise, e.g., no petroleum engineers or drilling experts were included on the panel..."

(c) "Although the compliance criteria will determine the final approach to human intrusion, DOE should...study the drilling experience in the oil, gas, mineral, and water exploration industries and develop historical trends to establish characteristics of drilling."

Response

(a) A formal QA procedure for the use of expert judgment was published in 1992 (Rechard et al., 1992). Further refinements of this QA procedure can and have been incorporated into WIPP Procedure No. PAP06, Use of Expert Judgment Panel Quality Assurance Procedures.

Specific uses of expert judgment are documented in Hora et al. (1991), Trauth et al. (1993), and Trauth et al. (1992). A further SAND report is being prepared by the expert panel addressing solubility to provide additional information on the process and results.

With respect to "substituting estimates for actual data," even when additional WIPP-specific data are collected, judgment may still be required to reconcile WIPP and non-WIPP data, to reconcile possible conflicting data, and to interpret the data for the current probabilistic application.

(b) The process used by the Markers Panel was to first develop design guidelines for long-term communicative markers based on the contributions from individuals in disparate related fields such as materials science, archaeology, and communications. Based on the design guidelines, the two teams comprising the Markers Panel each developed a conceptual design for a system of markers. Estimates of efficacy of the markers system over time were based on the conceptual design. Implicit in the deliberations was the assumption that sufficient testing was undertaken to determine, for example, the appropriate design of the foundation for stone markers to withstand possible fluctuations in surface level and still remain stable. A second assumption in the effort was to evaluate what was possible for a marker system (as a first approximation) with no cost constraints. Cost constraints may come into play regarding the definition of "practicable" in 40 CFR 191. There is much evidence from the fields related to marker design that suggest avenues to pursue to improve long-term survivability and communication.

We did not include a petroleum engineer on the Markers Panel, whose efforts were geared to long-term survivability of a marker system and continued interpretability. We concluded that a petroleum engineer's skills are not such as to contribute to long-term communication with future societies or construction of durable markers.

(c) If this guidance is included in 40 CFR 194, expert judgment will still be required to evaluate existing data, considering fluctuations in drilling intensity and depletion of resources, to estimate drilling intensities to use. Expert judgment will still need to be used in the design and evaluation of passive institutional controls for the consideration of their impact on inadvertent human intrusion, as allowed by Appendix C of 40 CFR 191: "The implementing agencies should consider the effects of each particular disposal system's site, design, and passive institutional controls in judging the likelihood and consequences of such inadvertent exploratory drilling."

References

Rechard, R.P., K.M. Trauth, and R.V. Guzowski. 1992. *Quality Assurance Procedures for Parameter Selection and Use of Expert Judgment Panels Supporting Performance Assessments of the Waste Isolation Pilot Plant.* SAND91-0429. Albuquerque, NM: Sandia National Laboratories.

WIPP PA Department. *Use of Expert Judgment Panel Quality Assurance Procedures.* PAP-06. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, R.P. Rechard, and D.R. Anderson. 1992. *The Use of Expert Judgment to Quantify Uncertainty in Solubility and Sorption Parameters for Waste Isolation Pilot Plant Performance Assessment.* SAND92-0479. Albuquerque, NM: Sandia National Laboratories.

Comment T200

VI. Use of Expert Panels and Review, Review of Investigator Judgement

Area in document Volume 3, Chapter 1, Page 1-7

"Is there any quality control or review of parameters derived from investigator's judgement?... Is there a way to specify the level of confidence in the investigator's judgement?

Response

All PA reports, published as SAND reports, undergo independent technical peer review.

As to the confidence in investigator's judgement, further discussion on this topic with EPA would be useful. EPA is requested to provide clarification.

Comment T201

Page 55: VII. EPA Review of SNL Responses to selected EEG Comments, Comment No. 3 (part 1).

Area in document Volume 1, Appendix B

(Restatement) The EPA agrees that SNL's request that EEG direct "constructive criticism...[toward] data...and models rather than on labeling the outcome as 'nonconservative'" is reasonable, but notes that the modeling system is complex and its evaluation requires "specialized expertise and significant personnel resources. No outside review group, including EEG has rigorously evaluated the basic mathematical and statistical theory underlying the computational procedures. In addition, the QA status of computer codes and data used in the WIPP PA severely limits the scope of review in these areas."

The EPA suggests that SNL should perform "bounding or conservative analyses for several reasons, including: 1) Bounding analysis can be useful in determining the level of effort to apply to alternative conceptual models. Performing only realistic modeling could severely limit the number of alternative conceptual models or issues that are evaluated; and 2) The bounding analysis can be used as a screening tool to evaluate the relative impacts of scenarios or alternative conceptual models and help identify important parameters."

Response

Review of the WIPP PA may well require considerable expertise and effort. QA on computer codes and reviewer access to the modeling system are high priorities for SNL.

"Bounding or conservative" analyses will be considered for inclusion in the systems prioritization.

While there may be cases when bounding analyses are appropriate, they, as a general rule, are discouraged for compliance determinations. The reason for this is clear, the Standard encourages reasonableness in expectation. Bounding analyses may be useful for parameters that are of little consequence or alternative conceptual models that are clearly bounded by the conceptual model being pursued.

Comment T202

Page 55-56: VII. EPA Review of SNL Responses to selected EEG Comments, Comment No. 3 (part 2)

Area in document Volume 1, Appendix B

(Restatement) The EPA repeats the EEG's observations that the 1991 PA was not conservative, noting that "the 1991 PA report used mean/median values for most key parameters." The comment further notes that PA analyses suggest that "the maximum amount of brine saturation that will ever occur in an undisturbed room (at the 10^{-4} probability level) is about 30%. This seems to be inconsistent with some assumptions in the Test Phase Plan and elsewhere where testing under fully inundated conditions is considered important."

Response

The 1991 PA (and the 1992 PA) did not use mean or median values for "most key parameters." Mean or median values were used for many of the hundreds of parameters required in mathematical modeling, but not for the "key" parameters. Those parameters for which uncertainties were deemed to be large or which had a strong influence on model outcomes were sampled from a distribution (see Section 6.2 of Volume 3 of the 1992 PA).

As long as any portion of the waste is saturated with brine, inundated gas-generation processes are important. Note that if the brine saturation in the disposal area is 30%, then inundated gas-generation reactions will occur in 30% of the waste. Note also that all values of brine saturation after time zero, including the 30% value referred to here, are model outcomes, rather than model assumptions or externally imposed limits. They are simply observations of the results of simulations involving complex coupled processes. Conceptual models and data used in the 1992 PA for two-phase flow and gas generation resulted in a wide range of brine saturations in the undisturbed waste (up to about 60%, see Volume 4, Figure 4.4-3). These conceptual models and alternatives will be considered for inclusion in the systems prioritization.

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Comment T203

Page 56: VII. EPA Review of SNL Responses to selected EEG Comments, Comment Nos. 15, 25, 82

Area in document Volume 1, Appendix B

(Restatement) The EPA repeats the EEG's comment that level 4 activity levels used in calculating cuttings releases could not exist at 3,000 years or later, because radioactive decay would have reduced the activity to lower levels. Activity levels high enough at time zero to result in activity level 4 of 184.01 Ci/m^2 at 3,000 years would be higher than anything reported in the system.

Response

The EEG's observation on the 1991 PA is correct. Transportation and WAC requirements were not explicitly considered in the formulation of the activity levels for CCDF construction, and the activities reported at later times for the highest level exceed the 200 fissile-gram-equivalent requirement of the WAC. The decision to include "nontransportable" waste in the 1991 PA was deliberate, and was based on uncertainty about future transportation requirements. The effect also occurs in the 1992 PA.

The effect of the "nontransportable" waste on CCDFs is minor, and results in an overestimation of the lower-probability/higher-consequence cuttings releases. Figure 3-4 in Volume 2 of the 1991 PA (SAND91-0893/2) shows cuttings-only CCDFs calculated for a single realization using average-activity cuttings and cuttings of multiple activity levels.

Comment T204

Page 56: VII. EPA Review of SNL responses to Selected EEG comments, Comment No. 53

Area in document Volume 1, Appendix B

"EEG stated 'the effect of colloidal materials and chelation on radionuclide transport has not been addressed in PA to date, nor has the full interaction of gas pressurization on transport down MB139 been fully conceptualized.' Sandia's response merely referred to two previous responses, neither of which addressed colloids, chelation or transport down MB139. EPA agrees this issue needs to be addressed in the PA."

Response

All of these issues are presently being examined, and will be included in the system prioritization. Colloids and chelating agents in the disposal room are considered in the actinide source term program (see Experimental Program Plan [EPP] 5.3.2.2 and 5.3.2.1). Colloids and chelating agents in the Culebra are considered in the non-Salado Transport Activity (see EPP 5.1.2). Pressure-dependent fracturing of anhydrite marker beds is being examined in the field with the Hydrofracture Studies (see EPP 5.2.4.7), and transport of VOCs is being considered in the Salado Transport Activity (see EPP 5.1.4).

Comment T205

Page 57: VII. EPA Review of SNL Responses to selected EEG Comments, Comment No. 88

Area in document Volume 1, Appendix B

(Restatement) EPA repeats the EEG's request for modeling of a scenario that includes flow of Castile brine to the surface. The EEG indicates that SNL agreed in 1992 to consider four cases: "(1) E1 or E2 during drilling; (2) E1 while Castile brine is allowed to flow; (3) E1 followed by E2 after Castile brine is allowed to flow; and (4) E1E2 after both have been abandoned."

Response

These scenarios may be considered for inclusion in the SPM.

Comment Intro 1

Page 1

"For individual and ground-water protection, calculations have been done to 1000 years, whereas the Standards (40 CFR 191) required by the Act would require calculations to 10000 years."

Response

The compliance application will include performance assessments for the required time periods.

Comment MI-01

Page 2

"Very few new results are presented in volumes 1, 2, and 3 of the *1992 Performance Assessment*."

Recommendation:

"We look forward to additional results on undisturbed performance, disturbed performance, sensitivity and uncertainty analysis in future volumes."

Response

Volumes 4 and 5 of the 1992 PA were published in the fall of 1993, and contain the additional results mentioned.

Comment MI-2

Page 2

"In previous performance assessments, the DOE noted that the calculated CCDF's are at least an order of magnitude below the allowable limits in the EPA Standards. In the *1992 Performance Assessment*, for the case of total release from fracture-flow in the Culebra, no sorption, and a constant intrusion rate, the mean CCDF comes to within a factor of two or three of the EPA containment requirement [Vol. 1, Fig. 5-4]. Despite this factor of safety, the outlier CCDFs may lie in the zone of violation of the containment requirements. For dual porosity flow and the same parameters, the mean CCDF is about a factor of ten away from the containment requirement."

Recommendation:

"The DOE should show the full uncertainty band of CCDFs when comparison with the containment requirement (40 CFR 191) is made."

<u>Response</u>

The families of CCDFs and selected percentile curves are displayed in Volume 4 of the 1992 PA. The presentation of a single CCDF is consistent with the guidance in what is now Appendix C as follows:

> "Compliance with §191.13. The Agency assumes that, whenever, practicable, the implementing agency will assemble all of the results of the performance assessments to determine compliance with §191.13 into a "complementary cumulative distribution function" that indicates the probability of exceeding various levels of cumulative release. When the uncertainties in parameters are considered in a performance assessment, the effects of the uncertainties considered can be incorporated into a single such distribution function for each disposal system considered. The Agency assumes that a disposal system can be considered to be in compliance with §191.13 if this single distribution function meets the requirements of §191.13(a)." [40 CFR 191 App. C]

The recommendation that the entire family of CCDFs should be used in a compliance application suggests guidance that goes beyond the recommendations for implementing 40 CFR §191.13(a). The EPA is expected to provide relevant criteria in 40 CFR 194.

<u>Comment</u> MI-3a

Page 2

"Beginning with the 1992 Performance Assessment, "expert judgment" is used for
a: solubilities of actinides. . ."

Recommendation:

"As experimental solubility values become available (e.g. Nitsche et al., 1992), use them in performance assessment."

<u>Response</u>

The DOE is currently investigating the issue and an activity involving the collection of experimental solubilities will be included in the System Prioritization Methodology (SPM).

SNL is developing a model that describes actinide solubility and colloid stability that will utilize experimental results as they become available to replace the current solubility estimates based on expert judgement. The sensitivity of compliance performance assessment to radionuclides solubilities will be investigated during SPM. SPM will also help in determining data quality objectives (DQOs) for the experimental program.

Comment MI-3b

Page 3

"For retardation coefficients, the second modification of the Cooperation and Consultation Agreement between the DOE and the State of New Mexico specifies that retardation coefficients shall be set to zero unless there are experimental data otherwise. Results using zero and non-zero retardation coefficients appear in Chapter 5 of Volume 1."

Recommendation:

"As experimental retardation coefficients become available, use them in performance assessment."

Response

The DOE is currently investigating the issue, and an activity involving the determination of non-zero retardation coefficients will be addressed by SPM.

The 92 PA was not intended to be a compliance document. Ranges of many parameters, including retardation coefficients, were included to determine the sensitivity of various performance measures to parameter variations. SNL is currently conducting experiments designed to collect data upon which a defensible retardation model can be constructed, for use in future compliance PAs. Measured K_d data will be used directly to the extent possible and meaningful.

Comment MI-3c

Page 3

This quotation summarizes our view on "expert judgment":

> Expert judgments are not statements about nature but rather about beliefs. Nor are they statements which can be extrapolated to a larger population of events and beliefs. Therefore, while there is a chance that conclusions based on an

expert judgment may be true about the world, it is not a good idea to say so because there is no justification in the method which allows this (Fleming 1991).

Recommendation:

"We object to the elicitation of subjective probabilities as practiced to estimate the probability of inadvertent intrusion and we offer a specific alternative suggestion, beginning on page six. Our specific objections are detailed below."

"(a) The probabilities that have been elicited from panels for the purpose of estimating future intrusion intensity (Hora, von Winterfeldt and Trauth, 1991) are subjective probabilities. To call them "expert judgment" is to give them an aura of respectability they do not deserve. The methods for eliciting such probabilities come from statistics (Savage, 1954) and experimental psychology (Edwards, 1954). The term "recognized expert" is usually applied to an individual who has published extensively in the particular field, but the interpretation of a "recognized expert" carries no more weight, and is not necessarily better, more accurate, or more valid, than the interpretation of an inexperienced investigator. Experience does not guarantee accurate interpretation; knowledge does. To foretell the future, each of us has insights, and scientists do not possess more insights than the common person. While the elicitation of opinions is valid, the elicitation of expert opinion on the future is gratuitous. That there has been no attempt to establish the qualifications of the panel members as experts on the future is telling. They simply are not."

"(b) The WIPP Performance Assessment Department invokes the interdisciplinary nature of an expert judgment panel as a reason to use such a panel. But "interdisciplinary" is not a synonym for "good" or "appropriate" any more than "single disciplinary" [sic] is a synonym for "bad" or "inappropriate." The advantage of multidisciplinary data interpretation over interpretation by an expert in a single discipline is not at all clear. For example, the marker panel (Rechard et al, 1993; Table I) lists experts in materials science, architecture, linguistics, communications, etc. How is the judgment of a linguist on materials hardness and durability relevant? Either the linguist accepts the material scientist's judgment, in which case the interpretation is not interdisciplinary, or the two differ in interpretation, in which case the material scientist's interpretation is clearly the more valid and that judgment should not be diluted."

"(c) The panels are not representative of modern United States, not representative of the modern world, and not representative of the historical continuity of the human race. There was only one woman on the markers panels [sic] and none in the futures panels [sic]. There are no representatives of indigenous cultures of the southwestern United States."

"(d) The elicitation process used was open-ended. While it is true that what people will want to mine over 10,000 years is unknown, let alone where they want to mine it, for a specific area the problem is simpler. An example is the Outer Continental Shelf Lands Act, which allows for oil and gas drilling in the sea beyond the three-mile limit, with a clause for "other minerals." When the Outer Continental Shelf Lands Act was first passed in the early fifties "other minerals" referred to sulfur. By the mid-1970's the focus of other minerals became construction aggregates around coastal cities. In the early 1990's, it is manganese crusts. For a specific location, with geologic information, we know what can be mined now and in the future. The propensity to mine will only change if there is a metamorphosis, or society changes its needs dramatically. If that had been borne in mind, the problem is much more circumscribed, and less fiction would have resulted."

"(e) Results of the open-ended elicitation process used by Hora (Hora, von Winterfeldt and Trauth, 1991) appears to have been used selectively. If a more circumscribed process had been used, then the methods available to combat cognitive bias (Tversky and Kahneman, 1974) could have been used. Unfortunately, the results used in the 1992 Performance Assessment reflect strongly the intervention of the analyst. The final result used a form

$$\lambda_t = d(1-p_1p_2)$$

where λ is the intrusion intensity, number of holes per time, d is the raw drilling intensity number of holes per time, p_1 is the probability of markers surviving, and p_2 is the probability that surviving markers are effective in deterring drilling, all functions of time. The paradigm was not elicited from any one panel but the result is a mixture of results from the panelists who probably did not understand how their inputs would be used."

"(f) A flagrant and important abuse of the analyst-assessor role is when Hora decided that there will be no intrusions allowed after 300 years (Hora memo, Vol. 3, p. A-76, last sentence). If institutional control prevents drilling from 0 to 100 years after closure, and by fiat no intrusions after 300 years, then the 1992 Performance Assessment considered only intrusion from 100 to 300 years. Are only flow and transport to be considered from 300 to 10,000 years? This is clearly counter to the spirit and letter of analyzing human intrusions for the entire regulatory period."

"(g) Most of the elicitation process is given in the Hora memo, volume 3, pp. A-71 through A-99. This memo includes a FORTRAN program to sample among the panels [sic], and produce realizations of intrusion intensities as functions of time for use in the 70 Monte Carlo runs. The computer program does not work. On page A-94, line 13, there is a three-dimensional array BOSTAB2 which is undimensioned and undefined, thus the program cannot possibly work. Since May 1992, EEG has requested a working copy of this program, first from Professor Hora, then from WIPP Performance Assessment Department, to no avail."

"(h) Appendix D of Vol. 3 of the 1992 Performance Assessment contains 12 pages of realizations of drilling intensity functions that are purportedly the results of using Hora's algorithm. The graphs show the intrusion rate and the cumulative number of intrusions as a function of time to 10,000 years. As noted above, Hora's program sets the intrusion rate to zero after 300 years, therefore these graphs cannot possibly be the results using Hora's computer program. If the analyses used a zero intrusion rate after 300 years, it is misleading to show the Appendix D graphs which show intrusion rates to 10,000 years."

"(i) **This is EEG's specific suggestion.** We suggest a simplified, focused and understandable alternative.

Figure 1 shows what we believe the exploratory drilling rate to be in any specific area. This figure shows how we see the evolution of oil and gas drilling as a function of time.

First there is a historical record of drilling in this area. This rate may be high, or it may be close to zero, but it is known. Call it a holes per area per year, a > 0. The U.S. Environmental Protection Agency's guidance of thirty boreholes per kilometer^2 over 10,000 years is such a rate.

We extend the historical drilling rate some time in the future. Call this b years, b > 0. Geologic knowledge should be used for this extension. If there is current oil and gas drilling, then it is likely for the exploration and development to continue for some time. If there is no current drilling in this area, then there will probably not be any drilling until we discover some new mineral to explore for in this area. This extension should extend beyond the period of active institutional control.

Given our present understanding of energy economics, we may postulate a decrease in oil and gas drilling, after a period of time, due to either exhaustion of the resources, or technological developments in some other fuel sources, or both. This decline can be represented by an experimental [sic] decay function, $y = y_0 \exp^{-ct}$. The rate of decrease is characterized by a single parameter, c.

For the long term, there should be a rate of intrusion that is

(a) non-zero; and

(b) above the USEPA threshold probability for events and scenarios [sic] to be considered or 10^{-8} per year. Call the rate d holes per area per year, $d \geq 10^{-8}$ per year.

The long-term, low intrusion rate cannot be zero because the hazard of the waste in the repository would not have decayed to harmless levels in just a few thousand years. To ignore such probabilities is to do an incomplete analysis.

The parameters a, b, c, and d completely specify the rate of inadvertent human intrusion in a readily understandable way. Subjective elicitation can now focus on these four parameters. Experimental methods are available to improve the elicitation process when it is focused on continuous variables (Seaver, Von Winterfeldt and Edwards, 1978)."

Response

(a) The panel members were chosen through an extensive and impartial process. They were selected because they have made part of their life's work the study of methodologies for futures research and have thought long and hard about the path that the future may take. The goal here is to scope the collection of foreseeable futures for a very broad discussion of who might be intruding, how they might be intruding, and why they might be intruding. This information can be applied to efforts to develop effective institutional controls. A discussion of how frequently future societies might be intruding can be utilized to determine how to implement EPA's guidance that exploratory drilling for resources is the most severe intrusive event that must be examined for compliance.

The sentence "Experience does not guarantee accurate interpretation; knowledge does" is confusing. One of the definitions for "knowledge" in The American Heritage Dictionary is "Familiarity, awareness, or understanding gained through experience or study." Experience (defined as "Active participation in events or activities, leading to the accumulation or knowledge or skill") is the basis upon which available data and information can be interpreted. Data in and of itself is useless without proper interpretation and appropriate application.

(b) The entire Markers Panel was not asked to interpret hardness and durability data. The Markers Panel was asked to develop marker design criteria, which have materials and communication aspects, among others. Each member of the team brought to the deliberations the contributions of his or her discipline to the development of an effective marker system. Recommendations based on a narrow materials science perspective (e.g., use titanium for markers as it is highly resistant to corrosion) may be subject to other constraints (human beings will recycle any materials from an unattended marker system if recycling is cost effective) and may need to be modified (use only a small subset of buried markers made from titanium to make mining an uneconomical prospect).

The point is that the teams are interdisciplinary, not the individual experts. The rationale for using teams of experts from many fields is that all questions related to human intrusion and passive institutional controls cannot be fielded from any single field of study. The issues to be addressed demand this organization. A discussion of interdisciplinary teams is provided in Bonano et al. (1989).

(c) The selection of the members of the Futures Panel and the Markers Panel focused more on the expertise within the pertinent disciplines rather than on strict representativeness. It should be recognized that the community of scientists active today is probably not a reflection of the cultural and ethnic make-up of the U.S. or the world. The range of organizations from which the experts were selected (Natural Resources

Defense Council, universities, institutes, etc.) provides rich diversity in political and environmental organizations. It should also be noted that both teams within the Markers Panel believed that testing was necessary before a final design for a marker system could be selected. In part, this testing was seen as necessary to ensure that messages are interpretable by individuals from a variety of cultures and whose societies have varying levels of technological sophistication. Such testing addresses concerns about diversity in the initial design process, and the inability to include individuals from all such groups in the initial deliberations.

(d) This comment proposes that the experts be directed as to what potentially intrusive activities to study. We believe that this is inappropriate and would be seen by peer reviewers as excessive direction from the analytic staff. The intent in examining possible future societies is to provide insights into the development of institutional controls and in how to implement EPA's guidance on frequencies of human intrusion. A narrow consideration of possible future societies would be inappropriate because it would not provide these insights.

(e) The members of the Futures Panel and the Markers Panel understood how their inputs would be used in performance assessment. This was accomplished during the training sessions held with the panel members and is supported by copies of the viewgraphs that were distributed and discussed. The training sessions allowed ample time for any questions to be clarified.

The Futures Panel was shown a viewgraph entitled "Logic Tree for Deterrence by Markers Given Time, Society, Mode of Intrusion, and Marker Criteria." This viewgraph shows the concept of intrusion deterrence being a function of a set of sequential states, each conditional on the previous state occurring, dealing with the physical markers and their ability to communicate. The probability of deterrence is then calculated as the product of this set of sequential states, which is consistent with the equation used to calculate the effective drilling intensity.

The Markers Panel Issue Statement describes the probabilities that are to be elicited, all of which are conditional on the previous sequential state occurring. The Markers Panel was shown a viewgraph entitled "How will the Expert Judgments be Used in the WIPP Performance Assessment?" This viewgraph states, in part, that marker system effectiveness will be used to modify the frequencies of intrusion as developed by the Futures Panel. This was reinforced by a training example showing the calculation of needed information from a series of sequential states that together define the information required.

This part of the comment seems to contradict part d of this comment and also the EEG's specific suggestion.

(f) This comment is factually in error. The paragraph referred to deals exclusively with the findings of the Boston team and not with the findings of all four teams. The paragraph from which the sentence was extracted reads:

The Boston team provided assessments for the drilling intensity that are conditional on both time and level of technology. The responses for exploratory drilling for hydrocarbons are shown in the following tables. Exploratory drilling for hydrocarbons was not thought to extend further than 300 years in the future.

This paragraph is followed immediately by a table labeled "BOSTON TEAM---DRILLING INTENSITY DISTRIBUTIONS.

Particularly in light of the following discussions of the findings of the three other teams, the assembly and analysis of results, and the listing in Volume 1 (pp. 4-8 and 5-4) of the six times of intrusion (100, 175, 350, 1000, 3,000, and 7250 years after decommissioning), we do not understand the confusion on this point.

(g) EEG's observation that they have been requesting the computer program since May 1992 is perhaps in error. The first draft of Dr. Hora's memo explaining how the judgments of the Futures and Markers Panels could be incorporated into the performance-assessment calculations (including a FORTRAN code for assembling human intrusion judgments) that SNL received was dated June 9, 1992.

The comment refers to an early version of the program that was modified prior to implementation. This amounted to a simple bypass of the program statements concerning the array BOSTAB2. This was done when a decision was made to include only exploratory boreholes, as required by 40 CFR 191. The correct version of the program and the data files were forwarded to the EEG in December 1993.

(h) The response to this comment is the same as part f of this response.

(i) The EEG proposal does not include any consideration of the potential impact of passive institutional controls in deterring inadvertent human intrusion. This is contrary to EPA's intent as stated in the guidance to 40 CFR 191:

"The Agency assumes that, as long as such passive institutional controls endure and are understood, they: (1) can be effective in deterring systematic or persistent exploitation of these disposal sites; and (2) can reduce the likelihood of inadvertent, intermittent human intrusion to a degree to be determined by the implementing agency. However, the Agency believes that passive institutional controls can never be assumed to eliminate the chance of inadvertent and intermittent human intrusion into these disposal sites."

The WIPP Project will continue to follow EPA's guidance related to inadvertent human intrusion and passive institutional controls in future performance assessments.

As an alternative to the procedures used by the WIPP Performance Assessment staff, EEG offers another methodology. Of course, in a human intrusion analysis, there is no obvious right or wrong way to approach the problem. Thus, one must return to basic principles in order to design the process. These principles include allowing the experts freedom to fully express their judgments, not biasing the experts either through the questions asked or the way the questions are asked, collecting the rationales for the judgments as well as the numerical encoding of the judgments, and asking questions relating to physically realizable values.

With regard to the first principle:

The WIPP analysis allows the experts to provide their own structure for analyzing the problem rather than having to accept an arbitrary structure such as that imposed by the EEG procedure. There is a growing body of evidence (Armstrong, Denniston, and Gordon [1975]; Hora, Dodd, and Hora [1992]; MacGregor, Lichtenstein, and Slovic [1988]) that demonstrates that the decomposition or structure used to analyze a problem is one of the most important determinants of the findings. This has long been known to decision analysts and is often used in probability elicitation for complex issues (Electric Power Research Institute [1986]). The EEG proposal would constrain the experts and precondition their answers. Moreover, the results would present an unduly limited view of possible intrusion modes.

With regard to the second principle:

Biased results can derive from several sources. The experts may be selected in a way that the group is biased. Psychological biases (e.g., overconfidence, availability, anchoring) may enter in the elicitation process. Questions may be asked in such a way that the answers are conditioned. We believe that every effort was made to keep the two panels as neutral in a collective sense as possible. Extensive training in psychological biases was conducted prior to the elicitation. This training lasted a little more than one-half day and included lecture, discussion, examples, and training quizzes. Recognized decision analysts conducted the elicitation sessions. During these sessions, the decision analysts were alert to identifying psychological biases and to assisting the experts in counteracting these biases.

With regard to the third principle:

The expert elicitation methodology explicitly put the importance of rationales ahead of the numerical findings. This is because the numerical findings are not credible without the rationales and the rationales are apt to be more useful in the long run than the numerical assessments. A key to obtaining these rationales is to allow the experts to analyze (decompose) the problem in a manner that matches their thinking. The EEG proposal would substitute a rigid model and would thwart obtaining the rationales for the judgments.

With regard to the fourth principle:

Specialists in expert judgment have long recognized that experts should be asked to respond to questions about physically realizable quantities and not be asked to respond to questions about parameters that do not have a direct physical embodiment (Winkler [1967]; Cooke [1992]). The exponent in the proposed EEG model is a parameter that does not have a directly realizable value. What meaning would the probability elicitation have if the expert did not believe in this model? How could an expert respond to questions about parameters that she/he does not believe in?

Regarding the specifics of the proposal, what justification is there for the exponential decay in drilling frequency? What justification is there for an exponential decay over time plotted on a log scale? Have these been derived from economic principles? They seem to be judgments made as a convenience to the analyst. It should be clear that the path taken by the WIPP PA team requires more effort on the part of the experts and the analysts because convenient assumptions are not substituted for deep and hard thinking. The EEG is requested to provide any references supporting the use of the exponential decay equation and the log time scale postulated.

Consider also the use of the historical drilling rate, a. The guidance provided by the EPA in 40 CFR 191 states that:

> "Therefore, inadvertent and intermittent intrusion by exploratory drilling for resources (other than any provided by the disposal system itself) can be the most severe intrusion scenario assumed by the implementing agencies."

The EPA assumes this because:

> "Furthermore, the implementing agencies can assume that passive institutional controls or the intruders' own exploratory procedures are adequate for the intruders to soon detect, or be warned of, the incompatibility of the area with their activities."

Therefore, the use of the historical drilling rate (including exploration and development) is a judgment by the EEG that the EPA's guidance on the matter should not be followed. While the EPA's guidance states that only exploratory drilling be considered, the EEG has made a judgment that exploratory drilling and drilling for the development of resources should both be considered. The use of a drilling rate unmodified by the impact of active and passive institutional controls is also a judgment by the EEG to ignore the EPA's guidance on the efficacy of institutional controls.
Consider next the variable b, the extension of the historical drilling rate some time into the future. The EEG proposal states both that "Geologic knowledge should be used for this extension." and that "This extension should extend beyond the period of active

institutional control." Should b be based on geologic knowledge c G's policy judgments?

On page 6 of the comments, the sixth line from the bottom, a justification is attempted for a 10^{-8} cutoff on drilling rates. The statement is made that the drilling rate cannot fall to zero because the radioactive waste would still be harmful. What is the basis for linking exploratory drilling to the hazard of the waste?

In the discussion of the impact of passive institutional controls on inadvertent human intrusion, the EPA states in the guidance to 40 CFR 191 that:

> "The Agency assumes that, as long as such passive institutional controls endure and are understood, they: (1) can be effective in deterring systematic or persistent exploitation of these disposal sites; and (2) can reduce the likelihood of inadvertent, intermittent human intrusion to a degree to be determined by the implementing agency. However, the Agency believes that passive institutional controls can never be assumed to eliminate the chance of inadvertent and intermittent human intrusion into these disposal sites."

It is for this reason that the WIPP Project has not assumed that the probabilities of such intrusions can be reduced to zero.

The EEG should realize that this proposal (and its basic framework) involves replacing the explicit use of expert judgment from an independent panel with the implicit judgment of the EEG ("Figure 1 shows what we [emphasis added] believe the exploratory drilling rate to be in any specific area. This figure shows how we [emphasis added] see the evolution of oil and gas drilling as a function of time."). Judgment is still employed, but now the very important framework is provided by a different entity. Part of the proposal also includes EEG's judgment that EPA's explicit guidance for (1) considering the impact of passive institutional controls in deterring inadvertent human intrusion and (2) including the rate of exploratory borehole drilling for the consideration of human intrusion should both be ignored.

The above discussion indicates that any consideration of human intrusion requires judgment, no matter what attempt is made to simplify the process. Any new treatment of human intrusion for the WIPP should be addressed by an independent expert judgment panel with appropriate expertise. The process should be approached in a manner whereby all unknowns/parameters are investigated in a way that reduces the assumptions made by interested parties.

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Comment MI-4

Page 8

The EEG is distressed about the status of documentation of computer codes used in the 1992 Performance Assessment.

Recommendation:

"DOE is urged to come up with a workable system of providing EEG with relevant documentation, so that EEG has reasonable access without undue burden on the workers."

Response

Documentation of computer codes necessary for compliance is in progress and will be completed to the extent needed to support the compliance application.

This documentation of WIPP performance assessment models and codes necessary for compliance is anticipated to be completed by the end of September 1995 or shortly thereafter.

Comment MI-4a

Page 9

"For PANEL, the actual equation solved is given at page 1-33. The documentation is needed to see if it is true."

Recommendation:

"DOE is urged to come up with a workable system of providing EEG with relevant documentation, so that EEG has reasonable access without undue burden on the workers."

Response

PANEL is currently at QA level "C" and is available on the remote access PA computer system in executable-only form. Work is underway to bring the code to full QA level "A" by September 30, 1995. Documentation and the source code will be available at that time.

Comment MI-4b

Page 9

SECOTP2D needs full documentation. The following questions need to be addressed in such documentation.

- How does SECOTP2D handle the conversion of the source term from zeroth dimensions? How is the injection rate, Q, determined? How does the source, Qc, spread?
- How are matrix diffusion effects in the Culebra calculated?
- Are there benchmarking results for the codes used for flow and transport calculations for the Culebra?

Recommendation:

"DOE is urged to come up with a workable system of providing EEG with relevant documentation, so that EEG has reasonable access without undue burden on the workers."

Response

Documentation of SECOTP2D is in progress and will be completed along with other WIPP performance assessment models necessary for compliance by September 30, 1995. Documentation is currently available for PRESECOTP2D, POSTSECOTP2D, PRESECOFL2D, and POSTSECOFL2D.

We acknowledge your comment and the following information will be included in documentation.

The 1992 PA involved only 2-D flow and transport calculations. The source term Q is introduced at a single computational cell. (This could be viewed as a vertical "line source", but this is redundant terminology for a 2-D calculation.) Physically, the source term is not "zeroth dimension". Certainly the source is not modeled as being spread out horizontally. The Qc term is provided by a decoupled calculation involving two-phase flow at the site and flow up the borehole (calculated by BRAGFLO and PANEL) as indicated in the 1992 PA documentation.

The "classic Neretnieks equation for matrix diffusion" is not used in our calculations. That approach is criticized in Huyakorn et al.,1983. The method outlined in Huyakorn is followed in the 1992 PA calculations.

There are benchmarking results comparing SWIFT and STAFF2D to the SECO codes. The SWIFT results agree with SECO- FLOW, as expected, except for one peculiar boundary condition problem for which SWIFT degenerates to only first order accuracy. These comparisons are presented in detail in an open literature publication from 1990. In 1993, open literature publication of the description of the SECO-TRANSPORT code (with many details) included a sample comparison with the STAFF2D algorithm (Roache, 1993a; Roache, 1993b; Salari et al., 1992; Roache, 1992; Roache, 1991; Roache et al., 1990). There is no need to perform extensive benchmarking, since the sample calculation already confirmed what is obvious from examination of the algorithms; STAFF2D is only first order accurate, while SECO-TRANSPORT is second order accurate, as well as 1-2 orders of magnitude more efficient for the same time step. There is no purpose served in examining STAFF2D since it has been superseded. The 1992 PA calculations were snapshots of progress at that time, requested by the National Academy of Sciences and DOE.

References

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Comment MI-4c

Page 9

The technical paper by Roache, 1993, touts the TVD algorithm, but does not define it. The algorithm begins with a variable transformation, and a key variable J is not defined. No results are given for verification of the dual porosity option in the paper.

Recommendation:

"DOE is urged to come up with a workable system of providing EEG with relevant documentation, so that EEG has reasonable access without undue burden on the workers."

Response

Documentation of all WIPP performance assessment models and codes necessary for compliance will be completed by September 30, 1995 or shortly thereafter.

The technical papers written and cited are not intended to be a substitute ultimately for full user documentation. However, neither are internal Sandia reports a substitute for open literature publications which have the advantage of wider scrutiny. We have produced 6 papers for external release on the SECO suite of codes, and more are in preparation.

"TVD" is a class of Total Variation Diminishing algorithms. There are more accurate algorithms than TVD in existence, though not, to our knowledge, embodied in robust production codes. There are also several algorithm families of comparable accuracy. Any tone of "touting" TVD occurs inadvertently due to the comparison with the inaccurate algorithms used in STAFF2D, for example. The variable J is the Jacobian of the transformation. No results were given for the dual porosity option because of time and space limitations; these results will be included in open literature papers now in preparation as well as in the final documentation.

Comment MI-4d

Page 9

"Finally, it is often claimed that because a computer code is constantly being revised, its documentation status is affected."

Recommendation:

"DOE is urged to come up with a workable system of providing EEG with relevant documentation, so that EEG has reasonable access without undue burden on the workers."

Response

We agree that the documentation status of specific code versions used in PA calculations are not affected by subsequent revisions. However, none of the codes used in the 1992 PA Snapshot were at QA level A, a requirement for release of source code. The primary reason for this is that the PA effort was focused on annual snapshots of compliance, model development required to capture alternate conceptual models, and QA of current code versions. PA codes necessary for compliance will be at a QA level A by the end of September 1995 or shortly thereafter. Codes are currently being made available to EPA and EEG on the WIPP Open VMS Network. Prioritization of the availability of codes is being set by EPA.

Comment MI-05

Page 10

"The *1992 Performance Assessment* elucidates the role of the Culebra as an isolation barrier. From Figure 5-6 of Vol. 1, it is seen that if there is enough flow in the Culebra, it does not offer much help in isolation. With matrix diffusion and sorption, the Culebra is a major natural barrier."

Recommendation:

"The DOE should delineate the roles of matrix diffusion and sorption through accelerated experimentation."

Response

SNL is currently conducting experiments designed to collect data upon which a defensible retardation model can be constructed, for use in future compliance PAs. These experiments examine the roles of matrix diffusion (physical retardation) and sorption (chemical retardation), and resulting model will also contain both mechanisms.

Comment MI-06

Page 10

"While the DOE has analyzed the beneficial effects of gas generation, the EEG continues to be concerned that the deleterious effect of gas generation, particularly the opening of new discharge pathways, has not been analyzed."

Recommendation:

"In future analysis, the deleterious effect of gas generation should be included."

Response

This question raises issues that will be included in the SPM.

In the past the project has studied gas generation mechanisms, its impact on fluid transport in the Salado, and its effect on room closure mechanisms. Currently the project is evaluating the potential for gas-driven enhanced permeability in the markerbeds as well as gas-driven spalling mechanisms.

The deleterious effects of gas generation are recognized. This question is very similar to EPA comment 118 and our answer is the same; these issues are not fully resolved and the project intends to use the SPM to identify acceptable methods for dealing with these concerns.

Comment MI-07

Page 10

"Currently, there is no correlation between sampled variables using Latin Hypercube sampling. In real life, many of the variables are related."

Recommendation:

"The performance assessment should either give reasons why physical correlations have been ignored, or show results with correlations."

Response

Correlations between variables will be included if and when defensible information is available to quantify them. In the absence of defensible data to support correlations, it is appropriate to treat variables as independent. In general, so long as the sampled parameter range includes the range of coupled parameters, we believe that the treatment of uncoupled parameters, as used in the 1992 PA, is conservative.

Comment MI-08

Page 10

The '92 PA is incorrect and unclear on the extent of natural resources near the WIPP site. There are more than oil and gas wells than the 56 indicated in the PA. DOE is basing its estimate on incorrect information from 1986. EEG showed (Silva and Channell, 1992) some of this [sic] data were incorrect.

The PA also states that with the exception of one well, resource extraction is not allowed within the proposed land withdrawal boundary.

The 1992 WIPP Land Withdrawal Act recognizes the validity of two specific oil and gas leases within the WIPP site boundary. The owner of one of those leases has recently filed an application for permits to drill eight directionally drilled oil wells that would be completed within the WIPP site boundary, but at depths greater than 6,000 feet to produce oil from within the WIPP site boundary. Thus, there seems to be no basis for the statement in the PA which indicates that resource extraction is not allowed within the boundary, except that the restriction is contained in the second modification to the Cooperation and Consultation Agreement (C & C) between the DOE and State of New Mexico.

EEG believes the second modification to the C & C has been incorrectly interpreted. The agreement is not limited to the first 6,000 feet of depth.

Recommendation:

"As in all DOE reports, performance assessment reports should accurately reflect the status of resource development near the WIPP site."

Response

The WIPP project continues to collect information on the existence and nature of natural resource-related boreholes in the vicinity of the WIPP site. The WIPP position on this issue was discussed at length in the DOE's response to EEG-50 (Memo Arthur to Neill, November 3, 1992; Copy sent to EPA) and will not be reiterated here.

Comment MI-09

M

Page 12

Silva and Channell (1992) showed that the document, "DOE Implementation of the Resource Disincentive Plan in 40 CFR 191.14(e) at the Waste Isolation Pilot Plant" (U.S. DOE, 1991) is inconsistent in reporting the number of oil and gas leases within the WIPP site boundary and the production status of those leases.

In the No-Migration Variance Petition (U.S. DOE, 1990a) the DOE states:

". . . Oil and gas exploratory drilling requires permits from the state and it is unlikely that prospective future well drillers would not be informed about the existence of WIPP. As an additional protective measure, the DOE has purchased all oil and gas leases in the area of the WIPP site to prevent any exploration now and in the future (Section 6.3.2)."

With respect to petroleum exploration and the human intrusion issue, the last sentence in this paragraph is simply wrong.

Weart (1983) and Brausch et al (1982) failed to recognize the potential crude oil resources for this area. Crude oil is now being produced from the former control zone IV.

Recommendation:

"The performance assessment effort must use the latest and verifiable data on oil and gas production near the WIPP, because the extent of oil and gas resources in this area is likely to be an important determinant of inadvertent human intrusion."

Response

The WIPP project continues to collect information on the existence and nature of natural resource-related boreholes in the vicinity of the WIPP site. The WIPP position on this issue was discussed at length in the DOE's response to EEG-50 (Memo Arthur to Neill, November 3, 1992; Copy sent to EPA) and will not be reiterated here.

Comment MI-10

Page 13

"One of the most important codes in the WIPP performance assessment is BRAGFLO and a brief summary is given in vol. 3, section 1.4.1. Equations 1.4.1-1 and 1.4.1-2 use rate constants and mole fractions (called "stoichiometry factors") to calculate the rate of gas generation. These factors, although not specifically referenced in this section, are referred to in the discussion on pp. 3-44 to 3-45. Median corrosion gas production rates are given as 6.3×10^{-9} moles H_2/m^2-s for inundated steel and 0.1 [-] for humid steel under aerobic conditions, and 0.5 [-] for inundated steel under anoxic conditions. An analogous set of rates are given for microbial gas generation, with units of moles of gas/kg cellulosics given only for inundated conditions. It should be noted that in the development of the equations on pp. 1-24 to 1-26, the rate constants and stoichiometric factors are given with perfectly acceptable units. Why aren't the dimensions the same for all these rates, if they are used for the same variable in BRAGFLO? How can a corrosion rate have the units of moles per unit area of exposed substrate in one case and no units in another? How can a dimensionless variable be used in the same place in an equation as a variable with units?"

Response

The gas production rate associated with inundated corrosion has units of mole gas/m^2-s. The units for the gas production rate associated with humid corrosion are the same. What is reported and stated in vol. 3 p. 4-46 is the relative humid rate. The product of the relative humid rate and the inundated rate is the humid rate. Similarly for biodegradation, the units for gas production rate due to biodegradation are reported as mole gas/kg-cellulosic/s not moles gas/kg, vol. 3 p. 3-50. The reason these units are different are due to differences in the way the rates are measured experimentally. Both corrosion rates and biodegradation rates are converted in the BRAGFLO preprocessor to units of [kg-metal/m^3/s] or [kg-cellulose/m^3/s] as required by BRAGFLO, vol. 3 p. 1-22.

Comment MI-10a

Page 14

"Although the assumption that radiolysis will contribute only negligible hydrogen formation at WIPP appears to have found general acceptance, the data developed by Kosiewicz (1981) show this need not be the case. In fact, the gas generation problem was first noticed in stored drums of TRU waste in which hydrogen had been generated by radiolysis. Moreover, the microbial generation model does not recognize the dependence of the microbial gas generation rate on the initial and continued presence and availability of microbes."

Recommendation:

"The gas generation calculations should include
(a) methane generation,
(b) radiolytically generated hydrogen."

Response

Radiolysis of combustibles and of brine is included in the reaction-path gas generation model currently under development, and will be evaluated as part of the SPM for inclusion in future (compliance) PAs. Current experimental information suggests that H_2 production by brine radiolysis is insignificant if actinide solubilities are as predicted by the expert panel. If current and planned experiments determine actinide concentrations (including colloids) to be significantly higher than predicted, then radiolytic hydrogen production may be significant and will be included in future PAs. Methane generation is addressed in the response to the next comment, MI - 10b.

Comment MI-10b

Page 14

"A more serious question arises about the use of these results. The gas generation rates and stoichiometry factors cited are those calculated by a model and are thus the result of model inputs rather than experimental data. Table I summarizes the results of the Sandia scientific investigations into gas generation, and distinguishes model calculations from experimental measurements. Model results are only as good as model inputs. Some model inputs include unsupported assumptions, such as the failure to include methane. Experimental data exist -- see Table I -- but have not been used in modeling. Moreover, as the Table I shows, models given different gas generation rates when given different inputs and assumptions, and the median of such calculated rates has little validity."

Recommendation:

"The relationships in the gas generation model should be validated before the gas generation model is incorporated into BRAGFLO."

Response

Methane production was not included in the 92 PA calculations. However, microbial methane production (methanogenesis) is included in the reaction-path gas generation model currently being developed. The SPM will examine the utility of the reaction path model. It should be noted that current experimental information has not resolved whether methanogenesis will occur, and, if so, whether it will result in a net increase or decrease in the total amount of gas in the repository. Current and planned experiments investigate microbial gas production under a range of conditions. The effect of the availability of microbes is being investigated (implicitly) by varying parameters such as the amount of nutrients in the various experiments, inoculation, humid vs. inundated conditions, etc. The possible long-term existence of microbes is simulated by non-inoculated experiments. The gas generation rates were based on experimental data and expert estimates. Additional relevant experimental data continue to be collected.

Comment: MI-11a

Page 16-17

In 1984 an EEG consultant analyzed potential nuclear criticality in the Culebra Aquifer and concluded that this needed to be thoroughly evaluated by DOE.

The potential nuclear criticality could occur if:

(1) sufficient quantities of a fissile radionuclide such as Pu-239 or U-233 are adsorbed on a large enough volume of aquifer matrix;

(2) there is sufficient hydrogen or other moderator available in the brine or matrix;

(3) the matrix or brine does not contain sufficient quantities of stable nuclides that can "poison" the reaction.

EEG's analysis indicated that, with the expected elemental composition of the brine and the Culebra aquifer matrix, nuclear criticality could occur in a block 7 m high x 0.5 m wide x 1 m long if the product of the distribution coefficient (K_d, mL/g) and plutonium solubility (moles/L) was greater than about 5.6×10^{-5} moles/g.

The possibility of a K_dS product of $> 5.6 \times 10^{-5}$ moles/g is credible. For example, the probability distributions for K_d and solubility from Volume 3 of SAND 91-0893 (pages 2-104 and 3-64) have approximate probabilities of occurrence of:

Species	Probability $K_dS > 5.6 \times 10^{-5}$
Pu^{5+}	0.025
Pu^{4+}	5×10^{-4}
U^{6+}	0.25
U^{4+}	0.11

Two pages were devoted to discussing nuclear criticality in the *1991 Preliminary Comparison* (Volume 1, page 4-52). SNL recognized that sorption can also occur in the backfill and at certain components of the seal system as well as in the Culebra Aquifer. The very remote possibility of a high-yield nuclear explosion is also discussed. We find no analysis of nuclear criticality in the *1992 Performance Assessment*. No schedule has been given for performing additional criticality evaluations.

EEG also believes the possibility of a high-yield nuclear explosion is very remote. One concern is with an instantaneous criticality excursion in which there is a brief burst of energy, neutrons, and gamma radiation. Perhaps more likely in this situation, where fissile material is being added very slowly in a solution, is a delayed criticality where the system does not become promptly critical. Such a system would behave much like a nuclear reactor and could produce fissions, perhaps in bursts, for extended periods of time. This phenomena has occurred in several process criticality accidents in the U.S., e.g. at Hanford in 1962 one system boiled for 37 hours. Also, the Oklo "natural reactor" in Gabon is believed to have operated in a similar fashion.

It is not obvious that a criticality accident would have a significant effect on a repository waste disposal system, even if a criticality accident occurs. Considerable heat would be produced, some brine would be vaporized, and minor amounts of fission products would be formed (it takes 8×10^{20} fissions to produce one curie of cesium-137). Also, the relatively high K_d values that would be necessary to make criticality possible are otherwise a benefit because the retard radionuclide transport.

Recommendation:

"The criticality issue needs to be thoroughly evaluated before it can be concluded that its effects are negligible.

Response

Criticality will be considered in analyses for event and process screening. A decision will be made whether to include it in the SPM.

Comment MI-11b

M

Page 17

"Subsidence could occur in the area overlying the WIPP some time after repository decommissioning. Subsidence can also occur from nearby potash mining. The *1992 Performance Assessment* identifies an event TS which is subsidence from mining of potash, but TS has not been analyzed. . ."

"No evaluation has yet been made of subsidence from potash mining. There are significant potash resources within the WIPP site boundary. However, the USEPA Standard requires analysis of only resource exploration drilling on site. But, it is appropriate to consider subsidence effects from potash mining offsite. . ."

Recommendation:

"EEG believes that subsidence effects need to be evaluated in much more detail and incorporated, in some manner, into the human intrusion scenarios."

Response

As noted in the 1992 preliminary PA (e.g., V. 1, p. 3-11, l. 16-19; V. 1, p. 6-3, l. 19-24), the effects on groundwater flow and radionuclide transport of subsidence related to potash mining will be examined using the regional three-dimensional hydrologic model that has been developed since the 1992 PA was completed.

Comment MI-11c

Page 18-21

The E1, E2 and E1E2 scenarios assume that the only material reaching the surface is drill-bit cuttings and some "cavings" from the annulus about the drill bit in the waste storage room. Brine flowing to the surface from encounter with a pressurized Castile brine reservoir was not assumed. EEG believes that brine flows to the surface should be assumed and that the consequences could be significant for the E1E2 scenario. Our reasons follow:

Sandia and DOE have described typical drilling practices elsewhere (Appendix C of SAND 89-0462 and in DOE February 7, 1990 response to EEG's comments on the Draft Supplement EIS). These responses explain how it is possible to have very little flow to the surface by closing in blow-out preventers within a few minutes, determining the pressure, and then preparing drilling mud of sufficient density to stop the flow before resuming drilling. For example, it was stated (in the February letter) that only 51 barrels flowed at WIPP-12 before shut in by a blow-out preventer.

The February 7, 1990 DOE letter went on to say that at WIPP-12 an additional 49,224 barrels flowed during deepening, geophysical logging, and further deepening before it was finally shut in for subsequent hydrologic testing. This additional flow was described as resulting from a "conscious decision."

It appears that virtually every time a pressurized Castile brine reservoir has been encountered in the vicinity of WIPP that "conscious decisions" have been made to allow varying amounts of brine to flow to the surface. Table II, extracted from tow WIPP reports (TME-3080 and TME-3153), describes remedial measures taken. Although the available data are not as detailed or as quantitative as one would like, it is clear that drilling practice through 1982 included release of brine at the surface whenever pressurized Castile brine reservoirs were encountered. There has been considerable drilling activity about the WIPP Site in the last few years and brine has been reported in seven wells. In two of these wells brine was reported to have flowed for three hours before stopping and for at least 12 hours. Records did not indicate how long the remaining wells flowed. It appears that, at least in some cases, commercial drillers still allow significant amounts of brine to flow to the surface and we believe scenarios should assume that any intruding driller will do likewise. Also, minor flows may not always be recorded in drilling logs, or perhaps even recognized.

Name of Well	Date Drilled	Initial Flow bbl/day	Remedial Action
Mascho-1	1937	8,000	No action to stop flow.
Mascho-2	1938	3,000	No action to stop flow.
Culbertson-1	1945		3,000 barrels estimated to flow to surface. No record of flow rate or duration.
Tidewater	1962	NA	12 pound/gallon drilling mud did not stop. Finally controlled [sic] by casing and cementing.
Shell	1964	20,000	Allowed to flow until artesian flow ceased.
Belco	1974	12,000	Brine flowed to surface for 26 hours with 14 pound/gallon drilling mud.
Gulf	1975	5,000	No records on total volume or duration of artesian flow.
ERDA-6	1975	660	WIPP hole. Estimate 19,0000 barrels could be produced by artesian flow.
Pogo	1979	1,440	Initial flow was after 14.6 pound per gallon drilling mud had been added. Stopped after 4 days with 15 pound per gallon mud.
WIPP-12	1981	12,000	WIPP borehole. Over 79,000 barrels produced. Estimate 350,000 barrels producible by artesian flow.

Table II References

U.S. Department of Energy, 1981a. *Brine Pocket Occurrences in the Castile Formation, southeastern* [sic] *New Mexico*, TME-3080.

U.S. Department of Energy, 1983. *Brine Reservoirs in the Castile Formation, Southeastern New Mexico*, TME-3153.

R.H. Neill *et al.*, 1983. *Evaluation of the Suitability of the WIPP Site.* EEG-23.

M

Brine released at the surface from the E2 scenario would be expected to increase the effective radius of the borehole and thus increase the amount of waste brought to the surface in suspension and in solution. The major effect could occur in the E1E2 scenario because brine present in the repository from the first encounter (which would be expected to be saturated in actinides) would be discharged at the surface.

In a November 3, 1992 response to EEG's concern about contaminated brine flow to the surface, SNL said: "We will repeat these subsidiary simulations using BRAGFLO for both release during drilling and long-term releases through abandoned boreholes. As you suggested at our previous meeting, there are four cases: (1) E1 or E2 during drilling, (2) E1 while Castile brine is allowed to flow, (3) E1 followed by E2 after Castile brine has been allowed to flow into the panel and then is available to flow through E2 during drilling, and (4) E1E2 after both have been abandoned.

EEG, in a November 9, 1992 letter to SNL, agreed these 4 cases were the appropriate ones to consider and urged SNL to perform the analysis posthaste.

Recommendation:

"EEG looks forward to seeing the results of the above mentioned analysis, and to include contaminated brine flow to the ground surface in future versions of human intrusion scenarios."

Response

These scenarios will be considered for inclusion in the SPM.

Comment MI-11d

M

Page 21

"A brine-slurry release scenario should be analyzed. A brine slurry might result from creep closure and gas generation. Such a brine slurry could be under greater than hydrostatic pressure and thus have a force capable of driving some or all of the slurry to the ground surface. The potential quantities of ejected brine might be less than that from the E1 scenario but the consequences could still be significant. The possible implications of a brine-slurry filled room were first raised by SNL in 1987 and were also evaluated in 1988 by EEG (Chaturvedi, Channell and Chapman, 1988)."

"SNL has responded that all evidence indicates that the possibility of a brine slurry existing in a waste storage room is essentially zero, and can be ignored

(SAND91-0893, Volume 1, Appendix B). Lappin et al., (1989) and the FSEIS (U.S. DOE, 1990b) are cited as support for this conclusion."

"The brine-slurry release scenario is related to undisturbed performance and cuttings release. Actually a similar, though probably less serious, release is considered in undisturbed performance when the waste storage room became partially or fully saturated only in the lower portion of the room. An effect of partial saturation and incomplete consolidation of the waste could be to lower the shear strength and result in greater quantities of waste being brought to the surface than calculated with the current cuttings model (E2) assumptions."

Recommendation:

"The EEG recommends a complete analysis of the brine-slurry release scenario. In addition, variants of the brine-slurry scenario in undisturbed performance and E2 need to be better understood."

Response

The Project believes a brine-slurry release in any context is unlikely. As stated in the comment several studies have been performed within the last several years on the possibility of brine-slurry in salt backfill in the underground environment. Results of the studies indicated brine-slurry in salt backfill would not impact performance because room closure rates (even considering likely gas generation rates) will decrease the available volume and recompact the salt to a point that even if brine fills the residual porosity a slurry cannot develop.

With respect to slurries forming from decomposing waste, itself, we believe closure rates and compaction would result in an end product with sufficient shear strength that a mobile slurry would not form. For further information, see Disposal Room and Cuttings Model Position Paper for Systems Prioritization and Technical Baseline, Section 4.2.4.5.

A model for representing incomplete consolidation of the waste in various degrees of decomposition has been developed, but it is not yet part of cuttings release calculations. Attention has been focused on pressurized gas. To date, the spall issue has usually been cast in terms of inflow of a fluid into an intrusion well, and the material that is carried along in this process. Variable strength materials will eventually be incorporated into the cuttings release calculations to encompass concerns that may exist about whether appropriate strength materials have been modeled.

Comment MI-11e

Page 22

"The USEPA Standard calls for human intrusion analysis that would create

> ... a ground water flow path with a permeability typical of a borehole filled by the soil or gravel that would normally settle into an open borehole [sic] over time—not the permeability of a carefully sealed borehole."

"In the 1991 and 1992 *Performance Assessments* the permeability of human intrusion boreholes was sampled lognormally between 10^{-11} m^2 and 10^{-14} m^2. This value was obtained from Table 2.2 of Freeze and Cherry (1979) for silty sand. The choice of silty sand is SNL's interpretation of USEPA guidance on borehole sealing cited above."

"EEG has several problems with the SNL interpretation. Table 2.2 in Freeze and Cherry (1979) shows a permeability range for silty sand from about 8×10^{-11} m^2 to 8×10^{-15} m^2. The same table also shows ranges of 10^{-9} m^2 to 2×10^{-13} m^2 for clean sand and 10^{-7} m^2 to 10^{-10} m^2 for gravel. It appears that a strict following of the EPA Guidance would require use of higher permeabilities, to include gravel in the borehole."

"EEG believes that the assumption of borehole permeability described in the EPA Standard is reasonable when considered along with the other assumptions in the guidance, but is not conservative in light of observed borehole sealing practices in the Delaware Basin. In 1989 the Bureau of Land Management found 6,527 shut-in and temporarily abandoned wells in New Mexico (USBLM, 1989). A temporarily abandoned well is simply abandoned, without proper plugging and sealing. The BLM made the following statement about wells in the Carlsbad area:

> At Carlsbad, we reviewed the status of 2 shut-in and 11 temporarily abandoned wells on a 15-well lease. These wells had been classified as shut-in or temporarily abandoned since the late 1960s without approval. There was no evidence these wells had been properly tested to ensure they were capable of producing oil or gas and properly classified. The operator of this lease stated that he did not perform well integrity tests because he estimated that it would cost about $2,000 per well. Additionally, he stated that he did not permanently plug wells because that would cost about $10,000 per well (USBLM, 1989)."

Recommendation

"Performance Assessment assumptions should consider conditions as they exist, not at the state-of-the-art or in full compliance with regulatory standards that are not being met now and may not be met in the future. For the human intrusion borehole, the range of degraded permeabilities should span sand and gravel."

Response

The DOE does not dispute the principal point raised here by the EEG, which is that boreholes are sometimes abandoned without proper plugging.

The DOE has interpreted the guidance quoted by the EEG to allow for consideration of the make-up of the formations penetrated by the borehole. The guidance indicates that the fill material should be that which would "normally settle into an open borehole over time." Detailed examination of the geologic column in the Air Intake Shaft (Holt and Powers, 1990, DOE/WIPP-90-051) revealed no gravel beds between the repository and the ground surface. Sandstones occur in the Gatuña Formation, the Dockum Group, and the Dewey Lake Red Beds, but they are generally composed of fine sands and silts, and are interbedded with siltstones. The cleanest sand that might enter the borehole is that which is present at the ground surface, and it is simply not credible to imagine a circumstance in which the entire length of an open borehole from the repository to the surface is filled with nothing but clean sand. As a further consideration on this point, open holes are not empty when abandoned: at the least, they contain the remains of the fluid used to drill the hole. For holes drilled below the salt section in the Delaware Basin, this fluid is a "mud" containing clays and other additives. These materials should also be included in estimation of the final permeability of an open hole. The DOE believes that silty sand is an appropriate approximation of the fill material in an unplugged borehole. For boreholes which have been plugged, the DOE believes that silty sand is an appropriate approximation for fill material in the entire hole, including both the degraded concrete plugs and intervening open sections.

Second, the DOE is aware of the 1989 BLM Office of Inspector General Audit Report, and agrees that it provides evidence that borehole plugging procedures are not always followed. The EEG's implication that the quoted paragraph has direct relevance to the WIPP, however, is inappropriate given the guidance in Appendix C of 40 CFR 191 that "inadvertent and intermittent intrusion by exploratory drilling...can be the most severe intrusion scenario assumed..." and that "the drillers [will] soon detect, or be warned of, the incompatibility of the area with their activities." The quote from the BLM report probably refers to production wells, rather than exploratory wells. Fifteen wells on a single lease are not examples of "intermittent" drilling. The BLM report does not distinguish between exploratory wells and production wells in its total number of improperly plugged wells in New

Mexico. All anecdotal examples described in the report appear to be production wells rather than exploratory wells, and were presumably abandoned many years after they were drilled.

Reference

Holt, R.R. and Powers, D.W 1990. *"Geologic Mapping of the Air Intake Shaft at the Waste Isolation Pilot Plant"*. Report Number DOE/WIPP 90-051.

DETAILED COMMENTS

Comment DC-01

Page 23

Area in document Volume 1, Section 3.1, Page 3-3

This section says that EPA expects the implementing Agency to use the same assumptions. But it does not say whether DOE does or does not.

Response

Appendix B defines the assumptions that may be used in determining compliance with Subpart B. The DOE recognizes these assumptions and has used them in the 1992 PA calculations. These assumptions may change with the release of 40 CFR 194 and as such will be incorporated in future PA calculations where appropriate.

Comment DC-02

Page 23

Area in document **Volume 1, Table 3-1, Page 3-14, Line 15**

"In this Table, techniques are given for assessing and reducing various kinds of uncertainties. For conceptual model uncertainty, an additional method for assessing its extent is to analyze alternate conceptual models. If alternate conceptual models can be rejected with confidence, then the favorite conceptual model has a better chance!"

Response

The DOE agrees with the comment. The importance of alternative conceptual models was discussed on the previous page of Volume 1 of the 1992 PA. Analysis of alternative conceptual models will be part of the SPM.

Comment DC-03

Page 23

Area in document Volume 1, Table 3-1, Page 3-14

"In Parameter Values and Variability, the use of expert judgment is said to be a method of assessing and reducing uncertainty. The fact is that the panel on solubilities greatly expanded the uncertainty range."

Response

The EEG's comment is correct. However, the heading on the column in question in the table is "Technique for Assessing or Reducing Uncertainty" (emphasis added). There was no intent to imply that expert panels should be expected to reduce uncertainty.

Comment DC-04

Page 23

Area in document Volume 1, Section 3.3.4, Page 3-17, Line 17

"The marker panel has not issued a report."

Response

The report has been published as:

Trauth, K. M., S. C. Hora, and R. V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment DC-05

Page 23

Area in document Volume 1, Section 4.1.1, Page 4-2, Line 28.

"The description of undisturbed performance should include a statement that the deleterious effects of gas fracturing have not been considered. The probability of gas fracturing is clearly above 10^{-4} in 10,000 years. Sandia's own experimental data suggest that without fracturing, the gas pressure is likely to reach and exceed lithostatic in hundreds of years."

Response

The DOE agrees that consequence analysis of undisturbed performance should include pressure-dependent effects on the permeability of anhydrite interbeds. The text referred to in the comment does not discuss consequence modeling. Rather, it defines undisturbed performance independently of modeling capabilities, to include "all expected changes in the system and associated uncertainties for the 10,000 years of concern...." "Fracturing" is included in the base case definition.

A statement relevant to the comment occurs in Section 4.3.1 of Volume 1 (page 4-12, lines 22-25): "The model used to represent the response of the repository and the surrounding strata to the generation of gas by waste degradation does not include effects of possible pressure-dependent fracturing of anhydrite layers within the Salado Formation."

Comment DC-06

Page 24

Area in document Volume 1, Section 4.2, Page 4-8, Line 38

"Why is the maximum number of holes in the 70 simulations only 20 per km^2 when Latin Hypercube is used to sample uniformly (I presume) over interval [0,30]? Isn't the key advantage of Latin Hypercube to "ensure full coverage of the range of each sampled variable.: (p. 4-14, line 10)?"

Response

The indicated statement in the 1992 PA documentation is incorrect. The rate constant for drilling intrusions was sampled uniformly on the interval $[0, 3.78 \times 10^{-4}\ yr^{-1}]$, which is equivalent to sampling on the interval $[0, 30/km^2/10^4\ yr]$. Specifically, the area of the waste panels in the 1992 PA was 0.126 km^2, which resulted in a maximum value for λ of

$$\lambda_{max} = (30/km^2/10^4\ yr)(0.126\ km^2) = 3.78 \times 10^{-4}\ yr^{-1}.$$

Confusion here arises from the erroneous statement in the 1992 PA that the number of intrusions are based on a waste area of 0.5 km^2. Note that 10 intrusions into 0.126 km^2 corresponds to 79.4 intrusions per 1 km^2.

N

Comment DC-07

Page 24

Area in document Volume 1, Section 5.1.2.1.1, Page 5-3, Line 27

"Is λ really random in both space and time? As implemented it appears to be only a variable of time."

Response

The rate constant λ is variable in neither time nor space for the "constant λ" cases. It varies nonrandomly with time (i.e., based on expert panel judgment about possible effectiveness of markers) and is fixed in space for the "time-dependent λ" cases in the 1992 PA.

The assumption of the Poisson process does involve the assumption that drilling is random in both space and time. Intrusions are assumed to be equally likely to occur anywhere within the region of interest, regardless of spatial location.

Consequence modeling (as opposed to probability modeling) for the 1991 and 1992 PAs assumed that all intrusions were located at the center of the waste disposal area for computational convenience. This assumption was made independently of the Poisson model used to determine intrusion probabilities.

M

Comment DC-08

Page 24

Area in document Volume 1, Section 5.1.2.1.2, Page 5-4, Line 35

"It is not clear why the intrusion and subsurface release times are specified rather than random. If intrusion and release times are random, the source strength can be calculated in PANEL using eq. 1.4.4-11 in Vol. 3. Are these 6 times of intrusion possible times of intrusion, or must the intrusions occur?"

Response

Documentation resolving this is available in Chapter 3 of Volume 1 of the 1991 PA (SAND91-0893/1) and in Chapter 3 of Volume 2 of the 1992 PA.

The 1992 PA used an importance sampling procedure based on the subdivision of the 10,000 year time period specified in 40 CFR § 191.13(a) into six subintervals. Scenarios were then defined by taking combinations of drilling intrusions in these time intervals. The randomness of the intrusion times is incorporated into the probabilities of these scenarios. For computational efficiency, releases were calculated only for drilling intrusions occurring at the midpoints of the six time intervals. Thus, the six times of intrusion serve as a representative time of intrusion for each subinterval and not an exact time for a particular intrusion. Although not used in the 1992 WIPP PA, Monte Carlo procedures that involve sampling the number, time, and location of drilling intrusions in the construction of CCDFs exist. However, these procedures would still require the mechanistic calculation of releases at a relatively small number of intrusion times, with these releases then being used in conjunction with appropriate interpolation techniques to estimate the releases associated with the randomly selected intrusion times.

A detailed description of the CCDF construction procedure used in the 1992 WIPP PA is given by Helton and Iuzzolino (1993). A description of a Monte Carlo procedure for the construction of CCDFs is currently being developed.

Reference

Helton, J. C., and J. J. Iuzzolino. 1993. "Construction of Complementary Cumulative Distribution Functions for Comparison with the EPA Release Limits for Radioactive Waste Disposal." *Reliability Engineering and System Safety* **40**: 277-293.

Comment DC-09

Page 24

Area in document Volume 1, Section 5.1.2.2, Page 5-6, Line 25

"Given our comment on the subjective elicitation process in Major Issues, we do not consider any of the results using λ_t to be valid."

Response

The probability of human intrusion will be reevaluated if and when additional guidance is available from the EPA.

Comment DC-10

Page 24

Area in document Volume 1, Section 5.1.2.2, Page 5-6, Line 27

"When releases are calculated for six intrusions, is it six holes? Does this correspond to S(4,1,0,1,0,0) in Table 3-2 of SAND91-083/1?"

Response

Releases are calculated for six times of intrusion, rather than for six intrusions. Up to 10 intrusions occur at these six times in the 1992 PA. Tables 2.5-3 and 2.5-4 in Volume 4 of the 1992 PA provide a listing of the computational scenarios considered (i.e., the combinations of intrusions in each time interval), together with the probabilities of these computational scenarios calculated using a selected time-dependent Poisson rate constant (Table 2.5-3) and the maximum value for the time-invariant rate constant (Table 2.5-4).

Comment DC-11

Page 24

Area in document Volume 2, Section 1.3.2, Page 1-4, Line 26

"CAMCON controls 75 codes for WIPP Performance Assessment. However, the key codes BRAGFLO, SECOTP2D, and CUTTINGS are run outside of CAMCON, and also probably SANTOS-SANCHO. Does this make CAMCON a general without troops?"

Response

BRAGFLO, SECOTP2D, and CUTTINGS interface to CAMCON through the Computational Data Base (CDB). This is accomplished through the use of pre- and post-processors which extract information or write results to the CDB. The CDB is common and available to all models used in PA calculations whether the models physically reside within CAMCON or not.

Once codes such as BRAGFLO and CUTTINGS become stabilized (from a conceptual point of view), they will be included within CAMCON. Until then, they will remain interfaced to CAMCON so the QA history (information trail) of each calculations is traceable. As long as traceability is maintained, it is not clear how compliance may be affected.

Comment DC-12

Page 24

Area in document Volume 2, Section 2.3.2.1, Page 2-47, Line 2

"The word should probably be 'pyrophoric'."

Response

The DOE agrees with the comment.

Comment DC-13

Page 24

Area in document Volume 2, Section 2.3.2.1, Page 2-47, Line 2

"The second half of this sentence does not make sense. I doubt if the limit is 1% of the weight of an empty container."

Response

The text referred to is incorrect. The correct wording is "1% of the weight of the waste package."

Comment DC-14

Page 24

Area in document Volume 2, Section 2.3.4.1, Page 2-54, Line 17

"The fractile method was used in eliciting estimates of actinide solubility. Seaver, von Winterfeldt and Edwards (1978) fo: : experimentally that other procedures were superior. The use of the fractile m. d needs to be justified."

Response

The experimental work reported in Seaver, von Winterfeldt, and Edwards (1978) has been found to be flawed with respect to the findings concerning the fractile method of elicitation. In the referenced study, the bounds (0th and 100th quantiles) were provided to the subjects under the direct assessment regime but not under the fractile assessment regime. This difference led to the apparent differences in calibration. The more recent work of Hora, Hora, and Dodd (1992) shows virtually no differences in calibration between direct and fractile assessments among scientists and engineers who participated in the NUREG-1150 study of nuclear power plant safety. Hora, Hora, and Dodd point up the mistake of Seaver, von Winterfeldt, and Edwards on page 149 of the reprint.

Reference

Hora, S.C., J.A. Hora, and N.G. Dodd. 1992. "Assessment of Probability Distributions for Continuous Random Variables: A Comparison of the Bisection and Fixed Value Methods," *Organizational Behavior and Human Decision Processes*. Vol. 51, 133-155.

Comment DC-15

Page 25

Area in document Volume 2, Section 2.3.4.1, Page 2-54, Line 20

"To use ionic-strength corrected data from Well J-13 from Yucca Mountain as the medium needs justification. There is increasing doubt at the Yucca Mountain Project that J-13 water is representative of anything."

Response

This was the only relevant data we had at the time of the 1992 PA. Research is underway to provide WIPP-specific data.

Comment DC-16

Page 25

Area in document Volume 2, Section 2.3.4.2, Page 2-55, Line 4

"The laboratory measurements of plutonium solubilities and sorption coefficients in brines fall short for several reasons:

- other actinides need to be measured;
- solubilities and sorption coefficients in Culebra water are needed;
- for the spectrum of possible conditions, calculations are better."

Response

In general, the SPM will help resolve this issue by:

- producing the project technical baseline document which will include "project defensible baseline data and assumptions".
- EEG will be asked to comment on the position paper used to define the baseline, which may or may not include the measurements referred to.

The current actinide source term program contains lab research and model development activities that address this issue.

Comment DC-17

Page 25

Area in document Volume 2, Section 2.3.4.2, Page 2-55, Line 6

"It is not clear how the results of the Source Term Testing Program will be useful or used in performance assessment. The current performance assessment uses the actinide solubility. The LANL experiments give a release rate, rather than a solubility. The LANL release rate will be proportional to inventory. The performance assessment department should state how it intends to use the two different sets of data."

Response

The current project technical baseline does not rely on solubility limits. Several sets of possible actinide solubility limits will be considered with the SPM methodology sets will include results from source term model development and data collection (LANL) to test the model.

The compliance PA will use input from the actinide source term model (under development) that will include solubility and colloid species, if stable. The LANL tests will provide confirmatory solubility and colloid data from actual waste materials.

Comment DC-18

Page 25

Area in document Volume 2, Section 2.3.5, Page 2-55, Line 13.

"The statement is made that at decommissioning, free brine will not be present within the emplacement area. Experience over the history of WIPP indicates that brine may be present throughout the Test Phase and Disposal Phase."

Response

The statement 'free brine will not be present within the emplacement area' should be replaced with 'some brine will be initially present within the emplacement area.' The PA conceptual model of the repository treats the amount of this initial brine or water content as an imprecisely known parameter and it is sampled as such.

Comment DC-19

M

Page 25

Area in document Volume 2, Section 4.2.3.1, Page 4-11, Line 11

"Some 'waste containers' will be composed of organic material? Do such containers meet the Waste Acceptance Criteria?

Response

The text in question is incorrect. All waste shipped to and emplaced in the WIPP must meet the Waste Acceptance Criteria (WAC). Section 3.2.1.1 of the WAC states "Waste containers for emplacement in the WIPP shall be non-combustible and meet all applicable requirements of 49 CFR §173.412 for Type A packaging." Currently, no organic containers exist which meet the WAC. In addition, the TRUPACT-II Certificate of Compliance section 5(b)(1) states that "Wastes must be packaged in 55-gallon drums, standard waste boxes (SWB), 55 gallon drums within standard waste boxes, or bins with standard waste boxes," all of which are of steel construction. The 55 gallon drums may indeed contain an organic poly liner, but currently the liner must be used in conjunction with the steel drum (the poly liner does not meet Type A requirements).

Comment DC-20

Page 25

Area in document Volume 2, Section 4.2.3.2, Page 4-13, Line 33

"All borehole plugs... degrade into material with properties similar to those of silty sand. Why not then plug above the Culebra?"

Response

The question is partially answered 4 lines later in the same paragraph: "A single plug above the Culebra is assumed to remain intact for Scenario E1, diverting all upward flow into the Culebra and maximizing radionuclide transport into that unit and toward the subsurface boundary of the accessible environment." (Volume 2, page 4-15, lines 6-8). Restricting flow and transport to a single unit yields an upper bound for subsurface radionuclide release, assuming that unit provides the most rapid transport path.

The question is also addressed in Volume 1 (page 4-6, lines 35-39): "In all three of these intrusion scenarios, borehole plugs are assumed to be emplaced and to perform so as to maximize fluid flow into the Culebra Dolomite Member of the Rustler Formation. These plug configurations have been chosen to facilitate examination of the specific scenarios, and do not reflect the most realistic conditions expected."

Future analyses conducted as part of the SPM will evaluate conditions in which the entire length of the borehole is assumed to have the properties of silty sand (representing degraded plugs and material which might settle into an open hole). Flow will be allowed to the ground surface and into stratigraphic units other than the Culebra.

Comment DC-21

Page 25

Area in document Volume 2, Section 5.2, Page 5-2, Line 22

"Prof. Helton's method of calculating intrusion probabilities is not trivial. The full explanation is worthy of a journal paper. The brief explanation here raises more questions than answers. As a matter of fact, this summary is incomprehensible and confusing."

Response

The material has been published as a journal article. See Helton (1993).

Reference

Helton, J. C. 1993. "Drilling Intrusion Probabilities for Use in Performance Assessment for Radioactive Waste Disposal." *Reliability Engineering and System Safety 40: 259-275.*

Comment DC-22

Page 25

Area in document Volume 2, Section 5.2, Page 5-4, Line 16

"Prof. Hora's algorithm gives drilling rates in units of holes/mi^2/10000 years, not holes/km^2/10,000 years."

Response

The Futures Panel provided their judgments in terms of mi^2. Subsequent calculations using the judgments were carried out in terms of the units provided. Conversion to km^2 was performed when the output was to be used in the performance assessment calculations.

Comment DC-23

Page 25

Area in document Volume 2, Section 5.3.2, Page 5-7, Line 22

The prior intrusion probability distribution was actually obtained from only the Boston Team, one of four Futures panels [sic], consisting of one lawyer, two social scientists and one physicist. Note that no one from a mineral-related field is on the panel.

Response

The statement that the prior intrusion probability distribution was obtained from only the Boston Team is factually in error. The paragraph referred to deals exclusively with the findings of the Boston team and not with the findings of all four teams. The paragraph from which the sentence was extracted reads:

The Boston team provided assessments for the drilling intensity that are conditional on both time and level of technology. The responses for exploratory drilling for hydrocarbons are shown in the following tables. Exploratory drilling for hydrocarbons was not thought to extend further than 300 years in the future.

This paragraph is followed immediately by a table labeled "BOSTON TEAM--DRILLING INTENSITY DISTRIBUTIONS.

Particularly in light of the following discussions of the findings of the three other teams, the assembly and analysis of results, and the listing in Volume 1 (pp. 4-8 and 5-4) of the six times of intrusion (100, 175, 350, 1000, 3,000, and 7250 years after decommissioning), we do not understand the confusion on this point.

The material that was presented to the Futures Panel, as well as the written material that they received, contained information about resource potential at the site. The reasoning behind the make-up of each team within the Futures Panel was to encourage a broader consideration of possible future societies rather than relying on current exploratory drilling rates.

M

Comment DC-24

Page 25

Area in document Volume 2, Section 7.1, Page 7-1, Line 2

"For the Bateman equations why not reference Bateman himself?"

Response

The Bateman equations will be referenced to the original source in future documents.

Comment DC-25

Page 26

Area in document Volume 2, Section 7.2.3, Page 7-1, Line 2

"Do you mean biodegradation of organic materials only?"

Response

Yes, the 92 PA assumed that gas was produced from 100% of the cellulosics, 50% of the rubber and none of the plastic waste.

Comment DC-26

Page 26

Area in document Volume 2, Section 7.6, Page 7-13, Line 3

"The use of the word compounds here is erroneous. A better word is fluid."

Response

The phrase in question is "may result in mobilization of dissolved, radionuclide-bearing compounds from waste...." The DOE agrees that "compounds" is the wrong word. "Dissolved" is also misleading here, because liquids may also transport radionuclides in suspended forms. Better wording would be "may result in mobilization of radionuclide-bearing fluids...."

Comment DC-27

Page 26

Area in document Volume 2, Section 7.6.1.2, Page 7-16, Line 26

"How is the scaling factor chosen? Who decides that it is reasonable? The same questions apply to the choice of $A_{R,\theta}$, and ϕ in (7-14). Where are the results of climate change shown?"

Response

The SPM will assess the system sensitivity to climate change. Documentation of the application of climate change in the 1992 PA is complete, and references are provided below.

The scaling factor g is chosen to ensure that for the maximum value of A_R heads will be approximately at the elevation of the spill point of Clayton Basin, in the hypothesized recharge area consistent with the confined-aquifer conceptual model used in the PA. The decision to limit future heads to this elevation was made by WIPP PA analysts, and is discussed in Section 6.4.1 of Volume 4 of the 1992 PA. Values for other terms in Eq. 7-14 are discussed in a memorandum by Swift in Appendix A of Volume 3 of the 1991 PA (SAND91-0893/3), and are also discussed in Section 6.4.1 of Volume 4 of the 1992 PA. Note that Eq. 7-14 is not intended to predict future climate variability: rather, it is designed to permit examining system sensitivity to uncertainty in climatic change. If system-level sensitivity warrants, different values for all climatic parameters can be considered.

Results of climate-change modeling in the 1992 PA are presented in Section 6.5 of Volume 4 of the 1992 PA. They are also reported by Swift et al. (1994).

Additional analyses of climate change are included in ongoing three-dimensional regional groundwater flow modeling.

Reference

Swift, P. N., B. L. Baker, K. Economy, J. W. Garner, J. C. Helton, and D. K. Rudeen. 1994. *Incorporating Long-Term Climate Change in Performance Assessment for the Waste Isolation Pilot Plant*. SAND93-2266. Albuquerque, NM: Sandia National Laboratories.

Comment DC-28

Page 26

Area in document Volume 2, Section 7.6.2, Page 7-18, Line 26

"The numerical model for solute transport is 2-dimensional. The conceptual model shown in Figure 7-4 is 3-dimensional."

Response

We agree that the statement on lines 5 and 6 of page 7-18 incorrectly implies that the conceptual model is two-dimensional. The correct point is that a two-dimensional numerical model is used to represent a three-dimensional conceptual model. The vertical (z) direction is introduced into the transport model by incorporating molecular diffusion of solutes in the vertical direction. (See equations 1.4.6-1 and 1.4.6-9 of Section 1.4.6 of Volume 3 of the 92 PA).

Comment DC-29

Page 26

Area in document Volume 2, Section 7.6.2, Page7-22, Line 13

"In a fracture, clay particles are unlikely to be regularly packed as in a crystalline lattice. There are ways to analyze random packing but we fail to see why it is necessary. The result in (7-22) is different from (7-21) by a factor of *a*/3. Because there is little justification for (7-22), why not just use (7-21)."

Response

We disagree that there is little justification for (7-22); the equation 7-22 approximates the retardation potential of clay particles packed in a fracture. Equation 7-21 is for the retardation from an unfilled fracture.

Comment DC-30

Page 26

Area in document Volume 2, Section 7.1, Page7-1, Line 7

"N_i here is the number of atoms of radionuclide I, not the activity. The activity of I is N_i."

Response

Correction accepted.

Comment DC-31
Page 26

Area in document Volume 3, Section 1.2, Page 1-8, Line 9

"In the upper right plot in Figure 1.2-1, why is the median/mean of a normal distribution 0.500001?"

Response

This is a typographical error; mean and median are the same for a normal distribution and both should be 0.5.

Comment DC-32

Page 26

Area in document Volume 3, Section 1.4, Page 1-24, Line 43

"In equation (1.4.1-9b) and eq. (1.4.1-11), the big dot used here for multiplication is confusing, and it is not needed. The dot is used on the previous two pages only for the dot product."

Response

Comment accepted.

Comment DC-33

Page 26

Area in document Volume 3, Section 1.4.4, Page 1-34

"....present equation (1.4.4-10) and explain that Cdi is treated as a known constant."

Response

Comment accepted; but "two page" development shows generality of PANEL (i.e., what it is capable of doing, not just what was done in 1992).

Comment DC-34

Page 26

Area in document Volume 3, Section 1.4.5, Page 1-38, Line 11

"Same comment on the big dot."

Response

The "big dot" in this case does mean dot product since the quantity K is a tensor.

Comment DC-35

Page 26

Area in document Volume 3, Section 2.3.3, Page 2-4

"The category of data source of engineering lore is used here in and in other places. Engineering lore is not defined on p. 1-13. In this case, the source is a refereed journal paper, which may well be non-WIPP Literature Data."

Response

Another editorial error: the parenthetical phrase "engineering lore" should have been deleted in the final draft of this report.

Comment DC-36

Page 26

Area in document Volume 3, Section 2.6, Page2-78, Line 14

The equation here does not make sense, and the definition of probability is not proper. For x as a random variate, try

$$P[b_r/b] = \{ 0, (0<x<0.5); \ x\text{-}0.5, (0.5<x\leq 1.0)"$$

Response

We disagree. This formula correctly represents the CDF if the unit step function is defined as $U(y) = 0$ if $y<0$; $U(y) = 1$ if $y \geq 0$.

Comment DC-37

Page 27

Area in document Volume 3, Section 2.6, Page 2-83

"Why is the median given here not equal to the median given on the previous page, line 13?"

Response

This is a typographical error; the median quoted in line 13 of pg. 2-82 should be 0.139, not 0.145.

Comment DC-38

Page 27

Area in document Volume 3, Section 2.6, Page 2-93

"This is a curious table. The range of partition coefficients extends to a region of no significance. One can calculate the lowest value of K_d which will give a positive coefficient using the *[given]* equation and data . Examination of this table says none of the nuclide's median partition coefficient will give a positive retardation. Why bother? Just forget retardation."

Response

The conclusions drawn in the comment are incorrect. The equation presented in the Comment is in error. The proper equation is Equation 1.4.6-8 on p. 1-42 (Vol. 3) of the 92 PA. This equation reduces to:

$$R = 1 + (0.48)K_d$$

Any value of K_d greater than zero will yield retardation greater than one. All of the ranges and medians given in the table produce positive retardation.

Comment DC-39

Page 27

Area in document Volume 3, Section 3.3, Page 3-22, Table 3.1

"A more correct term for 'activity conversion' is specific activity."

Response

The suggested editorial change will be made in future documents.

Comment DC-40

Page 27

Area in document Volume 3, Section 3.3, Page 3-24

"The *Table of Isotopes* gives the half life of Pu-239 as 7.61×10^{11} s."

Response

The *Chart of the Nuclides* gives the half life of Pu-239 as 2.41×10^{4} years, which converts to 7.60×10^{11} s. The most recent value for this parameter will be used in future documents.

Comment DC-41

Page 27

Area in document Volume 3, Section 3.4.2, Page 3-67

"Please identify the source given an 'input to the 1990 IDB'."

Response

The generator sites provide input data to Oak Ridge which were used in their compilation of the Integrated Data Base. Those input data are not independently published.

Comment DC-42

Page 27

Area in document Volume 3, Section 4.2, Page 4-6, Line 7

"Certainly this refers to a regular borehole. However, Figure 4.2-2 refers to changes in permeability as a function of 'time after intrusion. This legend cannot be correct. Should it be time after sealing?"

Response

On time scales of thousands of years, the difference between the "time of intrusion" and the "time of borehole sealing" is negligible.

Comment DC-43

Page 27

Area in document Volume 3, Section 4.2, Page 4-6, Line 11

"Surely the concrete plugs do not have initially the permeability and porosity of silty sand. On p. 3-14, the permeability of concrete is given as $2.7\text{x}10^{-19}$ m^2, where the permeability of silty sand has a median value of $3.16\text{x}10^{-12} m^2$."

Response

The PA assumptions are shown in Figure 4.2-2; in that figure, borehole permeability increases from initial values from approximately 10^{-19} to 10^{-15} m^2 to final values of approximately 10^{-12} m^2 at 150 years.

Comment DC-44

Page 27

Area in document Volume 3, Section 4.2.1, Page 4-4, Line 38

"Reference is made that the New Mexico Energy, Minerals, and Natural Resources Department, Oil Conservation Commission is the state agency responsible for negotiating plug and abandonment specifications and conducting inspections. The Oil Conservation Commission has not performed this function since 1978. On March 31, 1978, Division Order No. R-5709 established the Oil Conservation Commission remaining in name as an appellate board. Many people in the industry still refer to OCD as OCC, but that is not technically correct."

Response

Comment acknowledged.

Comment C1

"Sensitivity and uncertainty analysis identified that solubilities and retardation coefficiencies are the most important variables amendable to additional site or lab work.

The EEG objects to the use of subjective estimates of solubilities and retardation coefficients.

Current "expert" estimated solubilities span some 10^{12}M.

The sensitivity to solubilities is actually sensitivity to the source term.

The roles of the LANL Source Term Testing program and measurements of thermodynamic solubilities in PA are not clear."

Response

The current actinide source term program is developing experimental solubility and colloid data that will be related to PA through the actinide source term model. The LANL tests will be used to build confidence in the predictive capability of the model.

Comment C2

"Analysis of undisturbed repository performance shows significant potential for gas fracturing of anhydrite layers.

Even without fracturing, with anhydrite permeability near mean values and the availability of water, there will be gas flow to the disposal unit boundary.

Current analysis show that seals are not effective gas movement barriers.

There is a need for true multi-phase transport modeling, not currently done (SAND92-0700, V, p. 4-2)."

Response

Multi-phase transport modeling of VOCs and radionuclides in the Salado formation are being studied which can interface with BRAGFLO, a multi-phase flow code. The model VAST simulates gas phase transport of VOC and the model NUTS simulates brine phase transport. This should give the DOE the ability to study multi-phase flow and transport processes. As discussed in EEG question MI-6 the deleterious effects of gas generation and possible fracturing are recognized. Also note experimental programs in the lab and in the field are acquiring data to better assess 2-0 flow.

Comment C3

"Sensitivity and uncertainty analysis identified that the following sampled variables are NOT in the top 5 in importance:

- Culebra transmissivity fields
- Corrensite in fractures

Do not waste money with additional studies."

Response

Importance of these topics will be examined in the SPM.

Comment

(Restatement): Documentation of computer codes is not available. Reviewers need documentation on discontinued as well as current codes to properly assess the WIPP.

Response

Documentation of all computer codes necessary for compliance is in progress and will be included in the compliance application package.

Complete documentation of all WIPP performance assessment models and codes necessary for compliance will be completed by the end of September 1995 or shortly thereafter.

Comment P1

(Paraphrase): We are much more concerned about the use of subjective probabilities in human intrusion analysis. While we agree that human judgment is the only method of estimating these probabilities, we question the art and science used in volumes 1, 2, and 3.

This quotation summarizes our view on "expert judgment";

"Expert judgments are not statements about nature but rather about beliefs. Nor are they statements which can be extrapolated to a larger population of events and beliefs. Therefore, while there is a chance that conclusions based on an expert judgment may be true about the world, it is not a good idea to say so because there is no justification in the method which allows this (Fleming, 1991)."

Response

The Use of Expert Judgment as It Relates to the Consideration of Inadvertent Human Intrusion and Passive Institutional Controls

Within the containment requirements in 40 CFR 191.13, the EPA specifies that performance assessments will consider "all significant events and processes that may affect the disposal system". Human intrusion is one such event that could potentially affect a disposal system:

"The most speculative potential disruptions of a mined geologic repository are those associated with inadvertent human intrusion."

40 CFR 191 also places the consideration of human intrusion in a probabilistic sense by specifying the probabilities with which cumulative releases may occur:

"...cumulative releases of radionuclides to the accessible environment for 10,000 years after disposal from all significant events and processes that may affect the disposal system shall: (1) Have a likelihood of less than one chance in 10 of exceeding the quantities calculated according to Table 1 (Appendix A); and (2) Have a likelihood of less than one chance in 1,000 of exceeding ten times the quantities calculated according to Table 1 (Appendix A)."

The nature of potential human intrusion is that it is not guaranteed to happen. Thus, it is possible to intervene to impact human actions. 40 CFR 191 addresses the uncertainty of actions and the possibility of intervention in the following manner:

"To provide the confidence needed for long-term compliance with the requirements of 191.13, disposal of spent fuel or high-level or transuranic wastes shall be conducted in accordance with the following provisions [the assurance requirements, 40 CFR 191.14] ...(c) Disposal sites shall be designated by the most permanent markers, records, and other passive institutional controls practicable to indicate the dangers of the waste and their location."

Guidance in Appendix B of 40 CFR 191 suggests that information about the consequences of actions constitutes an intervention and will cause a change in behavior:

> "Furthermore, the implementing agencies can assume that passive institutional controls or the intruders' own exploratory procedures are adequate for the intruders to soon detect, or be warned of, the incompatibility of the area with their activities."
>
> and;
>
> "The [EPA] assumes that, as long as such passive institutional controls endure and are understood, they: (1) can be effective in deterring systematic or persistent exploitation of these disposal sites; and (2) can reduce the likelihood of inadvertent, intermittent human intrusion to a degree to be determined by the implementing agency."

Given the assumption that intervention is possible and can change behavior, 40 CFR 191 established the appropriate approach to incorporating intervention efficacy issues (based on design recommendations) in the performance assessments:

> "Determining compliance with § 191.13 will also involve predicting the likelihood of events and processes that may disturb the disposal system. In making these various predictions, it will be appropriate for the

implementing agencies to make use of...prevalent expert judgment relevant to the numerical predictions."

Further guidance from the EPA in Appendix B indicates that the DOE, as the implementing agency:

> "...should consider the effects of each particular disposal system's site, design, and passive institutional controls in judging the likelihood and consequences of such inadvertent exploratory drilling."
>
> and;
>
> "In fact, sole reliance on these numerical predictions to determine compliance may not be appropriate; the implementing agencies may choose to supplement such predictions with qualitative judgments as well."

This text suggests a dual realization (1) that existing information or information that is being developed that is pertinent to performance assessments must be considered, and can be of use even for complex issues such as human intrusion and passive institutional controls, and (2) that the human mind is the best tool for assimilating information and producing judgments. Expert judgment is particularly suited to examining experimental or observational data collected under conditions or for purposes different from those of current interest, and for cases where the data and information are not available exactly in terms of traditional laboratory experiments or are from disparate sources. Such expert judgment does not create information, but is a synthesizing process to provide meaningful input for current requirements.

Absent guidance from the EPA as to a set future to assume, and relying on EPA guidance in Appendix B of 40 CFR 191 for calculations involving human intrusion, performance assessments for the WIPP have addressed this issue. In light of the importance of human intrusion in performance-assessment calculations and the inability to conduct experiments relating to future societies, the SNL WIPP Project has deemed it appropriate for the WIPP to consider the input of individuals (independent of the Project) whose experience and expertise are involved in examining human actions, history, trends, technology, etc. Without outside, independent judgments, assumptions made within the Project might have been called into question. This was done to ensure that judgments were objective with respect to compliance. While the expert judgment process was undertaken to address human intrusion in the most adequate means practicable, it was done so with the knowledge that any attempt to quantify the actions of possible future societies is incomplete and fraught with controversy.

The consideration of potential future societies is important not only because of the need to consider the frequency of inadvertent human intrusion in performance assessments, but also for the benefit of thinking broadly when considering how to attempt to communicate with humans over the 10,000 year period of regulatory concern. The consideration of a broad range of motivations for capabilities of inadvertent human intrusion can be instructive as a base for setting "with whom" one may be trying to communicate. This was, in fact, one of the motivations for convening an expert panel to address future human intrusion. This broad thinking on human intrusion became input for a second expert panel charged with addressing the need for markers intended to communicate the dangers and location of the wastes.

Performance assessments for the WIPP will continue to be conducted in accordance with the requirements of 40 CFR 191 and the EPA's accompanying guidance. Any new guidance from the EPA would be implemented in the WIPP performance assessment calculations.

Comment P2

"The 1992 Performance Assessment shows compliance with the EPA standard under various conceptual models (1, Fig. 5-6, p. 5-14)."

"Would there be compliance if the intrusion probabilities of 1991 were used?"

"Differences between 1991 and 1992 are:

λ subjectively elicited in 1992

$\lambda = 0$, for $t > 2000$ years in 1992"

Response

This comment does not raise technical or regulatory issues.

Both the 1991 and 1992 PAs were preliminary, and show neither compliance nor noncompliance with the EPA standard. Determination of compliance will be based on a compliance application.

Because subsurface releases were not calculated for later times of intrusion in the 1992 PA, it is not possible to quantify the effect of the simplification on the location of the CCDF.

Results were presented in 1992 separately using both subjectively elicited functions for λ (the "time-dependent λ case") and the same time-invariant construction used in the 1991 PA (the "constant λ case").

Lambda was assumed to be equal to zero after 2,000 years only for subsurface releases. Cuttings releases at the surface from intrusions were calculated for the full interval from 100 to 10,000 years (see Figures 5-1 and 5-2 in Volume 1 of the 1992 PA).

Comment S1

"As a reminder, the assumption of 100 yr of administrative control in which no drilling intrusion can occur is equivalent to assume that $\lambda(t) = 0$ for $0 < t < 100$ yr." SAND92-0700/4, p. 2-19, at 49.

"...drilling intrusions assumed to be zero after 2000 yr." SAND92-0700/4, p. 2-19, l. 3.

"EEG objects to assuming away the problem."

Response

Decisions about the probability of drilling during the first 100 years after decommissioning may be reevaluated when additional guidance is available from the EPA.

It is correct that, for the purpose of calculating subsurface releases, the assumption that no intrusions occur after 2000 yr was made in the 1992 PA. Cuttings releases were calculated for the full interval from 100 to 10,000 years. The 1992 PA was preliminary, and results were not intended to be used as the basis for a compliance decision. The compliance application for WIPP will not be constrained in this manner.

Comment S2

"The analysis of CUTTINGS release is elegant. However, the effect of spallation of waste panels is not yet analyzed.

Moreover, the current cuttings analysis assumes perfect blow-out preventers acting immediately.

Drilling practice through 1982 included release of brine at the surface whenever pressurized Castile brine reservoirs were encountered. See Table 1."

Response

The DOE is currently investigating the issue, and it will be included in the SPM. (See EPP 5.4.3)

The effects of spalling are being examined by the DOE (EPP 5.4.3), and will be included in the SPM. Flow of brine to the ground surface during drilling, with and without blowout preventers, may also be evaluated in the SPM.

Comment S3

"The EEG reiterates that the following scenarios need attention.

- Removal of miracle plugs and seals
- Brine slurry discharge to the surface
- Subsidence from potash mining
- Criticality"

Response

The DOE is currently investigating each of these issues except for brine slurry formation, and they may be included in the SPM.

Modifications to the stratigraphic conceptualization used in BRAGFLO have been made since the 1992 PA that will allow simulation of borehole flow without the arbitrary perfect plugs above the Culebra.

Effects on groundwater flow of subsidence related to potash mining will be examined in the regional three-dimensional flow modeling (EPP 5.1.1.2).

The likelihood and consequence of criticality within the disposal system is presently under investigation (WBS 1.1.6.2.1).

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Comment AG-I1

Page 1

Initially, it is unfortunate that the 1992 PA contains no analysis of the performance of the proposed repository based on alternative engineered barriers and waste form modifications. DOE has an obligation to make an informed selection among engineered alternatives, such as cementation, vitrification, shredding, supercompaction, incineration, improved containers, various backfill materials, melting and possible removal of metals, waste loading configurations, disposal room configurations, and the like. An informed selection requires careful study of the benefits of alternative engineered barriers and waste form modifications.

Response

The DOE agrees on the benefit derived from such a study. An engineered alternatives benefit/detriment analysis is currently underway. This study will analyze possible alternatives such as those listed above and evaluate enhancements to performance with system-wide impacts. The results of this study will be complete late in fiscal-year 1995. This study will allow an informal selection of alternatives to the extent such alternatives are needed to comply with the performance standards.

Comment AG-I2

Page 1

An objective release rate standard should be applied to such modifications (see e.g., 10 C.F.R. §60.113), in addition to the CCDF.

Response

This comment suggests the creation of a regulatory standard not currently in effect for the WIPP. The DOE believes that the current regulatory standards provide adequate protection of human health and the environment.

Comment AG-I3

The precise form of such analysis raises further issues (e.g., what assumptions should be made as to future states and the nature of future human intrusions?) The PA is incomplete without such an analysis.

Response

The WIPP Project anticipates that 40 CFR 194 will provide guidance for assumptions on future states and future human intrusions.

Comment AG-I4

Page 1

Further, the PA contains no analysis of the performance of the WIPP site as compared with the performance of a site located outside a resource zone and, thus, not vulnerable to likely human intrusions. DOE should carry out a comparative analysis of one or more hypothetical sites to assess the disadvantages of the resource-rich WIPP site.

Response

By nature, a comparison of alternative sites should be qualitative. The PA report is not inherently designed to perform this type of analysis. The site selection process has already considered such alternative sites as specified in the Assurance Requirements of 40 CFR 191, specifically, the resource disincentive requirement. DOE/WIPP 91-029, Revision 1 states that:

> .....Any comparison of the overall protection afforded by one site to the overall protection of another, for purposes of compliance with §191,14(e), should be done on a purely qualitative basis. [The RDR] is a preliminary siting criteria. Thus, its primary purpose is to distinguish between potentially acceptable and potentially unacceptable sites. It is then the purpose of the containment requirements, the other assurance requirements, the individual protection requirements and the groundwater protection requirements to determine the ultimate acceptability of the site as a disposal system for radioactive wastes.

The results of this comparison conclude that the favorable features of the WIPP are reasonably expected to offset any enhanced risk of human intrusions associated with resources.

Reference

Department of Energy, 1991. *Implementation of the Resource Disincentive in 40 CFR Part 191.14 (e) at the Waste Isolation Pilot Plant.* DOE/WIPP 91-029, Revision 1. Carlsbad, New Mexico.

Comment AG-1a

1. Scenario selection:

This subject is primarily addressed in the 1992 PA in v. 2 ch. 4, which in turn refers to the 1991 PA (1992 PA V. 2, 4-4; 1991 PA, v. 1, ch. 4). The 1992 PA says that the process of scenario screening will be reexamined when the disposal regulation, 40 CFR 191, is repromulgated. (1992 PA, Volume 2, p 4-3). The screening process should also be reexamined when EPA issues its compliance criteria, 40 CFR 194. In such reexaminations the following should be considered:

Screening as now practiced employs the criteria of (a) low probability, (b) physical reasonableness, (c) small consequence, and (d) regulatory limitations. (1991 PA, Volume 1, p. 4 -12). In general we find that certain scenarios, once selected, have been screened or eliminated on bases that are not adequately explained in quantitative terms. We note in particular:

a. Erosion, sedimentation, flooding, mass wasting, glaciation, and sea level changes causing releases are excluded as not physically reasonable on the premise that climate changes in the next 10,000 years will be within the ranges of conditions occurring within the past 10,000 years (at 4-14, 4-15, 4-18, 4-19). Comments submitted to the EPA by Prof. Roger Y. Anderson (March 16, 1993) point out that the past 10,000 years have been extremely dry in comparison with the average of the past 800,000 years. During at least half of the next 240,000 years climatic conditions are expected to be significantly wetter than at present. Past changes in precipitation and streamflow in the area have involved brief and strong climate episodes departing greatly from average climate. Thus, the assumption of average climate appears to be inappropriate, and these scenarios should therefore be reexamined for inclusion.

Response

These events and processes have been reconsidered in the current scenario screening work. The observation here that each has the potential to be effected by climate change is correct. However, even assuming climatic changes during the next 10,000 years equivalent to the most extreme climates of the last two million years (an improbable assumption), none of these events and processes will effect disposal system performance. Arguments supporting these assertions will be provided for review with the documentation of the scenario development work.

The concern raised by Prof. Anderson in his letter to the EPA was not that the WIPP Project's approach to treating climatic variability during the next 10,000 years was inappropriate, but rather that the EPA should consider a regulatory period longer than 10,000 years. In this regard, both Prof. Anderson and the New Mexico Attorney

General's Office are proposing the creation of a new regulatory standard for radioactive waste disposal.

Comment AG-1b

Page 2

Pluvial periods are retained for further consideration. (1991 PA, Volume 1, p. 4-15). We note the fact and look to future performance assessments for analysis.

Response

Climatic change is retained in the scenario development work for inclusion in consequence analysis.

Comment AG-1c

Page 2

Magmatic activity is excluded on the basis of physical unreasonableness, with the explanation that a mid-Tertiary dike system within a zone of crustal weakness was not followed by similar magmatic formations during Pliocene-Pleistocene uplift and fracturing (1991 PA, v. 1, 4-23). The 1991 PA concludes that "a change in the geologic processes at this location has occurred." Such explanation is entirely too conclusory; a coherent factual hypothesis is required.

Response

Work is in progress to develop a sufficient basis for the screening magmatic activity out from further consideration in performance assessments. This work will be documented in a future version of the scenario development position paper. There is no reason to suspect that evidence will be found indicating that magmatic activity could be of sufficient probability or consequence to warrant inclusion in scenario development.

Comment AG-1d

Page 3

Deep dissolution is said to be part of the base-case scenario, insofar as it concerns dissolution along the Salado-Rustler contact. (1991 PA, v. 1, 4-27). It is not demonstrated how deep dissolution is incorporated into the modeling of base-case performance.

Response

Dissolution along the Rustler-Salado Contact Zone is treated in both the 1991 PA and in the current scenario development work as shallow dissolution, rather than deep dissolution (see 1991 PA, Volume 1, p. 4-25 through 4-28). In the 1991 PA, shallow dissolution was retained for inclusion in the base case because it is an ongoing process. The current scenario development work concurs that shallow dissolution is an ongoing process in the region, but screens it out from further consideration on the grounds that there is a low probability that its occurrence during the next 10,000 years can affect system performance.

Comment AG-1e

Page 3

The discussion of human-induced explosions state that seismic effects on the source term or the disposal system are likely to be addressed within parameter uncertainty during modeling. (v. 1, 4-32, 4-32). It should be demonstrated that in fact this will be done.

Response

Current scenario development work recognizes three categories of future human-induced explosions that could potentially affect the WIPP: explosions for resource recovery, underground nuclear testing, and acts of war. Explosions for resource recovery are considered to be of low consequence because liquid or gas resources suitable for recovery through fractures are not found at the repository horizon. Underground nuclear testing is presently retained for further consideration, although work is in progress to prepare a screening argument based on low probability. Acts of war are screened out on the basis that they are beyond the scope of the regulatory requirements. Parameter uncertainty analyses of the consequences of explosions will not be performed unless screening arguments indicate that explosions require further analysis.

Comment AG-1f

Page 3

The limitation of the type and amount of human intrusion to be considered is said to be based on the guidance in Appendix B of the 1985 version of 40 CFR 191. That guidance will be supersede by the forthcoming EPA compliance criteria, 40 CFR 194. Thus, the nature of intrusion to be considered must be reconsidered based on forthcoming EPA pronouncements. Further, even following the 1985 guidance, it is not accurate to state that each of the Futures Panel teams estimated future drilling densities substantially lower than 30 boreholes/km² in 10,000 years (1991 PA, v. 1, 4-33). The Boston Team, for example, developed a conditional distribution for the average number of boreholes per square mile per 10,000 years, ranging from 12.45 to 199.2 boreholes/mi² (SAND 90-3063, at IV-15). In addition, to consider potash and natural gas as the only two resources with economic potential at the WIPP (at 4-33, 4-38) ignores the substantial oil resources in the area (See EEG 9/93 Comments; Implications of Oil and Gas Leases at the WIPP on Compliance with EPA TRU Waste Disposal Standards, EEG-50 (June 1992), at 13).

Response

Drilling probabilities for use in a compliance application will be reevaluated following promulgation of 40 CFR 194.

With regard to the observation about the drilling rates estimated by the Boston Team, the appropriate information is given in Table IV-14, on page IV-16 of Hora et al., 1991 (SAND90-3063). Table IV-13, on page IV-15 of Hora et al. (1991), is an intermediate step in the analysis and was included as an example demonstrating the methodology used. Table IV-13 does not represent the conclusions of the Boston Team. As the reviewer notes, the probabilities given in Table IV-13 are conditional: in this case, on the certain occurrence of a particular set of future states that the Boston Team believed were in themselves unlikely.

Reference

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgement on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

(AG/92PA-1f)

M

Comment AG-1g

Page 3

Human intrusion of various types must plainly be considered in applying the containment requirements. The 1991 PA excludes mining intrusions at the WIPP site based on the 1985 EPA guidance (1991 PA v. 1, 4-43). We disagree that the EPA guidance excludes this scenario. Again, this exclusion must be reconsidered based upon the compliance criteria. We note that mining beyond the area of the waste panels is retained for scenario development (1991 PA, v. 1, 4-35) and anticipate that future PA's will evaluate such a scenario.

Response

Current scenario development work retains potash mining within the controlled area for further consideration. This decision will be reconsidered as appropriate following promulgation of 40 CFR 194.

Comment AG-1h

Page 3

Injection wells are also said to be limited in their PA consideration by the 1985 guidance (1991 PA, v. 1, 4-37). This scenario is not excluded even under the 1985 guidance, and Futures Panel teams were unable to estimate its probability (1992 PA, v. 2, 5-7). The scenario must also be reassessed based on the forthcoming compliance criteria. The PA statement that injection wells can be excluded based on lack of consequence (1991 PA, v. 1, 4-37) must be reconsidered in light of the demonstrable widespread effects of well injection in the course of secondary recovery of hydrocarbons typical of the Delaware Basin.

Response

Current scenario development work retains injection wells for further consideration in the PA. Injection of fluids for enhanced recovery of hydrocarbons or waste disposal is highly unlikely to occur directly into the Salado Formation or overlying strata. However, injection wells have the potential for some fluid to enter these strata inadvertently through faulty well casing and hence might affect disposal system performance.

M

Comment AG-1i

Page 4

The statement is made that withdrawal wells within the repository area are excluded by the 1985 guidance (1991 PA v. 1, 4-37). It is not at all clear that the drilling and operation of oil or gas withdrawal wells would be deemed a more "severe" intrusion scenario than exploration wells. Further, it is stated that water well emplacement is retained for scenario development (event E3), and we look to future PA's to analyze this scenario. EEG has noted that the TDS concentration in the H-L well referred to hovers close to 10,000 mg./1. EEG Preliminary Comments on 1991 Performance Assessment

("EEG 8/92 comments"), at 24). Moreover, water wells may be drilled in the future for purposes other than obtaining potable water.

Response

Wells drilled for any purpose (including withdrawal of hydrocarbons of water) within the controlled area that do not directly intersect waste are retained for further consideration in current scenario development work.

Comment AG-1j

Page 4

The grounds for exclusion of a scenario involving irrigation are not convincing (1991 PA, v. 1, 4-40, 4-41). The prospects of irrigation usage are said to be low based upon current land usage in the southeastern United States, current climate conditions, and current water commitments. Before irrigation can be excluded as a relevant scenario, it must be analyzed for probability and consequence based upon changes in such factors.

Response

Current scenario development work retains irrigation for further consideration before a screening decision can be made. Although irrigation is not presently practiced in the WIPP vicinity, climate change could result in different land use in the future. Modeling of the effect of climate change itself on regional groundwater flow will provide insight into possible consequences of irrigation and will provide the basis for a screening argument.

Comment AG-1k

M

Page 4

Similarly, the creation of an impoundment at Nash Draw is excluded as improbable, based on present day water supplies and usage (see EEG 8/92 comments, at 24-25). The scenario must be considered based on its probability and consequences, given possible future changes in such factors.

Response

Current scenario development work retains the creation of an impoundment in Nash Draw for further consideration before a screening decision can be made. Although water availability is insufficient at present to justify such an impoundment, climate change could alter that condition in the future. Modeling of the effect of climate change itself on regional groundwater flow will provide insight into possible consequences of damming and will provide the basis for a screening argument.

Comment AG-1l

Page 4

Subsidence due to the caving of the waste panels is excluded for lack of consequence (1991 PA, v. 1, 4-49), but this conclusion cannot be justified. First, the degree of subsidence is estimated from admittedly inappropriate analogues and using disposal room models that are obsolete. Second, it is assumed that subsidence occurs uniformly and without bed separations--hardly realistic assumptions. Third, the argument that the subsidence of the Rustler-Salado contact member is an analogous event demonstrating on disruptive consequence ignores the fact that naturally-occurring subsidence is much more gradual than collapse caused by mining. The scenario should be retained.

Response

Current scenario development work retains subsidence over the waste panels for further consideration before a screening decision can be made. Such subsidence has the potential to affect groundwater flow in overlying aquifers by providing pathways for increased vertical flow between units.

Note that a modeling study has recently been published by IT (1994) that suggests subsidence due to caving will have no long-term effects on performance of the non-Salado units.

Reference

Westinghouse Electric Corporation, 1994. *Backfill Engineering Analysis Report.* WID, Carlsbad, NM, 1994.

Comment AG-1m

Borehole seal degradation can be considered by sampling a range of input parameters in PA (1991 PA, v. 1, 4-49). Such a method should be adopted. At present assumptions are employed to maximize the flow of brine through the repository and thence to the Culebra, leading to possible underground release. As EEG has pointed out, assumptions as to seal effectiveness are not well-founded factually based on current practices in the basin (EEG 9/93 comments, at 22). PA should evaluate the likelihood and consequences of releases via improperly sealed boreholes.

Response

Current SPM analyses consider boreholes with fully degraded plugs or no plugs. Flow is allowed through degraded borehole-fill material into a simplified representation of all stratigraphic layers and to the ground surface.

WIPP PA analyses in the past have evaluated "consequences of releases via improperly sealed borehole." Borehole plugs were assumed to degrade immediately following emplacement to a silty sand-like material. Arbitrary "perfect" plugs were assumed only to divert flow in specific paths (i.e., through the waste panels and into the Culebra) to maximize flow and transport along these paths for the purposes of consequence analysis. These "perfect" plugs were not intended to represent properly sealed boreholes.

Comment AG-1n

Page 5

It has not yet been determined whether to consider a scenario involving nuclear criticality at some location in the transport of plutonium radionuclides. (1991 PA, v. 1, 4-53). EEG has demonstrated that the probability distributions of plutonium distribution coefficients and solubilities are consistent with the possible occurrence of nuclear criticality in the Culebra (EEG 9/93 comments, at 16). We look to future PA's for analyses of such event.

Response

Criticality will be considered in analyses for event and process screening. The SPM will determine whether to include criticality in PA calculations.

M

Comment AG-1o

Page 5

Subsidence related to solution mining has not been analyzed, although the 1992 PA recognizes the need (event TS; 1992 PA, v.2, 4-9). We inquire when this will be done.

Response

Current scenario development work retains potash mining for further consideration, without distinguishing between conventional and solution mining.

Comment AG-1p

Page 5

Possible brine flows to the surface during and in consequence of drilling should be analyzed. Any regulatory limitations on such a scenario must be reassessed after EPA issues its compliance criteria. Current practice is not consistent with an assumption that releases of contaminated brine will be minimal. (EEG 9/93 comments, at 18). Several cases for analysis exist (id. 21) and should be considered.

Response

This scenario will be evaluated in the SPM.

Comment AG-1q

Page 5

Formation of a brine slurry which is thereafter released in an intrusion event is another scenario which should be analyzed (see EEG 9/93 comments, at 21). Such analysis is obviously bound up with the PA development of the Radionuclide source term and the modeling of direct releases (cuttings, cavings, and spallings).

Response

The Project believes a brine-slurry release in any context is unlikely. As stated in the comment, several studies have been performed within the last several years on the possibility of brine-slurry in salt backfill in the underground environment. Results of the studies indicated brine-slurry in salt backfill would not impact performance because room closure rates (even considering likely gas generation rates) will decrease the available volume and recompact the salt to a point that even if brine fills the residual porosity a slurry cannot develop.

With respect to slurries forming from decomposing waste, itself, we believe closure rates and compaction would result in an end product with sufficient shear strength that a mobile slurry would not form. For further information, see Disposal Room and Cuttings Model Position Paper for Systems Prioritization and Technical Baseline, section 4.2.4.5.

A model for representing incomplete consolidation of the waste, in various degrees of decomposition has been developed, but is not yet part of cuttings release calculations. Attention has been focused on pressurized gas. To date, the spall problem has usually been cast in terms of inflow of a fluid into an intrusion well, and the material that is carried along in this process. Variable strength materials will eventually be incorporated into the cuttings release calculations to encompass concerns that may exist about whether appropriate strength materials have been modeled.

Comment AG-2a

Page 5

M

Human intrusion conceptual models:

Certain assumptions underlying the existing conceptual models of human intrusion appear to be nonconservative.

> "As stated above, the assumed efficacy of the borehole plug above the repository and above the Culebra may be nonconservative (1992 PA, v. 2, 4-15)"

Response

Current analyses consider boreholes with fully degraded plugs or no plugs. Flow is allowed through degraded borehole-fill material into a simplified representation of all stratigraphic layers and to the ground surface.

WIPP PA analyses in the past have evaluated "consequences of releases via improperly sealed borehole." Borehole plugs were assumed to degrade immediately following emplacement to a silty sand-like material. Arbitrary "perfect" plugs were assumed only to divert flow in specific paths (i.e., through the waste panels and into the Culebra) to maximize flow and transport along these paths for the purposes of consequence analysis. These "perfect" plugs were not intended to represent properly sealed boreholes.

Comment AG-2b

Computational approximations do not model E1 explicitly but assume that consequences are the same as E2. (1992 PA, v. 2, 4-18). This assumption should be questioned. The 1992 PA itself says that E1 release may exceed E2 releases (id.). Whether E2 releases will dominate E1 releases may also change when assumptions as to surface brine releases are altered and when spalling is modeled.

Response

Analyses performed for the first iteration of the SPM treated E1 and E2 individually, and assumed that E1-type intrusions would result in some flow of brine from the Castile Formation through the waste.

Comment AG-2c

Page 5

As currently modeled, an E1E2 release takes place only when both boreholes occur in the same time interval. (1992 PA, v. 4, 2-16). Indeed, it is assumed that the E1 and E2 intrusions occur simultaneously (1992 PA, v. 4, 2-18). Given the complexity of the numerous processes involved, it is not now possible to demonstrate that the assumption is conservative. We have been informed that Sandia has made initial experiments involving time-dependent drilling intrusions and inquire as to plans in this regard.

Response

The WIPP Project is currently investigating alternative approaches to CCDF generation that would facilitate consideration of intrusions at different times. This approach will be used in the SPM if it is demonstrated to be suitable.

Comment AG-2d

Page 6

The 1992 PA considers only intrusions at the 1,000 year point as to groundwater transport. The statement appears that the Poisson model of intrusions has a rate term of zero after 2,000 years (1992 PA, v. 4, 2-18, 2-19). These assumptions, which limit intrusions leading to groundwater releases to a single point at 1,000 years, are nonconservative and should be abandoned.

Response

The sampling of intrusion times may be needed for a complete analysis that would be used for compliance application. The decision to limit subsurface release calculations to a single time of intrusion was based solely on project resource limitations in 1992.

The 1991 PA included analysis of subsurface releases from 5 times of intrusion (1,000, 3,000, 5,000, 7,000, and 9,000 yr). Comparison of figures 4.1-2 (lower right frame) and 5.1-4 (upper right frame) of Volume 4 of the 1991 PA (SAND91-0893/4) shows the effect of later times of intrusion on dual-porosity subsurface releases. The 1990 PA sampled the time of intrusion (rather than including it in the probability model), and results of a formal sensitivity analysis including time of intrusion are published in Helton et al. (1991).

Reference

Helton, J.C., J.W. Garner, R.D. McCurley, and D.K. Rudeen. 1991. *Sensitivity Analysis Techniques and Results for Performance Assessment at the Waste Isolation Pilot Plant.* SAND90-7103. Albuquerque, NM: Sandia National Laboratories.

Comment AG-2e

Page 6

In examining direct releases by human intrusion ("cuttings") the 1992 PA assumes that intrusion times are at years 125, 175, 350, 1,000, 3,000, and 7,250 (1992 PA, v. 4, 2-26). The PA should demonstrate that those assumptions are conservative.

Response

The WIPP Project agrees that the times of intrusion chosen for analysis should be suitably representative of the complete set of all possible times during the regulatory period. This will be demonstrated as part of the compliance application.

Comment AG-3a

M

Page 6

Salado Formation data:

Sampled parameter values calling for further justification are as follows:

Undisturbed halite permeability is sampled over a narrower range in 1992 than in 1991. This parameter is a sensitive one in that it determines how quickly the panel will fill with brine, furnishing a vehicle for radionuclide release. (1992 PA, v. 3, 2-30). The PA department has stated that neither the 1991 nor the 1992 distribution represents the average far-field permeability, the quantity that should be used in two phase flow model (Id.). Approach based on experimental data seem necessary. What is planned?

Response

Undisturbed halite permeability is sampled over a wider range (not narrower) range in 1992 compared to 1991. The 1991 range was 5.4 x 10^{-22} m^2 to 8.6 x 10^{-22} m^2 (1991 Volume 3, p. 2-27). The 1992 range was 10^{-19} to 10^{-24} m^2 (1992 Volume 3, p. 2-29). Both the 1991 and the 1992 ranges are based on experimental measurements; however, the

1992 range, and to a lesser extent, the 1991 range may include data that are influenced by the excavation. This may result in somewhat higher permeabilities for the far field than true undisturbed measurements would suggest. At any rate, the use of these higher permeabilities allows for larger brine inflow than might be representative of intact material.

Comment AG-3b

Page 6

Undisturbed anhydrite permeability clearly requires improved data and modeling in forthcoming PA's. At present there is no representation of fracturing under pressure. The 1992 calculations may underestimate lateral gas migration in the anhydride and overestimate pressurization. (1992 PA, v. 3, 2-57, A-50). When may we expect a model that incorporates pressure-induced fracturing?

Response

An altered anhydrite submodel has been developed and incorporated into BRAGFLO subsequent to the 1992 calculations. In this submodel, permeability and porosity are functions of fluid pore pressure and are enhanced at elevated pressure.

Comment AG-3c

Page 6

The 1992 PA reports that work is in progress on modeling the possible pressure dependency of fracture permeability in anhydride interbeds, and results will be incorporated in future PA's. (1992 PA, v. 2, 2-42). There have been reports concerning such modeling plans at NAS WIPP panel meetings. What is the status of such modeling effort?

Response

See response to previous comment (AG-3b).

Comment AG-3d

Page 6

Anhydrite brine pore pressure in the far-field is another sensitive parameter now estimated by use of regression curves. (1992 PA, v. 3, 2-63). As the PA states, "[w]hether these results make physical sense remains to be determined." (Id.) What further efforts are planned in this area?

Response

PA no longer estimates far field pore pressure by the regression curves discussed in 1992 PA Volume 3, 2-63. PA continues to use data measurements believed to be representative of the far field. In the 1992 PA the far field pore pressure in anhydrite ranged from 12 to 13 MPa. The inclusion of the regression curves, which attempt to characterize pore pressure as a function of distances from the excavation, may cause some confusion. They are meant to be informative and are a possible way to estimate far field values but they are not used by PA.

Comment AG-4a

M

Page 7

Waste properties:

Several issues are unresolved concerning the waste inventory:

Concerning inventory data, the Experimental Program Plan, DOE/WIPP 94-008, refers to waste characterization efforts planned or underway at source facilities. (at 3-38, 4-14). DOE has also mentioned plans to develop performance-based waste acceptance criteria. The current PA model, however, employs inventory data based on data submitted by generator sites to the Integrated Data Base. (1992 PA, v. 3, 3-59 et seq., A-137). There is potentially large uncertainty as to volumes of combustibles and metals/glass (1992 PA, v. 3, 3-62). The parameters are significant to RCRA compliance and also, possibly, to radionuclide releases. DOE needs to clarify its plans to establish inventory data for PA.

Response

The DOE has developed the Baseline Inventory Report (BIR) and Database. This report integrates the waste generator sites' input to the Integrated Data Base and the Mixed Waste Inventory Report required by the Federal Facilities Compliance Act. A revision (Rev. 1) to the BIR is in process and will be provided in the second quarter of

FY 95. The BIR keys on eleven performance parameters that could be significant to PA and reports information relative to these parameters.

Comment AG-4b

Page 7

Initial waste saturation is a highly sensitive parameter for RCRA compliance, since it is a principal control on gas generation (1992 PA, v. 3, 3-69). The range of 0, 0.14 based upon investigator judgment calls for substantiation based on characterization efforts. The PA states that the "range of initial brine saturation currently used does not have a sound basis in measured data, and is expected to change." (1992 PA, v. 5, 6-1). What efforts are planned?

Response

The range of 0.40% to 5.2% was calculated from measured data presented in a memo (DA:93:10052, attached) from John Elliott, Westinghouse-Idaho to L.R. Fitch, WID, dated October 8, 1993. Waste characterization activities underway and planned at the waste generator sites continue to include determination of water content. The range used for compliance performance assessment will be updated to reflect all available data.

Comment AG-5a

Page 7

Radionuclide source term:

The radionuclide source term is highly dependent upon solubility data. We have these questions:

"Solubility" (more specifically, mobile actinide concentration) distributions have been constructed on the basis of expert judgement. For future performance assessments DOE proposes to develop a model and lab data with which to determine whether the constructed distributions are supported. Further, lab data will explore different brine compositions, including Salado brines altered by backfill constituents. At this early stage detailed comment is not appropriate. However we note the following:

The methods whereby test data may be deemed to support or "validate" solubility estimates or ranges must be explicitly stated and justified.

Response

Order-of-magnitude matches between experimentally measured quantities and model predictions will be considered to constitute "good" agreement. If model predictions diverge from measured data by more than an order of magnitude, then steps will be taken to determine the reason(s) for that divergence. The disagreement between predicted and measured quantities may be acceptable for purposes of compliance demonstration if the model is found to predict mobile actinide concentrations reasonably similar to those observed experimentally.

M

Comment AG-5b

Page 7

The duration of the experiments and the purported attainment of steady-state conditions must be supported by the proponent of data.

Response

We agree with the comment. Experimental data will be evaluated with respect to the degree to which steady-state conditions have been attained to determine the utility of the data to the program. However, the definition of steady-state conditions is complex; i.e., many different equilibria are involved in each potential experiment, and steady state may not be reached simultaneously for all of the equilibria of importance.

Comment AG-5c

Page 8

It must be explicit how data from the model will be incorporated into the BRAGFLO model.

Response

The Actinide Source Term model will be used to generate a table or series of tables which give mobile actinide concentrations under a variety of pH and other physical-chemical conditions. Data from other sources, such as the Gas Generation Program will be used to determine the set of conditions for each PA calculation. This set of conditions will determine, in turn, the table values to use for mobile actinide concentrations for that calculation. There will be a mixture of deterministic and probabalistic parameters involved in the determination of the appropriate set of physical-chemical conditions appropriate for the calculations.

Comment AG-6

Page 8

The hazardous waste source term, if modeled at all, is not yet incorporated into assessments of gas and brine migration. Please explain how this is to be done.

Response

The project is currently evaluating two conceptual models for the hazardous waste source term. One assumes constituents are present in sufficient quantities to saturate the gas phase. The other uses waste characterization data to determine the source term. This topic will be the subject of a position paper and will be included in the SPM.

Comment AG-7a

Page 8

Radionuclide inventory:

For the 1992 PA the radionuclide inventory is estimated based on input to the 1991 Integrated Data Base (1992 PA, v. 2, 2-50). The CH-TRU inventory is scaled up from the current and projected CH-TRU inventory at five high-volume generating sites. However, uncertainty in the CH-TRU inventory is large, particularly given the potential changes in the sources of CH waste due to given the potential changes in the sources of CH waste due to changes in weapons production. (1992 PA, v. 2, 2-5). It is possible that DOE may seek to dispose at WIPP of waste from cleanup operations or weapons dismantlement. The RH-TRU inventory in the IDB is approximately the same as the WIPP design capacity and is not scaled up (id.). However, there is also uncertainty as to the characteristics of yet-to-be characterized RH-TRU waste. There is also talk of performance-based waste acceptance criteria. In these circumstances, we point out the following:

DOE must explore the ranges of uncertainty of radionuclide inventory as an element of its PA uncertainty and sensitivity studies. Further, since radionuclide inventory is within the control of DOE (as opposed to being a subjectively unknown variable), random sampling within a stated range may be inappropriate, and it may be necessary to employ "worst case" assumptions.

Response

Compliance performance assessment will be done according to the requirements of 40 CFR 191, which provides requirements for radionuclide releases based on a normalized inventory, and thus is not sensitive to uncertainties in the actual transuranic element inventory.

Comment AG-7b

Page 8

DOE should reexamine the determination to exclude RH-TRU waste from calculations of underground releases (1992 PA, v. 3, 3-28) as estimates of RH-TRU inventory are refined and canister design assumptions become clearer.

Response

This decision will be reconsidered in future analyses.

M

Comment AG-7c

Page 8

DOE must clarify its position as to the time as of which the curie content of TRU waste should be ascertained for purposes of calculating release limits. In a related context DOE has said that the curie content should be determined as of 100 years after disposal. See Final Supplement to EIS, 1990, v.2, at 18-19. Will such procedure be employed in future PA's?

Response

(a) The release limits in 191.13(a), found at Appendix A, Table 1 to 40 CFR Part 191, are designed to regulate releases of radionuclides with half lives of 20 years or more. These limits specifically apply to: Americium-241 or -243, Carbon-14, Cesium-135 or -137, Iodine-129, Neptunium-237, Plutonium-238, -239, -240, or -242, Radium-226, Strontium-90, Technetium-99, Thorium-230 or -232, Tin-126, Uranium-233, -234, -235, -236, or -238; and to any other alpha-emitting radionuclide with a half-life greater than 20 years, and to any other radionuclide with a half-life greater than 20 years that does not emit alpha particles.

The waste unit determines the release limits for a specific repository. EPA based the waste unit derivation on a philosophy of equivalence between the various types of waste regulated by 40 CFR Part 191. Therefore, the curie content of the inventory should be determined at the time that most nearly approximates the assumptions EPA used in deriving the release limits. The following is quoted from a draft report, Risk Assessments of Spent Fuel, Transuranic, and High-Level Radioactive Wastes in Mined

Repositories, Technical Support Document, prepared by S. Cohen & Associates, Inc., January 20, 1994 (RAE-9231/1-3), page 2-23.

In the version of 40 CFR 191 promulgated in 1985, Note 1(c) specified that the Table 1 release limits applied to each 100 million curies of beta/gamma-emitters with half-lives between 20 and 100 years; Note 1(d) stated that the limits applied to each 1 million curies of beta/gamma-emitters with half-lives greater than 100 years; and Note 1(e) stated that they applied to each 1 million curies of transuranic wastes containing alpha-emitters with half-lives over 20 years. EPA did not provide a detailed explanation of the basis for these quantities. The preamble to the proposed 40 CFR 191 published in 1982 stated that the 1 million curie quantity specified in Note 1(e) for TRU waste was chosen "so that the standards would require radioactivity from either high-level or transuranic wastes to be isolated with about the same degree of effectiveness" [U.S. Environmental Protection Agency, Preamble to Proposed 40 CFR Part 191, 47 FR 58200, December 29, 1982.] Furthermore, the Draft EIS stated that the reference values of 1 million curies of TRU waste and 1,000 MTHM of spent fuel "were selected so that about the same fraction of transuranic radionuclides would be retained for either high-level or transuranic waste" [US EPA, "Draft Environmental Impact Statement for 40 CFR 191: Environmental Standards for Management and Disposal of Spent Nuclear Fuel, High-Level and Transuranic Radioactive Wastes," EPA-520/1-82-025, December, 1982, p. 116]. These estimates had been made based on looking at the number of TRU curies in 10 year old spent fuel and rounding to the nearest arithmetic order of magnitude.

(b) The Final Supplement to EIS, 1990, Volume 2, at B18-19 discusses the use of the inventory in developing the initial source term for transport calculations. The waste unit upon which the release limits were based in the technical support reference, Lappin et. al., 1989, was the estimated inventory at decommissioning. These are two separate concepts. The waste unit and release limits must be calculated from the initial inventory; transport calculations must take into account the decay of the radioactivity over the course of the 10,000 years of regulatory concern. What the FSEIS reports is the assumption used to start transport calculations, which began after the end of active institutional controls. Table B.2.13 presents the initial radionuclide inventory in CH TRU waste for the assessment of long-term performance, and Table B.2.14 gives the modified inventory, which was modified by assuming that the radioactivity has decayed for 100 years. Table B.2.13 is the source of the waste unit, which may be seen to be 6.66 by summing the activities of TRU radionuclides with halflives greater than 20 years and dividing by 1 million. This is consistent with the method in the technical support document cited by DOE (A. R. Lappin, R. L. Hunter, D. Garber, and P. B. Davies, eds., 1989; Systems Analysis, Long-Term Radionuclide Transport, and Dose Assessments, Waste Isolation Pilot Plant (WIPP) Southeastern New Mexico, March 1989, SAND89-0462, Sandia National Laboratories, Albuquerque, New Mexico).

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Calculating the waste unit after the end of the 100-year period of active control would be inconsistent with the spent-fuel equivalency rationale developed by the EPA to allow derivation of a single set of releases limits with effectively equivalent waste units.

Comment AG-7d

Page 8

DOE must ensure that its PA analyses conform to waste inventory projections contained in, e.g., its environment impact statements and other authorizations (e.g., transportation and RCRA permits) and vice-versa.

Response

Due to changes in DOE's mission, the DOE is continuing to evaluate waste inventory projections as the generator site work scopes change. This task included the generation of the WIPP Transuranic Waste Baseline Inventory Report. Sandia will use the source information contained in that document in their ongoing PA analyses. The DOE will continually update and improve inventory projections, to the extent these impact PA, the update will be evaluated.

The other "authorizations" do not affect inventories of radionuclides. Transportation criteria address the radionuclide loading of any single container and/or any TRUPACT-II. This does not limit the total "inventory " used by PA. RCRA does not address radionuclides at all.

Comment AG-8a

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Repository-waste interactions:

Page 9

Modeling of the complex interactions among gas generation, repository closure, and brine flow is clearly still in development. Models of particular aspects of these processes are being developed outside of the PA process. Comments prompted by the current state of development are:

As to the gas generation model, numerous uncertainties were outlined by Larry Brush at the July 7-9, 1993 meeting between DOE and EPA. Without reiterating these, it should be clear that such uncertainties need to be addressed, either to resolve them or to determine that they are not important.

Response

The Gas Generation Program is addressing these uncertainties. The first step is to determine if the uncertainty either directly or indirectly has a significant impact on performance. Resolution of uncertainties found to be significant can then be approached by additional experimental work (included as an activity for evaluation by the SPM) or, if that is not possible, then the range of parameter(s) values implied by the uncertainty is sampled during the PA analysis. Alternative conceptual models will be handled the same way.

Comment AG-8b

Page 9

PA should clarify in what sense it is not now possible or practical to use a coupled mechanical and fluid flow model. The transition between the data reported by SANCHO and those employed in BRAGFLO must be made clear. Assumptions employed in the 1992 PA ignore certain factors and should be questioned for conservatism and sensitivity, e.g., representation of porosity changes during decreasing gas pressure; possible differential closure among rooms in a panel; spatial variation in pore pressure and gas generation rate; brine phase in SANCHO; gas escape in SANCHO; constant gas generation rate SANCHO; creep closure after intrusion (1992 PA, v. 4, 4-15 through 4-20).

Response

The use of a fully coupled mechanical and multi-phase fluid flow model is not now technically feasible because, (1) no such general model currently exists for material undergoing time-dependent deformation (creep) and, (2) the computational effort required by such a model would preclude its use in probabilistic PA calculations where many hundreds or thousands of simulations are required. (Such a model could be used to verify isolated cases).

Recent verification studies of the PA implementation of the porosity surface indicate satisfactory prediction of pressure and porosity during creep consolidation (Arguello and Stone, 1994a; Arguello and Stone, 1994b; Arguello and Stone, 1993). The implementation is verified under conditions of undisturbed repository, human intrusion, and high brine saturation cases. This suggests the resources required to develop and use a fully coupled model is not warranted at this time.

References

Arguello, J.G. and C.M. Stone. 1994a. *Corrections to Errata in Memo Entitled: "Performance Assessment Verification Calculations - Revised SANCHO Calculation for Comparison with BRAGFLO Run #63."* Memo. Albuquerque, NM: Sandia National Laboratories.

Arguello, J.G. and C.M. Stone. 1994b. *Performance Assessment Verification Calculations - Revised SANCHO Calculation for Comparison with BRAGFLO Run #63.* Memo. Albuquerque, NM: Sandia National Laboratories.

Arguello, J.G. and C.M. Stone. 1993. *Performance Assessment Verification Calculations - SANCHO Comparisons with BRAGFLO Runs 18, 42, and 63.* Memo. Albuquerque, NM: Sandia National Laboratories.

Comment AG-9a

M

Page 9

Modeling of brine and gas flow in BRAGFLO raises the following questions:

It has been pointed out by EPA (comments at 2/22-25 DOE-EPA meetings) that BRAGFLO assumes a rigid isothermal rock body with no non-Darcy flow and omits consideration of colloids and particulates. Hydrologic properties are symmetrical. Further, whether Darcy's law can be expanded to the continuum modeled by BRAGFLO is not known. In light of these comments, how does DOE propose to justify its conceptual models and support the rejection of alterative conceptual models?

Response

BRAGFLO doesn't assume a rigid rock body in the sense that rock compressibility effects are rigorously modeled in all materials, and the effect of creep consolidation on repository porosity is accounted for. BRAGFLO does assume isothermal Darcy-flow. Data collected at WIPP are analyzed and have been found to fit Darcy-flow models.

The redistribution model and clay consolidation model are also supported by data collected at WIPP. An activity will be planned and included in the SPM to evaluate the advantages of selecting any but the more conservative of these models.

Comment AG-9b

Page 9

Alternative conceptual model uncertainty with respect to relative permeability and capillary pressure is dealt with via a sampling technique (1992 PA, v. 4, 4-24). The supporting Sandia memorandum states that the choice of conceptual models "could be significant" (Webb, at A-149). What approach will be taken better to resolve the uncertainty?

Response

Until sufficient data are collected to establish the two-phase flow parameters for WIPP materials, the uncertainty will continue to be captured by sampling both model description and parameter values over a wide range as in the past. Capillary pressures have been measured for anhydrite in the laboratory. Additional capillary pressure and relative permeability measurements on anhydrite are planned.

Comment AG-9c

The disturbed performance model is scaled to match the initial excavated volume of a single panel. Such a model assumes effective panel seals. In what way will this assumption be tested in future PA's (see 1992 PA, v. 4, 5-1)?

Response

Current PA modeling capability allows consideration of intrusion into a panel separated from the rest of the repository volume by a panel seal. The effectiveness of panel seals in preventing flow and transport between panels can be examined in this model by varying seal permeability.

Comment AG-9d

Page 10

The disturbed performance model extends to the Culebra only. Will more recent strata be incorporated in subsequent PA's?

Response

Yes. The stratigraphy now used in the BRAGFLO model includes a simplified representation of the Rustler and the Dewey Lake.

Comment AG-9e

Page 10

The initial brine saturation of the waste is sampled within the range of 0.0 to 0.14 (1992 PA, v. 4, 5-13; v.5, 2-9). The PA reports that the 1991 sampling range extended from 0.0 to 0.276, the maximum being the residual saturation that the waste could contain and still comply with transportation requirements. (1992 PA, v. 5, 2-8). For the 1992 PA the sampling range has been restricted due to "numerical constraints imposed by the creep closure model that was implemented by 1992" (1992 PA, v. 5, 2-8). Please explain the numerical constraints. What efforts are under way to replace the arbitrary range with a more accurate figure?

Response

The numerical constraints arose from the small time steps needed to correspond to the short time constant required for fluid compression. The 1991 and 1992 PA analyses used artificially high waste moisture content values to test the PA codes for their sensitivity to waste water content. See the reply to Comment AG-4b for a discussion of recent waste moisture content data.

Comment AG-9f

Page 10

Sampling methods admittedly sample correlated variables independently. Will a method be adopted to refine sampling procedures in this respect?

Response

We now sample correlated variables. This was done in the 1991 and 1992 PAs for some variables. The techniques for implementing correlations are available. The problem is obtaining a good characterization of what their correlations should be.

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Comment AG-9g

Page 10

The 1992 PA observes, concerning gas and brine migration, that contaminated brine must displace all brine-saturated pore volume in a grid block before it can move to the next grid block (1992 PA, v. 5, 4-14), and that if some of the pore volume is occupied by gas, travel distances must be increased proportionately (1992 PA, v. 5, 4-15). What is being done with regard to this prospect?

Response

Such estimates of brine migration as done in 1992 and reported in 1992 PA, Volume 5, Chapter 4 are no longer necessary. A new model, "NUTS" will calculate the transport of radionuclides or dissolved constituents in the brine phase directly within the Salado formation. This is a much more rigorous and accurate method for determining how far contaminated brine may migrate.

Comment AG-10

Page 10

The permeability of borehole fill is a sensitive parameter with respect to radiation releases (1992 PA, v. 4, 5-36). PA assumes that initially the drillers place casing and cement and sand plugs (1992 PA, v. 3, 4-4). PA further assumes that the plug conforms to OCD orders, specifying a solid cement plug through the Salado (id.). The figure (1992 PA, v. 3, Fig. 4.2-1) does not depict such plugs. The PA discussion does not indicate the sensitivity of initial borehole plug permeability, and this should be discussed and the assumptions justified, if significant. Also, do the characteristics of silty sand (1992 PA, v. 3, 4-6) reproduce those of a degraded concrete plug?

Response

The 1992 PA made the assumption that degradation of borehole plugs was instantaneous. No assumptions were made about the initial properties of borehole plugs, and performance estimates do not reflect any contribution such plugs might make in delaying or reducing releases.

Quantitative data are not available about the long-term properties of degraded concrete borehole plugs. The use of silty sand as an analog for degraded concrete is a qualitative estimate. However, it should be noted that silty sand is also an appropriate analog to use for material that might fill an open hole, based on the EPA guidance in Appendix C of 40 CFR 191 that analyses should consider a "flowpath with a permeability typical of a borehole filled by the soil or gravel that would normally settle into an open hole over time..." Examination of the stratigraphic section above the repository (see Holt and Powers, 1990) indicates that if a hole in the salt were to stay open for any period of time, silty sand provides an appropriate representation for what might settle into an open hole.

Reference

Holt, R.M., and D.W. Powers. 1990. *Geologic Mapping of the Air Intake Shaft at the Waste Isolation Pilot Plant.* DOE-WIPP 90-051. Carlsbad, NM: Westinghouse Electric Corporation.

Comment AG-11a

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Page 11

Castile brine reservoir:

Several parameters are sensitive in analyses of human intrusion scenarios:

Castile brine pressure is estimated based upon limited (i.e., WIPP-12) wellhead data; the range is derived from the 1989 Systems Analysis (SAND 89-0462), at 3-148, but the data underlying the derivation of the initial pressure base case and range are not set forth.

Response

The underlying data for Castile brine pressure are found in Popielak, R.S., R.L. Beauheim, S.R. Black, W.E. Coons, C.T. Ellingson, and R.L. Olsen. 1983. *Brine Reservoirs in the Castile Formation, Waste Isolation Pilot Plant (WIPP) Project, Southeastern New Mexico.* TME-3153. Carlsbad, NM: U.S. Department of Energy. (WIPP Observation Data).

Comment AG-11b

Page 11

Bulk storativity is another reservoir attribute estimated from WIPP-12 data. Bulk storativity expresses the ratio of fluid discharged to pressure decrease. It seems correct to base estimates on data as to long-term pressure changes when modeling long-term groundwater releases. However, in modeling surface releases, should not short-term pressure changes be the basis for the estimates?

Response

Bulk storativity is a parameter that is constant over time. At early-time (short-term), fluid discharges are large and pressure decrease is rapid. At later-time (long-term), fluid discharges are smaller and pressure decrease is minimal. The bulk storativity determined from either short-term or long-term testing is the same and can therefore provide a basis for modeling surface releases, irrespective of duration.

Comment AG-12a

Page 11

Human intrusion probability estimates:

The estimation of the probability of human intrusion will be one of the most sensitive parts of the PA. Draft EPA compliance criteria propose estimation of a rate of intrusion based on historical data. If such process is adopted some of the following comments may be inapplicable:

Is it correct to express the frequency of intrusion as a random variable? The likelihood of an intrusion is affected by the information obtained in--and thus the occurrence of -- prior intrusions. The nonrandom nature of the second and successive holes may be important at WIPP, where the first hole is quite likely to intersect extractable resources. Is it not more appropriate to use a multilevel probability analysis?

Response

Assumptions about the independence of intrusion events are based primarily on the guidance contained in Appendix C of 40 CFR 191. Three points are relevant. "Inadvertent and intermittent intrusion by exploratory drilling for resources (other than any provided by the disposal system itself) can be the most severe intrusion scenario assumed." Passive institutional controls "can be effective in deterring systematic or persistent exploitation" of the site. "[P]assive institutional controls or the intruders' own exploratory procedures are adequate for the intruders to soon detect, or be warned of, the incompatibility of the area with their activities."

The approach to determining drilling probabilities will be reconsidered if appropriate when 40 CFR 194 is promulgated.

Comment AG-12b

Page 11

The probability of certain scenarios is affected by the fraction of the disposal area overlying Castile brine reservoirs. The PA casts doubt upon attempts to correlate reservoir data points (1992 PA, v. 3, 5-7) and to identify the stratigraphic location of brine reservoirs in the region (Id., 5-10, 5-11). What efforts are underway to narrow these uncertainties?

Response

The area-fraction of the panels underlain by Castile brine was treated in the 1991 and 1992 PAs as an uncertain parameter, and sampled in the Monte Carlo analyses. Sensitivity analyses in both PAs indicated that uncertainty in this parameter had little effect on overall performance (see Volume 4 of the 1991 PA, Table 6-1, and Volume 4 of the 1992 PA, Table 9-3). No activities are planned at this time to reduce uncertainty in the spatial extent of the brine reservoir.

Comment AG-12c

Page 11

There is a need for an iterative approach to expert judgment estimates of the probability of human intrusion. Performance of this task, like others, ought to improve with practice and refinement.

Response

Future states of society and the probability of intrusion are unknowable. Thus, there is no expectation of closing in on a specific value. While the process and practice of expert judgement may improve, there is no way to tell if the "answer" is any better.

Comment AG-12d

Page 11

Issue identification--the description of the intrusion scenarios to be considered by experts--can be carried out separately from estimation of probabilities. In such event the questions assigned to different groups must be stated with precision. We do not approve the approach of making overlapping assignments, as appears to have occurred as between the Futures Panel and the Markers Panel.

Response

Stephen C. Hora and Detlof von Winterfeldt, two of the four authors of the 1990 Bonano et al. report, were consultants to Sandia on the conduct of the Futures Panel and were the normative specialists for the elicitations. Stephen C. Hora was also part of the Markers Panel effort.

The effort undertaken with the Futures Panel and the Markers Panel was not to identify scenarios for modeling in a performance assessment, but to address alternative futures. SAND90-3063 test prepared by the authors intentionally does not use the term "scenarios." Text prepared by the teams sometimes uses the term "scenarios," since they did not realize the special usage of the term in performance assessment.

The issue statement for the Futures Panel (SAND90-3063, p. G-3) states:

> The future human intrusions team members are asked to address primarily the issues related to societal development and activities that could lead to inadvertent human intrusion in a time frame that extends 10,000 years after disposal. Other expert teams will address the issues related to marker and barrier development.

The Futures Panel was also asked to consider active controls and records systems (as aspects of societal development that could impact intrusion). In fact, they were specifically requested not to consider the impact of markers in considering the frequency of intrusions (p. G-4): "It should be assumed that markers or signs placed to deter human intrusion have vanished or are no longer effective."

The issue statement for the Markers Panel (SAND92-1382, p. A-3) stated: "...consider passive markers for deterring inadvertent human intrusion, defining characteristics for selecting and manufacturing markers to be placed at the WIPP, and judging the performance of these markers over a 10,000 year period."

The members of both panel were briefed on 40 CFR 191 and the requirement for markers. While markers were not in the charter of the Futures Panel, some panel members chose to make statements/recommendations about markers. DOE chose to publish all the material received by the teams and to not censor it.

References

Bonano, E.J., S.C. Hora, R.L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories.* SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-12e

Page 12

Selection of panel members: Bonano el al., Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories (1990) suggests that generalists, specialists, and normative (i.e., expert elicitation) experts be selected for each elicitation. Thus, there is a need to identify the applicable specialties with respect to each issue, a task which was done with inadequate care in the 1990 elicitation. For example, to consider a specific issue of hydrocarbon drilling practices, it seems necessary to consult an oil field geologist or similar industry expert, but this was not done in 1990.

Response

Stephen C. Hora and Detlof von Winterfeldt, two of the four authors of the 1990 Bonano et al. report, were consultants to Sandia on the conduct of the Futures Panel and were the normative specialists for the elicitations. Stephen C. Hora was also part of the of the Markers Panel effort.

The Expert Panel Selection Criteria for both the Futures Panel (SAND90-3063, p. K-3) and the Markers Panel (SAND92-1382, p. E-3) contained:

> (1) tangible evidence of expertise, (2) professional reputation, (30) availability and willingness to participate, (4) understanding of the general problem area, (5) impartiality, (6) lack of economic or personal stake in the potential findings, (7) balance among team members so that each team has the needed breadth of expertise, (8) physical proximity to other participants so that teams can work effectively, and (9) balance among all participants so that various constituent groups are represented."

The text and appendices of both SAND90-3063 (pp. 11-2 to 11-4, Appendices h, I, and J) and SAND92-1382 (pp. 2-4 to 2-6, Appendices B, C, and D) indicate the effort that was undertaken to fulfill the selection criteria. Individuals in a variety of disciplines from professional societies, technical journals, universities, and government agencies, as well as individuals from environmental and public interest groups were queried for nominations for both the Futures Panel and Markers Panel.

The materials that was presented to the Futures Panel, as well as the written material that they received, contained information about resource potential at the site. The reasoning behind the make-up of each team within the Futures panel was to encourage a broad consideration of possible future societies.

The consideration of potential future societies is important not only because of the need to consider the frequency of inadvertent human intrusion in performance assessments, but also for the benefit of thinking broadly when considering how to attempt to communicate with humans over the 10,000 year period of regulatory concern. The consideration of a broad range of motivations for and capabilities of human intrusion can be instructive as a base for setting "with whom" one may be trying to communicate. This was, in fact, one of the motivations for convening an expert panel to address future human intrusion. This broad thinking on human intrusion became input for the Markers Panel, charged with addressing the need for markers intended to communicate the dangers and location of the wastes.

This dual purpose for the Futures Panel opened up the discussion of long-term markers to address a broad range of possible future societies and modes of intrusion, while allowing the performance assessment process to focus on exploratory drilling, as EPA directed in the Guidance.

References

Bonano, E.J., S.C. Hora, R.L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories.* SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-12f

Page 12

Formation of judgments: Members may, in principle, be elicited singly, or in panels of members who perform parallel tasks, or as a team which separately perform parts of a single task. Bonano et al. point out the hazard that members may unconsciously be influenced by one another's judgment (at 42-43). Diversity in scientific backgrounds must be deliberately sought (at 15). Conflicts of interest must be carefully avoided (at 14). the tasks of team members must be clearly defined so that members are confined to their specialty (at 15).

Response

Stephen C. Hora and Detlof von Winterfeldt, two of the four authors of the 1990 Bonano et al. report, were consultants to Sandia on the conduct of the Futures Panel and were the normative specialists for the elicitations. Stephen C. Hora was also part of the Markers Panel effort.

(a) Influence

Bonano et. al. (1990, p. 3), in a discussion of the advantages of a formalized expert-judgment process, state:

> *Well-thought-Through Design for Elicitation*. The procedures that will be used in a formal expert-judgment process are designed specially for the problem being faced. The design relies on the knowledge of the problem domain to be studied. *Careful planning of the process can substantially reduce the likelihood of critical mistakes that will render information suspect or biased* [emphasis added]. Mistakes such as including experts with motivational biases, failing to document rationales, inadvertently influencing the experts' responses, failing to check for consistency, and allowing individuals to dominate group interactions *can be avoided* [emphasis added].

(b) Diversity, avoiding conflict of interest

The Expert Panel Selection Criteria for both the Futures Panel (SAND90-3063, p. K-3) and the Markers Panel (SAND92-1382, p. E-3) contained:

> (1) tangible evidence of expertise, (2) professional reputation, (3) availability and willingness to participate, (4) understanding of the general problem area, (5) impartiality, (6) lack of economic or personal stake in the potential findings, (7) balance among team members so that each team has the needed breadth of expertise, (8) physical proximity to other participants so that teams can work effectively, and (9) balance among all participants so that various constituent groups are represented.

The text and appendices of both SAND90-3063 (pp. 11-2 to 11-4, Appendices H, I, and J) and SAND92-1382 (pp. 2-4 to 2-6, Appendices B, C, and D) indicate the effort that was undertaken to fulfill the selection criteria. Individuals in a variety of disciplines from professional societies, technical journals, universities, and government agencies, as well as individuals from environmental and public interest groups were queried for nominations for both the Futures Panel and Markers panel.

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(c) Confining experts to their specialty

When dealing with such complex issues as future societies and long-term markers, the concern of confining experts to their specialty does not address the salient point.

For example, the Markers Panel was not asked to interpret hardness or durability data. The Markers Panel was asked to develop marker design criteria, which have materials and communication aspects, among others. Each member of the team brought to the deliberations the contributions of his or her discipline to the development of an effective marker system. Recommendations based on a narrow materials science perspective (use titanium for markers as it is highly resistant to corrosion) may be subject to other constraints (human beings will recycle materials from an unattended marker system for which it is cost effective) and may need to be modified (use only a small subset of buried markers made from titanium to make mining an uneconomical prospect).

The point is that the teams are interdisciplinary. The rationale for using teams of experts is that questions related to human intrusion and passive institutional controls cannot be fielded by an single discipline. The issues to be addressed demand this organization. A discussion of interdisciplinary teams is provided in Bonano et al. (1990), especially see p. 15:

> No expert teams are necessary if the results of expert judgments from individuals or panels are naturally packaged to integrate the analysis. However, at other times the natural package of information based on experts' judgments can only be acquired from an expert team comprised of specialists in related but synergistic disciplines.

The above quote indicates that the natures of the issue dictates whether the judgments from individual experts can be collected separately or whether experts must use the contributions from their disciplines "synergistically" to provide the necessary information.

<u>References</u>

Bonano, E.J., S.C. Hora, R.L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063, Albuquerque, NM: Sandia National Laboratories.

Comment AG-12g

Page 12

Training: Members should be trained in methods to induce an accurate estimate of probabilities and to reduce bias. (at 16-20). The methods include decomposition of an issue into several less complex problems, stating implicit estimates explicitly, and declaring all assumptions.

Response

Both SAND90-3063 (p. 1-3) and SAND92-1382 (p. 2-6) indicate that at the first meeting of each panel, training in the subject of expert judgment/probability assessment took place. Stephen C. Hora and Detlof von Winterfeldt conducted the training for the Futures Panel and Stephen C. Hora conducted the training for the Markers Panel.

The reports documenting the work of the Futures Panel (SAND90-3063) and the Markers Panel (SAND92-1382) show how the issues were decomposed. See also the response to comment AG/92PA-12h.

References

Bonano, E.J., S.C. Hora, R. L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C. D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgement on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-12h

Page 12

Decomposition of issues: The issue should be decomposed into a decision tree or event tree, including all factors deemed relevant by the members, that will guide the determination of probabilities. Disagreement as to the nature of the appropriate decomposition reflects an aspect of uncertainty.

Response

Bonano et al. (1990) list several types of decompositions for factual (not value) problems. They are fault trees, event trees, conditioning of possible events on known or hypothesized events, decision trees, and algorithmic decomposition (based on a mathematical function).

The Bonano report states:

> The previously described decompositions of factual and value problems are fairly formal in that they express the results as trees or functions. Decomposition can also be used less formally. The goal of a less formal procedure might be to promote deeper insight into the rationale for judgments and to enhance the interchange of beliefs and assumptions about the likely causes of studied events without formally encoding the decomposition. The decompositions might be in terms of causal or mitigating factors that are loosely related to the event or quantity of interest. In this form, decomposition enhances the experts' introspection and communication.

Thus, decomposition does not need to follow a particular rigid format. The Futures Panel and the Markers Panel teams each provide judgments based on decompositions of the issue statement. These decompositions are documented in SAND90-0363 and SAND92-1382.

References

Bonano, E.J., S.C. Hora, R. L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C. D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-12i

Page 12

Documentation of elicitation: The "normative expert" elicitator should document the specialist's judgments and any reasoning offered in support. The intuitive conclusion and any intermediate probability estimates should be recorded and compared and the specialist asked to reconcile any inconsistencies.

Response

The documents produced describing the Futures Panel effort (SAND90-3063) and the Markers Panel effort (SAND92-1382) "document the specialists judgments and any reasoning offered in support."

The expert judgment elicitation process and the judgments derived from it (including intermediate probabilities) are documented in SAND90-3063 and SAND92-1382. Support for the positions taken by the teams can be found in the reports prepared by each team.

The Preface of SAND90-3063 indicates that a draft of the report was sent to the Futures Panel members for their review. The Markers Panel members had two opportunities for review of the elicited material--immediately after the elicitation meeting and again prior to the publication of SAND92-1382 (Preface).

References

Bonano, E.J., S.C. Hora, R. L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C. D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M, S.C. Hora, and R. V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

M

Comment AG-12j

Page 12

Scenario selection: When the task involves scenario generation, the normative expert shall be required to employ methods of forward induction, i.e., construction of scenarios by creating a forward-looking "event tree", and backward induction, i.e., reasoning backward based on hypothesized performance or nonperformance of the repository and postulating causes.

Response

As stated in the response to comment AG92/PA-12d, the effort undertaken with the Futures Panel and the Markers Panel was not to identify scenarios for modeling in a performance assessment, but to address alternative futures. SAND90-3063 text prepared by the authors intentionally does not use the term "scenarios." Text prepared by the teams sometimes uses the term "scenarios," since they did not realize the special usage of the term in performance assessments.

Given that the task was not to generate scenarios, Bonano et al. (1990, p. 25) indicate that "value-driven event and scenario generation" and "analogy - or antinomy-driven event and scenario generation" are the two other techniques that can be used for "event and scenario" identification. They go on to state that:

"Any of these three techniques can be combined with various forms of interactions among experts...Furthermore, they can be substantially enhanced by involving individuals with very different perspectives regarding the repository...Since the purpose at this point is to assure comprehensiveness, any inputs that are novel and creative should be appreciated."

In addition, Bonano et al. indicate that:

"Forward and backward induction builds on the notion that scenarios are logical sequences of events linked through processes."

Bonano et al. (1990, pp. 29-30) list several types of decompositions for factual (not value) problems. They are fault trees, event trees, conditioning of possible events on known or hypothesized events, decision trees, and algorithmic decomposition (based on a mathematical function).

The Bonano report (p. 31) states:

"The previously described decompositions of factual and value problems are fairly formal in that they express the results as trees or functions. Decomposition can also be used less formally. The goal of a less formal procedure might be to promote deeper insight into the rationale for judgments and to enhance the interchange of beliefs and assumptions about the likely causes of studied events without formally encoding the decomposition. The decompositions might be in terms of causal or mitigating factors that are loosely related to the event or quantity of interest. In this form, decomposition enhances the experts' introspection and communication."

Thus, decomposition does not need to follow a particular rigid format. The Futures Panel and the Markers Panel teams each provided judgments based on decompositions of the issue statement. These decompositions are documented in SAND90-3063 and SAND92-1382. The alternative futures developed by the four teams of the Futures Panel followed a variety of techniques (including, but not limited to forward and backward induction) to develop this "novel and creative" input. This creativity was an essential part of broadening the thinking regarding human intrusion.

<u>References</u>

Bonano, E.J., S.C. Hora, R.L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories.* SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

M

<u>Comment</u> AG-12k

Page 13

Probability elicitation techniques: Since the probability issue is critical, criteria should direct the use of probability elicitation techniques to generate the probability estimates. These include fractile techniques, whereby members must estimate the .05, .05, and .95 probabilities, and interval techniques, whereby members must estimate probabilities at various magnitudes of the unknown value.

Response

It is inappropriate that "criteria should direct the use of probability elicitation techniques." As discussed below, the nature of the decompositions themselves indicates the techniques to be used.

Bonano et al. (1990, p. 33) state:

"Drawing on this literature, there appear to be four distinct classes of procedures, depending on the nature of the uncertain quantity (discrete events vs. continuous random variables) and the nature of the questions asked (magnitude judgments about events vs. indifference judgments about gambles). The resulting taxonomy is shown in Table 2.1...The eight techniques listed in this taxonomy [direct probability, direct odds, fractile technique, interval technique, reference gambles (for both discrete and continuous), and certainty equivalent (for both discrete and continuous)] are the most commonly used ones in the quantification of probability judgments."

Both types of information (discrete and continuous) were elicited from the teams, so the techniques used also varied. Information elicited from the Markers Panel was of the discrete form. Information elicited from the Futures Panel was of both the discrete and continuous forms. The types of information elicited are discussed in SAND90-3063 and SAND92-1382. Fractile and interval techniques are only applicable to continuous quantities.

References

Bonano, E.J., S.C. Hora, R.L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories.* SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C., D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M., S.C. Hora, and R.V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant.* SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-121

Page 13

Combinations: Bonano et al. point out that to average probability judgments of different members or teams addressing the same issue would be erroneous, because it would mask the range of uncertainty reflected by the different judgments (at 42-43, 47). In all situations the reported judgments should include the individual members' conclusions.

Response

The topics of future states of society and long-term markers are interdisciplinary ones. The perspectives of different disciplines must be incorporated to fully answer the questions asked. In these cases, the unit of analysis is the interdisciplinary team, so the judgments of the teams and the members of the teams are indicated. Regarding future states of society and long-term markers, the perspectives of individual team members, and thus the disciplines they represent, are equally important and are given equal weighting. An "average" is an analytical combination where all the experts have equal weighting. It is thus appropriate to average over the individual expert (discipline) judgments.

References

Bonano, E.J., S.C. Hora, R. L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C. D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M, S.C. Hora, and R. V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

Comment AG-12m

M

Page 13

Documentation: There should be a complete record of the elicitation process, including formulation of issues, selection of member, and each stage of the elicitation process. It is particularly important to subsequent application of estimates to make clear what factors were or were not considered in reaching a probability judgment (e.g., general knowledge of WIPP; effect of markers). Further, the reasoning giving rise to probability estimates and any support in other methods of probability estimation should be recorded. As stated, individual members' conclusions should be set forth. Members should be identified by name (at 44-45).

Response

SAND90-3063 and SAND92-1382 provide a record of the elicitation process. These reports also include the issue statements and discuss the decomposition of the issues. In addition, the appendices reproducing the individual and team perspectives discuss approaches and rationales.

The topics of future states of society and long-term markers are interdisciplinary ones. The perspectives of different disciplines must be incorporated to fully answer the questions asked. In these cases, the unit of analysis is the interdisciplinary team, so the judgments of the teams and the members of the teams are indicated. Regarding future states of society and long-term markers, the perspectives of individual team members, and thus the disciplines they represent, are equally important and given equal weighting. An "average" is an analytical combination where all the experts have equal weighting. It is thus not inappropriate to average over the individual expert (discipline) judgments.

References

Bonano, E.J., S.C. Hora, R. L. Keeney, and D. von Winterfeldt. 1990. *Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories*. SAND89-1821. Albuquerque, NM: Sandia National Laboratories.

Hora, S.C. D. von Winterfeldt, and K.M. Trauth. 1991. *Expert Judgment on Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND90-3063. Albuquerque, NM: Sandia National Laboratories.

Trauth, K.M, S.C. Hora, and R. V. Guzowski. 1993. *Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant*. SAND92-1382. Albuquerque, NM: Sandia National Laboratories.

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Comment AG-12n

Page 13

In the 1992 PA conversion from drilling rates to scenario probabilities is inadequately explained. Whether in the future separate futures panel "teams" will constitute a "range" to be sampled is not known. If this is done, it must be made clear what team-estimated conditions (e.g., state of technology) are chosen for sampling and why, and exactly which probability (or probability density function) is thereafter selected. Conversion to drilling intensity must be made explicit. We are concerned about the observations by EEG that the realizations in Appendix D of v. 3 are not the actual results of the program described in the Hora memorandum in Appendix A and that the probability of intrusion is reduced to zero after year 300 for the Boston Team (EEG 9/93 comments at 5). It also seems plain that the intrusion rate overall is zero after 2,000 years (1992 PA, v. 4, 2-19). This seems erroneous.

Response

Future documents will attempt to more clearly explain the conversion from drilling rates to scenario probabilities. Whether separate future panel "teams" will constitute a range to be sampled in the future will be impacted by the promulgation of 40 CFR 194. We agree that team-estimated conditions chosen for sampling and probabilities and the justification for such choices should be explicitly stated.

The program described in Dr. Hora's memorandum was a slightly earlier version than that used in the analysis. The difference in programs amounted to a simple bypass of the program statements concerning the array BOSTAB2. This was done because a decision was made not to include injection wells in the analysis at the time because the current performance assessment models include only exploratory boreholes, as required by 40 CFR 191. The Boston Team provided assessments for the drilling intensity that are conditional on both time and level of technology. The Boston Team concluded that exploratory drilling for hydrocarbons would not extend further than 300 years in the future and provided their own justification. Dr. Hora's program is a means to sample from the judgments of the Futures Panel and Markers Panel in addressing human intrusion and uses the judgments as developed by the teams. It is correct that for the purpose of calculating subsurface releases, the assumption that no intrusions occur after 2,000 years was made in the 1992 PA. Cuttings releases were calculated for the full interval from 100 to 10,000 years. The 1992 PA was preliminary, and results were not intended to be used as the basis for a compliance decision. The compliance application for WIPP will not be constrained in this manner.

Comment AG-13a

M

Page 13

Repository/shaft design:

Several questions arise concerning the engineered components of the disposal system:

Seal design and performance: Design standards call for MB 139 and other anhydrite layers to be sealed below and above each panel and drift with grout. (1992 PA, v. 2, 2-48). PA should provide a detailed description of the placement and composition of the grout.

Response

Effective placement of grout into Marker Bed 139 has been part of the technology development activities performed at the WIPP facility. Specifically, as indicated in the test plan for the Small-Scale Seal Performance Test-Series F (Ahrens, 1992), this underground test at the WIPP was "intended to demonstrate equipment and techniques for producing, injecting, and evaluating microfine cementitious grout." The grouting was completed in March, 1993, and the final report is currently being prepared.

Reference

Ahrens, Ernst H. 1992. *Test Plan - Sealing of the Disturbed Rock Zone (DRZ), Including Marker Bed 139 (MB139) and the Overlying Halite, Below the Repository Horizon, at the Waste Isolation Pilot Plant - Small-Scale Seal Performance Test - Series F.* Albuquerque, NM: Sandia National Laboratories.

Comment AG-13b

Page 13

Preconsolidated salt in seals and backfill is said to consolidate to a median permeability of 1 x 10^{-20} m^2 within 100 years. (1992 PA, v. 3, 3-14). Reference must be cited for this permeability figure. Sandia has said recently that permeability of 10^{-18} m^2 or less is necessary to retard gas flow. Experiments have been planned to support such figure but have not yet been conducted. See Nowak et al., Initial Reference Seal System Design: Waste Isolation Pilot Plant (SAND 90-0355), at 27.

Response

Numerous laboratory studies have been completed which document the permeability of compacted, crushed salt samples. These data are summarized in Figure 4 of Hansen et al. (1993). The relationship is shown between the permeability of the crushed salt samples and the relative/fractional density of the sample. This paper also references the numerous reports in which the data were first documented.

Reference

Hansen, F.D., G.D. Callahan, and L.L. Van Sambeek. 1993. *Reconsolidation of Salt as Applied to Permanent Seals for the Waste Isolation Pilot Plant*, 3rd Conference on the Mechanical Behavior of Salt, September 14-16, 1993, Ecole Polytechnique, 91129 Palaiseau, Cedex-France.

Comment AG-14a

Page 14

Culebra flow and transport model:

The following matters deserve attention as to the model of flow and transport in the Culebra:

PA uses past climates to limit projected future variability in precipitation. (1992 PA, v. 2, 2-27; v. 4, 6-11). It has been asserted by Professor Roger Y. Anderson (March 16, 1993 comments to EPA) that a broader range of variability then the range of Pleistocene variation is appropriate. Please comment.

Response

Please see the response to AG-1a. Prof. Anderson's comments to the EPA did not disagree with the approach taken in WIPP PAs to treating climate variability during the next 10,000 years. Rather, Prof. Anderson's purpose was to ask the EPA to consider extending the regulatory period to 100,000 years to allow consideration of a range of climate variability more representative of the entire Pleistocene. This would represent a significant change in present regulatory requirements.

M

Comment AG-14b

Page 14

Modeling assumes no vertical flow above the Salado (1992 PA, v. 2, 2-24). In light of the imprecise knowledge of recharge patterns (1992 PA, v. 2, 2-36, 2-38) the assumption of no vertical flow must be scrutinized (see 1992 PA, v. 4, 6-2). What is planned?

Response

The WIPP Project is currently investigating the effects of vertical flow and recharge that may vary in both space and time through the use of a three-dimensional regional groundwater-flow model. Results of this modeling program will be used to evaluate the adequacy of the confined, two-dimensional approximation used to model flow and transport in the Culebra for PA.

Comment AG-14c

Page 14

Hydraulic conductivity in the Culebra is insufficiently characterized, in that the variability of, and controls on variability of, fracture porosity are not known (1992 PA, v.2, 2-16, 2-19). What efforts are planned to improve the state of knowledge? Fracture porosity and spacing are sensitive parameters (1992 PA, v. 3, 2-79, 2-81).

Response

This comment implies that it is necessary to have knowledge of the distribution of fracture porosity in order to characterize hydraulic conductivity. It is correct that the hydraulic conductivity of the Culebra very much depends on the degree to which open fractures are present. However, it is not necessary to know the fracture porosity or spacing in order to use results of hydraulic tests (e.g., pumping tests, slug tests, and drill-stem tests) to calculate values of hydraulic conductivity. Although there is clearly uncertainty in the distribution of hydraulic conductivity, SNL currently believes that this distribution is sufficiently well characterized.

The 92 PA does point out that there is insufficient information about matrix and fracture porosities to map their spatial distribution (v. 2, p. 2-16). Fracture porosity and spacing, as noted in the comment, are sensitive parameters in dual-porosity transport calculations. Values of these parameters have been obtained from tracer tests.

Comment AG-14d

Page 14

The 1992 PA notes that the groundwater geochemistry of the Culebra is inconsistent with a north-south flow pattern (1992 PA, v. 2, 2-36). It is stated that as the groundwater flow model is developed and refined, the potential significance of uncertainty in the location and amount of future recharge will be re-evaluated. (1992 PA, v. 2, 2-38). What are the PA plans to resolve, or examine the significance of, this uncertainty?

M

Response

It should be emphasized that it is not certain that an inconsistency between the hydrochemical facies of Culebra waters and the inferred north-to-south flow direction actually exists. As discussed in the 1992 PA (Volume 2, p 2-36, 13-20), Lambert and Carter (1987), Lambert (1991), and Chapman (1988) have proposed hypotheses to explain the relationship between the hydrochemical facies and flow directions. Lambert and Carter favor a model in which the hydrochemical facies still reflect a past flow direction. This model considers vertical flow into the Culebra from overlying strata to be insignificant. In contrast, the model proposed by Chapman includes recharge from the surface in the region of facies zone B. Whether or not one of these models provides a satisfactory explanation of the hydrochemical facies is not resolved and it is not clear that the resolution of this issue would have a direct effect on demonstrating compliance.

PA is using a three-dimensional regional model to investigate the impact of spatially and temporally varying recharge on groundwater flow in the vicinity of WIPP. The model calculates recharge distributions and groundwater flow rates and directions given assumptions about the amount of moisture available to infiltrate to the water table. Results of this modeling program will be used to evaluate the adequacy of the confined, two-dimensional, approximating used to model flow and transport in the Culebra for the 1992 PA.

References

Chapman, J.B. 1988. *Chemical and Radiochemical Characteristics of Groundwater in the Culebra Dolomite, Southeastern New Mexico. EEG-39.* Santa Fe, NM: Environmental Evaluation Group, Environmental Improvement Division, Health and Environment Department, State of New Mexico.

Lambert, S.J. 1991. *"Isotopic Constraints on the Rustler and Dewey Lake Groundwater Systems," Hydrogeochemical Studies of the Rustler Formation and Related Rocks in the Waste Isolation Pilot Plant Area, Southeastern New Mexico.* Eds. M.D. Siegel, S.J. Lambert, and K.L. Robinson. SAND88-0196. Albuquerque, NM: Sandia National Laboratories.

Lambert, S.J., and Carter, 1987. *Uranium-Isotope Systematics in Groundwaters of the Rustler Formation, Northern Delaware Basin, Southeastern New Mexico. 1. Principles and Preliminary Results.* SAND87-0388. Albuquerque, NM: Sandia National Laboratories.

M

Comment AG-14e

Page 14

An agreement between DOE and the State requires that radionuclide retardation be demonstrated with experimental data (1992 PA, v. 4, 6-3). Further, PA notes that experimental data as to distribution factors "cannot be extrapolated directly to a complex natural system" (1992 PA, v. 2, 2-30). At the recent NAS WIPP subcommittee meeting it was specifically stated by one member that laboratory data as to distribution factors is no reliable guide to retardation in the field. What are DOE's plans to develop retardation data that both satisfies its agreement with the State and affords scientific reliability? Will tests on site with nonradioactive analogue tracers be conducted (see 1992 Pa, v. 2, 2-41)?

Response

Decisions about how to proceed with experimental plans to demonstrate retardation at laboratory and field scales will be based on outcomes of SPM analyses that include these activities. At present, SPM2 results that could provide the basis for such decisions are anticipated in the Spring 1995.

Comment AG-14f

Page 15

How will the effect on the Culebra flow field of injection of fluids from the repository via an intrusion wellbore be measured and modeled? (see 1992 PA, v. 4, 6-2).

Response

CAO has no plans at this time to measure directly the effects of the injection of repository brine into the Culebra through an intrusion borehole. Scoping analyses of such effects will be modeled using existing PA codes before a decision is made about including brine injection into full system analyses.

Comment AG-14g

Page 15

The PA states that there is insufficient information to characterize the vertical variability of flow within the Culebra (1992 PA, v. 4, 6-1, 6-2). Will this issue be explored further?

Also, is it appropriate to model the Culebra as a rock body of uniform thickness, and, if not, what improvements are planned? (1992 PA, v. 4, 6-4).

Response

PA calculations of flow and transport in the Culebra involve two basic steps. First a regional flow field of specific discharge is calculated. Specific discharge is the rate at which groundwater flows through a unit cross-sectional area of rock. Although specific discharge has the units of velocity, it does not represent the actual velocity of flow. The actual velocity (interstitial velocity) is faster because fluid can only flow through the area provided by the pores and fractures in the rock, rather than the entire cross-sectional area. Assuming no vertical variability and uniform thickness for the Culebra when calculating the field of specific discharge does not introduce any error because these calculations are based on values of transmissivity, a parameter that integrates the hydraulic conductivity of the unit over its entire thickness. These assumptions do affect the calculated actual velocities. Observations at the Air Intake Shaft (Holt and Powers, 1990) indicate that, at this location, most of the flow in the Culebra occurs through about one quarter of its thickness. Assuming that flow occurs only through one quarter of the Culebra thickness results in a calculated velocity four times greater than would be calculated assuming that flow is over the entire thickness, if the same porosity were assumed for each effective thickness. However, the variation of velocity due to the sampled range of fracture porosity is several orders of magnitude. Therefore, there is no current plan to collect data to better characterize the vertical heterogeneity of the Culebra. Although it would be possible to represent the actual thickness of the Culebra in flow and transport models, the change in calculated velocity would be small compared to uncertainty in fracture parameters.

Reference

Holt, R.M. and D.W. Powers, 1990. *Geologic Mapping of the Air Intake Shaft at the Waste Isolation Pilot Plant.* DOE/WIPP 90-051.

Comment AG-14h

Page 15

The radionuclide transport model is a obviously sensitive parameter (1992 PA, v. 4, 6-3, ch. 8). How will DOE select among the alternatives for the next PA?

Response

Future iterations of the SPM will use the transport model that is considered to be consistent with currently available information. Alternative models (e.g., those that take credit for sorption and/or matrix diffusion) will be examined through consideration of activity sets that may be undertaken if SPM analyses indicate that their completion will lead to an acceptable probability of demonstrating regulatory compliance.

Comment AG-14i

M

Page 15

The flow and transport model assumes an intrusion over the center of the disposal area. However, the future borehole may be 315 m closer to the subsurface boundary. (1992 PA v. 4, 6-4, 6-7). Will the parameter of borehole location be changed or sampled in future analysis?

Response

Current modeling capabilities permit varying the location of the intrusion borehole. Decisions about the specific analyses for the SPM or for a compliance application will be made after baseline information for the SPM has been determined.

Comment AG-14j

Page 15

The Culebra flow model includes 70 transmissivity fields, which are calibrated to steady-state and transient head data. The exercise generated inconsistencies between the modeled and observed conditions. (1992 PA, v. 4, 6-26). It would seem that the model requires refinement based on data concerning factors, not now incorporated, affecting the observed head data. Is such an effort planned?

Response

The problem indicated in 1992 PA, Volume 4, pg. 6-26 relates to the RMSE of calculated heads at wells H-7 and USGS-1 measured well water levels being 2 to 4.3 meters lower in realizations than the observed well water levels. The comment concerns the effect of boundary conditions at the southern portion of the model domain on the estimation of the transmissivity field within the WIPP site boundary. It is probable that the boundary conditions assumed at the *model boundary* are not of great importance in determining the T-field geostatics *within the WIPP site boundary*. The rationale for this statement is the comparison that was made in papers by Rubin and Dagan (1988, 1989) in which a Green's function approach was used to analyze the effects of a constant head boundary and impervious boundary conditions on the head variogram and cross-covariance of head and lnk. The results of this study showed little difference between these covariances and the covariances generated for an aquifer with infinite boundaries when the boundary is located a few correlation scales from the area of interest. Assuming an exponential covariance for lnT, the correlation length for the Culebra is around 3km. The SW model boundary is at least 12km from the WIPP boundary so the effect of boundary conditions at this boundary are probably not significant. Finally, since solute transport trends to be in a southerly direction in the WIPP Culebra release scenarios, the effect of lower than measured heads at the southern boundary of the land withdrawal area will tend to increase the NS gradient, and to increase transport out of WIPP. Thus this error is conservative (tending to increase releases). In any case, the sensitivity of the T-field inside the WIPP boundary to bounding sets of boundary conditions should be examined when the next set of T-field calculations are made for completeness.

A re-calculation of the transmissivity field using transmissivity and head data that has been measures since the 1992 PA will be conducted for the final compliance document. Re-calculation of transmissivity fields for the SPM2 may take place if there is sufficient time in the process. The Geostatistical Expert Group (GxG) has conducted a series of blind test problems to determine the adequacy of the pilot point method as well as a number of other inverse methods in estimating the transmissivity field of a number of WIPP-like test problems. The results of this exercise will be available in April 1995. From these results a decision will be made as to whether GRASP-INV or another inverse method is used for future generation of transmissivity field realizations. The code used in calculation of the final transmissivity calculation will be required to be a level A Quality Assurance.

<u>References</u>

Rubin, Y. and G. Dagan, 1988. *Stochastic Analysis of Boundaries Effects on head spatial Variability in Heterogeneous Aquifers*, 1, Constant Head Boundary, Water Res. Res., 24(10), 1689-1797, 1988.

Rubin, Y. and G. Dagan, 1989, *Stochastic Analysis of Boundary Effects on Head Spatial Variability in Heterogeneous Aquifers*, 2, Impervious Boundary, Water Res. Res., 25(4), 707-712, 1989.

M

<u>Comment</u> AG-14k

Page 15

In the SECO-TRANSPORT model vertical fractures are not incorporated. Should the model be improved in this respect?

Response

The SECO-TRANSPORT is two-dimensional; it contains only lateral directions and therefore does not explicitly include the orientation of fractures. However, the existence of vertical fractures is included in the conceptual model of the Culebra and their affect on flow and transport is fully included in the current version of the model. Therefore, there is no need to change this code in any way to account for vertical fractures.

Comment AG-15a

Page 15

M

Cuttings model:

The direct release model -- now confined to releases through interaction of drill bit and string and the repository contents -- raises the following questions:

The drill bit diameter is a sensitive parameter under the current PA and is sampled using a cumulative distribution function based on past drilling practice in the Delaware Basin. (1992 PA, v. 4, 7-1). A principal exploration target is gas, and the bit diameter should approximate that used for gas exploration.

Response

As noted in a previous comment (see AG-1f), oil is also a major exploration target in the vicinity of the WIPP. The Project believes that it is appropriate to consider drill bit diameters representative of all drilling activities that reach the repository horizon in the vicinity.

Comment AG-15b

Page 15

The model for spallings releases is still being developed. (1992 PA, v. 2, 7-27). We caution against too rigid assumptions as to the practice which would be followed by a driller who encounters a pressurized zone.

Response

Drilling practices which may affect spalling releases have been examined during development of the spalling model. In part, characterization of these practices is dependent on interpretations of regulatory guidance.

Comment AG-15c

Page 15

Based on the Sandia presentation at the February 22-25 meeting between EPA and DOE, we believe that it cannot be assumed that intruders would soon detect the presence of the repository and discontinue activities. Please comment.

Response

The DOE's interpretation of the "soon detect" guidance provided by the EPA in Appendix C of 40 CFR 191 is given Appendix A of the March 1994 *Draft Compliance Status Report for the Waste Isolation Pilot Plant* (page A-11, entry 64):

> Performance assessment shall be based on the assumption "that passive institutional controls or the intruders' own exploratory procedures are adequate for the intruders to soon detect, or be warned of, the incompatibility of the area with their activities." "Soon detect" shall be defined based on reasonable interpretations of drilling practices and expert judgement. "Soon detect" cannot be strictly interpreted based on current drilling practices since currently, drillers do not worry about encountering underground waste repositories during their operations. Once such repositories are in place, exploratory drillers may become aware of their existence in areas where they are looking for resources.

Comment AG-16a

M

Page 16

Compliance demonstration:

There are certain fundamental issues with the methods of demonstrating compliance which are not addressed:

How are the variables which are to be sampled selected from among all others? For example, why is the future waste inventory not sampled as to one or more variables?

Response

Parameters were selected for sampling in the 1992 and previous PAs based on several factors, including: degree of uncertainty about the correct values to use in analysis; analyst judgement about the potential affect on outcomes of this uncertainty; information from previous sensitivity analyses; and significant changes in information about the parameter since previous analyses. Because the 1992 PA and previous PAs were preliminary, and were not intended for use in a compliance application, the total number of sampled parameters was restricted to those believed to be most important. Experience indicates that the 1992 list of sampled parameters did include those to which the 1992 models were most sensitive. The selection of parameters to be used in support of a compliance application will be made when models and data for the application have been determined.

With respect to future waste inventory, parameters characterizing the components that participate in gas generation reactions (i.e., iron, cellulosics, and water) were sampled in the 1992 PA. The radionuclide inventory was not sampled, in part because there was insufficient information available to characterize the uncertainty, and in part because performance is relatively insensitive to the total number of curies emplaced in the system.

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Comment AG-16b

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How is the sufficiency of sampling methods (e.g., range, frequency) evaluated? By what criteria does the occurrence of one or more "outlier" curves dictate a revision in sampling methods?

Response

In general, the occurrence of an outline curve does not dictate a revision in sampling methods. We used two sampling methods in the 1991 and 1992 WIPP PAs: Importance sampling to incorporate stochatic (i.e., aleatory) uncertainty into the analyses and Latin Hypercube Sampling (LHS) to incorporate subjective (i.e., epistemic) uncertainty into the analyses. The robustness of LHS has been investigated previously (e.g., Iman and Helton, 1988; Iman and Helton, 1991). Further, we have conducted comparisons between the use of importance sampling procedures and Monte Carlo procedures for CCDF generation, with both procedures giving similar results. Also, the stability of the importance of sampling procedures is investigated by Helton and Iuzzolino (1993).

References

Iman, R.L. and J.C. Helton, 1998. *An Investigation of Uncertainty and Sensitivity Analysis Techniques for Computer Models.* Risk Analysis. Volume 8, 71-90.

Iman, R.L. and J.C. Helton, 1991. *The Repeatability of Uncertainty and Sensitivity Analyses for Complex Probabilistic risk Assessments.* Risk Analysis. Volume 11, 591-606.

Helton, J.C. and H.J. Iuzzolino, 1993. *Construction of Complementary Cumulative Distribution Functions for Comparison with the EPA Release Limits for Radioactive Waste Disposal.* Reliability Engineering System Safety. Volume 40, no. 3, 277-930.

Comment AG-16c

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What is the suggested criterion for "reasonable expectation" with respect to the CCDF realizations? What principles, if any, support the selection of mean, median, or a stated percentile?

Response

The DOE's interpretation of "reasonable expectation" is given in Appendix A of the March 1994 *Draft Compliance Status Report for the Waste Isolation Pilot Plant* (p. A-3, entry 15):

> The "reasonable expectation" standard is equivalent to the "reasonable degree of certainty" standard of 40 CFR §268.6. It is a burden of proof greater than the "preponderance of evidence" standard, but not as great as the "beyond a reasonable doubt" standard. The standard requires that reasonably trustworthy information and data be provided such that the totality of the facts and circumstances are sufficient, given the DOE's or the EPA's scientific and technical expertise, to warrant a "firm belief" that performance measures will be met. Quantification of these terms is not appropriate. Assessment of compliance to a "reasonable expectation" or a "reasonable degree of certainty" is therefore, a qualitative assessment of the quantitative results.

Criteria for compliance with 40 CFR §191.13 will be provided by the EPA in 40 CFR 194.

In practice, the final assessment of compliance will always be qualitative as it involves the assessment of many types of information from many sources. However, when a distribution of CCDFs is derived from subjective (i.e., epistemic) uncertainty, it is the location of these CCDFs relative to the boundary line in 40 §CFR 191.13 that is important. Thus, on a formal basis, a summary curve is the logical quantity to consider. However, there is no way to justify any particular curve (e.g., mean or 75%, 90%, 99%...). This is a value judgment on the part of the relevant regulator.

Comment AG-16d

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Why is 4/3 the number of variables a sufficient number of vectors to demonstrate the full range of variability in input parameters? Is another number required to generate a relevant mean, median or other percentile? (see 1992 PA, v. 1, 4-14).

Response

The rule derives from the sample size necessary for the numerical implementation of the Iman/Conover restricted pairing technique.

The robustness of Latin Hypercube Sampling has been investigated in several studies (e.g., Iman and Helton, 1988; Iman and Helton, 1991) and has been found to provide a high degree of repeatability with a relatively small sample size (e.g., 2-3 times the number of variables under consideration. However, a procedure for defining an "optimum" sample size is not known. Iman (1982) has presented a procedure using replicated sampling to place confidence intervals on quantities estimated with Latin Hypercube Sampling.

References

Iman, R.L. and J.C. Helton, 1998. *An Investigation of Uncertainty and Sensitivity Analysis Techniques for Computer Models.* Risk Analysis. Volume 8, 71-90.

Iman, R.L. and J.C. Helton, 1991. *The Repeatability of Uncertainty and Sensitivity Analyses for Complex Probabilistic risk Assessments.* Risk Analysis. Volume 11, 591-606.

Iman, R.L. 1982. *Statistical Methods for Including Uncertainties Associated with Geologic Isolation of Radioactive Waste Which Allow for a Comparison with Licensing Criteria.* In Proceedings of the Symposium on Uncertainties Associated with the Regulation of the Geologic Disposal of High Level Radioactive Waste, Gatlinbximum intrusion rate and a consideration of the effect of uncertainties in scenario probabilities arry, Oak Ridge, TN, pp. 145-57.

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Comment ED/92PA-1

The NMED comment contains extensive discussion of data available on the occurrences of water in the Dewey Lake Redbeds in WIPP-area wells.

The main concerns raised in the comment are summarized in the following two quotes:

"NMED/WIPP staff have concluded that the size and continuity of the Dewey Lake perched ground water zone are not adequately characterized."

"NMED/WIPP staff believe that the...hydrologic investigation documented in the Dewey Lake at H-14 was a test failure, and not an indicator of hydraulic properties of water-bearing zones in
the formation."

Response

Site investigations prior to sinking the shafts showed that the Dewey Lake Formation does not contain enough water to sample in the vicinity of the shafts at the WIPP Site (ref. Bechtel borings, No migration Petitions). This was later confirmed during sinking of the shafts (Holt and Powers 1984, 1986 and 1990) and by the drilling of a number of near-by test wells.

Although there was some moisture that seeped into the Air Intake Shaft (AIS) during construction, most came from just below the fill, four feet below the top of the shaft. The underlying Gatuna Formation (4-13 ft below the surface) was described as locally moist with the exposed surface locally exhibited a NaCl-rich efflorescent crust (Holt and Powers, 1990, Figure 5, Sheet 3 of 22). Moisture and salt crusts were also noted at the base of the Santa Rosa Formation, above the contact with the Dewey Lake (Holt and Powers, 1990, Figure 5, Sheet 3 of 22). The mapping reported fractures in the upper few feet (to a depth of 45 ft below the surface) that were moist and exhibited efflorescence (Holt and Powers, 1990, Figure 5, Sheet 4 of 22). Minor amounts of moisture and salt efflorescence were noted on the shaft surface at approximately 112 ft, 152 ft, 157 ft, and 164 ft. No other moisture was noted on the Dewey Lake surfaces exposed in the shaft at the time of mapping. The contact with the upper Rustler Formation is about 513 ft below the surface.

This water in the Gatuna, Santa Rosa, and upper Dewey Lake was saline and formed salt encrustation as it evaporated in the shaft. Holt and Powers (1990, section 3.4.4) point out that this must have originated as water that came in contact with the WIPP salt storage pile. The source must be very local and the water probably seeped in at the retention pond that is west of the salt pile and north of the parking lot.

Data from 10 test wells drilled between the WIPP shafts and the local ranch water wells confirm that the Dewey Lake is dry and that there is no reason to suspect that there is any potential pathway to connect the shafts to the ranch water supply.

DOE feels that the hydrology of the Dewey Lake is well characterized in the vicinity of the WIPP shafts, and that there is no reasonable potential for the Dewey Lake to provide a release pathway.

Throughout the southwest, where there are surface sand dunes and water is scarce, ranchers typically drill for water at the edges of the dune fields. The dunes collect infiltrating rainwater and funnel that into local, usually perched, water tables in whatever is below the sand. Such water supplies are usually local, subject to rapid depletion, and are easily contaminated. Two wells at the nearby Mills Ranch typify this practice.

No pump testing has been performed at either the Barn or the House wells. These are private ranch wells, not DOE test wells. Additionally, there is no record of how the wells were completed. The DOE did collect water at the Barn well as part of the Water Quality test Program. This water tested high in nitrates (WIPP-DOE 92-007, Section 5.1), logically a product of the animal excrement in the corrals. Test holes drilled between the WIPP shafts and these wells showed that there was no reason to suspect any potential for hydrologic connection.

It is true that the Dewey Lake in the vicinity of the ranch wells is not well characterized. However, the conditions at the ranch are obviously different from and not applicable to those in the vicinity of the WIPP shafts. Since there is no reasonable pathway for release to those wells, the condition at the ranch does not need further characterization. Additionally, since perched, local, water-bearing zones are known to be limited and easily disturbed, the risk of disturbing the local rancher's water supply by DOE pumping test suggest that such tests should not be done unless required by the local ranchers, in which case he would acknowledge that damage to his water supply could occur from the testing that he was requesting.

As of October 13, 1994, WQSP holes 1, 2, 4, 5, and 6 have been drilled. No significant moisture in the Dewey Lake formation was encountered in holes 1, 2, 4, and 5. Fresh water was encountered in WQSP6 (section 29, T22S, R31E, 1667 FSL and 1329 FWL) at a depth of 182-208 feet. Water was produced at a rate of approximately 30 gallons per minute for 15 minutes. This well was cased through the Dewey Lake as per the drilling plan. A second well has been drilled at this site as a test well. The well was completed in the water bearing part of the Dewey Lake Formation, from 180 to 210 feet. For characterization purposes, the well was cored from 160 to 220 feet. The DOE will perform additional testing at this location.

It is important to point out, however, that in the vicinity of the shafts, adequate characterization of the Dewey Lake exists and is valid. There is no reasonable mechanism for a pathway to exist from the WIPP shafts to the WQSP-6 location.

The purpose, nature, and conclusions of the tests performed at H-14 should be restated. First, the purpose of the tests was to try to determine if a water table exists in the lower Dewey Lake Redbeds immediately above the Rustler Formation. The test was never intended to address possible perched water in the Dewey Lake. Second, the tests performed were a drillstem test and a pressure-pulse test. No pump test was performed. Third, the conclusions of the tests were simply that the presence or absence of a water table in the lower Dewey Lake could not be determined from the test data. No conclusions about the presence or absence of perched water at H-14 or any other location were drawn.

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Comment ED/92PA-2

Geohydrology of the Dewey Lake.

The Dewey Lake Formation is characterized as a deltaic sequence of alternating, thinly bedded siltstone and mudstone with lenticular interbeds of fine- to coarse-grained sandstone (Mercer, 1983; D'Appolonia 1982b). Mercer (1983) contends that ground water occurs perched or semi-perched in lenticular sands in the upper Dewey Lake. Abundant gypsum-filled veinlets and fractures observed in dry lower portions of the formation suggest several interpretations concerning ground water occurrence in the formation.

- Sulfate-rich water has circulated through the formation at some time in the past (Davies, 1989).
- Water undersaturated with respect to sulfate has not dissolved the gypsum precipitate in the lower unit, and
- Ground water in the formation would most likely occur above the gypsum-filled veinlet zone (in the upper Dewey Lake) (Holt and Powers, 1990).

The zone isolated for the H-14 Dewey Lake hydraulic test was within the lower portion of the formation characterized by gypsum-filled veinlets (SAND89-0202). The zone tested was 30 meters (100 feet) below the deepest onsite occurrences of Dewey Lake ground water.

Response

The DOE agrees with the statement in the bullets. Holt and Powers (1990, section 3.4.4) did note the change in fracture filling from partial, Calcite fillings in the upper part of the Dewey Lake to a depth of 164.5 ft where it changed to the denser, incremental fibrous gypsum filling below that level. They postulate that perched water tables in the Dewey Lake might be held up by the denser fillings below that level. This hypothesis has yet not been tested. However, data from the recently-completed wells at WQSP-6 and the planned testing at that location should help to determine if their idea has merit.

The DOE also agrees with the final comment concerning H-14 tests. If Holt and Powers are correct in their hypothesis, the presence of the gypsum-filled veinlets explain why that part of the Dewey Lake could not be tested. It was probably too impermeable.

Comment ED/92PA-3

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Extent of Dewey Lake Ground Water. Mercer (1983) provides a commonly quoted statement suggesting that occurrences of Dewey Lake perched ground water zones are localized and isolated. In fact, on a regional scale, this is probably true. However, the term localized is not well defined, and as demonstrated in the previous discussion, its "localized" nature does not preclude significant occurrence of the aquifer onsite. Furthermore, its localized and isolated character also does not preclude hydraulic connection with off-site zones. The only hydraulic conductivities reported for the Dewey Lake Formation are from packer-permeability tests conducted during the foundation investigation. Given the potential for testing the wrong zone within the Dewey Lake, and the limited areal extent of the foundation investigation, it seems unreasonable to extrapolate this information site wide.

Lambert (1992) has noted meteoric recharge at Ranch Well (Lambert 1992) and Holt and Powers (1990) have suggested recent meteoric rainwater may be responsible for occurrences of moist and wet units described down to 50 meters (165 feet) below the surface in the AIS. NMED/WIPP staff have also observed prodigious amounts of free water seeping in within the Dewey Lake Formation at the Exhaust Shaft. Video surveyors report increased inflow in the Dewey Lake during years of heavy precipitation. The potential for enhanced meteoric recharge in future wetter-cooler climates may increase the size and flow characteristics of the Dewey Lake water-bearing units.

Response

Please provide a reference and documentation from the NMED/WIPP staff supporting your statement that "prodigious amounts of free water" are flowing into the Exhaust Shaft from the Dewey Lake Formation. The DOE acknowledges that condensation occasionally occurs on the station A instrumentation, but there is no documentation that we know of that moisture is from the Dewey Lake Formation. That portion of the shaft has been lined and grouted, and the last direct inspection showed that the grouting was successful in sealing off the formations above the Salado.

Comment ED/92PA-4

Dewey Lake and Performance Assessment.

Because of its relatively high permeability and regional persistence, the Culebra member of the Rustler Formation has been the focus of migration pathway studies for the WIPP site Performance Assessment. The Dewey Lake Formation has been downplayed, evidently based on limited hydrologic information. Several arguments question the rationale for this decision:

the data reviewed indicate that the Dewey Lake Formation is a ground water resource offsite, although it's characterization as an underground source of drinking water is debatable;

- the areal distribution of the Dewey Lake Formation on-site is uncertain and does not preclude connection of onsite and offsite perched ground water zones; and
- information on water quality, transient recharge and flow characteristics of the Dewey Lake water-bearing zone (s) is nonexistent or not well documented.

Response

See response to ED/92PA-1. The DOE believes that the hydrologic characteristics of the Dewey Lake near the shafts have been adequately characterized. The formation does not yield water and there is no reasonable potential for the Dewey Lake to provide a release pathway.

First bullet: DOE agrees, see comment ED/92PA-1.

Second bullet: The aerial distribution of the Dewey Lake Formation is well known from numerous test borings. It is present and continuous everywhere beneath the site. At the WIPP site the Dewey Lake Redbeds are 476 ft thick and consists of interbedded reddish-brown fine sandstone, siltstone, mudstone, and claystone (Holt and Powers, 1988, section 3.4). The upper part of the unit is indurated and many fractures are filled with calcite. Below 164.5 ft in the AIS, the cement changed to fibrous gypsum, which indicates incremental growth (Holt and Powers, 1990, section 3.4.3). The unit is quite tight and although no hydrologic tests were performed, rocks of similar composition typically have very low permeabilities.

Third bullet: See response to ED/92PA-1. The Dewey Lake is so dry at the site that it has not been possible to collect water samples. Water from the Barn Well south of the site is quite high in nitrates, probably as a result of contamination from the livestock excrement.

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Comment ED/92PA-5

NMED/WIPP staff hope that this transmittal supports and augments the three NMED/WIPP conclusions quoted in Table 1. There are several practical approaches that might address this issue, none of which explicitly involve drilling new wells to respond to this concern:

- If new Sandia research or DOE/WIPP monitoring wells are planned, use air as a circulation media to permit detection of shallow ground water. If a monitoring well is proposed, complete a monitoring zone within the Dewey Lake interval where ground water flow is observed.
- Describe a conceptual hydrologic model of the Dewey Lake Formation, considering potential recharge in future climatic regimes.
- Include or preclude, based on available information, the potential for the Dewey Lake to act as a secondary migration pathway.

Response

New wells for both Westinghouse (WQSP-1 through 6) and Sandia (H-19 b1 through 7) are being drilled using air as the circulation medium. Neutron and television logging will be performed in these wells to try to identify saturated zones within the Dewey Lake. As of October 13, 1994, WQSP holes 1,2,4,5, and 6 have been drilled. No significant moisture in the Dewey Lake Formation was encountered in holes 1,2,4, and 5. Fresh water was encountered in WQSP 6 (Sec. 29, T22S, R31E, 1667 FSL and 1329 FWL) at a depth of 182-208 feet. Water was produced at a rate of approximately 30 gallons per minute for 15 minutes. This well was cased through the Dewey Lake as per the drilling plan. A second well will be drilled at this site and completed in the Dewey Lake to collect water samples and determine the extent of saturation.

Answers to the second and third bullets will come from the three-dimensional modeling program in progress.

Comment ED/92PA-10

The following comment is a ground water monitoring recommendation occurring in "Assessment of Off-Site Radioactivity Surveillance Systems at the Waste Isolation Pilot Plant" (NMED/WIPP 93-002):

> "Current Magenta observation wells are located outside potential contaminant migration pathways. Although the Magenta is not considered in formal risk analyses conducted in support of the Performance Assessment, a ground water monitoring system may require consideration of the potential for contamination of this water-bearing zone. Some investigators have suggested that leakage downward from the Magenta Formation may be a component of recharge for the Culebra water-bearing unit (Mercer, 1983; Seigel et al., 1991). The groundwater radiological baseline could be improved by sampling the Magenta water-bearing zone in the potential contaminant flow path and closer to the western boundary of Zone II (extent of underground excavation)."

This recommendation occurs within the context of environmental monitoring, in which all "potential pathways" are monitored. This is a main theme of EPA's RCRA Ground-Water Monitoring Technical Enforcement Guidance Document (TEGD). The comment seems pertinent to developing a contaminant migration model for PA analyses.

Response

Supra-Salado rock flow properties may be important to facility performance, therefore a strategy aimed at characterizing and measuring these properties over a relatively short time period may be beneficial.

Comment ED/92PA-11

A variety of human-initiated factors, including the quality of borehole completion and abandonment, are proposed as causes for a collapse feature located about 100 miles southeast of WIPP in Texas (Johnson, 1987). In that case, abandoned boreholes evidently provided a pathway through which shallow ground water infiltrated and dissolved Salado salt. This dissolution led to the formation of a cavity and collapse of overlying strata. The development of the War Wink Sink is described in the following article:

K. Johnson, 1987, "*Development of Wink Sink in west Texas due to salt dissolution and collapse*"; 2nd Multidisciplinary Conference on Sinkholes/Orlando; p. 127-136.

There are twenty-eight drill pads with 44 wells within the WIPP land-withdrawal area. Most WIPP-related wells are used for site characterization. Only two penetrate to great depth near the facility, including DOE-1 drilled to 4065 feet and ERDA-9 drilled to 2889 feet. Seven resource exploration boreholes are also located within the site boundary, including oil wells Badger Unit (Section 15) and Cotton Baby (Section 24). Abandoned and sealed during the 1970's, only Badger Unit and Cotton Baby penetrate the entire evaporite sequence. Although there are important differences between the WIPP site location and War Wink Sink, substantial similarities warrant consideration of borehole-induced solution subsidence as a potential PA scenario process. The 1992 preliminary performance assessment considers only collapse of the repository opening and shafts as a human-induced subsidence process.

Response

The DOE agrees with the observations and fails to see any reason for concern. The situation at the WIPP is far different than that of the Wink Sink. For further explanations, please refer to the response to ED/92PA-12.

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Comment ED/92PA-12

Supply of Water Undersaturated with Respect to Soluble Evaporates Present at Depth. At the Wink Sink, Dewey Lake and Santa Rosa water bearing zones are proposed as sources of undersaturated ground water. Other potential sources include ponds holding oil field brine and possibly fresh water used as circulation media. Ground water undersaturated with respect to salt also exists at the WIPP site in the Dewey Lake Formation. Whereas the War Wink Sink is an example of accelerated subsidence on a short-time frame, the subsidence/dissolution issue at the WIPP is a much longer-term concern. An increase of 4 or 5 inches/year in average annual precipitation possible during the next 10,000 years may provide an increased source of undersaturated ground water. An increase in precipitation increases the possibility for meteoric water to infiltrate pathways created by boreholes. At present, long-term climatic variability is incorporated only into projections for ground water recharge to the Culebra.

Obviously soluble evaporates exist at the WIPP site and are susceptible to dissolution. The potential for natural deep-dissolution has been addressed in great detail and characterized as physically unreasonable in the 1992 PA analysis. A review of the PA analyses and Sandia reports, however, suggests that artificial causes of solution-subsidence have not been considered. Because they are relevant to borehole-induced subsidence, conclusions regarding natural solution-subsidence are summarized:

- Breccia chimney formation, a natural solution-subsidence structure, is associated exclusively with the Capitan aquifer (Snyder and Gard, 1982). Such structures are believed to result from the collapse of overlying strata into cavities formed in the

Capitan Limestone. The Capitan aquifer does not underlie the WIPP site and there are no examples of breccia features in the vicinity of the WIPP. Consequently, there is only a remote probability of a breccia chimney forming under the facility site.

- Ochoan evaporite deposits in the WIPP area and the northern Delaware Basin have undergone blanket dissolution. Along the western margin of the basin west of the WIPP, dissolution breccia indicates massive removal of the Salado Formation. Within two miles west of the WIPP site is Nash Draw, a dissolution feature cased by removal of the Salado and soluble beds of the Rustler Formation. Assuming the process of dissolution is an advancing front moving toward the WIPP site, there is general agreement that the rate of movement is slow enough to provide an adequate safety factor.

- Anderson (1981) proposed that deep-seated dissolution of Ochoan evaporates has not only occurred over the Capitan aquifer, but also within the basin where selective dissolution in the lower Salado has undercut overlying salt beds. Eight boreholes drilled near the WIPP facility, however, did not show any evidence of extensive dissolution predicted Anderson's model. There is general agreement that this proposed process of natural dissolution will not affect the WIPP repository.

Response

The DOE agrees with these comments and fails to see any reason that natural solution-subsidence is a realistic concern. The situation at the WIPP is far different than that at the site of the Wink Sink.

Bullet 1. The DOE agrees. Breccia chimneys have been discussed in detail elsewhere, and since the WIPP is not over the Capitan Reef, there is no reasonable expectation that such a feature will form at the WIPP.

Bullet 2. The DOE agrees.

Bullet 3. The DOE agrees.

An additional point is that at Wink Sink, there is an overlying fresh-water aquifer in the Triassic Santa Rosa Sandstone that can supply copious amounts of fresh-water to an improperly completed or abandoned drillhole. An oil well drilled in 1928 is located in the middle of the Wink Sink. It was abandoned in 1964, at which time 600 feet of casing was removed. The sink subsequently collapsed in 1980. (Ref.: Baumgardner, B. W., Jr., 1988) Although other scenarios have been proposed, it seems most likely that fresh-water from the Santa Rosa Sandstone flowed down into the salt through the open, abandoned, drill hole and dissolved the salt to form a large chamber which subsequently collapsed.

No such aquifer exist at the WIPP. Additionally, the fact that the brine pocket in the Castile are maintained at pressures higher than those in the Bell Canyon show that there is no naturally-existing connection between the units in the vicinity of the WIPP which might allow alternative "density current" models proposed to explain Wink Sink to operate.

The DOE also does not feel that artificial causes of solution subsidence are reasonable concerns. There is the potential for small amounts of fluid flow to occur if a man-made connection were made between the Bell Canyon, Salado, and/or the Culebra (Lappin, 1988, Section 2), but this would require a deliberately-created open hole or improperly abandoned hole. For solution to occur in the halite beneath the repository, such a drillhole would have to be through or in the immediate vicinity of the waste storage panels. No such drillholes exist today. The closest well that penetrates the entire evaporate sequence is Badger unit in Section 15, is approximately 2 miles from the WIPP storage panels.

Comment ED/92PA-13

Outlet (Sink) for Dissolved Salts.

At the Wink Sink, the Salado is underlain by the Tansill, Yates and the Capitan sequence, formations that contain numerous vugs and solution cavities for brine to migrate. Fractured, degraded cement and corroded iron well casing are believed to have provided a migration pathway for dissolved salts to reach the permeable and porous pre-Salado strata. The WIPP site is located 17 km (10 miles) from the subsurface extent of the Capitan Reef. Similar to arguments against breccia pipe formation, this suggests a low potential for borehole-induced solution-subsidence. However, conditions for a sink, or outlet, for dissolved solutes may exist under the WIPP site. Zones of porosity are created in the Bell Canyon Formation during primary withdrawal of oil, and could be enhanced by future hydrofracing or secondary/tertiary oil recovery activities. Lateral migration through pre-existing anhydrite dissolution features in the Castile or Salado also provide a possible sink for the brine.

Response

The concern that downward flow into the Bell Canyon Formation could cause catastrophic collapse as occurred at Wink Sink does not seem to be reasonable. There is no source of copious amounts of fresh water above the Salado, as there was at Wink Sink. There is a potential for small amounts of fluid flow to occur if a man-made connection were made between the Bell Canyon, Salado, and/or the Culebra, but this would require a deliberately-created open hole or improperly abandoned hole. For dissolution to occur in the halite beneath the waste storage panels and create the potential for collapse that might breach the repository, such as a drill hole would have to be through the immediate vicinity of the waste storage panels. No such drillholes exist today. The fact that the brine pockets in the Castile are maintained at pressures higher than those in the Bell Canyon show that there is no naturally-existing connection between the units in the vicinity of the WIPP. The fact also shows that there is also no lateral pathway for fluid flow in the Castile.

Reference

Lappin, A. R., 1988, *Summary of Site-Characterization Studies Conducted From 1983 Through 1987 at the Waste Isolation Pilot Plant (WIPP) Site, Southeastern New Mexico*. SAND88-0157. Albuquerque, NM: Sandia National Laboratories.

Comment ED/92PA-14

Energy-Hydrostatic Head or Density Gradient.

A mechanism is required to drive shallow freshwater downward through the salt and the resulting brine into underlying pre-Salado strata. At the Wink Sink, the hydraulic head of the Santa Rosa Formation provides such a mechanism. Hydraulic heads of the Tansill, Yates and Capitan Formation are all lower than the Santa Rosa, suggesting downward flow would be initiated if the shallow and deep aquifers were connected. To determine whether a similar condition exists at the WIPP, Dewey Lake and Bell Canyon hydraulic heads and solution density within the boreholes would need to be considered. Brinster (1991) cites Lappin (1988) as concluding that vertical flow would be downward if the Culebra and Bell Canyon were connected by a borehole. This conclusion is contrary to the vertical flow potential if one only considered a comparison of fresh-water equivalent heads.

Response

Also see response to ED/92PA-12

Vertical flow is controlled by actual hydraulic heads and fluid densities, not by equivalent freshwater heads. If density effects, including probable dissolution of halite that would occur in an open borehole, are taken into account, flow between the Rustler or higher formations and the Bell Canyon would be downward in an open borehole (Beauheim, 1986).

Reference

Beauheim, R.L., 1986. *Hydraulic-Test Interpretations for Well DOE-2 at the Waste Isolation Pilot Plant (WIPP) Site.* SAND86-1364. Albuquerque, NM: Sandia National Laboratories.

Comment ED/92-PA15

Conduits for Migration of Dissolved Solids.

Upward of 100 boreholes are completed inside or within the one-mile boundary of the WIPP Land withdrawal boundary, including 46 offsite oil and gas wells, 44 onsite WIPP-related boreholes and seven onsite resource exploration boreholes. Most were drilled, completed and plugged using standard oil field practices, including sulfate-resistant cement and corrodible iron-casing. Deep boreholes penetrating the Salado, Castile, or Bell Canyon formations would more likely impact the repository horizon. ERDA-9, DOE-1, Badger Unit and Cotton Baby satisfy such criteria. How these onsite boreholes will perform, and how the multitude of other production wells adjacent to the WIPP boundary will interact on a 10,000 year time frame are underlying issues. Over the long-term, the presence of these wells might influence, to some degree, the current geologic stability of the site.

Response

Possible groundwater flow and radionuclide transport through boreholes that do not directly intersect the waste-emplacement panels are retained for further consideration in the scenario development process.

Comment ED/92PA-16

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Extent of Possible Subsurface and Surface Disruption.

The surface expression of the Wink Sink measures 110 m (360 ft) in width and 34 m (110 ft) in depth. Presumably, dissolution occurred over the entire Salado stratigraphic sequence between 396 (1,300) to 655 m (2,150 ft) below the surface. However, the actual size of the cavity causing the collapse is not reported. Based on the size of the surface expression of the Wink Sink, it appears only ERDA-9 could impact the facility. In addition, no wells within roughly 1.5 miles of the facility penetrate the Bell Canyon Formation. This may constrain the size of the disturbed zone to the volume of brine accessible to pre-existing anhydrite dissolution features in the Castile or Salado. In this case, important parameters such as the cavity size and depth of the cavity would be controlled by the degree of interconnection between boreholes provided by dissolution features.

Response

ERDA-9 was drilled into the top of the Castile Formation at 2,889 feet. It has since been doubled cased, with 10 3/4 inch outer steel casing and, inside of that, a 7 in steel casing. Additionally, the lower 350 ft has been filled with cement, to a depth of about 2,540 ft. There is no source for copious amounts of freshwater to enter ERDA-9. In addition to the 350 ft of cement in the hole, the Castile Formation effectively prevents any downward flow from ERDA-9 to the Bell Canyon. The DOE feels there is no reasonable expectation that ERDA-9 could cause the development of a Wink Sink type feature.

Comment ED/92PA-17

Subsidence Potential: Oil Withdrawal

No discussion occurs in the 1992 preliminary performance assessment concerning the rationale for dismissing surface subsidence caused by withdrawal of oil and gas. The 1991 preliminary comparison, however, succinctly states an NMED concern:

> "Areas where oil and gas are withdrawn have the potential of surface subsidence in response to the removal of the confined fluid that supports some of the weight of the overburden" (SAND89-0893/1, page 4-37) [the reference should be to SAND91-0893/1].

There are several inaccurate statements on page 4-38 of SAND91-0893/1 that have precipitated NMED comment to the Performance Assessment Group on this issue:

> "Natural gas in the Morrow Formation was concluded to be the only possible hydrocarbon resource with economic potential in the area" (Keesey, 1976, 1979).

> "Because of the depth and rigidity of the possible production horizons, subsidence would not be expected to occur if gas (if present) was removed" (Brausch et al., 1982).

In regard to the potential for subsidence from oil and gas production activities, Brausch et al. 1982 (TME 3156) acknowledges that the "...withdrawal of hydrocarbons from reservoir horizons (from wells adjacent to WIPP boundary) could, theoretically, lead to subsidence at the WIPP site...(although) such subsidence is not typically associated with gas production" (TME 3156). As most wells along the WIPP boundary are oil wells producing from the Delaware Mountain Group, this argument is erroneous. Furthermore, considering that oil is currently a viable resource in the area, and specifically adjacent to the WIPP boundary, Keesey's reference seems outdated.

Having reviewed TME 3156 (Brausch et al., 1982) and other subsidence issues on other projects, NMED/WIPP staff believe that the case against subsidence is not adequately covered in TME 3156 or the 1991 performance assessment. There may be some validity for emphasizing the rigidity of the overlying strata as an argument against oil withdrawal subsidence; however, this suggestion is not referenced in TME 3156 (p. 52). TME 3156 analyzes subsidence events related to potash extraction activities insufficient detail, but provides only a brief paragraph on subsidence related to extraction of oil and gas. Additional analyses would require characterization of the area of potential withdrawal zones, the potential for consolidation of these zones, and the rheological properties, thickness and span of strata overlying hydrocarbon resource zones.

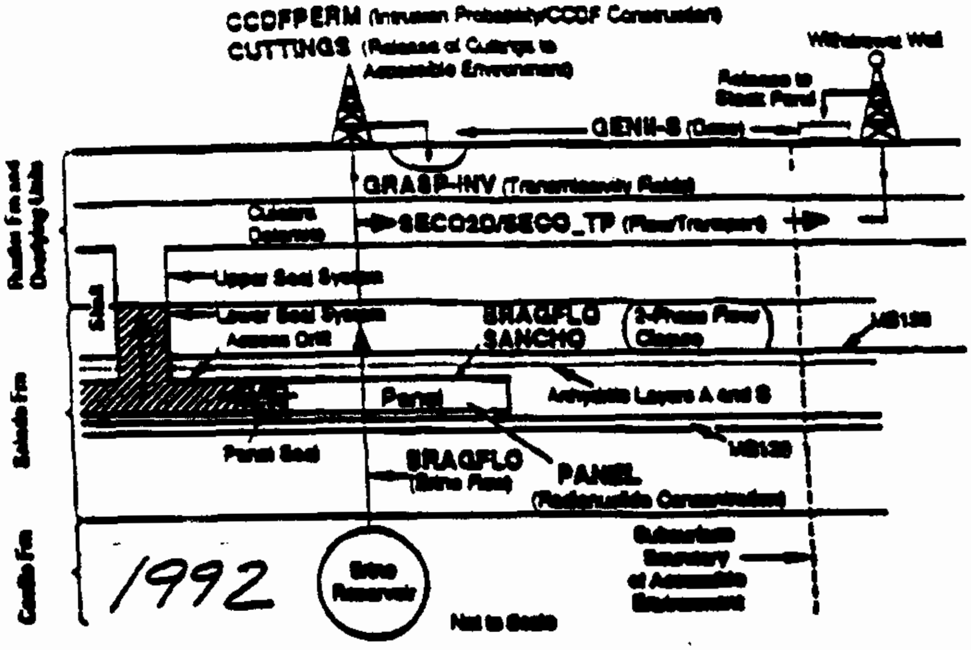
Response

The project acknowledges that screening arguments must be updated to reflect recent oil exploration activity in the WIPP area.

Subsidence related to oil and gas extraction will be considered in the scenario development process.

The DOE acknowledges that screening arguments must be updated to reflect recent oil development activity in the WIPP area.

Appendix A EEG Supportive Figures



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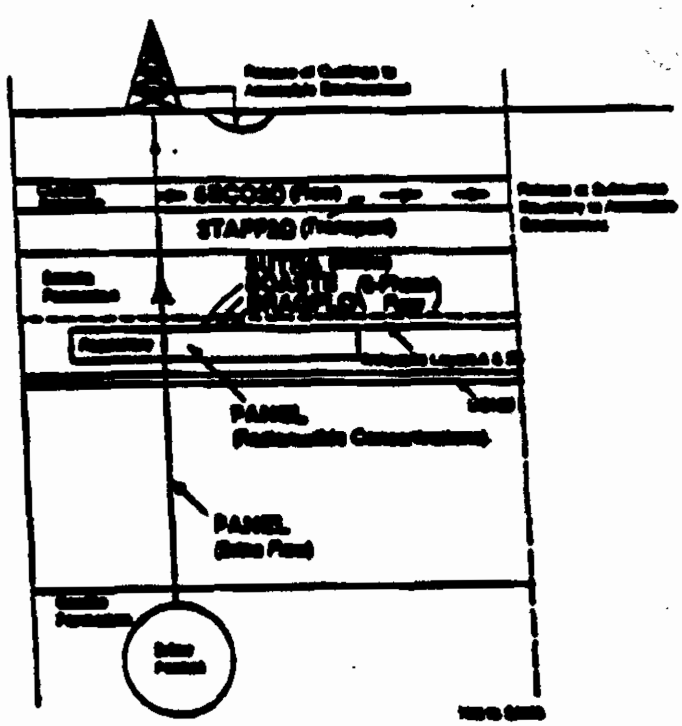

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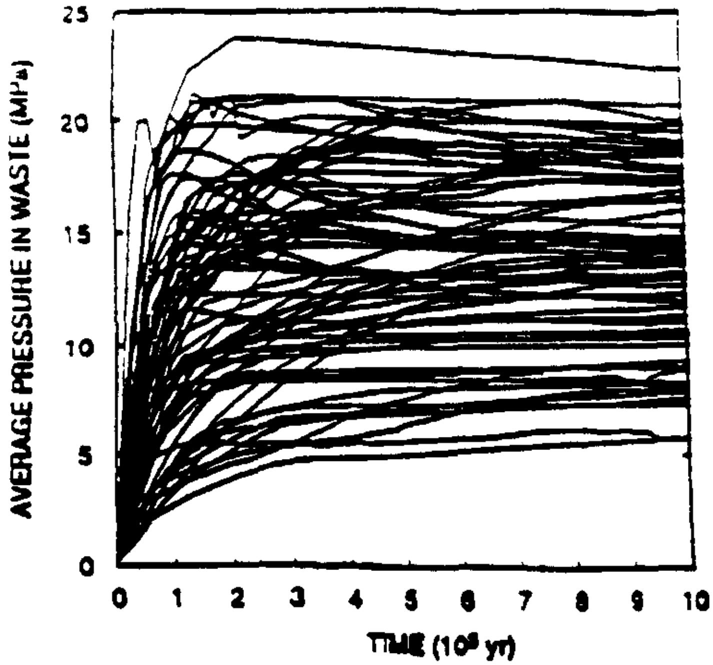
EEG Figure for Comment MI4



TRI-6342-2794-0

Figure 4-1. Volume Average Pressure in the Waste Repository.

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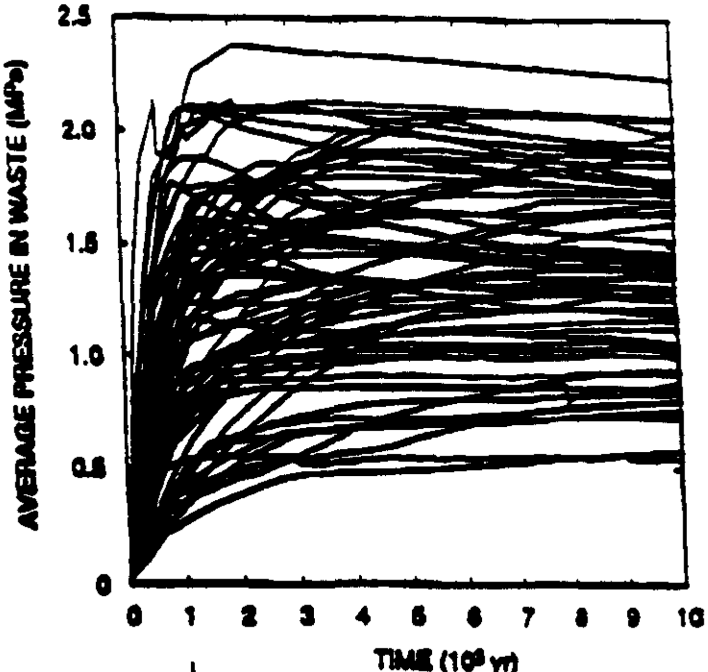

Figure 4-32.

[illegible]

Volume Average Pressure in the Waste Repository, Single Shaft Mo

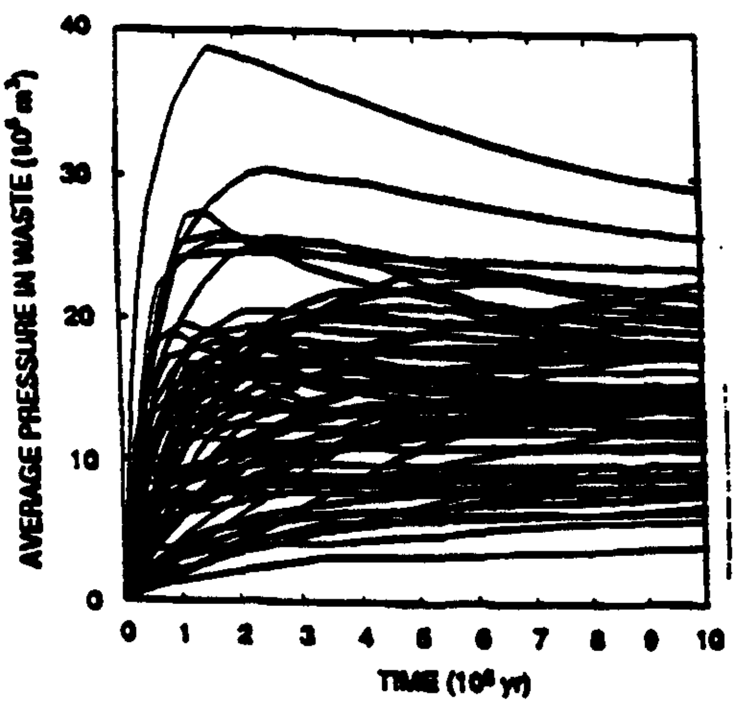

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Figure 4-39.

[illegible]

Volume Average Pressure in the Waste Repository, Fixed Waste Porosity Model.

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EEG Figure for Comment C-2

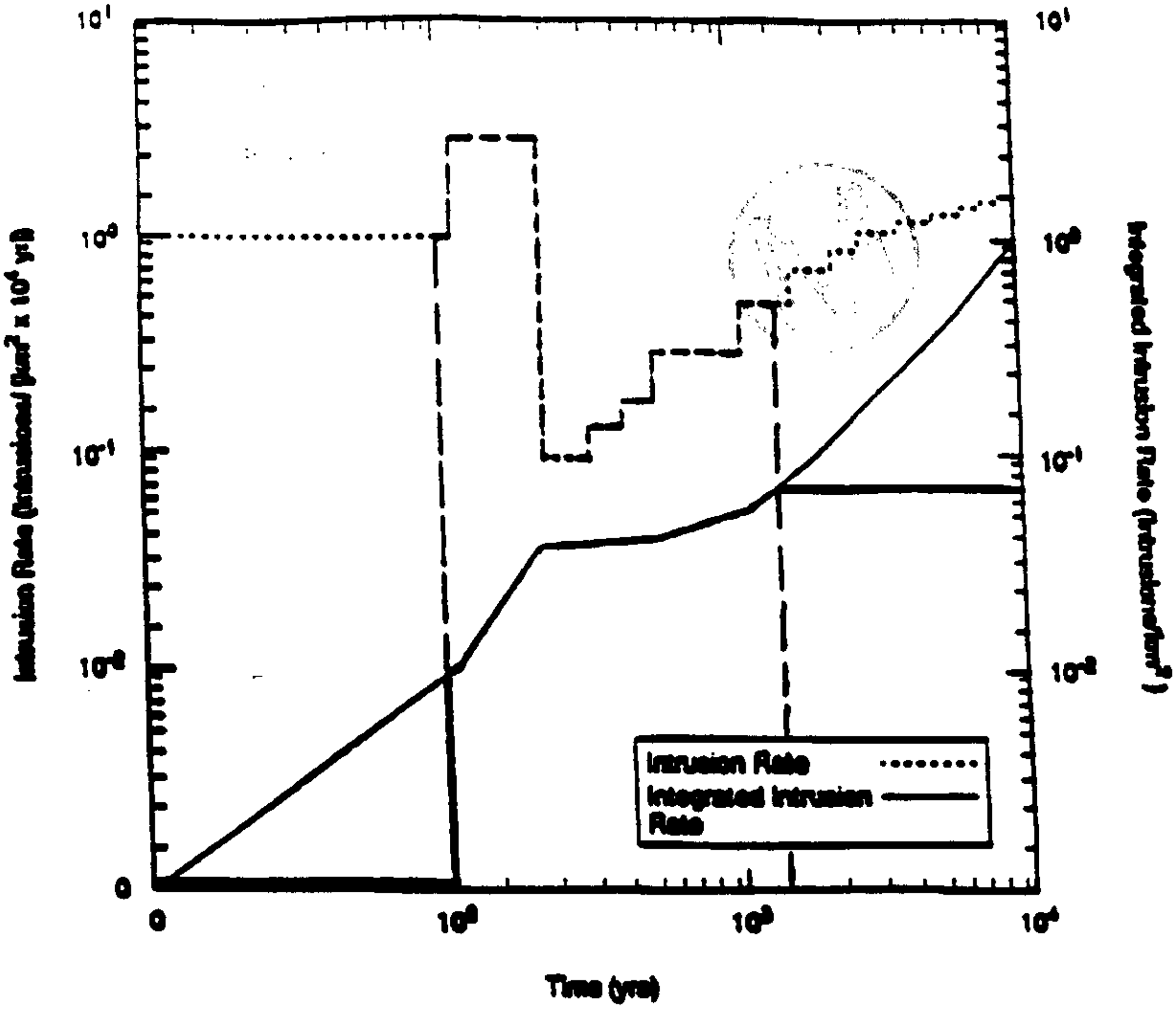

Figure 2.3-1. Example time-dependent rate term used in Poisson model for drilling intrusions in the 1992 WIPP performance assessment (Volume 3, Appendix B, Figure B-43). The rate $\lambda(t)$ as used in this chapter has units of yr^{-1} and is obtained by multiplying the rate indicated in this figure by 0.126 km^2 (i.e., the area of emplaced waste) and performing the indicated division by 10^4; further, $\lambda(t)$ is set to zero for the first 100 yrs when 100 yrs of administrative control is assumed. The rate $\lambda(t)$ was a sampled variable in the 1992 WIPP performance assessment; this figure shows the drilling rate with the largest integrated value (i.e., expected number of drilling intrusions) over 10,000 yr. In this and other similar figures, a hyperbolic sine transformation is used to generate the scales on the abscissa and ordinate; this transformation allows the plotting of zero, which is not possible when a logarithmic transformation is used.

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EEG Figure for Comment S-1

Table II. CASTILE BRINE RESERVOIR INTERACTIONS IN WIPP AREA

Name of Well	Date Drilled	Initial Flow bbl/day	Remedial Action
Mascho-1	1937	8,000	No action to stop flow.
Mascho-2	1938	3,000	No action to stop flow.
Culbertson-1	1945		3,000 barrels estimated to flow to surface. No record of flow rate or duration.
Tidewater	1962	NA	12 pound/gallon drilling mud did not stop. Finally controled by casing and cementing.
Shell	1964	20,000	Allowed to flow until artesian flow ceased.
Belco	1974	12,000	Brine flowed to surface for 26 hours with 14 pound/gallon drilling mud.
Gulf	1975	5,000	No records on total volume or duration of artesian flow.
ERDA-6	1975	660	WIPP hole. Estimates 19,000 barrels could be produced by artesian flow.
Pogo	1979	1,440	Initial flow was after 14.6 pound per gallon drilling mud had been added. Stopped after 4 days with 15 pound per gallon mud.
WIPP-12	1981	12,000	WIPP borehole. Over 79,000 barrels produced. Estimates 350,000 barrels producible by artesian flow.

Table II References

• U.S. Department of Energy, 1981a. *Brine Pocket Occurrences in the Castile Formation, southeastern New Mexico*, TME-3080.

• U.S. Department of Energy, 1983. *Brine Reservoirs in the Castile Formation, Southeastern New Mexico*, TME-3153.

• R. H. Neill *et al.*, 1983. *Evaluation of the Suitability of the WIPP Site.* EEG-23.

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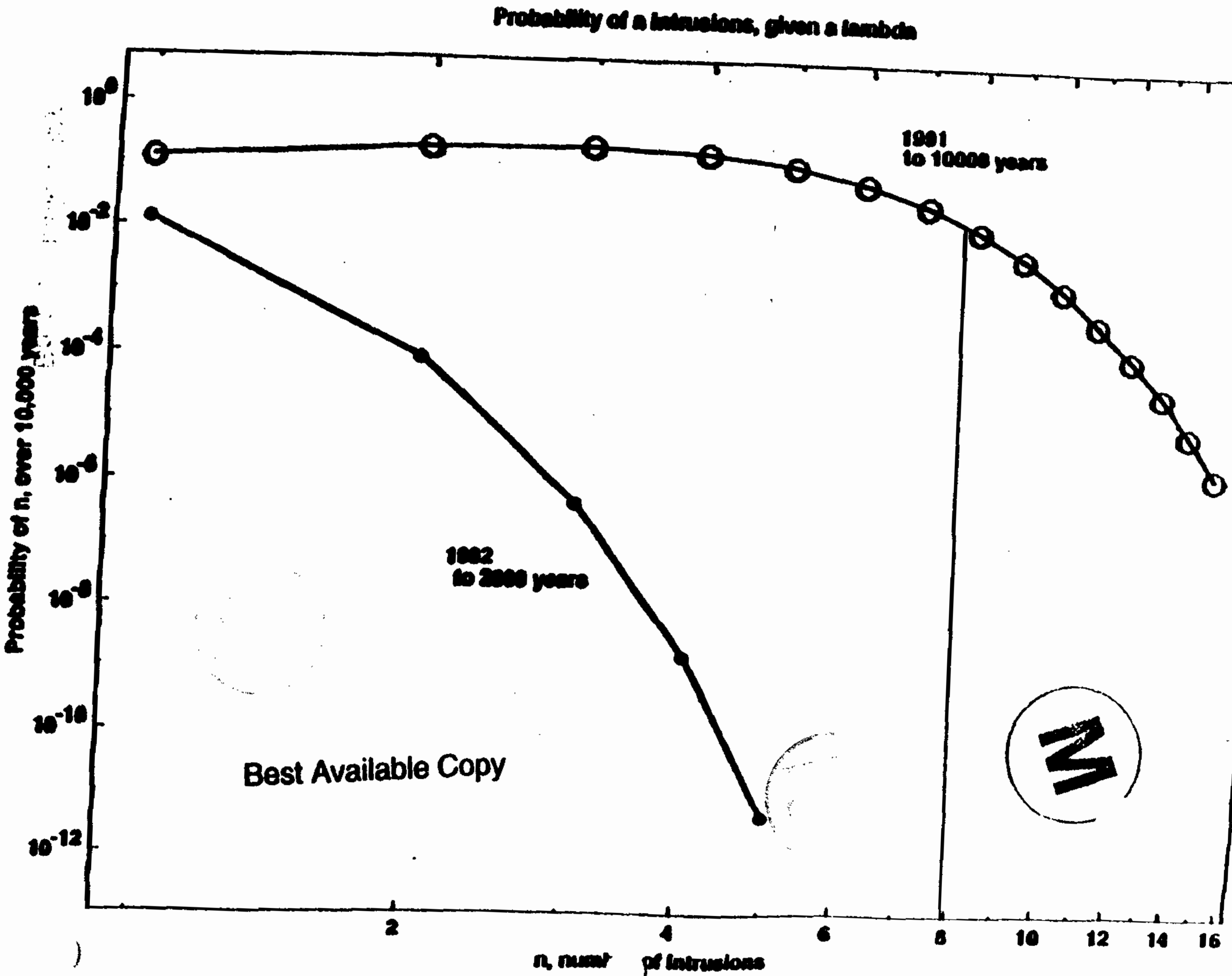

EIS Figure for Comment P-2 A-6

Appendix B NMAG Supportive Memos

NMAG Supportive Memos for AG-8b

Sandia National Laboratories

Albuquerque, New Mexico 87185

date: April 14, 1994

to: B. M. Butcher, 6345 (MS 1341)

J. G. Argüello C. M. Stone

from: J. Guadalupe Argüello and C. M. Stone, 1561

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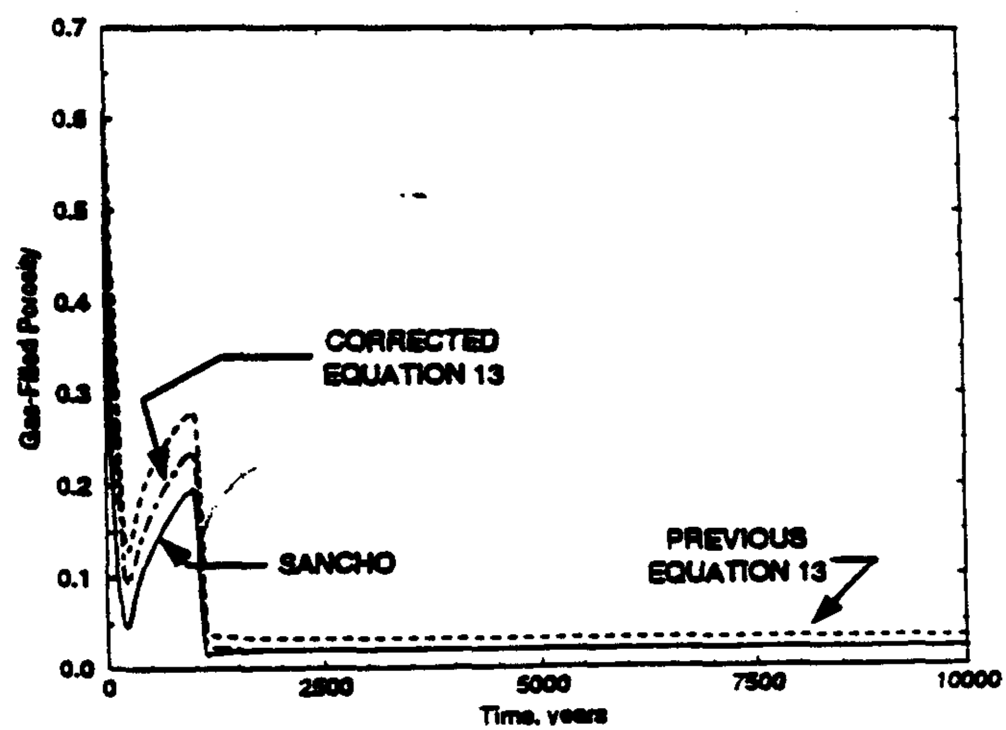
subject: Corrections to Errata in Memo Entitled: "Performance Assessment Verification Calculations - Revised SANCHO Calculation for Comparison With BRAGFLO Run #63"

As a result of a typographical error, Equation 13 of the subject memorandum [1] is incorrect. We are in complete agreement with Vaughn [2] that Equation 13 should have read as follows:

$$\varphi = \frac{\left[\frac{(1 - SBAVW)\,PORVOLW}{12.65}\right]}{\left[\frac{PORVOLW}{12.65} + 1229\right]}$$

Unfortunately, the error was propagated into the manipulation of the BRAGFLO results used to generate the curve labelled "BRAGFLO" in Figure 5 of the same memo. Consequently, the corrected "BRAGFLO" curve (along with the old one) is shown below.



As a result of the corrected manipulations of the BRAGFLO quantities, much better agreement of porosity predictions between BRAGFLO and SANCHO is apparent at later times. Nonetheless, the general conclusion of the memo [1], that there still remain differences between SANCHO and BRAGFLO, remains unchanged. For example, at the local maximum which occurs at about 1000 years, we see that the BRAGFLO porosity results are about 25 % greater than those from SANCHO. Furthermore, because the pressure predictions are unaffected by Equation 13, the difference in pressure predictions between the two codes, as originally discussed in Reference 1, remains as well.

We strongly support the suggestion [2] of a meeting where everyone can get a better understanding of what was done in both codes so that we can reach closure on the issue of the discrepancies between the results.

References

1. Argüello, J. G. and C. M. Stone, "Performance Assessment Verification Calculations – Revised SANCHO Calculation For Comparison With BRAGFLO Run #63," Memorandum to B. M. Butcher, 6345, Sandia National Laboratories, Albuquerque, New Mexico, January 20, 1994.

2. Vaughn, P., "Comments on Argüello and Stone Memo dtd 1/20/94 Concerning Verification of Porosity Surface Under High Brine Saturation Conditions," Memorandum to B. M. Butcher, 6345, Sandia National Laboratories, Albuquerque, New Mexico, March 18, 1994.

JGA:1561

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MS0443	1561	J. G. Argüello

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Sandia National Laboratories

Albuquerque, New Mexico 87185

date: January 20, 1994

to: B. M. Butcher, 6345 (MS1341)

from: J. Guadalupe Arguello and C. M. Stone, 1561

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subject: Performance Assessment Verification Calculations – Revised SANCHO Calculation For Comparison With BRAGFLO Run #63

Introduction

Performance assessment (PA) of the Waste Isolation Pilot Plant (WIPP) requires that numerous scenarios be investigated to determine if compliance is achievable. This leads to a very large number of analyses that need to be performed during the course of the investigations. Because of the large number of analyses required, the numerical models used by PA must be fast and efficient and consequently contain less detail, in terms of computed disposal room closure, than would be ideal. The reason for this is that the complexity of salt creep and the compute-intensive analyses required to determine its response make it prohibitive to directly calculate disposal room closure in PA analyses at this time. Yet, the effects of disposal room closure must be included in PA compliance analyses. Therefore, detailed disposal room creep closure calculations are typically performed using separate large, complex, nonlinear finite element based codes that are fast and efficient, in their own right, but on a different scale. The calculational results from disposal room creep closure analyses must then be recast in a form suitable for use in PA compliance analyses. The process is as follows:

1) First, a set of disposal room creep closure calculations using a finite element code (e. g., SANCHO or SANTOS) is performed for several discrete values of gas generation (leading to number of moles of gas versus time) and a set of corresponding room porosity histories (room porosity versus time) is obtained for those discrete values.

2) From those results, a "porosity" surface is defined that provides the porosity of a single WIPP disposal room as a function of the number of moles of gas present (or the pressure resulting from that amount of gas) and time.

3) This concept of a "porosity" surface is then used by the PA compliance analysis code (BRAGFLO). The results from the first step are used directly as input to the code; it then interpolates to determine the porosity (in a panel or the repository) as a function of time for any arbitrary gas generation (or pressure) path, accounting for the influence of brine saturation. In this manner, the effects of disposal room closure are indirectly accounted for in the PA calculation.

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The recasting of the finite element analysis disposal room creep closure results into a form suitable for input to PA requires a number of simplifying steps and assumptions, all of which have the potential of introducing uncertainty into the results.

A method for checking the validity of the procedure was previously examined at the request of the project [1]. The method began by recovering data on the variation in gas content and porosity with time in the waste/backfill region of a disposal room from a performance assessment compliance calculation, in which the effects of disposal room closure had been included indirectly via the concept of a "porosity surface"[2]. This surface was derived from the results of several earlier analyses [3] using the gas generation rates and other details defining the Baseline room model [4]. The gas-versus-time data from the compliance calculation was then used as an input gas generation history for a disposal room closure calculation using the SANCHO [5] code to compute the porosity of the room as a function of time. The porosity history obtained from SANCHO was then compared with the original porosity history recovered from the PA calculation. The difference between the two porosity histories represented translation error, and differences arising from the various assumptions and simplifications.

Three cases, designated as runs 18, 42, and 63, from PA's 1992 compliance analysis were investigated in that study [1]. Run #18 represented a human intrusion into a practically dry (low brine saturation) panel; Run #42 was for an undisturbed, practically dry, repository; and Run #63 represented a human intrusion into a panel that became largely brine saturated later in time. Satisfactory agreement was observed between the performance assessment porosity histories and the more detailed histories computed with SANCHO for repository environments with low brine saturations. Results differed, however, for the third case (Run #63) when saturation of the panel occurred. It was suspected that the discrepancy for the third case could be due to the assumption in the SANCHO calculations, that all void volume in the room is occupied by gas (this assumption had always been used, even in the original calculations used to generate the porosity surface), whereas the BRAGFLO calculations explicitly included brine saturation. The details of how this is done in the BRAGFLO calculations are not known to the authors; consequently, it is difficult to comment on how consistent the method of doing so is with the assumptions used in generating the original porosity surface. The purpose of the present investigation is to revisit this third case, adjusting the void volume this time to account for the presence of the brine. A SANCHO calculation has thus been performed in which the void volume in the room, at any time, is reduced by the volume of brine in the room at that time, as computed by BRAGFLO.

This memo thus serves to document the results of a disposal room closure analysis performed with SANCHO using gas-versus-time, *and brine-versus-time*, information from Run #63 of the performance assessment calculations previously performed with the BRAGFLO code. The PA Run #63 BRAGFLO case, and its corresponding computed gas history and brine volume history (used as input to the SANCHO calculations) are briefly described in the next section of the memo. The disposal room geomechanical model used for the SANCHO computation is then discussed. The results from the analysis are then presented, and the porosity histories obtained from SANCHO are compared to those from the original BRAGFLO runs. Finally, comments on the validity of using "porosity surfaces" in PA analyses are offered for consideration.

Description of PA Run #63 Case

The BRAGFLO results for Run #63 were transmitted to organization 1561 by B. M. Butcher, 6345 using data provided by Jim Bean, 6342. The transmitted information is included in tabular form in Appendix I. Once again, the results in Table I of Appendix I represent a human intrusion into a panel that becomes largely brine saturated.

The information from these tables used as input to the SANCHO calculation was that labelled "GASVOLW," "SBAVW," and "PORVOLW." As indicated in Appendix I, the information in the column labelled "GASVOLW" is the gas volume in the waste – at the corresponding time – in cubic meters at a pressure, P=101325 Pa, and at a temperature, T=300.15 °K. The information in the column labelled "SBAVW" is the average brine saturation in the waste, while that in the column labelled "PORVOLW" is the waste pore volume (gas and brine) in cubic meters computed with BRAGFLO. By using the Ideal Gas Law,

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$$PV = nRT, \qquad \text{(EQ 1)}$$

where P is the pressure in Pascals, V is the volume in cubic meters, R is the universal gas constant, and T is the temperature in degrees Kelvin, the number of moles of gas, n, with time was computed. This quantity of moles of gas was divided by 12.65 rooms per panel to get an "equivalent room" value for use in the SANCHO disposal room model. The information was written to a file that was subsequently read by SANCHO during the disposal room closure calculation. The resulting equivalent room gas content history that was written to the file is shown in Figure 1. By multiplying the quantity "SBAVW" by "PORVOLW," the brine volume with time was also computed. This quantity was again divided by 12.65 rooms per panel to get an equivalent room value and was written to a file for use in the SANCHO computation. The resulting equivalent room brine volume history that was written to the file is shown in Figure 2.

Geomechanical Model

A two-dimensional plane strain disposal room model, as shown in Figure 3, was used for the SANCHO analysis. This same model was used for the Baseline calculations described by Stone in his October 6, 1992 memorandum [3]. The model represents the room as one in an infinite array of disposal rooms located at the repository horizon. Consequently, symmetry boundary conditions are applicable between rooms. If it is further assumed that gravity forces do not greatly affect the material response near the room [6] and that the stratigraphy is all-salt, use can be made of vertical symmetry boundary conditions to arrive at the quarter-symmetry model shown in the figure. The model contains 618 elements and 664 nodal points. A zero-displacement boundary condition in the horizontal direction was

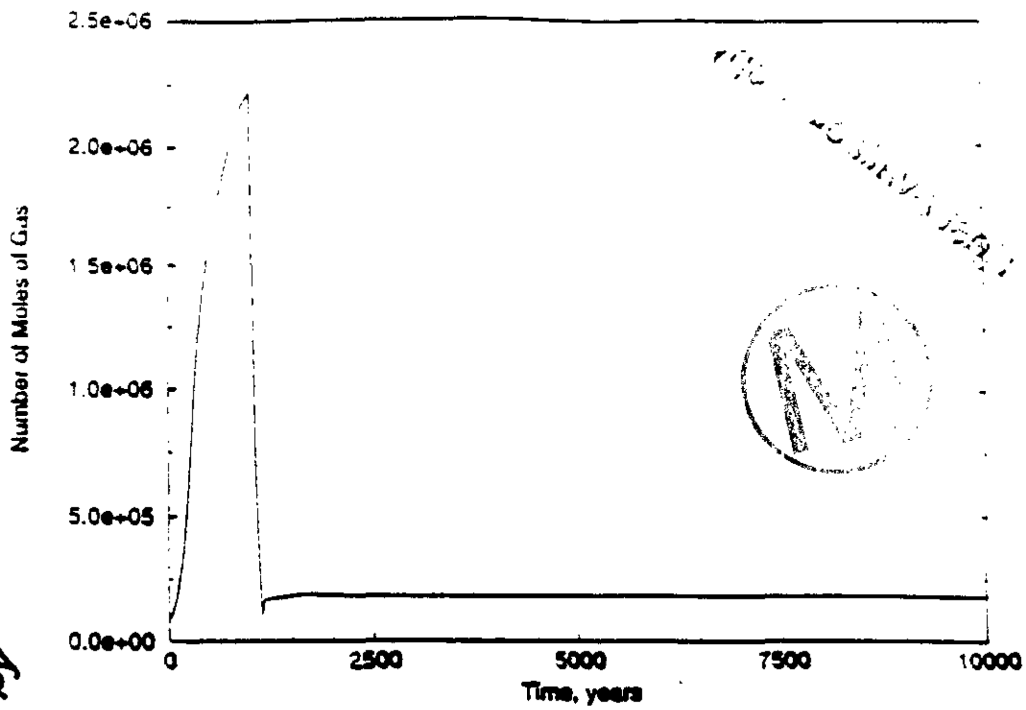

Figure 1. Equivalent Room Gas Content History From BRAGFLO Run #63

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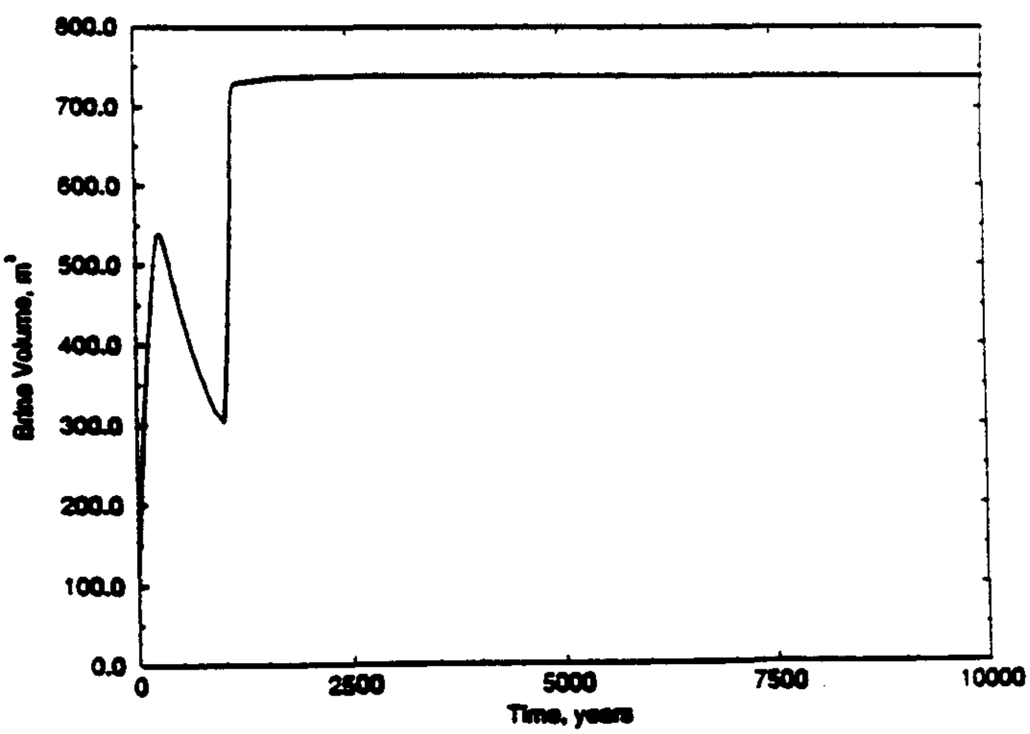

Figure 2. Equivalent Room Brine Volume History From BRAGFLO Run #63

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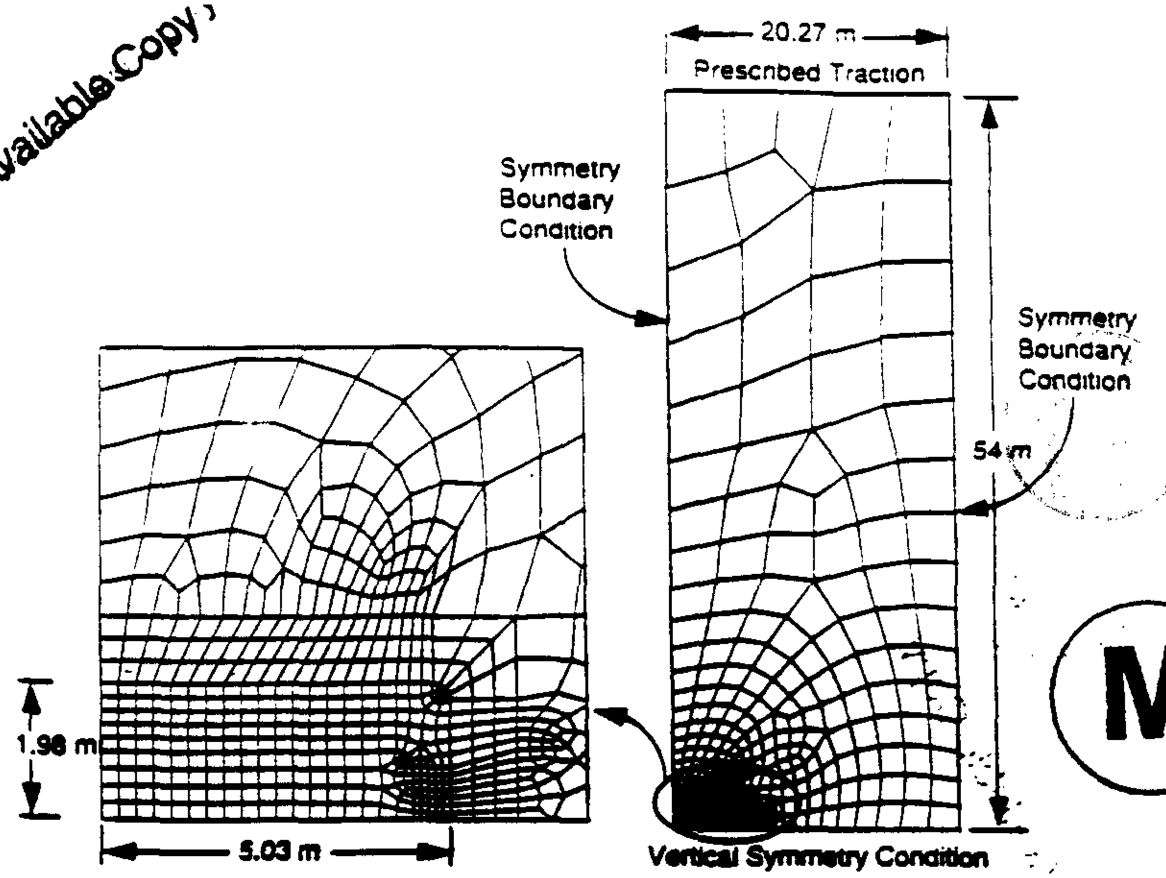

Figure 3. Finite Element Mesh and Boundary Condition Used in SANCHO Analyses

applied on both the left and right boundaries of the model. A zero-displacement boundary condition in the vertical direction was also applied on the lower boundary. A prescribed normal compressive traction of 14.8 MPa was applied on the upper boundary to simulate the overburden load at the repository horizon. Within the room, a pressure $P = (nRT)/V$, was applied along the room boundary. In this case, n was the current number of moles of gas in the room, interpolated from the external file of gas content history, and V was the current void volume in the room reduced by the current brine volume, interpolated from the external file of brine volume history.

As indicated in the idealization of Figure 3, the quarter-room dimensions are 1.98 m high by 5.03 m wide. Within the room is material representing the waste, the crushed salt backfill, and the headspace. The volume of the waste and drums is 1663 m^3 distributed along the 91.44 m length of the room. This results in a nominal cross-sectional area of waste of 18.19 m^2. For the quarter-symmetry room, this translates to a height of 1.01 m and a width of 4.5 m. The remainder of the disposal room volume was filled with crushed salt backfill having a porosity of 0.4. The disposal room volume corresponding to the headspace, 653 m^3, was discretized as though it was crushed salt, and special capabilities previously added to SANCHO [3] were used to model this region and its response. In essence, the crushed salt material representing the headspace was modeled as a very low modulus, elastic material until the disposal room volume had decreased by 653 m^3, at that point, the crushed salt backfill model was invoked for the remainder of the calculation, thus replacing the elastic material response.

The elastic-secondary creep constitutive model described by Krieg [7] was used for the intact salt. The model can be decomposed into an elastic volumetric part,

$$\dot{\sigma}_{kk} = 3K\dot{\varepsilon}_{kk} \quad \text{(EQ 2)}$$

and a deviatoric part,

$$\dot{s}_{ij} = 2G\left[\dot{e}_{ij} - (1.5)^{\frac{(n+1)}{2}} A exp\left(-\frac{Q}{RT}\right)(s_{kl}s_{kl})^{\frac{(n-1)}{2}} s_{ij}\right]. \quad \text{(EQ 3)}$$

In Equation 3, s_{ij} is the deviatoric stress defined as,

$$s_{ij} = \sigma_{ij} - \frac{\sigma_{kk}}{3}\delta_{ij} \quad \text{(EQ 4)}$$

and e_{ij} is the deviatoric strain defined by

$$e_{ij} = \varepsilon_{ij} - \frac{\varepsilon_{kk}}{3}\delta_{ij}. \quad \text{(EQ 5)}$$

The material parameters K,G, A, n, Q, R, T appearing in Equations 2 and 3 are the bulk modulus, shear modulus, material creep constant, creep exponent, activation energy, universal gas constant, and absolute temperature in degrees K, respectively. The values of the shear and bulk moduli used in the current work were obtained by dividing the values given by Krieg [7] by a factor of 12.5. This artificial reduction in the moduli has been shown to produce good agreement between computed and in-situ closures [8] when an all-salt stratigraphy is used to model the formation. The material constants are shown in Table 1.

Table 1: Material Constants Used With the Elastic/Secondary Creep Model

PARAMETER	VALUE
G	992. MPa
K	1656 MPa
A	5.79×10^{-36} $Pa^{-4.9}sec^{-1}$
n	4.9
$Q/(RT)$	20.13

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The material models and constants used for the waste and crushed salt backfill were identical to those used in earlier studies [9,10]. The crushed salt backfill model was developed by Sjaardema and Krieg [11] based on data from creep-consolidation experiments on crushed salt. In this material model, creep is included in both the volumetric and the deviatoric response. The form of the model, shown in the following equations,

$$G = G_0 exp(G_1 \rho) \tag{EQ 6}$$

$$K = K_0 exp(K_1 \rho) \tag{EQ 7}$$

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$$\dot{s}_{ij} = 2G\dot{e}_{ij} - 2GA(\rho_{intact}/\rho)^n exp\left(-\frac{Q}{RT}\right)(s_{kl}s_{kl})^{\frac{(n-1)}{2}} s_{ij} \tag{EQ 8}$$

$$\dot{\rho}_{creep} = B_0 (exp(B_1 \rho) - 1) exp(A^* \rho) \tag{EQ 9}$$

is such that the mechanical response of the crushed salt becomes identical to that of the intact salt as the density of the backfill approaches the density of the intact salt. The variables in Equations 6 through 9 not previously defined are material constants B_0 and B_1, obtained from the creep consolidation experiments and the density ρ computed from the equation:

$$\rho = \rho_0 exp\left(\int_{t_0}^{t} \dot{e}_v dt\right) \tag{EQ 10}$$

where ρ_0 is the density at time t_0. The intact density of the salt appears in Equation 8 as ρ_{intact}. The elastic moduli are assumed to depend on the density of the backfill through the relationships shown in Equations 6 and 7. The constants K_0 and K_1 were determined by using the least squares method to fit the modulus data to the function in Equation 6. In the fit, the function was constrained so that the bulk modulus of the crushed salt was equal to the bulk modulus of the intact salt when the crushed salt was fully compacted. No experiments have been conducted to determine how the shear modulus varies with density, so the shear modulus was assumed to vary according to the same exponential form as the bulk modulus. The constant G_0 was selected so that the shear modulus for the crushed salt was equal to that of the intact salt when the crushed salt was fully consolidated, and the constant G_1 was assumed to be the same as K_1. Because the shear and bulk moduli of the intact salt were divided by 12.5, G_0 and K_0 were divided by the same factor. Table 2 lists the values of the creep constants and elastic constants used for the backfill material. The stress-strain behavior of the waste was represented by a volumetric plasticity model [5] with a piecewise linear function defining the relationship between the mean stress and the volumetric strain. Compaction experiments on simulated waste were used to develop this relationship. The deviatoric response of the waste material has not been characterized. It is anticipated that when a drum filled with loosely compacted waste is compressed axially,

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Table 2: Material Constants Used With the Crushed Salt Backfill Model

PARAMETER	VALUE
G_0	864 Pa
G_1	6.53×10^{-3} m^3/kg
K_0	1.41×10^3 Pa
K_1	6.53×10^{-3} m^3/kg
A	5.79×10^{-36} Pa$^{-4.9}$sec^{-1}
n	4.9
Q/RT	20.13
A^*	-17.3×10^{-3} m^3/kg
B_0	1.3×10^8 kg (m^3 sec)$^{-1}$
B_1	0.82×10^{-6} Pa^{-1}

the drum will not undergo significant lateral expansion until most of the void space inside the drum has been eliminated.

For the volumetric plasticity model, the yield surface in principal stress space is a surface of revolution with its axis centered about the hydrostat and the open end pointing into the compression direction. The open end is capped with a plane which is at right angles to the hydrostat. The deviatoric part is elastic-perfectly plastic so the surface of revolution is stationary in stress space. The volumetric part has variable strain hardening so the end plane moves outward during volumetric yielding. The volumetric hardening is defined by a set of pressure-volumetric strain relations. A flow rule is used such that deviatoric strains produce no volume change (associated flow). The model is best broken into volumetric and deviatoric parts with the deviatoric part resembling conventional plasticity. The volumetric yield function is a product of two functions describing the surface of revolution and the plane normal to the pressure axis. These are given by

$$\phi_s = \frac{1}{2}s_{ij}s_{ij} - (a_0 + a_1 p + a_2 p^2) \qquad \text{(EQ 11)}$$

$$\phi_p = p - g(e_v) \qquad \text{(EQ 12)}$$

where a_0, a_1, a_2 are constants defining the yield surface, p is the pressure, and e_v is the volume strain. The form of g is defined in this problem by a set of piecewise linear segments relating pressure-volume strain. Table 3 lists the pressure-volumetric strain data used for the waste drum model. The elastic material parameters and constants defining the yield surface are given in Table 4.

Table 3: Pressure-Volumetric Strain Data Used in the Volumetric Plasticity Model for the Waste Drums

PRESSURE (MPa)	$\ln(\rho/\rho_0)$
0.028	0.032
0.733	0.741
1.133	0.898
1.667	1.029
2.800	1.180
10.17	1.536

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Table 4: Material Constants Used With the Volumetric Plasticity Model for the Waste

PARAMETER	VALUE
G	333 MPa
K	222 MPa
a_0	0.
a_1	0.
a_2	3.

The analysis was carried out to a simulation time of 10,000 years. The input file for the SANCHO analysis is included in Appendix II.

Results of the Analyses

The results of interest are the pressure buildup in the room and the corresponding disposal room porosity. It is these quantities that will be compared with the PA calculational results to assess if the results derived from the earlier disposal room closure calculations are being correctly incorporated in the PA analyses.

Figure 4 shows the gas pressure within the disposal room computed by SANCHO on the basis of the gas content and brine volume histories coming from BRAGFLO. The pressure increases quickly to a value slightly greater than 20 MPa at around 300 years. It then drops at a slower rate to a value slightly greater than about 15 MPa by the time that the human intrusion occurs at 1000 years. The effect of the human intrusion is to suddenly drop the pressure to a lower value, slightly greater than about 10 MPa, with an immediate reversal to increase it to its maximum value of slightly greater than 23 MPa at around 1200 years. Thereafter, the pressure slowly drops for the remainder of the simulation to a value of

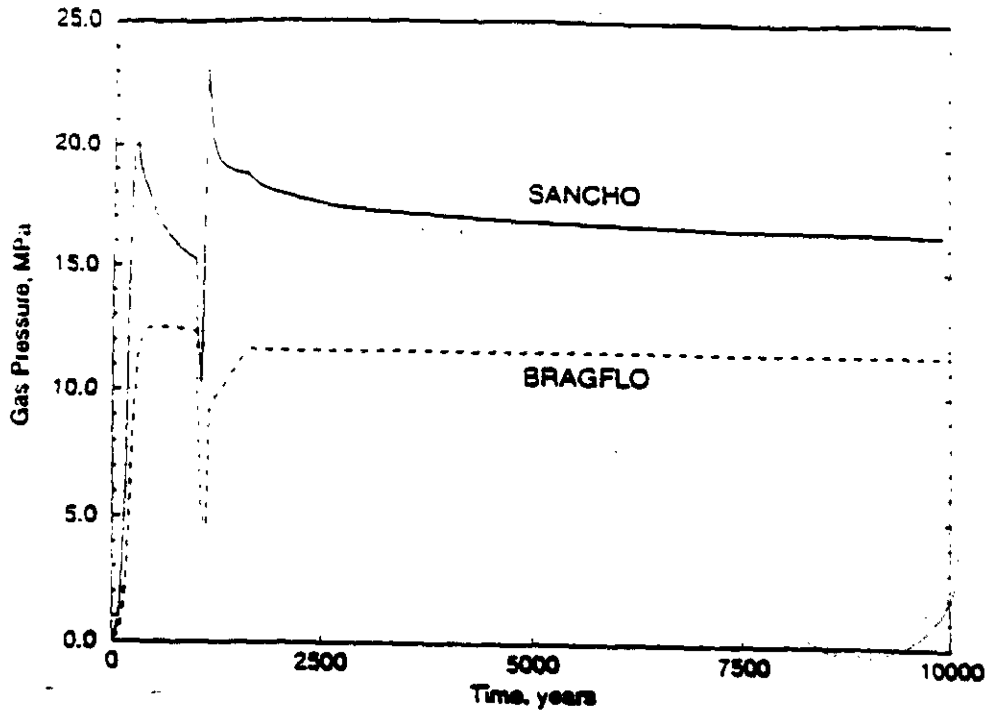

Figure 4. Comparison of Gas Pressure Histories for Run #63

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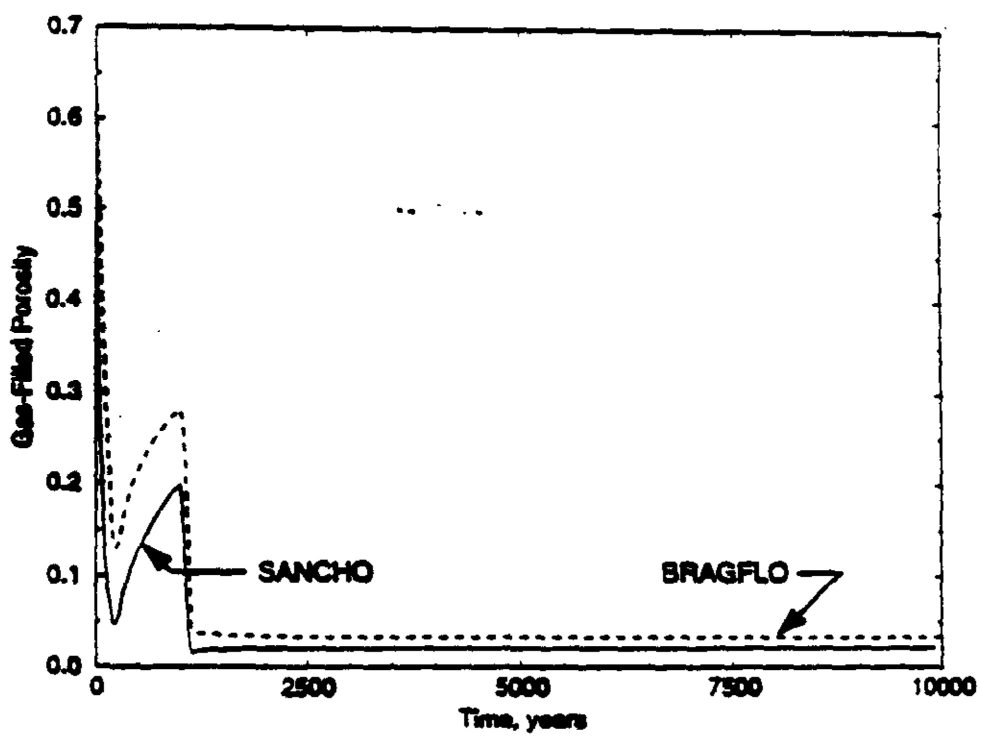

Figure 5. Comparison of Room Porosity Histories for Run #63

around 16.5 MPa at 10,000 years. Shown as well in the figure is a plot of the column labelled "PRESWAST" coming from Table I of Appendix I. This quantity is the waste gas pressure in Pascals computed by BRAGFLO. The BRAGFLO results show a trend similar to the SANCHO results, except that the pressure rises more slowly and to a lower value initially, drops more after the human intrusion, and rises to a much smaller value subsequently. Thus, the BRAGFLO curve shows a rise to about 12.5 MPa in about 300 to 400 years, slowly decreasing by a slight amount until the human intrusion event occurs at 1000 years. The human intrusion results in a drop to a value of about 4.5 MPa at about 1200 years. Thereafter, the pressure increases by a significant amount to a value of about 11.7 MPa at about 1700 years and remains constant. Thus, at the end of the 10,000 year simulation, the SANCHO gas pressure value is about 40 % larger than that for BRAGFLO. However, a basic difference in the nature of the curve is apparent when compared to that from SANCHO in that the reversal that was seen at around 1200 years in the SANCHO results is not seen here for the BRAGFLO results. In fact, it appears as if a pressure cut-off may have been used in the BRAGFLO calculation.

Figure 5 shows the corresponding room porosity history computed by SANCHO. The porosity drops quickly as the room closes and reaches a value of about 0.05 at about 200 years. It then begins to increase and reaches a value of about 0.2 immediately prior to the human intrusion event. The porosity drops quickly after human intrusion and reaches its minimum value of about 0.015. Thereafter, it increases very gradually throughout the remainder of the simulation, reaching a value of about 0.02 at the end. Shown in the figure also is a porosity value computed from the BRAGFLO results provided in Appendix I. This porosity value was computed by using the following equation,

$$\varphi = \frac{\left[\frac{(1-\mathrm{SBAVW})\,\mathrm{PORVOLW}}{12.65}\right]}{\left[\frac{(1-\mathrm{SBAVW})\,\mathrm{PORVOLW}}{12.65} + 1229\right]}, \qquad \text{(EQ 13)}$$

where "PORVOLW" is the waste pore volume in cubic meters and "SBAVW" is the average brine saturation in the waste (both of these quantities coming from the columns labelled as such in Table I of Appendix I), 12.65 is the number of rooms per panel, and 1229 is the volume of solids in the room. It is apparent that the BRAGFLO porosity results essentially mimic and parallel those described for SANCHO, except that they are higher at all times. The value of porosity from BRAGFLO is seen to asymptote to a value slightly greater than about 0.03. Thus, the value of porosity at the end of the simulation computed with BRAGFLO appears to be about 50 % greater than computed with SANCHO.

Summary and Conclusions

Because recasting of the calculational results for closure of a disposal room model into suitable input for use in performance assessment compliance analyses requires various simplifications and assumptions, all of which have the potential of introducing uncertainty into the results, several calculations to check the validity of the recasting procedure were performed in an earlier study [1]. The process began by recovering information on the variation in gas content and porosity with time around the waste/backfill region of a disposal room from a performance assessment compliance calculation. This information included the effects of closure of the room indirectly via the concept of a "porosity surface." The gas versus time data was then used as the gas-generation input for a disposal room closure calculation with the SANCHO computer code. The results were then compared with the original porosity history recovered from the performance assessment calculation. Three cases were investigated in that study: a human intrusion into a practically dry (low brine saturation) panel – Run #18; an undisturbed, practically dry (low brine saturation) repository – Run #42; and a human intrusion into a panel that becomes largely brine saturated – Run #63. Satisfactory agreement was observed between the performance assessment porosity histories and the more detailed histories computed with SANCHO for repository environments with low brine saturations. Results differed, however, for the third case (Run #63) when saturation of the panel occurred. A source of the discrepancy for the third case was thought be due to the assumption in the SANCHO calculations, that all void volume in the room is occupied by gas (this same assumption was used in generating the original porosity surfaces used by BRAGFLO), whereas the BRAGFLO calculations explicitly included brine saturation. Thus, it followed that for the case when the panel becomes largely brine saturated, the presence of the brine could prove significant.

A new calculation has been performed for this case, using SANCHO, to determine if the effect of including the brine volume in the SANCHO calculation will resolve the discrepancy between its results and those from BRAGFLO. The new calculation is identical to the earlier one except that the void volume in the room at any time for this case has been reduced by the volume of brine in the room (this information also came from the BRAGFLO run). Results from this analysis show that there still remain differences: at the end of the 10,000 year simulation, the gas pressure computed with SANCHO is about 40 % greater than that coming from BRAGFLO, and the porosity is 50 % smaller. However, the new results are in much better agreement than those from the earlier calculation. This appears to indicate that for cases in which saturation of the repository is significant, the presence of the brine volume in the room will also significantly influence the response of the room.

Better agreement does not necessarily indicate that a complete resolution to the discrepancy between the results for SANCHO and those for BRAGFLO has been achieved. In fact, an inconsistency is inherent in the present calculation in the sense that the brine volume entering the room depends on the pressure within the room, which depends on the closure of the room, which is what we are trying to compute in this calculation. So we are in essence cheating here in that we are assuming that the pore volume computed by BRAGFLO is correct even though it is based on previous SANCHO calculations in which brine was not included. Had it been possible to include brine in those previous calculations used to generate the original porosity surface, the room pressures would have been higher. In short, the process is a coupled one in which closure, gas generation, and brine inflow exert some influence on each other. To attempt to model the process in a staggered manner as has been done in BRAGFLO by using a previously generated porosity surface from SANCHO to predict brine inflow, is bound to lead to results that are inconsistent to some degree.

In addition, without knowing the details of how the information from the original porosity surface from SANCHO is used by BRAGFLO, it will not be possible to completely resolve the inconsistencies that remain. For example, the gas pressure curves from the two codes around the time of human intrusion are very different in nature and it appears that a pressure cut-off may have been invoked in the BRAGFLO calculation.

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Appendix I

Results From PA's 1992 Compliance Analysis Using BRAGFLO – Transmittal From B. M. Butcher, 6345, of 7/19/93 (Data Provided by Jim Bean, 6342)

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```
--------------------------------------------------------------------
THE DATA IN FILES [illegible]_018.DAT, [illegible]_043.DAT, AND [illegible]_063.DAT
IS OUTPUT FROM BRAGFLO. FILES ARE IN DIRECTORY U7:[[illegible].BUTCHER]
--------------------------------------------------------------------

-------------------------
VARIABLE    DEFINITION
-------------------------
N          = [illegible] STEP NUMBER
TIME       = ELAPSED TIME IN SECONDS
[illegible]  = GAS GENERATION RATE IN CUBIC METERS/SEC
[illegible]  = GAS GENERATION RATE IN MOLES/SEC
[illegible]  = AVERAGE BRINE SATURATION IN WASTE
PORVOLW    = WASTE PORE VOLUME (GAS AND BRINE) IN CUBIC METERS
[illegible] = WASTE (GAS) PRESSURE IN PASCALS
[illegible] = POROSITY OF WASTE (VOID VOLUME/TOTAL VOLUME)
[illegible]  = GAS VOLUME IN WASTE (CUBIC METERS AT P=101325 PA AND T=300.15K)
[illegible]  = GAS VOLUME AT RESERVOIR CONDITIONS (CUBIC METERS AT P=[illegible])
[illegible]  = CUMULATIVE GAS GENERATED IN WASTE (CUBIC METERS AT P=101325)
           = INTEGRATED VALUES OF [illegible] ACCORDING TO FOLLOWING:
             [illegible](T=0) = 0
             [illegible](T) = [illegible](T-DT) + 0.5*DT*([illegible](T) + [illegible](T-DT))
```

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Table I. Grope File From PA's Compliance Analysis - Run # 63 (GROPE_063.DAT)

#Step	Time	GASGENV	GASGENM	BRAVM	PORVOLM	PRESWAST	POROSITY	GASVOLM	GASVOL	GASGENC
1	0.00000E+00	0.00000E+00	0.00000E+00	4.54400E-02	3.04292E+04	1.01325E+05	6.60170E-01	2.90465E+04	2.90465E+04	0.00000E+00
2	2.13042E+05	1.30007E-05	5.31751E-04	4.54399E-02	3.04200E+04	1.01335E+05	6.59996E-01	2.90414E+04	2.90305E+04	1.39005E+00
3	1.04376E+08	1.32744E-05	5.42946E-04	4.07919E-02	2.75072E+04	1.09963E+05	5.96784E-01	2.83993E+04	2.61651E+04	2.42083E+03
4	1.50317E+09	2.21557E-05	9.06204E-04	1.57562E-01	1.83140E+04	2.33683E+05	3.97332E-01	3.55503E+04	1.54204E+04	2.57033E+04
5	2.20402E+09	2.77981E-05	1.13690E-03	2.26665E-01	1.53504E+04	3.65614E+05	3.33035E-01	4.27105E+04	1.18710E+04	4.33084E+04
6	2.83950E+09	3.35560E-05	1.37250E-03	2.97164E-01	1.34193E+04	5.81115E+05	2.91137E-01	5.10690E+04	9.63123E+03	6.24717E+04
7	2.90040E+09	3.47564E-05	1.42160E-03	3.11006E-01	1.30901E+04	5.99612E+05	2.83997E-01	5.29262E+04	9.00740E+03	6.40934E+04
8	3.62163E+09	4.22321E-05	1.72736E-03	4.03441E-01	1.14221E+04	9.02250E+05	2.47809E-01	6.55222E+04	6.02396E+03	9.26409E+04
9	4.50496E+09	5.24130E-05	2.14301E-03	5.20130E-01	9.89369E+03	1.98904E+06	2.14645E-01	8.60736E+04	4.66036E+03	1.38245E+05
10	5.09171E+09	5.65443E-05	2.31270E-03	5.70726E-01	9.37450E+03	2.47959E+06	2.03305E-01	9.50543E+04	3.94923E+03	1.65053E+05
11	5.67662E+09	6.29022E-05	2.57600E-03	6.57572E-01	8.86031E+03	3.07430E+06	1.92403E-01	1.13426E+05	3.03676E+03	2.00810E+05
12	6.49698E+09	6.76961E-05	2.77690E-03	7.17729E-01	8.76675E+03	5.90907E+06	1.90199E-01	1.39782E+05	2.47460E+03	2.54492E+05
13	6.49867E+09	6.79016E-05	2.77730E-03	7.17820E-01	8.76675E+03	5.93782E+06	1.90199E-01	1.40049E+05	2.47300E+03	2.54607E+05
14	6.77256E+09	6.89484E-05	2.82011E-03	7.30641E-01	8.86083E+03	6.66392E+06	1.92414E-01	1.51170E+05	2.38890E+03	2.73360E+05
15	7.52703E+09	6.99416E-05	2.86072E-03	7.42005E-01	9.01890E+03	8.40034E+06	1.95672E-01	1.83438E+05	2.31964E+03	3.25790E+05
16	7.63867E+09	7.01010E-05	2.86728E-03	7.44757E-01	9.09713E+03	8.62101E+06	1.97367E-01	1.87929E+05	2.32190E+03	3.33335E+05
17	7.80920E+09	7.01477E-05	2.86916E-03	7.45329E-01	9.15696E+03	9.16936E+06	1.98622E-01	2.00101E+05	2.33150E+03	3.51120E+05
18	8.60144E+09	6.89059E-05	2.81037E-03	7.30121E-01	9.36332E+03	1.03693E+07	2.03142E-01	2.43756E+05	2.52696E+03	4.06210E+05
19	9.14651E+09	6.80809E-05	2.78662E-03	7.20017E-01	9.48853E+03	1.08807E+07	2.05143E-01	2.67230E+05	2.64739E+03	4.38064E+05
20	9.52241E+09	6.72269E-05	2.74963E-03	7.09857E-01	9.53368E+03	1.12511E+07	2.06830E-01	2.88438E+05	2.76899E+03	4.63495E+05
21	9.76114E+09	6.66652E-05	2.72756E-03	7.02923E-01	9.57703E+03	1.14665E+07	2.07779E-01	3.01183E+05	2.84512E+03	4.79479E+05
22	9.98549E+09	6.61760E-05	2.70674E-03	6.96696E-01	9.61301E+03	1.16059E+07	2.08560E-01	3.12671E+05	2.91566E+03	4.94303E+05
23	1.02420E+10	6.55059E-05	2.68250E-03	6.89460E-01	9.65161E+03	1.17568E+07	2.09397E-01	3.25324E+05	2.99721E+03	5.11282E+05
24	1.04877E+10	6.50403E-05	2.66050E-03	6.82076E-01	9.68616E+03	1.18705E+07	2.10103E-01	3.36566E+05	3.07100E+03	5.27331E+05
25	1.08644E+10	6.42327E-05	2.62723E-03	6.72087E-01	9.72765E+03	1.20276E+07	2.11047E-01	3.52008E+05	3.18204E+03	5.51681E+05
26	1.12974E+10	6.32712E-05	2.58790E-03	6.61111E-01	9.77300E+03	1.21668E+07	2.12031E-01	3.70642E+05	3.33196E+03	5.79285E+05
27	1.17852E+10	6.22461E-05	2.54597E-03	6.48557E-01	9.81432E+03	1.22626E+07	2.12927E-01	3.88790E+05	3.46917E+03	6.09899E+05
28	1.24503E+10	6.09666E-05	2.49372E-03	6.32811E-01	9.85861E+03	1.23258E+07	2.13800E-01	4.10519E+05	3.61099E+03	6.50074E+05
29	1.30760E+10	5.99806E-05	2.45004E-03	6.19031E-01	9.87022E+03	1.24100E+07	2.14140E-01	4.28354E+05	3.75235E+03	6.88616E+05
30	1.37323E+10	5.88734E-05	2.40882E-03	6.07251E-01	9.87274E+03	1.24544E+07	2.14195E-01	4.44117E+05	3.87751E+03	7.27662E+05
31	1.57780E+10	5.60899E-05	2.29090E-03	5.72181E-01	9.87413E+03	1.24789E+07	2.14225E-01	4.84757E+05	4.22434E+03	8.45171E+05
32	1.62980E+10	5.51079E-05	2.25401E-03	5.62055E-01	9.87413E+03	1.24770E+07	2.14225E-01	4.96290E+05	4.32433E+03	8.73617E+05
33	1.93469E+10	4.92261E-05	2.01343E-03	5.15267E-01	9.87413E+03	1.24615E+07	2.14225E-01	5.40690E+05	4.78632E+03	1.03290E+06
34	2.14110E+10	4.69176E-05	1.91901E-03	4.86730E-01	9.87413E+03	1.24507E+07	2.14225E-01	5.80521E+05	5.06009E+03	1.13234E+06
35	2.29304E+10	4.40936E-05	1.80350E-03	4.67366E-01	9.87413E+03	1.24392E+07	2.14225E-01	6.01907E+05	5.25930E+03	1.20145E+06
36	2.43250E+10	4.27993E-05	1.75056E-03	4.50710E-01	9.87413E+03	1.24316E+07	2.14225E-01	6.20367E+05	5.42360E+03	1.26207E+06
37	2.56784E+10	4.16507E-05	1.70359E-03	4.35940E-01	9.87413E+03	1.24263E+07	2.14225E-01	6.36793E+05	5.56952E+03	1.31885E+06

Table I. (Continued)

38	2.69221E+10	4.85867E-05	1.66007E-03	4.22260E-01	9.87413E+03	1.24216E+07	2.14225E-01	6.52010E+05	5.70460E+03	1.37032E+06
39	2.84870E+10	3.07610E-06	1.25821E-03	4.07832E-01	9.87413E+03	1.23897E+07	2.14225E-01	6.67841E+05	5.85011E+03	1.42614E+06
40	3.15570E+10	1.78964E-05	7.31992E-04	3.89731E-01	9.87413E+03	1.23842E+07	2.14225E-01	6.82722E+05	6.02500E+03	1.50083E+06
41	3.15572E+10	1.77774E-05	7.27128E-04	3.88192E-01	9.87422E+03	1.23806E+07	2.14227E-01	6.84330E+05	6.04113E+03	1.50083E+06
42	3.15620E+10	1.77760E-05	7.27067E-04	3.88184E-01	9.87423E+03	1.23800E+07	2.14227E-01	6.84275E+05	6.04121E+03	1.50092E+06
43	3.15640E+10	1.77784E-05	7.27042E-04	3.88181E-01	9.87422E+03	1.23803E+07	2.14227E-01	6.84213E+05	6.04124E+03	1.50097E+06
44	3.15792E+10	1.77776E-05	7.27134E-04	3.88196E-01	9.87393E+03	1.22818E+07	2.14230E-01	6.83203E+05	6.04091E+03	1.50123E+06
45	3.16495E+10	1.77361E-06	7.26948E-04	3.88812E-01	9.86897E+03	1.19682E+07	2.14113E-01	6.65917E+05	6.03100E+03	1.50240E+06
46	3.16780E+10	1.77700E-05	7.26857E-04	3.89344E-01	9.86996E+03	1.17773E+07	2.14047E-01	6.55210E+05	6.02473E+03	1.50290E+06
47	3.17620E+10	1.78653E-05	7.31540E-04	3.90780E-01	9.85976E+03	1.13848E+07	2.13913E-01	6.32869E+05	6.00668E+03	1.50414E+06
48	3.21184E+10	1.88928E-05	7.72760E-04	4.06726E-01	9.83036E+03	9.51667E+06	2.13275E-01	5.19830E+05	5.83200E+03	1.51090E+06
49	3.34084E+10	2.05061E-05	1.16932E-03	5.32202E-01	9.77326E+03	8.07571E+06	2.12036E-01	2.56390E+05	4.57190E+03	1.54152E+06
50	3.37089E+10	3.26907E-05	1.33710E-03	5.85461E-01	9.76474E+03	5.33176E+06	2.11851E-01	2.06438E+05	4.04787E+03	1.55097E+06
51	3.46286E+10	4.40534E-05	1.80187E-03	7.36095E-01	9.75491E+03	4.70206E+06	2.11630E-01	1.16832E+05	2.57437E+03	1.58626E+06
52	3.50050E+10	4.81326E-05	1.96070E-03	7.85831E-01	9.75367E+03	4.62316E+06	2.11611E-01	9.26421E+04	2.00893E+03	1.60365E+06
53	3.57823E+10	4.95106E-05	2.02507E-03	9.26660E-01	9.75260E+03	4.85461E+06	2.11500E-01	3.21100E+04	7.32927E+02	1.64156E+06
54	3.61919E+10	4.95041E-05	2.02600E-03	9.33100E-01	9.70399E+03	6.96269E+06	2.12269E-01	4.07871E+04	6.53680E+02	1.66104E+06
55	3.69346E+10	4.95310E-05	2.02500E-03	9.39150E-01	9.82750E+03	9.33610E+06	2.13213E-01	5.22554E+04	5.98004E+02	1.69366E+06
56	5.16039E+10	1.67392E-05	6.84663E-04	9.44144E-01	9.86350E+03	1.16213E+07	2.13994E-01	5.91643E+04	5.50926E+02	2.10311E+06
57	7.51626E+10	1.91798E-10	7.84478E-09	9.46660E-01	9.86354E+03	1.16241E+07	2.13995E-01	5.66124E+04	5.27029E+02	2.30027E+06
58	2.09723E+11	1.91798E-10	7.84478E-09	9.46763E-01	9.86398E+03	1.16521E+07	2.14005E-01	5.65363E+04	5.25129E+02	2.30028E+06
59	1.44283E+11	1.91798E-10	7.84478E-09	9.46790E-01	9.86402E+03	1.16547E+07	2.14003E-01	5.65196E+04	5.24865E+02	2.36029E+06
60	1.70843E+11	1.91798E-10	7.84478E-09	9.46781E-01	9.86396E+03	1.16503E+07	2.14004E-01	5.65085E+04	5.24950E+02	2.30029E+06
61	2.13603E+11	1.91798E-10	7.84478E-09	9.46763E-01	9.86386E+03	1.16440E+07	2.14002E-01	5.64983E+04	5.25123E+02	2.30030E+06
62	2.47963E+11	1.91798E-10	7.84478E-09	9.46742E-01	9.86374E+03	1.16370E+07	2.13999E-01	5.64881E+04	5.25323E+02	2.30031E+06
63	2.82523E+11	1.91798E-10	7.84478E-09	9.46721E-01	9.86363E+03	1.16298E+07	2.13997E-01	5.64777E+04	5.25524E+02	2.30031E+06
64	3.18570E+11	1.91798E-10	7.84478E-09	9.46700E-01	9.86352E+03	1.16228E+07	2.13994E-01	5.64671E+04	5.25726E+02	2.30032E+06

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Appendix II

Input File for SANCHO (Version 2.0.0) Run

```
PRESSURIZED ROOM CALCULATION - MOLES & BRINE VS TIME FROM BRAGFLO RUN 63
CONTROL,3,2,0,3
PLANE
ELEMENTS,1
ISTRESS,CONSTANT,-14.8E6,-14.8E6,-14.8E6
ISTRESS,OFF,2,3
NODES,1
DXSCALE,.5
PLOT,ELEMENT,STRESS,STRAIN,STATE
PLOT,NODAL,DISP,TEMP,RESIDUAL
PLOT,GLOBAL,RMAG,ITER
PLIMIT,.2E6
SOLUTION,10.,100,2500,100.
TIMESTEP,0.,400000,3.1536E11
TIMEPRNT,0.,3.1536E9,3.1536E11
TIMEPLOT,0.,1.5768E7,3.1536E7,1.5768E8,3.1536E9,3.1536E8,6.3072E10,*
3.1536E9,3.1536E11
ENDSET
MATERIAL,5,1.,0.,0.0
***HALITE***
1.984E9,1.656E9,5.79E-36,4.9,20.13,0,.001
MATERIAL,7,1.,0.,0.
   * * CRUSHED SALT BACKFILL * *
848.,6.53E-3,1408.,6.53E-3,5.79E-36,4.9,20.13,*
-17.3E-3,0.0,1.3E8,0.82E-6,1.300E3,2140.,.0005
MATERIAL,2,1.,0.,0.
* * CRUSHABLE FOAM MODEL FOR DRUMS * *
3.333E8,2.223E8,0.,0.,3.
.03230,.02833E6,.741,.733E6,.898,1.1333E6,1.029,1.667E6,*
1.18,2.8E6,1.336,10.167E6
ENDSET
DISPR,1
DISPZ,2
PRESSURE,300,0.
CENTER,0.,0.
PRESSURE,200,14.8E6
PHISTORY,0.,1.0,1.0E12,1.
END
```

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Sandia National Laboratories

Albuquerque. New Mexico 87185

date: November 19, 1993

to: B. M. Butcher, 6345 (MS1341)

from: J. Guadalupe Arguello and C. M. Stone, 1561

subject: Performance Assessment Verification Calculations – SANCHO Comparisons to BRAGFLO Runs 18, 42, and 63

Introduction

Performance assessment (PA) of the Waste Isolation Pilot Plant (WIPP) requires that numerous scenarios be investigated to determine if compliance is achievable. This leads to a very large number of analyses that need to be performed during the course of the investigations. Because of the large number of analyses required, the numerical models used by PA must be fast and efficient and consequently contain less detail, in terms of computed disposal room closure, than would be ideal. The reason for this is that the complexity of salt creep and the compute-intensive analyses required to determine its response make it prohibitive to directly calculate disposal room closure in PA analyses at this time. Yet, the effects of disposal room closure must be included in PA compliance analyses. Therefore, detailed disposal room creep closure calculations are typically performed using separate large, complex, nonlinear finite element based codes that are fast and efficient, in their own right, but on a different scale. The calculational results from disposal room creep closure analyses must then be recast in a form suitable for use in PA compliance analyses. The process is as follows:

1) First, a set of disposal room creep closure calculations using a finite element code (e. g., SANCHO or SANTOS) is performed for several discrete values of gas generation (leading to number of moles of gas versus time) and a set of corresponding room porosity histories (room porosity versus time) is obtained for those discrete values.

2) From those results, a "porosity" surface is defined that provides the porosity of a single WIPP disposal room as a function of the number of moles of gas present (or the pressure resulting from that amount of gas) and time.

3) This concept of a "porosity" surface is then used by the PA compliance analysis code (BRAGFLO). The results from the first step are used directly as input to the code; it then interpolates to determine the porosity (in a panel or the repository) as a function of time for any arbitrary gas generation (or pressure) path, accounting for the influence of brine saturation. In this manner, the effects of disposal room closure are indirectly accounted for in the PA calculation.

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The recasting of the finite element analysis disposal room creep closure results into a form suitable for input to PA requires a number of simplifying steps and assumptions, all of which have the potential of introducing uncertainty into the results.

A method for checking the validity of the procedure has been examined at the request of the project [1]. The method begins by recovering data on the variation in gas content and porosity with time in the waste/backfill region of a disposal room from a performance assessment compliance calculation, in which the effects of disposal room closure have been included indirectly via the concept of a "porosity surface"[2]. This surface was derived from the results of several analyses [3] using the gas generation rates and other details defining the Baseline room model [4]. The gas-versus-time data from the compliance calculation are then used as an input gas generation history for a disposal room closure calculation using the SANCHO [5] code to compute the porosity of the room as a function of time. The porosity history obtained from SANCHO is then compared with the original porosity history recovered from the PA calculation. The difference between the two porosity histories represents translation error, and differences arising from the various assumptions and simplifications. In this manner, we can assess if the results derived from disposal room closure calculations are being correctly incorporated in the PA analyses.

This memo serves to document the results of three disposal room closure analyses performed with the SANCHO code using gas-versus-time data from performance assessment calculations previously performed with the BRAGFLO code. The three PA BRAGFLO cases, and their corresponding computed gas histories (used as input to the SANCHO calculations) are briefly described in the next section of the memo. The disposal room geomechanical model used for the SANCHO computations is then discussed. The results from the analyses are then presented, and the porosity histories obtained from SANCHO are compared to those from the original BRAGFLO runs. Finally, comments on the validity of using "porosity surfaces" in PA analyses are offered for consideration.

Description of PA Cases

Three cases from PA's 1992 compliance analysis, using porosity surface information, were investigated. The BRAGFLO results for each of three cases – designated as runs 18, 42, and 63 – were transmitted to organization 1561 by B. M. Butcher, 6345 using data provided by Jim Bean, 6342. The transmitted information is included in tabular form in Appendix I. Table I in Appendix I shows the results for Run #18 which represents a human intrusion into a practically dry (low brine saturation) panel. Table II shows the results for Run #42 which represents an undisturbed, practically dry, repository. Table III shows the results for Run #63 which represents a human intrusion into a panel that becomes largely brine saturated.

The information from these tables that was used as input to the SANCHO calculations was that labelled "GASVOLW". As indicated in Appendix I, the information in the column labelled "GASVOLW" is the gas volume in the waste – at the corresponding time – in cubic

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meters at a pressure, P=101325 Pa, and at a temperature, T=300.15 °K. By using the Ideal Gas Law,

$$PV = nRT, \tag{EQ 1}$$

where P is the pressure in Pascals, V is the volume in cubic meters, R is the universal gas constant, and T is the temperature in degrees Kelvin, the number of moles of gas, n, with time was computed. This quantity was divided by 119.64 rooms per repository or 12.65 rooms per panel to get an "equivalent room" value for use in the SANCHO disposal room model. For each of the three cases, the information was written to a file that was subsequently read by SANCHO during the disposal room closure calculation. The resulting equivalent room gas content histories that were written to these files for the three cases are shown in Figure 1.

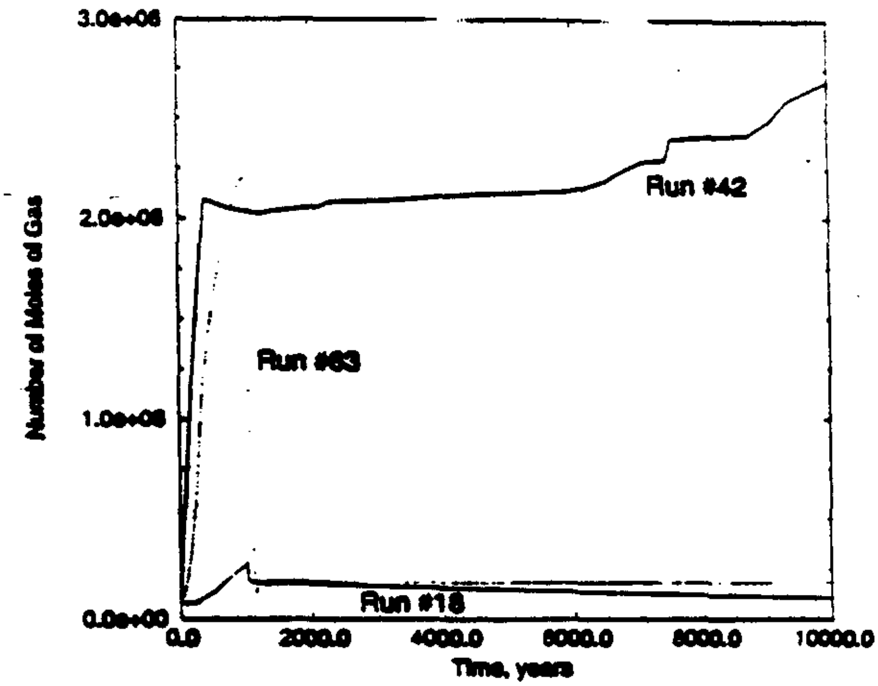

Figure 1. Equivalent Room Gas Content Histories From BRAGFLO Runs

Geomechanical Model

A two-dimensional plane strain disposal room model, as shown in Figure 2, was used for the SANCHO analyses. This same model was used for the Baseline calculations described by Stone in his October 6, 1992 memorandum [3]. The model represents the room as one in an infinite array of disposal rooms located at the repository horizon. Consequently, symmetry boundary conditions are applicable between rooms. If it is further assumed that gravity forces do not greatly affect the material response near the room [6] and that the stratigraphy is all-salt, use can be made of vertical symmetry boundary conditions to arrive at the quarter-symmetry model shown in the figure. The model contains 618 elements and

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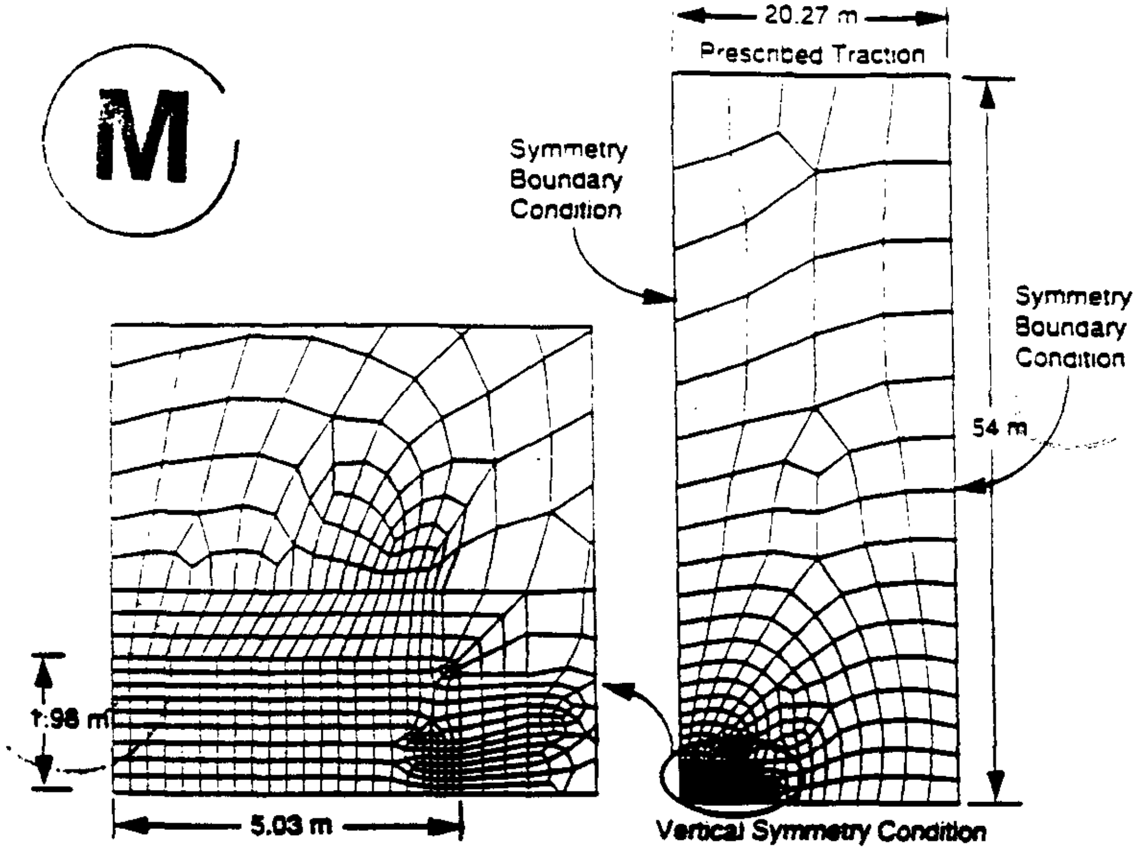

Figure 2. Finite Element Mesh and Boundary Condition Used in SANCHO Analyses

664 nodal points. A zero-displacement boundary condition in the horizontal direction was applied on both the left and right boundaries of the model. A zero-displacement boundary condition in the vertical direction was also applied on the lower boundary. A prescribed normal traction of -14.8 MPa was applied on the upper boundary to simulate the overburden load at the repository horizon. Within the room, a pressure $P = (nRT)/V$, was applied along the room boundary. In this case, n was the current number of moles of gas in the room, interpolated from the external file of gas content history, and V was the current void volume in the room.

As indicated in the idealization of Figure 2, the quarter-room dimensions are 1.98 m high by 5.03 m wide. Within the room is material representing the waste, the crushed salt backfill, and the headspace. The volume of the waste and drums is 1663 m^3 distributed along the 91.44 m length of the room. This results in a nominal cross-sectional area of waste of 18.19 m^2. For the quarter-symmetry room, this translates to a height of 1.01 m and a width of 4.5 m. The remainder of the disposal room volume was filled with crushed salt backfill having a porosity of 0.4. The disposal room volume corresponding to the headspace, 653 m^3, was discretized as though it was crushed salt, and special capabilities previously added to SANCHO [3] were used to model this region and its response. In essence, the crushed salt material representing the headspace was modeled as a very low modulus, elastic material until the disposal room volume had decreased by 653 m^3; at that point, the crushed salt backfill model was invoked for the remainder of the calculation, thus replacing the elastic material response.

The elastic-secondary creep constitutive model described by Krieg [7] was used for the intact salt. The model can be decomposed into an elastic volumetric part,

$$\dot{\sigma}_{kk} = 3K\dot{\varepsilon}_{kk} \qquad \text{(EQ 2)}$$

and a deviatoric part,

$$\dot{s}_{ij} = 2G\left[\dot{e}_{ij} - (1.5)^{\frac{(n+1)}{2}} A exp\left(-\frac{Q}{RT}\right) (s_{kl}s_{kl})^{\frac{(n-1)}{2}} s_{ij}\right]. \qquad \text{(EQ 3)}$$

In Equation 3, s_{ij} is the deviatoric stress defined as,

$$s_{ij} = \sigma_{ij} - \frac{\sigma_{kk}}{3}\delta_{ij} \qquad \text{(EQ 4)}$$

and e_{ij} is the deviatoric strain defined by

$$e_{ij} = \varepsilon_{ij} - \frac{\varepsilon_{kk}}{3}\delta_{ij}. \qquad \text{(EQ 5)}$$

The material parameters K,G, A, n, Q, R, T appearing in Equations 2 and 3 are the bulk modulus, shear modulus, material creep constant, creep exponent, activation energy, universal gas constant, and absolute temperature in degrees K, respectively. The values of the shear and bulk moduli used in the current work were obtained by dividing the values given by Krieg [7] by a factor of 12.5. This artificial reduction in the moduli has been shown to produce good agreement between computed and in-situ closures [8] when an all salt stratigraphy is used to model the salt formation. The material constants are shown in Table 1.

Table 1: Material Constants Used With the Elastic/Secondary Creep Model

PARAMETER	VALUE
G	992. MPa
K	1656 MPa
A	5.79×10^{-36} $Pa^{-4.9}sec^{-1}$
n	4.9
$Q/(RT)$	20.13

The material models and constants used for the waste and crushed salt backfill were identical to those used in earlier studies [9,10]. The crushed salt backfill model was developed by Sjaardema and Krieg [11] based on data from creep-consolidation experiments on crushed salt. In this material model, creep is included in both the volumetric and the deviatoric response. The form of the model, shown in the following equations,

$$G = G_0 exp(G_1 \rho) \qquad \text{(EQ 6)}$$

$$K = K_0 exp(K_1 \rho) \qquad \text{(EQ 7)}$$

$$\dot{s}_{ij} = 2G\dot{e}_{ij} - 2GA(\rho_{intact}/\rho)^n exp\left(-\frac{Q}{RT}\right)(s_{kl}s_{kl})^{\frac{(n-1)}{2}} s_{ij} \qquad \text{(EQ 8)}$$

$$\dot{\rho}_{creep} = B_0 (exp(B_1 \rho) - 1) exp(A^* \rho) \qquad \text{(EQ 9)}$$

is such that the mechanical response of the crushed salt becomes identical to that of the intact salt as the density of the backfill approaches the density of the intact salt. The variables in Equations 6 through 9 not previously defined are material constants B_0 and B_1 obtained from the creep consolidation experiments and the density ρ computed from the equation:

$$\rho = \rho_0 exp\left(\int_{t_0}^{t} \dot{e}_v dt\right) \qquad \text{(EQ 10)}$$

where ρ_0 is the density at time t_0. The intact density of the salt appears in Equation 8 as ρ_{intact}. The elastic moduli are assumed to depend on the density of the backfill through the relationships shown in Equations 6 and 7. The constants K_0 and K_1 were determined by using the least squares method to fit the modulus data to the function in Equation 6. In the fit, the function was constrained so that the bulk modulus of the crushed salt was equal to the bulk modulus of the intact salt when the crushed salt was fully compacted. No experiments have been conducted to determine how the shear modulus varies with density, so the shear modulus was assumed to vary according to the same exponential form as the bulk modulus. The constant G_0 was selected so that the shear modulus for the crushed salt was equal to that of the intact salt when the crushed salt was fully consolidated, and the constant G_1 was assumed to be the same as K_1. Because the shear and bulk moduli of the intact salt were divided by 12.5, G_0 and K_0 were divided by the same factor. Table 2 lists the values of the creep constants and elastic constants used for the backfill material.

Table 2: Material Constants Used With the Crushed Salt Backfill Model

PARAMETER	VALUE
G_0	864 Pa

Table 2: Material Constants Used With the Crushed Salt Backfill Model

PARAMETER	VALUE
G_1	6.53×10^{-3} m^3/kg
K_0	1.41×10^3 Pa
K_1	6.53×10^{-3} m^3/kg
A	5.79×10^{-36} Pa$^{-4.9}$sec^{-1}
n	4.9
Q/RT	20.13
A^*	-17.3×10^{-3} m^3/kg
B_0	1.3×10^8 kg (m^3 sec)$^{-1}$
B_1	0.82×10^{-6} Pa^{-1}

The stress-strain behavior of the waste was represented by a volumetric plasticity model [5] with a piecewise linear function defining the relationship between the mean stress and the volumetric strain. Compaction experiments on simulated waste were used to develop this relationship. The deviatoric response of the waste material has not been characterized. It is anticipated that when a drum filled with loosely compacted waste is compressed axially, the drum will not undergo significant lateral expansion until most of the void space inside the drum has been eliminated.

For the volumetric plasticity model, the yield surface in principal stress space is a surface of revolution with its axis centered about the hydrostat and the open end pointing into the compression direction. The open end is capped with a plane which is at right angles to the hydrostat. The deviatoric part is elastic-perfectly plastic so the surface of revolution is stationary in stress space. The volumetric part has variable strain hardening so the end plane moves outward during volumetric yielding. The volumetric hardening is defined by a set of pressure-volumetric strain relations. A flow rule is used such that deviatoric strains produce no volume change (associated flow). The model is best broken into volumetric and deviatoric parts with the deviatoric part resembling conventional plasticity. The volumetric yield function is a product of two functions describing the surface of revolution and the plane normal to the pressure axis. These are given by

$$\phi_s = \frac{1}{2} s_{ij} s_{ij} - (a_0 + a_1 p + a_2 p^2) \qquad \text{(EQ 11)}$$

$$\phi_p = p - g(e_v) \qquad \text{(EQ 12)}$$

where a_0, a_1, a_2 are constants defining the yield surface, p is the pressure, and e_v is the volume strain. The form of g is defined in this problem by a set of piecewise linear segments relating pressure-volume strain. Table 3 lists the pressure-volumetric strain data

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Table 3: Pressure-Volumetric Strain Data Used in the Volumetric Plasticity Model for the Waste Drums

PRESSURE (MPa)	$\ln(\rho/\rho_0)$
0.028	0.032
0.733	0.741
1.133	0.898
1.667	1.029
2.800	1.180
10.17	1.536

used for the waste drum model. The elastic material parameters and constants defining the yield surface are given in Table 4.

Table 4: Material Constants Used With the Volumetric Plasticity Model for the Waste

PARAMETER	VALUE
G	333 MPa
K	222 MPa
a_0	0.
a_1	0.
a_2	3.

The three SANCHO analyses were carried out to different simulation times depending on how far the calculation ran before reaching a specified time limit on the computer. These will be indicated in the appropriate results section. The input file for one of the SANCHO analyses is included in Appendix II. The other files are identical except for the title line, and the fact that a different external gas content history file was read for each run (this feature was internal to the code and did not change the input file).

Results of the Analyses

The results of interest are the pressure buildup in the room and the corresponding disposal room porosity. It is these quantities that will be compared with the PA calculational results to assess if the results derived from the earlier disposal room closure calculations are being correctly incorporated in the PA analyses.

Run #18

As previously noted, BRAGFLO Run #18 represents the case of a human intrusion into a practically dry (low brine saturation) panel. This SANCHO calculation was carried out for 2200 years of simulation time. Figure 3 shows the gas pressure within the disposal room computed by SANCHO on the basis of the gas content history coming from Run #18. The pressure increases quickly to its maximum value of slightly greater than 2.0 MPa at the time that the human intrusion occurs at 1000 years. The effect of the human intrusion is to suddenly drop the pressure from this maximum to a lower value of around 1.3 MPa. Thereafter, the pressure remains around this lower value for the remainder of the simulation. Shown as well in the figure is a plot of the column labelled "PRESWAST" coming from Table I of Appendix I. This quantity is the waste gas pressure in Pascals computed by BRAGFLO. It can be seen that the maximum gas pressure computed with BRAGFLO is slightly greater than 1.8 MPa. This curve follows the same trend as that for the SANCHO run in that after the human intrusion occurs, the pressure drops to a value that remains relatively steady at a value slightly greater than 1.1 MPa. Thus, there is a difference of approximately 10 % between the SANCHO and the BRAGFLO gas pressure results.

Figure 4 shows the corresponding room porosity history computed by SANCHO. The porosity drops quickly as the room closes and continues to decrease throughout the simulation, reaching a value of about 0.22 at the end. The rate of decrease has slowed down significantly by this time, and the porosity will thus probably level off at around that value. The room pressure at the time of the human intrusion is such that its effect on the porosity history is almost imperceptible, appearing only as a slight blip at 1000 years on this curve. Shown in the figure also is a porosity value computed from the BRAGFLO results provided in Appendix I. This porosity value was computed by using the following equation,

$$\varphi = (PORVOLW/12.65)/(PORVOLW/12.65 + 1229) \qquad \text{(EQ 13)}$$

where "PORVOLW" is the waste pore volume in cubic meters coming from the column labelled as such in Table I of Appendix I, 12.65 is the number of rooms per panel, and 1229 is the volume of solids in the room. The value of porosity from BRAGFLO computed on this basis is seen to asymptote to a value slightly greater than 0.26. Thus, the minimum value of porosity computed with BRAGFLO appears to be about 18 % greater than the minimum computed with SANCHO.

Run #42

BRAGFLO Run #42 represents an undisturbed, practically dry (low brine saturation) repository. The SANCHO calculation in this case was carried out for about 3500 years. Figure 5 shows the computed room gas pressure history from SANCHO. The room pressure rises quickly for about the first 350 to 400 years of the simulation and reaches a value of around 9.0 MPa. Thereafter, it slowly decreases for about 500 years and then slowly increases for the remainder of the simulation period reaching a value just below 9.5 MPa at that time. The BRAGFLO results ("PRESWAST" from Table II of Appendix I) are also seen in the figure. This curve exhibits a more gradual increase with time, earlier,

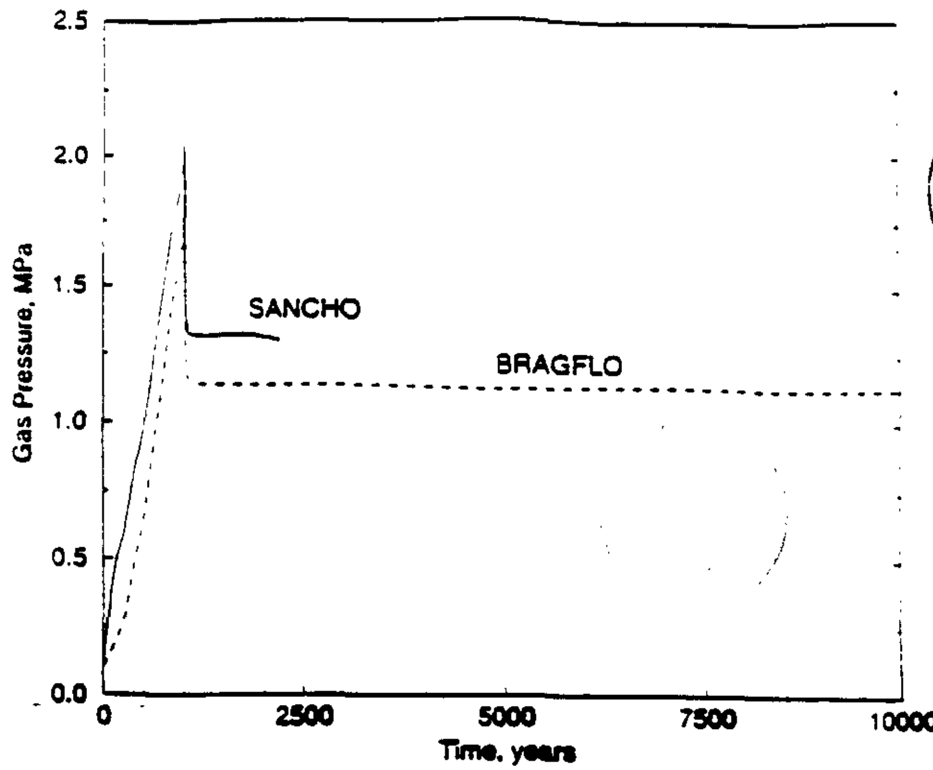

Figure 3. Comparison of Gas Pressure Histories for Run #18

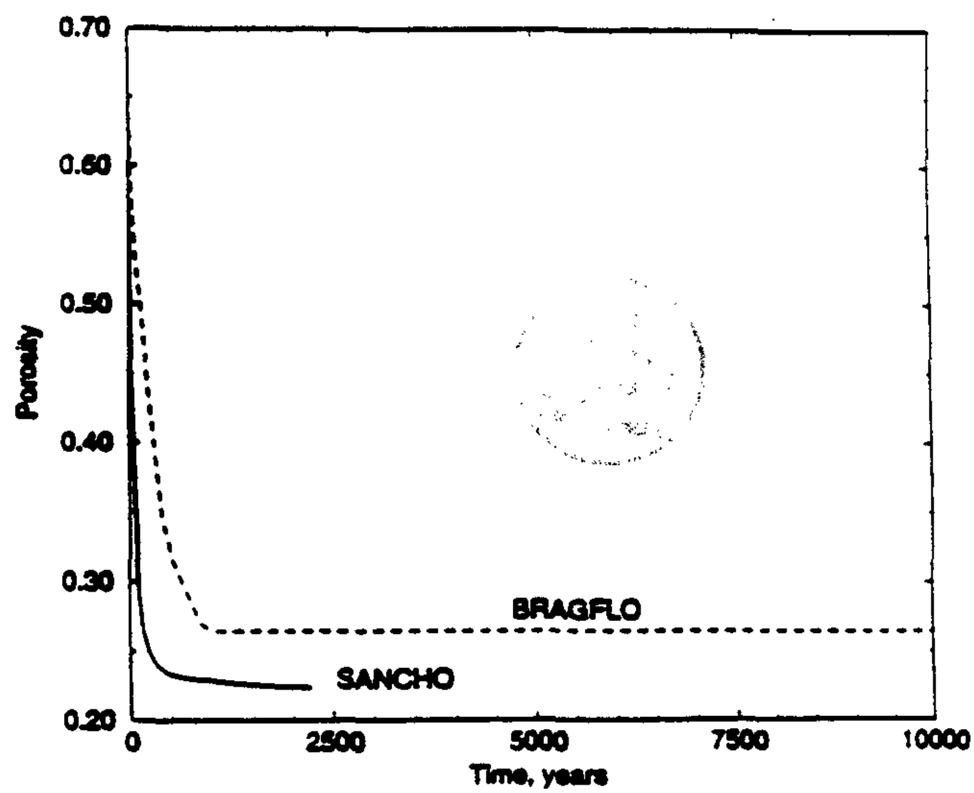

Figure 4. Comparison of Room Porosity Histories for Run #18

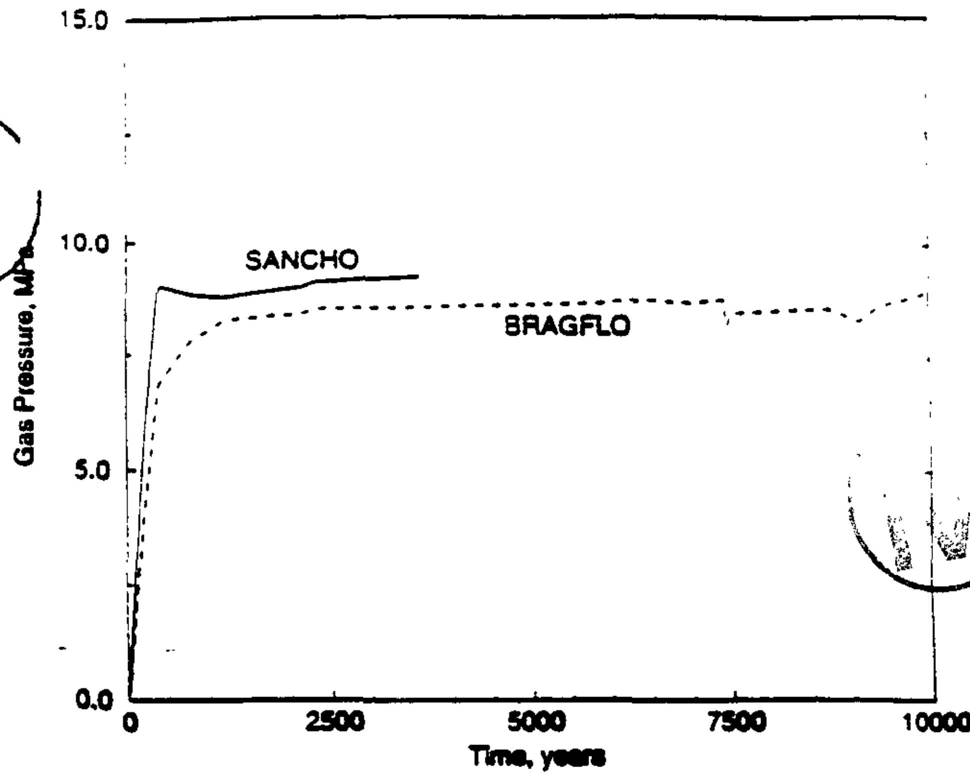

Figure 5. Comparison of Gas Pressure Histories for Run #42

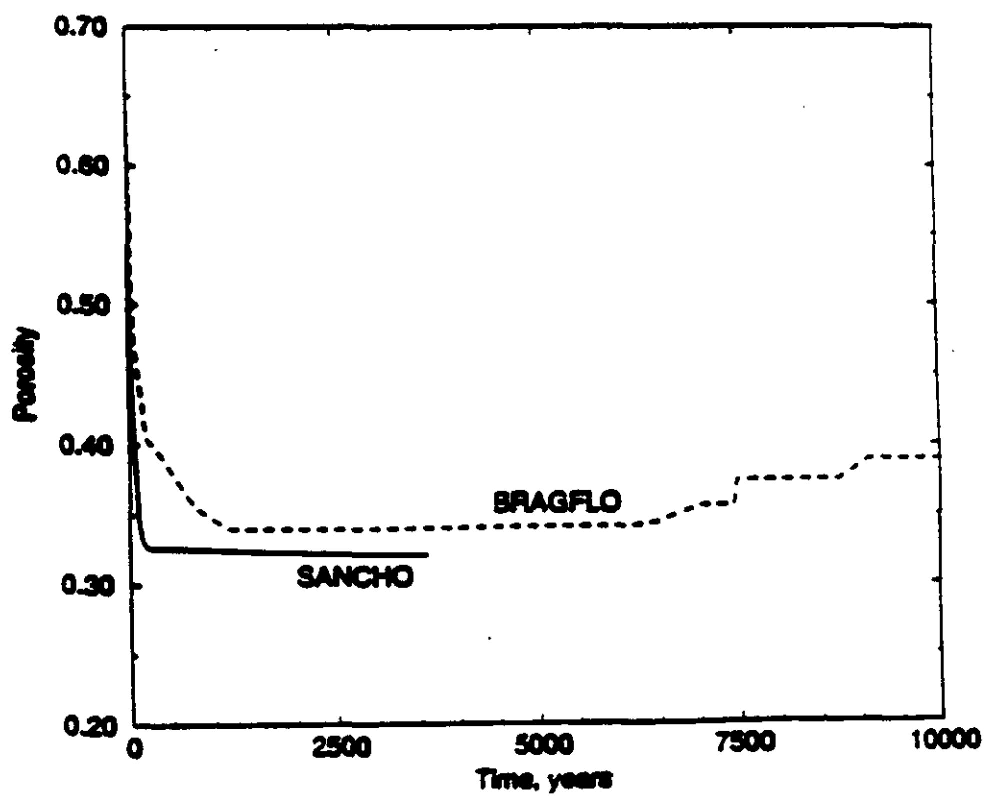

Figure 6. Comparison of Room Porosity Histories for Run #42

to a value of about 8.5 MPa at 3500 years. The pressure increases with time thereafter, but only to a value slightly greater than 8.5 MPa. Once again, as was the case with Run #18, the pressure computed with BRAGFLO remains smaller than that computed with SANCHO. At 3500 years, the BRAGFLO value is approximately 9 % smaller than that computed with SANCHO.

Figure 6 shows the corresponding room porosity histories for both SANCHO and BRAGFLO. For SANCHO, the porosity drops quickly from its initial value to a value of about 0.33 within the first 200 to 250 years. Thereafter, it drops by an extremely small amount for the remainder of the calculation to a value slightly greater than 0.32 at 3500 years. The BRAGFLO results come from using "PORVOLW" in Table II of Appendix I and Equation 13 with 119.64 rooms per repository, instead of the 12.65 rooms per panel. The porosity in this case is seen to decrease more slowly to a value of about 0.34 at around 150 years, remaining practically constant for the next 6000 years. Thus, at 3500 years, the BRAGFLO result is approximately 5 % larger than that for SANCHO.

Run #63

BRAGFLO Run #63 represents a human intrusion into a panel that becomes largely brine saturated. The SANCHO calculation for this case was carried out for about 8800 years. Figure 7 shows the computed room gas pressure histories for both SANCHO and BRAGFLO. For the SANCHO curve, the pressure rises to a maximum value of about 12.5 MPa in 1000 years, at which time the human intrusion event occurs. The human intrusion results in a tremendous drop in the pressure to a minimum value of about 0.15 MPa at about 1200 years. Thereafter, the pressure increases by a small amount to a value of about 1.25 MPa at about 1700 years and remains constant through the end of the simulation. The BRAGFLO results ("PRESWAST" from Table III of Appendix I) show a similar trend, except that the pressure rises more quickly initially, drops less after the human intrusion, and rises to a much larger value subsequently. Thus, the BRAGFLO curve shows a rise to about 15 MPa in about 300 to 400 years, slowly decreasing by a slight amount until the human intrusion event occurs at 1000 years. The human intrusion results in a drop to a value of about 4.5 MPa at about 1200 years. Thereafter, the pressure increases by a significant amount to a value of about 11.7 MPa at about 1700 years and remains constant. Thus, at the end of the SANCHO simulation, at 8800 years, the BRAGFLO gas pressure is about 8.5 times bigger than that for SANCHO.

Figure 8 shows the corresponding room porosity histories for both SANCHO and BRAGFLO. For the SANCHO curve, the room porosity quickly drops to a value of about 0.27 at about 250 years and increases very slightly until the human intrusion event occurs. The effect of the human intrusion is to drop the pressure by a tremendous amount allowing the room to close considerably, leading to a significant decrease in the room porosity. Thus, within the next 1500 years, the porosity drops to a value slightly over 0.22. Thereafter the decrease in porosity slows considerably and appears to asymptote to the 0.22 value. The BRAGFLO results come from using "PORVOLW" in Table III of Appendix I and Equation 13. The porosity in this case is seen to drop quickly to a value of about 0.36 at about 250 years, increase quickly to a value of about 0.39 at 400 years, and remain fairly

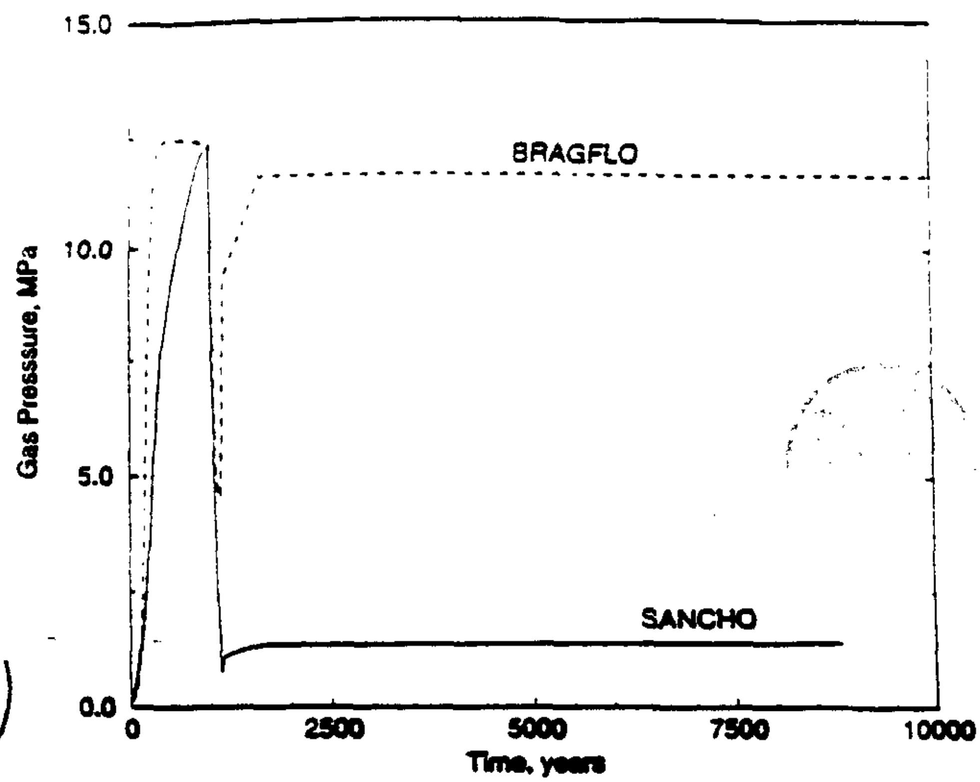

Figure 7. Comparison of Gas Pressure Histories for Run #63

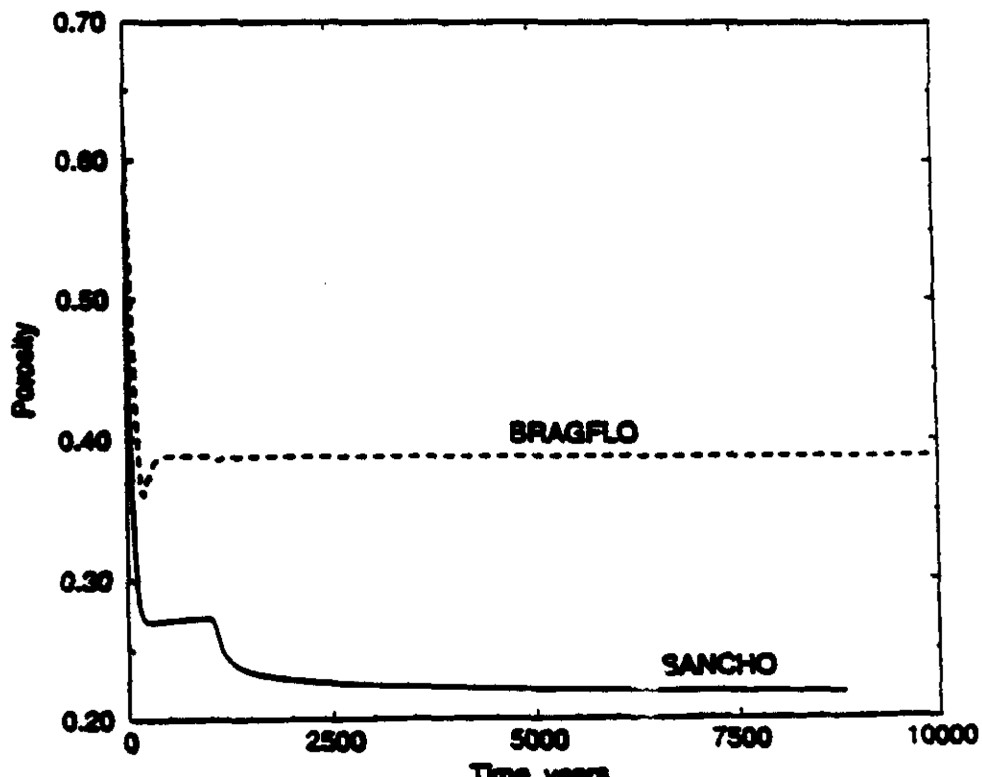

Figure 8. Comparison of Room Porosity Histories for Run #63

constant thereafter, except for a minute blip during the human intrusion. Thus, at 8800 years, the BRAGFLO value of room porosity is about 77 % greater than the value computed with SANCHO.

Summary and Conclusions

Because recasting of the calculational results for closure of a disposal room model into suitable input for use in performance assessment compliance analyses requires various simplifications and assumptions, all of which have the potential of introducing uncertainty into the results, several calculations to check the validity of the recasting procedure have been performed. The process began by recovering information on the variation in gas content and porosity with time around the waste/backfill region of a disposal room from a performance assessment compliance calculation. This information included the effects of closure of the room indirectly via the concept of a "porosity surface." The gas versus time data was then used as the gas-generation input for a disposal room closure calculation with the SANCHO computer code. The results were then compared with the original porosity history recovered from the performance assessment calculation.

Three cases were investigated: a human intrusion into a practically dry (low brine saturation) panel – Run #18; an undisturbed, practically dry (low brine saturation) repository – Run #42; and a human intrusion into a panel that becomes largely brine saturated – Run #63. Satisfactory agreement was observed between the performance assessment porosity histories and the more detailed histories computed with SANCHO for repository environments with low brine saturations. Results differed, however, for the third case when saturation of the repository occurred.

The discrepancy for the third case may be due to the assumption in the SANCHO calculations, that all void volume in the room is occupied by gas (and this has always been the case, even in the original calculations used to generate the porosity surface), whereas the BRAGFLO calculations explicitly include brine saturation. For the case when the room becomes largely brine saturated, this effect may prove significant. A future calculation for this case using SANCHO is planned in which the void volume in the room at any time will be reduced by the volume of brine in the room (this information will also come from the BRAGFLO run). This calculation should provide a resolution to the discrepancy.

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Appendix I

Results From PA's 1992 Compliance Analysis Using BRAGFLO – Transmittal From B. M. Butcher, 6345, of 7/19/93 (Data Provided by Jim Bean, 6342)

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----------------------------------------------------------------------
THE DATA IN FILES GROVE_018.DAT, GROVE_042.DAT, AND GROVE_061.DAT
IS OUTPUT FROM BRAGFLO. FILES ARE IN DIRECTORY U7:[JSBEAN.BUTCHER]
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VARIABLE    DEFINITION
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N         = DATABASE STEP NUMBER
TIME      = ELAPSED TIME IN SECONDS
GASGENV   = GAS GENERATION RATE IN CUBIC METERS/SEC
GASGENM   = GAS GENERATION RATE IN MOLES/SEC
SBAVW     = AVERAGE BRINE SATURATION IN WASTE
PORVOLW   = WASTE PORE VOLUME (GAS AND BRINE) IN CUBIC METERS
PRESWAST  = WASTE (GAS) PRESSURE IN PASCALS
POROSITY  = POROSITY OF WASTE (VOID VOLUME/TOTAL VOLUME)
GASVOLW   = GAS VOLUME IN WASTE (CUBIC METERS AT P=101325 PA AND T=300.15K)
GASVOL    = GAS VOLUME AT RESERVOIR CONDITIONS (CUBIC METERS AT P=PRESWAST)
GASGENC   = CUMULATIVE GAS GENERATED IN WASTE (CUBIC METERS AT P=101325)
          = INTEGRATED VALUES OF GASGENV ACCORDING TO FOLLOWING:
            GASGENC(T=0) = 0
            GASGENC(T) = GASGENC(T-DT) + 0.5*DT*(GASGENV(T) + GASGENV(T-DT))
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Table I. Grope File From PA's Compliance Analysis - Run # 18 (GROPE_018.DAT)

Step	Time	GASGENV	GASGENM	SBAVN	PORVOLM	PRESMAST	POROSITY	GASVOLM	GASVOL	GASGENC
1	0.00000E+00	0.00000E+00	0.00000E+00	6.29700E-02	3.04292E+04	1.01325E+05	6.60170E-01	2.05131E+04	2.05131E+04	0.00000E+00
2	1.13329E+06	4.06600E-06	1.66306E-04	6.29700E-02	3.04090E+04	1.01336E+05	6.59740E-01	2.04971E+04	2.04941E+04	1.30390E+00
3	4.70195E+08	4.06661E-06	1.66331E-04	6.29932E-02	2.65172E+04	1.07001E+05	5.75305E-01	2.66535E+04	2.40468E+04	1.90965E+03
4	1.11396E+09	4.06733E-06	1.66361E-04	6.30213E-02	2.33235E+04	1.10097E+05	5.06016E-01	2.56610E+04	2.10536E+04	4.52783E+03
5	1.80554E+09	4.06787E-06	1.66303E-04	6.30410E-02	2.06610E+04	1.31766E+05	4.63913E-01	2.49235E+04	1.91711E+04	7.34090E+03
6	2.36763E+09	4.06619E-06	1.66396E-04	6.30544E-02	1.82207E+04	1.44604E+05	3.95402E-01	2.43017E+04	1.70793E+04	9.62750E+03
7	7.88820E+09	4.08360E-06	1.67030E-04	6.36509E-02	1.15444E+04	2.61072E+05	2.50462E-01	2.79113E+04	1.00695E+04	3.21336E+04
8	1.21777E+10	4.16903E-06	1.69702E-04	6.61603E-02	8.21401E+03	4.92693E+05	1.78225E-01	3.72176E+04	7.67125E+03	4.97670E+04
9	1.50046E+10	4.17600E-06	1.70006E-04	6.72072E-02	7.46059E+03	6.03958E+05	1.61062E-01	4.13612E+04	6.95918E+03	6.15540E+04
10	1.57700E+10	4.18510E-06	1.72282E-04	6.79613E-02	7.24530E+03	6.45509E+05	1.57191E-01	4.29045E+04	6.75500E+03	6.47073E+04
11	2.75390E+10	4.31975E-06	1.76608E-04	7.27443E-02	5.71315E+03	1.45597E+06	1.23950E-01	7.55319E+04	5.29755E+03	1.14801E+05
12	3.02437E+10	4.33594E-06	1.77367E-04	7.33670E-02	5.63526E+03	1.50357E+06	1.22260E-01	8.09175E+04	5.22101E+03	1.26073E+05
13	3.15570E+10	4.36544E-06	1.77736E-04	7.37339E-02	5.59177E+03	1.69360E+06	1.21317E-01	8.57035E+04	5.17947E+03	1.32208E+05
14	3.15570E+10	4.36030E-06	1.77083E-04	7.30439E-02	5.58991E+03	1.76783E+06	1.21276E-01	8.86634E+04	5.17713E+03	1.32208E+05
15	3.15571E+10	4.36033E-06	1.77856E-04	7.38561E-02	5.58991E+03	1.76780E+06	1.21276E-01	8.86646E+04	5.17706E+03	1.32208E+05
16	3.15585E+10	4.35379E-06	1.78077E-04	7.40753E-02	5.58992E+03	1.76831E+06	1.21276E-01	8.86652E+04	5.17584E+03	1.32215E+05
17	3.15957E+10	4.49600E-06	1.83927E-04	7.95840E-02	5.59003E+03	1.76130E+06	1.21279E-01	8.85069E+04	5.14515E+03	1.32379E+05
18	3.17401E+10	5.19671E-06	2.12556E-04	1.06843E-01	5.59065E+03	1.83053E+06	1.21292E-01	8.93440E+04	4.99500E+03	1.33118E+05
19	3.17622E+10	5.24560E-06	2.16557E-04	1.08420E-01	5.59070E+03	1.83543E+06	1.21293E-01	8.93943E+04	4.98451E+03	1.33191E+05
20	3.17627E+10	5.24609E-06	2.16574E-04	1.08444E-01	5.59070E+03	1.83563E+06	1.21293E-01	8.93932E+04	4.98442E+03	1.33194E+05
21	3.17680E+10	5.24942E-06	2.16710E-04	1.08570E-01	5.59060E+03	1.83400E+06	1.21293E-01	8.93120E+04	4.98370E+03	1.33222E+05
22	3.18146E+10	5.23682E-06	2.16195E-04	1.08062E-01	5.59014E+03	1.77293E+06	1.21281E-01	8.64072E+04	4.98595E+03	1.33466E+05
23	3.23069E+10	5.03436E-06	2.05914E-04	1.00202E-01	5.58564E+03	1.26930E+06	1.21184E-01	6.25330E+04	5.02550E+03	1.35994E+05
24	3.31698E+10	4.97472E-06	2.03476E-04	9.79843E-02	5.58459E+03	1.15282E+06	1.21161E-01	5.69674E+04	5.03739E+03	1.40213E+05
25	3.77456E+10	5.00173E-06	2.04579E-04	9.90246E-02	5.58447E+03	1.13904E+06	1.21150E-01	5.62264E+04	5.03147E+03	1.63130E+05
26	5.44325E+10	5.15152E-06	2.10706E-04	1.06786E-01	5.58447E+03	1.13071E+06	1.21150E-01	5.58405E+04	4.99925E+03	2.47769E+05
27	5.87056E+10	5.20810E-06	2.16173E-04	1.09943E-01	5.58447E+03	1.13921E+06	1.21150E-01	5.55515E+04	4.87050E+03	2.70152E+05
28	7.55754E+10	6.01296E-06	2.45939E-04	1.37987E-01	5.56456E+03	1.14206E+06	1.21159E-01	5.39369E+04	4.81391E+03	3.65451E+05
29	1.20135E+11	6.20500E-06	2.57103E-04	1.93093E-01	5.50450E+03	1.14272E+06	1.21159E-01	5.06659E+04	4.50170E+03	5.77972E+05
30	1.44695E+11	3.97181E-06	1.62454E-04	2.45204E-01	5.50441E+03	1.13181E+06	1.21157E-01	4.60047E+04	4.21509E+03	7.55225E+05
31	1.79255E+11	3.83696E-06	1.56930E-04	2.93600E-01	5.50440E+03	1.13609E+06	1.21157E-01	4.37633E+04	3.94437E+03	8.90160E+05
32	1.91797E+11	3.96659E-06	1.62240E-04	3.13292E-01	5.50443E+03	1.13400E+06	1.21157E-01	4.25600E+04	3.83407E+03	9.39096E+05
33	2.00000E+11	4.10017E-06	1.67704E-04	3.24422E-01	5.50443E+03	1.13472E+06	1.21157E-01	4.19017E+04	3.77272E+03	9.72215E+05
34	2.25022E+11	4.32622E-06	1.76950E-04	3.55492E-01	5.50444E+03	1.13560E+06	1.21150E-01	4.00060E+04	3.59922E+03	1.07760E+06
35	2.59562E+11	2.70797E-06	1.10761E-04	3.99322E-01	5.50437E+03	1.12707E+06	1.21156E-01	3.70332E+04	3.35441E+03	1.19915E+06
36	2.94142E+11	2.96682E-06	1.21348E-04	4.40449E-01	5.50438E+03	1.12803E+06	1.21156E-01	3.45276E+04	3.12475E+03	1.29721E+06
37	3.15570E+11	3.12624E-06	1.27869E-04	4.63740E-01	5.50439E+03	1.12961E+06	1.21156E-01	3.29619E+04	2.90329E+03	1.36250E+06

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Table II. Grope File From PA's Compliance Analysis - Run # 42 (GROPE_042.DAT)

#Step	Time	GASGENW	GASGENM	SBRAVW	PORVOLM	PRESWAST	POROSITY	GASVOLM	GASVOL	GASGENC
1	0.00000E+00	0.00000E+00	0.00000E+00	1.99600E-02	2.91726E+05	1.01325E+05	6.60178E-01	2.05909E+05	2.05909E+05	0.00000E+00
2	7.02000E+03	6.66126E-04	2.63650E-02	1.99681E-02	2.91724E+05	1.01327E+05	6.60173E-01	2.05913E+05	2.05907E+05	2.26080E+00
3	2.22192E+06	6.44253E-04	2.63511E-02	2.00629E-02	2.90170E+05	1.02339E+05	6.56637E-01	2.07197E+05	2.04354E+05	1.62907E+03
4	4.16916E+06	6.44600E-04	2.63603E-02	2.02236E-02	2.87526E+05	1.03602E+05	6.50673E-01	2.08259E+05	2.01711E+05	1.60300E+04
5	3.66235E+07	6.45823E-04	2.64030E-02	2.10537E-02	2.75626E+05	1.13160E+05	6.33743E-01	3.01287E+05	2.69023E+05	1.71669E+04
6	3.12879E+08	6.40123E-04	2.65093E-02	2.31236E-02	2.44416E+05	2.00975E+05	5.53115E-01	4.73320E+05	2.30764E+05	2.02130E+05
7	6.54067E+08	6.50709E-04	2.66151E-02	2.51023E-02	2.17207E+05	3.24135E+05	4.91561E-01	6.76671E+05	2.11737E+05	4.24482E+05
8	1.04902E+09	6.55623E-04	2.68161E-02	2.80939E-02	1.80774E+05	5.21613E+05	4.09093E-01	9.01010E+05	1.75515E+05	6.01864E+05
9	2.35755E+09	6.62580E-04	2.70977E-02	3.45751E-02	1.31744E+05	1.30063E+06	2.98130E-01	1.62141E+06	1.27109E+05	1.54427E+06
10	5.00105E+09	6.52453E-04	2.66064E-02	3.65700E-02	1.01772E+05	3.64055E+06	2.30313E-01	3.40796E+06	9.90609E+04	3.00803E+06
11	6.31140E+09	6.40753E-04	2.65760E-02	2.66310E-02	9.86540E+04	4.00602E+06	2.25519E-01	3.75603E+06	9.72211E+04	4.14060E+06
12	1.22170E+10	0.00000E+00	0.00000E+00	0.00000E+00	9.39203E+04	6.04900E+06	2.12542E-01	6.10912E+06	9.39203E+04	6.05945E+06
13	2.44862E+10	0.00000E+00	0.00000E+00	0.00000E+00	8.09603E+04	7.79779E+06	2.03214E-01	5.96200E+06	8.09603E+04	6.05945E+06
14	3.75690E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56201E+04	8.27216E+06	1.71129E-01	5.89249E+06	7.56201E+04	6.05945E+06
15	3.99660E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56216E+04	8.30925E+06	1.71132E-01	5.90422E+06	7.56216E+04	6.05945E+06
16	4.25932E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56233E+04	8.32250E+06	1.71136E-01	5.92019E+06	7.56233E+04	6.05945E+06
17	4.50766E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56251E+04	8.33076E+06	1.71140E-01	5.93266E+06	7.56251E+04	6.05945E+06
18	4.67993E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56260E+04	8.33796E+06	1.71142E-01	5.93761E+06	7.56260E+04	6.05945E+06
19	4.89305E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56266E+04	8.34637E+06	1.71144E-01	5.94330E+06	7.56266E+04	6.05945E+06
20	5.13920E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56275E+04	8.35784E+06	1.71146E-01	5.95103E+06	7.56275E+04	6.05945E+06
21	5.40049E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56289E+04	8.37235E+06	1.71149E-01	5.96105E+06	7.56289E+04	6.05945E+06
22	5.69534E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56302E+04	8.38779E+06	1.71152E-01	5.97177E+06	7.56302E+04	6.05945E+06
23	5.96909E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56313E+04	8.40052E+06	1.71154E-01	5.98040E+06	7.56313E+04	6.05945E+06
24	6.21972E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56320E+04	8.40921E+06	1.71156E-01	5.98643E+06	7.56320E+04	6.05945E+06
25	6.52203E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56328E+04	8.41810E+06	1.71158E-01	5.99253E+06	7.56328E+04	6.05945E+06
26	6.86493E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56358E+04	8.44652E+06	1.71163E-01	6.01061E+06	7.56358E+04	6.05945E+06
27	7.01707E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56390E+04	8.48702E+06	1.71172E-01	6.03973E+06	7.56390E+04	6.05945E+06
28	7.32926E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56425E+04	8.52396E+06	1.71179E-01	6.06503E+06	7.56425E+04	6.05945E+06
29	7.58342E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56437E+04	8.53596E+06	1.71182E-01	6.07322E+06	7.56437E+04	6.05945E+06
30	9.69788E+10	0.00000E+00	0.00000E+00	0.00000E+00	7.56866E+04	8.56338E+06	1.71279E-01	6.09520E+06	7.56866E+04	6.05945E+06
31	1.01200E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.57163E+04	8.56712E+06	1.71346E-01	6.10021E+06	7.57163E+04	6.05945E+06
32	1.05570E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.57459E+04	8.57084E+06	1.71413E-01	6.10512E+06	7.57459E+04	6.05945E+06
33	1.08890E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.57730E+04	8.57601E+06	1.71477E-01	6.11087E+06	7.57730E+04	6.05945E+06
34	1.11540E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.58019E+04	8.58265E+06	1.71540E-01	6.11764E+06	7.58019E+04	6.05945E+06
35	1.14383E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.58319E+04	8.58971E+06	1.71600E-01	6.12405E+06	7.58319E+04	6.05945E+06
36	1.17090E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.58598E+04	8.59672E+06	1.71671E-01	6.13106E+06	7.58598E+04	6.05945E+06
37	1.19962E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.58898E+04	8.60290E+06	1.71739E-01	6.13853E+06	7.58898E+04	6.05945E+06

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Table II. (Continued)

38	1.22976E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.59216E+04	8.60892E+06	1.71811E-01	6.14515E+06	7.59216E+04	6.05945E+06
39	1.25081E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.59514E+04	8.61559E+06	1.71879E-01	6.15209E+06	7.59514E+04	6.05945E+06
40	1.28924E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.59834E+04	8.62144E+06	1.71951E-01	6.15866E+06	7.59834E+04	6.05945E+06
41	1.32867E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.60154E+04	8.62776E+06	1.72023E-01	6.16555E+06	7.60154E+04	6.05945E+06
42	1.35124E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.60451E+04	8.63374E+06	1.72091E-01	6.17202E+06	7.60451E+04	6.05945E+06
43	1.38568E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.60747E+04	8.63770E+06	1.72158E-01	6.17713E+06	7.60747E+04	6.05945E+06
44	1.42443E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.61630E+04	8.63702E+06	1.72357E-01	6.18383E+06	7.61630E+04	6.05945E+06
45	1.50670E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.62248E+04	8.64163E+06	1.72497E-01	6.19199E+06	7.62248E+04	6.05945E+06
46	1.84281E+11	0.00000E+00	0.00000E+00	0.00000E+00	7.62290E+04	8.68034E+06	1.72507E-01	6.22416E+06	7.62290E+04	6.05945E+06
47	1.89608E+11	4.57128E-06	1.06973E-04	1.37743E-06	7.62410E+04	8.71829E+06	1.72534E-01	6.24554E+06	7.62469E+04	6.07181E+06
48	1.91660E+11	4.57437E-06	1.07108E-04	3.03938E-06	7.62747E+04	8.73084E+06	1.72610E-01	6.25684E+06	7.62744E+04	6.08028E+06
49	1.93082E+11	4.57076E-06	1.06982E-04	9.60738E-07	7.63019E+04	8.73712E+06	1.72672E-01	6.26337E+06	7.63018E+04	6.08733E+06
50	1.96376E+11	5.05302E-06	2.06710E-04	4.95682E-06	7.65852E+04	8.74718E+06	1.73313E-01	6.29340E+06	7.65848E+04	6.10357E+06
51	2.00552E+11	5.06009E-06	2.06966E-04	1.75892E-05	7.69870E+04	8.75493E+06	1.74222E-01	6.33174E+06	7.69856E+04	6.12469E+06
52	2.05026E+11	3.79184E-05	1.14180E-03	2.77460E-05	7.74376E+04	8.76074E+06	1.75242E-01	6.37275E+06	7.74355E+04	6.19861E+06
53	2.07996E+11	1.59044E-04	6.53700E-03	4.27287E-05	7.81363E+04	8.76162E+06	1.76823E-01	6.43063E+06	7.81330E+04	6.47741E+06
54	2.12318E+11	1.09049E-04	4.40299E-03	2.65027E-04	7.94408E+04	8.71721E+06	1.79775E-01	6.50518E+06	7.94197E+04	7.05916E+06
55	2.16003E+11	9.07725E-06	4.03996E-04	2.43626E-04	8.02076E+04	8.72327E+06	1.81510E-01	6.57245E+06	8.01081E+04	7.32810E+06
56	2.21460E+11	1.32054E-04	5.43396E-03	4.06403E-04	8.13120E+04	8.68489E+06	1.84010E-01	6.63396E+06	8.12789E+04	7.65903E+06
57	2.23581E+11	5.72608E-06	2.34207E-04	3.83560E-04	8.13120E+04	8.71426E+06	1.84010E-01	6.65567E+06	8.12808E+04	7.80730E+06
58	2.25807E+11	5.71962E-06	2.33934E-04	4.46178E-04	8.13120E+04	8.73086E+06	1.84010E-01	6.66710E+06	8.12757E+04	7.82012E+06
59	2.28031E+11	5.71213E-06	2.33636E-04	5.06167E-04	8.13120E+04	8.74725E+06	1.84010E-01	6.67859E+06	8.12707E+04	7.83283E+06
60	2.31215E+11	1.60951E-06	6.91037E-05	7.28901E-04	8.13120E+04	8.75901E+06	1.84010E-01	6.68570E+06	8.12536E+04	7.84461E+06
61	2.34349E+11	1.76065E-06	7.23409E-05	9.61470E-04	8.13120E+04	8.76092E+06	1.84010E-01	6.69128E+06	8.12338E+04	7.85003E+06
62	2.35514E+11	1.50231E-04	6.14469E-03	3.30243E-03	8.78412E+04	8.19859E+06	1.98785E-01	6.76143E+06	8.75441E+04	7.93057E+06
63	2.37434E+11	1.61106E-06	6.59271E-03	1.19129E-03	8.78412E+04	8.47029E+06	1.98785E-01	6.99257E+06	8.77366E+04	8.23753E+06
64	2.40626E+11	1.52048E-04	6.21903E-03	1.20082E-03	8.78412E+04	8.48205E+06	1.98785E-01	7.00174E+06	8.77350E+04	8.73745E+06
65	2.44028E+11	1.52052E-04	6.21917E-03	1.21064E-03	8.78412E+04	8.49302E+06	1.98785E-01	7.01093E+06	8.77349E+04	9.25472E+06
66	2.46595E+11	1.34230E-06	5.49024E-05	1.25074E-03	8.78412E+04	8.50122E+06	1.98785E-01	7.01646E+06	8.77313E+04	9.45168E+06
67	2.48862E+11	1.37159E-06	5.61002E-05	1.34415E-03	8.78412E+04	8.50730E+06	1.98785E-01	7.02050E+06	8.77231E+04	9.45460E+06
68	2.51804E+11	1.40845E-06	5.76070E-05	1.45994E-03	8.78412E+04	8.51337E+06	1.98785E-01	[illegible]454E+06	8.77130E+04	9.45766E+06
69	2.53538E+11	1.46798E-06	6.00427E-05	1.64394E-03	8.78412E+04	8.52042E+06	1.98785E-01	[illegible]02070E+06	8.76968E+04	9.46130E+06
70	2.56259E+11	1.53266E-06	6.26886E-05	1.84360E-03	8.78412E+04	8.52811E+06	1.98785E-01	7.03341E+06	8.76793E+04	9.46518E+06
71	2.58969E+11	6.92090E-07	2.83080E-05	2.06204E-03	8.78412E+04	8.53351E+06	1.98785E-01	7.03625E+06	8.76618E+04	9.46848E+06
72	2.61581E+11	7.21326E-07	2.95035E-05	2.23430E-03	8.78412E+04	8.53861E+06	1.98785E-01	7.03889E+06	8.76449E+04	9.47024E+06
73	2.64289E+11	7.51486E-07	3.07371E-05	2.43216E-03	8.78412E+04	8.54395E+06	1.98785E-01	7.04169E+06	8.76276E+04	9.47226E+06
74	2.67085E+11	7.82462E-07	3.20040E-05	2.63576E-03	8.78412E+04	8.54953E+06	1.98785E-01	7.04463E+06	8.76097E+04	9.47430E+06
75	2.69880E+11	0.00000E+00	0.00000E+00	2.83916E-03	8.78412E+04	8.55397E+06	1.98785E-01	7.04667E+06	8.75918E+04	9.47548E+06

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Table II. (Continued)

76	2.72685E+11	0.00000E+00	0.00000E+00	3.04342E-03	8.78612E+04	6.55724E+06	1.90785E-01	7.04779E+06	8.75739E+04	9.47548E+06
77	2.75407E+11	[illegible]	6.47862E-04	3.23038E-03	8.78612E+04	6.57162E+06	1.90785E-01	7.05759E+06	8.75574E+04	9.49703E+06
78	2.87636E+11	1.63623E-04	6.69262E-03	5.05550E-03	9.36562E+04	6.39933E+06	2.11945E-01	7.28375E+06	9.31627E+04	1.05764E+07
79	2.95007E+11	[illegible]	6.48650E-04	3.39571E-03	9.36562E+04	6.62021E+06	2.11945E-01	7.56420E+06	9.33382E+04	1.12550E+07
80	3.00219E+11	2.04370E-06	8.35941E-04	3.66460E-03	9.36562E+04	6.71171E+06	2.11945E-01	7.64011E+06	9.33317E+04	1.13584E+07
81	3.15570E+11	1.50014E-06	6.56306E-04	3.70045E-03	9.36603E+04	6.86930E+06	2.11954E-01	7.85308E+06	9.33137E+04	1.16293E+07

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Table III. Grope File From PA's Compliance Analysis - Run # 63 (GROPE_063.DAT)

Nstep	Time	GASGENW	GASGENM	SBAVM	PORVOLM	PRESMAST	POROSITY	GASVOLM	GASVOL	GASGENC
1	0.00000E+00	0.00000E+00	0.00000E+00	4.84400E-02	3.04292E+04	1.01325E+05	6.60178E-01	2.90465E+04	2.90465E+04	0.00000E+00
2	8.13042E+05	1.30007E-08	8.31751E-06	4.84399E-02	3.04200E+04	1.01335E+05	6.59996E-01	2.90414E+04	2.90305E+04	1.39005E+00
3	1.86374E+08	1.32744E-05	5.62946E-06	4.87819E-02	2.75072E+04	1.00903E+05	5.96784E-01	2.83993E+04	2.61651E+04	2.42083E+03
4	1.80317E+09	2.21557E-05	9.06264E-06	1.87862E-01	1.83140E+04	2.33603E+05	3.97332E-01	3.55503E+04	1.54284E+04	2.57833E+04
5	2.20402E+09	2.77981E-05	1.13659E-03	2.26665E-01	1.53804E+04	3.65414E+05	3.33035E-01	4.27105E+04	1.10710E+04	4.33084E+04
6	2.82950E+09	3.35560E-05	1.37250E-03	2.97184E-01	1.36192E+04	5.51115E+05	2.91137E-01	5.10690E+04	9.43123E+03	6.24717E+04
7	2.90060E+09	3.47564E-05	1.42160E-03	3.11086E-01	1.30901E+04	5.99612E+05	2.83997E-01	5.29262E+04	9.00740E+03	6.48934E+04
8	3.63163E+09	4.23331E-05	1.72736E-03	4.03441E-01	1.14221E+04	9.02150E+05	2.47809E-01	6.55222E+04	6.61396E+03	9.26409E+04
9	4.38494E+09	5.26130E-05	2.14361E-03	5.20130E-01	9.80349E+03	1.90984E+06	2.16645E-01	8.60736E+04	4.66036E+03	1.30245E+05
10	5.09171E+09	5.69443E-05	2.31276E-03	5.70726E-01	9.37450E+03	2.47880E+06	2.03305E-01	9.50543E+04	3.94923E+03	1.65853E+05
11	5.67662E+09	6.29032E-05	2.57600E-03	6.57572E-01	8.86031E+03	3.07430E+06	1.92403E-01	1.13426E+05	3.03676E+03	2.08810E+05
12	6.49690E+09	6.76941E-05	2.77690E-03	7.17729E-01	8.76675E+03	5.90907E+06	1.90199E-01	1.39702E+05	2.47440E+03	2.56492E+05
13	6.49867E+09	6.79016E-05	2.77729E-03	7.17820E-01	8.76675E+03	5.93752E+06	1.90199E-01	1.40049E+05	2.47380E+03	2.56607E+05
14	6.77256E+09	6.89484E-05	2.82011E-03	7.30661E-01	8.86803E+03	6.66392E+06	1.92416E-01	1.51170E+05	2.30090E+03	2.73340E+05
15	7.52783E+09	6.99416E-05	2.86073E-03	7.42005E-01	9.01890E+03	8.40036E+06	1.95672E-01	1.83438E+05	2.31964E+03	3.25796E+05
16	7.63567E+09	7.01016E-05	2.86725E-03	7.44757E-01	9.08713E+03	8.62101E+06	1.97367E-01	1.87929E+05	2.32190E+03	3.33335E+05
17	7.88920E+09	7.01477E-05	2.86916E-03	7.45129E-01	9.15496E+03	9.16936E+06	1.98622E-01	2.00181E+05	2.33150E+03	3.51128E+05
18	8.68144E+09	6.89059E-05	2.81837E-03	7.30131E-01	9.36332E+03	1.03693E+07	2.03142E-01	2.43756E+05	2.52696E+03	4.06210E+05
19	9.14681E+09	6.80805E-05	2.78662E-03	7.20817E-01	9.45553E+03	1.08807E+07	2.05143E-01	2.67238E+05	2.64739E+03	4.38064E+05
20	9.52341E+09	6.72269E-05	2.74969E-03	7.09857E-01	9.53360E+03	1.12513E+07	2.06030E-01	2.88438E+05	2.76099E+03	4.63495E+05
21	9.76116E+09	6.66852E-05	2.72754E-03	7.02923E-01	9.57703E+03	1.14665E+07	2.07779E-01	3.01103E+05	2.86512E+03	4.79479E+05
22	9.98569E+09	6.61768E-05	2.70674E-03	6.96696E-01	9.61301E+03	1.16059E+07	2.08560E-01	3.12671E+05	2.91566E+03	4.94363E+05
23	1.02420E+10	6.55059E-05	2.68250E-03	6.89460E-01	9.65161E+03	1.17560E+07	2.09397E-01	3.25324E+05	2.99721E+03	5.11202E+05
24	1.04077E+10	6.50403E-05	2.66059E-03	6.82076E-01	9.68416E+03	1.18765E+07	2.10103E-01	3.36564E+05	3.07108E+03	5.27333E+05
25	1.08664E+10	6.42327E-05	2.62723E-03	6.72087E-01	9.72765E+03	1.20276E+07	2.11067E-01	3.52808E+05	3.18284E+03	5.51681E+05
26	1.12974E+10	6.32712E-05	2.58790E-03	6.61111E-01	9.77300E+03	1.21481E+07	2.12031E-01	3.70642E+05	3.31196E+03	5.79285E+05
27	1.17052E+10	6.22461E-05	2.54597E-03	6.48557E-01	9.81432E+03	1.22424E+07	2.12927E-01	3.88790E+05	3.44917E+03	6.09899E+05
28	1.24563E+10	6.09666E-05	2.49372E-03	6.32911E-01	9.85861E+03	1.23258E+07	2.13808E-01	4.10519E+05	3.61899E+03	6.50874E+05
29	1.30760E+10	5.99046E-05	2.45004E-03	6.19031E-01	9.87022E+03	1.24108E+07	2.14140E-01	4.28354E+05	3.75223E+03	6.88616E+05
30	1.37323E+10	5.80734E-05	2.40002E-03	6.07251E-01	9.87274E+03	1.24544E+07	2.14195E-01	4.46117E+05	3.87751E+03	7.27662E+05
31	1.57780E+10	5.60059E-05	2.29090E-03	5.72181E-01	9.87413E+03	1.26789E+07	2.14225E-01	4.86757E+05	4.22434E+03	8.45171E+05
32	1.62900E+10	5.51079E-05	2.25401E-03	5.62055E-01	9.87413E+03	1.26770E+07	2.14225E-01	4.96290E+05	4.32433E+03	8.73617E+05
33	1.93649E+10	4.92261E-05	2.01343E-03	5.15267E-01	9.87413E+03	1.26619E+07	2.14225E-01	5.48690E+05	4.78632E+03	1.03290E+06
34	2.16110E+10	4.69176E-05	1.91901E-03	4.86730E-01	9.87413E+03	1.26507E+07	2.14225E-01	5.80521E+05	5.06609E+03	1.13234E+06
35	2.29364E+10	4.40936E-05	1.80358E-03	4.67366E-01	9.87413E+03	1.26392E+07	2.14225E-01	6.01907E+05	5.25930E+03	1.20145E+06
36	2.43250E+10	4.27993E-05	1.75056E-03	4.50710E-01	9.87413E+03	1.26316E+07	2.14225E-01	6.20367E+05	5.42360E+03	1.26287E+06
37	2.56704E+10	4.16507E-05	1.70359E-03	4.35940E-01	9.87413E+03	1.26263E+07	2.14225E-01	6.36793E+05	5.56952E+03	1.31085E+06

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Table III. (Continued)

38	2.69221E+10	4.05867E-05	1.66007E-03	4.22268E-01	9.07413E+03	1.24216E+07	2.14225E-01	6.52010E+05	5.70460E+03	1.37032E+06
39	2.84070E+10	3.07610E-05	1.28821E-03	4.07832E-01	9.07413E+03	1.23097E+07	2.14225E-01	6.67041E+05	5.05011E+03	1.42614E+06
40	3.15870E+10	1.78864E-05	7.31992E-04	3.09731E-01	9.07413E+03	1.23042E+07	2.14225E-01	6.02722E+05	6.02508E+03	1.50083E+06
41	3.15871E+10	1.77774E-05	7.27125E-04	3.00192E-01	9.07422E+03	1.23006E+07	2.14227E-01	6.04330E+05	6.04113E+03	1.50083E+06
42	3.15620E+10	1.77760E-05	7.27067E-04	3.00184E-01	9.07423E+03	1.23000E+07	2.14227E-01	6.04275E+05	6.04121E+03	1.50092E+06
43	3.15460E+10	1.77754E-05	7.27062E-04	3.00161E-01	9.07422E+03	1.23003E+07	2.14227E-01	6.04213E+05	6.04124E+03	1.50097E+06
44	3.15792E+10	1.77776E-05	7.27134E-04	3.00196E-01	9.07393E+03	1.22010E+07	2.14220E-01	6.03203E+05	6.04091E+03	1.50123E+06
45	3.16095E+10	1.77361E-05	7.24946E-04	3.00012E-01	9.06097E+03	1.19602E+07	2.14113E-01	6.65917E+05	6.03100E+03	1.50244E+06
46	3.16700E+10	1.77700E-05	7.26857E-04	3.09346E-01	9.06596E+03	1.17773E+07	2.14047E-01	6.55210E+05	6.02671E+03	1.50290E+06
47	3.17420E+10	1.78853E-05	7.31560E-04	3.90708E-01	9.05976E+03	1.13840E+07	2.13913E-01	6.32069E+05	6.00660E+03	1.50414E+06
48	3.21106E+10	1.80820E-05	7.72768E-04	4.06726E-01	9.83034E+03	9.81667E+06	2.13275E-01	5.19030E+05	5.83200E+03	1.51090E+06
49	3.26004E+10	2.85061E-05	1.16922E-03	5.32203E-01	9.77324E+03	8.07571E+06	2.12036E-01	2.56390E+05	4.57190E+03	1.54152E+06
50	3.37009E+10	3.26007E-05	1.33710E-03	6.08461E-01	9.74474E+03	5.33176E+06	2.11051E-01	2.06438E+05	4.04707E+03	1.55097E+06
51	3.46286E+10	4.40536E-05	1.80107E-03	7.36095E-01	9.75691E+03	4.76206E+06	2.11630E-01	1.26032E+05	2.57437E+03	1.58626E+06
52	3.50058E+10	4.81326E-05	1.96070E-03	7.08031E-01	9.75367E+03	4.62316E+06	2.11611E-01	9.26421E+04	2.00893E+03	1.60365E+06
53	3.57022E+10	4.93106E-05	2.03807E-03	9.26040E-01	9.75260E+03	4.55461E+06	2.11500E-01	3.21100E+04	7.32927E+02	1.64156E+06
54	3.61919E+10	4.95041E-05	2.02400E-03	9.33100E-01	9.78399E+03	6.56269E+06	2.12269E-01	4.07072E+04	6.53600E+02	1.66184E+06
55	3.60346E+10	4.95310E-05	2.02590E-03	9.39150E-01	9.02750E+03	9.33610E+06	2.13213E-01	5.22554E+04	5.90004E+02	1.69366E+06
56	5.16059E+10	1.67392E-05	6.84663E-04	9.44144E-01	9.86358E+03	1.16213E+07	2.13994E-01	5.91643E+04	5.50936E+02	2.10311E+06
57	7.51626E+10	1.91798E-10	7.84475E-09	9.46860E-01	9.86384E+03	1.16241E+07	2.13995E-01	5.66124E+04	5.27029E+02	2.38027E+06
58	1.09723E+11	1.91798E-10	7.84475E-09	9.46763E-01	9.86398E+03	1.16521E+07	2.14005E-01	5.65363E+04	5.25129E+02	2.38028E+06
59	1.44203E+11	1.91798E-10	7.84475E-09	9.46790E-01	9.86402E+03	1.16567E+07	2.14005E-01	5.65196E+04	5.24865E+02	2.38029E+06
60	1.78843E+11	1.91798E-10	7.84475E-09	9.46781E-01	9.86396E+03	1.16803E+07	2.14004E-01	5.65005E+04	5.24950E+02	2.38029E+06
61	2.13462E+11	1.91798E-10	7.84475E-09	9.46763E-01	9.86306E+03	1.16440E+07	2.14002E-01	5.64903E+04	5.25123E+02	2.38030E+06
62	2.47963E+11	1.91798E-10	7.84475E-09	9.46742E-01	9.86374E+03	1.16370E+07	2.13999E-01	5.64081E+04	5.25323E+02	2.38031E+06
63	2.82522E+11	1.91798E-10	7.84475E-09	9.46721E-01	9.86363E+03	1.16290E+07	2.13997E-01	5.64777E+04	5.25524E+02	2.38031E+06
64	3.15570E+11	1.91798E-10	7.84475E-09	9.46700E-01	9.86252E+03	1.16220E+07	2.13994E-01	5.64671E+04	5.25726E+02	2.38032E+06

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Appendix II

Input File for SANCHO (Version 2.0.0) Run

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PRESSURIZED ROOM CALCULATION  - MOLES VS TIME FROM BRAGFLO RUN 42
CONTROL,3,2,0,3
PLANE
ELEMENTS,1
ISTRESS,CONSTANT,-14.8E6,-14.8E6,-14.8E6
ISTRESS,OFF,2,3
NODES,1
DXSCALE,.5
PLOT,ELEMENT,STRESS,STRAIN,STATE
PLOT,NODAL,DISP,TEMP,RESIDUAL
PLOT,GLOBAL,RMAG,ITER
PLIMIT,.2E6
SOLUTION,10.,100,2500,100.
TIMESTEP,0.,400000,3.1536E11
TIMEPRNT,0.,3.1536E9,3.1536E11
TIMEPLOT,0.,1.5768E7,3.1536E7,1.5768E8,3.1536E9,3.1536E8,6.3072E10,*
3.1536E9,3.1536E11
ENDSET
MATERIAL,5,1.,0.,0.0
***HALITE***
1.984E9,1.656E9,5.79E-36,4.9,20.13,0,.005
MATERIAL,7,1.,0.,0.
   * * CRUSHED SALT BACKFILL * *
648.,6.53E-3,1408.,6.53E-3,5.79E-36,4.9,20.13,*
-17.3E-3,0.0,1.3E8,0.82E-6,1.300E3,2140.,.0005
MATERIAL,2,1.,0.,0.
* * CRUSHABLE FOAM MODEL FOR DRUMS * *
3.333E8,2.222E8,0.,0.,3.
.03230,.02833E6,.741,.733E6,.898,1.1333E6,1.029,1.667E6,*
1.18,2.8E6,1.536,10.167E6
ENDSET
DISPR,1
DISPZ,2
PRESSURE,300,0.
CENTER,0.,0.
PRESSURE,200,14.8E6
PHISTORY,0.,1.0,1.0E12,1.
END
```

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