

**PEER 13 - Engineered Alternatives Task Force Report Peer
Review Comments**





INTERNATIONAL
TECHNOLOGY
CORPORATION



June 26, 1996

Project No 767014

Mr. Steven Wagner
Waste Isolation Division
Westinghouse Electric Corporation
P. O. Box 2078
Carlsbad, New Mexico 88221

Transmittal of Information on the 1991 Peer Review of the Engineered Alternatives Task Force Report

Attached please find as requested, copies of the signed Engineered Alternatives Task Force (EATF) peer review comment resolution forms. These forms document the external peer review that was performed on the EATF report entitled "Evaluation of the Effectiveness and Feasibility of the Waste Isolation Pilot Plant Engineered Alternatives: Final Report of the Engineered Alternatives Task Force" (DOE/WIPP 91-007, Rev 0, Vol I and II) during the first half of 1991. Four peer reviewers were selected for the review:

H. Eric Nutall, Ph.D., Nutall & Associates, Inc.
Douglas Brookings, Ph.D., University of New Mexico
Robert J. Budnitz, Ph.D., Future Resources Associates, Inc.
Donald E. Shaw, P.E., Engineering and Management Consultant

The reviewers comments and agreed-upon dispositions are contained in the four attachments to this letter. Please don't hesitate to call me at (505)262-8726 if you need any additional information regarding this peer review.

Respectfully submitted,

ITT CORPORATION

Jonathan Myers
Senior Technical Associate

cc: Mark Crawley
Project File

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July 30, 1991

Project No. 91-003

Mr. Mark S. Abashian
IT Corporation
5301 Central Avenue, NE
Suite 700
Albuquerque, NM 87108

Letter of Transmittal
Comment Resolution Forms
Peer Review EATF Final Report

Dear Mark,

Enclosed are the comment resolution forms for the risk assessment portion of the EATF Final Report that you sent me on June 13, 1991. I apologize for the delay in returning them. Unfortunately, they arrived while I was involved with other projects. I could not get to them sooner. Since you have sent me a copy of the report, it is obvious that the report was released before I could return them. I hope that the delay did not cause much inconvenience.

I have signed all of the comment resolution forms. There are no problems with the resolutions given. As a general note I might add that if additional work is done on the EATF issue, some of my comments might be included in the work scope. I am referring particularly to tying the risk assessment to the FEIS. While this was necessary in order to use the relative risk approach adopted, there are issues such as providing input for decisions among various treatment alternatives that were not an issue for the FEIS, but would be for the EATF. This would mean that an absolute as opposed to relative approach to the risk assessment would be required, but it may be worthwhile if treatment becomes a necessity because of problems with the existing design.

Once again I want to commend you and the others at IT on the effort represented by the EATF report. It represents a viable approach to examining repository performance issues and should find considerable utility in the future. Give me a call if you have any questions regarding my comments.

Sincerely,


Donald E. Shaw



DOCUMENT REVIEW COMMENT BOARD



- 1 a. Author/Editor: Mark Abashian/Fritz Seiler 2. a. Project Number: 301302.09.02.02
b. Reviewer: Donald E. Shaw
c. Date: 3/04/91 b. Page 1 of 22
3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*
- 5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

GENERAL COMMENTS

N GEN

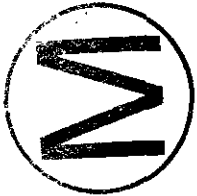
The risk assessment represents a comprehensive analysis of risks associated with the alternative waste forms. However, as a general comment, the scope of the risk assessment appears to be inconsistent with the scope of the evaluation of engineered alternatives. The primary difficulty lies in the objective of the risk assessment which appears to have been to perform a relative risk evaluation as if no other factors were involved in the tradeoff among the various alternative. Actually the relative risks associated with various waste treatment alternatives and location options are an ingredient in the overall evaluation of feasible waste alternatives and siting scenarios. Transportation risks are a part of the total transportation issues including RCRA sampling, waste acceptability, etc. Occupational and public risks from waste treatment facility operation are part of the total infrastructure issue relative to the question of multiple vs. a central treatment facility. Unfortunately, while totally correct for a stand-alone risk assessment, the aggregation of risk components masks the ingredients of various alternative waste treatments and location options that might be amenable to remediation in the alternative treatment design.

This is somewhat beyond our scope, but the information has been provided in Attachment G.4. See Attachment G.4. *oh*

N GEN

One important factor in the risk assessment is the weighting functions assigned to the relative risk indices. As noted there was insufficient time for a panel survey and it was necessary to select a set of weighting factors. A question that arises is to the degree of sensitivity of risk analysis results to the assumed weighting factors. Additional analyses varying the factors could have provided insight into the importance of the weighting factors to the overall conclusions of the risk assessment.

See above. *oh*



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1 a. Author/Editor: Mark Abashin/Fritz Selter 2. a. Project Number: 301302209202202

b. Reviewer: Donald E. Shaw

c. Date: 3/04/91

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4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5a. M,N,E b. Where Located

c. Comments/Corrections

d. Disposition

N GEN

Another factor relative to the combination of the weighting factors and the aggregation of risk components is that the influence of voluntary as opposed to involuntarily assumed risks. Occupational risks tend to be voluntarily in that workers assume the risk for the perceived benefits of employment. Society, while not unconcerned about worker risks, is probably less concerned about such risks that are voluntarily assumed than it is about involuntary risks associated with cancers or genetic effects. Thus the weight of occupational and public risks to the same scenario with the same consequences could be significantly different. A key issue is the degree to which transportation related risks are seen as voluntary or involuntary. Risks to transport crews are probably viewed as voluntary while risks to the public are probably considered involuntary. Risks to transport crews would seem to represent a larger component of transportation risk than risk to the public because of the exposure times and probabilities of being involved in an accident. While probably much smaller in terms of risk public involuntary assumption of these risks may result in a higher weighting factor compared with transport crew risks. Yet a weighting factor which varies directly with the level of risk could result in exactly the opposite in the analysis. Aggregation over all transportation risks masks these differences.

Aggregation in Attachment G takes this into account and thus does not mask these differences. See Attachment G. *ok*

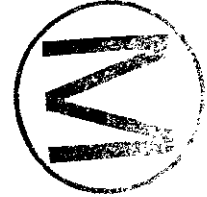
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- 1 a. Author/Editor: Mark Abashian/Fritz Seiler 2. a. Project Number: 301302.09.02.02
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- E GEN The Appendix I report could probably be restructured with significant benefit to the reader if more of the discussion of assumptions and result tables were contained in a main body of the report with the equations and technical details appendicized. As it is with an executive summary and attachments it is not clear what portion constitutes the report and what portions are material meant to amplify the report information. Along that line there are mathematical discussions which probably are at a higher level than most readers are familiar with unless they work in the area of risk assessment. At times the mathematical issues appear more like technical papers than a portion of an applied risk assessment with almost no mention of the problem at hand. To the degree possible specific comments have been made along these lines where appropriate. Text revised so that there is an executive summary, main body of the report, and appendices. The structure is already strongly "appendicized". I do not think that further disjunction would be useful. The mathematical parts form the backbone of the comparison. *ok*
- E GEN Throughout the report the references to equations and tables tend to need checking. In many cases equation numbers appear to have a portion of the number missing. This appears to be a result of changes in section numbering which were not caught in the equation numbers. Such instances are pointed out where they have been found, but no attempt was made to review the entire report with the objective of checking equation, table and figure numbers. Done. *ok*
- E GEN Throughout the risk assessment appendix there are headings followed by subheadings followed by sub-subheading with no text between. While there is nothing wrong with that approach, it is not the one used for Volume I where general introductory remarks were made for following subsections. As a matter of preference, introducing the material to be discussed in the subsections seems to provide a better flow. Done where appropriate. *ok*





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N GEN In the derivation of risk reduction factors for the various scenarios given in Attachment E, more explanation of how the basic equation is derived might be helpful. In some cases the first equation written for a given risk reduction factor is not evident from the list of symbols and the issues of the scenario. Done where appropriate. *oh*

N GEN In some of the attachments, particularly E and F, there are blank sections with notations of additional material to be supplied. Also, there are table or figure numbers or references which are not fully specified. It was assumed that these issues would be taken care of such that no mention is made of missing sections. Now supplied. *oh*

EXECUTIVE SUMMARY

E I-ES The Executive Summary has an Executive Summary. This raises confusion as to what the remainder of the section entitled Executive Summary is. At first it appeared as if it is the main body of Appendix I. However, much of the important discussion does not occur in the Executive Summary section, but is located in the attachments. Some restructuring is required. That was a labeling error. See new Executive Summary on pages I-i to I-iv. *ok*

E I-ES The combinations of risks and scenarios might be depicted graphically in some form as an aid to the reader. Are such graphics available in the FEIS or FSAR? If so, it might be worthwhile to extract them if possible. I wish there were such a graph! *oh*
So do I.

M I-ES This paragraph was selected because it contains the reference to what is called Table 2-1. The table number should be fixed if the convention used is to number tables with the section that contains them. Also, the table itself should be modified to correct the location option numbers to agree with the way they are referred to in the text, Option Nos. 0 through 4. Old table was inserted by mistake. See new Table I-2-1 on page I-7. *oh*

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 4. Approval for Report Issuance: *Donald E Shaw* Date: *7/27/91*

S.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

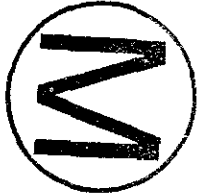
E I-ES 18 3 Is there a significance to the designation of scenarios as N1, C2, etc. It probably doesn't have to be explained, but it might be helpful to use a table. The primary problem is that not everything discussed is assigned a designation. Note in Volume I, the three human intrusion scenarios were designated E1, E2 and E1E2. Are the N1, C1 designations a result of another document such that these designations are standard to those involved routinely with WIPP? Taken from FSEIS, FSAR, and calculation briefs. *ok*

M I-ES 19 4

Are stops at traffic signals included in those considered to be due to road conditions? If not, should they be included? We are bound by KADTRAN assumptions. *why? Comment?*

M I-ES 20 4 In the first paragraph of Section 2.3.2.1.1, there is a sentence that states, "The drum is assumed to contain the 12.9PE-Ci cited as the average alpha radioactivity per drum." What about beta particles and gamma radiation? Several times mention is made of penetrating radiation that is presumed to be gamma. In the drum drop what happens regarding beta particles and gamma radiation? The average drum has a certain level of α , β , and γ -activity and neutrons. Neutrons and gammas always come out; beta and alphas only in a spill. Put such a remark in assumptions in Sections 2.2.1.1 and 2.4.1. See pages 1-5 and 1-25. *ok*

M I-ES 24 1 It would seem that accidents associated with treatment facilities would represent a primary issue in a risk analysis which is focusing on relative risks among various treatment options. While scope limitations are understandable, it is difficult to understand omitting events which are a result of the reasons for performing the assessment. Is there anything which can substantiate the claim that the bias won't be severe enough to invalidate basic conclusions. Industrial accidents would seem to be a source of risk which is not greatly smaller than forklift accidents. Or, if there is data that says otherwise, it could be cited to substantiate omitting treatment facility accidents. The reason given is that routine exposures are highest. Accidents have larger consequences but occur less frequently, otherwise health and safety will change the SOPs. *ok*



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d. Disposition

No, it means "by themselves". They are, of course, included in the averages. Text amended. See page 1-25. *df*

Very important. But there are no data. Model calculations are outside of scope. This assumption introduces a bias, but on the small 5000-year risks. *df*

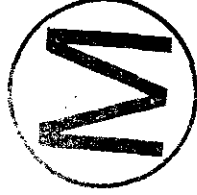
A complete set of these values were provided in Table E-6 on pages 1-215 to 1-217. *df*

(corrected to Figure B-1. See pages 1-31 and 1-70. *df*

Typo. Corrected. See page 1-32. *df*

5.a. M,N,E b. Where Located c. Comments/Corrections

5.a. M,N,E	b. Where Located	c. Comments/Corrections
M-ES 27	3	The sixth dot under this paragraph states, "Maximally exposed individuals are not considered." What does this mean? It sounds as if the risks to persons receiving maximum possible exposures from a given scenario are not considered which is confusing.
M-ES 28	1	In the second dot under the first paragraph, it states, "The treatment of the waste is assumed not to affect isotope leachability." What does this mean? It seems as though isotope leachability is an important item if it refers to leaching by groundwater in the repository.
M-ES 28	3	What are the regional averages given in the FELS relative to fractions of travel in urban, suburban, and rural regions? It would seem that the location of the various possible treatment facilities would differ considerably relative to demographics. This may be accounted for in the FELS data. Defining the term regional average and possibly giving a table of values or showing them on maps would be helpful.
E-ES 32, 1		In the first paragraph at the top of the page, reference is made to Figure 3-1. A Figure 3-1 was not found.
M-ES 32	4	There is something wrong in the first sentence of this paragraph, "The weight with which the single attribute utility functions are multiplied in the linear combination and the social value judgments for each risk component."



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M I-ES
33
1

Relative to the issue of ranking and uncertainties, the uncertainties are considered to provide a more conclusive ranking of alternatives and location options so that clearly defined differences can be found. Is ranking a primary issue associated with the risk assessment? If the risk analysis were to be used solely as the basis for decision making it would be. However, when the risk analysis results represent one of other issues associated with decision making, does a ranking based solely on risk have meaning to the overall objective. The ranking of primary concern is the ranking of location options and treatment alternatives in view of all constraining parameters including effectiveness in solving gas generation and human intrusion problems, technical feasibility etc.

Ranking of alternatives is outside the scope of this effort, as such rankings require the subjective assignments of weighting factors for risk, cost, schedule, etc. *oh*

M I-ES
35
3

In the paragraph on Risks in Post-Closure to Human Intrusion, a sentence states, "The reduction factors for the public risk by inhalation are the same as those of the drill crew for all scenarios." Considering that the drill crew is in the immediate vicinity of the borehole and could inhale volatiles contained in drill cuttings, while the public would presumably be further away from the borehole where doses would be less due to air dispersion, this assumption seems questionable. It is recognized that any assumptions regarding the circumstances surrounding the actions of people 5000 years in the future is speculation, but if the scenarios are to be evaluated, the assumptions should be as realistic as possible. It is probably conservative to assume the public reduction factors are the same as the drill crew, but the conservatism should be cited as the basis for the assumption.

This is an "absolute risk" argument. Equations (E.1.1) and (E.1.2) show that statement is correct, not conservative. See page I-147.

Agree



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<p>N I-ES 38 1</p>	<p>The sentence in this paragraph that states, "It is prudent to discard some small risks because a larger risk of the same exposure is already being considered." If the weights reflecting the perceptions of both risks are the same, the smaller risk can be neglected because it does not add anything of significance. However, "prudent" is probably not the right word to describe neglecting the smaller risk. If the weights reflecting the perceptions are not the same and the weighting of the smaller risk compensates for the larger, neglecting the smaller may not be reasonable even though they both arise from the same scenario. The example of cancer and genetic damage may not be a good one because genetic damage may be perceived to be a much greater risk.</p>	<p>I agree. The word "prudent" has been substituted by "expedient" and some further comments (Section 4.3.1, first paragraph). See page I-38. <i>oh</i></p>
<p>B I-ES 38 5</p>	<p>In the first sentence of the last paragraph, insert "changes resulting from treatment" after the word "volume."</p>	<p>Done. See page I-39. <i>oh</i></p>
<p>B I-ES 39 2</p>	<p>The reference to Table 4-1 should be changed to "Table 4.2-1." Also, in Table 4.2-1, the left hand column could include the list of aggregated risks from page I-ES-38.</p>	<p>Table reference changed to "Table I-4-1." See page I-41. <i>oh</i> No room and, I think, no real need.</p>
<p>M I-ES 41 4</p>	<p>How dependent are the results on the weighting factors given in Table 5-1? Could some sensitivity study be done to determine this? If the results change significantly relative to each other with changes in these weighting factors, the results just reflect the weighting factors. Since the all the risks associated with treatments generally all increase relative to the baseline, the chance is that while the augmented risk reduction indices would change with a change in weighting functions, their relative values would remain about the same.</p>	<p>This is correct, but there is a sensitivity to the relative weights assigned to the components. The sensitivity is that assigned here to the decision maker's weights. <i>oh</i></p>

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<p>N I-ES 41 4</p>	<p>A problem with the method of aggregating risks is that it incorporates both voluntary and involuntary components in the same aggregated risk. The importance of this is that weighting factors for voluntary and involuntary risk components may vary considerably. For example the weighting factor for a truck driver hauling TRUPACT containers getting cancer could be considerably less than the weighting factor for the public getting cancer from transport of wastes because in the case of the driver the risk is voluntary while it is involuntary for the public. Aggregating both of these under the risk cancer masks the voluntary/involuntary nature of the risks.</p>	<p>Not aggregated across voluntary and involuntary risks. Thus no masking. See Attachment G. <i>ok</i></p>
<p>M I-ES 42 1</p>	<p>The first sentence at the top of the page says that "absolute and relative weights are given in Table 5-1." There is only one weight given in Table 5.1 and it is labelled Normalized Weight.</p>	<p>Corrected. See page I-44. <i>ok</i></p>
<p>E I-ES 43 1</p>	<p>Table 5-2 could be better portrayed in matrix form with alternative numbers and brief descriptions across the top and location options with brief descriptions along the left side (or vice versa). The mathematical symbols should be avoided if possible.</p>	<p>Done. See Figure I.5-1 on page I-47. <i>ok</i></p>



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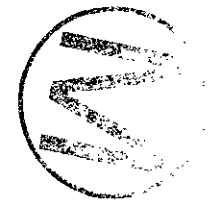
5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

M I-ES
43
2

While a formal indifference analysis indicates that only 12 of the possible 120 combinations show confirmed indifference (inability to distinguish between two risk augmentation indices as being significantly different) an engineering view of the results would indicate that all of the values are essentially the same. A problem with the numerical aspects of the analysis is that input for things like uncertainties in risk reduction factors as well as the factors themselves are entered with one significant figure accuracy and results are produced to three significant figures. While this is done repeatedly today because of computers, an old professor once engraved it in our brains forever that you cannot have more significant figures in the output than you have in the input. If this is used, most of the values in Table 5-2 become 1 while maybe three could be 2 if the error is additive. While that professor has long since passed away, there is an issue here. How can precision be manufactured, and while the trends among the values generally agree with what would be expected, is the input sufficiently well defined to support a precise analysis? Or, could it equally be concluded that to the level of precision in the input, there is really no significant difference in risk among the various alternatives and location options? (All of the input was not checked to see with what precision it is known, but when it comes to the really global issues associated with a risk analysis it is doubtful that more than 1 significant figure accuracy is available.)

All error calculations are done correctly and there is no fabrication of false accuracy. Only one digit more than justified is carried throughout to avoid rounding problems. In the final results the error determines the number of significant digits of the result. See Section 2.4.6 on page 1-27.

de



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N I-ES
 46
 1
 In plotting the probability distributions, different line styles should be used (recognizing that there are a great number of lines) to distinguish between the different distributions. Particularly where the distributions are plotted as negative values Figure 5-1 gives the appearance that what is actually plotted are continuous functions that rise above and dip below the line at places. ~~All error calculations are done correctly and there is no fabrication of false accuracy. Only one digit more than justified is carried throughout to avoid rounding problems. In the final results the error determines the number of significant digits of the result. See Figures 2, 4, 6.~~
 Comments on Figure I-5-1 to avoid this misunderstanding. *oh*

M I-ES
 47
 1
 In the first dot under the first paragraph, Level III should be Level II if the information on Table 5-3 has been interpreted correctly.
 Corrected. See page I-50. *oh*

ATTACHMENT B

E I-B
 1
 1
 Attachment B is so mathematical and essentially not tied to any specifics of the risk assessment, it seems that it should perhaps be pushed farther back in the document after those attachments that give models and data relative to the risk assessment. While providing the requisite background, Attachment B, is not necessary for readers concerned primarily with modeling and results.
 The presentation is disaggregated enough. Attachment B is not background information; it is the crux of the whole method. *oh*

E I-B
 2
 4
 Is Eq. B.2.3 correct? It is the same as B.2.2 and the range of subscripts is shown as from 1 to I while the text states 1 to J.
 No, wrong copy of equation. Corrected. See page I-65. *oh*



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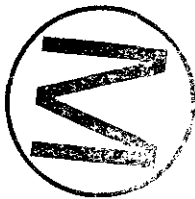
- E I-B In the text, a utility, capital theta k is mentioned, but it does not show up in Eq. B.3.11. It is U_k not Θ_{kj} . See page I-76. *oh*
 11 While it can be deduced, it might be less confusing to define it.
 2

- M I-B- Reference is made to Eq. B.5. There is no B.5 and it appears the reference should be (B.2.5) and (B.3.3) corrected. See page I-72.
 12 B.2.5. Also later in the paragraph is a reference to Eq. B.7 which should be B.2.7. *oh*
 3 This problem occurs throughout Attachment B. Where found it is noted, but the
 equation references should be checked relative to the equation numbers.

- M I-B In the first sentence there is a reference to Appendix C.1, Aggregation of Risk Corrected. See page I-72.
 12 Components. There is no Appendix C. There is an Attachment C and Section C.1, but *oh*
 4 C.1 deals with propagation of errors and the derivation of the algebraic form of Eq.
 B.3.13. Section C.2 appears to deal with aggregation, and the derivation appears to be
 given in Section C.2.1.1. This should be checked and corrected. Later in the same
 paragraph Eq. B.8 should be B.2.8.

- M I-B There is a reference to Appendix C.1 should probably be Attachment C.2. Done. See page I-74. *oh*
 14
 2

- E I-B There is a typo. Eq. B.27 should be B.2.7. *oh*
 17 Changed to B.4.2 which is the right equation
 1 number in the revised version. See page I-80.



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- c. Date: 3/04/91
- b. Page 13 of 22
- 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
- 4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

ATTACHMENT C

- | | | |
|--------------------------|--|--|
| <p>B I-C
2
3</p> | <p>Perhaps Eq. C.1.2 and C.1.3 could be switched, giving the more general form when the x quantitatives are not independent then specializing for the case where they are independent.</p> | <p>C.1.3 is more general but is an appendage here - more an afterthought. <i>oh</i></p> |
| <p>N I-C
3
3</p> | <p>It could be mentioned that underlying probabilities of risk components are almost never know and that some assumed distribution is almost always used, and that the normal distribution is often approached with large samples.</p> | <p>The central limit theorem is not really the point here. It is not what we are doing here. <i>oh</i></p> |
| <p>N I-C
4
6</p> | <p>It might be mentioned that Eq.C.1.13 is for the case where the stochastic parameters are independent.</p> | <p>Done. <i>oh</i></p> |
| <p>N I-C
6
3</p> | <p>Table C.1-1 is referenced in this paragraph. It might be more instructive of a figure showing the variation of the contribution with the ratio B.</p> | <p>Both done. See page I-94. <i>oh</i></p> |

ATTACHMENT D

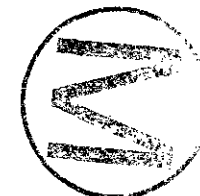
- | | | |
|--------------------------|--|---|
| <p>N I-D
3
1</p> | <p>It might be mentioned that using forklift accident data from similar industries is likely to be conservative because of improved safety procedures associated with waste handling and treatment facilities.</p> | <p>True at the beginning, but after a few years? Since forklift incidences are given as a rough estimate only (1% of occupational accidents). I did not consider it appropriate to introduce such a reduction factor. With better data, yes, I would. <i>oh</i></p> |
|--------------------------|--|---|

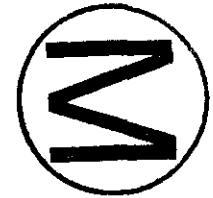
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 c. Date: 3/04/91 b. Page 14 of 22
 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
 4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

- | | | |
|--------------------------|---|---|
| <p>M I-D
3
3</p> | <p>Neglecting accidents in waste handling and treatment facilities is probably not a good idea simply because it amounts to neglecting a risk associated with the objective of the study which is to evaluate relative risks associated with treatment. Since specific forms of some treatments are not known and the form of the plants are not known, there is probably no basis for thinking that they would be any more or less prone to accidents than other processing plants. Could an approach similar to that for forklifts have been used in which accident data from chemical processing plants could be cited as to probabilities. Such a citation could provide additional justification for neglecting accidents in treatment facilities.</p> | <p>Specific radiation accidents are important for credibility but yield small risks due to H&S restrictions. Evaluating them is far beyond the scope of this study. <i>ok</i></p> |
| <p>M I-D
3
5</p> | <p>The statement that "Such risks do not fall under the scope of evaluating overall changes in risks due to treatment of the wastes" relative to diesel exhaust is confusing. If treatment processes have greater needs for diesel powered transporters than the baseline condition, the risk should increase in proportion to the different amount of anticipated use. Thus the risk reduction factor would be less than 1 and could vary among the different treatment alternatives.</p> | <p>Nonsense due to a mixup of several garbled statements. See third paragraph of Section D.1.4 on pages I-101 and I-102. <i>ok</i></p> |
| <p>M I-D
4
2</p> | <p>Why not consider the risks mentioned? Are they sufficiently small to be neglected? Or are they a washout that would enter all alternatives including the baseline to the same degree? Some justification should be provided relative to the expected effect on the results of neglecting events.</p> | <p>Not treated in FSEIS or FSAR. <i>ok</i></p> |





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c. Date: 3/04/91 b. Page 15 of 22
3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
4. Approval for Report Issuance *Donald E Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

- M I-D 10 3 Taking the systematic error equal to the random error was arbitrary. If nothing is known about systematic errors, it's probably as good an assumption as anything else. How sensitive are results to assumptions such as this? Was a similar approach used elsewhere relative to systematic errors? The reason of differences between the treatment facilities as a basis for systematic errors relative to increased manpower needs would also apply to other issues. This is an example of level of precision in input vs. level of precision in output. Manpower is a very important quantity. Therefore, an effort was made here to include effects of a systematic error. Effort to do that everywhere is beyond time and money limits. *oh*
- M I-D 12 2 The assumption that the population exposure around a treatment facility is the same as around WIPP probably biases the results toward WIPP or western treatment sites because it seems that larger populations would be expected at eastern treatment sites. Resulting in permitting restrictions, assumed to pull it down to similar values. *oh*
- M I-D 18 3 In the last sentence of the last paragraph, a strain rate of 0.10 ± 0.03 is assumed for creating the perforation. Is there a reference for that value, it seems pretty specific, particularly the 0.03 error, for just an assumption. If there is no reference, how sensitive are the results to this assumption? The sensitive quantity in equations (D.3.20) is y^2 not e . Reference given. The sensitivity for e is taken into account by the error propagation calculation. The errors should be large enough involving 0 and 0.2 at the 2% level. *oh*
- E I-D 26 1 Line spacing changed from single to space and a half at this point. *oh*
- M I-D 26 1 In Eq.D.3.32, there is a one plus epsilon on the right side with epsilon defined as a small fraction. In Table D.3-7 the value given is 1 ± 0 or epsilon is 0. This should be checked as to which is correct. If epsilon is zero it should be stated after the definition of epsilon as to why it is zero. $\epsilon = 0$ due to model assumption. Inserted sentence to that effect. See page I-124. *oh*



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- c. Date: 3/04/91
- b. Page 16 of 22
- 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
- 4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5.a M,N,E	b. Where Located	c. Comments/Corrections	d. Disposition
M I-D 26 2		The reference to Eq. 3.14 should probably be B.3.14. Also Eq. B.3.34 gives values of 1/0.4 while Table D.3.7 shows 1+0.3 for these values.	Typing error; corrected. See page I-124. <i>ok</i>
M I-D 32 1		There is a typo in the last line of the first paragraph. "shortage" should be "storage".	Corrected. See page I-126. <i>ok</i>
ATTACHMENT E			
M I-E 1 3		The f sub "exp" in this paragraph should be f sub "dep".	Done. See page I-147. <i>ok</i>
M I-E 3 3		In the last paragraph it is argued that because of modular construction the same fraction of total personnel is assumed to be exposed. Since treatment facilities are essentially undefined from some treatments, is it reasonable to assume, modular construction or not, that the same fraction of personnel would be exposed?	Yes. The accident will happen in one module only, so the crew of that module only will be exposed. Corresponding text change made. See page I-150. <i>ok</i>
E I-E 4 3		Switching back and forth between occupational and public risks becomes confusing. Perhaps subsection headings could be used to distinguish between the two. This comment applies throughout Attachment E.	Too much potential for typos if a change is attempted. Besides, we follow FSEIS scenarios. <i>ok</i>
M I-E 11 2		It is stated that ratios are the same for occupational and public risks. This is confusing. If the exposure is underground, how is the public exposed? It would seem that the only risk component would be worker exposure in the underground environment.	They are. See equations (E.1.1) and (E.1.2) on page I-147. <i>ok</i>

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 c. Date: 3/04/91 b. Page 17 of 22
 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
 4. Approval for Report Issuance: *Donald E Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

M I-E The first sentence of this paragraph beginning with "As long as the last three ..." should
 18 be extended with "for each treatment" after the word "same" which is at the end of the
 4 sentence as written. Otherwise it reads as if the public and occupational risk reduction
 factors are the same which does not follow from the assumption that the factors are
 independent of treatment. See resolution to preceding comment. *oh*

E I-E There is something wrong with the last line of this paragraph. Last sentence deleted. *oh*
 73
 1

M I-E An explanation that treatment alternatives affect the number of truck miles travelled
 78 because of volume changes resulting from treatment as the basis for the expression in
 1 Eq. B.6.1 would be helpful. At first, it would appear that the risk of a traffic fatality
 would be independent of treatment because an accident would not be dependent on
 what the truck was hauling. Done. See page I-213. *oh*

M I-E Does the term L sub "s", the distance between stops include stops at traffic signals? No, L is a distance, stops affect time. *oh*
 86
 1

M I-E It would seem that the exposure for vehicles travelling in the opposite direction would
 89 involve some time while the vehicle is in the proximity of the TRUPACT truck, and
 1 that the duration would depend on the speed of the vehicle, the speed of the TRUPACT
 truck, and an exposure distance. There does not seem to be a duration or speed of
 other vehicles in the list of parameters. Given by RADTRAN scenarios to which we are
 tied. *oh*



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 c. Date: 3/04/91 b. Page 18 of 22
 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
 4. Approval for Report Issuance: *Donald E Shaw* Date: *7/24/91*

S.a M,N,E	b. Where Located	c. Comments/Corrections	d. Disposition
M I-E 90 1		In the last line of this paragraph should the reference be to Eq. E.6.4 or E.6.14?	It has been corrected to E.6.14. <i>oh</i>
M I-E 93 1		For vehicles travelling in the same direction the maximum exposure would occur during overtaking, passing, and leaving the truck behind with an envelope around the truck during which exposure would occur. The length of that envelope would seem to set the duration of the exposure. Assuming an envelope length of 100 feet and an incremental speed of the passing vehicle of 10 mph (14.7 fps) it would require about 6.8 seconds to overtake, pass, and leave the exposure zone. What is the basis for the 2 seconds given in the list of symbols?	Again, a RADTRAN scenario. <i>oh</i>
E I-E 106 3		Is there something missing before the start of the last paragraph? From the last line of the preceding paragraph it appears that an expression is missing.	Orphaned equation (WordPerfect 5.1 idiosyncrasy). This has been corrected. <i>oh</i>
E I-E 110 3		Just before the last line on the page, it appears that an equation is missing based on the last line of the previous paragraph ending in "given by."	Same as above. <i>oh</i>
M I-E 119 6		The assumption relative to public exposure from drill cuttings being essentially zero seems appropriate, but in earlier discussions in the Executive Summary it appeared that the two were considered to be the same.	The risk reduction factors are the same, the risks are minute. <i>oh</i>



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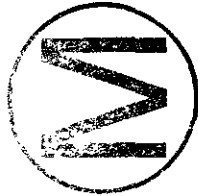
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c. Date: 3/04/91 b. Page 19 of 22
3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

S.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

ATTACHMENT F

- | | | |
|-------|---|--|
| E I-F | Line spacing changed from space to space and a half. | Corrected. ✓ |
| 1 | | |
| 1 | | |
| 4 | | |
| 1 | | |
| M I-F | If understood correctly, the purpose of Section F.2.1 is to develop baseline risk coefficients so that risk reduction factors can be determined for the various treatments, and that this necessary because there are no associated baseline risks in the FEIS. If this is the case, an introductory paragraph to that effect at the start of Section F.2.1 would be helpful in understanding the remaining sections. The introduction should discuss how the industry wide data was applied to the estimate of treatment baseline risks. | No. The risk coefficients are developed in Section D.4.11. <u>ok</u> |
| 3 | | |
| 1 | | |





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c. Date: 3/04/91
d. Page 20 of 22
3. Report Title: EATR Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*
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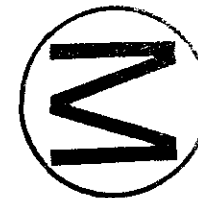
ATTACHMENT G

M-1-G
1
2

In the discussion of the risk components which were dropped in the aggregation process, the 45 components dealing with genetic damage, while smaller than the cancer risks, may be considered more important in terms of weighting to some evaluators. While the basis for dropping them is understood, it would be helpful if numeric comparisons of the genetic and cancer components were made in support of the decision to drop them. Concerning the six components dealing with non-cancer risks from chemical toxicants, their uncertainty would not seem to be a good basis for dropping them. If there is considerable uncertainty, their risks could be higher than anticipated. What would happen if they were included with large error coefficients. Would they remain very small?

Reference made to the BEIR III, IV and V reports. Sentence changed to convey relative sizes. See page I-307. *oh*

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 b. Reviewer: Donald E. Shaw
 c. Date: 3/04/91 b. Page 21 of 22
 3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
 4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

M I-G

4
1

Eqs G.1.2 and G.1.3 highlight a problem relative to weights in aggregation of risks into supercomponents that was troublesome where it was discussed in Attachment C. Weighting by the relative contribution to the total risk does not treat perceptions of risk. While using the law of diminishing marginal utility in the sense that a small additional component of risk causes a small additional disutility, the issues of voluntary vs. involuntary risks is not considered. In the transportation supercomponent, the risk of traffic fatalities overwhelms the other risks of cancers caused by radiation exposure to the point that the latter two can be essentially dropped. However, traffic fatalities occur with or without hauling TRU nuclear waste and when a person gets in a car and goes out on the highway, he voluntarily assumes the risk of traffic fatalities that could result from truck accidents because of the perceived benefits of his travel. On the other hand, most drivers on the road would not voluntarily assume the risk of cancers due to radiation exposure. Thus, in this case the largest component is voluntary risk and the two small components are involuntary. Consequently, the small components may have far, far greater disutility than the largest component. By weighting traffic impact fatalities the heaviest, the effect is to say that the risk associated with the various alternatives is merely due to the presence of more trucks on the road and the same risk could be obtained if it were newsprint being transported in the same quantities over the same distances. This would seem to produce a totally different reaction in the public's acceptance of the risk compared to cancers caused by radiation which they did not know they were exposed to.

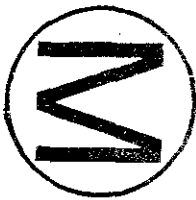
Perceptions of risk components dictate the grouping of aggregates. The actual weighting is done separately. Grouping here scrupulously separates voluntary and involuntary risks. *ok*

M I-G

6
1

The heavy weighting of risk components on impact injuries does not include consideration of voluntary vs. involuntary risks. See comment on transportation fatalities. This supercomponent also turns out to be simply a measure of the effect of more trucks on the road regardless of what they are carrying.

The scope includes all short-term risk components. New Section G.4 discusses the influences of transport general occupation and radiation exposure. See page I-357. *ok*



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b. Reviewer: Donald E. Shaw
c. Date: 3/04/91 b. Page 22 of 22
3. Report Title: EATF Final Report of the Engineered Alternatives Task Force, Vol. II (Draft, Jan. 1991)
4. Approval for Report Issuance: *Donald E. Shaw* Date: *7/24/91*

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

M I-G
8
1

What are the relative contributions of the two components? This supercomponent is an example of another problem with aggregating risks. Since the objective of the risk assessment is to provide information for decision-makers relative to determining optimum strategies for waste treatment if necessary because of problems with the existing design, there are other factors involved in the decision beyond risk, and the individual components may have an impact on decisions. For example if fork lift accidents far outweigh general industrial accidents, there may be a basis for the development of treatment alternatives that mitigate forklift problems such as different packaging and handling schemes. Understanding of risk components is important to that process.

I agree but that is a different use of risk analysis than the one discussed here. *Agreed, but if there's more work done, this would provide useful information.*

M I-G
31
1

The absolute weights shown in Table G.2-1 give 56% weight to transportation related risks which essentially are impact accidents that are independent of what the trucks are carrying. Considering the voluntary aspects of assuming risks of highway travel, this weighting seems far too high and essentially unrelated to radioactive aspects of the TRU waste disposal. It seem that a heavy weighting on non radioactive aspects waste treatments unfairly biases the results against treatment technologies. The point is that the same conclusion would be reached if the issue were questions of whether to manufacture shoes at a variety of facilities or at a central facility and that the shoe manufacturing process involved some degree of chemical carcinogenic and non-carcinogenic risk. Could the risk augmentation indices be recalculated using societal weighting factors which weight supercomponents 1 and 2 with a combined normalized weight of no more than 10 percent. Better yet could the weights for 1 and 2 be varied over a range with curves produced for the results.

56% is less than some nationally recognized experts propose. Again the new section G.4 discusses the different contribution. What is proposed in the comment is outside the scope of this study. See Section G.4.

Again, this would provide a basis for additional work.

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2. a. Project Number 301302.01.06.02

b. Page 1 of 4

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

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Date:

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c. Comments/Corrections

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E	Ex. Sum. Pg. XVII Para. 3	Second sentence beginning with some seems unnecessarily vague.	Sentence has been reworded. See page ES-i. (SC)
E	Ex. Sum. Pg. XXIII Para. 2	"This is caused by the assume coupling between anoxic corrosion and brine inflow." (Explain coupling)	A brief explanation has been provided. Also, this section has been reworded as per comments from Mr. Donald Shaw. (SC) An explanation of the coupling has been included in a detailed discussion of the processes and coupling in a revised Chapter 2. See page ES-viii and Section 2.1. (SMD)
E	Ex. Sum. Pg. XXIII Para. 4	"Improvements of one order of magnitude in pressure"	The improvements are one order of magnitude in the measure of relative effectiveness (MRE). See page ES-ix. (SMD)
E	Ex. Sum Pg. XXV Para. 2	Shorter work of periods--does this mean payoff times? Clarify!	A definition of "work-off" periods is provided in the latest revision of the report. See page ES-x. (SC)

N **NUTTALL & ASSOCIATES, INC.**

1445 HONEYSUCKLE DRIVE, N.E. □ ALBUQUERQUE, NEW MEXICO 87122 □ 505/293-2250



5/16/91

Mr. Mark S. Abashian
IT Corporation
5301 Central Avenue, NE
Suite 700
Albuquerque, NM 87108

Dear Mr. Abashian:

Enclosed are the signed comment resolution sheets. I appreciated the opportunity to review the EATF draft Final Report.

Regards,

A handwritten signature in cursive script that reads 'H. E. Nuttall'.

H. E. Nuttall, PhD

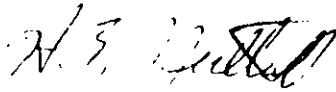
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c. Date: 3/21/91

2. a. Project Number 301302.01.06.02
b. Page 2 of 4

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:



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c. Comments/Corrections

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E	PK	Ex. Sum Pg. XXV Para. 2	Last sentence seems unclear. "bottoms up" needs to be better defined.	Sentence has been reworded for better clarity. See page ES-x. (SC)
M	PK	Chap. 3 Pg. 3-9 Table	Type! The units for hydraulic conductivity needs to be shifted to that column.	Comment already incorporated in latest revision of the report, page 3-7. (SC)
E		Chap. 3 Pg. 3-10 Para. 1	Add a glossary of acronyms.	Comment cannot be incorporated because a list of acronyms <u>already exists</u> in the report. It can be located immediately after the Table of Contents, and is also listed in the Table of Contents. See pages xii-xiii. (SC)
E	PK	Chap. 3 Pg. 3-17 Para. 3	First sentence. Hydraulic conductivity is not considered in a glass. Change wording.	Sentence reworded, "It is important to note that fluid movement within certain materials such as glass, is due to diffusion or . . .". See page 3-17. (DV)



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2. a. Project Number 301302.01.06.02

b. Page 3 of 4

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

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5. a. M,N,E b. Where Located

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M *OK* Chap. 3
Pg. 3-17
Para. 3
Several questions about the hydraulic conductivities presented in this paragraph. Also concern about hydraulic conductivities in unsaturated flow.

Brine flow through the waste/backfill composite is assumed to be saturated. A more detailed discussion of brine flow and hydraulic conductivities is included in the revised Chapter 2.0 (Section 2.1). (SMD)

M *E* App. B.3
Pg. B-5
Para. B.3-1
Why assume that the brine flow stops when lithostatic rather than hydrostatic pressure is exceeded?

A discussion of the assumptions is provided in text along with some justification (page 2-8). (SMD)

E *OK* App. B.4
Pg. B-8
Para. 3
Replace air gap with void space.

The term air gap has been replaced with the term clearance. See page B-13. (SMD)

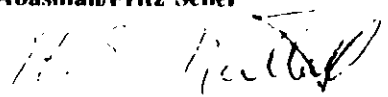
M *OK* App. B.6
Pg. B-11
Show and explain 1-D transient flow equation. I am not familiar with hydraulic diffusivity.

The 1-D transient flow equation is provided and the term hydraulic diffusivity is defined as the ratio of the hydraulic conductivity to the specific storage. See page B-16. (SMD)



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2. a. Project Number 301302.01.06.02

b. Page 4 of 4

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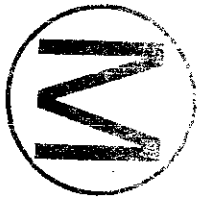
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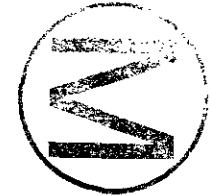
N

GEN

The chemical and physical processes at the proposed WIPP facility are exceedingly complex and involve many coupled interactions. The approach used in this report seems both practical and logical given the nature of the problem. The fact that the models have not been validated is an important consideration and should be of high priority for future work. I have some concerns about the aspects of two phase flow during gas generation with brine intrusion and if the repository can truly maintain pressures well in excess of lithostatic. In summary, given the complexity of the problem and the degree of required assumptions, this study is a reasonable approach and produced interesting results.



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1 a. Author/Editor: Mark Abashian/Fritz Seiler
b. Reviewer: Douglas G. Brookins *Douglas G. Brookins*
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2. a. Project Number: 301302.01.06.02

b. Page 1 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

Date:

5.a M,N,E b. Where Located

c. Comments/Corrections

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E	Ch. 1 Pg. 4 Para. 2	The sentence "All of the types of waste are in a chemically stable.....". <u>Add</u> the reference that documents that this is the case. (Brookins)	The appropriate reference, NuPac (1989), has been added. See page 1-6. (SC)
E	Ch. 1 Pg. 6 Para. 2	<u>Add</u> reference at end of paragraph. (Brookins)	Comment has already been incorporated into Revision 1 of the document. See page 1-8. (SC)
M	Ch. 1 Pg. 7 Para. 4	Last sentence on page... provide detail showing that processes that can dissipate gas pressures are "slow". This is important, for if these processes are rapid, then a built-in depressurization mechanism exists. (Brookins)	The sentence has been reworded to read, "As shown later in Section 4.0, these processes are slow relative to the ...". See pages 1-9 to 1-10. (SC)
N	Ch. 2 Pg. 1 Bullet 4	Give predicted <u>range</u> of brine inflow here. (Brookins)	No absolute range has been established. (SMD)
M	Ch. 2 Pg. 4 Eqn. 2.3-1	I am <u>not</u> convinced that this is the critical reaction involved. (Brookins)	Other reactions will be provided in the final version of the report. (SMD)



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2. a. Project Number: 301302.01.06.02

b. Page 2 of 14

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N	Ch. 2 Pg. 7 Para. 3, s e c o n d bullet	Give range of Salado Formation permeabilities as well as average. (Brookins)	Text modified to reflect range of $1 \times 10^{-22} \text{m}^2$ to $1 \times 10^{-16} \text{m}^2$ with an expected permeability of 3.4×10^{-21} (Rechard, et al., PA-9, 1990). See page 2-9. (SMD)
M	Ch. 2 Pg. 8 Para. 1, first sentence on page	This needs a reference; some data (i.e. argon) argues for advection of gas by the Salado. (Brookins)	The threshold pressure is so large that advection into the Salado Formation may be ruled out for any gas. See Section 2.1. (SMD)
M	Ch. 3 Pg. 1 Para. 5, last sentence on page	This is a very confusing sentence: "...solubility...investigated...lack of...data." If investigated, aren't there limits on data that can be used? (Brookins)	Sentence reworded as, "Investigations in radionuclide solubility (Rai et al., 1983; Felmy et al., 1989; Marietta et al., 1989) have shown variabilities of six orders of magnitude. The EATF has therefore assumed a value of 1×10^{-6} molar for all radionuclide solubilities (Marietta et al., 1989)." See page 3-1. (DV)



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1 a. Author/Editor: Mark Abashian/Fritz Seiler
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2. a. Project Number: 301302.01.06.02

b. Page 3 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

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5.a M,N,E	b. Where Located	c. Comments/Corrections	d. Disposition
N	Ch. 3 Pg. 2 Para. 5 RE...bentonit e	<u>Add</u> reference for this section. (Brookins)	Reference is (Butcher, 1990b). See page 1-12. (DV)
M	Ch. 3 Pg. 3 note 4	Assumption needs justification (i.e., 50% void filling). (Brookins)	The following has been added to footnote (4), "This assumption results from uncertainties in backfill emplacement methodology and the use of slip- sheets between layers of drums." See Table 3-1, page 3-3. (DV)
M	Ch. 3 Pg. 10 Para. 2	Assumption of contaminated liquid stabilized by cementation needs verification. (Brookins)	Sentence changed to read, "The contaminated liquid is concentrated and then solidified by cementation (Allen et al., 1982." See page 3-8. (DV)
N	Ch. 4 Pg. 1 Para. 2 (last sentence of page)	This assumption is contradictory to earlier assumptions (RE gas generation, C 2). (Brookins)	This sentence has been deleted. (SMD)

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2. a. Project Number: 301302.01.06.02

b. Page 4 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

Date:

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d. Disposition

M



Ch. 4
 Pg. 2
 Para. 2
 through
 pg. 4
 para. 5

This entire discussion of fracturing is weak and needs to be strengthened. The reader is left in the dark. Data from fracture distributions, etc., during mining and other operations should be available. (Brookins)

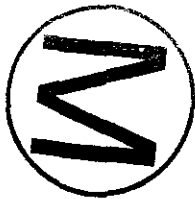
The prediction of fracturing and subsequent propagation is a complex issue which is being investigated by SNL (DOE/WIPP 89-011 WIPP Test Phase Plan: Performance Assessment Document). This has been stated in the text. See page 4-2. (SMD)

N

Ch. 4
 Pg. 3
 Para. 3 last
 sentence

What is justification for using maximum H₂ generation rate? (Brookins)

A maximum H₂ generation rate based on a SNL number is assumed if sufficient brine is available to maintain this generation rate; otherwise the reaction rate is scaled down in the Design Analysis Model. (SMD)



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1 a. Author/Editor: Mark Abashian/Fritz Seiler
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2. a. Project Number: 301302.01.06.02

b. Page 5 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

Date:

5.a M,N,E

b. Where Located

c. Comments/Corrections

d. Disposition

N Ch. 4 "...pressurizing smaller void volume...". Is advection by backfill and other surroundings in
Pg. 7 this assumption? Not clear. (Brookins)
Para. 5 last sentence

The void volume within the waste/backfill composite is assumed to be available for gases to occupy. This void volume will be less for the case of a vented repository than for the baseline case since venting will permit higher creep closure and subsequent reduction in this void space, since the fluid pressure does not build up during the first 100 years. Text has been revised accordingly. See page 4-8. (SMD)

N Ch. 4 Have the anhydrite beds been tested for gas behavior (i.e., argon, radon, etc.)? This would
Pg. 11 be useful information. (Brookins)
Para. 5

No gas behavior test data is currently available, but testing is underway. (SMD)

N Ch. 6 I think there are studies on behavior of cementiferous materials in evaporite environments...
Pg. 8 references? (Brookins)
Para. 6 last sentence

We are looking for references, though the statement in the text is a result of the Cement Panel. (DV)

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2. a. Project Number: 301302.01.06.02

b. Page 6 of 14

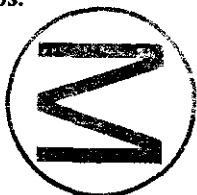
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M	Ch. 6 Pg. 9 Para. 1 entire pp.	Potential for increasing handler/operator exposure is increased in shredding; this should be mentioned. (Brookins)	This is mentioned in Section 6.3.7, "All components of the overall risk that involve the actual treatment of the wastes lead to additional injuries and fatalities." (DV)
M	Ch. 6 Pg. 27 Para. 4 entire pp.	A statement on the <u>relative risk</u> of the various scenarios with those from other types of waste treatment would be appropriate here. (Brookins)	Outside the scope of this work. (SC)
N	Ch. 6 Pg. 29 Para. 2 entire pp.	Compare to non-TRU waste shipments would be helpful here. (Brookins)	Outside the scope of this work. (SC)
N	Ch. 6 Pg. 37 Para. 1 last sentence	What is basis for EID interpretation of RCRA? Factual information (if so, should be documented) or guesswork? (Brookins)	The reference for this information is Paul Drez of IT Corporation. (DV)
N	Ch. 7 Pg. 4 Para. 2,3 entire pps.	I suggest cartoon-like figures to illustrate the text for these paragraphs. (Brookins)	Comment noted; we will try to find appropriate renderings of the processes. (DV)



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2. a. Project Number: 301302.01.06.02
 b. Page 7 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

Date:

5.a M,N,E b. Where Located

c. Comments/Corrections

d. Disposition

N	Ch. 8 Pg. 1 Para. 4 general	If gas-sorbing backfill is used, gas pressure might not build up f(v). (Brookins)	Comment noted, though gas sorbing backfill is a separate alternative. The paragraph is limited to minimization of space around the waste. (DV)
M	Ch. 8 Pg. 3 Para. 1 general	Yes, elimination of Fe containers greatly reduces the already small amounts of gas that might be generated, yet this would involve additional handling and exposure and extreme cost. Not a well thought through alternative. (Brookins)	Disagree. Relative to the cost of <u>not</u> opening WIPP or other waste treatment alternatives, container cost is cheap. Addition exposures will result as with any other treatment alternative. (DV)
M	Ch. 10 e n t i r e c h a p t e r (references)	References are incomplete in many cases and inappropriate. <u>All</u> personal communications should be so listed in the text and <u>not</u> included in the references. All legitimate references must have exact title, exact pagination, exact and complete location materials, etc. (Brookins)	The format followed in the report for personal communication references is the standard IT format. Efforts are underway to make references as complete as possible. (SC)
N	Ch. B Pg. 29 Para. 5 (B.13)	I suggest some commentary on other reactions that may be likely that could depressurize the system. (Brookins)	Other reactions that may be likely will be included in the final version of the report. (SMD)

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2. a. Project Number: 301302.01.06.02

b. Page 8 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance:

Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

M Ch. B
 Pg. 50
 Para. 3(?)
 last sentence
 on p.

Why is instantaneous dissolution of radionuclides in brine assumed? This needs verification and precise referencing, or a good explanation. (Brookins)

Leachability of the waste forms is difficult to quantify at this time. SNL has not yet attempted to do this for PA and are assuming an infinite leach rate, i.e., instantaneous dissolution (Rechard et al., 1990, p III-19). (SMD)

M Ch. D
 Pg. 1,5
Assumptions

These assumptions need precise documentation! Of the assumptions listed, I agree with the last one (...far-field pressure...remains constant) but not necessarily with the others. (Brookins)

A more detailed discussion of the assumptions will be provided in the final version of the report. (SMD)

N Ch. E
 Pg. 1
 Reactions
 E-1,2,3

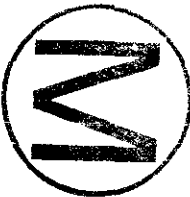
The pH of many attacked cements is above 10, hence CO₂, not HCO₃, may be the product. Reaction of the calciferous materials with other materials present (i.e., bentonite) should also be considered. (Brookins)

While other reactions and products can be produced, there is insufficient data to justify those reactions. The effect of portlandite reactions on peak index pressures is a negligible phenomena. The use of bentonite was not considered in the alternatives. (SMD)

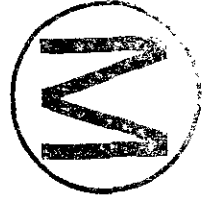
M Ch. F
 Pg. 4
 bullet three;
 assumptions

I do not agree that assumptions have been adequately described... and justified. (Brookins)

A more detailed discussion of assumptions and their justification will be provided in the final version of the report. (SMD)



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2. a. Project Number: 301302.01.06.02

b. Page 9 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

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N	Ch. G Pg. 3 Para. 4 (Microbial activity)	I recommend some discussion of the role of H ₂ S earlier in the report; i.e., either refute its importance/role early on or include proper discussion of it. (Brookins)	A brief note about the role of H ₂ S has been added in the revised Chapter 2. See page 2-3. (SC)
N	Ch. G Pg. 7 Para. 2 generic comment	Salt stored at the surface at WIPP has a recrystallized salt armor on it; would it be possible to re-create this kind of armor on the outer part of salt used as backfill? (Brookins)	Recrystallized salt armor requires the addition of water to the backfill. Addition of water underground is not advised. (MSA)
N	Ch. 1-D Pg. 2 Para. 3 (all)	Give reference for justification of sludge proportion. (Brookins)	Given, Vetter, 1990 (page 1-100). (FS)
N	Ch. 1-D Pg. 3 Para. 5 (generic comment)	It would be useful to provide an estimate of the non-radiological health effects. (Brookins)	Too complex to explain here. (FS)

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2. a. Project Number: 301302.01.06.02
 b. Page 10 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

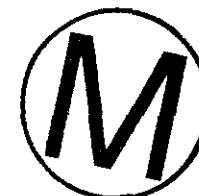
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N	Ch. 1-D Pg. 5 Para. 5 (RE: chemical wastes...)	Some VOCs may be partially encapsulated in solids and released only when shredded, thus the assumption made here may not be totally valid. (Brookins)	Added sentence in D.2.2.3, page I-103. (FS)
E	Ch. 1-D Pg. 18 Para. 4	Provide <u>date</u> for Bennet et al. (Brookins)	Done. See page I-116. (FS)
M	Ch. 1-E Pg. 2 Para. 5 Re comment on l i n e a r hypothesis	<u>Many</u> studies (i.e., NAS-NRC) argue convincingly that a quadratic, not linear, relationship holds for dose-health effect for radiation. Further, threshold must be considered. Further, hormesis must be considered. These possibilities, even if blatantly ignored for the purposes of this document, must be mentioned to the reader. (Brookins)	Only BEIR III, IV and V used by FSEIS and this study. (FS)
N	Ch. 1-E Pg. 14 Para. 2 ...density... s e l f adsorption...	A <u>reference</u> to document the assumptions made here is needed. (Brookins)	Done by explanation. See page I-158. (FS)
E	Ch. 1-E Pg. 82 Para. 1	Provide reference where indicated. (Brookins)	Done. (FS)



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2. a. Project Number: 301302.01.06.02
 b. Page 11 of 14

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

N	Ch. 1-E Pg. 107 Para. 2 -- e a r l y e f f e c t . . . r a d i a t i o n . . .	I am hard pressed to believe TRU wastes could be released in point-source quantities to cause radiation sickness! (Brookins)	So am I! Yet the neutrons and gammas could - just barely. (6.4.2.1, first paragraph). See page 1-241. (FS)
M	Ch. 1-E Pg. 108 Para. 1 ...cancer...etc	Even if the linear hypothesis is used, then in all fairness, a relative risk chart showing the same calculation for workers, others exposed to radiation from coal (mining, processing, transport, disposal) and from earth excavation operations where Th, U are above background is needed so that the reader of this document can put the TRU releases into proper perspective. (Brookins)	Absolute risks are for the FSEIS to discuss. Our scope is more narrow. (FS)
M	Ch. 1-E Pg. 119 Para. 2 . . . w i n d e r o s i o n . . .	Comparative figures for fly ash, cement wastes, uranium mill tailings, other mine wastes, coal piles and coal wastes are, again, needed to accurately inform the reader of the relative TRU risks involved. (Brookins)	Same as above. (FS)
E,M	Ch. 1-F Pg. 8	Section on discussion of Table F.2-2 is missing (Brookins)	Inserted. See page 1-281. (FS)

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 b. Page 12 of 14

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N GEN The mathematical treatment seems rigorous and complete. The numerous assumptions made throughout, however, may range from those that are strong to those that are weak. It is not always clear, even with discussion of errors, how the weighing of all parameters is carried out and, further, if the conservative approach used throughout is necessary. It is not! See discussion in Section 5.2.3, page I-49. (FS)

N GEN This document is, I think, intended to address high- to low-probability types of scenarios or incidents in which public health and environment can be impacted by TRU handling, transport and disposal. For the most part this objective is reached. Comment appreciated.

N GEN However, in order to put this document and its appendices into proper perspective, it is necessary to provide a ranking of relative risk for similar types of operations more familiar to the lay public. For example: risk of TRU transported wastes vs. transport of coal, chemical wastes, gasoline-other flammables, etc.; release radiation to the environment (on site workers to away from site) from coal processing, burning, disposal of wastes; cement production; comparison of TRU payload to arsenic, selenium, cadmium, lead, mercury from numerous operations (all with higher interaction potential to the public) such as coal, earth excavation, mining, processing, and so on. Outside the scope. (FS)



N GEN Further, would the scenarios presented in this report result in greater or less risk if applied to handling of chemicals? of coal? of petroliferous materials? of non-U mine wastes? and so on. In order for the public to get a true feel for the relative risk of TRU disposal at WIPP, a few of these scenarios should be run through for comparative purposes. Would be fun, but outside scope. (FS)

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b. Page 13 of 14

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N	GEN	No discussion is provided on background radiation and studies of areas of high (i.e., Minas Gerais, Brazil, Kerala, India) or normal-slightly elevated background radiation (i.e. China) areas. A comparison of worst case release of TRU radiation to increase in total US background radiation due to coal transport-process-burning and commentary on normal US background radiation fluctuations is highly recommended. Comparison with indoor radon risk is necessary, also.	Same as above. (FS)
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N	GEN	Even though the calculated probabilities for many of the scenarios are low, the text does not always emphasize this fact; hence the reader may be falsely concerned about low-probability incidents.	I tried to address that in many places. (FS)
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N	GEN	Some in-depth discussion of applicability of the linear hypothesis to radiation releases is a must. Without such a discussion, the report is severely weakened.	As far as needed, see Section 2.4.5, page I-27. (FS)
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b. Page 14 of 14

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N	GEN	The various sections on gas generation and behavior could be stronger; i.e., possibly discussed in greater detail or more fully referenced, etc.	A more detailed discussion of gas generation and coupling with other processes has been included in a revised Chapter 2.0, Section 2.1. (SMD)
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20 June 1991

TO: Mark S. Abashian
International Technology Corporation

FROM: Robert J. Budnitz
President

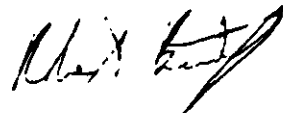
SUBJECT: Review Comments on Appendix I of Report
DOE/WIPP 91 - 007

A few days ago I received "comment resolution" responses intended to respond to my comments to IT on the referenced report. My comments had been provided in letters to Fritz Seiler of IT dated March 8 and March 22, 1991.

I have studied the information you sent, and I have no problem with any of it. I have signed the relevant sheets on each page, and am pleased to return them to you.

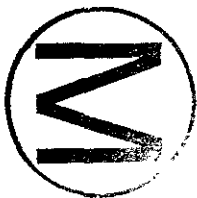
I continue to believe that one key criticism of mine holds. However, I suppose that on this matter Fritz Seiler and I will continue to "agree to disagree". This issue is fundamental to the whole study, and revolves around my conclusion that the use of risk reduction factors (RRFs) as the key figure of merit for the study, and especially the use of aggregations of RRFs, is not a sensible way to carry out the analysis nor to present the study results. Fritz disagrees. Oh, well.

Thank you very much for the opportunity to have participated in this effort, If there are any further questions, please don't hesitate to contact me.



Robert J. Budnitz, President





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3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

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N GEN

Technical Risk-Analysis Work: The complex technical risk-analysis work, aimed at determining risk-reduction factors for the many different risk endpoints and for 16 different alternative scenarios, is of high quality and deserves commendation. The technical information buried in the back of the Attachments to this report can provide an excellent basis for decision-makers to understand the various risk issues, and to make decisions about them. The choice of alternative scenarios, the assumptions made to limit the scope of the analysis, the risk endpoints identified, and the analysis methods used are all fully acceptable to me. I am particularly pleased with how the analysis of the specific risk endpoints was accomplished in a way that focused on the key issues relevant to the alternative scenarios. This part of the report can be a goldmine for further study by experts, as well as of use to decision-makers if presented properly.

Comment appreciated.

Some vital information is unfortunately missing--specifically, the base-case absolute numerical risk numbers are missing for some of the risk endpoints, because they were not otherwise available and were not developed specifically for this analysis. As I will discuss below, I believe that this missing information is essential to enable decision-makers to make sense of the overall results and then to make sensible decisions based on the analysis.

See later comments.

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R. J. Budnitz

b. Page 2 of 11

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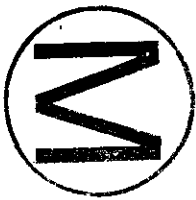
d. Disposition

N GEN

A Single Figure-of-Merit Useful for Decision-Makers: Although I admire the attempt to come up with a single figure-of-merit, useful to decision-makers, by which to judge the overall benefit/disbenefit of each of the various scenarios being studied, in my view the effort has not succeeded. The methodology does use established decision-theory methods to identify and calculate such a single figure-of-merit, and seems to break some new ground, but in my opinion the single figure-of-merit identified is not sufficiently useful to decision-makers to justify the continuation of work along these lines.

In fact, I believe that the use of a single figure-of-merit obscures rather than illuminates the situation. Decision-makers are in my opinion fully capable of dealing with multiple attributes presented separately, and of weighing them their own ways for decision-making purposes--this goes on every day in the upper-management board rooms of large enterprises and agencies. But to make these judgments, decision-makers need the best available disaggregated information about the issues at hand, in this case the best absolute numbers and uncertainties about the specific risk endpoints. I don't believe that high-level decision-makers generally use aggregated information very much or very well, and I certainly don't believe that the aggregated information based on the single figure-of-merit developed in this report will be of much use.

In our direct discussions we agreed to disagree. While I grant the use of disaggregated information by many decision makers in finance and industry, I maintain that you need a procedure such as the one proposed here if you must document and defend your decision before your superiors or in an adversarial procedure in court. In that situation, a "gut-feeling" evaluation process will not do. While a decision maker may arrive at a decision in this way, he may then use his insights in a quantified procedure such as the one suggested here. This is a major difference between private industry and the Department of Energy.



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- 1 a. Author/Editor: Mark Abashian/Fritz Seiler 2. a. Project Number: 301302.09.02.02
b. Reviewer: Robert J. Budnitz *[Signature]*
c. Date: 3/08/91 b. Page 3 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

5. a. M,N,E b. Where Located c. Comments/Corrections d. Disposition

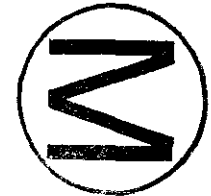
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Risk Reduction Factors and Their Aggregation: In my view, the approach of identifying and working with risk-reduction factors (RRFs) is a very useful intermediate step toward what is actually needed. Indeed, calculating RRFs is often simpler than calculating absolute magnitudes of risks, for reasons cited well in the report. However, I believe that for decision-makers these RRFs cannot adequately substitute for knowing the actual magnitudes of the risks involved as well as the RRFs, except in special cases, such as when almost no changes occur (RRF near unity) or when the absolute risk magnitudes are minuscule small for both the base-case scenario and the alternative scenarios.

I also do not believe that there is any sensible way for decision-makers to use an aggregated figure-of-merit developed by combining various different risk-reduction factors. Since RRFs are one step removed from the actual risks themselves, the aggregation of RRFs further obscures rather than illuminates the information, no matter which aggregation method is used. While I am especially bothered by the logic supporting aggregation by geometrical averaging--that is, averaging the logarithms--I don't think that any method of RRF aggregation can be helpful to decision-makers without the underlying information about the absolute risk magnitudes.

I agree with the basic observations. The baseline risk is important for any evaluation. However, an appropriate aggregation process does not obscure the issues; in fact, the weighted geometric average used here will result in meaningful supercomponents, if the aggregation extends over an appropriate set of risk reduction factors.

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c. Date: 3/08/91 b. Page 4 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

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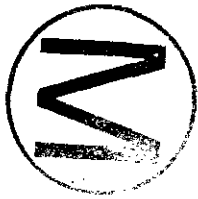
My Recommendation: I recommend that this project's effort be redirected toward the objective of developing absolute risk information for each of the many risk endpoints being studied (for the base case as well as the 16 alternative scenarios). Most of this information is already contained in the report, so developing the missing information does not seem very difficult, and especially because for some of the missing risk endpoints a bounding estimate can be used to establish that the risk is tiny, and can be dismissed.

Since all of the RRFs have already been developed, the absolute risk information can then be put together with the RRFs (multiplied) to provide a tabulation of all of the different risk endpoints for the base case and each alternative scenario. This should be the project's final product. I believe that it can be easily accomplished, and will in my opinion satisfy the needs of decision-makers about as well as is currently feasible using today's state-of-the-art in risk assessment. Without this absolute risk information, I do not believe that decision-makers can make effective decisions.

Certain of the many risk endpoints can be combined when they are fully commensurate--that is, when they are measured in the same units and decision-makers can otherwise easily figure out what the combined numerical values mean, and why. As an example, it may be helpful to decision-makers to add all near-term occupational deaths from all causes into a single aggregate figure. This will be relatively easy to do once earlier objectives are completed.

To summarize my perspective: As I see it, besides (i) the very nice risk-analysis work itself, the project has to date emphasized as its endpoint objectives the following two "products": (ii) developing RRFs and (iii) developing a single figure-of-merit as its approach to assist decision-makers.

Absolute risks for the alternatives are beyond the scope of this study, and are not one of the project deliverables. As for the rest, see earlier responses to comments on pages 2 and 3.



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b. Reviewer: Robert J. Budnitz *RJB*
c. Date: 3/08/91 b. Page 5 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

In my view, each of these two latter objectives is incorrect. There are thus two fundamental differences between what I would like to see and what has been accomplished so far in this project. They are:

See responses on page 4.

- I do not believe that risk-reduction factors are, by themselves, useful pieces of information for risk decision-making, absent the underlying numerical values of the risks themselves. Thus my emphasis would be different--I'd emphasize absolute risks, not RRFs.
- I do not believe that it is feasible to "combine" different incommensurable risks into a single figure-of-merit in a way that helps decision-makers. In my view, decision-makers require knowledge in disaggregated form about each of the many different risk attributes. Thus in my view efforts at combining them into a single figure-of-merit are intrinsically a waste of effort because rational decision-makers will always want the disaggregation. Again, my emphasis would be different--I'd develop a tabulation, including estimated uncertainties, and I'd be sure to annotate it thoroughly.

Given the two points, it follows that I also do not believe at all in the usefulness of approaches that combined RRFs.* [*As a subsidiary but key point, I also cannot conceive of any weighting method for RRFs that would be useful to decision-makers except one that weighs by the sizes of the underlying absolute risks.] To me, combining them, absent the underlying absolute risk numbers, produces a second layer of opacity that further obscures progress toward what should be the overall objective (giving useful information to decision-makers.)

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 c. Date: 3/08/91 b. Page 6 of 11

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5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page I-ES-1 First Para. The text says that the purpose of the study is "to evaluate numerically the balance between the changes in the short-term risk components and compare them to the expected improvement in the long-term risks." I concur in this broad objective. I question whether the approach of using a single figure-of-merit can best meet this objective. Since the short-term and long-term risk components are not commensurable (and not solely because of the discount-rate issue!), I believe that the only reasonable approach is developing a tabulation for a decision-maker to ponder. See earlier response to comment on "single figure of merit" on page 2.

Page I-ES-1 Second Para. The 4X4 matrix of four site combinations and four treatment options is in my view a fully acceptable approach to this study. It is rich enough to provide meaningful insights but not so complex as to overwhelm the decision-maker. Now put in executive summary. See pages I-ii and I-iii.

Page I-ES-2 Last Sentence on Page I don't agree that the "general result" is "insensitive even to the largest source of bias, the decision maker." How can this be so? Furthermore, it isn't necessary, or even desirable, that it be so! Quite the opposite--I'd like the decision-maker to weigh the multiple attributes in his head. In any event, the case is not made anywhere that I saw in the text to support this statement as written. In direct discussion we agreed to that statement if it pertains to the narrow grouping of Level II treatments and the spread of level III treatments.

Page I-ES-3 Second Para. The text tells us that scaling the risk components "to the level of activity required by the different treatment options" was "one of the primary challenges." I don't understand this comment. This seems on its face to be not nearly as difficult as many other aspects of the analysis, not the least of which is doing the risk analyses of the various components themselves. Toned down to "primary tasks". See page I-1.

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c. Date: 3/08/91 b. Page 7 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page I-ES-3
(Third Para)
through I-ES-6 (at top)
The text here presents a very nice summary of the range of issues covered in the risk analyses themselves.

Page I-ES-7
First full para.
The text says that the "risk comparison" . . . is based on the evaluation of the complete mathematical expression for the risk and a study of the treatment dependence of each parameter." This isn't correct as written. In any event it isn't necessary to develop such a "complete mathematical expression" to carry out the objective of this project. The results and insights don't require such, nor is it in fact achievable.
In order to avoid such an exact reading, the sentence has been reworded using "approximating the risk." See page I-4.

Page I-ES-7
Sec. 2.2.2.1
The assumptions here are fully acceptable, and even though they introduce biases the biases shouldn't be debilitating if explained clearly to decision-makers.

Page I-ES-8
(Sec. 2.2.1.2) and Table 2.1
The table is confusing, and doesn't track one-to-one with the text. I had a difficult time, at first, figuring out what was what, and finally did not only by going back to the main text.
Erroneous insertion of an old table corrected. See Table I-2-1 on page I-7.



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c. Date: 3/08/91  b. Page 8 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page I-ES-18
Sec. 2.3

The scenario descriptions are very helpful as introductions to the main analysis. The accident scenarios (Sec. 2.3.2) are especially nicely written, and at the right level of detail for an executive summary. However, I'd suggest that it would be helpful to add, for each scenario, exact descriptions of the one or more risk endpoints that will be calculated. If placed either in the text or in tabular form, such descriptions could help guide the reader to just what is actually to be analyzed in the body of the report.

Will be done in final draft in July which will include a table of all components, end points, etc. (new Attachment A).

Page I-ES-24
Sec. 2.3.5

The discussion of the human-intrusion scenarios is too abbreviated in my view. If I were not already intimately familiar with the scenarios from other work for Sandia/WIPP, I would not have been able to follow them well enough. These need some further explanations, for example to describe what risk endpoints are to be analyzed, and why, and how. The reader ought to be introduced, for example, to the issue of driller doses vs. homeowner doses at the 5,000-year time horizon, and the issues of direct consumption vs. airborne pathways vs. ingestion pathways. This is the place to discuss those ideas, to give the decision-makers an overview of the issues.

Descriptions are expanded. See pages I-22 and I-23.

Page I-ES-27
over to
I-ES-28

The assumptions all seem OK to me, except for one: what does it mean that "somatic effects of radiation are not evaluated"? I believe that cancer is a somatic effect, and that the term "somatic effects" is usually used mainly to distinguish them from genetic and/or teratogenic effects.

Corrected to "somatic effects other than cancer". See page I-25.

Page I-ES-29
Sec. 3.1.1

This is a nice description of risk in terms of scenarios, consequences, and probabilities.



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 c. Date: 3/08/91 b. Page 9 of 11

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4. Approval for Report Issuance: Date:

5.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page
I-ES-29 over to
Page 30 (top)

The last sentence on p. 29 says that "For any multi-component risk, this means that a formulation must be found to reduce the multitude of component values to one characteristic measure which can then be compared to two alternatives." I strongly disagree with this sentence. By analogy, can there ever be one single measure of a baseball shortstop's effectiveness? Don't hitting, fielding, running, teamwork, and so on, all need to be evaluated? And don't different situations? Why pretend that they can be lumped into "one characteristic"?

See earlier response to comment on "single figure of merit" on page 2.

Added a paragraph on evaluations based on more detailed information. See pages 1-28 and 1-29.

Page
I-ES-30
Sec. 3.1.3
First Para.

There are a number of advantages to using risk-reduction factors, and they are cited well here. However, this does not lead logically to the conclusion in Section 3.1.4 (see next comment).

See next statement.

Page
I-ES-31
Sec. 3.1.4
Second Para.

The text tells us that "aggregation is best done at the level of risk reduction factors." I strongly disagree, for reasons cited above. I also believe that the text doesn't support this conclusion, either here or in the backup material in Attachments B and C. The text goes on to tell us, concerning the problem of large RRFs on small risks dominating the arithmetic, that "weighted averaging of the components will avoid that problem." Furthermore, an "unweighted geometric average is usually preferable." This is wrong in my view. I have already stated why I think that any weighting scheme not tied directly to the underlying absolute risk values cannot be correct. But I'm all the more mystified by the geometric-average approach. *Elegant mathematics aside*, how can it be correct as a matter of common sense? If a very large RRF applied to a minuscule risk really makes no difference to a decision-maker, then it is best ignored, not geometrically averaged! The only way to "ignore" it is to consider the absolute size of the risk, thereby observing how small it actually is. No other way can or will work.

The appropriate weighting scheme is discussed in more detail now, particularly in Attachment G to illustrate that the principles of the report and those here are in agreement and are adhered to as far as possible. See Attachment G.



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c. Date: 3/08/91 b. Page 10 of 11

3. Report Title: EATF Final Report of the Engineered Alternatives Task Force (Draft, Jan. 1991)

4. Approval for Report Issuance: Date:

S.a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page
I-ES-32
Sec. 3.2.2
The elegance and rigor of using multi-attribute utility functions cannot substitute for the observation that they are not useful to decision-makers, in my view. Therefore, although they are attractive to me theoretically, I recommend that they be abandoned, "unique interpretation" (Page I-ES-32, last paragraph) or not.

See earlier statement.

Page
I-ES-33
Sec. 3.3
This section on ranking, and the backup material in the body of the text, are interesting but not very useful. I don't think anybody needs lessons on how to decide whether or when the ordinal ranking of things is "indifferent." People intrinsically do this sort of thing every day, and mathematical rigor is not very helpful to them.

This study was in luck insofar as the errors do not overlap too much, so that these criteria are not needed except in a few cases.

Page
I-ES-33
Sec. 4 on
Scaling
Much of the text here was very useful, but the discussion on "scaling" in the first two paragraphs seems trivial to me, although I suppose that it needs to be said.

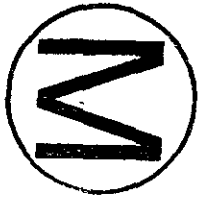
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Page
I-ES-38
Sec. 4.3.2
The discussion of "supercomponents" is very useful, although the logic of dealing with RRF averages is wrong in my view, as stated earlier. Some nice insights can found in this section!

--

Page
I-ES-41
Sec. 5.1.1
The text on societal weighting bothers me. I cannot find any justification for the relative weights of 10, 7, 5, and 4 chosen for the first four risk supercomponents--nor can I find justification here or elsewhere for the weights of 1, 0.3, 0.1, and 0.2 for the last four. In my opinion, this is precisely where decision-makers need to intervene before, not after, the analysis is completed and the details obscured by the weighting process.

The decision maker does come in, right here. More text has been added here and in Section G to stress this point. See pages I-43 to I-45.



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c. Date: 3/08/91 b. Page 11 of 11

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4. Approval for Report Issuance: Date:

5. a M,N,E b. Where Located c. Comments/Corrections d. Disposition

Page I-ES-44, Figure 5-1. The figure is marvelous as a means of displaying information, including the uncertainties! Unfortunately, the information displayed is flawed in my view for reasons cited elsewhere. Congratulations on this figure!

Page I-ES-46 Sec. 5.2.2 This section is very useful, although because of the way that the aggregation of RRFs was done, I cannot know whether this information is actually robust based on the underlying analysis.

Page I-ES-47 Sec. 5.2.3 This section, like 5.2.2, puzzles me. In the third paragraph, the text says that "The unavoidable use of unweighted aggregation introduces biases of unknown amounts and signs. It is believed that, by the choice of supercomponents and the use of the geometrical averages, their influence is kept as small as possible." I do not believe this.

--
Added a section on biases. See pages I-49 and I-50.

As we discussed, this is based on the low risks of all accidents combined.

RESOLUTION TO COMMENTS ON ATTACHMENTS D, E, AND F

Page 1, Section entitled "Introduction"

No resolution deemed necessary.

Page 1 and 2, Section entitled "Conservatism versus Realism"

We acknowledge that, at times, the analysis is extremely conservative and does approach worst-case scenarios which are unlikely to happen in reality. However, from our experience thus far in the area of transuranic (TRU) waste (especially in our dealings with various regulatory agencies), we find that more often than not a worst-case scenario is required or recommended by these agencies. As an example, although the estimated time of transporting TRU waste from the sites to WIPP is about 3 to 5 days, the Nuclear Regulatory Commission (NRC) requires that many of the calculations (e.g., decay heat limits for each waste category) are based on an extremely conservative shipping duration of 60 days. Thus, our experience tells us that it is preferable to err on the side of being overly conservative rather than being realistic.

Page 2, Section entitled "Simplicity"

No resolution deemed necessary.

Page 3, Section entitled "Applicability"

We agree with the reviewer that, by necessity, the models were often a bit too crude, as is the case of the C10 accident. Also, the assumptions are frequently too pessimistic. We are, however, bound here by the assumptions of the FSEIS, which are part of the baseline risk. In some cases, altogether too pessimistic assumptions can be canceled by an appropriate scaling model. Unfortunately, the "no respirators are donned . . ." assumption, for which the absolute risks are calculated in the FSEIS, is not of that type. A comment on biases introduced into a risk comparison by the practice of calculating "worst case" risks and a place for using "best estimate" risks can be found in Section 5.2.3.

Overall, we agree with the reviewer that some of the modeling assumptions are "crude and probably pessimistic." The underlying bases behind some of these assumptions are explained above. Also, as pointed out by the reviewer, the applicability of the models seems adequate for the job at hand, given the complexity of the issues and the time constraints involved in the project.

Page 4, Section entitled "Linearity"

It is true that the assumption of linearity may not always be valid for every situation imaginable. However, as the reviewer himself points out, such issues are not generally expected to





"invalidate" the analyses, given the approximate nature of these analyses. We believe that under the constraints involved, a deviation from the linearity assumption would have complicated the analyses beyond the scope of the project.

Page 4, Section entitled "Broad Comments on the Data Used in RRF Analysis"

We agree with the reviewer that it is important for the decision makers to know that "those insights dependent on detailed numerical values are probably less useful than those in which the specific numbers don't matter much." The limitations of the risk analyses presented in Appendix I, and recommendations to decision makers about its applicability, are explained in Section 9.2 of the revised version of the report. Please see highlighted text on pages 9-7 and 9-8 of Revision 2 of the report.

Page 5, Section entitled "Broad Comments on the Assumptions"

The reason behind selecting worst-case scenarios instead of realistic ones is explained by the resolution to similar comment earlier.

Page 5, Section entitled "Uncertainties"

We agree that uncertainties in the risk modeling are often understated, particularly because no time was available to do a decent job on estimating the influence on systematic errors. In some cases, we are also convinced that the standard random errors should be larger, but by how much? There simply was not enough time to do a proper evaluation. In the final version, there will be a new Section 5.2.4, inserted before Section 5.2.5, Conclusions, which will discuss the underestimate of the standard errors and their consequences.

Page 6, Section entitled "Overview or Summary Comments"

We agree with the reviewer that additional effort is needed to calculate absolute risks to assist the decision maker. We acknowledge the importance of the absolute risk values, but would like to reiterate that these were not one of the deliverables for this project. The highlighted text on pages 9-7 and 9-8 from the latest version of the report underlines the importance of calculating absolute risk values.



(Section 9.2.3). Thus, the decision maker must weigh the uncertainties involved against the relative importance of schedule concerns, and then decide if additional data are required to make a decision.

Application of EATF Regulatory Analysis - The EATF analysis presented in Section 6.0 shows that significant uncertainty exists in the area of regulatory compliance, and therefore it is difficult to estimate the time periods required for permitting a facility. The EATF has considered the experience of other projects in various states, and also the different state and federal regulations that affect the permitting process. It has been observed that in general, the timeframe required for facility permitting varies with the type of facility being considered (i. e., Level II or Level III), and the proposed facility location.

As discussed in Section 6.0, waste characterization may be required to comply with the New Mexico EID or RCRA requirements. The extent of waste characterization required by RCRA will have a significant influence on the choice of an alternative, especially if the cost of such characterization is comparable to the cost of processing the waste. At the moment, the extent of characterization required by RCRA is not well defined, and thus increases the uncertainties of estimating the requirements for regulatory compliance. Therefore, although the various factors that affect the regulatory issues have been explained in Section 6.0, the EATF has refrained from presenting precise estimates of facility permitting time.

Application of EATF Risk Assessment - The risks associated with implementing candidate alternatives can be compared with the baseline design using the results of the risk assessment summarized in Section 6.0 and discussed in detail in Appendix I. Although the analysis presented in Section 6.0 relates to Alternatives 1, 2, 4, and 8, and does not include all 14 alternatives, the four options analyzed by the EATF represent the whole range of treatments involved in the 14 alternatives. For example, the waste treatment involved in Alternative 5 is similar to that of Alternative 4, and the waste treatment involved in Alternatives 9 and 13 are similar to Alternative 8.

The results of the EATF risk assessment show that Level II treatments result in a slight increase in risk relative to the baseline design, and this increase is generally independent of the number of facilities. In contrast, for Level III treatments, the dominance of transportation risks favors treatment of wastes at multiple facilities before transporting the wastes to WIPP. This is because the Level III treatment of waste before shipment substantially reduces the transportation risks, and this reduction more than compensates for the increase in occupational risks associated with the Level III treatment of waste.

Since risk is an abstract quantity, the results of any risk analysis are often used by equating risk to some tangible quantity (e. g., the number of lives saved by an unit decrease in absolute risk). However, the use of such an approach is not recommended for the results of the EATF risk assessment for reasons outlined below.

The EATF analysis has involved estimating the risks of alternatives as ratios relative to the baseline case. This approach was used to cancel the uncertainties that are common to the baseline case and the alternatives. In theory, one could take the relative risk reduction ratios estimated by the EATF, and combine these numbers with the absolute baseline risk provided in the FSEIS/FSAR to arrive at an absolute risk for each alternative. However, it should be noted that this would only serve to bring back the uncertainties present in the FSEIS/FSAR calculations, and therefore not advisable.



Regulatory Considerations - Since Alternative 4 involves thermal treatment, it would be expected that the regulatory requirements for this alternative would be more extensive than Alternative 3.

Schedule - Alternative 3 would take 5-7 years for implementation, whereas Alternative 4 would take 8-11 years.

Cost - Project costs for Alternative 4 are expected to be substantially higher than for Alternative 3.

Effectiveness - Alternative 4 would reduce peak index pressures to lithostatic. Alternative 3 would be ineffective in reducing pressures to lithostatic, and the maximum peak index pressures predicted for this alternative are 20% higher than the lithostatic pressure. In general, Alternative 3 may be more effective against human intrusion, whereas Alternative 4 is more effective for addressing gas generation.

The interactive processes that are expected to be involved in such an optimization process are illustrated below with an example. Assuming that the candidate alternatives are limited to Alternatives 3 and 4, the analysis of the EATF would provide the reader with the following results:

Once the data on the effectiveness and feasibility of candidate alternatives have been compiled, the next step is to use the data to select an optimal alternative. As shown in Figure 9-1, the optimal alternative should be decided by simultaneous consideration of five different components for each alternative. These are effectiveness, cost, schedule, regulatory considerations, and the health and safety risk associated with an alternative. The reader should note that while a relative measure of these factors can be obtained for each alternative using the results of the EATF, their relative importance needs to be decided by the eventual decision maker (e. g., the DOE). Unless the relative importance of these factors is established, the results of the EATF for each factor will remain mutually exclusive, and therefore cannot be aggregated for optimizing the choice of an alternative.

9.2.3 Selection of an Optimal Engineered Alternative

Thus, while applying the results of the risk assessment, the reader should remember the limited scope of this study. As explained in detail in Table I-ES-1 in Appendix I, the different combinations of alternatives and treatment locations fall into four different risk groups. The EATF feels that while a more rigorous analysis might change the numerical values of the risk reduction factors within each of the four groups, it is unlikely to result in reclassification of any option from one group to another.

If it is not possible to make a decision without the absolute risk values for the alternative, then the EATF recommends either one of two optional approaches. The first approach involves an estimation of the absolute baseline risks using a more rigorous method than the one used in the FSEIS/F SAR. This should reduce the uncertainties, and the EATF results of relative risk could then be combined to arrive at absolute risks for the alternatives. The second approach is less rigorous, and involves expressing the value of relative risk reduction for each risk component (e. g., transportation fatalities, etc.) in terms of a tangible quantity such as the number of lives saved, etc. Once each of the eight components considered by the EATF have been expressed in terms of a tangible quantity, they could then be aggregated using societal weights for each component to arrive at a tangible value of risk reduction for each alternative.