176
4-15-77

GENERAL ATOMIC

GA-A13617
VOLUME VIII
UC-77

Rn 904

HTGR ACCIDENT INITIATION AND PROGRESSION ANALYSIS STATUS REPORT

VOLUME VIII RESPONSES TO COMMENTS ON AIPA STATUS REPORT

Prepared under
Contract EY-76-C-03-0167
Project Agreement No. 51
for the San Francisco Operations Office
U.S. Energy Research and Development Administration

DATE PUBLISHED: JANUARY 1977

DISTRIBUTION OF THIS DOCUMENT IS UNLIMITED

NOTICE

This report was prepared as an account of work sponsored by the United States Government. Neither the United States nor the United States Energy Research and Development Administration, nor any of their employees, nor any of their contractors, subcontractors, or their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness or usefulness of any information, apparatus, product or process disclosed, or represents that its use would not infringe privately owned rights.

Printed in the United States of America
Available from
National Technical Information Service
U.S. Department of Commerce
5285 Port Royal Road
Springfield, Virginia 22161
Price: Printed Copy \$8.00; Microfiche \$3.00

DISCLAIMER

This report was prepared as an account of work sponsored by an agency of the United States Government. Neither the United States Government nor any agency Thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of any information, apparatus, product, or process disclosed, or represents that its use would not infringe privately owned rights. Reference herein to any specific commercial product, process, or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply its endorsement, recommendation, or favoring by the United States Government or any agency thereof. The views and opinions of authors expressed herein do not necessarily state or reflect those of the United States Government or any agency thereof.

DISCLAIMER

Portions of this document may be illegible in electronic image products. Images are produced from the best available original document.

Volume VIII was prepared by the Safety and Reliability Branch of the Plant Engineering Division under the supervision of V. Joksimovic, Branch Manager, and W. J. Houghton, Section Leader.

Coordinating Author: P. H. Raabe

Contributors:	P. H. Raabe	D. D. Orvis
	D. Alberstein	T. Pasternak
	A. W. Barsell	R. D. Pfremmer
	R. K. Deremer	J. N. Sharmahd
	F. S. Dombek	F. A. Silady
	K. N. Fleming	R. J. Stokely
	G. W. Hannaman	G. R. Tully
	D. L. Hanson

1

2

3

CONTENTS

1.	INTRODUCTION	1-1
	References	1-3
2.	SUMMARY.	2-1
	2.1. Plateout	2-2
	2.2. Earthquake Occurrence Rates.	2-4
	2.3. Validity of Data	2-6
	References	2-8
3.	U. S. NUCLEAR REGULATORY COMMISSION.	3-1
	References	3-1
	3.1. Comments from Attachment 1 of Ref. 1 on P. 3-1	3-2
	References	3-68
	3.2. Comments from Attachment 2 of Ref. 1 on P. 3-1	3-69
	References	3-106
	3.3. Comments from Attachment 3 of Ref. 1 on P. 3-1	3-107
	References	3-128
	3.4. Comments from Attachment 4 of Ref. 1 on P. 3-1	3-129
	3.5. Comments from Attachment 5 of Ref. 1 on P. 3-1	3-134
	References	3-212
	3.6. Comments from Ref. 2 on P. 3-1	3-214
4.	AEROJET NUCLEAR COMPANY.	4-1
	4.1. Review of AIPA Methods and Technical Work.	4-2
	4.2. Use of AIPA Results for Absolute Comparisons of Risk . . .	4-42
	4.3. The ERDA-NRA R&D Decision Criteria	4-55
	Reference.	4-62
5.	COMMENTS BY DR. J. B. FUSSEL, UNIVERSITY OF TENNESSEE, CONSULTANT TO ORNL	5-1

1. INTRODUCTION

The first seven volumes of this report provide formal documentation of the status of the ERDA-sponsored Accident Initiation and Progression Analysis (AIPA) study as of the end of FY75. That portion of the report was given broad distribution to such government agencies, industrial organizations, and academic institutions as the following:

1. The general UC-77 distribution, which includes:
 - a. Various ERDA offices, such as the Division of Nuclear Research and Applications, Division of Naval Reactors, Division of Operational Safety, and the ERDA Library.
 - b. Various NRC offices, such as the Director of Licensing, Director of Regulation, Division of Nuclear Regulatory Research, Division of Reactor Safety Research, and the NRC Library.
 - c. The individual members of the ACRS.
 - d. Such national laboratories as ANL, Batelle Northwest, BNL, LASL, and ORNL.
 - e. Other governmental agencies, such as EPA, NASA, National Technical Information Service, and Library of Congress.
 - f. Such utilities as Philadelphia Electric Company and Public Service of Colorado, together with the Electric Power Research Institute.

- g. Such industrial firms as Allied Chemical Corporation, General Electric Company, and Westinghouse Electric Corporation.
2. Aerojet Nuclear Company (ANC), a company under separate contract with ERDA to perform an independent review of the AIPA study.
 3. Academic institutions, including MIT (Dr. Norman Rasmussen, director of the Reactor Safety Study, Ref. 1), UCLA (Dr. David Okrent, a member of the ACRS), and the University of Tennessee (Dr. J. B. Fussell, a consultant for ORNL).
 4. Several other organizations (upon their request), including the California State Energy Commission, Battelle Columbus Laboratories, Combustion Engineering, General Electric Company, Project Management Corporation, Science Applications Incorporated (forwarded by EPRI for review), and Pickard, Low, and Garrick.
 5. Organizations involved in evaluations of gas-cooled-reactor technology, including A. D. Little, United Engineers, Nuclear Utility Services, and Associated Nuclear Services of Great Britain.
 6. Several European-based organizations (predominantly upon their request), such as Hochtemperatur-Reaktorbau (HRB), Kernforschungsanlage (KFA), and Bonnenberg + Drescher in Germany; Commissariat à l'Energie Atomique (CEA) in France; Ab Atomenergi and the Swedish Board for Energy Source Development in Sweden; and the director of the Safety and Reliability Directorate of the United Kingdom Atomic Energy Authority (UKAEA) in the United Kingdom.

Comments on the Status Report have been actively solicited from these and other organizations. The present volume (the eighth in the AIPA Status Report) documents all of the formal written comments that have been

received as of September 30, 1976, together with the responses to those comments.* The comments as presented herein are direct quotations from the manuscripts as submitted by the reviewers; none have been paraphrased. The comments are presented in the same order as submitted by the reviewers and are generally addressed individually.

The detailed comments and responses are presented in Sections 3 through 5, with a brief summary discussion provided in Section 2. Section 3 covers the comments as transmitted by personnel in the following NRC and NRC contractor organizations (Refs. 2 and 3): Office of the Executive Director for Operations (NRC), Division of Project Management (NRC), Division of Reactor Safety Research (NRC), HTGR Safety Evaluation Division (BNL), RSR HTGR Safety Research Program (LASL), and Office of HTGR Safety Studies for RSR (ORNL). Sections 4 and 5 cover comments provided by Aerojet Nuclear Company (Ref. 4) and J. B. Fussell of the University of Tennessee (Ref. 5).

Throughout this volume, frequent references are made to WASH-1400 (Ref. 1) and the AIPA Status Report (Ref. 6). Full reference information is provided for them in this section. In other sections the callouts for these two references are abbreviated.

REFERENCES

1. "Reactor Safety Study -- An Assessment of Accident Risks in U.S. Commercial Nuclear Power Plants," NRC Report WASH-1400 (NUREG 75/014), October 1975.
2. Knuth, Donald F., letter of transmittal of comments by NRC personnel on R&D decision criteria to Douglas C. Bauer of ERDA/NRA, April 26, 1976.
3. Knuth, Donald F., letter of transmittal of comments by NRC and NRC contractor personnel on the AIPA Status Report to Douglas C. Bauer of ERDA/NRA, May 17, 1976.

* Since the comments prepared by Science Applications Incorporated arrived after the cut-off date, the responses to their comments are not addressed herein but have been addressed separately.

4. Stewart, Milan E., "GCR Accident Review -- FY-1976 Report," Report No. RE-S-76-140, Idaho National Engineering Laboratory, Aerojet Nuclear Company, July 1976.
5. Cole, T. E., of ORNL, letter to V. Joksimovic of GA, transmitting review comments by J. B. Fussell of University of Tennessee.
6. "HTGR Accident Initiation and Progression Analysis Status Report," ERDA Report No. GA-A13617 (seven volumes), General Atomic Company, published October 1975 to February 1976.

2. SUMMARY

It has been gratifying that the AIPA Status Report has been so well received. In a report documenting the review comments by ANC (Ref. 1), it is noted that ". . . a considerable amount of excellent work has been compiled in the report." ANC also states that "GA has made important additional contributions to the methodology in the areas of accident initiating event identification, the modeling of common mode failures, human factors modeling, and particularly the staging of the analysis effort so that no undue attention is given to details." A letter transmitting the comments of NRC personnel (Ref. 2) states that "The AIPA study . . . is a valuable initial contribution to the quantification of public risks associated with HTGR systems." Another letter of transmittal (Ref. 3) provides a summary of the detailed comments submitted by NRC and NRC contractor personnel. This letter states that "The work which has been performed by ERDA contractors in this study is generally of good quality and makes a substantial contribution to HTGR technology. There is more to be done, and it is hoped that ERDA will continue to support this effort both in the extension of sequences considered and in the refinement of methodology and data." The letter goes on to state that "Comparability with the Reactor Safety Study for LWRs, WASH-1400, would be very valuable." One of the detailed comment sets (transmitted as Attachment 5 to Ref. 3) indicates that, while the reviewers took exception to certain assumptions, methods, and conclusions of the AIPA study, they found ". . . the AIPA study a disciplined and useful discussion of HTGR safety and risks." It is further stated that "On balance, we believe that the current benefits of the study, with respect to the potential guidance it offers to design efforts and to R&D planning for HTGRs, outweigh the difficulties in performing the study and the criticism that the study may be 'premature.'"

Within the framework of the overview comments, the detailed comments (which are presented in Sections 3 to 5 below) provide an independent critique of the study and serve as a useful basis for upgrading and improving the study effort as it is continued. These detailed comments identify areas in the Status Report that require some further clarification, and they indicate areas of reviewer disagreement with study assumptions, methodology, analyses, results, and conclusions. With regard to clarification, responses have been prepared to provide additional information in an effort to enhance clarity. With regard to the various concerns brought out by the comments, it is believed that these areas of reviewer concern generally have little or no effects on study results and virtually no effects on study conclusions. However, the summary provided in Ref. 3 identifies three areas of concern which the reviewers believe may potentially affect the study -- namely, plateout within the PCRV, earthquake occurrence rates, and validity of data. The summary of these three areas as given below indicates that, even though the computations have been modified along the lines suggested in the reviewers' comments, the associated impact on study results and conclusions is minor. Nonetheless, in view of the complexities of these areas, the AIPA study team does not consider them to be closed.

2.1. PLATEOUT

Two comments^{*} raise specific concerns about the very low nonplateout fractions cited in the Status Report for plateout within the PCRV under core heatup conditions. These concerns arise from questions raised as to the applicability of the King furnace data in estimating the escape fractions. The comments point out that some of the surfaces available for plateout in the King furnace experiments were at temperatures much lower than would reasonably be expected in the PCRV. One comment mentions that large differences exist between the escape fractions presented in the AIPA Status Report and those estimated for the Windscale accident.^{**}

^{*} Comment 1 in Section 3.2 and Comment 5 in Section 3.3.

^{**} The comment mentions that the pressure, temperature, and flow rates in the Windscale reactor differ from those in an HTGR during accidents.

In responding to the comments, it is noted that there is really very little physical basis for comparing the Windscale accident with potential HTGR accidents. The Windscale No. 1 Pile was air-cooled and normally operated at 200°C or less. It appears most probable that the introduction of water (to put out the fire) plus the low temperatures (lower than those of the HTGR) resulted in the formation of large quantities of volatile iodine compounds (particularly methyl iodide), which are not readily adsorbed on stacks or particulate filters. Since the formation of such penetrating iodine species would not be expected to an appreciable extent in HTGRs, it seems reasonable to expect PCRV escape fractions to be lower for HTGRs than for those experienced during the Windscale accident.

In an effort to enhance the credibility of the estimated PCRV escape fractions, the analysis was modified recently to make use of both the plateout data from the King furnace experiments and that from the Windscale accident. Recent efforts have also entailed the development of a more realistic model of fission product transport from the PCRV to the containment. The old model, upon which the Status Report was based, assumed an instantaneous transport, reflecting the licensing background; the new model is time dependent.

The two basic modifications mentioned above work in opposite directions. The results (discussed in greater detail in the response to Comment 1 in Section 3.2) show a modest net decrease in the whole body gamma dose. The associated impact of these recent modifications on latent health effects is also small. At the 10^{-9} per year frequency, the interim estimates of health effects are ≤ 5 latent cancer fatalities and ≤ 40 thyroid nodules. This compares with the ≤ 1 latent cancer fatality and ≤ 5 thyroid nodules that are derivable from the results presented in the Status Report. Thus, the modifications have only a minor effect on the numerical results of the study over a meaningful range of frequencies and no effect on the study conclusions. However, the need for further work on PCRV plateout is recognized, and ERDA-funded experimental and computer code development efforts along this line are in progress. It is intended that the modified results presented in this volume be used until such time as these (or other) efforts

provide an indisputable technical basis for establishing revised estimates of PCRVR plateout.

2.2. EARTHQUAKE OCCURRENCE RATES

One comment^{*} raises concern about the very low estimates cited in the Status Report for the occurrence rates of large seismic events, quoting the value of 1.5×10^{-8} per year for an SSE to illustrate their point. The comment indicates that the occurrence rate estimates for large seismic events should be increased by several orders of magnitude.

The response to this comment points out that the state-of-the-art of predicting earthquake occurrence rates at specific locations (like the sites of nuclear power plants) does not appear to be very advanced. In support of this it is noted that occurrence rate predictions provided by several seismic experts for each of several sites exhibit wide variations at each site (Ref. 4). Furthermore, there is diversity in opinion as to the correct fundamental form of the formulation for seismic attenuation, a consideration that can significantly affect occurrence rate estimates.

While the basis for the comment was not explicitly identified, it was surmised that it stems from the occurrence rate estimates given in WASH-1400,^{**} where the occurrence rates for ground accelerations in the vicinity of those normally specified for SSEs are of the order of 10^{-4} per year. The occurrence rates given in WASH-1400 have been quoted directly from a UCLA report by Hsieh et al. (Ref. 5). For this reason, the Hsieh report serves as the fundamental source for the higher occurrence rate estimates considered in the response.

The results presented in the Hsieh report appear to be consistent with the report title in that the occurrence rate values represent averages as estimated over two large land areas (the areas east and west of the Rockies) in the continental U.S. No statements have been identified in the report

^{*}Comment B-4 in Section 3.5.

^{**}Main Report, Section 5.4.1, Table 6.

that indicate that the seismicity within either area is uniform or that the reported occurrence rate estimates can properly be applied to any specific locality, such as the site of a nuclear power plant. To the contrary, the report itself indicates that the occurrence rate estimates presented therein tend to overestimate the true occurrence rates for localities with lower than average seismicity. Furthermore, there are data that give a strong indication that seismicity is not uniform over the large areas considered in the Hsieh report (Refs. 4, 6, and 7). This notion of seismic non-uniformity is further enhanced by the site-specific provisions of 10CFR100, Appendix A. Thus, it seems reasonable to conclude that the occurrence rates presented in the Hsieh report are not necessarily representative of those at the sites of nuclear power plants and that the Hsieh results tend to provide overestimates in these instances since it is generally intended that such siting be in localities of lower than average seismicity. As a consequence of these observations, it is believed that the occurrence rate estimates given in WASH-1400 for large seismic events (which are quoted directly from the Hsieh report) are too high. While the use of such high estimates may be consistent with the conservatism that have traditionally been employed in the licensing process, such usage is not consistent with a basic AIPA study objective for analysis realism.

In view of the current state-of-the-art of techniques for estimating earthquake occurrence rates at specific sites as a function of earthquake size, the analysis was revised recently to make use of estimates derived from both the Hsieh report and the AIPA Status Report in order to enhance the credibility of the resulting estimates. These revised occurrence rate estimates have been developed by evaluating the geometric means of the high estimates from the Hsieh report and the median estimates given previously in the AIPA Status Report. The modified occurrence rate estimates obtained in this manner are about two orders of magnitude higher than those given in the Status Report. These results, when combined with the consequence modifications discussed in Section 2.1, reflect a modest change in the risk estimates and necessitate no changes in the study conclusions. In view of the foregoing discussion, it is recognized that there is a need for improving the state-of-the-art of estimating the occurrence rates for large seismic events at specific sites.

2.3 VALIDITY OF DATA

Several comments^{*} raise concerns with regard to limitations of the data base used for the AIPA study. The comments point to a need for further knowledge and experience due to the early state of HTGR developmental and operational history. They indicate that, although the available data base is limited, it could have been used more effectively. The lack of formal references to Peach Bottom I, Fort St. Vrain, and European gas-cooled reactor experience is mentioned, and the use of operating experience for comparable components is questioned because of the potential effects of differences in component operating conditions, maintenance intervals, and inspectability.

The responses to these comments bring out several important considerations. Although the operational history of the HTGR itself is quite limited, the AIPA study was not hindered by this because there is a wealth of operating history of power plants (both in the U.S. and abroad) that is directly applicable to HTGRs. Some equipment items in the HTGR, particularly in the balance of plant, have operating conditions and environments similar to those of many such items in LWRs and fossil-fired power plants in the U.S., which makes all of the associated operational data directly applicable to the HTGR. The substantial experience base developed from European gas-cooled reactor operation was applied^{**} where the differences in design configuration and coolant type had little effect on operational performance characteristics. In cases where HTGR equipment applications differed significantly from those in other power plants, larger uncertainties were used. Since the Fort St. Vrain data were limited primarily to construction and subsystem testing experience at the time of Status Report publication, those data were of limited value. However, Peach Bottom I operating experience was available for the AIPA Status Report in both the probability and

^{*}Comments A-4 and A-5 in Section 3.5 and the comments in Section 3.4.

^{**}Because of the limited availability of data from foreign experience, an effort to improve the availability of such data has been made via such foreign contacts as membership in the Systems Reliability Services of UKAEA. In addition, the services of a British consultant were contracted throughout the study.

the consequence evaluations. The most useful Peach Bottom I operating experience involves material performance in high-temperature helium and the behavior of graphite fuel.

It is also noted that, for risk assessment purposes, it is frequently unnecessary to obtain information at the same levels of assembly as required for many of the more traditional reliability analyses performed in support of code requirements for specific subsystems. Such reliability analyses are often performed at the lowest levels of assembly. On the other hand, the AIPA risk assessment was performed for the complete plant. While the risk assessment required the same relative depth of analysis as is typically needed for traditional reliability analyses, the risk assessment generally dealt with information from the highest levels of assembly. Thus, less emphasis tended to be placed on the lower levels of assembly since such considerations generally had a small impact on the overall results. Instead, attention was focused primarily on system interfaces and the evaluation of common mode failure characteristics, because these considerations have a significant impact on the assessment of plant risk. In conjunction with this it should be noted that a similar situation existed in the case of the WASH-1400 study. In testimony given recently before a working group of the ACRS (Ref. 8), it was noted that ". . . if you look at the causes of system failure probabilities which affect the likelihood of occurrence of accident sequences and you look at those contributors you find that the component failure contributors do not swamp out -- are not the total contribution to system failure; they are only one of a number."

There is another characteristic of probabilistic risk assessment that tends to alleviate concerns about data limitations -- namely, the numerical results tend to pinpoint the relatively most important data deficiencies that may exist and to provide an indication of the importance of any such deficiencies on an absolute scale. This capability exists because of three fundamental facets of the probabilistic risk assessment methodology:

1. Models for calculating probability, consequence, and risk from various parameters.

2. Median values of the parameters used in the models.
3. Uncertainties in the values of the parameters used in the models.

These factors provide a proper weighting of each parameter in terms of its overall effect on the calculation of probability, consequence, and risk. The results of the AIPA study are, indeed, a fitting testimony to the existence of this basic characteristic since the R&D recommendations were strongly influenced by the relatively most important uncertainties developed from the analysis.

REFERENCES

1. Stewart, Milan E., "GCR Accident Review -- FY-1976 Report," Report RE-S-140, Idaho National Engineering Laboratory, Aerojet Nuclear Company, July 1976, Section 2.
2. Knuth, Donald F., letter of transmittal of comments by NRC personnel on R&D decision criteria to Douglas C. Bauer of ERDA/NRA, April 26, 1976.
3. Knuth, Donald F., letter of transmittal of comments by NRC and NRC contractor personnel on the AIPA Status Report to Douglas C. Bauer of ERDA/NRA, May 17, 1976, with five attachments.
4. Okrent, D., "A Survey of Expert Opinions on Low Probability Earthquakes," UCLA Report UCLA-ENG-7515, February 1975.
5. Hsieh, T., D. Okrent, and G. E. Apostolakis, "On the Average Probability Distribution of Peak Ground Acceleration in the U.S. Continent due to Strong Motion Earthquakes," UCLA Report UCLA-ENG-7516, March 1975.
6. Richter, C. F., Elementary Seismology, W. H. Freeman and Company, San Francisco, 1958, pp. 448-452.
7. Ahorner, L., and W. Rosenhauer, "Probability Distribution of Earthquake Accelerations with Applications to Sites in the Northern Rhine, Central Europe," J. Geophysics 41, 592-3 (1975).
8. Meeting of the Reactor Safety Study Working Group of the Advisory Committee on Reactor Safeguards, Washington, D.C., October 12, 1976, testimony of Mr. Saul Levine, p. 36 of uncorrected stenographic transcript of the proceedings.

3. U.S. NUCLEAR REGULATORY COMMISSION

Responses to comments submitted by NRC and NRC contractor personnel are presented in the following subsections. The first five subsections present the comments quoted, in order, from the attachments to Dr. Knuth's letter of transmittal (Ref. 1). The sixth subsection presents the comments quoted from another letter by Dr. Knuth (Ref. 2). References for each set of comments and responses are listed at the end of each subsection.

REFERENCES

1. Knuth, Donald F., letter of transmittal of comments by NRC and NRC contractor personnel on the AIPA Status Report to Douglas C. Bauer of ERDA/NRA, May 17, 1976, with five attachments.
2. Knuth, Donald F., letter of transmittal of comments by NRC personnel on R&D decision criteria to Douglas C. Bauer of ERDA/NRA, April 26, 1976.

3.1. COMMENTS FROM ATTACHMENT 1 OF REF. 1 ON P. 3-1

A. General Comments

Comment A-1

For the most part I found the exposition of what was done to be clear and unambiguous. Notable in this regard is the treatment of "common mode failures". They are taken care of by essentially a "fudge factor" and this seems to be a straightforward way to dispose of a knotty problem. Realistic? - I don't know, but straightforward, yes.

Response

It is agreed that the method used to treat common mode failures is clear, unambiguous, and straightforward. The use of the terminology "fudge factor" implies that a subjective, arbitrary allowance was made in the analysis to account for common mode failures. Such a term might more appropriately be applied to two other approaches that were in use prior to the application of conditional probability methods to develop the common mode frequency ratios (that is, the betas) as presented in the AIPA Status Report. One approach was to simply establish a lower limit for system unreliability based on experience and engineering judgment. It was considered appropriate to assume independence among redundant component failures as long as the system unreliability prediction did not fall below the limit. Although there is some merit in the recognition that systems could not be designed with perfect reliability, the method seems to be grossly oversimplified. The other approach, which was used in WASH-1400, was to estimate the redundant system unreliability (or unavailability) as the geometric mean of the upper and lower bounds. The upper bound was taken to be the unreliability (or unavailability) assuming totally dependent failures, and the lower bound that of the redundant system assuming independent failures. For example, for a system consisting of two redundant assemblies,

the upper bound on the system unreliability would be q (the same as that of a single assembly), and the lower bound would be q^2 . The geometric mean of these bounding values was used to provide an estimate of system unreliability which included a consideration of the effects of common mode interdependencies, yielding the following expression for the estimated system unreliability:

$$q_s = q^{3/2} .$$

While this approach seems better than that discussed above, there are two different problems associated with such a formulation. One is that the proportion of all failures that are attributable to common mode causes decreases toward zero as the total failure probability decreases toward zero. This is in contradiction with actual experience, which indicates no significant variations in failure cause proportions even though a decrease in total system unreliability is observed over a multiyear period due to product improvements introduced during that time (Ref. 1). The other problem is that, based on the geometric mean approach, the total system unreliability can be made arbitrarily low by providing a high enough level of redundancy. While this may be possible, experience data exist that provide a reasonable basis for questioning the validity of such a statement (Ref. 2).

It is believed that the method used in the AIPA study for treating common mode failures is superior to the two methods discussed above for two reasons. The first is that the logic model developed for redundant systems properly describes the system failure modes which can occur when components are subject to independent failure, common cause failure, and propagating failures. This aspect was overlooked in the methods described above, since all they do is manipulate the output of a reliability model which recognizes only the independent failures. The fact that there is a new model involved with the method used in the AIPA study may have been overlooked because of its simplicity. In order that this aspect of the method can be

understood more clearly, the reliability equations listed in Table 4-1 of Volume II of the Status Report have been reproduced here, with a reliability model introduced that is somewhat more elegant mathematically than that used in Volume II.

Consider a one-out-of-two, redundant, nonrepairable system which is subject to independent, common cause, and propagating failure modes. Four distinct states are defined for the system, S_0 , S_1 , S_2 , and S_3 :

$S_0 \equiv$ both units operating

$S_1 \equiv$ unit 1 failed, unit 2 operating

$S_2 \equiv$ unit 1 operating, unit 2 failed

$S_3 \equiv$ both units failed

If the state transitions are described as a Markov process, the time-dependent behavior of the state probabilities is governed by the following set of differential equations:

$$\frac{d}{dt} [\text{Prob } \{S_0\}] = -(Z_{01} + Z_{02} + Z_{03}) \text{Prob } \{S_0\} \quad ,$$

$$\frac{d}{dt} [\text{Prob } \{S_1\}] = -Z_{13} \text{Prob } \{S_1\} + Z_{01} \text{Prob } \{S_0\} \quad ,$$

$$\frac{d}{dt} [\text{Prob } \{S_2\}] = -Z_{23} \text{Prob } \{S_2\} + Z_{02} \text{Prob } \{S_0\} \quad ,$$

$$\begin{aligned} \frac{d}{dt} [\text{Prob } \{S_3\}] &= Z_{03} \text{Prob } \{S_0\} + Z_{13} \text{Prob } \{S_1\} \\ &\quad + Z_{23} \text{Prob } \{S_2\} \quad , \end{aligned}$$

where $Z_{ij} \equiv$ constant transition rate from state i to state j .

Since the states are mutually exclusive, the system reliability $R(t)$ is given by:

$$R(t) = \text{Prob } \{S_0\} + \text{Prob } \{S_1\} + \text{Prob } \{S_2\}$$

which, after solving the differential equations, becomes

$$\begin{aligned} R(t) = & \exp [-(Z_{01} + Z_{02} + Z_{03})t] \\ & + \frac{Z_{01}}{Z_{13} - (Z_{01} + Z_{02} + Z_{03})} \left\{ \exp [-(Z_{01} + Z_{02} + Z_{03})t] \right. \\ & \left. - \exp(-Z_{13}t) \right\} + \frac{Z_{02}}{Z_{23} - (Z_{01} + Z_{02} + Z_{03})} \left\{ \exp [-(Z_{01} \right. \\ & \left. + Z_{02} + Z_{03})t] - \exp(-Z_{23}t) \right\} . \end{aligned}$$

Further, each unit is assumed to have a constant failure rate λ , which can be expanded:

$$\lambda = \lambda_i + \lambda_p + \lambda_{cc}$$

where λ_i = failure rate for independent failures,

λ_p = failure rate for failures that propagate resulting in failure of the second unit,

λ_{cc} = failure rate for common cause failure of both units.

The relation between the Z and λ terms is described as follows:

1. The only way to transfer from state S_0 to either S_1 or S_2 is via independent failure. Hence,

$$Z_{01} = Z_{02} = \lambda_i .$$

2. Once in state S_1 or S_2 , transition to S_3 will occur if the single remaining operating unit fails by any mode. Hence,

$$Z_{13} = Z_{23} = \lambda \quad .$$

3. Transfer from state S_0 to S_3 (directly) can occur in three ways: common cause failure, propagating failure originating in unit 1, or propagating failure originating in unit 2:

$$Z_{03} = \lambda_{cc} + \lambda_p + \lambda_p = \lambda_{cc} + 2 \lambda_p \quad .$$

Using the definitions of the parameters β and γ from Volume II of the Status Report,

$$\beta = \frac{\lambda_{cc}}{\lambda}$$

and

$$\gamma = \frac{\lambda_p}{\lambda_i + \lambda_p} \quad ,$$

and the definitions of the Z terms listed above, one obtains

$$R(t) = 2(1 - \gamma) e^{-\lambda t} - (1 - 2\gamma) e^{-(2-\beta)\lambda t} \quad ,$$

which is identical to the one-out-of two expression in Table 4-1 of volume II. Similar logic can be used to derive the expressions for one-out-of-three and two-out-of-three configurations. It should be noted that β and γ are conditional probabilities and not "fudge factors." On the other hand, if β and γ are fudge factors, so is λ , which could imply that the entire field of reliability engineering is simply the manipulation of fudge factors!

The second reason why the AIPA method is believed to be superior, and, in fact, realistic, is the ability to estimate the values of these parameters from nuclear power plant experience. Without this feature, the method would be useless insofar as quantitative analysis is concerned. Although the estimates of β and γ listed in Volume II are based on rather sparse data, the resulting reliability predictions approach more closely what has been observed from experience than did previous methods. Efforts are currently oriented toward improving the data base used in Volume II.

Comment A-2

Ambiguities and possible numerical errors in the report were, for me, traceable to the loose way in which statistical concepts and terminology are used. The term "confidence interval" is used to refer to at least four distinct things:

- a. a statistical confidence interval pertaining to some unknown constant,
- b. points on a subjective probability distribution pertaining to some unknown constant,
- c. points on an assumed probability distribution of some random variable,
- d. points on an estimated (from data) probability distribution of some random variable.

All of these and some other things are called uncertainties and then mathematically combined. I have no idea how to interpret the results other than mathematically: These functions were combined in these ways yielding these answers. How this sort of answer relates to HTGR accidents I don't know. I do know that the results are not statistical confidence intervals - not statements about what accident probabilities and consequences are supported by, or in agreement with, the available data.

These concerns may not have any effect on the conclusions reached in the report. However, I can give examples of situations in which mixing confidence intervals and probability intervals can yield numerically misleading results. Fortunately, in instances I've encountered, the error tends to be on the conservative side in that the probabilities of undesired events tend to be overstated. Nevertheless, as it is important to distinguish among various isotopes, for example, so it seems (to me) equally important to distinguish among various "uncertainty" intervals or distributions.

Response

While the term "confidence" has a very specific, well-defined meaning for statisticians, it is often used by engineers in a much less precise way, primarily along the lines of the appropriate dictionary definition of the term. Since the report is based on inputs from both engineers and statisticians, the term "confidence" conveys a variety of meanings.* However, all such usages reflect one fundamental intent -- to provide a measure of the uncertainty entailed in the estimation of a parameter. The uncertainties in the input parameters are combined in a manner that is believed to provide a reasonable and realistic assessment of the uncertainties of the output parameters -- that is, of the event sequence branch probabilities and consequences.

*Further discussion is provided in this section in the response to Comment B-22.

Comment A-3

The stated purpose of the analysis is to determine HTGR research priorities. I wonder whether all this analysis was necessary to achieve reasonable priorities. I wonder if about the same priorities would have been obtained by asking several knowledgeable people their priorities and then collating these.

Response

An example of the reliance on expert opinion occurred in preparation of ORNL-4968 (Ref. 3), when two questions were posed implicitly during the report preparation:

1. What qualifies as a safety research problem?
2. What priority and level of effort does each item warrant?

Table 5-7 of Volume IV of the Status Report provides a ranked listing of nine specific, detailed, safety R&D items of interest as defined by the AIPA study. Comparison of this list with the 200 R&D items in ORNL-4968 illustrates the merit of the AIPA approach:

1. Three of the list of nine are not included within the broad, general guidelines of ORNL-4968.
2. Two of the six problems addressed were designated as safety-related.
3. Three of the six problems addressed had an "A" priority and three a "B". As ORNL-4968 contains 76 "A" items and 71 "B" items,* the AIPA designated tasks could well have been neglected if the priority ranking of the ORNL-4968 draft had been implemented.

* Unpublished data, December 1973 draft of ORNL-4968.

4. The tasks described in ORNL-4968 are, in most cases, only a quasi fit with those defined in the Status Report.
5. The reheater tube leak is downplayed in ORNL-4968 as an "unlikely serious accident."

Even discounting the difference in time of preparation (ORNL-4968 reflects 1973 input; GA-A13617, 1975), it is clear that the conventional interplay of ideas based on expert opinions would not have properly defined all the safety research issues and assigned them appropriate priorities. Only the systematic, quantitative procedure followed in probability risk assessment makes such definition possible.

Comment A-4

It may be mathematically convenient to regard equal risk events as those which have equal products of probability times consequence, but I doubt that public perception of risks is that well-behaved. How much effect on the choice of research priorities would other risk criteria have had? For example, probability times the square of the consequence, or times $\exp(\text{consequence})$? Or one might rank the events by the probability of exceeding some specific consequence. With all the work that has been done to estimate probabilities and consequences, it seems a shame to consider only their product.

Response

This comment appears to confuse the concept of "risk" with that of a "utility function." The concept of "risk" was not introduced by the AIPA study team but rather is an established concept taken from decision theory. Risk can be considered the expected yearly value of the consequence associated with operation of reactors for many years, as it is presently defined. There is no meaning whatsoever associated with the product of the probability and some arbitrary function of the consequence. It may very well be true that a plot of the public acceptance curve on a risk plot would not be a curve of constant risk. However, one does not redefine the concept of risk merely to accommodate public perception. There is a tool in decision theory, known as a utility function, that properly accommodates acceptability. Farmer introduced a proposed utility function for accidental releases of I-131 in his landmark paper on risk assessment in 1967 (Ref. 4).

The Status Report does not merely consider the risk parameter in interpreting the results of AIPA. In fact, the decision criteria established by ERDA, which are to be used to make the final list of R&D recommendations, are based on the comparison of the results of the AIPA study with a risk envelope curve for LWRs based on the WASH-1400 results. (This

is used in the absence of a well-defined utility curve for public acceptability.) The LWR risk envelope curve as seen in a probability-versus-consequence plot is not an equal risk line.

B. Detailed Comments

Comment B-1
(Page 4-14 of Volume II)

The Poisson and exponential distributions are related but not interchangeable.

Response

It is agreed that the two distributions are not interchangeable. However, this consideration has no impact on the results of the study.

Comment B-2

(Pages 4-23, 4-28 of Volume II)

- a. The notion that because one is uncertain about drawing analogies, one can therefore treat parameters of interest as lognormally distributed random variables is pure mysticism.
- b. Furthermore, the fact that different applications, or stress levels, may yield different failure rates doesn't mean that the failure rate varies randomly.
- c. Still furthermore, the uncertainties in WASH-1400 were conveniently depicted as lognormal distributions, not "accurately described."
- d. The fact that one ignores possible differences in data sources does not mean that the resulting estimate is generic. Combining apples and oranges does not necessarily yield your generic fruit salad.
- e. The first expressions for β and γ are parameters. Expressions (4-42) and (4-43) are estimates of these parameters. These concepts are not interchangeable.

Response

The assumption that the time of failure of an equipment item is a random variable described by the exponential distribution (Eq. 4-24 of Volume II) is commonly used in reliability engineering as well as in the WASH-1400 and AIPA studies. There are two fundamental engineering considerations for its use: it is a one-parameter distribution and therefore mathematically convenient to use, and the quantity and quality of equipment failure data currently available, and likely to be available in the future,

hardly warrant the use of a more complicated distribution form (e.g., Weibull). Experience has shown that the constant failure rate property of the exponential distribution is, more or less, observed by electronic and electrical equipment during a specific portion of the equipment life cycle, and not observed at all for most mechanical devices (see, for example, Ref. 5). Therefore, it is not generally true that the equipment failure rates, and the functions of these failure rates, like β and γ , are fixed constants. Since it is not understood how individual parameters vary with time, we conveniently depict these parameters as random variables which are distributed lognormally. The AIPA study team maintains that the assumption that λ is random is as valid as assuming the times of failure to be random.

The objective of probabilistic risk assessment is not only to improve the understanding of reactor safety, but also to attempt to quantify the lack of understanding. If the understanding of the physical processes involved with equipment failure were approaching perfection, there would be no need to treat certain variables as being random since one could solve the problems deterministically. Unfortunately, one does not have, and probably will never have, a level of understanding approaching perfection. However, by treating parameters as random variables, this lack of understanding can at least be quantified. This approach is well suited to the overall objectives of the study: to recommend needed R&D.

It is recognized that the use of any theoretical distribution to represent the characteristics of any specific physical parameter is always open to question. It is also recognized that it is impossible for the lognormal distribution to provide an exact fit for most of the parameters considered thus far in the AIPA study. This follows from the fact that the lognormal variable ranges from zero to infinity, whereas most of the parameters considered in the AIPA analysis have finite ranges (probabilities range from zero to one, earthquake magnitudes have a finite upper limit dictated by the physical capacity of the earth to store the mechanical energy of distortion without rupture, etc). The choice of distribution form as made

by the AIPA study team was based on a very fundamental engineering consideration -- the analyses had to be of reasonable accuracy and had to be completed within the constraints of time and funding. The choice of the lognormal distribution to describe uncertainties in probabilities was based on the following characteristics and considerations:

1. It is practical to use because:
 - a. It is a two-parameter family.
 - b. An adequate foundation of statistical theory exists for its use.
 - c. It was one of the distributions included in the capability of the SAMPLE code.
 - d. It lends itself well to describing the uncertainties of the small, positive probabilities and failure rates encountered so frequently in the AIPA study.
2. It can be shown that the uncertainty distribution for the annual occurrence probability for a complete branch in an event tree generally tends toward lognormality, regardless of the distribution forms of the parameters used in calculating the branch point probability. Thus, even though the process of approximating individual input parameter uncertainties with the lognormal distribution may result in substantial errors in the uncertainty distributions of these parameters individually, the lognormal uncertainty distribution established therefrom for the annual occurrence probability for a complete branch is reasonably accurate. It is here -- at the complete branch level -- that the accuracy consideration really matters.

3. The theoretically founded statements of 2, above, were corroborated during the WASH-1400 study. This general topic was addressed in a paper by Dr. W. E. Vesely (Ref. 6). He stated that variations in the distributions assumed for the uncertainties for the input parameters were generally found to have little effect on the uncertainty distribution of the output parameter of interest, and that the lognormal distribution provided an adequate description of the uncertainties in the parameters of concern.
4. The lognormal distribution was selected for the analyses performed in the WASH-1400 study.

Work is in progress on modifications to the SAMPLE code that will permit the input of more general distribution forms (including tables) for the input parameter uncertainty distributions in those cases where the distribution form can be defined.

Comment B-3
(Page 4-29 of Volume II)

Table 4-3 is inconsistent with the discussion on page 4-28. β_{95} for all equipment is calculated as though there were a single β pertaining to all the equipment types. It does not reflect the variability discussed on page 4-28.

Response

The intent of Table 4-3 was to show the confidence limits on the β and γ factor estimates based on the sample of observed abnormal occurrences. The table fairly well demonstrates that a sufficient number of common mode failures has occurred to warrant a general reliability model for common mode failures in redundant systems. It is agreed that β_{95} does not reflect the variability discussed on page 4-28. The uncertainty in β or γ which is computed, for example, as β_{95}/β_{50} from this table results in uncertainty factors ranging from about 1.3 to 5. In the fault tree analysis performed in Volume IV, larger uncertainty factors were used to reflect the variability aspect. In most cases the uncertainty factor in the common mode parameter was assessed at 10.

Comment B-4
(Page 4-32 of Volume II)

Table 4-4 conveys a false sense of precision. The denominators of the percentages given are quite small -- 6 in the first and third rows, 14 in the second and fourth.

Response

It is agreed that the denominators used to compute the cause contribution percentages in Table 4-4 are too small to warrant the number of significant figures listed. In future work, it is planned to adopt a display technique suggested by the same reviewer at the February 19, 1976, AIPA presentation in Germantown, namely, to list rational fractions of the sample (e.g., $2/6$ rather than the percentage of 33.3). This should eliminate the apparent false sense of precision, which was not intended in this table.

Comment B-5
(Pages 4-45 ff of Volume II)

The lognormal distribution of MTOR is meant to represent the variability in expected reaction time among different operators. However, the selection of that distribution is based on 4 "experts'" estimates of the (average) time for an average operator to respond. Thus Figures 4-9 and 4-11 actually show the imprecision in the estimate of average log MTOR, not the variability of log MTOR's among some collection of operators. If operators vary more than do the experts' estimates about the average operator (or more precisely, the average MTOR where the average is taken over all operators), then the result is that the probability of operator error is underestimated in the report. This is the sort of error which can happen when all uncertainties are treated the same. The imprecision of an estimated mean of a group of individuals is not the same as the variability of the individuals and so it is incorrect to treat them interchangeably. What is needed (in the absence of "real" data) is experts' opinions on percentiles of the MTOR distribution other than the median.

Response

In the absence of actual data on the operator response time for the example in Volume II, it was found that the variability in the experts' estimates of the median response time was greater than their individual estimates of the variability (of operator response time). Thus, in this case the operator response was estimated as if the time given by each expert was the actual time he would have required to perform the action. In cases where the variability estimate of the experts was greater than the variability in the average response time estimates, the larger estimated uncertainty was used. The estimate of operator response time is important where time-dependent radioactive releases are concerned; otherwise, these times have an insignificant effect on the risk assessment, due to the slow response times associated with HTGR systems.

The estimate of the average limiting probability is made independently of the response time. It is based on the level of information available to the operator, the type of training received, and the clarity of such information. It was found that these factors also influence estimates of response time. That is, rapid response times are the result of clearly defined problems with well-defined responses, and thus the probability of failure has been ranged from 0.001 to 0.0001. Conversely, slow response times are the result of unclear information with undefined responses, and failure probability estimates have ranged from 0.9 to 0.01. Each event tree branch point case was considered on an individual basis with appropriate error bands. Since there is rather weak correlation between the MTOR estimates and the average limiting probability, the likelihood that the probability of reactor operator error has been underestimated or is outside the error bands in the AIPA report appears to be small. It should be noted that the operator response model was of secondary importance in all but a few of the accident sequences analyzed. In the case of the small reheater leak, Branch SM, the operator response model was the dominant factor in the probability prediction. The R&D recommendation (item 5, Table 3-3, Volume IV) arising from this branch reflects a recognition that the operator reliability model needs further development.

Comment B-6
(Pages 4-53ff of Volume II)

The earthquake analysis seems quite thorough. I was particularly favorably impressed by the fact that within-site and site-to-site variability were distinguished. This is the sort of distinction missing from the operator response modeling so I find this inconsistency puzzling.

Response

The comment regarding the thoroughness of the earthquake analysis and the relative simplicity of the operator response model is appreciated. However, it is not apparent that this represents an inconsistency in the methodology. A careful examination of the event trees in Volumes III and IV would reveal that the operator response model is of secondary importance in the assessment of a vast majority of the sequence probabilities and uncertainties. This is true since most of the branch point events in the event trees represent automatic responses of the plant systems to the initiating events. For this reason, a relatively large portion of the methodology development effort was devoted to realistic modeling of hardware unavailability and unreliability. The common mode failure methods that were developed reflect the impact of a wide variety of human errors on these hardware probabilities. In a relatively small number of cases, including reheater leak Branch SM in Volume IV, the operator response model was of major importance. It was recognized that the human operator reliability model needs to be more fully developed, and therefore an R&D recommendation was made to do so (item 5, Table 3-3, Volume IV). Thus, while it is agreed that the operator response model needs further refinement, it is felt that the methodology development is balanced in view of the nature of the results.

Comment B-7
(Page 4-100 of Volume II)

In contrast with the claim of the last sentence on this page, there is no "theoretical basis" for treating all uncertainties as random variables. In fact, the basis is that it is convenient, has intuitive appeal, and is one way to simplify complicated problems.

Response

The response to this comment is covered in the response to the comment on pp. 4-23, 4-28 of Volume II (Comment B-2).

Comment B-8
(Page 4-101 of Volume II)

The lognormal distribution is defined over the entire positive real line. Thus it is not "especially appropriate" for probabilities which are constrained in the interval (0, 1). There is a great deal of lognormal folklore developing in the nuclear risk assessment business.

Response

The response to this comment is covered in the response to the comment on pp. 4-23, 4-28 of Volume II (Comment B-2).

Comment B-9
(Page 4-107 of Volume II)

I would like to see the derivation of (4-234).

Response

The derivation of Eq. 4-234 is attached. It should be noted that this equation, like many that are developed from the propagation of variance formula, is an approximation and, as such, must be applied with caution.

ATTACHMENT

PROPAGATION OF LOGNORMAL UNCERTAINTY FOR SUMS

It is of interest to establish a methodology for a summation model. Begin by considering the following sum of probabilities:

$$P_T = \sum_{i=1}^n P_i \quad .$$

For this analysis, it is assumed that all of the P_i are small, that their uncertainty distributions can be adequately approximated by the lognormal distribution, and that the uncertainty distribution for P_T can also be adequately approximated by the lognormal distribution.

With the above assumptions as a guide, the following transformations are introduced:

$$p_i = \ln P_i \quad ,$$

$$p_T = \ln P_T \quad .$$

It follows, then, that the p_i and p_T are all approximately normally distributed with means of μ_i and μ_T and standard deviations of σ_i and σ_T . Consider a symmetrical (or the logarithmic scale) uncertainty band of probability γ . Define a parameter z (a function of γ only) such that

$$P \left\{ p_{iL} \leq p_i \leq p_{iU} \right\} = \gamma$$

where $p_{iL} = \mu_i - z\sigma_i$,

$$p_{iU} = \mu_i + z\sigma_i \quad ,$$

$$\frac{1}{\sqrt{2\pi}} \int_{-\infty}^z e^{(-1/2x^2)} dx = 1 - \gamma/2 \quad .$$

Exponentiation of the first two equations above yields

$$\begin{aligned} e^{P_{iL}} &= e^{\mu_i} e^{-z\sigma_i} \quad , \\ e^{P_{iU}} &= e^{\mu_i} e^{z\sigma_i} \quad , \end{aligned}$$

Define the median probabilities and the lower and upper limits by

$$\begin{aligned} P_{iM} &= e^{\mu_i} \quad , \\ P_{iL} &= e^{P_{iL}} \quad , \\ P_{iU} &= e^{P_{iU}} \quad , \end{aligned}$$

or

$$\begin{aligned} \mu_i &= \ln P_{iM} \quad , \\ P_{iL} &= \ln P_{iL} \quad , \\ P_{iU} &= \ln P_{iU} \quad . \end{aligned}$$

Also, define the uncertainty (or error) factor as follows:

$$f_i = e^{z\sigma_i} \quad .$$

From the above formulas it follows that

$$\begin{aligned} P_{iL} &= P_{iM} f_i^{-1} \quad , \\ P_{iU} &= P_{iM} f_i \quad , \end{aligned}$$

where

$$\text{Prob} \left\{ P_{iL} \leq P_i \leq P_{iU} \right\} = \gamma \quad .$$

By analogy, the following expressions for P_T and p_T can be given:

$$\text{Prob} \left\{ p_{T_L} \leq p_T \leq p_{T_U} \right\} = \gamma \quad ,$$

$$p_{T_L} = \mu_T - z\sigma_T \quad ,$$

$$p_{T_U} = \mu_T + z\sigma_T \quad ,$$

and

$$e^{p_{T_L}} = e^{\mu_T} e^{-z\sigma_T} \quad ,$$

$$e^{p_{T_U}} = e^{\mu_T} e^{z\sigma_T} \quad .$$

Define the medians and lower and upper limits by

$$P_{T_M} = e^{\mu_T} \quad \text{or} \quad \mu_T = \ln P_{T_M} \quad ,$$

$$P_{T_L} = e^{p_{T_L}} \quad \text{or} \quad p_{T_L} = \ln P_{T_L} \quad ,$$

$$P_{T_U} = e^{p_{T_U}} \quad \text{or} \quad p_{T_U} = \ln P_{T_U} \quad ,$$

$$f_T = e^{z\sigma_T} \quad .$$

Thus,

$$P_{T_L} = P_{T_M} f_T^{-1} \quad ,$$

$$P_{T_U} = P_{T_M} f_T \quad ,$$

where

$$\text{Prob} \left\{ P_{T_L} \leq P_T \leq P_{T_U} \right\} = \gamma \quad .$$

The summation equation given at the beginning can easily be expressed in terms of the transformed variables, yielding

$$e^{P_T} = \sum_{i=1}^n e^{P_i} \quad .$$

From standard statistical formulas for the propagation of moments it follows that, approximately,

$$e^{\mu_T} = \sum_{i=1}^n e^{\mu_i} \quad ,$$

$$\sigma_T^2 = \sum_{i=1}^n \left(\frac{\partial P_T}{\partial P_i} \right)^2 \bigg|_{\mu_i} \sigma_i^2 \quad ,$$

where it is assumed that the P_i are statistically independent of one another. The first expression is easily transformed into

$$P_{T_M} = \sum_{i=1}^n P_{i_M} \quad .$$

For the second expression, evaluate the partial derivatives as follows:

$$e^{P_T} \left(\frac{\partial P_T}{\partial P_i} \right) = e^{P_i} \quad .$$

Evaluating the derivative at the mean yields

$$\left(\frac{\partial p_T}{\partial p_i} \right) \bigg|_{\mu_i} = \frac{e^{p_i}}{e^{p_T}} \bigg|_{\text{mean}} = \frac{e^{\mu_i}}{e^{\mu_T}} ,$$

which is easily transformed into

$$\left(\frac{\partial p_T}{\partial p_i} \right) \bigg|_{\mu_i} = \left(\frac{p_{iM}}{p_{TM}} \right) .$$

The formula for the variance, after multiplying by z^2 , becomes

$$(z\sigma_T)^2 = \sum_{i=1}^n \left(\frac{p_{iM}}{p_{TM}} \right)^2 (z\sigma_i)^2$$

or

$$(\ln f_T)^2 = \sum_{i=1}^n \left(\frac{p_{iM}}{p_{TM}} \right)^2 (\ln f_i)^2 .$$

The above results can be summarized as follows:

$$\left. \begin{aligned} p_{iL} &= p_{iM}/f_i \\ p_{iU} &= p_{iM} f_i \\ \text{Prob } \{p_{iL} \leq p_i \leq p_{iU}\} &= \gamma \end{aligned} \right\} \dots \dots \dots \text{relations valid for each} \\ \text{probability } p_i$$

$$\left. \begin{aligned} P_{TM} &= \sum_{i=1}^n P_{iM} \\ f_T &= \exp \left[\sqrt{\sum_{i=1}^n \left(\frac{P_{iM}}{P_{TM}} \ln f_i \right)^2} \right] \end{aligned} \right\} \text{ . . combining formulas for the sum}$$

$$\left. \begin{aligned} P_{TL} &= P_{TM}/f_T \\ P_{TU} &= P_{TM} f_T \\ \text{Prob} \left\{ P_{TL} \leq P_T \leq P_{TU} \right\} &= \gamma \end{aligned} \right\} \text{ . . . relations valid for the sum}$$

$$P_T = \sum_{i=1}^n P_i$$

This completes the derivation.

Comment B-10
(Pages A1-9, A1-10 of Volume IV)

Table A1-2 lists several steam generator failures. In the text, apparently all but one of these is disregarded and then this one is further discounted by a factor of 2. All of this seems rather "uncertain" to me and (if I believed that this uncertainty could be accounted for by the error factor) the error factor of 4 seems too small. The numbers for intermediate and small leaks could stand a little more justification also.

Response

The applicability of the data is qualified by footnotes at the bottom of Table A1-2. Also, not all of the leaks shown on Table A1-2 are large leaks. Only one failure of an applicable large leak was found from the table, and this failure was the result of questionable welding techniques.

Since the hardware item of interest is the reheater section of the steam generator, which is not subject to the localized boiling heat transfer problems of the steam generators, it was judged that the failure rate of this section could be reasonably lowered by a factor of 2. Therefore, it is estimated that $P_f = 0.002 \text{ yr}^{-1}$ for the reheater.

The development of an error factor follows directly from Eq. A1-1 as follows:

$$P_f (\alpha = 0.95) = 0.0119 \text{ yr}^{-1}$$

$$P_f (\alpha = 0.50) = 0.0042 \text{ yr}^{-1}$$

$$P_f (\alpha = 0.05) = 0.00089 \text{ yr}^{-1}$$

The error factor is therefore $f = (0.0019/0.00089)^{1/2} = 3.36$. These results lead to the following rounded values: $P_f = 0.004 \text{ yr}^{-1}$, with an error factor $f = 4$ (steam generator).

Although the large leak occurrence rate is based on GCR experience, the small and intermediate leaks were derived from PWR experience. These data were normalized to that of HTGR based on the relative frequencies of large leaks.

The failure rates presented on page A1-11 of Volume IV are the result of this evaluation.

Comment B-11
(Page A1-14 of Volume IV)

In Vol. II (p. 4-28) it was stated that β would be treated as a distributed parameter. Here in Vol. IV it's being treated as a constant.

Response

The methodology for treatment of β as a distributed parameter was explained in Volume II. This methodology was consistently followed in Volume IV also. Each major system with redundant components includes a "common mode factor" to account for common system dependencies. In the case of the main loop isolation system of the PPS, the major component failure rate median and uncertainties are identified in Table A1-3; X(5) is the common mode unavailability of the redundant subsystems. As described in Volume II, this parameter is specified by a median value and an error factor which is fit to a lognormal distribution. It is then combined with the other distributed failure probabilities using the SAMPLE computer code. Hence, the common mode factor is treated as a distributed parameter.

What is the algebraic expression for X_{CM} in terms of the Y's and Z's?

Response

The equation for the median value of X_{CM} may be formulated by multiplying the effective component β times the failure probability of each element contributing to common mode isolation failure listed on Fig. A1-13. Based on the variable names in Table A1-4, the equation for determination of the X_{CM} median is

$$X_{CM} = \beta_1 Z_1 + \beta_2 Z_2 + \beta_3 Z_3 + \beta_4 Y_1 + \beta_5 Y_3 + \beta_6 Y_4 + \beta_7 Y_5 + \beta_8 Y_2 \quad .$$

In this case β_1 through β_7 were assessed at 10% and β_8 at 1%, based on the common mode failure data and causes presented in Volume II. About 25% of the β fraction is the result of human errors such as poor maintenance or incorrect operation, as shown in Table 4-4 of Volume II. The common mode contribution from the control room operators as they fail to respond to a given situation is considered by increasing the normal response time, thus lowering the probability of effective corrective action for a given time frame, and is not included in the evaluation of X_{CM} for hardware and equipment. The probability calculation of X_{CM} in the above equation is

$$\begin{aligned} X_{CM} = & 0.1 [(4 \times 10^{-5}) + (1 \times 10^{-6} \times 720/2) + (1 \times 10^{-6} \times 720/2) \\ & + (3 \times 10^{-4}) + (1 \times 10^{-4}) + (1 \times 10^{-6} \times 24/2) \\ & + (3 \times 10^{-8} \times 24/2)] + 0.01 (1 \times 10^{-4}) = 1.18 \times 10^{-4}. \end{aligned}$$

In addition, some of the elements contributing to the failure probability in event 4 have been accounted for more than once in other fault trees. The double accounting was removed by the methods described in Section A.1.10 of Volume IV. This adjustment results in a probability of 5.4×10^{-5} for Q_4 in the large leak event.

Comment B-13
(Page A1-27 of Volume IV)

I note that Q_4 equals X_{CM} . Given this dominance it may be useful to consider whether $\beta = .10$ is appropriate in this case. Also, one begins to wonder if it is worthwhile modeling multiple failures. Just add up all the single failure probabilities and divide by 10. That simplifies the analysis considerably.

Response

The equation for Q_4 is given on page A1-20. The dominance of common mode factors in that equation produces a result which is approximately equal to X_{CM} . However, the independent components of Q_4 have contributed to failures in other events. Thus, each event probability must be corrected for the interevent dependency factors, and the simplistic approach of combining single failures and multiplying by β , suggested by the reviewer, should be used carefully.

Comment B-14
(Appendix A1 of Volume IV)

The operator has various tasks to perform during any event sequence. There are apt to be all sorts of dependencies among his response times to various tasks. How much support is there for the way this has been modeled?

Response

It is agreed that the operator does have various tasks to perform in any of the sequences which may occur. Nuclear power plant operators are trained to respond to specific anticipated events, and given a thorough background in nuclear, mechanical, and electrical engineering which enables them to respond and adapt to unanticipated events. Task dependency was considered in estimating the mean time to operator response (MTOR). There were, of course, all sorts of dependencies which were considered. The selection of branch points was judiciously made in order that the simplistic first-order model would properly serve the purpose of the analysis.

Volume II (pages 4-39 through 4-52) describes the general methodology used, which includes references such as WASH-1400 and should be consulted for specific details of the operator model.

In addition to those references, further work using similar approaches are documented in the literature (Refs. 7-9). Edwards and Lees (Ref. 7) provide many viewpoints on how operators perform in process control situations; this book supports the idea that the probability of the operator correcting deficiencies increases in time. The Kaman report (Ref. 8) uses a more detailed method of considering the operator error rates as a function time ($1/\text{MTOR}$) by allowing the error rate to vary in time. Their method of treating the operator error discretely in time approximates complicated distributions such as the Weibull or gamma. The AIPA study, of

course, used a constant operator error rate as a first approximation. Thus, the work done by Kaman employs a more advanced operator model than that used in the AIPA study, and appears to confirm the use of a constant MTOR as a first approximation. Sabri, Hussening, and Danofsky (Ref. 9) have considered important parameters affecting operator response to abnormal conditions and provided mathematical expressions to predict operator reliability. These include the dependency effects of stress, training, and retirement.

Comment B-15
(Page A1-45 of Volume IV)

I don't see how a Q_7 median of 5.2×10^{-3} can be obtained from equation (A1-13) and Table A1-6.

Response

This comment has revealed a typographical error in the equation for Q_7 . The plus sign between $X(8)$ and $X(9)$ should be a product symbol. From the SAMPLE simulation of Q_7 , a median value of 4.7×10^{-3} was determined which was adjusted to 5.2×10^{-3} when interdependencies were considered. In addition, the section M of the fault tree referred to in Fig. A1-21 was omitted. This section is similar to sections of Fig. A1-13 where typically an "and" gate would represent two different potential operator actions that can occur within the times allowed for these operator actions. Such operator actions were considered to be interdependent as illustrated by the following example. The operator is taking corrective action as the result of a tube leak and elects to perform the first action as backup to the automatic system when he notes an apparent mechanical failure to isolate the leaking loop. This failure might exist because the monitors are still reading high. He then makes a second attempt. Continued attempts to isolate the leaking loop with no success would finally result in his backup action, which is isolation of the system with the turbine intercept valves as the plant goes off the line.

Comment B-16
(Page A2-7 of Volume IV)

The first paragraph is the sort of weak argument that this type of analysis forces one to do. I suppose it can't be avoided.*

Response

It is not clear that the estimate of the hot-standby failure rate on page A2-7 is based on a "weak argument" inasmuch as 15 reactor years of operating data were used as a basis. Two differences were noted between the designs of PWRs and HTGRs which were assumed to cancel in the estimate of the median frequency of turbine trip. It is believed that this assumption is reasonable, but nonetheless an uncertainty factor has been assigned that is significantly larger than would be obtained, for example, by calculating the confidence limits on the experience data using the chi-squared distribution.

*The specific wording of this comment was modified slightly (without an impact on comment meaning) as requested by the reviewer.

Comment B-17
(Page A2-8 of Volume IV)

Should the frequency of turbine trip be adjusted for availability of the plants?

Response

The experience base used to estimate the frequency of turbine trip was computed simply by counting the number of turbogenerators in the sample which was observed over one year. This results in 15 reactor-years (or turbine-years) of experience. It is agreed that it would have been appropriate to adjust this experience for plant availability so that the resultant product represents the total "on-line" experience. It was decided not to correct for plant availability because the numerical impact is negligible. In fact, this "refinement" in the estimate of turbine trip frequency would have been lost when the number is rounded off to one significant figure, since the average plant availability during 1973 was about 0.7.

Comment B-18

(Comment a on page 3-11 of Volume VI)

Item 4. The median of a function of one or more variables is not necessarily equal to the function of the median of those variables. For example, X might be normally distributed with a mean of zero and a variance of 1.0. Its median is zero. The median of X^2 is not equal to $(0)^2$. Also, if X and Y are lognormally distributed, the median of $X + Y$ is not equal to the sum of the medians of X and Y.

Response

The essence of the statements in the comment is correct and provides confirmation of the need to exercise care in estimating the median of a function of one or more variables in terms of the function of the medians of those variables. Two aspects of "exercising care" are displayed in the Status Report. First, Eq. 2-5 of Vol. V expresses (on the logarithmic scale) the median as the sum of the function of the medians and several additive terms that provide an approximate, frequently conservative means for including the effects of random uncertainties in estimating the median. Second, since this formulation (as well as that for analyzing propagation of variance) is based on the assumption of functional linearity with respect to the variables of interest, a check was made by plotting the functional variations for those variables (see Volume V, Sections 3 through 5). These plots indicate that most variables very nearly comply with the linearity assumption over the intervals of primary interest, indicating that the analysis approach is sufficiently accurate for those variables. However, other variables depart from linearity (a few to a large degree), indicating that some error is incurred in estimating the median of the function. Two means are available for reducing this error. One would entail the use of a higher-order functional approximation -- at least second order, but more probably of order four* or more -- to fit the

* A third-order approximation would cause little or no change in the formulation for estimating the median over that provided by a second-order approximation.

functional variations as plotted in Volume V. Such an approach would entail what were judged to be excessive increases in costs (for developing the required computational formulas and computer codes, and for the increased computer run time) when compared with the relatively small improvement anticipated in computational accuracy. The other means of error reduction is Monte Carlo simulation. Since very complex computer programs are required in all of the fission product release calculations in Volumes V and VI, computer run costs would prohibit the use of the simulation approach. (Further discussion on the evaluation of the median of a function is presented in the response to Comment B-27 (Comment a on page 3-15 of Volume VI)).

Comment B-19
(Comment b on page 3-11 of Volume VI)

Item 5. Varying one parameter at a time is an inefficient and potentially inaccurate way to determine parameter influences.

Response

It should be borne in mind that event consequences are derived using a complex series of computer programs which simulate time-dependent fission product transport in the HTGR. For each computer code, the study engaged the services of the corresponding cognizant engineer who was experienced in performing parameter analyses. Therefore, it was considered faster and more cost-effective to vary the parameters in the usual way, singly, in computer run batches. The alternative of attempting to determine a functional relationship for such a complex calculation to perform Monte Carlo sensitivity was deemed impractical.

The limitations of the technique used are discussed in Section 2.3 of Volume V. Equations are presented for the generalized propagation of errors including parameter correlation coefficients. These equations simplify to the form shown in Eq. 3-2 in Volume VI if all the important variables are independent. Some variables affecting the initial fission product inventories were known to be interdependent; for these, correlation coefficients were determined for the generalized propagation of errors (Volume V). Also, parameters of liftoff of plated-out activity, distribution of plated-out activity, and gasborne activity are interdependent and were co-varied for PCRV depressurization analysis (Section 5.2 of Volume VI). Most of the other parameters appeared to be independent or have weak cross-coupling effects. Therefore, the parameter analysis performed is considered suitable for the derivation of representative uncertainties.

Comment B-20

(Comment a on page 3-12 of Volume VI)

The fact that lack of data means that only upper and lower bounds are available does not justify the assumption of a uniform probability distribution. For example, someone may tell me that the shortest surname in the Washington phone book has one letter and the longest has 27. This doesn't mean the number of letters in surnames is uniformly distributed between 1 and 27.

Response

True, but in the example one would probably not choose a uniform distribution because experience is available to intuitively know that the distribution is peaked around a moderate number of letters. In the study cases, no experience is available -- only extreme limits are known, indicating that a finite distribution exists. While it is true that the selection of the uniform distribution was an essentially arbitrary one, there are several factors that support such a selection:

1. It is a simple, finite, statistical distribution.
2. It provides a practical representation of the fact that there is no basis for giving greater statistical "weight" to one value of the variable in the prescribed interval than to any other value in that interval.
3. If a mathematically dependent variable is a function of several mathematically independent variables, then the form of the statistical distributions of these individual independent variables tends to have a decreasing effect on the statistical distribution of the dependent variable as the number of independent variables

increases. Since the consequence assessments involve a sufficiently large number of variables, the choice of distribution forms for those variables tends to be noncritical.

Comment B-21
(Comment b on page 3-12 of Volume VI)

I recognize most of the words in the last paragraph but the intended meaning is unclear.*

Response

Perhaps the following paraphrasing of that paragraph will improve its clarity.

The objective of the analysis is to provide a realistic assessment of radiological consequence in terms of the various variables that affect consequence. The assessment makes extensive use of two basic types of parameters: (1) medians, which provide a central measure of a variable whose value is not precisely known, and (2) uncertainty factors, which provide a measure of the degree of uncertainty in the knowledge of the values of the variables. Since the radiological consequence is ultimately expressible as a function of a number of mathematically independent variables, the median and uncertainty factor for the consequence can ultimately be estimated in terms of the medians and uncertainty factors for those variables, based on the computational techniques presented in Section 2 of Volume V.

*The specific wording of this comment was modified slightly (without an impact on comment meaning) as requested by the reviewer.

Comment B-22

(Comment a on page 3-13 of Volume VI)

What are called 95% confidence limits are not. They are percentiles on the indicated distributions.

Response

The comment is, of course, correct within the framework of statistical analysis. However, the term "confidence" is also rather widely used by engineers in providing some measure of how "good" the engineers "feel" about the numbers being used. This "feel" could run the gamut from an out-and-out guess to a judgment extrapolated from prior experience to an assessment completely definable in terms of directly applicable experience data. It is unfortunate that this confusion of terminology exists, but this problem is not unique to the statistical field.* Throughout this report, terms like "confidence interval" and "uncertainty interval" have been used interchangeably mostly in the sense of "engineering confidence" rather than "statistical confidence."

* For example, for the structural analyst the term "stress" has a very precisely defined meaning involving the ratio of the load acting on an area to that area. However, the term also has more general meanings -- a high temperature, the corrosive effects of an acid, and a loud noise (which has an effect on human emotions) all constitute different forms of "stress."

Comment B-23

(Comment b on page 3-13 of Volume VI)

M is undefined. It is apparently a sample size. If so, then the expressions for μ and σ^2 yield the sample mean and variance, usually denoted by $\bar{X}$ and s^2 . If one actually had a normal distribution with mean μ and variance σ^2 , and if a random sample of size M were obtained from that distribution, then the sample mean and variance, $\bar{X}$ and s^2 , are not apt to equal μ and σ^2 . If another sample were taken, different values of $\bar{X}$ and s^2 would result, etc. Thus, treating $\bar{X}$ and s^2 as though they were μ and σ^2 ignores what's usually called "sampling variability" and this can be a serious error. Correct mathematical expressions for μ and σ^2 would have been integrals, not finite sums.

Response

It is agreed that the more standard symbology ($\bar{X}$ and s^2) should have been used in the figure (instead of μ and σ^2). It is well known that the sample values $\bar{X}$ and s^2 vary from one sample to another drawn at random from the same normal population and that they provide unbiased estimates of the population parameters μ and σ^2 . It is true that, for the normal and log-normal distributions, μ and σ^2 should more properly be expressed as integrals.

Comment B-24

(Comment c on page 3-13 of Volume VI)

Confidence limits don't exist by themselves. Rather, we calculate confidence limits on some unknown constant. For example, with data modeled as coming from a normal distribution, one might calculate a 95% upper confidence limit on μ , on σ^2 , on $\mu + 1.645\sigma$, on σ/μ , etc. The expressions given in Figure 3.2 for $\chi_{.95}$ in the normal and lognormal cases might be thought of as approximate upper (or lower) 50% confidence limits on the 95th percentile of the distribution of χ , but not as 95% confidence limits on something.

Response

The essence of this comment is correct, and the response to comment B-22 (comment a in this set for page 3-13) is applicable here as well. However, in view of that response, it is probably not valid to think of the $\chi_{.95}$ values ". . . as approximate upper (or lower) 50% confidence limits on the 95th percentile of the distribution of χ . . ." for a number of variables since the nature of the information available for those variables would not warrant such a sophisticated statistical interpretation.

Comment B-25

(Comment d on page 3-13 of Volume VI)

In light of the above, I wonder whether Max and Min in the uniform distribution case refer to the sample max and min or to the distribution max and min.

Response

Actually, the terms "Max" and "Min" refer to neither the population nor a sample drawn from that population. As mentioned in another response,* the uniform distribution was selected for variables for which no applicable experience data were available. Only the limits on the possible range of variability were known, usually based on physical constraints.

* See the response to Comment B-20 in this subsection.

Comment B-26
(Page 3-14 of Volume VI)

In vol. IV, medians of functions were obtained by Monte Carlo. Now, here in vol. VI they are being obtained by evaluating the function at the median value of the arguments of the function. Why this inconsistency? Also, the "uncertainties" are determined differently. In vol. IV they're determined by Monte Carlo; in vol. VI by linear error propagation. Why?

Response

It would have been preferable to use Monte Carlo simulation throughout the AIPA analysis to evaluate medians and uncertainties of functions. This was possible in Volume IV where the probability functions were relatively simple and, therefore, could easily be evaluated the large number of times required for Monte Carlo simulation by means of a low-cost computer program. However, in the case of the calculations reported in Volumes V and VI, the computational formulas were much more complex; the cost of performing Monte Carlo simulations with the associated codes would have been prohibitive.

Comment B-27

(Comment a on page 3-15 of Volume VI)

On this page, it is claimed that somehow randomness justifies estimating the median of a function by evaluating the function at median values of the arguments. Actually what is necessary is that $\log D$ be linear in the $\log a_i$'s.

Response

In order for the median of a function to be adequately approximated by the function of the median values of the arguments, three conditions actually have to be satisfied:

1. The function is sufficiently linear with respect to each argument over its range of importance (i.e., $\log D$ must be approximately linear in the $\log a_i$'s).
2. The variances associated with the systematic uncertainties in the arguments are sufficiently small that the additive variance terms in Eq. 2-5 of Volume V are negligible with respect to the first term (the function of median argument values).
3. The statistical distributions of all of the arguments (i.e., of the $\log a_i$'s) are sufficiently symmetrical that the higher statistical moments (i.e., above the variance) do not overwhelm the approximate functional linearity and, hence, do not cause a significant contribution to the evaluation of the median of the function.

The need for linearity (condition 1) is satisfied for the arguments considered in Volume VI (as exemplified by Figs. 4-17, 5-8, and 5-18); some of the functional relationships considered in Volume V deviate from linearity, as discussed in greater detail in the response to Comment B-18

(Comment a to page 3-11 of Volume VI). No systematic uncertainties (condition 2) were entailed in the analyses reported in Volume VI, but the few such uncertainties considered in Volume V were taken into account by the computational formula given by Eq. 2-5 in Volume V. In the case of distributional symmetry (condition 3), the symmetry on the logarithmic scale is satisfied by most arguments because of their lognormal distributions. The error introduced by the few other distributions used in the study that are not symmetrical on the logarithmic scale was deemed insufficient to warrant the increased analysis costs entailed in implementing either a Monte Carlo simulation approach or an expanded propagation-of-moments type of formulation (see the response to Comment B-18 (Comment a on page 3-11 of Volume VI)).

Comment B-28

(Comment b on page 3-15 of Volume VI)

The "random uncertainties," which have a standard deviation of $\sigma_{\log a_i}$, apparently average out for the purpose of estimating the median $\log D$. However, they are propagated to yield $\sigma^2 \log D$, so by the statement at the top of the page, they must therefore be "systematic uncertainties". How can they be both?

Response

From the comment it appears that the referenced description is not sufficiently clear. Perhaps the following discussion will add clarity.

Consequences were evaluated in terms of a number of variables that are known to vary in a random statistical manner and are taken to be uncorrelated. These random variations (or uncertainties) were classified in two categories: those called "systematic uncertainties," and those called "random uncertainties." The systematic uncertainties account for variations that are identically the same (in both magnitude and direction) throughout the entire space under consideration (as in the case of variations that are exactly the same over the entire core). The random uncertainties, on the other hand, account for variations that differ (in magnitude and/or direction) from one location to another in space (like variations that differ from one fuel element to another in the core). These local variations can occur with the same or different variances from one location to another. Further discussion of this subject is presented in Section 2.1 of Volume V.

The variables considered in the analyses reported in Volume VI were found to have only systematic uncertainties. These systematic uncertainties were identified in Eq. 3-2b in terms of the standard deviation $\sigma_{\log a_i}$ and were used in that propagation-of-variance formula in evaluating

the dose uncertainty. No uncertainties were used in evaluating the median dose, as indicated by Eq. 3-2a. In contrast to this, the variables considered in the analyses reported in Volume V were found to have systematic and random uncertainties. The random uncertainties (with variances of $\sigma_{\ln d_{ri}}^2$) were used in evaluating function medians, and the systematic uncertainties (with variances of $\sigma_{\ln d_{si}}^2$) were used in evaluating function uncertainties, as indicated in Eqs. 2-5 and 2-6 in Volume V.

Comment B-29

(Comment c on page 3-15 of Volume VI)

The expression for $\sigma_{\log D}^2$ seems to equate analytical derivatives to numerical derivatives and this is incorrect. If $\log D$ is a very nonlinear function of the $\log a_i$'s, then propagation could yield very misleading variances. I find no recognition of this in the report.

Response

The analytical derivatives were approximated by numerical derivatives (based on standard finite-difference techniques) to facilitate their evaluation by existing computer codes. To base the calculations on the analytical derivatives would have necessitated the development of all of the analytical models and of the complex series of computer codes required for their numerical evaluation -- a formidable, very expensive approach that could not have been completed within the time and funding constraints of the study.

With regard to the assumed functional linearity, there is a discussion of this in Section 2.2 of Volume V. It should be noted that the assumption of linearity is satisfied by the variables considered in Volume VI (as exemplified by Figs. 4-17, 5-8, and 5-18); some of the functional relationships considered in Volume V deviate from linearity, as discussed in greater detail in the response to Comment B-18 (Comment a on page 3-11 of Volume VI). As indicated in that response, the added cost of improving the computational accuracy was deemed not appropriate for the small improvement anticipated.

Comment B-30
(Page 3-16 of Volume VI)

The "exp" in equation (3-4a) should be deleted.

Response

Agreed.

Comment B-31
(Page 3-20 of Volume VI)

Here the word "uncertainty" apparently means a standard deviation of some random variable. In earlier pages, (3-14, for one) uncertainty seemed to refer to the random variable and there we had random uncertainties and systematic uncertainties. Other places speak of uncertainty factors, usually equal to $\exp(1.645\sigma)$. These uses are inconsistent and confusing. They make me uncertain about the whole analysis.

Response

As indicated on page 3-14 of Volume VI, the uncertainties in a random variable are classified in two categories: those called "systematic uncertainties," and those called "random uncertainties." As indicated on page 3-15, that portion of the study reported in Volume VI emphasizes the systematic uncertainties and does not consider random uncertainties. (Both forms of uncertainty are considered in Volume V.) In subsequent discussions in Volume VI, the term "systematic uncertainties" has been simplified to "uncertainties." The amount of uncertainty in a variable is measured quantitatively in terms of the standard deviation on the logarithmic scale and the uncertainty factor on the linear scale, where the uncertainty factor f and standard deviation (on the logarithmic scale) σ are related by $f = \exp(1.645\sigma)$.

The above terminology is used consistently throughout Volume VI. While this usage may not be entirely consistent with currently accepted statistical terminology, this should in no way detract from the validity and value of the results and conclusions -- as is also true of the use of the term "confidence" in an engineering sense (see the response to Comment B-22 in this subsection).

Table 3-7. Some of the "95% Confidence Intervals" are intervals, some are single numbers. How much support is there for these numbers? Are they all treated as lognormal? If so, how much support for this assumption?

Response

The intervals refer to lower and upper 95 percentiles of the variables, while single values refer to the upper 95 percentiles of the variables. The lognormal, normal, and uniform distributions were used, as deemed appropriate, to provide the best approximations to the statistical distributions of the variables used in the consequence calculations. However, it is not really important to accurately know the distribution forms for the variables since, according to the central limit theorem, the distribution of the sum of several variables tends toward normality regardless of the forms of the distributions for those variables^{*}; a reasonably accurate determination of the mean and variance of the approximately normally distributed sum can be obtained from a reasonably accurate knowledge of the means and variances of the variables included in the sum. Thus, since it was found that the logarithm of the consequence varies very nearly linearly with respect to the logarithms of the variables considered in the analyses reported in Volume VI, and since the consequence depends on a large number of variables, it follows that the consequences tend to be lognormally distributed and that the choice of distribution forms for the variables does not critically affect the evaluation of the consequence distribution.

^{*}This is true as long as the number of variables in the sum is large enough and as long as the sum is not determined primarily by only one variable (or other small subset).

Comment B-33
(Page 4-9 of Volume VI)

In Vol. II, the uncertainty factor associated with a variable X_1 was defined as $\exp(1.645\sigma_1)$, which must be greater than 1.0. I don't recall seeing a different definition in this volume, but apparently here the uncertainty factor is $D_{i,95}/D_{i,50}$, else how can the first row of Table 4-3 be explained?

Response

It is true that the fundamental definition of the uncertainty factor is $\exp(1.645 \sigma_1)$. However, since the analyses documented in Volume VI were based on finite difference techniques implemented by computer, the above exponential formulation was transformed into a form more directly compatible with those techniques, as discussed on pages 3-14 through 3-16 of Volume VI. Notice in Eq. 3-5 that $D_{i,95\%}$ is directly related to the $a_{i,95\%}$ value of X_1 . Thus, if D is an increasing function of X_1 , then $D_{i,95\%}$ is greater than the median dose. However, if D is a decreasing function of X_1 , then $D_{i,95\%}$ is less than the median dose. Notice also that, in Eq. 3-4b,

$$\begin{aligned} f &= \exp(\Delta \ln D_{95\%}) \quad , \\ &= \exp(\ln D_{95\%} - \ln D) \quad , \\ &= \exp[\ln(D_{95\%}/D)] \quad , \end{aligned}$$

or, as indicated in the comment,

$$f = D_{95\%}/D \quad .$$

From this it follows that the ratio $D_{i,95\%}/D$ (and, hence, the associated uncertainty factor) is computed to be greater than one when D is an increasing function of X_1 (as is generally the case) and less than one when

a decreasing function of X_i (as in a few cases, like the containment release height). This explains the origin of the values referred to in Table 4-3.

Two points should be made concerning this evaluation of uncertainty factors. First, $D_{i,95\%}$ is the dose corresponding to the 95th percentile of X_i . This is the 95th percentile of the dose for an increasing function of X_i and the 5th percentile of dose for a decreasing function of X_i . Second, it doesn't matter whether the contributing uncertainty factors are evaluated in terms of the ratio of the 95th percentile of dose to the median dose or the ratio of the 5th percentile of dose to the median dose; the result is the same in both cases. Let ϕ be the uncertainty factor obtainable by the former approach, and ϕ' by the latter. It is clear, for a log-normal distribution, that

$$\phi' = 1/\phi \quad .$$

On the basis of Eq. 3-4a, it is seen that $\Delta \ln D_{95\%}$ (and, hence, the total uncertainty factor on dose, based on Eq. 3-4b) is based on the sum of terms of the following form:

$$(\ln D_{i,95\%} - \ln D)^2$$

or

$$[\ln (D_{i,95\%}/D)]^2 \quad .$$

If $D_{i,95\%}$ is the 95th percentile of dose, then the dose ratio is ϕ_i , and the term becomes

$$(\ln \phi_i)^2 \quad .$$

If $D_{i,95\%}$ is the 5th percentile of dose, then the dose ratio is ϕ_1 , and the term becomes

$$(\ln \phi_1')^2 = [\ln (1/\phi_1)]^2 = (-\ln \phi_1)^2 = (\ln \phi_1)^2 \quad .$$

Thus, the resultant contribution to the total uncertainty factor for dose is exactly the same for both bases of evaluation.

Comment B-34
(Page 4-41 of Volume VI)

Can uncertainty about experimental applicability really be probabilistically combined with variation within an experiment?

Response

There appears to be no reason a priori why not, since they are separate and independent sources of error.

Comment B-35
(Pages 5-10, 5-11 of Volume VI)

There appears to be notable variation among data sources. Was this separated from the variation within a data source? Also, it seems rather excessive to fit a seventh degree polynomial to such data. Again, here as elsewhere, confidence bounds are given without saying what they are bounds on.

Response

With steam-graphite reaction rates, one is dealing with large uncertainties (near one order of magnitude) in correlating existing data. Thus, the variation among data sources regarding the burnoff effect on reaction rates was found to be relatively small, within about 20%. Since this was about the same degree of variation as that within a data source, no differentiation was made. As shown in Table 5-6 of Volume VI, the burnoff effect uncertainty makes a negligible contribution to the total uncertainty of steam ingress events.

The seventh-order polynomial was chosen since lower order polynomials were found to lack an accurate fit throughout the entire broad range of interest. It was convenient to use this seventh-order polynomial since it was already programmed in the existing OXIDE-3 computer code as described in GA-A12493 (LTR-7). The bounds are considered to be 95 percentiles on the parameter values.

REFERENCES

1. Taylor, J. R., "A Study of Failure Causes Based on U.S. Power Reactor Abnormal Occurrence Reports," IAEA Symposium on Reliability of Nuclear Power Plants, Innsbruck, 1975 (Paper No. SM-195/16).
2. Ibid, Fig. 2.
3. Unpublished data, December 1973 draft of ORNL-4968.
4. Farmer, F. R., "Siting Criteria -- A New Approach," in Proceedings of IAEA Symposium on Containment and Siting of Nuclear Power Plants, April 3-7, 1967, pp. 303-329 (Paper No. SM-89/34).
5. Graham, John, Fast Reactor Safety, Academic Press, New York, 1971, Fig. 1.34, p. 64.
6. Vesely, W. E., "Reliability Quantification Techniques Used in the Rasmussen Study," presented at the Conference on Reliability and Fault Tree Analysis, Berkeley, California, September 5, 1974.
7. Edwards, E., and F. P. Lees, The Human Operator in Process Control, Taylor & Francis Ltd., London and Halsted Press, New York, 1974.
8. Becar, J. J., and D. E. Wood, "Analysis of Effect of Human Interactions on HTGR Safety and Reliability," Report KSC-1037-2, UC-77, Kaman Sciences Corporation, Colorado Springs, Colorado, prepared for ERDA under contract No. AT(04-3)-1037, 1976.
9. Sabri, Z. A., A. A. Hussening, and R. A. Danofsky, "An Operator Model for Reliability and Availability Evaluation of Nuclear Power Plants," Engineering Research Institute, Iowa State University, Ames, Iowa, February 1976.

3.2. COMMENTS FROM ATTACHMENT 2 OF REF. 1 ON P. 3-1

Comment 1 (From Cover Letter)

The presence of surfaces at room temperature in the "King furnace" set of experiments raises questions as to the applicability of the fission product release data to the LOSP accident sequence BD.

Response

It was realized that the major fraction of I, Te, and Cs which condensed on the cool metal surfaces during the P-series of tests might make release estimates based on these tests low compared with PCRV conditions. Therefore, additional tests I1 and I2, with the King furnace modified to better simulate the expected conditions, were performed using higher helium egress flow rates (x4) than those expected in the PCRV. These tests also showed most plateout on water-cooled surfaces and could not be used to develop median release estimates. Thus, as reported, a one-twelfth scale model was tested (tests I3 and I4) which better simulated the actual reactor internals. The release fractions used in the consequence calculations were based either directly or indirectly on these results and are believed to be much more representative of PCRV conditions than release fractions based on the prior experimental results.

Most (70%) of the iodine plateout in the one-twelfth scale experiment was found on metal surfaces 300°C or hotter, and 76% occurred on surfaces over 200°C, as shown in Fig. 1. This figure shows that most of the iodine deposition occurred on surfaces between 200° and 500°C. Thus, sufficient evidence is believed to exist for iodine plateout at these temperatures. The mechanism for plateout is believed to be equilibrium sorption vapor pressure controlled. This means that it is neither kinetics nor surface saturation controlled. Hence, it is adjudged that little of the approximately 20% plateout on the 90°C liner surfaces might have escaped had these

Fig. 1. Distribution of iodine plateout in one-twelfth scale model upper plenum simulation test I-4 (Ref. 3)

surfaces been hotter. Egress rates from the PCRV are quite low after depressurization so that equilibrium conditions should dominate. In recognition of the question of the applicability of the original experiments, additional tests that better simulate the expected reactor environmental conditions are in the planning stage at present.

A revised set of escape fractions has been derived for interim use which are intended to be responsive to the reviewer's comments. The interim approach includes escape fraction estimates from both the Windscale incident and the King furnace experiments in an attempt to enhance the credibility of AIPA escape fractions until better experimental data and computer models become available. An interim probability density function (pdf) for each volatile element escape fraction was developed, as shown in Fig. 2 for iodine. In this distribution, the median King furnace value F_A used in AIPA Phase I work is taken to be the 5th percentile, while the estimated Windscale value F_W , per Ref. 1, is taken to be the 95th percentile, consistent with the earthquake frequency interim approach. Because PCRV escape fractions greater than unity are physically nonsensical, the distribution is truncated at unity. The shape of the distribution is assumed to be nearly lognormal except for the truncated upper tail and a small additional skewness caused by a small shift in the median. This skewness was postulated to reflect the results of the P-series King furnace tests described in Refs. 2 and 3, which tended to shift the median slightly to the high side, closer to the average P-series value (2% for iodine). This process was repeated for cesium, tellurium, and strontium to establish the median interim values F_i summarized in Table 1.

It is noted that the P-series test results were not used for establishing the iodine median escape fraction and were modified to establish medians for the other elements. The P-series tests are considered to yield escape fractions that are higher than they should be because the helium egress rate was higher than reactor conditions and the plateout surface area was relatively small. Therefore, it can be considered that the (unmodified) P-series tests represent, in addition to the Windscale

Fig. 2. Probability density function (pdf) for iodine escape (nonplateout) fraction from PCRV

TABLE 1
PCRV ESCAPE FRACTIONS FOR VOLATILE CONDENSABLE HEALTH HAZARDS

Element	Phase I Value, F_A	Windscale Accident Value, $F_W^{(a)}$	Interim Median Value, F_I
I	4×10^{-4}	0.3	2×10^{-2}
Cs	3×10^{-4}	0.13	2×10^{-2}
Te	4×10^{-4}	0.13	2×10^{-2}
Sr	2×10^{-6}	6×10^{-4}	8×10^{-5}

(a) Taken from Ref. 1

incident results, another probably conservative plateout datum point which should help to establish a more widely accepted set of PCRV escape fractions that can be used until such time as more highly defensible realistic estimates can be developed from a better data base and improved computer models.

A comparison of the median interim nonplateout fractions with the AIPA Phase I values and the Windscale incident values is presented in Table 1. It is noted that the median interim values are about 50 times higher than the Phase I values but are still within the uncertainty factors of 100 to 3000 for the various elements assigned in the Status Report.

The offsite radiological dose levels resulting from the interim escape fractions were calculated for unrestricted core heatup with both isolated and nonisolated containments. These doses, given in Table 2, were evaluated using methodology similar to that in the AIPA report and were developed considering both external exposure from plume or cloud submersion and lifetime inhalation dose commitments for the critical organs. This table also compares the interim revisions with those given in the Status Report. In general, due to increased PCRV escape fractions, the inhalation dose increases for all organ categories over that given in the Status Report, while the whole body dose remains nearly the same. This is due to the fact that whole body exposure is dominated by the noble gas emissions from the PCRV, which do not change significantly.

The increase in inhalation doses from the Status Report is shown in Table 2 for a change in PCRV plateout fractions plus improvements in the fission product transport modeling compounded with the interim plateout fractions. The major improvement is a mechanistic modeling of the PCRV egress flow (about 2%/hr) through the relief valve, versus the instantaneous egress assumed in the Status Report. For both cases, the change in the HTGR risk envelope or in latent health effects is minimal. The reason for this is that the whole body gamma dose was dominant in the Status Report results and the interim results effected an increase in the

TABLE 2
CORE HEATUP BRANCH MEDIAN DOSES AT 2500-m RECEPTOR LOCATION

	Integrated Dose at 2500-m Location (rem)			
	External Exposure	Inhalation Commitment		
	Whole Body Gamma	Thyroid	Bone	Lung
Core Heatup with Isolated Containment (BD Branch)				
AIPA Status Report	1.1×10^{-1}	2.5×10^{-3}	9.0×10^{-6}	1.0×10^{-4}
Interim plateout	9.4×10^{-2}	7.8×10^{-2}	1.1×10^{-3}	7.0×10^{-3}
Interim plateout plus fission product transport modeling improvements	6.9×10^{-2}	5.9×10^{-2}	1.0×10^{-3}	6.3×10^{-3}
Core Heatup with Nonisolated Containment (BF Branch)				
AIPA Status Report	11.8	5.5	3.6×10^{-2}	3.6×10^{-1}
Interim plateout	4.5	6.8×10^1	2.4×10^{-1}	2.7
Interim plateout plus fission product transport modeling improvements	1.4	9.7	1.3×10^{-1}	8.2×10^{-1}

inhalation doses just sufficient to bring these inhalation doses up to around the same level, but not to a higher level of importance.

Another important facet of probabilistic risk assessment is the evaluation of uncertainties. The approach often used in AIPA to evaluate uncertainty factors is to take the ratio of upper limit to median values of a parameter. Such an approach might be grossly applied as follows for escape fractions:

$$U = F_W / F_1$$

However, since the plateout distribution is slightly skewed, the uncertainty was evaluated as if the distribution were normally and symmetrically distributed on a log plateout scale between the 5th and 95th percentiles. In addition, the plateout uncertainty was slightly modified to more accurately account for the effect of the truncated plateout distribution at unity plateout when combining all uncertainties in lognormal statistical equations. The interim plateout uncertainty was established in this way to be a factor of 24. Using this uncertainty factor of 24 for PCRV plateout, together with the uncertainty factors as given in the Status Report for containment plateout and leak rate (Volume VI) and for weather (Volume II), the overall uncertainty factor of 93 on inhalation dose can be estimated as follows:

<u>Parameter</u>	<u>95 Percentile Dose Uncertainty</u>
PCRV plateout	24
Containment plateout	10
Containment leak rate	5
Weather and others	5
Statistical Combination:	93

Using this overall uncertainty factor of 93, the latent health effects were calculated by the LATCH computer code for the core heatup Branch BD interim doses, as presented in Tables 3 and 4 based on the dose propagation

TABLE 3
THYROID LATENT HEALTH EFFECTS FOR BRANCH BD ($U_{95} = 93$)

Occurrence Probability (yr^{-1})	Release of Iodines (Ci)	Inhalation Thyroid Dose (rem)	Total LPZ Thyroid Dose (rem)	Thyroid (man-rem)	Thyroid Nodules ^(a)	Thyroid Cancer Fatalities
5×10^{-7}	47.1	0.059	0.128	49.9	0.02	0.002
10^{-7}	1596	2.0	2.4	934	0.3	0.03
5×10^{-8}	3989	5.0	5.7	1950	0.6	0.06
10^{-8}	27,920	35	37	14,390	4.7	0.47
10^{-9}	231,360	290	295	1.15×10^5	38	3.8

(a) Health effect risk coefficients taken from WASH-1400.

TABLE 4
ORGAN-BY-ORGAN HEALTH EFFECTS AT 10^{-9} YR^{-1} FREQUENCY

Organ	10^{-9} Yr^{-1} Doses ^(a)		WASH-1400 Risk Coefficients	Latent Cancer Fatalities
	LPZ (rem)	(man-rem)		
Whole body gamma	5	1980	100×10^{-6}	0.20
Thyroid	295	1.15×10^5	33×10^{-6}	3.79
Lung	36	1.40×10^4	22×10^{-6}	0.31
Bone	10	3900	6.9×10^{-6}	0.03
				4.3 total

(a) Based on $U_{95} = 10$ for whole body gamma and $U_{95} = 93$ for inhalation doses.

in Fig. 3.* At 10^{-9} year $^{-1}$ frequency, the interim estimates of health effects are ≤ 5 latent cancer fatalities and approximately 40 thyroid nodules. This compares with the ≤ 1 latent cancer fatality and ≤ 5 thyroid nodules that are derivable from the AIPA Phase I analysis.

To demonstrate the effect of choice of PCRV nonplateout fraction probability distribution on health effects, a calculation was performed for varying skewness of the iodine PCRV escape fraction in Fig. 2. The 5 and 95 percentiles were fixed at the King furnace and Windscale values as in Fig. 2. However, the median escape value was varied from 0.01 to 0.1, resulting in progressively skewed distributions. These distributions were input as tables to the STADIC code and combined using Monte Carlo techniques with fixed lognormal distributions for the other major parameters (containment plateout and leak rate and weather). Results of the computer analysis indicate that the thyroid dose distribution deviates from a lognormal shape, especially in the upper end tail, due to the truncated PCRV escape distribution. The overall deviation becomes more pronounced for more highly skewed distributions of PCRV escape fractions, as expected. This departure from the lognormal shape is such that the above-quoted latent health effects are lower over the probability range of interest when the modified (non-lognormal) distribution is used. Regarding the effect of choice of median PCRV nonplateout fraction, it was found that the health effects at 10^{-9} yr $^{-1}$ probability vary by less than 50% as the median PCRV escape fraction varies from 1 to 10%.

In regard to the need for additional experimental data on plateout of fission products within the PCRV under core heatup conditions, it is agreed that a need exists as concluded in the AIPA Status Report. Further integral

* It should be noted that the dose uncertainty distribution is taken to be approximately lognormal. This approximation is believed to be reasonably accurate in the main, central portion of the distribution and to become more and more in error as one proceeds further and further into the upper tail. Any such errors that may exist in those portions of the upper tails shown in Fig. 3 have not as yet been investigated and, thus, are not taken into account in that plot.

Fig. 3. Cumulative probability versus LPZ dose at 2.5 km for core heatup Branch BD

type experiments which better simulate the expected reactor conditions are presently in the planning stage and are being funded by ERDA under the Safety Research Task. Comparison of the results of these tests with calculations using the model now under development is expected to show whether a need exists for kinetic data to describe adsorption, chemical reaction, and desorption under core heatup conditions.

Comments 2 and 3
(From Cover Letter)

2. The values used for the ranking of fission product and core chemistry R&D ranking cannot be reproduced using the data quoted elsewhere in the report.

3. The ranking of fission product R&D shows surprising results that appear to be insufficiently substantiated.

Response

It appears that the reason why the R&D ranking was not reproduced is that the organ doses were not evaluated as the sum of the whole body gamma (external exposure dose) and the respective internal dose to the organ caused by inhalation, as was done in the AIPA study. For example, the thyroid health effect is determined from the whole body gamma and inhalation thyroid doses, not the inhalation thyroid dose alone.

The surprising results shown by the fission product R&D ranking are partially explained in the preceding paragraph. Also, there is a lack of recognition that some parameters, such as PCRV plateout, affect whole body beta doses differently than whole body gamma and, thus, one cannot always use uncertainties on total whole body dose as indicative of whole body gamma uncertainties.

Comment 4

(Comment on PCRV Plateout, from Page 3)

In general, all the fission product release data given in Table 4-9 corresponds to release from an orifice situated downstream from surfaces maintained at or about room temperature and hence capable of condensing some fission product vapors.

Response

The response to this comment is covered in the responses to previous comments in this subsection.

Comment 5

(Comment on PCRV Plateout, from Page 4)

It is somewhat surprising that the uncertainty factor of 50 for experimental applicability of iodine is increased by the normalization factor of 40 to obtain the experimental applicability uncertainty factor of 2000 for release fractions of elements other than iodine. The AIPA document does not elaborate on this assumption.

Response

It was implicitly assumed that the same uncertainty factor of 50 applied to the other elements tested as well as to iodine. The normalization factor of 40 corrects for the different experimental conditions, mainly higher flow rate, of the first series of tests. While these factors would ordinarily have been combined statistically, they were simply multiplied to reflect the higher level of uncertainty judged to exist in this case.

Comment 6

(Comment on PCRV Plateout, from Pages 4 and 5)

The values given in Table 4-10 of Volume 6 that have been reviewed above do not agree as would be expected for consistency with values of the transport sequence factors and their uncertainties shown in Table 4-6 of Volume 4. However, the values of either table for the PCRV escape transport factors are not significantly different from what can be computed using the reference data and the assumptions made.

Response

The values given in Tables 4-10 and 4-13 of Volume VI for PCRV escape fractions and uncertainty factors during core heatup are consistent with the transport sequence factors and their uncertainties in Table 4-6 of Volume IV allowing for roundoff. The PCRV escape fraction for other nuclides (bone) in Table 4-6 of Volume IV is based mainly on the Te and Sb of group 10, Table 4-10 of Volume VI, rather than the Sr of group 1. The uncertainties in Table 4-10, Volume VI, are total uncertainties including data scatter as noted in the column heading.

Comment 7
(Comment on PCRV Plateout, from Page 5)

It may be questioned whether the basic release data which pertains to fission product egress from heated volumes through passages maintained at room temperature can be applied to release from the PCRV of an HTGR undergoing a core heatup. Furthermore, the data is normalized to the scale model data obtained in an experiment where the iodine had access to cooler plates of the thermal shield. An HTGR with fiber insulation might be expected to permit much less access of iodides to surfaces below about 300°C by diffusion, especially under natural convection conditions, than the experimental equipment using multiple plate insulation.

Response

This comment is partially covered in the response to Comment 1 of the cover letter, the main point being that some tests showed most of the iodine plateout on surfaces 300°C and higher even with cooler surfaces nearby. In an HTGR, little diffusion to the cooler liner would be expected until the cover plates over the fiber insulation had reached their failure temperature. At this time, the cooler liner would become available for plateout.

Comment 8

(Comment on PCRV Plateout, from Page 5)

Calculations by L. Epel show that the temperature of the pressure relief valve exit tube can be expected to be of the order of a few hundreds of degrees Celsius and hence that substantial deposition in such a tube is not probable.

Response

It is agreed that the relief valve exit tube provides for a relatively small amount of plateout, but not necessarily on the basis of the temperature being too high. Information provided in the response to Comment 1 (see Fig. 1) shows experimental evidence of substantial I-131 plateout on surfaces having temperatures of a few hundred degrees Celsius and higher. The reason for believing that significant plateout does not occur in the relief valve tube is that this surface represents only a small fraction of the total surface area of the PCRV inner cavities and heat exchanger tubing which are available to accept plateout. As noted in the response to Comment 1, a temperature and flow dependent plateout computer code is being developed which will provide for a more adequate modeling of this important phenomenon.

Comment 9
(Comment a on R&D Guidelines)

a) In reference to the probability model of core cooling and the operator response model, it is not clear whether the present effort can be extended much further without specific construction details. In the Reactor Safety Study (WASH 1400) effort it was found that the estimation of some probabilities required the physical observation of existing installation and review of operator manuals.

Response

The AIPA study team disagrees with the basic premise behind this comment. Undoubtedly, there are generic aspects of the probability modeling of core cooling and operator response that could benefit from further study. For example, the core cooling model includes in its formulation a consideration of cooling system restoration probabilities, which are based on an estimate of the permissible duration of cooling system outage. The allowable outage duration is the time from cooling system failure to the attainment of PCRV critical damage limits, which are the temperatures at which PCRV components are damaged in a manner that permanently prevents subsequent cooling of the core. The analysis was based on the knowledge that the critical damage limit is determined by the thermal barrier cover plates in the upper plenum, and the assumptions that all of the plates fail at 1500°F and that the cover plates and exposed thermal barrier material all fall to the irregular surface (formed by the top reflector orifice plenums, flow valves, and control rod guide tubes) in such a way as to provide sufficient blockage of core coolant channels to preclude adequate core cooling. This is recognized to represent a condition more severe than would reasonably be anticipated in such a situation.*

* Volume IV, pp. A2-31 through A2-34.

Further analysis of the mechanisms of cover plate failure and of the feasibility of circulating primary coolant through the fallen cover plate material would provide the basis for a more realistic probability model of core cooling.

As an example in the area of operator response modeling, the same basic form of model (namely, exponential, with a constant mean time to respond) was used throughout the study. While such a model may be adequate for most of the short-term sequences considered in the study, there are undoubtedly some sequences (like some of the long-term sequences associated with small reheater leaks) for which the model is not adequate. Furthermore, it was assumed that, when an operator responds, the effects of his response on potential releases are no worse than if he had not responded at all. While this may be true in a majority of the sequences considered, there are undoubtedly instances in which an operator could respond in such a way as to unnecessarily increase the consequences of an accident. Another fruitful generic area of investigation might be to estimate the degree by which operator responses can enhance the overall safety of the HTGR, particularly in conjunction with the rather large time intervals that are allowable for cooling system outages. Thus, there are still generic areas in which improvements can be made in operator response models and on the overall understanding of the impacts (both desirable and undesirable) of the operator on plant safety.

The comment also makes reference to the use of specific plant construction and operator manual data in some of the analysis steps documented in WASH-1400. In this regard, four observations seem apropos.

1. There are variations in the constructional and operational details among the approximately 50 plants that were in operation at the time WASH-1400 was issued. Changes have also been made at individual plants after they became operational as a result of retrofits necessitated by changes in NRC licensing and regulatory requirements.

2. WASH-1400 is intended to provide results that are representative of the first 100 plants to become operational. However, the analysis of constructional and operational details was restricted to only two plants [one BWR (Peach Bottom, Unit II) and one PWR (Surry Power Station, Unit I)] that were considered to be representative of all 100 plants. The study did not delve into the constructional and operational details at all existing plants and did not assess medians and uncertainties from these results, nor did it assess the upgrading effects of retrofits. This subject apparently constitutes a separate follow-up study.
3. It would be interesting to know if the improvements in probabilities in WASH-1400 which resulted from physical observation of the existing installations and review of operator manuals had a significant effect on the location of high risk branches on a probability-consequence plot. Studying WASH-1400 has revealed no significant impact.
4. As HTGR constructional and operational details become available, this information should be useful in refining the analyses performed in the AIPA study (note that a refinement is anticipated, not a gross adjustment).

The AIPA study is viewed from two different, but equally important, points of view. On the one hand, input data for the study were obtained from many different sources, including the reference plant design and all of the operating experience deemed appropriate for consideration in the study. On the other hand, the output from the study is recognized to provide insight and information that serves as useful feedback for reference plant design as well as for plant construction and operation. In particular, it is anticipated that an improved understanding of operator psychology and of the modeling of their responses could provide valuable insights that would be useful in improving instrumentation and methods of display in the control room and operator manuals.

Comment 10
(Comment b on R&D Guidelines)

b) The limited review of the AIPA report confirms the great need for experimental data on plateout of fission products within the PCRV under core heatup conditions.

Response

It is agreed that there is a need for experimental data on fission product plateout during core heatup transients. The recognition of this need is demonstrated by the fact that this was one of the R&D recommendations included in Volume IV of the AIPA Status Report. ERDA recognized the importance by agreeing to fund development of a detailed computer code plus initiation of a new series of experiments in FY-77. Further details appropriate in response to this comment are contained in the detailed response to the BNL comments on PCRV plateout phenomena (Comments 4 through 8 in this subsection).

Comment 11
(Comment c on R&D Guidelines)

c) It is surprising that equipment and structural survival appears mentioned only in connection with seismic events and not in connection with LOSP induced core heatup.

Response

There are many instances in which equipment and structural failure are considered. A broad range of such failures were considered as initiating events in the preliminary analysis (Volume III). The list of such failures includes the following:

1. Helium instrument line break
2. PCRV purge header break
3. Rapid PCRV depressurization
4. Gas waste surge tank rupture
5. Liquid waste tank rupture
6. Main steam pipe rupture
7. PCRV structural failure

In conjunction with the potential for damage resulting from the unlikely occurrence of a core heatup transient following an LOSP, there is a rather lengthy qualitative discussion of the structural integrity of the PCRV and containment.* As a matter of fact, it was precisely this area that led to the inclusion of a study of PCRV and containment structural integrity among the R&D suggestions.** This recommendation is currently being implemented. There is other equipment (like the circulators and steam generators) whose operating capabilities are adversely affected by the temperatures developed during a core heatup transient. However, such equipment either does not reach its critical damage limits as soon as do

* Volume IV, pp. 4-36 through 4-39.

** Volume IV, Table 5-7 on p. 5-9.

the cover plates for the thermal barrier in the upper plenum or it does not reach it at all. In view of the conservative assumption that the attainment of 1500°F at the upper thermal barrier cover plates causes a permanent loss of core cooling capability* (as discussed in the response to Comment 9, above), concern for structural integrity was focused on those plates for the purposes of the Phase I AIPA analysis. It is anticipated that the adoption of more realistic assumptions in this regard would admit the possibility of restoration of core cooling even after the 1500°F limit may be surpassed. In such a case, the structural integrity and critical damage limits of other equipment might become more important for the purposes of further refinements in the AIPA analysis.

* Volume IV, p. A2-33.

Comment 12

(Comment on Safety R&D Ranking, from Pages 23 and 24)

...attempts were made to reconstruct the derivation of indicative factors of fission product parameters for ranking of safety R&D for the case of core heatup. It was expected that using the expressions and data given elsewhere in the report the results could be reproduced. This attempt was unsuccessful because apparently the data used for the ranking is not the same as the intermediate results presented elsewhere in the report. A sample of the difficulties found in recreating the results will follow.

In Table 6-2 skin is one of the organs used in the evaluation; however, the tables of dose rate such as Table 4-13 do not give the data separately for skin. In Table 6-2 the Man-Rem dose is obtained from the median dose by multiplication times 394. The health effect is obtained by multiplying the Man-Rem figure times the factor called total cases in Table 7-1 of Volume I. Both of these are consistent. However, although the "Total Uncertainty Factor" for whole body gamma agrees with the "Overall Uncertainty Factor" of Table 4-13, corresponding agreement for thyroid and bone could not be located anywhere else in the report. Notice that the uncertainty factor for thyroid is 436 in Table 4-13 and 27 in Table 6-2. Similarly, the median dose for whole body in Table 6-2 agrees within a factor of 2 with the median dose given in Table 4-13, but the median doses for thyroid and bone seem to have no relation with the corresponding values given in Table 4-13.

Using the ratios

$$\frac{U_T - U_{T-j}}{U_T}$$

computed from the data in Table 4-13 for the cases of Fuel Release Rate, PCRV Plateout and Decay Heat, the final parameters F_T given in the report could not be reproduced either from the data in Tables 6-2 or 4-13.

Response

A simple error is being made in trying to reproduce the numbers in the R&D ranking tables. Namely, the thyroid and bone doses are being confused with inhalation thyroid and inhalation bone doses. Both external radiation (whole body gamma) and internal exposure (inhalation doses) can cause individual organ health effects. Therefore, the following formulas should be used:

Whole body health effect doses = whole body gamma

Thyroid health effect doses = whole body gamma + inhalation thyroid

Bone health effect doses = whole body gamma + inhalation bone

Skin health effect doses = whole body gamma + beta

Overall uncertainties for organ health effects should be calculated as the square root of the sum of the squares of the component uncertainty factors weighted by the component fractional dose contribution. Thus, in the example given, the thyroid uncertainty factor is the statistical combination of the whole body gamma uncertainty factor of 25 and the inhalation thyroid uncertainty factor of 436. Because whole body gamma dose is dominant, the total thyroid uncertainty factor is calculated to be close to the value of 27, listed in Table 6-2 of Volume VI.

Regarding completeness and consistency of the data in Volume VI for reconstruction of the R&D ranking, rechecking indicates the data are sufficient and consistent. Median dose tables for the events (Tables 4-13, 5-6, 5-17, and 5-18) list whole body gamma plus beta doses and inhalation thyroid and bone doses in a consistent format. Tables 6-1 through 6-5 list median whole body gamma doses and total thyroid, bone, and skin doses. Total uncertainty factors, which can be independently checked from the component uncertainty factors in the tables mentioned above, are listed

also in Tables 6-1 through 6-5. These values can be used directly in Eqs. 3-8 and 3-9 (in Section 3 of Volume VI) to compute R&D index ranking factors.

Comment 13

(Comment on Safety R&D Ranking, from Page 24)

The health effect factors of Table 7-1 and the median doses given in Table 4-13 would indicate that the ranking of parameters is dominated by noble gases, through whole body dose. Hence, it would be expected that the ranking would reflect the uncertainty factors for whole body dose of Table 4-13, and that PCRV plateout would be ranked first instead of second as shown in Table 6-2. Therefore, it is concluded that the justification of the ranking is not within the body of the report.

Response

The case of PCRV plateout is unusual in that this parameter affects whole body gamma doses differently from whole body beta. Most parameters affect the major dose contributors, which are the same for both beta and gamma doses. However, plateout affects the whole body gamma contributing xenons (through the iodine precursor effect) to a minor degree and the whole body beta contributor Rb-88 to a major extent. Rubidiums are treated in the same group as cesiums and have plateout fractions of 3.1×10^{-4} median and 0.93 maximum (Table 4-10 of Volume VI). Therefore, variation of plateout fractions to the upper (95 percentile) limits releases nearly all rubidium from the PCRV. Figure 4-20 shows Rb-88 to be a contributor to whole body beta. Note that in Table 6-2 PCRV plateout is important mainly due to its large effect on inhalation thyroid dose and not due to its smaller effect on the more dominant whole body gamma dose.

Comment 14

(Comment on Safety R&D Ranking, from Pages 24 and 25)

The ratio of uncertainties used for the computation of F_{organ} factors creates an undesirable artifact, because by dividing by the overall uncertainty it lowers the ranking of all the parameters that are controlling a given organ when the organ has a large uncertainty factor. For example, the ratios for the PCRV plateout fraction for thyroid and bone are 0.873 and 0.972, respectively, using the data of Table 4-13. This tends to mask the ratio of 6.6 in uncertainties. Perhaps a more significant form of the F_{organ} factors could be obtained by the simple use of the difference $U_T - U_{T-j}$.

Response

The type of formulation suggested in the comment -- to replace the ratio $(U_T - U_{T-j})/U_T$ by the difference $U_T - U_{T-j}$ -- was previously considered (in a somewhat different form) and discarded as an index for parameter ranking. The reason for rejecting the difference is as follows. The difference $U_T - U_{T-j}$ basically provides a measure of (but is not actually directly equal to) the uncertainty U_j . If U_j were used in place of the ratio $(U_T - U_{T-j})/U_T$ in Eq. 3-8 of Volume VI, then U_j would be multiplied by the median dose (and some other factors). The product of these two quantities would yield a single dose value -- namely, the upper 95% uncertainty limit on dose. This single value would not be the uncertainty (by itself) and, hence, would not provide a good measure of the capacity for reducing the uncertainty by performing R&D. Thus, it is concluded that the use of U_j (or some heuristically equivalent form, like $U_T - U_{T-j}$) would not provide a proper basis for defining the F_{organ} factor.

It should be noted that, if the F_{organ} factors were calculated by the proposed expression, the results would not alter the ranking of the top three parameters shown in Table 6-6 of Volume VI. As seen in Tables 6-1 through 6-5, the total uncertainty factors differ by at most a factor of 2,

while the top three parameters have F_{organ} factors more than an order of magnitude greater than the remaining parameters listed. The order of the top three parameters remains unchanged due to the comparable total uncertainty factors found for the core heatup event (Table 6-2).

The numbers presented in the comment suggest some confusion over the interpretation of Tables 6-1 through 6-5. The simple ratio of the inhalation dose uncertainty factors (6.6) is not of much significance. The organ health effects are based on the sum of the inhalation and whole body gamma doses. The whole body gamma dose is the major contributor. Of greater significance are the facts that, because the whole body gamma dose is the major dose, (a) the overall uncertainty factors on the organ health effects are all about the same for core heatup (Table 6-2), and (b) the changes in these overall uncertainty factors caused by eliminating the plateout uncertainty are moderate. It should further be pointed out that the direct ratioing of uncertainties probably does not constitute a valid basis for assessing the validity of the current AIPA formulation. The reason for this is that the total uncertainty is not determined from a linear combination of contributing uncertainties. Based on Eq. 3-1b of Volume VI, it would appear that the ratio of the logarithms or of the squares of the logarithms of the contributing uncertainties would be more appropriate.

Comment 15

(Comment on Safety R&D Ranking, from Pages 25 and 26)

The main trend of the ranking of parameters shown in Table 6-6 is implicit in the uncertainties shown in Tables 4-13, 5-6, 5-17, and 5-18 in conjunction with the probabilities of the main events, and their estimated consequences. The main features of the ranking of Table 6-6 are consistent in general with the data given above. However, there are some surprising results in the final ranking which are difficult to explain because of the difficulty of reproducing the results. The Fuel Release Rate being ranked above the PCRV Plateout is somewhat surprising considering the parameter uncertainty factors shown in Table 4-13. The ranking of Decay Heat in third place is surprising because the uncertainty of the Decay Heat estimates is of the order of 10% according to R. E. Sund in GA-A12499 (GA-LTR-4) and because on physical grounds one would expect that an error in the Decay Heat expression would only change the time of the release of the noble gases and iodine which are the dominating fission products in the core heatup event. The ranking of the Decay Heat in third place is consistent with the assignment of parameter uncertainties summarized in Table 4-13. What is surprising is the magnitude of the uncertainty factor given in Table 4-13. A possible explanation for this surprising result is that the core heatup event involves subevents in which the release is dependent on CACS restart and repair times.

Response

A main value of the AIPA approach to safety R&D recommendations is that many intuitive impressions are tested against quantitative evaluation. Oftentimes the impressions are proved superficial because the entire picture was not in view. For example, researchers on a specific data input often feel that existence of large uncertainties in those data automatically justifies the need for further research, or that a parameter's strong effect on a particular dose must be important. When one considers what is

controlling to safety, small uncertainties in key parameters can be important and large uncertainties in other parameters can have little effect on the overall safety picture. On the other hand, it is felt that the quantitative calculations should be contrasted against intuition as a check and as an aid to understanding.

The explanation for the parameter ranking in Table 6-6 is as follows. The uncertainty factor on whole body dose due to PCRV plateout is listed in Table 4-13 as 1.4, but this is mainly due to the effect on whole body beta dose as explained in a previous response. Plateout has a smaller effect on the controlling whole body gamma dose. In fact, the importance of PCRV plateout stems mainly from its large uncertainty effect on the secondary inhalation thyroid dose, as shown in Table 6-2 (94% of F_T is due to thyroid). The largest uncertainty on the controlling whole body gamma dose is a factor of 1.3 (Table 4-13) for particle release rates, which is high enough to overshadow the large plateout uncertainty on the thyroid dose. The second highest uncertainty on whole body gamma dose is 1.19 (Table 4-13) for decay heat. The decay heat variation, as recommended in GA-A12499 (Ref. 1), is 20% for times up to 1000 sec and 10% thereafter. Thus, the core fission product release is sensitive to change in temperature history and this is consistent with the experience in core heatup analyses. It is true that both fuel particle release rates and decay heating simply change the time of release of noble gases and iodines. However, these fission products decay rapidly enough that such delay reduces their inventories. It should be borne in mind that the uncertainty factors in Table 4-13 pertain to LOSP Branch BD (intact containment) and the delay uncertainty factors due to decay heat and particle release rates are higher for sequences such as LOSP Branch BF (nonintact containment).

Comment 16

(Comment on Safety R&D Ranking, from Pages 26 and 27)

The usefulness of this ranking for guidance on allocation of R&D resources is reduced by two difficulties in methodology. Some R&D subjects deal with causal effects in the event trees which might affect the probabilities. For example, fission product plateout in the primary system could mechanically degrade metallic parts and alter conditional probabilities of failure. Research on such phenomena is not covered in the present AIPA methodology for ranking. The second difficulty is the absence of the expected reduction in uncertainty of parameters as a result of research from the figure of merit used for ranking. For example, in an unrealistic case, if the ranking were to be extended to include the meteorological parameter the meteorology would receive a top ranking because of the "uncertainty factor" of 16. However, it is clear that research in meteorology cannot reduce the inherent randomness of the weather. In a realistic case, "PCRV Plateout" and "Decay Heat" are given about equal ranking, even when data on PCRV Plateout is extremely sparse and research in this area appears to be capable of substantially reducing uncertainties whereas Decay Heat data are comparatively abundant and substantial uncertainty reduction does not appear possible. A more successful ranking of topics for research would include as a factor the equivalent of the "information received in a message" from information theory. That is defined as the logarithm of ratio of the uncertainty before the research is conducted to the expected uncertainty after the research is finished.

Response

With regard to the first difficulty, the AIPA study team recognized, on a conceptual level during the early stages of the Phase I analysis, the potential for interdependencies between consequences and occurrence probabilities of various branch sequences. Also, certain consequences could cause consideration of new branches. While this was acknowledged as a

potentially important consideration, it was judged to be, on a broad and inclusive basis, a level of analytical detail beyond that to be included within the scope of the Phase I effort. It was, therefore, postponed for rigorous consideration in subsequent and more detailed analysis. There are, however, examples of such interdependencies included in the Phase I work. Thermal analysis of core heatup guided the evaluation of failure probabilities of the PCRVR relief valve to reclose. Steam-graphite reaction consequences strongly affected containment flammability and failure probabilities during steam ingress events. At the present time, a detailed study is being performed of the effect of core heatup conditions on the probabilistic structural integrity of the PCRVR, an R&D item that was included in the Status Report recommendations (Volume IV).

With regard to the second difficulty, it is noted that the example uncertainty of 16 for weather was acknowledged in Volume II, Section 5, to be substantially conservative, and the more realistic uncertainty is on the order of 3. Nonetheless, the point in the comment that the degree by which R&D can be expected to be reduced in uncertainty seems to have merit. Some exploratory work has been performed in the form of cost-effectiveness concepts for the ranking of parameter importance. Not only are anticipated improvements considered but also attendant development costs to achieve the improvements. This particular approach has provided some preliminary insights, although the basis is somewhat different from the decision-theory approach suggested in the comment.

First implementation of these concepts was made in a recent test specification prepared for the core heatup and fuel particle release rate experiments being performed under ERDA Project Agreement 51 (189 No. SU029), HTGR Safety Research Task. Analysis was made, as part of the test justification, of the potential reduction in AIPA dose uncertainties as a result of the allocated funding. This was matched against the uncertainty reduction needed to cause the particle release rate safety index factor in Section 6 of Volume VI to be lowered to a level with the decay heat index factor.

Comment 17

(Comment on Research Selection Criteria, from Pages 39 and 40)

The comparison of the estimation of risk of AIPA with the envelope of risk of WASH 1400 to see if the main sequences fall under the conditions of criteria 2 and 3 has not been made. However, on the basis of release fractions it appears as if all of the AIPA accident sequences fall under criterion 1. If this tentative assignment is correct, then criteria 2 and 3 become largely superfluous. Three important areas of research appear to be covered only by extension in criterion 5. Namely, the continuing reevaluation of the estimation of risk accident sequences as new information develops, the continuing search for new initiating events and unrecognized dependencies to support the tacit assumption of completeness, and the analysis of low probability accidents which can be neglected from the point of view of risk but require understanding as a matter of regulatory responsibility.

Response

The first point made in the comments is that the AIPA risk envelope was not compared with that of WASH-1400 in the Status Report. This is correct. The Status Report basically documents the progress of the study up to the status meeting and presentation made to ERDA in June 1975. The work completed at that time represented a substantial beginning of a continuing study in which the methods for using the AIPA quantitative measures of safety for deriving R&D suggestions were still in the formative stage. One step in this evolving technology was the development of a set of general R&D criteria. A set of three criteria was originally conceived at about the time of the June 1975 meeting. As a result of the joint efforts of ERDA, ANC, and the AIPA study team spanning a period of several months after that meeting, the set was reviewed and improved to form an expanded version containing five criteria. This set of five criteria was documented in a section of the Status Report entitled "Future Work" to provide an

indication of the general nature of the approach to be taken in future efforts to develop R&D recommendations, as indicated in the report.*

The next point made in the comments is that all of the AIPA accident sequences fall under category 1 and, hence, that categories 2 and 3 become largely superfluous. In this regard, a couple points of clarification are in order. First, one of the implied objectives of this study (although not specifically identified as such) is the development of a quantitative, probabilistically oriented methodology that can be used by ERDA to provide guidance in the selection of safety R&D in a wide variety of gas-cooled-reactor projects. The AIPA project serves as a vehicle for the development of the methodology, and the application of that methodology to the HTGR serves as a pilot study, as well as a means of assuring that the methodology that evolves provides a practical, useful tool for accomplishing its intended purpose. Within this context, the need for including criteria 2 and 3 for the sake of the generality of the complete set of criteria and, as a result, the generality of the methodology being developed is clear. The second point of clarification is that, because of the uncertainty levels entailed in the AIPA study, some of the low consequence accident sequences may not fall under the provisions of criterion 1. In this regard, there is a need to better define the application and implementation of criterion 3 to these low consequence sequences.

With regard to the comments concerning continuing reevaluation of risk estimates and the continuing search for new initiating events, the AIPA study team agrees. This is consistent with the foregoing discussion. It is, perhaps, a moot point as to whether the quest for "completeness" is justified by criterion 5 (the judgment of experts being required to decide when the analysis is "sufficiently" complete for practical purposes) or by criteria 1 through 3 (to assure an "adequate" basis for comparison of risk envelopes). The latter concept is preferred, with criterion 5 being

*Volume I, p. 8-2.

reserved for R&D guidance from bases other than those provided directly by the AIPA methodology.

With regard to the use of criterion 5 to justify further analysis of low probability accidents as a matter of licensing responsibility (even though they may appear to be unimportant from a risk point of view), there is general agreement, at least for some such accidents. It is recognized that, in some cases, uncertainties in probability and consequence models may indicate a need for further study and/or R&D to improve modeling accuracy. In other cases, it may be quite clear that further analysis is justified in conjunction with the consideration of alternative design options, with economics included as one of the variables. In still other cases, it may be clear that neither form of analysis would be worthwhile.

It is suggested that there are two important facets of regulatory responsibility:

1. Assure that the safety of nuclear plants is acceptable for all credible accident sequences, based on considerations of both probability and consequence.
2. Assure that there is a proper balance of safety requirements so as to avoid situations wherein disproportionate sums of time, talent, and money are concentrated on a few accident sequences to attain very high levels of safety while other accident sequences exist, for which lesser sums of time, talent, and money are spent, that have significantly lower levels of safety. Examples of such paradoxes are available from the LWR regulatory process.

It is believed that studies like AIPA and WASH-1400 can provide useful insight and guidance in both of these aspects of regulatory responsibility.

REFERENCES

1. Clarke, R. H., "An Analysis of the 1957 Windscale Accident Using the Weerie Code," Ann. Nucl. Sci. Eng. 1, 73 (1974).
2. "PSC Quarterly Progress Report for Period Ending September 30, 1967," USAEC Report GA-8276, General Dynamics, General Atomic Division, October 30, 1967. See also GA-8600, GA-8038, and GA-7939 in the same series.
3. Public Service Company of Colorado, "Fort St. Vrain Nuclear Generating Station Final Safety Analysis Report," Appendix D, 1967 (USAEC Docket 50-267).
4. Sund, R. E., "Afterheat Calculation for the HTGR," Gulf General Atomic Report Gulf-GA-A12499-A (GA-LTR-4), Amendment A, November 1, 1973, Amended July 1974.

3.3. COMMENTS FROM ATTACHMENT 3 OF REF. 1 ON P. 3-1

Comment 1

On page 5-12 of Vol. 1, GAC refers to a "threshold for latent fatalities" from the Biological Effects of Ionizing Radiation (BEIR) report. GAC appears to misunderstand the dose-risk correction models that are used in this report. The BEIR report does not suggest that a threshold for latent fatalities exists; in fact, the analyses in the report are based on a linear, non-threshold model. GAC again illustrates their lack of understanding of the BEIR report non-threshold model by the introduction of the "BEIR line" (at 10^4 man-rem whole body) in Fig. 9-2 on page 9-5 of Vol. 1. The BEIR report does not develop, or even suggest, such a "threshold" line below which no long-term cancer deaths are likely. GAC appears to have decided that accidents that result in less than 10^4 man-rem whole body give acceptable consequences (a few latent fatalities). It should be made clear that this criterion for acceptable consequences is that of GAC and not that of the BEIR report.

Response

The BEIR Committee concludes that the excess mortality expected subsequent to low-level irradiation of the general population is between 50 and 165 induced cancer deaths per million man-rem whole body exposure during the first 25 to 27 years after irradiation. From this range, a representative value of 100 latent deaths per 10^6 man-rem might be inferred or an individual risk on the order of 1×10^{-4} deaths per man-rem. Since latent somatic effects are integrated over the population at risk and can only be estimated on a statistical basis, it is difficult to appreciate the concept of individual risk when applied to somatic effects. Further, it is difficult to appreciate the concept of fractional death, e.g., 0.01 deaths per 100 man-rem or 0.1 deaths per 1000 man-rem. This prompted the definition of the concept of an "integer latent effect threshold" for use in HTGR accident consequence comparisons. The integer latent effect threshold

is defined as that exposure level that, on the average, may be expected to produce one latent somatic effect. From the BEIR studies, an exposure level of 10^4 man-rem can be inferred for the threshold defined in this manner. However, this 10^4 man-rem exposure does not imply that lower exposure levels produce no latent cancer fatalities; such effects are possible but, as indicated in Fig. 9-2 of Volume I, are not likely.

The comment correctly points out that the BEIR report does not develop a "threshold" line value for latent effects but, rather, that this criterion is attributable to the AIPA study team's interpretation of the committee's findings. This distinction will be clearly specified in future work.

It should be clarified that no formal statement has been made in the Status Report about the acceptability of accident consequences. However, it appears reasonable to conclude that exposure levels that cause almost no cancer deaths are reasonably acceptable. This conclusion is based on the following quotation from WASH-1400.*

"In the largest accident predicted in the study the 1500 latent cancer fatalities would be distributed over approximately 10 million people. The normal incidence rate of fatal cancer in this population is about 17,000 (Ref. 9) per year. Thus the largest potential accident would represent an increase over the normal rate of 1500/17000, or about 9%. This effect would probably not be measurable statistically because of the large variations in the normal rate."

The two fatality figures quoted above are both substantially higher than the almost no cancer fatalities predicted in the AIPA study over a meaningful range of frequencies.

* WASH-1400, Main Report, Section 5.5.4.1, October 1975.

Comment 2

In Vol. VI, GAC considers doses from four sources (p. 3-1): (a) whole body dose from external gammas, (b) whole body dose from external betas, (c) thyroid dose from inhalation of iodine, and (3) bone dose from inhalation of bone-seekers. A discussion of the lung dose from the inhalation of radionuclides is limited to a footnote on page 3-21 and to a very short discussion in Section 6 (Safety R&D Significance of Fission Product Data) with no presentation of the calculated results (this lung dose discussion appears to be an after-thought that was put into the volume near its completion).

It appears that GAC has not considered all of the sources of latent health fatalities. Doses from internal emitters should be considered on an organ-by-organ basis. The only organs GAC considered in detail for internal doses are the thyroid and the bone skeleton. It is clear, from Table 3-5, Vol. VI, that the bone dose refers to the bone skeleton dose rather than the bone marrow dose. This choice of the skeleton dose (which leads to bone cancer), rather than the bone marrow dose (which leads to leukemia), is unfortunate as leukemia from the bone marrow dose (for most inhaled radionuclides) produces more latent fatalities than does bone cancer from the bone skeleton dose. In addition, lung cancer from the lung dose (again, for most inhaled radionuclides) produces more latent fatalities than does bone cancer from the bone skeleton dose. It is thus puzzling why GAC chose to emphasize the less important bone skeleton dose rather than either the bone marrow or lung dose.

Response

Studies to date indicate that the overwhelming latent somatic health damage mechanism, in the envelope of HTGR sequences, is correlated to external exposure whole body irradiation. The HTGR environmentally released source term consists predominantly of noble gases with only small amounts of inhalable aerosol particulates present. For this reason, HTGR

accident-related doses delivered by internal emitter commitments are secondary in comparison with external whole body irradiation of the same organ. Confirmation of these findings is maintained by estimating the internal inhalation organ commitment for thyroid gland, skeletal bone, lung, gastrointestinal tract, and total body categories for each and every HTGR accident sequence evaluated to date (Ref. 1).

Bone-marrow-related health effects (primarily leukemia) are considered as part of the total soft tissue health consequence factor [see Volume VI, Table 3-5, Note (a)]. This manner of accounting for bone marrow doses is consistent with that in WASH-1400 (see Tables VI 9-4 and 9-5). Consideration of the inhalable contribution to bone marrow irradiation would not affect the findings reported in Volume VI [e.g., for the LOSP Branch BF (see Volume VI, Table 4-12), the inhalation skeletal bone dose is on the order of 2.7% of the external whole body dose]. Further, the bone seekers deposited in the skeleton (primarily strontium) irradiate the bone marrow at a level of approximately 20% of the skeletal dose, which results in an overall total body external exposure to bone marrow inhalation dose ratio of 0.27%.

The inhalation lung dose and dose to the GI tract were routinely calculated by the consequence model, as were other organ doses. Because the whole body gamma dose was so dominant in the health effects, the inhalation lung dose was not emphasized in the Status Report. It is agreed that, for perspective and completeness, the inhalation lung dose should be included. Therefore, the detailed isotope contributors and total inhalation lung doses (30-day doses at 2.5 km) are presented in Table 1 for LOSP Branches BD and BF. These doses can be considered to have uncertainties similar to the inhalation bone doses (Table 4-13 of Volume VI).

Some clarification is needed with regard to health effects. The AIPA health effect calculations did not involve complex health models such as in WASH-1400 since the doses were smaller, and, in general, dose threshold

TABLE 1
INHALATION LUNG MEDIAN DOSE CONTRIBUTORS

LOSP Branch BD (Containment Isolation)			LOSP Branch BF (No Containment Isolation)		
Isotope	Dose (rem)	Percent Total	Isotope	Dose (rem)	Percent Total
Te-132	2.9×10^{-5}	29.3	Te-132	9.4×10^{-2}	26
I-131	1.8×10^{-5}	17.9	Cs-134	6.5×10^{-2}	18
Te-129m	1.6×10^{-5}	16.4	I-131	5.0×10^{-2}	14
Cs-134	1.2×10^{-5}	12.0	Te-129m	4.7×10^{-2}	13
Cs-137	5.5×10^{-6}	5.5	Cs-137	2.9×10^{-2}	8
Others	<u>1.9×10^{-5}</u>	<u>18.9</u>	Others	<u>7.6×10^{-2}</u>	<u>21</u>
Total	1.0×10^{-4}	100	Total	3.6×10^{-1}	100

bands for early morbidities and fatalities were not exceeded. For latent health effects, the calculations used the EPA factors listed in Table 3-5 of Volume VI. These factors are similar to those in Appendix VI of WASH-1400.

Comment 3

In Vol. VI, GAC refers to a "30-day bone dose" produced by internal emitters. It is not clear that this dose is: (a) that delivered to the bone from the inhaled radionuclides during the first 30-day following the accident, or (b) that delivered to the individual during his remaining lifetime from the radionuclides inhaled during the first 30 days following the accident. The first definition is correct only for radionuclides that have short effective half-lives (a few days) in the organ (either short radioactive half-lives or short biological half-lives). The second definition is correct for all radionuclides. GAC needs to clearly (and correctly) define what they mean by the 30-day dose to an organ from an internal emitter.

Response

Inhalation organ dose (bone, thyroid, etc.), as reported in the APIA Status Report, represents the total lifetime commitment dose delivered to an individual subsequent to either a 2-hr or 720-hr inhalation exposure. For the AIPA study, an essentially infinite (50-year) integration interval is used to represent the total lifetime commitment dose from inhaled, internally deposited radionuclides.

Comment 4

GAC based their criterion of the acceptability of the consequences of an accident (that is, 10^4 man-rem) on information obtained from the BEIR report (see Vol. 1, p. 9-6). However, they base their calculations of the number of health effects on an EPA report (see Vol. 6, Table 3-5). Since the BEIR report is the basis for practically all calculations of health effects (including the EPA report), why not base the health effect calculations on the BEIR report? It is certainly the more fundamental document.

Response

It should be made clear that no formal statement has been made in the Status Report about the acceptability of accident consequences. However, the study has informally treated as unquestionably acceptable those exposure levels that are, over a meaningful range of accident sequence frequencies, well below the threshold of acute fatalities and below the integer latent effect threshold of latent cancer facilities discussed in the response to the first comment.

The fundamental reference nature of the BEIR report is acknowledged. Future health effect evaluations will be based predominantly on BEIR committee findings. Additionally, the utilization of the latent health effect consequence methodology developed for the WASH-1400 LWR assessment study is being explored.

Comment 5

The PCRV release fractions are discussed in Volume 6. The release fractions are based on experiments done in the 1960's. To my knowledge, the results of these experiments have not been discussed in the open literature, but are discussed only in Appendix D of the Fort St. Vrain FSAR. The amount of data supporting the PCRV release fractions given in Table 4-10 is small.

The release fraction for iodine is given as 3.6×10^{-4} with an uncertainty of 100. It is of interest to compare this release value with the amount of iodine that escaped from the Windscale reactor during the core fire in 1958. By using an estimate of the core power level (a few hundred megawatts), one can estimate the iodine release to the environment to be about 10%. For this accident, the iodine that was released from the fuel elements traveled down the fuel channel, down a duct ~ 5 ft diam x 50 ft long, and up a 450 ft stack (ambient temperature). In spite of this long path and the corresponding large, cool surface area, approximately 10% of the iodine escaped. Even though the pressure, temperature, and flow rates are different in the Windscale reactor than in the HTGR during accidents, the differences between the Windscale release values and those suggested by GAC are large. The GAC value for iodine release needs further investigation.

Also, the effects of surface contamination on the PCRV release fractions have not been investigated in the GAC King-furnace experiments. Plateout of one isotope can affect the amount of plateout of another isotope.

Response

The results of the PCRV release fraction experiments have been discussed in PSC Quarterly Progress Reports (Ref. 2), as well as the Fort

St. Vrain FSAR (Ref. 3), all of which represent open literature information. These describe the King furnace and the one-twelfth scale model tests. Additional tests to simulate PCRV release are presently in the planning stage.

In European work, loop plateout experiments are being performed by KFA in Germany and CEA in France. In-pile plateout tests have been carried out since 1969 in the Saphir loop in the Pegase reactor at Cadarache. At the end of 1975, 12 different experiments were performed, of which 7 have been evaluated. Test durations were between 8 and 19 weeks with a total irradiation time of 14,000 hr. At Juelich, 18 different experiments have been performed since 1972 using the Vampyr I loop in cooperation with the AVR, and more extensive experiments are planned for 1977 using a new loop, Vampyr II. In addition to this in-pile loop, an out-of-pile loop named SMOC is being developed by KFA Juelich for investigations of plateout mechanisms and desorption experiments. Experiments in SMOC are intended to be correlated with a plateout computer code called "PATRAS," which has been used to correlate equilibrium plateout profiles measured in Vampyr experiments.

While it is interesting to compare the HTGR release estimate for iodine with that from the Windscale reactor accident, there is little physical basis to suggest that the accident conditions can be compared. The Windscale No. 1 pile was an air-cooled, graphite-moderated reactor which normally operated at 200°C or less. The presence of the water used to put out the fire plus the lower temperatures compared with the HTGR most probably resulted in the production of large quantities of volatile iodine compounds during the accident, especially methyl iodide, which would not be readily adsorbed on the stack or the particulate filter. Under HTGR conditions, very little formation of such penetrating iodine species would be expected. This is based on the iodine and methyl iodide injection experiments (Ref. 4) on the CO₂-cooled Windscale AGR, which showed rapid methyl iodide decomposition at hot duct temperatures of only 460°C, and on the Dragon tests (Ref. 5). In the Dragon tests, irradiated fuel was exposed to

high moisture (2420 to over 250,000 vpm) plus 27,000 vpm H_2 at 1250° to 1300°C with low measured formation of penetrating iodine species. This indicates that, at HTGR temperatures, little methyl iodide formation is expected as long as there is no large concurrent steam ingress.

Comment 6

Although some attention has been given to large scale hydrolysis in the AIPA work, no more than an estimate has been made of the influence of everyday tech spec levels of moisture in the primary coolant. It is stated that, "The second most important variable contributing to the noble gas activity uncertainty is the increase in R/B for failed TRISO particles due to hydrolysis. The magnitude of this effect was estimated; no detailed calculational methods are presently available which couple the effect of moisture transport in graphite to steady state fission gas release" (Vol. 5 p. 1-5). A rough hand calculation indicates that the noble gas release can be increased by 50% due to low level moisture in the primary coolant. Since noble gases are credited with causing the major off-site dose, a cursory treatment of this major influence on noble gas release seems unexplainable. The failure of GA to deal with hydrolysis in detail causes the noble gas source terms to be questionable.

Another point of irritation in this area, is that the data which was available and used for hydrolysis calculations is referenced as unpublished data.

Response

"Major offsite doses" are due to noble gases but only those initially in the fuel body and released during core heatup. Uncertainties in the initial gasborne activity in the primary coolant are important mainly due to their effect on the reheater leak consequences as shown in Table 6-6 of Volume VI of the Status Report. There, the gasborne activity is shown to be the fifth-ranking parameter in the safety R&D index, quantitatively far behind the leading parameters affecting core heatup. Therefore, a change, due to hydrolysis, in the initial gasborne inventory median value and/or uncertainty would not affect the HTGR risk envelope. Median consequences and/or uncertainties of the lesser risk events, reheater leak and slow depressurization, would not change significantly, as indicated below.

Calculations performed at an early stage in the study indicated, as the reviewer states, that fuel hydrolysis could increase the primary coolant inventory by as much as 50%. At that time, rigorous computer methods for calculating hydrolysis effects were under development but were not available. Consequently, a simple computer code was written and applied for treating hydrolysis within the scope of the Phase I effort. The basic equations are presented on page 4-6 of Volume V. Large uncertainties were applied which caused the hydrolysis uncertainty to be the second greatest uncertainty in the primary coolant inventory. Thus, the importance of hydrolysis was recognized, but the lack of rigorous methods caused the noble gas initial gasborne source terms to be "questionable" only in the sense of having large uncertainties relative to fuel body source terms. The overall uncertainty in the initial gasborne activity is less than a factor of 1.6, which is not large relative to some other parameter uncertainties.

Two computer programs will be available soon for more rigorous evaluation of coupled hydrolysis and moisture transport and reaction in graphite. The most rigorous thermochemical code is a steady-state version of OXIDE-3 (Ref. 6). This code simulates two-dimensional heat and mass transfer and chemical reactions in the fuel block fuel rod and graphite web geometry at various axial locations for various refueling regions. A specialized program which also includes R/B effects of hydrolysis is the HYDROBURN code, in which moisture diffusion and reaction are handled in one-dimensional geometry (coolant hole centered). This code can be used in a stand-alone mode, or it can be used as a subroutine of the SURVEY core performance code, use of which is discussed in Section 4 of AIPA Volume V. Both codes are intended to be employed in future work. Efforts are under way in the ERDA-funded HTGR Base Program to obtain further basic data on hydrolysis effects for use in computer calculations. These measurements include measurements of hydrolysis rates and hydrolysis effects on R/B for both unirradiated and irradiated kernels. The new data and data used in AIPA Phase I work are being published in Base Program Quarterly reports (e.g., see Ref. 7).

Comment 7

All of the plateout calculations should be questioned due to inadequacies in the data and shortcomings in the calculational techniques.

It is stated in section 8.4 of volume 5 (page 8-6) that the data is extrapolated, that data is not available for some materials of interest, and that the available data was taken under conditions which are not comparable to reactor conditions. If the data is poor, there is no way the results can be good.

The plateout sensitivities of ^{137}Cs and ^{90}Sr were calculated based on the GASSAR expected total plateout inventory (Vol. 5, pages 5-8 and 5-14). These sensitivities should have been based on the median total plateout inventories discussed in section 4 of volume 5. As it was done, the uncertainties were calculated around one set of conditions and were applied to another. Although incorrect, this approach could be accepted if the sensitivity curves were straight, however, the figures of section 5 of volume 5 demonstrate this not to be the case.

When calculating the total plateout inventory "all condensible fission products which are released are assumed to plate out." (Vol. 5, page 4-9). General Atomic states that this is an "added conservatism" in the calculation. Actually this reduces the circulating activity available for release during accidents which do not involve lift-off and is therefore not a conservative assumption. Only if GA's circulating and plateout activity code, RAD, does not contain a mass balance can their statement possibly be conservative.

Response

The reviewers have commented that ". . . all of the plateout calculations should be questioned due to inadequacies in the data and shortcomings in the calculational techniques . . ." This comment is

supported by only two explicit complaints: (1) the large extrapolation of sorption isotherms used, and (2) the calculation of Cs-137 and Sr-90 sensitivities about a base case other than the median. While these two expedients may not be rigorous, they do not invalidate the overall analysis to the extent that trends in the results and the relative ranking of dominant variables are significantly distorted. For that matter, the plateout distributions of La-140, Ce-144, Ag-110m, and Te-131m (Figs. 5-4 through 5-7 in Volume V) are not affected at all by these criticisms.

Some extrapolation of sorption data is unavoidable. Currently available experimental techniques (pseudo-isopiestic, Knudsen cell, etc.) impose a practical lower limit on attainable partial pressures of about 10^{-11} atm with data typically taken in the range of 10^{-10} to 10^{-6} atm. However, the partial pressure of key radionuclides (Cs, Sr, I) in the HTGR coolant would range from 10^{-15} to 10^{-12} atm and perhaps even lower. Some improvement in the reference data base, however, can be expected. Currently, there are some pertinent systems for which no data exist in the open literature [for example, for iodine on T22 (2-1/4% Cr, 1% Mo chromaloy steel) or for cesium on Incoloy 800, to name the two of greatest importance]; therefore, sorption data taken on related materials (1% Cr, 1/4% Mo chromaloy and SS-304 stainless, respectively) must be used. Efforts are presently under way to characterize the pertinent systems, but necessity of extrapolation to reactor conditions will persist.

While the extrapolation of the sorption isotherms may not be completely accurate, such an approach has been used with some success to predict the plateout distribution of cesium in the BMI deposition loop and the Dragon and Peach Bottom HTGRs (Ref. 8). Results of these analyses indicate that the reference isotherms at least describe the temperature dependence of the sorption process correctly, but that absolute sorptivities are predicted with mixed success. These observations give some credence to the reference methodology.

The problems with the sorptivity data discussed in Section 8 of Volume V and commented upon here are reflected in the large uncertainty assigned to the data. This uncertainty is, in turn, reflected in the large uncertainty calculated for the plateout distributions in Section 5 of Volume V. It was the intent of these calculations to identify those parameters contributing most to the uncertainty in the plateout distributions, thereby identifying those areas where more research would improve the calculations the most. The study in Volume V clearly shows that further sorption data for primary circuit components should be considered. Of course, an R&D recommendation on this subject would also have to include considerations presented in Volumes VI and IV.

It is true that the sensitivities of the plateout distributions of Cs-137 and Sr-90 were calculated around one set of conditions (GASSAR expected inventories) and were applied to another (median inventories from Section 4). This was acknowledged as a shortcoming in the analysis. However, to correct this condition would have required rather extensive computational efforts, at a substantial cost to the study. It was anticipated that such efforts would have so small an effect on the computed plateout results and, thus, would cause so small a change (if any) in the study results and conclusions that such additional expenditures could not be justified.

The base case for the Cs-137 sensitivity studies used GASSAR expected inventories rather than AIPA median ones; i.e., the total circuit inventory was taken as 16,000 Ci of Cs-137, which is a factor of about 2.7 lower than the median inventory. The attendant effect on the Cs-137 distribution is seen in Fig. 5-1. The LASL reviewers commented that ". . . this approach could be accepted if the sensitivity curves were straight, however, the figures of Section 5 of Volume V demonstrate this not to be the case . . ."

The sensitivity curves are, in general, not straight for large perturbations from the base case. However, it must be recalled that these are normalized change plots (Section 5.1, page 5-3), and it is contended

that normalized sensitivity curves for perturbations about a base case slightly displaced from the original base case would not be markedly different. The rationale for this contention is as follows. From Fig. 5-8, it is seen that the Xe-137 precursor has little effect on the Cs-137 distribution, which is completely dominated by the directly released component in the expected case, and even more so in the median case. Now the median Cs-137 distribution (Fig. 5-1) could be drawn on Fig. 5-10, taken as the base case, and new sensitivity curves generated. From careful inspection, and some inductive reasoning, it is apparent that the new normalized sensitivity curves would not be markedly different from those in Fig. 5-11. Further induction reveals that new versions of Figs. 5-12, 5-14, and 5-16 would also yield normalized sensitivity curves not unlike the original ones. While all this is clearly not rigorous, the implication is that the errors are not large. In fact, it is contended that errors are probably much smaller, for example, than those associated with the somewhat arbitrary choices of sigmas for the gaseous diffusivity and graphite and metal sorptivities.

For Sr-90, the problem is considered more serious than for Cs-137. The difficulty with Sr-90 arises not because the median inventory is much less than the expected inventory (346 Ci versus 4100 Ci) but rather because the relative contribution from the gaseous Kr-90 precursor to the total Sr-90 is higher in the median case than in the expected case (26% and 5% of the total, respectively). Since the plateout distributions of Sr-90 from the Kr precursor and the directly released Sr-90 are different, the shape of the composite distribution is dependent on the relative contributions.

The qualitative consequences to the sensitivity calculations are as follows. From inspection of Fig. 5-18, it is seen that, in the expected case, the specific activity in the economizer section of the steam generator and downstream is dominated by the gaseous Kr-90 precursor component, while the specific activity upstream of the economizer is controlled by the directly released component. In the median case, this transition point would be displaced further upstream; consequently, the

effect of changes in the gaseous component have been underestimated for some sections of the steam generator, but those sections downstream are unaffected, as are the lower reflectors.

The effects of changes in the directly released component for the expected base case are shown in Fig. 5-20. The effects are most pronounced upstream of the circulators. Using the median base case, the affected zone would be displaced somewhat further upstream, but in the affected zone, the magnitude of the displacements from the base case and, hence, the slopes of the sensitivity curves would be unchanged.

Although not obvious from Fig. 5-22, changes in the gaseous diffusivity have slightly larger effects on the directly released component than on the gaseous precursor component. Hence, the slopes of the sensitivity curves in the expected base case are generally larger than they would be in the median base case.

From Fig. 5-24, changes in the graphite sorptivity affect the specific activity of Sr-90 only in the lower reflector blocks. Since the activity here is dominated by the directly released component in both the expected and median base cases, the normalized sensitivity curves would not be affected.

From the above, it is seen that use of the expected base case rather than the median base case can result in either over or under prediction of the slopes of the sensitivity curves. However, from Table 5-2, the variance in Sr-90 is generally dominated by contributions from the metallic release and the gaseous diffusivity uncertainties. Hence, the net effect of having used GASSAR expected rather than median values as the base case for the Sr-90 sensitivity studies was to overestimate the total variance for some location in the primary circuit. It is believed that the magnitude of the error is a factor of 3 or less and that the effect is confined to the steam generator.

With regard to plateout assumptions in the RAD code, it is indeed the case that RAD does not contain a mass balance. This is explicitly pointed out on page 12 of Ref. 5. For calculations of plateout activity, the RAD code conservatively assumes that all condensable fission product species released from the core are plated out. For calculations of circulating activity, a plateout removal rate constant is calculated based on a functional plateout per pass τ , the value of which is taken as $0 \leq \tau < 1$.

Comment 8

The gaseous circulating activity source terms were calculated at mid-year because they adequately approximate the annual averaged release calculations (Vol. 5, page 4-4). This is a true statement, but inaccurate for the circumstances being considered. One is not interested in doses calculated from annually averaged source terms, but with doses calculated with peak source terms. It can be shown that the end of year source terms are 20% higher than the mid-year source terms. This implies that the reported doses are only conservative for accidents occurring in the first half of a year.

Since RAD is an integral code using core average operating information the assumption that the release fraction is constant with burnup (Vol. 5, page 4-3) is incorrect and not conservative. The release fraction increases by ~40% during an annual burnup cycle. This increase is reflected directly in the release to the primary coolant.

Response

As indicated in Section 1 of Volume I of the Status Report, the AIPA study was, like WASH-1400, striving for a realistic risk assessment based on realistic assessments of probability and consequence. This was achieved, whenever possible, by using best estimates of median values of input data, thereby obtaining median values of fission product activities for use in subsequent AIPA dose calculations. The uncertainties associated with median values of input data were then used to determine the uncertainties associated with the calculated median releases.

For licensing and safety analysis purposes, doses are calculated with the design source term criteria contained in GASSAR. The results of AIPA Volumes V and VI show that a high confidence level is assigned to the releases and dose rates obtained in GASSAR.

Comment 9

It is most interesting that GA did not include thermal diffusion effects in their nominal calculations, but included them in their variance calculations (Vol. 5, page 3-3). This becomes more interesting if one notes that the thermal diffusion uncertainty ranks third in contribution to the 90-Sr release uncertainty (Vol. 5, page 3-51). It is hard to understand why a parameter which influences the uncertainty to such a degree does not merit being included in the nominal release calculation. If this parameter has such an influence on the uncertainty it is bound to have an influence on the median value. Mathematically this approach also seems to be very weak.

Response

A review of the literature on thermal diffusion in solid-state systems indicates that the heat of transport associated with this process can assume a range of values. In some systems, negative heats of transport are found (diffusion occurring up the thermal gradient), while in others, positive heats of transport are found (diffusion down the thermal gradient).

Accordingly, in the calculation of the median values of metallic fission product release, the median value of the heat of transport was taken to be zero, and thermal diffusion effects were treated as a systematic uncertainty contributing to the uncertainty in release. This is entirely appropriate, since no experimental evidence exists that thermal diffusion is a significant factor for the transport of metallic fission products in graphite.

It should be noted that, although thermal diffusion uncertainty ranks third in contribution to the Sr-90 release uncertainty, if the uncertainty in thermal diffusion were entirely eliminated, the variance in the release would be reduced by only 6%.

REFERENCES

1. Buckley, D. W., "TDAC, An Analytical Computer Program to Calculate the Time-Dependent Radiological Effects of Radionuclide Release," General Atomic unpublished data, May 1976.
2. "PSC Quarterly Progress Report for Period Ending September 30, 1967," USAEC Report GA-8276, General Dynamics, General Atomic Division, October 30, 1967. See also GA-8600, GA-8038, and GA-7939 in the same series.
3. Public Service Company of Colorado, "Fort St. Vrain Nuclear Generating Station Final Safety Analysis Report," 1967 (USAEC Docket 50-267).
4. Hillary, J. J., and J. C. Taylor, "Behavior of Methyl Iodide in the Windscale AGR," J. Brit. Nucl. Energy Soc. 14, 159 (1975).
5. Shepherd, J., et al., "Corrosion Experiments on Irradiated Dragon Fuel Compacts," Dragon Project Report DP-833, 1973.
6. Perroomian, M. B., A. W. Barsell, and J. C. Saeger, "OXIDE-3: A Computer Code for Analysis of HTGR Steam and Air Ingress Accidents," General Atomic Report GA-A12493 (GA-LTR-7), January 15, 1974.
7. "HTGR Fuels and Core Development Program Quarterly Progress Report for the Period Ending February 28, 1975," General Atomic Report GA-A13353, March 31, 1975.
8. Hanson, D. L., "Results of the General Atomic Deposition Loop Program," ERDA Report GA-A13140, General Atomic Company, April 1, 1976.
9. Haire, M. J., and D. W. McEachern, "Gaseous Radioactivity Levels in the Primary Coolant of an HTGR," USAEC Report GA-A12946 (GA-LTR-14), General Atomic Company, October 1, 1974.

3.4. COMMENTS FROM ATTACHMENT 4 OF REF. 1 ON P. 3-1

Comments

With regard to the evaluation of probabilities, I had two areas of concern. First, have any assumptions been made about the design of the balance of plant that will have a major impact on the final result if that design characteristic was changed? Second, have probabilities been based on operating experience for components that are really comparable in terms of operating conditions, maintenance intervals, and inspectability? A major point of concern involves the steam generators since they separate the primary system coolant from the steam system that is outside of the containment; I would question the applicability of performance data for this component in previous gas-cooled reactor designs to the commercial HTGR design.

Going on to the question of whether events of maximum risk point the way to the major requirements for R&D, I would agree with Dr. Sastre's comment that careful attention should be given to the possible improvement in risk evaluation or risk reduction from the work. Some areas of uncertainty may never be definable, or it may always be desirable to make the "worst possible" evaluation in safety analysis. It appears reasonable that a better definition of release rates is both a desirable and reasonable area for work. Plate-out and lift-off studies have questionable importance unless it can be proved that plate-out is a dependable mechanism for the reduction of activity in the primary loop. One point that is not made is the evaluation of fuel performance and therefore release rates on irradiation.

The goal indicated for the R&D appears to mix two objectives, although each may serve the other. One objective is to reduce the maximum risk to the public that might be encountered in plant operation; the other is to better define the risk so that licensing uncertainties would be eliminated and plant costs reduced. Both of these might be acceptable objectives for the ERDA-sponsored study.

Response

With regard to the first concern about the evaluation of probabilities, the design for the balance of plant (BOP) was the reference design of the summer of 1975 for the detailed Phase I evaluations in the study. A small number of characteristics of the reference design had changed between the preliminary analyses and the detailed analyses, and these differences can be pointed out in the results; however, the differences do not have a major impact on the final results. There is no known design of a component that would have a major impact on the final results if it were changed in a moderate way so as to still satisfy the requirements on the component. However, major changes in design requirements for systems in the plant could change the risk assessment. Such changes were examined in Section 5 of Volume III in the study of design options.

Regarding the second concern about probabilities, the probabilities (and consequences) dependent on equipment exclusive to gas-cooled reactors were based on operating information from these reactors where available. In cases where the equipment application in the HTGR is significantly different from other gas-cooled reactors, larger uncertainties were employed. Some common components in the HTGR are similar in operating conditions and environments to fossil-fired power plants in the U.S., and, thus, a considerable amount of this operating power plant reliability data was applicable to the HTGR. The Peach Bottom I experience which is most useful to the AIPA studies involves material performance in high-temperature helium and the behavior of graphite fuel. Some data in these areas were available for the AIPA Status Report in both the probabilistic and consequence evaluations. Only the limited experience of construction and subsystem testing at Fort St. Vrain was available for use in the Status Report. In a majority of the analyses involving the release of core and primary coolant activity, it was assumed that the reactor is operating at 100% power when the initiating event occurs; therefore, FSV experience was of rather limited value. As FSV gains operating experience on components similar to those in the HTGR, these data can be useful in continuing AIPA studies. Foreign gas-cooled reactor experience was utilized where the differences in design

configuration and coolant type had little effect on the operating performance characteristics. Some specific examples where foreign and U. S. gas-cooled reactor operating experience was utilized in the analysis include the following:

1. Steam generator tube leak probabilities
2. High-temperature steam graphite reaction parameters
3. Parameters characterizing fission product behavior in graphite
4. PCRV experience
5. Main turbine generator load rejection reliability
6. Relief valve closure reliability
7. Component failure modes and initiating event sequence responses

Data available in these cases show that there are variations in the failure rates, and many of these variations appear to be random. This is accounted for in the analysis by including uncertainty factors in the evaluation of the failure rates so that the reviewer can see the effect of the random aspects of the input data. In the specific case of the reheater, both large and small leaks were studied since both can release radioactivity to the environment. In the case of the steam generators, a broad spectrum of leaks in the economizer-evaporator-superheater was considered in scoping the problem initially, but only large leaks of about 1 sq in. in flow area were included in the preliminary and detailed analyses. Such leaks are rare but do have the potential for releasing radioactivity to the public. (The very small leaks which are customarily reported in connection with nuclear steam generators were not studied in detail because the likelihood of releasing radioactivity to the public is exceptionally small.)

It is proper to question the applicability of experience data for steam generators involving very large leaks of about 1 sq in. of flow area. The only leak known of this general size occurred in the United Kingdom, and that experience was used as a basis for the frequency estimate. It is true that these data may not be directly applicable because the British steam generators of those generations were constructed by means of field

welding wherein it was not possible to utilize the controls which have subsequently been developed for steam generator fabrication in the United States. Therefore, the United Kingdom experience would be expected to be conservative (pessimistic) when applied to future United States practice.

It is agreed that careful attention should be given to the possible improvement of risk evaluation, and, in fact, this work is continuing. Attention will also be given to the question of risk reduction in the course of comparing HTGR risk with that of other accepted nuclear power plants. Whether uncertainty is definable is a matter of degree. To clarify the degree of definition that is required, the risk analyses need to be examined to determine how great a specific uncertainty may be allowed to be before it has an undesirable impact on the maximum risk of the plant. The definition of a "worst possible" safety analysis is subjective and does not have a clear and strong connection to what may actually happen. WASH-740 is one example of a "worst possible" analysis; in the future, a meteorite strike on a nuclear power plant could presumably be chosen as a "worst possible" analysis. The reason that these "worst possible" analyses do not have much credence is that they are not very probable, and the probabilistic risk analysis used in the AIPA study contributes toward a quantification and comparison of the probabilities for a large number of accidents, including many that are beyond the customary "worst possible" that might be chosen for analysis.

It has appeared in the past that a better definition of release rate was both desirable and reasonable. Further consideration of this question could well be based on the release values indicated in the AIPA report, the uncertainties in those values, and the impact that they have on the predictions made regarding the doses that might be received by the public in an accident. Such explicit examination of the question would also be suitable regarding the importance of plateout and liftoff studies, and the clarification of the degree to which they influence public safety.

The goals indicated for safety research and development for the HTGR are best expressed in the AIPA Status Report in Volume 1, page 8-2, where criteria for selecting R&D are presented. For accidents that appear to be of potential key public safety concern, NRA funding support should be provided for design options that would reduce the likelihood and/or the potential consequences, and that can be practically implemented. Another objective for accidents having a small enough risk is to evaluate the risk sensitivity in various safety-related plant options; this might lead to reductions in plant costs. It is believed that these are acceptable objectives for the ERDA-sponsored study.

3.5. COMMENTS FROM ATTACHMENT 5 OF REF. 1 ON P. 3-1

A. General Comments

Comment A-1

We recognize that the ERDA sponsored Accident Initiation and Progression Analysis (AIPA) study using the approach pioneered in WASH-1400 provides a disciplined and organized approach to the overall enlightenment of the factors affecting HTGR safety. The more quantitative benefits of the technique are also useful; such as the potential for a summary comparison of HTGR and LWR safety and risk, or the identification and priority assessment of R&D topics.

Response

It is gratifying that recognition is being given to the benefits to be derived from probabilistic risk assessment methods with respect to the disciplined and organized approach to safety evaluation. The event tree format adopted in the AIPA study has proved useful in displaying alternative system responses to potential initiating events in terms of varying degrees of equipment success or failure and of operator responses. It has also served as a guide in the systematic quantitative evaluation of branch probabilities and radiological consequences. Such a systematic approach, it would seem, could be utilized in routine NRC safety evaluations to show whether Staff concerns or resources, as well as those of applicants, were optimally deployed -- that is, whether we are making "mountains out of molehills," or vice versa. It appears that the recommendations of the ACRS, as expressed in their July 14, 1976, letter to L. V. Gossick, are along this same line.

Also recognized are the potential quantitative benefits of the probabilistic risk assessment methodology in such applications as a summary comparison of HTGR risk versus that of LWRs. Such a comparison has been

made outside of the scope of the AIPA Status Report and has revealed that HTGRs compare favorably with LWRs. In fact, all event tree branch points that form the risk envelope for HTGRs on a probability-consequence plot fall within (that is, on the acceptable side of) the corresponding risk envelope as established from WASH-1400 for LWRs, with some of the important HTGR points being significantly within this LWR envelope (Refs. 1, 2).

Comment A-2

On balance, we believe that the current benefits of the study, with respect to the potential guidance it offers to design efforts and to R&D planning for HTGRs, outweigh the difficulties in performing the study and the criticism that the study may be "premature".

Response

The AIPA study team agrees that the study is not "premature." Whereas the Rasmussen study was performed "after the fact" to see if LWRs are "safe enough," the purpose of the AIPA study was, as the comment states, to guide the design and R&D efforts so as to provide safety knowledge and enable the optimization of safety systems "before the fact." It is believed that such safety studies should be performed at an early stage for any emerging reactor type.

Even though reliability statistics for specific HTGR components were not available in some instances, it was found that data for similar components could be employed in the AIPA study. One of the benefits of the probabilistic risk assessment approach is that it shows whether such approximations in assessing the reliability of specific equipment (with allowances for uncertainties) are significant enough to warrant a more detailed reliability evaluation. Application of the methodology can also be an ongoing process in which better reliability data can be "plugged in" as they become available for actual HTGR components or for equipment deemed more representative of HTGR components.

Comment A-3

For reactor safety analysis purposes (and probably most other purposes) the overall usefulness of the study is dependent on its credibility both within the HTGR technical community and, eventually, with the public at large. We believe that satisfactory achievement of a reasonable credibility goal should be a principal objective of the study with care taken to assure that the rationale for the assumptions, methodology, and data base employed are fully defensible. This does not mean, of course, that all assumptions must meet the test of being indisputably conservative, or that the methodology can not differ from WASH-1400 in those details where needs exist to reflect the special aspects of the HTGR. Rather, the study must demonstrate that it is objective and must use the available data base and methodology in a cautious and reasonable manner.

Response

The need for independent reviews of the AIPA study has been recognized, and any serious, constructive comments on it have been welcomed. Errors that are identified by such reviews will be corrected. Opinions or judgments offered in writing will be carefully reviewed and, if deemed appropriate, actions will be taken. To this end, a formal review and project monitoring process has been included in the program: ORNL was requested by ERDA to review the consequence models, and the ERDA-funded participation by Aerojet Nuclear Company has resulted in a detailed review of the probability aspects of the study and of the identification of initiating events. Also, Leslie Cave, an English-based consultant with a broad background in the risk assessment field, was hired (with the concurrence of ERDA) to perform periodic reviews during the course of the study.

To obtain a broad range of viewpoints on the methodology, assumptions, data, results, conclusions, and recommendations of the study, comments have been solicited from many other sources. The Status Report was broadly distributed via UC-77. Complementary copies of the report, with a request

for comments, were sent to Dr. Norman Rasmussen of MIT and Dr. David Okrent of UCLA, and presentations were given to them, their staffs, and their students to better inform them about the overall study. Report copies were also sent to knowledgeable individuals in other organizations, including those who more recently evaluated the potential of the HTGR technology -- namely, A. D. Little, United Engineers, and Associated Nuclear Services of Great Britain. Similarly, the NRC Staff and its contractors were requested to comment, and, indeed, this very response is a result of the NRC review. Several foreign organizations have commented on the report, including KFA/ISF, HRB, and Bonnenberg and Drescher. In addition, negotiations are in progress for a review of the probabilistic aspects of the study by SRS of the UKAEA.

In addition to the above, several papers have been written for the general technical community (Refs. 1, 3-6). One very important aspect of the methodology developed for AIPA is the quantitative treatment of common mode failures (Refs. 4, 5). There has been widespread favorable comment on the methodology. With respect to independent evaluation, it should be noted that the AIPA beta-factor approach for common mode failures can be verified by the methods described in Ref. 7. Furthermore, the AIPA equations for common mode probability analysis (Volume II) have been reproduced by application of the Markov chain approach. Therefore, the AIPA study team concurs with the first part of this comment, and activities are under way to achieve general acceptance of the validity and credibility of the AIPA study.

The second part of this comment implies that the assumptions and data base may not be conservative or may not have been applied in a cautious and reasonable manner. Several comments are appropriate in response to such concerns. First of all, the AIPA (as well as WASH-1400) study objectives were to deal with realistic estimates of risk, rather than the conservative assessments that are made in conjunction with current practices in licensing and siting evaluations. However, in Volume VI of the AIPA report, the

uncertainties associated with doses are combined statistically for the reheater leak, core heatup (comparable to the MHFPR), steam ingress, and depressurization events, and the upper bound consequences are compared with Fulton PSAR doses. It should be noted that the upper 95% limits for 2-hr and 30-day whole body doses for the core heatup sequence slightly exceed those for the Fulton MHFPR, which incorporates licensing-basis conservatism. Second, the treatment of common mode failures in the AIPA study is one of the most significant differences from WASH-1400 methodology, is fully defensible, and, as noted above, is independently verifiable. Third, a comparison of the AIPA and WASH-1400 consequence codes has been initiated. An analysis of selected WASH-1400 accident sequences and hypothetical HTGR source terms has been completed. The results were transmitted to NRC/RSR on July 26, 1976, for comparison with results obtained by the WASH-1400 consequence code. Fourth, the AIPA study based the consequence assessments on the widely used nuclide effectivities of ICRP-2, thereby making the AIPA effectivities in reasonable agreement with such other applications and guides as WASH-1535, Rempersigh, and NRC Regulatory Guide 1.109 (preliminary). However, the nuclide effectivities used in WASH-1400 are the values proposed by the International Commission on Radiological Protection (ICRP) Task Group on Lung Dynamics (TGLD) back in 1966. Table 1 provides a basis for comparing effectivities and indicates that, for the nuclides of greatest importance, the TGLD values used in WASH-1400 are generally the lowest -- ranging to nearly two orders of magnitude lower in one case. Finally, it should be realized that the AIPA study may not be complete and is being conducted in a "cautious and reasonable manner." Following the initial phase of risk assessment based on median probabilities and median consequences, a second detailed phase applied uncertainty bands to probabilities and consequences. Analysis and development of methodology and data base will be continued as necessary until an acceptable level of completeness is achieved.

TABLE 1
EFFECTIVITY SOURCE IDENTIFICATION
(in rem/Ci)

Nuclide	TGLD Inhalation Model WASH-1400	ICRP-2 Inhalation Model			
	AIPA	NRC Reg. Guide 1.109	WASH-1535 ^(a)	Rempersigh	
<u>Lung</u>					
Sr-89	7.8 X 10 ³	1.8 X 10 ⁵	1.8 X 10 ⁵	1.8 X 10 ⁵	1.8 X 10 ⁵
Sr-90	1.8 X 10 ⁴	1.2 X 10 ⁶	1.2 X 10 ⁶	1.2 X 10 ⁶	1.2 X 10 ⁶
I-131	2.4 X 10 ³	2.1 X 10 ⁴	0	2.1 X 10 ⁴	2.1 X 10 ⁴
I-133	3.1 X 10 ³	5.1 X 10 ³	0	5.1 X 10 ³	--
Cs-134	5.1 X 10 ⁴	5.1 X 10 ⁵	1.2 X 10 ⁴	5.5 X 10 ⁵	5.5 X 10 ⁵
Cs-137	4.0 X 10 ⁴	4.5 X 10 ⁵	9.4 X 10 ³	4.5 X 10 ⁵	4.4 X 10 ⁵
Pu-238	3.1 X 10 ⁸	1.9 X 10 ⁸	1.8 X 10 ⁸	1.9 X 10 ⁸	1.9 X 10 ⁸
Pu-239	2.0 X 10 ⁸	1.8 X 10 ⁸	1.7 X 10 ⁸	1.8 X 10 ⁸	1.8 X 10 ⁸
<u>Thyroid</u>					
Sr-89	1.2 X 10 ³	0	0	0	0
Sr-90	2.8 X 10 ³	0	0	0	0
I-131	1.1 X 10 ⁶	1.49 X 10 ⁶	1.49 X 10 ⁶	1.44 X 10 ⁶	1.47 X 10 ⁶
I-133	1.8 X 10 ⁵	4.0 X 10 ⁵	3.7 X 10 ⁵	3.9 X 10 ⁵	--
Cs-134	1.4 X 10 ⁴	0	0	0	0
Cs-137	9.5 X 10 ³	0	0	0	0
Pu-238	6.0 X 10 ²	0	0	0	0
Pu-239	5.6 X 10 ²	0	0	0	0
<u>Bone</u>					
Sr-89	3.0 X 10 ⁴	4.2 X 10 ⁵	3.8 X 10 ⁴	4.1 X 10 ⁵	4.1 X 10 ⁵
Sr-90	2.8 X 10 ⁶	3.9 X 10 ⁷	1.2 X 10 ⁷	1.1 X 10 ⁷	1.1 X 10 ⁷
I-131	2.1 X 10 ²	0	3.2 X 10 ³	0	0
I-133	9.2 X 10 ¹	0	1.1 X 10 ²	0	--
Cs-134	4.7 X 10 ⁴	3.0 X 10 ⁴	4.7 X 10 ⁴	3.7 X 10 ⁴	3.7 X 10 ⁴
Cs-137	3.6 X 10 ⁴	6.1 X 10 ⁴	6.0 X 10 ⁴	6.1 X 10 ⁴	6.1 X 10 ⁴
Pu-238	7.6 X 10 ⁸	5.7 X 10 ⁹	2.7 X 10 ⁹	5.7 X 10 ⁹	5.7 X 10 ⁹
Pu-239	8.7 X 10 ⁸	6.6 X 10 ⁹	3.1 X 10 ⁹	6.6 X 10 ⁹	6.6 X 10 ⁹

(a) WASH-1535, Fundamental Statement-LMFBR.

Comment A-4

As HTGRs are in an early state of development and operational history, the quantitative conclusions drawn should be presented in a more tentative manner and a caveat offered that subordinates the conclusions to the development of further knowledge and experience.

Response

First, it should be pointed out that the lack of large HTGR operational history did not significantly hinder the study. Although reliability data for some HTGR components were not available, generic data for valves, pumps, heat exchangers, steam system components, etc., were plentiful, and much care was taken to construct a reliability data bank for related and representative equipment. Through the use of the risk assessment methodology, it is possible to identify the equipment whose reliability is significant and identify where better data are required for more accurate risk assessment. It is difficult to assess probabilities associated with structural failures, and, in this respect, HTGRs are not different from LWRs.

Second, the "caveats" have been directly stated or implied throughout the AIPA Status Report:

1. The title of the report includes the phrase "Status Report" and not "Final Report."
2. On page 1-2 of the "Introduction" to Volume I, it is stated, "... this report ... may not have achieved a complete risk assessment ... analysis must be continued to achieve greater completeness. Further development of methodology and data base are also important ..."

3. In the "Summary and Conclusions" of Volume I, page 9-3, it is stated, "Final conclusions about the absolute and relative nuclear safety of the HTGR will be reached with care. When using the results of this study to consider such a conclusion, it should be noted that uncertainties in present results are, in many cases, substantial; in addition, the study so far has been based on a representative, but not all-inclusive, list of initiating events." It is further stated that the results provide a reasonable and strong indication that HTGRs are exceptionally safe under the many conditions covered in the study. However, it is felt that the "caveats" are needed only to qualify conclusions related to the absolute safety of HTGRs.
4. In the title and "Introduction" of Volume IV, "Phase I Analyses and R&D Recommendations," the phrase "Phase I" is used, emphasizing that more work remains to be done.

Thus, it is seen that the reader is warned sufficiently about the evolving nature of the study and conclusions. This is not meant to imply, however, that the conclusions are invalid or to be ignored -- they are to be used to define a current set of safety-related R&D programs. Results of such recommended R&D, as well as results of equipment developmental tests and results of Fort St. Vrain operational experience, can be factored into future risk analyses to produce an advanced set of R&D recommendations. Thus, the study can proceed in a somewhat iterative manner, shifting R&D emphasis according to the best risk analysis available to accommodate changes in the design or in the policy regarding the consequence and/or risk criteria.

Comment A-5

A general observation that applies to many facets of the study is that the available data base, limited as it is, is not as effectively used as it might be. We find very little formal reference made to Fort St. Vrain and Peach Bottom I experience, and to the substantial available foreign experience (e.g., Dragon, AVR, AGR Program).

Response

It is agreed that there is little formal reference to the experience from Fort St. Vrain, Peach Bottom, and other gas reactors. However, the probabilities and consequences dependent on equipment exclusive to gas-cooled reactors were based on operating information from these reactors where available. In cases where the equipment application in the HTGR is significantly different from other gas-cooled reactors, larger uncertainties were employed. Some common components in the HTGR are similar in operating conditions and environments to fossil-fired power plants in the U.S., and, thus, a considerable amount of this operating power plant reliability data was applicable to the HTGR. Other components in the HTGR, however, are exclusive to gas-cooled reactors, and application of information from the operating gas-cooled reactors to these components in the AIPA study are discussed below.

The Peach Bottom I experience which is most useful to the AIPA studies involves material performance in high-temperature helium and the behavior of graphite fuel. Some data in these areas were available for the AIPA Status Report in both the probabilistic and consequence evaluations. It was found that the operating record includes very little in the way of GCR component failures. A program is under way to obtain additional information during the Peach Bottom I decommissioning activities. It is expected that these data will be valuable in reducing some of the uncertainties in fission product release and plateout codes and the behavior of fission products and materials under operating conditions.

During the AIPA Status Report evaluations, only the limited experience of construction and subsystem testing at Fort St. Vrain was available. In a majority of the analyses involving the release of core and primary coolant activity, it was assumed that the reactor is operating at 100% power when the initiating event occurs; therefore, FSV experience was of rather limited value. As FSV gains operating experience on components similar to those in the HTGR, the data can be useful in continuing AIPA studies.

Foreign gas-cooled experience was utilized where the differences in design configuration and coolant type had little effect on the operating performance characteristics. These data were also useful in both the probability and consequence evaluation areas.

Some specific examples where foreign and U.S. gas-cooled reactor operating experience was utilized in the analysis include the following:

1. Steam generator tube leak failure rates
2. High-temperature steam/graphite reaction parameters
3. Parameters characterizing fission product behavior in graphite
4. PCRV experience
5. Main turbine generator load rejection reliability
6. Relief valve closure reliability
7. Component failure modes and initiating event sequence responses.

It is agreed that additional data from FSV, Peach Bottom, and foreign experience would be useful in continued AIPA evaluations. The U.S. nuclear operating reliability data are becoming available through NPRDS, and as soon as FSV becomes operational its experience will also be included. Data

from foreign experience are not as easily obtainable as from NPRDS, abnormal occurrence reports, operating status reports, etc., available in the U.S. However, foreign contacts are being maintained through a GCR data bank, e.g., membership in Systems Reliability Services in Great Britain, to obtain data relevant to GCRs. Such data also supplement U.S. information on the components common to both GCRs and other reactor types.

In the AIPA study, the limited gas-cooled data were used, where possible, supplemented with the more extensive information from fossil and other nuclear-fueled central power station data. An attempt was made to effectively use this information in developing R&D recommendations and, when new information becomes available, reductions in the data uncertainty ranges or adjustments to the probabilities can be made, if substantive.

In conjunction with the discussion of data needs, it should be pointed out that, for risk assessment purposes, it is frequently unnecessary to obtain information at the same level of detail as required for many reliability analyses. This situation exists because of differences in analysis scope and objectives. Reliability analyses are frequently performed on subsystems as a part of a design review and improvement process or as a check on compliance with applicable codes, standards, and regulations. In such cases it usually becomes necessary to perform the analyses from the subsystem level down to the "nuts and bolts" level, with the reliability block diagrams, failure modes and effects analyses, checklists, and other such analysis tools reflecting these levels of analytical detail. On the other hand, the probabilistic risk assessment used in AIPA was performed with at least the same relative degree of detail, but in a different way. It began at the highest level of assembly (the complete plant) and extended down through systems and subsystems. While less emphasis tended to be placed on the "nuts and bolts" details within a subsystem (an area having a small impact on risk assessment at the plant level), attention was focused primarily on system interfaces and the evaluation of common mode failure characteristics -- that is, areas that have a primary impact on the evaluation of plant risk. Information developed during the course of the AIPA

program tends to confirm the above statements. For example, for the LOSP-BD sequence of events, the preliminary analysis yielded an annual occurrence rate of about 1×10^{-7} . The more detailed Phase I analysis gave a value of about 1×10^{-6} , and the additional detail of subsequent (Phase II) analyses yields a current estimate of about 3×10^{-7} . Thus, adding more and more detail at successive analysis stages has had a relatively minor impact on plate-level probability estimates. This tends to indicate the lesser need to rely on the "nuts and bolts" level of information detail that is required in many reliability analyses.

Comment A-6

The study dismisses as insignificant several important concerns of the NRC and ACRS brought forward in the Summit and Fulton reviews. On the other hand some of the basic assumptions influencing the course and results of the study appear to reflect the GAC positions taken during the Summit and Fulton construction permit safety reviews.

Response

In an attempt to understand this comment, the Safety Evaluation Reports and ACRS letters for the Summit and Fulton applications were reviewed for outstanding issues and contrasted to the AIPA study.

With respect to Summit, for example, the following lists of regulatory concerns were reviewed:

1. Section 1.6 of the staff's SER lists 26 issues which resulted in facility design modification as a result of NRC review.
2. Section 1.7 of the SER lists requirements for future technical information including an R&D program and seven hardware items listed as prototypical, subject to generic evaluations.
3. Section 18.0 of the SER supplement lists 11 items identified by the ACRS as requiring resolution or staff follow-up (the eleventh being performance data from Fort St. Vrain).
4. The ACRS letter of March 12, 1975, reiterates most of the same concerns plus others identified by the staff.

Each item was considered for its relationship to the AIPA study. In general, none of the items were addressed directly in the AIPA treatment so it is difficult to determine where an item was "dismissed" or where "basic assumptions" may be contrasted. Our examination of these lists indicated that perhaps the many items could be classified into four broad categories: (1) design adequacy of safety-related components, (2) instrumentation [which is really a special case of (1)], (3) external influences, and (4) plant operations. Those categories of issues are further subdivided in Table 2.

Except for earthquakes, the AIPA study does not deal with category (3). However, considerations of the other categories are implicit in the risk assessment inasmuch as the levels of component reliability and performance employed are based on practices dictated by regulations, industry standards, and prudent operation. There is no basic conflict between AIPA and regulatory bodies regarding the need to verify and ensure adequacy and reliability of safety-related and other equipment. However, risk assessment philosophy and results might lead to conflict between the arbitrary imposition of items from categories (1), (2), and (4) by regulatory bodies which do not actually reduce risk to the public.

For example, certain of the items related to very specific calculational assumptions such as containment backpressure during CACS operation or the heat transfer coefficient employed for the CAHE. Such details are of minor importance in the overall risk assessment. On the other hand, one area that comes to mind where assumptions differ markedly from those used by the staff in the Summit and Fulton SERs is that of the dynamics of fission product release during the unrestricted core heatup sequence, which is related to the licensing siting event. As appropriate for AIPA, mechanistic median values of parameters were employed as contrasted to the staff's conservative site evaluation.

The following information is provided in an attempt to clarify certain points of the AIPA study which may have been misunderstood.

In the course of the preliminary AIPA study, all possible sources of radioactivity in the plant were considered, and 17 representative initiating events were chosen for evaluation. It is acknowledged that the 17 initiating events may not be totally comprehensive. Of these 17 events, many were included in the licensing reviews (SARs) for Summit and Fulton. The most notable among these were the design basis events known as rapid depressurization of the PCRV (DBDA) and the moisture ingress event due to steam generator tube rupture. Even though many event sequences included multiple failures, which generally would worsen consequences, the probabilistic risk assessment showed that the DBDA and moisture ingress events are not dominant contributors to the risk associated with the HTGR and were not worthy of further study in the Phase I effort. To this extent, "the study dismisses as insignificant," as the reviewer states, these events. However, this conclusion is a direct consequence of the application of probabilistic risk assessment to all events equally; there was no attempt to bias the results to influence licensing reviews.

A similar result appears in WASH-1400, wherein it is concluded that the double-ended licensing design-basis LOCA is not a major contributor to LWR risk and that, in fact, more likely smaller breaks in the primary circuit are a dominant risk.

It should be realized that the assumptions used in the course of the AIPA study are generally quite different from those employed in the licensing process. First of all, AIPA uses quantitative probabilistic methods -- a complete departure from the licensing type of analysis. The sequence of events included the possibility of multiple failures in safety-related equipment, which contrasts with the single-failure criterion employed in regulatory reviews. Event sequences included rare occurrences beyond those considered "credible" in licensing documents, e.g., sequences initiated by earthquakes of magnitude greater than SSE, and loss of offsite power event leading to 115 different branches, including loss of emergency power and auxiliary core cooling.

The part of the AIPA study most directly comparable to licensing evaluations is the consequence analysis. While the same physical processes are modeled (fission product release from fuel, transport in the core, plateout, atmospheric dispersion, etc.), AIPA employed median values of parameters in realistic models with uncertainties treated statistically and displayed as uncertainty bands in the results. This is in direct contrast with licensing analyses, which assume conservative or worst-possible values of parameters usually compounded arithmetically in the most pessimistic direction. Furthermore, the models employed, e.g., for core heatup and fission product release from fuel particles, attempted to be as realistic as possible within the limitations of knowledge that exists in any field. Although the models may be similar to those employed in licensing analyses owing to the fact that the same physical phenomena are being modeled, there was no attempt to "reflect GAC positions taken during the Summit and Fulton construction permit safety reviews." In fact, it should be noted that in Volume VI, dose consequences with uncertainties are compared with Fulton PSAR consequences for core heatup, steam ingress, and depressurization events. The upper 95% limits for 2-hr and 30-day whole body doses for the AIPA core heatup sequence slightly exceed those for the Fulton MHFPR, which incorporates licensing-basis conservatisms.

TABLE 2
CATEGORIES OF ISSUES

1. Design adequacy of safety-related components
 - a. Design calculations
 - b. Environmental quality
 - c. In-service inspection
 - d. Material properties
 - e. Industrial codes
 - f. Quality group and quality assurance plan
 - g. R&D and tests
 - h. Postulated transients including anticipated accident without scram (ATWS)
 - i. Feedback from Fort St. Vrain
2. Instrumentation
 - a. Adequacy
 - b. Environmental quality
 - c. Separation of safety/non-safety
 - d. Redundant power/air supplies
3. External influences
 - a. Plant and fuel security
 - b. Aircraft hardening
 - c. Turbine missiles
 - d. SSE intensity
4. Plant operation
 - a. Crew size
 - b. Control room ventilation versus fumes and fission products
 - c. Manual versus automatic actuation of engineered safety features (ESF)
 - d. Diesel automatic trip

Comment A-7

Our overall assessment of the seven-volume study is that revisions, some of which are rather fundamental, are required to advance the cause of the study's credibility and its general utility. We would anticipate that some of the revisions will result in expansion and improved definition of certain of the safety R&D recommendations presented as conclusions to the study. This should be of advantage to both the immediate and long-term needs of the HTGR program. Various detailed comments implying the revisions and expansions to the R&D recommendations are included in the sections on Methodology, Consequences and R&D Recommendations given below.

Response

There are no plans to revise the seven-volume report per se, because it is a status report of an ongoing project. The decision to publish such an interim status report was based on a desire to disseminate information about the study and solicit constructive criticism. As discussed in the response to Comment A-3, efforts are under way to verify the objectivity of the study and thereby augment its credibility. As work progresses, expansions or revisions of the present results may result, but it is not agreed that "fundamental" revisions are "required." With respect to R&D recommendations, it is to be understood that the AIPA recommendations based on risk assessment are not intended to imply that normal equipment developmental testing and evaluation are superseded. On the contrary, it is expected that such tests will be conducted to verify, as well as possible, the data employed in the AIPA study. This matter is discussed further in responses to more explicit comments.

B. Detailed Comments

Comment B-1 (Comment 1 on Methodology)

The validity of the methodology to reach objective conclusions appears highly dependent on the correctness of the judgments used to select the initiating events for the event trees. Improved means are needed to assure that no credible initiating events are overlooked. As a suggestion it would appear possible that a conscientious search and evaluation of information developed from fault trees might be a helpful "feedback" mechanism to validate, modify, or augment the initiating events selected.

Response

The conclusions derived via risk assessment are sensitive to the set of initiating events selected for study. The approach used for selecting initiating events entailed the identification of all sources of radioactivity in a plant, the barriers to the release of radioactivity to the environment from those sources, and the various initiating events that could potentially cause a breach of those barriers. Then, based on prior experience gained from probability and consequence evaluations, the initiating events that were expected to be a part of the risk envelope on a probability-versus-consequence plot were identified. This was accomplished by comparing the events two-by-two; consequences were compared for two events of about the same probability, or probabilities were compared when the consequences were about the same. To the risk-envelope events so identified were added several other initiating events that were of general interest by virtue of their being design basis events. The entire selection approach outlined above was necessitated by the recognition of the fact that it would be impossible to analyze all identifiable initiating events within the practical constraints of time, funding, and manpower; the list of initiating events to be formally analyzed had to be restricted to a practical number that were expected to be the significant contributors to the risk envelope.

While the above approach was a systematic one that seemed to provide a practical basis for beginning the AIPA study, there was a need to develop a convenient means for displaying the various alternatives considered. Such a means has been developed by Aerojet Nuclear Company, using a fault tree format.* The top level event in the tree identifies the undesired release, and the sources of radioactivity are indicated at the next level. The third level is used to identify plant operational modes. The lowest levels of the tree provide an indication of the fission product barriers and a list of the corresponding initiating events that could cause a breaching of those barriers. Figure 1 indicates the basic format for such a display.

The systematic approach described above tends to enhance (but not necessarily assure) the completeness of the analysis. Additional confidence in study completeness can be obtained by comparing the analysis results with the results of other independent studies and by using current numerical results as a basis for checking prior judgments and for probing to identify any key initiating events that may have been inadvertently omitted. Comparisons with WASH-1400 have most definitely revealed no glaring omissions in the AIPA study. Planned efforts in the AIPA study are directed toward improving and expanding the methodology for selecting initiating events as well as evaluating particular initiating events which are suggested on the basis of experienced judgment. An example of the latter is "failure of the core support posts," which is currently being analyzed. One way to improve the methodology, which is being investigated, is to generate new initiating events based on what was learned in analyzing the original set. For example, the highest risk event analyzed in the original set is a core heatup initiated by loss of all offsite power. Loss of offsite power coupled with turbine trip represents a failure mode for the main cooling system. This knowledge leads to the question: "What are the other credible failure modes of the main cooling system?" This question was asked in Volume III in choosing LOSP, but a more sophisticated

*The format was presented by ANC during a meeting held at their facility on March 17 and 18, 1975.

Fig. 1. Fault tree format for identifying initiating events

answer to the question may be obtained, for example, via fault tree analysis that could generate new initiating events for core heatup, which could be evaluated against the original selection to improve the credibility of the method. It should be noted, however, that the problem of "completeness" is not peculiar to the selection of initiating events, but is shared with all probability prediction methods, including fault tree analysis.

Comment B-2
(Comment 2 on Methodology)

Human factors affecting HTGR risk should be explored in greater detail. The report correctly recognizes the benefits of the large core heat capacity to provide more time for mitigating corrective actions (e.g., restart of diesels). In making this recognition, however, the report is too optimistic as it deems it impossible for operator action to aggravate rather than mitigate the accident. More appropriately the report should acknowledge the potential for incorrect operator response, albeit highly improbable, and quantitatively account for it in the fault trees. Statements in the last paragraph of Volume IV, page A4-1 are not defensible. Further, human factors affecting equipment reliability related to quality assurance, maintenance and inservice inspection were not satisfactorily discussed. As is well known, such human factors are very closely related to equipment reliability.

Response

It is agreed that human factors affecting HTGR risk should be explored in greater detail (for example, item 5, page 3-5, in Volume IV of the Status Report). It is the intent of this R&D suggestion to review in more detail potential operator errors which may be coupled with the initiating event. For example, confusion of the operator which increases the likelihood of an error could be caused by similar instrument responses occurring from widely different events.

In the AIPA report, operator actions were divided into two categories, success or failure, which could also be called mitigating or nonmitigating. General mitigating responses to abnormal plant conditions usually involve power reduction and plant shutdown when a question of safety arises. If the operator is aware of an unusual condition, and his initial assessment is wrong or he institutes the wrong control function initially, his observation of the ensuing transient would demonstrate the need for continued

corrective actions. If the need for subsequent action arises, the operator would institute an orderly shutdown procedure in most cases. Providing the operator is aware of the abnormal condition, the inherently slow response time of the HTGR core provides the operator with ample time for subsequent corrective actions in a calm, professional manner and gives him an opportunity to consult others on the operating staff, including the superintendent. Thus, the most likely nonmitigating transient arises from operator inaction rather than initially wrong actions, since attempts to mitigate will eventually lead to plant shutdown and isolation. Hence, it is not deemed impossible for the operator to initially aggravate the potential course of an initiating event; however, we do allow him to be self correcting. To explore every potentially wrong operator action at each branch point of an event sequence was judged to be unproductive of higher risk sequences. High risk branches have been identified which are highly dependent on operator actions, and additional research has been suggested in one of these areas. This recommendation includes an awareness that methods for assessing the impact on risks due to operator errors need to be improved.

The method for treating common mode failures (see Section 4 of Volume II) consistently and systematically assigns realistic failure rates and common mode failure parameters to every redundant system analyzed in the fault trees. (In the case of nonredundant systems in the BOP, operating experience data were effectively used.) The generic failure rates and common mode parameters used in the study include design errors, operator and maintenance errors, QA errors, and many others (see Table 4-2 in Volume II). The inspection and testing frequencies were systematically considered in the unavailability calculations found in Volumes II and IV.

Comment B-3
(Comment 3 on Methodology)

The study has considered the time-delay advantages to HTGR safety both in the event trees and by discussion of the equation $\text{risk} = \text{consequence/time-delay}$ (which reflects the benefits due to more time for evacuation). We recommend that the focus on the time-delay aspects of the HTGR be augmented and better coordinated with studies of human factors. A highly credible discussion of the effects of inherent time-delay factors on HTGR risks could be an especially useful output of the study.

Response

The long time delays inherent in the HTGR afford greater opportunity for the operating crew to minimize the consequences of such events as a core heatup. The Brown's Ferry fire demonstrated the ability of reactor operating crews to successfully respond to these unanticipated events under adverse conditions. The AIPA study does consider some operating crew actions with regard to CACS operation and restoration of main loop cooling during the loss of offsite power and turbine trip event. However, for the case where both cooling systems are unavailable, additional human intervention was not considered at the time. Since the largest AIPA risks are associated with the core heatup Branch BD (page A2-2 in Volume IV), various operator actions can be explored which would increase public safety as shown in Fig. 2. The operating crew has a few hours to restore core cooling by either CACS or main loop cooling before the gas temperatures are high enough to cause bottom head cross duct damage with the potential for restriction or blockage of the helium flow path. During this time the reactor operator should be considering actions which increase the time to component failure and reduce the potential releases. An example follows in which the delay aspects of the HTGR could be analyzed to provide the operators with suggested actions to enhance public safety.

Fig. 2. Beneficial effects of operator action on HTGR safety

Consider a situation where component damage has occurred during the course of an accident sequence. What can the control room operator do to reduce the potential consequences? For example, if the system depressurizes through the primary coolant relief valves, the operator could possibly close the block valves to eliminate a direct fission product path to the containment. It may also be possible to redirect liner cooling flow to the top head to maximize heat removal. These actions could possibly increase the fission product holdup time, allowing for additional radioactive decay, thus diminishing the source strength.

The most appropriate operator actions at various times during high risk events should be determined by careful analysis prior to the event. Under some accident conditions, the obvious operator action may not be the most desirable. A reference manual regarding core heatup events could help the operators make timely decisions which enhance public safety in a given situation. It is recognized that human interaction at critical times during an event sequence can increase overall plant safety, and, as such, R&D suggestion 5 (page 3-5 in Volume IV) has been included. This research suggestion applies to the operator actions which most likely affect the HTGR high risk event of a small reheater tube leak which allows primary coolant to escape into the secondary steam.

Comment B-4
(Comment 4 on Methodology)

The probability of large motion seismic events is taken so low that the credibility of the entire study is endangered. For example, the mean probability of occurrence at a given reactor site of an SSE is taken as 1.5×10^{-8} per year (Volume IV, page A3-57). The AIPA study should be revised to include occurrence probabilities of large seismic events several orders of magnitude greater.

Response

This particular comment deals with the assessment of earthquake occurrence rates at specific sites as a function of earthquake size. It has been found difficult to prove, in a reasonably acceptable manner, what occurrence rate values are correct because the overall state-of-the-art of making such assessments does not appear to be very advanced. In support of this it should be noted that

1. A survey (Ref. 8) of seven seismic experts yielded a large scatter in predicted occurrence rates for strong motion earthquakes at individual sites. The range in data scatter for specific accelerations of interest at individual sites averages nearly three orders of magnitude and runs as high as five orders of magnitude at one site (Davis Besse) for a ground acceleration of 0.15 g.
2. There are many formulas for estimating seismic attenuation; most of them are determinable from two basic, functionally different formulations (Refs. 9-11). These formulas are so different that order-of-magnitude variations can occur in estimates of earthquake occurrence rates (Ref. 12).

In view of the above observations, it is necessary to know and understand the basis for the comment in order to be able to respond to it

in a meaningful manner. Since no definitive statement in this regard is contained therein, it is necessary to surmise its basis. One obvious possibility is that the SSE occurrence rate estimates cited in the AIPA Status Report are quite different from those that might have attained some level of acceptability in recent years. These latter estimates generally fall in a range from about 10^{-4} per year to about 10^{-3} per year and are supported by WASH-1400* and by the figures cited at ACRS hearings. Another possibility is that SSE occurrence rates of the the order of 10^{-4} to 10^{-3} per year may seem intuitively reasonable because large earthquakes are observed so frequently around the world (like those that occurred recently in the Philippines, Guatemala, China, and Italy). However, there appear to be reasonable grounds for questioning the validity of such considerations.

Consider, first, the intuitive assessment: large earthquakes occur frequently around the world; therefore, the occurrence rate of SSEs at nuclear plant sites must be "high" -- say, of the order of 10^{-4} to 10^{-3} per year. A recent article published last year (Ref. 13) shows that, while large earthquakes (with magnitudes of about 8 on the Richter scale) occur fairly frequently around the world (one to ten times per year), earthquakes causing 0.15 g to 0.25 g acceleration levels (which are of the order of SSE accelerations in the eastern U.S. can occur with frequencies as low as 10^{-7} to 10^{-6} per year in the northern Rhine area.** Thus, such an intuitive approach should not be relied upon since it tends to be misleading.

A more concrete source for estimates of the order of 10^{-4} to 10^{-3} per year is provided by WASH-1400,* which cites such values for the SSE acceleration levels that are currently in common use in the eastern U.S.

*Main report, Section 5.4.1, Table 6.

**Comparing data for the northern Rhine with that for the eastern United States seems perfectly justified since the Hsieh report indicates that the seismicity and geological structure of western and central Europe (which includes the northern Rhine) are similar to those of the eastern U.S. These similarities were used as the basis for combining the appropriate European seismic data with that for the eastern United States (Refs. 13 through 15).

However, WASH-1400 references the Hsieh report (Ref. 16) as the sole source of the occurrence rate estimates. Thus, this Hsieh report seems to be an ultimate source for these "accepted" occurrence rate estimates and serves as the focal point for the discussion that follows.

The Hsieh report is divided into two basic analysis parts. The first part deals with the establishment of average occurrence rate estimates, as a function of Richter magnitudes, in two parts of the U.S. -- east of the Rockies and west of the Rockies. It is assumed that earthquakes occur randomly with respect to time and location within each region; in other words, earthquakes are assumed not to be influenced by local tectonic structures (Ref. 17). The second analysis part makes use of various attenuation formulas to develop estimates of occurrence rates as a function of peak ground acceleration. For the eastern U.S., estimates are presented for firm, average, and soft sites. These results are still based on the assumed uniform seismicity within each region; the differences in occurrence rate estimates are attributable solely to differences in seismic attenuation from one type of site to another (Ref. 18).

The first observation to be made concerning the Hsieh analysis is that the assumed uniform seismicity within a major area of the United States runs contrary to the information presented by Richter (Ref. 19) and the site-specific orientation of 10CFR100, Appendix A. Thus, while the results contained in the Hsieh report appear to be representative for large areas within the U.S., there is no guarantee that the results are representative for individual, local sites within a region. In recognition of this fact, the authors of the Hsieh report have indicated that "This assumption (of uniform seismicity) tends to make any prediction for an 'average' site yield a higher probability of accelerations, while for sites near historical major earthquakes (e.g., New Madrid) the prediction should be low" (Ref. 20). Since nuclear power plants tend to be sited in localities of relatively low seismicity, it follows that the results in the Hsieh report are not representative of such sites -- they yield overestimates of earthquake occurrence rates for nuclear power plant sites. While these conservatisms might be appropriate for licensing considerations if they were made

explicit, they are not consistent with the objective of realism in the AIPA study. Thus, the results in the Hsieh report are not considered to be applicable.

At the time that the Phase I AIPA earthquake analysis was performed, an approach was taken which, it was hoped, would contribute some new insights that might be useful in advancing the state-of-the-art in this field. The use of many of the often-quoted occurrence rate estimates was avoided since it was suspected that they might be biased on the high side in support of licensing conservatism. The AIPA approach attempted to develop median, site-specific estimates of occurrence rates, to be consistent with the characteristic of local variations in seismicity as indicated by Richter (Ref. 21) and with the site-specific orientation of 10CFR100, Appendix A. A statistical analysis was performed on actual experience data at each of several sites. Use was made of the lognormal distribution of accelerations which had been demonstrated in prior data analyses (Ref. 22). The use of such a distribution offered the advantages of high efficiency of the statistical analyses and of providing a basis for relating occurrence rates to ground acceleration levels. The approach also permitted an assessment of prediction uncertainties.

Thus, there appears to be a dilemma. On the one hand, the comment criticizes the AIPA estimates as being too low; on the other hand, the estimates that appear to serve as the underlying frame of reference and comparison for the comment are believed to be too high. In an effort to resolve this situation in such a way as to enhance the credibility of the AIPA study results and conclusions, it was decided to modify the AIPA estimates so that they are half way between the two extremes on a logarithmic scale. These modified estimates are to be used in the interim until a more comprehensive technical evaluation can be performed.

The first step in making the modification is to transform the results in the Hsieh report into a form compatible with that used in the AIPA study. The conservative occurrence rates developed from the Hsieh report

are based on an SSE acceleration of 0.25 g (to be compatible with more recent licensing practices in the eastern United States). The associated occurrence rate numbers are shown in Table 3 and are developed from Table A-2 and Fig. A-2 of the attachment to this response. (The symbology used in the figures and tables is consistent with that used in the AIPA Status Report.) The median AIPA results are given in Table 4 and are taken directly from Table A3-12 of Volume IV of the Status Report. Table 5 gives modified median estimates of occurrence rates (geometric means of the corresponding values in Tables 3 and 4) and modified values of uncertainty factors (ratio of occurrence rates in Table 3 to the modified median occurrence rates in Table 5). The results in Table 5 are used in the modified event trees shown in Figs. 3 and 4 (modified from Figs. A3-5 and A3-7 in Volume IV of the AIPA Status Report).

In parallel with efforts to modify the earthquake occurrence rate estimates to enhance their credibility was a similarly motivated effort in the area of consequence assessment. The most significant considerations involved in the modification of the consequence estimates were the revision of PCRV plateout fractions, and the incorporation of a more realistic modeling of the time-dependent transport of fission products from the PCRV to the containment.* The results of the revised consequence analyses are presented in Table 6.

The probability data given in Figs. 3 and 4 and the consequence data in Table 6 are used to develop the modified summary risk plot shown in Fig. 5 (modified from Fig. 3-1 in Volume IV of the AIPA Status Report). These results indicate small decreases in the consequences for the two earthquake sequences of interest (as well as for LOSP-BD) and large increases (up to two orders of magnitude) in the annual occurrence rates for those same

*The details of the complete analysis entailed in revising the consequence assessments are considered to be beyond the scope of this particular response, although some facets of that analysis (namely, those dealing with enhancing the credibility of the PCRV escape fractions during core heatup events) are presented elsewhere in response to specific comments on that subject (see comment 1 from Section 3.2 and comment 5 from Section 3.3).

TABLE 3
PRESCRIBED UPPER LIMIT ON EARTHQUAKE OCCURRENCE RATES

Range of α	Occurrence Rate, λ_{95} , based on Hsieh data (occurrences/year)
$\alpha \geq 1.0$ (0.25 g)	$1.5 \times 10^{-4} + X$
$\alpha \geq 1.2$ (0.30 g)	$9.9 \times 10^{-5} + X$
$\alpha \geq 1.4$ (0.35 g)	$6.8 \times 10^{-5} + X$
$\alpha \geq 1.6$ (0.40 g)	$4.9 \times 10^{-5} + X$
$\alpha \geq 1.8$ (0.45 g)	$3.6 \times 10^{-5} + X$
$\alpha \geq 2.0$ (0.50 g)	$2.7 \times 10^{-5} + X$
$1.0 \leq \alpha < 1.2$	5.1×10^{-5}
$1.2 \leq \alpha < 1.4$	3.1×10^{-5}
$1.4 \leq \alpha < 1.6$	1.9×10^{-5}
$1.6 \leq \alpha < 1.8$	1.3×10^{-5}
$1.8 \leq \alpha < 2.0$	9.0×10^{-6}

TABLE 4
PRESCRIBED LOWER LIMIT ON EARTHQUAKE OCCURRENCE RATES

Range of α	Occurrence Rate, λ_5 , based on AIPA data (occurrences/year)
$1.0 \leq \alpha < 1.2$	1.5×10^{-8}
$1.2 \leq \alpha < 1.4$	4.0×10^{-9}
$1.4 \leq \alpha < 1.6$	1.3×10^{-9}
$1.6 \leq \alpha < 1.8$	4.5×10^{-10}
$1.8 \leq \alpha < 2.0$	1.8×10^{-10}

Fig. 3. Earthquake event tree for magnitude interval $1.0 \leq \alpha < 1.2$ (prescribed interim results -- from Fig. A3-5, Volume IV, of the Status Report)

Fig. 4. Earthquake event tree for magnitude interval $1.4 \leq M < 1.6$ (prescribed interim results -- from Fig. A3-7, Volume IV, of the Status Report)

TABLE 5
PRESCRIBED INTERIM PARAMETER VALUES

Range of α	Interim Median Earthquake Occurrence Rate, λ_{50} (occurrences/year)	Interim Uncertainty Factor for 90% Uncertainty Interval
$1.0 < \alpha < 1.2$	8.7×10^{-7}	60
$1.2 < \alpha < 1.4$	3.5×10^{-7}	90
$1.4 < \alpha < 1.6$	1.6×10^{-7}	120
$1.6 < \alpha < 1.8$	7.7×10^{-8}	170
$1.8 < \alpha < 2.0$	4.0×10^{-8}	200

TABLE 6
PRESCRIBED INTERIM CONSEQUENCE VALUES

State of Containment Isolation	Median Dose (rem, WB- γ)	Uncertainty Factor
Isolated	6.9×10^{-2}	10
Not isolated	1.4	10

Fig. 5. Summary risk plot, Phase I analysis (modified from Fig. 3-1, Volume IV, of the Status Report)

earthquake sequences. The data in Figs. 3 and 4 and Table 6 can be combined to provide the risk data shown in Table 7 (modified from Table 5-7 in Volume IV of the Status Report). Notice that the modifications in consequence values and in the earthquake occurrence rates clearly did not alter the ranking of the event sequence branches when considered in order of decreasing median risk. However, these modifications have had a small impact on the ordering with respect to the R_{95} risk values, since the interim R_{95} value of 5×10^{-7} rem/year for the $\alpha = 1.5$ earthquake Branch S is slightly higher than the R_{95} value of 2×10^{-7} rem/year for RHL-LA. (A similar change in the ordering of the RHL-SM and the interim LOSEP-BD R_{95} values is also evident, but the effect is, again, a small one.) Although the results presented in Table 7 indicate a small increase in the relative importance of the interim $\alpha = 1.5$ earthquake Branch S with respect to RHL-LA (and of RHL-SM with respect to the interim LOSEP-BD), such a change in relative importance is rather insignificant based on three considerations:

1. The establishment of a ranking by relative importance is based primarily on median values and secondary on R_{95} values.
2. The R_{95} risk values are very close together.
3. The small numerical differences in the R_{95} risk values tend to be of the same order as round-off errors and, hence, tend to be obscured thereby.

Thus, while the modifications have a minor impact on the numerical results and, possibly, on the relative ranking of event sequence branches by risk, they do not affect the AIPA study conclusions or R&D recommendations.

It is of interest to compare the modified median occurrence rates developed above with the results of other assessments. The median occurrence rate derivable from the Okrent survey (Ref. 23) for a 0.25 g SSE

TABLE 7
RECOMMENDATIONS FOR HTGR SAFETY RESEARCH AND DEVELOPMENT
(Modified from Table 5-7, Volume IV, GA-A13617)

Initiating Event and Branch Computational Point(a)	Justification from AIPA		90% Uncertainty Band		Area of R&D Interest	R&D Problem No.	Summary of Approach
	Median Risk (rem/yr)	Uncertainty Factor	R ₅ (rem/yr)	R ₉₅ (rem/yr)			
LOSP-BD Point a	7×10^{-8}	50	1×10^{-9}	3×10^{-6}	Probability of cooling system failure	1	Probability model of core cooling
					Uncertainty in PCRV integrity in core heatup transient	2	PCRV integrity study and effect on containment
					Fission product release uncertainty	3	HTGR fission product release and plateout tests
						4	Development and verification of fission product release and plateout code
RHL-SM, Point r	2×10^{-8}	180	1×10^{-10}	4×10^{-6}	Operator action uncertainty	5	Development of better probabilistic operator response model
RHL-LA, Point r	2×10^{-8}	10	2×10^{-9}	2×10^{-7}	RH tube failures	6	Performance of metallurgy studies leading to physical understanding of tube failure
						7	Tube failure model, test, and verification
EQ- α = 1.5 S, Point a	6×10^{-10}	900	7×10^{-13}	5×10^{-7}	Uncertainty in Eq. occurrence probabilities	8	Earthquake frequency studies
EQ- α = 1.1 O, Point a	1×10^{-11}	300	4×10^{-14}	3×10^{-9}	Equip. survival probabilities	9	Confirmation of equipment and structure survival

(a) r = Status Report; a = Interim result.

acceleration is 8×10^{-6} per year, which is about an order of magnitude higher than another assessment, based on the northern Rhine area (Ref. 24), indicates that occurrence rates as low as about 4×10^{-7} per year exist for an acceleration level of 0.25 g (based on a linear extrapolation on a log-log plot of data given for 0.1 g and 0.3 g accelerations); this result is less than half of the 9×10^{-7} value in Table 5. The results of the Rhine-area analyses are considered reasonably applicable to the eastern United States since, as noted previously, the two areas are considered seismically and geologically similar.

The above discussion leads to the following conclusions:

1. While the results presented in the Hsieh report are representative (in the sense of average) for large areas in the United States, they are not representative of the sites of nuclear power plants.
2. Application of the Hsieh results to nuclear plant sites tends to yield high estimates of earthquake occurrence rates.
3. The modified SSE occurrence rates presented herein are half way (on a logarithmic scale) between the medians given in the Status Report and the conservative Hsieh results.
4. The modified SSE occurrence rates are in reasonable agreement with other assessments that may be applicable to the AIPA study.
5. The modifications in the earthquake occurrence rates, coupled with recent modifications in consequence estimates, cause no changes in event sequence rankings with respect to median risk but do cause small, essentially insignificant changes with respect to R_{95} risk values. However, these minor changes do not affect the overall study conclusions or R&D recommendations.

TRANSFORMATION OF HSIEH RESULTS

The purpose of this attachment is to transform the Hsieh results into a form compatible with that of the AIPA study. This transformation is based on the results presented in Table 6 of the Hsieh report (Ref. 25) for the Eastern United States.* (For convenience, this table has been copied and is included as Exhibit A-1.) Since nuclear power plants tend to be located at firm sites, are the lowest of those shown in the table, the figures shown in the table for firm sites are used in this analysis.

As can be seen in Exhibit A-1, only four ground acceleration increments are listed. This is not a complete listing of increments, for many intervening increments (like 0.3 g and 0.6 g) have been omitted from the table, as have increments beyond 1.0 g.** The truth of this statement follows from a careful comparison of the occurrence rates listed in Exhibit A-1 for finite acceleration increments for average sites with the occurrence rates given in Exhibit A-2 (a copy of Table 5 from Ref. 25) for semi-infinite acceleration intervals under column I. (The equivalence of these two occurrence rate listings, although not specifically identified as such in the text, can be deduced from information presented in the report, with Table 7 from Ref. 25 serving as a key starting point in the deductive process; the equivalence can be confirmed by performing the appropriate calculations in accordance with the formulation prescribed in the footnote in Exhibit A-1.)

Since the data listing in Exhibit A-1 is not complete, it is necessary to interpolate to establish occurrence rates for the intervening increments. Figure A-1 is based on the four data points in the exhibit and

* Table 6 is the basis for all WASH-1400 occurrence rates.

** It appears that a much different interpretation of the Hsieh results was erroneously used as the basis for the earthquake analysis presented in WASH-1400 (Main Report, Section 5.4.1). If the above interpretation were to be applied in the WASH-1400 analysis (together with another interpretational change identified subsequently), the result would be an increase in about one order of magnitude in the probabilities cited in WASH-1400 for earthquake-induced core melt accidents.

Table 6. Probability of Ground Acceleration per
Year in the Eastern U.S. for Different Site Conditions¹³

Ground Accel. (incremental)*	Soft	Average	Firm
0.1g	7.7×10^{-3}	3.6×10^{-3}	1.4×10^{-3}
0.2g	1.9×10^{-3}	6.5×10^{-4}	1.5×10^{-4}
0.5g	2.0×10^{-4}	4.6×10^{-5}	7.9×10^{-6}
1.0g	3.5×10^{-5}	1.0×10^{-5}	6.9×10^{-7}

*The increment includes a band of accelerations around that specified, e.g., the probability at 0.1g is equal to the average of the two probabilities 0.05g to 0.1g and 0.1g to 0.15g.

Table 5. The Annual Probability of Ground Acceleration in the Eastern U.S.

gr. acc. (cumulative)	I. $\log N = -2.71 - 0.839M$	II.* $\log N = -9.22 + 1.37M - 0.186M^2$	III.** $\log N = -3.908 - 0.636M$
$\geq 0.05g$	9.60×10^{-3}	3.78×10^{-3}	1.11×10^{-2}
$\geq 0.10g$	4.07×10^{-3}	1.32×10^{-3}	4.86×10^{-3}
$\geq 0.15g$	2.35×10^{-3}	6.85×10^{-4}	2.85×10^{-3}
$\geq 0.20g$	1.48×10^{-3}	4.03×10^{-4}	1.82×10^{-3}
$\geq 0.25g$	1.04×10^{-3}	2.68×10^{-4}	1.29×10^{-3}
$\geq 0.30g$	7.82×10^{-4}	1.95×10^{-4}	9.71×10^{-4}
$\geq 0.40g$	4.63×10^{-4}	1.07×10^{-4}	5.79×10^{-4}
$\geq 0.5g$	3.03×10^{-4}	6.70×10^{-5}	3.81×10^{-4}
$\geq 0.6g$	2.32×10^{-4}	5.00×10^{-5}	2.93×10^{-4}
$\geq 0.8g$	1.15×10^{-4}	2.32×10^{-5}	1.45×10^{-4}
$\geq 1.0g$	5.44×10^{-5}	1.05×10^{-5}	6.91×10^{-5}

*The New Madrid EQs are neglected (more or less).

**The b value is determined by the max. likelihood method.

Fig. A-1. Hsieh results for firm sites

provides the basis for the interpolation. The interpolated values are listed in Table A-1, together with values taken directly from Exhibit A-1. The interval ranges listed in Table A-1 are designated in accordance with the footnote in the exhibit.

Two kinds of occurrence rates are listed in Table A-1: average and total. The so-called "average" values represent the average of two probability values -- one for the lower half of the acceleration interval, and the other for the upper half of the interval -- as indicated in the footnote in Exhibit A-1. Thus, the total probability of occurrence of an acceleration in the full interval must be twice the "average" value given for the interval.* These total probability values are listed in the last column of Table A-1.

The total probability values discussed above are appropriately combined to yield the cumulative probability values listed in Table A-2 and plotted in Fig. A-2, as a function of acceleration level. The value of X, the probability of accelerations >1.05 g, is unknown; there is no way of evaluating it from the data in Exhibit A-1. The best that can be done at this point is to surmise that the value of X is "small" compared with many of the numerical entries appearing in Table A-2, particularly those corresponding to the smaller acceleration levels. However, estimating the value of X is not critical to this analysis since it cancels when differences are evaluated in estimating occurrence rates for finite acceleration intervals.

Table A-2 and Fig. A-2 present the transformed Hsieh results in a form appropriate for use in modifying the AIPA occurrence rates. This concludes the transformations presented in this Attachment.

* This differs from the erroneous interpretation in WASH-1400 (Main Report, Section 5.4.1) that the values as given in Exhibit B-1 are total occurrence probabilities for accelerations within the full interval.

TABLE A-1
HSIEH RESULTS FOR FIRM SITE

Ground Acceleration (g)		Average Annual Occurrence Rate for Acceleration Interval		Total Annual Occurrence Rate for Acceleration Interval (occurrences/year)
Interval Designation	Interval Range	Hsieh's Table 6	Interpolated (Fig. A-1)	
0.1	0.05 - 0.15	1.5×10^{-3}		2.8×10^{-3}
0.2	0.15 - 0.25	1.5×10^{-4}		3.0×10^{-4}
0.3	0.25 - 0.35		4.0×10^{-5}	8.0×10^{-5}
0.4	0.35 - 0.45		1.6×10^{-5}	3.2×10^{-5}
0.5	0.45 - 0.55	7.9×10^{-6}		1.6×10^{-5}
0.6	0.55 - 0.65		4.2×10^{-6}	8.4×10^{-6}
0.7	0.65 - 0.75		2.5×10^{-6}	5.0×10^{-6}
0.8	0.75 - 0.85		1.5×10^{-6}	3.0×10^{-6}
0.9	0.85 - 0.95		1.0×10^{-6}	2.0×10^{-6}
1.0	0.95 - 1.05	6.9×10^{-7}		1.4×10^{-6}

Fig. A-2. Plot of data from Table A-2

TABLE A-2
CUMULATIVE PROBABILITIES FROM HSIEH RESULTS

Ground Acceleration, Semi-Infinite Range (g)	Annual Occurrence Rate for Acceleration Range ^(a) (occurrences/year)
≥ 0.05	$3.2 \times 10^{-3} + X$
≥ 0.15	$4.5 \times 10^{-4} + X$
≥ 0.25	$1.5 \times 10^{-4} + X$
≥ 0.35	$6.8 \times 10^{-5} + X$
≥ 0.45	$3.6 \times 10^{-5} + X$
≥ 0.55	$2.0 \times 10^{-5} + X$
≥ 0.65	$1.1 \times 10^{-5} + X$
≥ 0.75	$6.4 \times 10^{-6} + X$
≥ 0.85	$3.4 \times 10^{-6} + X$
≥ 0.95	$1.4 \times 10^{-6} + X$
≥ 1.05	X

(a) The occurrence rate X (for ground accelerations > 1.05 g) cannot be quantitatively determined directly from the results listed in Exhibit A-1.

Comment B-5
(Comment 5 on Methodology)

The methodology limits containment failures to the cases where the containment building does not isolate and the filtering system does not function. The basic structure remains intact in all events considered. No series of accident related events that would destructively fail the containment (such as the steam explosion mechanism for water reactors) is deemed credible for HTGRs. While this may eventually be found to be reasonable, the report is deficient in not acknowledging the need for further analysis and study to confirm this assertion.

Response

The AIPA study included containment failure modes other than the case where the isolation valves fail to close. One such instance was included in the Phase I earthquake analysis. In Section A3.7 of Vol. IV, it is noted that there are two ways in which failure to maintain the essentially leak-tight integrity of the containment could occur following a large earthquake:

1. A failure of the containment isolation valves.
2. A loss of leak-tight integrity of the containment caused by cracking of the liner and concrete.

Both of these failure modes were considered in evaluating the success and failure probabilities associated with the event tree heading "containment isolation." The consequence assessments for both modes of failure were based on the high containment leakage rates as given for nonisolated containments in Table 3-7 of Vol. VI.

The initiating event "PCRV structural failure" analyzed in Volume III includes sequences where missiles generated from large PCRV closure failures penetrate the containment building structure. The probability of

this event was found to be small since it requires multiple structural failures of the pressure vessel. Hence, the event was not reanalyzed in depth as was done for other events discussed in Volume IV.

An accident-related event leading to containment failure is discussed in Volume VI. The initiating event is a steam generator tube rupture, and the sequences that lead to containment failure postulate global flammability in the containment. For burning to occur, there must be sufficient production of flammable gases in the PCRV and a subsequent venting of these gases to the containment building. Consequently, only those branches for which the leaking steam generator is not isolated and dumped, and either a penetration failure occurs or the relief line fails to close, lead to global flammability. Global flammability is assumed to result in a loss of forced core cooling and containment leakage equivalent to nonisolation. Therefore, the failure is not given in terms of the destruction of the basic structure, but rather from the point of view of fission product retention capability.

Nevertheless, the containment failure treated is somewhat limited and this limitation was recognized in the Status Report. This was considered to be "reasonable" since a phenomenon such as a steam explosion does not apply to HTGRs. In Volume I, the report does indeed acknowledge the need for further analysis of the containment integrity by recommending a study of PCRV integrity and effect on containment. Table 6-2 states that in this recommended R&D study, which is now in progress, "Computer programs would be used to assess the effect of released heat and gases on containment building integrity."

Comment B-6
(Comment 6a on Methodology)

In addition to the limitations assigned to the containment failure mode, we identified other limitations or exclusions included in the methodology that could have a significant effect on the conclusions of the study. The more prominent are:

- a. The limitation of the maximum depressurization area to 100 square inches.

Response

The comment expresses the concern that the depressurization rates in the study were limited to the failures of PCRV closures not exceeding 100 sq in. in area. While this is true for event 3, the reviewer apparently overlooked event 16, where the total loss of a large-diameter closure of up to 163 sq ft was considered.

The 100 sq in. is the maximum flow area of the flow restrictors, which were located in the large PCRV penetrations for the specific purpose of restricting the outflow in case of penetration closure failure and were designated an engineered safety feature (ESF). This maximum blowdown area could not be exceeded by any failure of any PCRV penetration without the concurrent failure of flow restrictors, which would be an event of a very low probability. Rather than extend the event tree of event 3 to include these low probability branches, a more severe sequence (event 17) was postulated in which the combined failure of penetration liner and/or redundant anchors could lead to the ejection of the large closure plug, and expose a blowdown area of up to 163 sq ft.

It is possible that the misunderstanding regarding blowdown area limitations might have been avoided by analyzing both events as progression branches of a single event tree. However, in the preliminary analysis, the

identification of the 100 sq in. rapid depressurization is related to release of primary coolant, and the highly improbable penetration failure is related to the possibility of core overheating and release of radioactivity in the fuel.

Comment B-7
(Comment 6b on Methodology)

In addition to the limitations assigned to the containment failure mode, we identified other limitations or exclusions included in the methodology that could have a significant effect on the conclusions of the study. The more prominent are:

- b. The failure to consider steam ingress events beyond the rupture of a single steam generator tube.

Response

The initiating event analyzed is an essentially offset shear rupture of a single tube at the location (near the feedwater tubesheet) yielding the highest inleakage rate (~ 24 lb/sec after a few seconds). The frequency of such an event is estimated at $2.5 \times 10^{-3} \text{ yr}^{-1}$ (Ref. 26). Less severe ruptures have higher probabilities of occurrence but are inconsequential because at lower leak rates the PCRVR relief valve does not lift. This is true even when the decisive PPS failure (failure to identify and isolate the leaking steam generator after reactor trip) is postulated. On the other hand, multiple tube ruptures result in only slightly higher consequences up to about 7 feedwater or 14 superheater tube breaks (Ref. 26). Beyond this point the risk increases several orders of magnitude because the resultant leak rate is such that the PCRVR relief valves lift even with correct PPS action. However, because of the conservative design of the PCRVR relief system and PPS isolation functions, high consequences or risks do not occur even for all tube ruptures in a steam generator (Ref. 27). Occurrence probabilities of independent multiple tube ruptures are small. To date, no significantly high occurrence probability has been quantitatively identified for "common cause" multiple ruptures or for mechanistic links between a tube rupture and other accidents such as depressurization, although their consequences are known from previous work such as Ref. 26.

This is largely attributable to conservative design margins inherent in the HTGR steam generators relative to short-term pressure and temperature effects.

The above reasons formed the basis for the judgment that the single offset tube rupture is the major event for the Phase I AIPA risk assessment.

Comment B-8
(Comment 6c on Methodology)

In addition to the limitations assigned to the containment failure mode, we identified other limitations or exclusions included in the methodology that could have a significant effect on the conclusions of the study. The more prominent are:

- c. The failure to consider potential air-graphite reactions subsequent to a major accident.

Response

Air-graphite reactions have been considered in the steam ingress events of Volume VI. In the core heatup event, the only path for air ingress is through the 20-sq-in. relief valve after blowdown. Due to the decay heat, the PCRV gas is expanding. Furthermore, for every mole of O_2 that might be convected down the vertical and across the horizontal tube, 2 moles of gas would be produced from the air-graphite reaction. Therefore, in this case with only the relief valve opening, air ingress is self-limiting. Another opening would be required for significant air ingress to take place. The PCRV integrity study, currently in progress, is examining the area further.

Comment B-9
(Comment 6d on Methodology)

In addition to the limitations assigned to the containment failure mode, we identified other limitations or exclusions included in the methodology that could have a significant effect on the conclusions of the study. The more prominent are:

- d. The effects of corrosion by oxidants and other impurities in the primary system on structural or equipment failure potentials.

Response

Effects of corrosion by oxidants or other impurities during the accident on structural failure were analyzed during steam ingress events. Large amounts of steam reacting with the core graphite produce quantities of $\text{CO} + \text{H}_2$ which can be potentially flammable in the containment air, and this effect was analyzed in detail (Volume VI). The effect of graphite burnoff on structural integrity of the core was analyzed in PSARs and Ref. 26, where it was shown that the degree of burnoff causes insignificant structural degradation. On the basis of this body of work, the judgment was made in the AIPA Phase I analysis to exclude branch sequences leading to probabilistic core structural damage.

For similar reasons, the effects of corrosion during plant operation prior to the accident were not considered in the Phase I work, except in probabilities of a steam generator tube failure initiating event. Currently, the effect of prior oxidation of core support posts on a post failure initiating event is being studied.

Comment B-10
(Comment 7 on Methodology)

In conjunction with the statements of items 5 and 6 above, we remark that an important aspect of WASH-1400 was to consider highly remote events including those with an occurrence probability less than those of the design basis accidents used for licensing purposes. If the GA study wishes to limit the study to considerations of the licensing design basis accidents, this should be explicitly stated.

Response

In responding to this comment, the following interpretation has been made as to what is meant by the comment: the WASH-1400 study included many highly unlikely events or, at least, many more than were included in the AIPA study; therefore, the AIPA study may be deficient and may not be compatible or comparable with WASH-1400. It is hoped that this interpretation is consistent with the reviewer's intent.

While some aspects of this comment appear to be correct, the AIPA study team is not in complete agreement with it. While it is acknowledged that the WASH-1400 study did include many highly remote event sequences with annual occurrence rates significantly lower than those of licensing design basis accidents, it of course did not include a detailed evaluation of all conceivable highly remote sequences. This style of analysis follows from the basic philosophy of applying probabilistic risk assessment -- that there is a need to include in the detailed evaluations only those sequences that have a significant impact on the risk assessment; there is no need to include in such detailed analyses the vast array of sequences that can be conjured up by the virtually limitless nature of the human imagination. This same philosophy was applied in the AIPA study, resulting in the inclusion of many highly remote event sequences and the exclusion of many others from detailed evaluation. This basic philosophy of applying probabilistic risk assessment resulted in the adoption of an essentially stepwise analysis approach that screened out the relatively less important

and sometimes highly remote event sequences in the non-detailed analysis before progressing to the detailed level of evaluation. It is believed that this analysis approach has resulted in an efficient identification and evaluation of representative accident sequences that define the risk envelope for HTGRs.

While the screening approach mentioned above did result in the elimination of a majority of the highly remote accident sequences that do not significantly contribute to the risk envelope, the logic tree format utilized in both the WASH-1400 and AIPA studies necessitated the inclusion of many such sequences in both studies. As a result, the WASH-1400 and AIPA studies were not limited to the considerations of licensing design basis accidents -- not in the spectrum of initiating events and not in the treatment of event sequences. For the AIPA study, this meant that: (1) initiating events were chosen to be representative occurrences which include every source of radioactivity in the plant (WASH-1400 did not explicitly employ this approach); (2) the sequence of events, including both high and low probability initiating events, accounted for potential multiple failures which led to remote events and were certainly not "licensing design basis accidents."

The study described in the Status Report was performed in two stages, both of which went beyond "licensing design basis accidents." In the first stage (Preliminary Study), seventeen categories of initiating events were reported. (Investigations discussed briefly in Section 3 of Volume III considered even more sequences.) Treatment of multiple branches in the event trees for these categories resulted in 268 branches. The occurrence frequencies of the 17 events ranged from 1×10^{-1} to $1 \times 10^{-10} \text{ yr}^{-1}$. The latter plus two initiating events at 2×10^{-6} , one at 1×10^{-7} , and one at 7×10^{-9} are most certainly remote events. Similarly, in the second stage (Phase I) of more detailed examination, earthquakes of SSE and 1.5 SSE intensity were considered to have frequencies of 1.5×10^{-8} and $1.3 \times 10^{-9} \text{ yr}^{-1}$, respectively -- again, remote events. Even the modified occurrence rates [see Comment B-4 (Comment 4 on Methodology)] have frequencies of about 9×10^{-7} and 2×10^{-7} per year, respectively -- still remote events.

To elaborate, there were a number of initiating events considered in the preliminary analysis (Volume III) that are not licensing design basis accidents. The most significant of these events was the ejection of the entire closure in large penetration in the PCRV top head (involving an area greater than the 100 sq in. considered for the design basis depressurization accident. This event goes far beyond any of the HTGR licensing design basis accidents. Further evidence that the AIPA study goes beyond considering design basis events can be found throughout the preliminary and Phase I analyses. The event and fault trees allow a multiplicity of failures -- in sharp contrast to the use of the single failure criterion in licensing analyses of design basis accidents. This allowance for a multiplicity of failures can lead to conditions of unrestricted core heatup transients (comparable with the core melt conditions analyzed in WASH-1400 for LWRs) -- conditions that have not been considered as design basis events in the licensing process because of the application of the single failure criterion. The earthquake analysis described in Volume IV provides still further evidence in that it was extended to cover earthquake-induced ground accelerations ranging from those of the SSE to twice that level -- which goes far beyond the licensing design basis consideration of SSE accelerations as an upper limit. It should also be pointed out that a large number of the event sequences analyzed during the preliminary and Phase I analyses had very low occurrence rate estimates that were expressed in terms of the symbol " ϵ " (representing occurrence rates of 10^{-10} yr $^{-1}$ or less) -- substantially below the occurrence rates traditionally associated with design basis accident sequences used for licensing purposes.

To facilitate greater appreciation of the scope of the AIPA study, three different aspects of the AIPA study are specifically pointed out. First, one of the relatively most important accident sequences developed and analyzed during the AIPA study is the sequence identified as LOSP-BD, a sequence that results in an unrestricted core heatup transient. This scenario does not constitute a licensing design basis accident for the HTGR

any more than the core melt condition analyzed in WASH-1400 constitutes a licensing design basis accident for LWRs. Second, the steam ingress accident, which is regarded as a design basis accident for licensing considerations, was found to be of relatively low importance (by virtue of relatively low values of both consequence and probability) and, as a result, was the subject of a preliminary design option study, as reported in Volume III. Third, the AIPA Status Report describes the status of an ongoing study, as noted in Volume I. The study is not yet complete; further work is required in identifying additional initiating events for inclusion in the study, performing the associated preliminary analyses for the added initiating events, performing more detailed analyses (Phase I style) as deemed appropriate from the preliminary screening, and extending the analysis detail in the specific areas of interest as identified by the more detailed analyses.

As has been indicated above, the AIPA study has also considered many very remote accident sequences, including a large number with occurrence rates considerably less than those of licensing design basis accidents. The study resulted in the identification of the unrestricted core heatup transients as one of the principal contributors to the risk envelope for HTGRs -- a situation comparable with the identification of the core melt condition in WASH-1400 as one of the principal contributors to the LWR risk envelopes. In the realm of these considerations, it is concluded the WASH-1400 and AIPA studies are reasonably compatible and comparable.

Comment B-11
(Comment 8 on Methodology)

It has not been adequately demonstrated that all of the three events selected for the detailed analysis presented in Volume IV are the proper candidates upon which to concentrate the major efforts of the study. This difficulty may stem from the problems in methodology stated above or be the result of initial judgment. For example, we suggest that the reheater tube leak event should be considered as an anticipated abnormal occurrence since it has a rather well defined and limited consequence. In our judgment this would be sufficient reason to eliminate its consideration for detailed study so that other initiating events of competing concern would not be obscured.

Response

It was, of course, necessary to rely on some initial judgments to permit scoping of the quantitative analyses to conform with the practical constraints of time, manpower, and funding. However, in recognition of the potential for biasing the study by such judgments, provisions were made for periodic reviews of the study by ANC and a European-based consultant with a broad background in the operation and safety evaluation of GCRs. Other reviews have also been requested for this same purpose, and the comments to which these very responses are directed are a result of such reviews.

The fundamental motivation for applying probabilistic risk assessment is to develop a realistic quantification of the risks associated with events, equipment, and operator performance. In so doing, the AIPA study team attempted to provide an unbiased ranking of risks in several stages, as described in Volume III, and as reiterated below:

1. Identify all sources of radioactivity in the plant.
2. Identify all means by which release could occur.
3. Identify all mechanisms (initiating events) that could trigger the release.

4. Perform coarse screening to identify initiating events which appear important enough to warrant a formal coarse-cut risk analysis.
5. Perform formal coarse-cut risk analysis.
6. Select high risk events for detailed study.

In addition to those events which survived such screening, designated design basis events, which had been studied extensively in the past based on the judgments of experts, were carried even though their associated risk appeared low. From this screening process, events of "competing concern" were treated as objectively as possible, and no important sequences were "obscured": the small reheater tube leak emerged as an event of higher relative risk than one might have suspected.

As Fig. 2-14 of Volume III (repeated as Fig. 4-1 of Volume IV) indicates, the reheater tube leak (event 9) shares the highest risk with loss of offsite power leading to core heatup (event 7), and earthquake (event 6). This was deemed (a mutual decision by ERDA, ANC, and the AIPA study group) to qualify reheater tube leak events for the more detailed Phase I analysis. It is only in the next lower risk band that the following additional events occur: moisture inleakage (event 8), gas waste tank rupture (event 6), and drop of solid waste container (event 13).

The AIPA study group, as well as the majority of the attendees of the February 1975 AIPA presentation in Germantown, including representatives of ERDA, ANC, and ORNL, believed that the three initiating events selected for detailed analysis in Volume IV represented the most important initiating events in the original list of seventeen analyzed in Volume III.

It is seen, therefore, that retaining reheater tube leak for the follow-on, detailed examination phase did not exclude or "obscure" events of competing risks. Therefore, it would not be proper to merely accept such events as anticipated abnormal occurrences in the plant design because

this approach would undercut the overall philosophy of attempting to optimize the plant design and use of resources while maintaining minimum risk to the health and safety of the public.

Comment B-12
(Comment 1 on Consequences)

We agree that the King-Furnace experiment is not adequately representative of the iodine release that could occur in the event of a core heatup accident. Further experiments are necessary that more realistically represent the potential conditions in the release paths from the core to the environment in the event of a core heatup event. This research should consider the various types of credible release paths through the PCRV and containment and the self-heating effects of iodine plated out on the surfaces of the paths.

Response

The AIPA study team is pleased by the concurrence of this reviewer with the R&D recommendations concerning the fission product release and plateout characteristics that were identified in the AIPA Status Report. While it is believed that useful data have been obtained from the King furnace experiments for the plateout of I, Cs, Sr, Rb, and Ba on some materials, it is also recognized that a need exists to expand upon this data base by obtaining adsorption data for some PCRV materials for which no such data exists, obtaining critical surface concentration data for volatile species, and extending existing sorption data to cover a greater temperature range. There are other comments that also question the applicability of the King furnace data to core heatup events.* The immediate response to those comments entails the development of PCRV plateout characteristics that are a compromise between those developed from the King furnace data and those derivable from data published for the Windscale incident. These modified plateout characteristics have been developed to enhance the credibility of this aspect of the study and are to be used in the interim until better data become available. The long term response to those comments is embodied in the implementation of the R&D recommendations

* See Section 3.2, Comment 1 from cover letter, and Section 3.3, Comment 5, and responses thereto.

identified in the Status Report. These include further integral-type experiments that better simulate expected reactor conditions (currently in the planning stage) and the development of an improved computer code based on a more adequate temperature- and flow-dependent model of plateout (currently in progress). A comparison between the experimental data and the results from the new computer code is expected to indicate whether there is a need for kinetic data to describe adsorption, chemical reaction, and desorption under core heatup conditions. It is expected that the self-heating effects caused by radioactive decay after plateout can be taken into account by the new code by using an iterative approach and that these effects can be simulated in integral model experiments by additional external heating. ERDA-funded research is also under way to consider additional potential release paths resulting from failures of equipment (such as relief valves) and potential thermally induced breaches of such fission product barriers as the PCRV and liner during core heatup events.

Comment B-13
(Comment 2 on Consequences)

We support continued work on consequence models as GAC has centered attention on external doses and neglected aspects considered important in the WASH-1400 report (e.g., respiratory impairment). When the computer program associated with the consequence calculations for WASH-1400 becomes available we would support the statement made in the meeting that its use for the HTGR study be explored. We caution that the fission product source terms, release times, and transport mechanisms will differ in detail from LWRs for the controlling accidents but agree that a consistent comparison with LWR consequences would be a desirable goal. Should the differences in the physical mechanisms between the controlling accidents for LWRs and HTGRs not permit direct use of the WASH-1400 computer program we would recommend that any special HTGR program that might be developed incorporate, as a minimum, the biological models of the WASH-1400 study.

Response

The AIPA study group is pleased with the reviewer's continuing support of the development of realistic computer models for radiological consequences and has intended for some time to explore the possible use, for the AIPA study, of the computer program used in the WASH-1400 consequence calculations. Ultimately, a consistent comparison with LWR consequences would certainly be a desirable goal and is one that GA is working toward. Initial steps have already been taken along such lines. Several source terms for typical accidents considered in WASH-1400, together with fixed meteorological conditions, were selected by mutual agreement of the AIPA study team and NRC/RSR for comparative dose determinations using the computer programs used for the WASH-1400 and AIPA studies. In addition, several hypothetical HTGR source terms were submitted by the AIPA study team for inclusion in the comparative evaluation. The numerical evaluation of these two sets of source terms with the program used for the AIPA study

was completed, and these results were transmitted to NRC/RSR in July 1976 for comparison with the results obtainable from the programs used in the WASH-1400 study.

For the AIPA Status Report, as well as continuing studies, consequences for the HTGR risk envelope are routinely evaluated for both external exposure and aerosol inhalation exposure pathways. Specific dose categories estimated are:

External exposure

1. Whole body gamma dose
2. Whole body skin beta dose

Internal inhalation organ commitment

1. Thyroid gland
2. Skeletal bone
3. Lung
4. Gastrointestinal tract
5. Total body

Our studies to date indicate that the overwhelming damage mechanism in the HTGR risk envelope is correlated to external exposure whole body irradiation, with the inhalation exposure pathway producing secondary consequence effects. However, future work (in particular, that related to PCRV plateout) may alter the relative importance of all significant exposure pathways and the associated health consequence effects.

It is recognized that the WASH-1400 study is undoubtedly the most thorough and complete compendium of reactor accident studies for LWRs sited in the U.S. However, it is felt that total incorporation of all biological

models used in the WASH-1400 study for HTGR use is inappropriate. For example, the relatively low levels of exposure in the postulated HTGR accident sequences, considered over a meaningful range of frequencies, preclude early high-level threshold dependent effects [e.g., respiratory impairment (SD_{50} of 4500 rads), bone marrow related mortalities ($LD_{50/60}$ of 510 rads, etc.]. On the other hand, the late somatic consequence effects modeled in WASH-1400 are inappropriate for incorporation into HTGR consequence models.

Comment B-14
(Comment 3 on Consequences)

We comment that the estimation of consequences for the controlling HTGR accidents will depend on final design choices and operational technical specification as well as various analytical models and associated experimental data. In this regard we notice that in Volume VI, GA discusses the R&D importance of fission product source and transport mechanisms and provides a priority listing of recommended safety R&D in Table 6-6 based on uncertainties in data. The study should also reflect those uncertainties in the consequence model that reside in uncommitted design choices and those technical specifications that may not be fully developed until the plant is close to operating. This aspect of the uncertainty should be made clear by GA and a statement provided that recognizes the consequence analysis as tentative and subject not only to R&D findings but also to final design choices and operating requirements.

Response

With regard to the statements dealing with the effects on consequence assessments of changes in design and in operational technical specifications, it should be noted that such considerations can take on two basic forms: generic (fundamental) and specific (detailed). For the generic case, the big effects result from such characteristics as the use of graphite fuel, helium coolant, and the PCRV. Since the major advantages achieved by these characteristics would not be designed out, the AIPA study provides a proper representation of the effects of these major characteristics. For the case of specific, detailed changes, it is recognized that there may be an impact on risk assessment. However, the nature of that impact must be identified and clearly understood. In general terms, it is believed that this kind of impact would be rather small. It would not be appropriate, for example, to incorporate the effects of a multitude of potential design and operational variations into a single risk assessment

in terms of an evaluation of a single overall uncertainty factor. The reason for this is that the probabilistic risk assessment is intended to be applied to a single design/operational configuration, and this was an underlying consideration in both the AIPA and WASH-1400 studies. Both studies made use of one plant of each type as the basis for all probability and consequence models -- the HTGR reference plant in AIPA, and one BWR (Peach Bottom, Unit II) and one PWR (Surry Power Station, Unit I) in WASH-1400. Neither study assessed the modeling uncertainties associated with specific design, constructional, or operational deviations of various other plants from the plants used as the bases for the analyses performed. However, it would be appropriate to assess the changes in the risk assessment (both median and uncertainty factor) that occur as a result of various design and operational changes. The AIPA study team is also interested in assessing the effects of various design and operational options on risk assessment and is actively performing risk assessments for various options being considered in the current Lead Plant design efforts.

It is well known that predictions (both probability and consequence) may not be exact. It was the recognition of that fact that prompted the inclusion of uncertainties within the framework of the probabilistic assessment used in the AIPA study. The analysis presented in the AIPA Status Report represents the best efforts based on the reference plant design. As the design is changed, the risk assessment must be changed, but the changes are not expected to be significant.

It is desirable to clarify the extent to which the R&D priorities are based on data uncertainties. An initial screening (which is a coarse form of importance ranking) was performed during the preliminary analysis where uncertainty was not assessed. The screening was based solely on the preliminary median estimates of probability, consequence, and risk. During the Phase I analysis, uncertainties became a consideration, but certainly not the sole consideration. It should be noted that the priority ranking shown in Table 6-6 of Volume VI is determined solely by the values of the

index of R&D significance. According to Eqs. 3-7 through 3-10, the index depends on several parameters:

1. Median dose
2. Dose conversion factor
3. Uncertainty ratio
4. Organ considered
5. Organ health effect factor
6. Event sequence considered
7. Event sequence probability

Thus, uncertainty is only one of many considerations involved in the rankings developed in Volume VI. In addition, Table 5-7 of Volume IV shows that uncertainty is not the sole basis for the ranking (order-of-importance listing) of the general R&D recommendations since they are not listed in order by uncertainty factor. They are listed in order of decreasing values of median risk and of upper 95% risk (the latter depending on both the median and the uncertainty factor). This is deemed a much better use of the results in establishing the relative importance of sequences. The use of uncertainty factors alone is not a valid approach since that would ignore the very important consideration of absolute placement on a P-C (probability versus consequence) plot.

Comment B-15
(Comment 1 on R&D Recommendations)

Safety R&D recommendations are summarized in Volume IV, Table 5-8 in a priority sequence based on the quantitative risk information developed in the study. The priority assignment considers the degree of uncertainty in either the probability or the consequence factor in the risk equation, and ranks high for R&D attention those programs having the greatest potential for success in reducing the uncertainty. While we believe this approach to be supportive to the overall selection of safety R&D programs for HTGRs, we hold the position that this method should be supplemental and confirmatory to the more traditional bases for selecting R&D priorities. Our position in regard to the "Decision Criteria" for HTGR Safety R&D is given in Enclosure 2.

Response

It has long been recognized that the use of probabilistic risk assessment techniques provides insights and perspectives not otherwise attainable. The use of such techniques spans a broad spectrum of practical applications, ranging from high level management decision-making to such technically oriented tasks as establishing optimum policies for stocking spare parts. It is believed that the same qualities that have made the probabilistic approach so effective in these other areas will prove to be indispensable in providing a practical, objective, and realistic basis for identifying R&D needs.

Of course, the use of probabilistic techniques in identifying R&D needs is currently in the early stages of development. For this reason, these techniques may not assure completeness in the identification of safety R&D topics. It was the recognition of this fact that was responsible for the inclusion of the last of the R&D decision criteria,* thereby

* AIPA Status Report, Volume I, Section 8.

including specific provisions for the use of more traditional techniques. (It is not entirely clear from the comment what the "more traditional bases" are, but they certainly are not risk. They are probably more closely linked to an understanding of accident phenomena and an assessment of the potential for improvements, based on the judgments of experts. It is within this context that the last of the R&D decision criteria provides for the use of more traditional techniques.) However, it is anticipated that the AIPA methodology will be improved to the point that supplementary ideas will be provided by traditional methods and, ultimately, that the AIPA type of approach will stand alone as the sole source of identification of R&D needs.

While it is true that the risk assessment methodology may not be all-encompassing at this stage, it is recognized that the AIPA methodology offers now (even at this early stage of development) some unique characteristics and capabilities not otherwise available. For example, this approach provides a quantitative assessment of risk (rather than a qualitative, subjective assessment of risk) that consists of two different, but equally important, facets:

1. Medians
2. Uncertainty factors

The medians provide best estimates of probabilities, consequences, and risks, while the uncertainty factors provide an indication of the degree of accuracy that can realistically be ascribed to the median estimates. Both facets are important in establishing a meaningful quantitative basis for some form of sorting or ranking according to relative importance with respect to public safety and environmental concerns. The results can be displayed in such a manner as to encourage objectivity (rather than subjectivity) in the overall review of the results and in the derivation of R&D recommendations. Also, the AIPA methodology is exploring a quantitative basis for establishing the relative importance of individual parameters.

Another important point that should be made is that the R&D decision criteria are intended for use primarily by ERDA, but can be of significant importance to NRC, too. The NRC interests in R&D are presumably focused in two areas: (1) the development and updating of safe (conservative), enforceable licensing and regulatory positions and (2) efforts devoted to the demonstration of compliance (or noncompliance) with licensing and regulatory positions. On the other hand, ERDA attention is focused on several areas of interest and concern: (1) identifying areas of potential key public safety and environmental concern, (2) determining a priority ranking of the areas of potential R&D interest, (3) investigating areas of highest ranking to identify the important contributors to safety and environmental concern, and (4) developing the technology necessary to resolve these concerns. Implementing this last item entails the development of a broad, realistic base of technical knowledge (by testing and analysis) to provide necessary design feedback information and data. Also included is the investigation of human operator responses, particularly under the higher stress conditions that result from accident occurrences. Thus, while the AIPA approach may not be perfectly suited to the identification of R&D needs for NRC, it is believed appropriate for the needs of ERDA. Moreover, it is deemed possible to adopt the basic quantitative AIPA methodology, with some appropriate modifications, for use in identifying R&D recommendations for NRC.

Comment B-16
(Comment 2 on R&D Recommendations)

We find that the R&D listed in Table 5-8 is only partially consistent with R&D needs identified elsewhere (e.g., the Summit and Fulton Safety Evaluations). In the forthcoming GASSAR Interim Safety Evaluation the earlier safety R&D requirements and assessments of NRR will be updated and should be compared against Table 5-8 at that time.

Response

In view of the objectivity employed in deriving the recommended safety R&D in Table 5-8 as contrasted to the subjective selection of R&D by traditional means, it is not surprising that the respective recommendations are "only partially consistent." This contrast is consistent with experience within the AIPA study itself. Candidate R&D ideas were developed on a subjective, judgmental basis earlier in the Phase I effort prior to completion of the quantitative risk assessments. Following the quantitative evaluation of risks, many of the preliminary R&D candidates were not justifiable because the associated risk levels and uncertainties were not high enough. Although some of the candidates on the preliminary R&D list might produce benefits not related to safety, these were not included in the final list of R&D suggestions.

This is not to say that equipment developmental tests need not be performed. On the contrary, the AIPA analyses implicitly assume that normal, or usual, equipment tests and qualifications will be performed to assure that components and systems function as designed. However, the AIPA results indicate that the need for reliability data for such components and systems is not generally of high priority in safety research. Rather, it was found that the need for such information was justifiable only in a few cases where the event sequence was a relatively high risk branch and the uncertainty in component failure probability was relatively high.

On the other hand, the quantitative risk assessment did indicate that some areas of R&D recommended in licensing documents, such as the Summit and Fulton PSARs and SERs, are actually of low value with respect to reducing risk to the health and safety of the public. One example of this is research on steam/water ingress effects: the preliminary probabilistic risk assessment showed that the consequences associated with even the most remote sequence are minimal and the entire spectrum of possible branches is two or more orders of magnitude less than the risk envelope for all events. Because of the low risk associated with steam/water ingress, this area was actually selected for a design option study to determine if a more economic, but acceptably low risk, system could be established. (The elements of this study are reported in Volume III.) In this area, then, there may actually develop some inconsistencies with R&D needs identified elsewhere.

Similarly, the rapid PCRV depressurization based on a 100-sq-in. blowdown area (current DBDA) was shown by the probabilistic risk assessment to yield a risk of 3 to 4 orders of magnitude lower than the highest risk events. Since this event is postulated to occur by the failure of an ASME Section III, Class 1, pressure vessel closure, it is questionable whether such events should be considered in the design envelope and whether R&D resources should be expended for matters relating to such a low risk event. Here, too, the AIPA R&D recommendations will be inconsistent with those developed elsewhere, unless the latter are revised.

The fact that the GASSAR review may result in an update of the staff position on R&D is appreciated. Since the AIPA effort is continuing, the AIPA recommendations for safety-related R&D are also subject to revisions. Undoubtedly, there will remain between the respective recommendations fundamental inconsistencies of the types discussed above because of the differences in subjective, intuitive approach versus the objective, quantitative approach. Although the R&D recommended by the staff will most likely be performed as a requisite for licensing, it is hoped that the results of the AIPA study will influence such requirements to a small degree in the near term and to a greater extent over the long term as probabilistic risk assessment methods become more acceptable as a licensing tool and as the AIPA study, itself, achieves more widespread acceptance.

Comment B-17
(Comment 3 on R&D Recommendations)

The R&D topics listed in Table 5-8 are not described in sufficient detail to permit an adequate and comparative assessment with respect to other HTGR Safety R&D activities or recommendations. We suggest that the description be augmented to provide details comparable to those customarily supplied on Form 189.

Response

It was not intended that the R&D recommendations described in the Phase I Status Report be developed in the detail needed for a detailed work statement. They were meant to be preliminary, subject to more final recommendations to be developed subsequently. However, item 3, "HTGR Fission Product Release and Plateout Tests," and item 4, "Development and Verification of Fission Product Release and Plateout Codes," have been incorporated in FY-76 as a part of the Safety Research Task, ERDA Project Agreement No. 51. As such, they are fully described in the task work plan and in the test specifications prepared in FY-76 on 189a No. SU029.

REFERENCES

1. Fleming, K. N., W. J. Houghton, and V. Joksimovic, "Status of the AIPA Risk Assessment Study of High-Temperature Gas Cooled-Reactors," submitted for publication in Systems Reliability Service, Quarterly Digest, UKAEA (GA-A13970).
2. Bliss, C. E., et al., "HTGR Containment Design Options: An Application of Probabilistic Risk Assessment," ERDA Report GA-A14147, General Atomic Company, to be published.
3. Houghton, W. J., V. Joksimovic, and D. E. Emon, "Methods of Probabilistic Safety Analysis for Gas-Cooled Reactors," Trans. Am. Nucl. Sci. 21, 210-211 (1975).
4. Fleming, K. N., "A Reliability Model for Common Mode Failure in Redundant Safety Systems," Proceedings of the Sixth Annual Pittsburgh Conference on Modeling and Simulation, April 23-25, 1975 (GA-A13284).
5. Fleming, K. N., and G. W. Hannaman, "Common Cause Failure Considerations in the Prediction of HTGR Cooling System Reliability," IEEE Transactions, Special Issue on Nuclear System Reliability and Safety, August 1976 (GA-A13658).
6. Barsell, A. W., F. A. Silady, and V. Joksimovic, "Realistic Assessment of Consequences of HTGR Accidents," submitted for publication in Nuclear Safety (GA-A14038).
7. Apostolakis, G. E., "The Effect of a Certain Class of Potential Common Mode Failures on the Reliability of Redundant Systems," UCLA Report UCLA-ENG-7528, November 1975.
8. Okrent, D., "A Survey of Expert Opinions on Low Probability Earthquakes," UCLA Report UCLA-ENG-7515, February 1975.
9. Newmark, N. M., "Comments on Conservatism in Earthquake Resistant Design," paper presented at a meeting of the Ad Hoc Committee on Seismic Design Bases, Atomic Industrial Forum, Washington, D.C., September 18, 1974, Table 1b.
10. Hsieh, T., D. Okrent, and G. E. Apostolakis, "On the Average Probability Distribution of Peak Ground Acceleration in the U.S. Continent due to Strong Earthquakes," UCLA Report UCLA-ENG-7516, March 1975, pp. 13, 15, and 21.

11. Howell, B. F., Jr., and T. R. Schultz, "Attenuation of Modified Mercalli Intensity with Distance from the Epicenter," Bulletin of the Seismological Society of America 65, No. 3, (651-65) June 1975.
12. Hsieh, op. cit., p. 32.
13. Ahorner, L., and W. Rosenhauer, "Probability Distribution of Earthquake Accelerations with Applications to Sites in the Northern Rhine, Central Europe," Journal of Geophysics 41, 587, 592-3 (1975).
14. Hsieh, op. cit., pp. 1-2.
15. Gutenberg, B., and C. F. Richter, Seismicity of the Earth and Associate Phenomena, Princeton University Press, New Jersey, 1954, Figure 1.
16. Hsieh, loc. cit.
17. Ibid., p. 1.
18. Ibid., pp. 15, 21, and 22.
19. Richter, op. cit., pp. 448-452.
20. Hsieh, op. cit., p. 1
21. Richter, C. F., Elementary Seismology, W. H. Freeman and Company, San Francisco, 1958, pp. 448-452.
22. Newmark, op. cit., p. 7.
23. Okrent, op. cit.
24. Ahorner, op. cit.
25. Hsieh, T., op. cit.
26. Barsell, A. W., and M. B. Perroomian, "Consequences of Water Ingress into the HTGR Primary Coolant," General Atomic Report GA-A13171, April 1975.
27. ACRS Hearing on the Summit Power Station Preliminary Safety Analysis Report, USAEC Docket No. 50-450 and 451, Delmarva Power and Light, Denver, January 30, 1975.

3.6. COMMENTS FROM REF. 2 ON P. 3-1

Introductory Comments

The AIPA study which has been supported by ERDA and which serves as a basis for the proposed criteria as transmitted in Glen Newby's letter of March 18 to Herbert Kouts, is a valuable initial contribution to the quantification of public risks associated with HTGR systems. In view of the formative state of the HTGR technology, primary reliance on such a risk assessment for identification of needs and determination of priorities for safety R&D is premature. It would be appropriate for the AIPA study results to augment and to influence, but not at this time to replace, the more traditional procedures for identifying R&D needs.

Response

The recognition given in the introductory comments regarding the contribution that the AIPA study has made in providing a quantitative assessment of the public risk associated with HTGRs is appreciated. Although the study was initiated to provide guidance in identifying safety-significant R&D based on an evaluation of relative risks, it has become increasingly evident that the study has provided a major portion of an assessment of the risk to the health and safety of the public from the HTGR. It was this realization that led to the recognition that the AIPA and WASH-1400 studies are comparable in the sense of providing quantitative measures of public risk and eventually led to the R&D criteria as presented in the Newby letter, wherein the WASH-1400 results serve as a basic guide to risk acceptability and, hence, to the development of R&D recommendations.

While it is true that HTGR technology is, by itself, in a more formative state than that of LWRs, it should be noted that HTGRs do not stand alone as a totally new concept in reactor technology. The HTGR represents one specific form of GCR, a class of reactor systems for which considerable applicable experience data have been accumulated over a number of years

from the operation of GCRs in Europe and Japan -- particularly the Dragons and AGRs in England. Also, the experience data accumulated from the successful operation of the Peach Bottom reactor, a direct forerunner of the HTGR, are applicable to the HTGR, as are data from Fort St. Vrain. Furthermore, even though there are major differences in the reactor technologies applied in LWRs versus those of GCRs, it should be noted that the same fundamental technologies and conventional designs and operating characteristics are involved for the BOP in both types of reactor systems. Therefore, there is a wealth of experience data from LWRs and GCRs that are directly applicable to the BOP portion of the HTGR. Thus, it appears that adequate data bases exist for a quantitative risk assessment of both the NSS and the BOP portions of HTGRs.

It is agreed that the AIPA study results cannot, at this time, stand alone as the means of identifying R&D needs. It was the recognition of this fact that caused the inclusion of category 6 in the decision criteria (as transmitted in the Newby letter of March 18*), thereby incorporating specific provisions for the use of more traditional procedures for the identification of R&D needs. However, it is likewise recognized that the AIPA results provide sound, quantitatively based insights and guidance with regard to R&D needs in many areas that might not otherwise be attainable and definitely would not be reviewed by the licensing process. There are three factors that contribute to this condition. First, there is an adequate data base to perform a quantitative risk assessment for HTGRs, as discussed above. Second, the AIPA study was based on a reference design of sufficient detail to permit a meaningful risk assessment to be made. (This reference design was essentially the same as the Fulton and Summit applications, which were approved by NRC.) As a matter of fact, as design and operational changes were made during the course of the study, modeling and numerical modifications were made that reflected the impact of the changes on the assessments of probability, consequence, and risk. Third,

*A copy of the criteria as transmitted in the Newby letter of March 18 is attached at the end of this subsection as a convenience for the reader.

the study is based on a quantitative assessment of risk (rather than a qualitative, subjective assessment of risk) that consists of two different, but equally important, facets:

1. Medians
2. Uncertainty factors

The medians provide best estimates of probabilities, consequences, and risks, while the uncertainty factors provide an indication of the degree of accuracy that can realistically be ascribed to the median estimates. Both facets are important in establishing a meaningful quantitative basis for some form of sorting or ranking according to relative importance with respect to public safety and environmental concerns. Further elaboration on the subject of AIPA and traditional means for developing R&D recommendations is presented in the responses to the numbered comments, particularly comment 2.

Comment 1

The modification of the decision criteria to increase the number of categories should have introduced changes in categories 4 and 5 to reflect the insertion of category 3. The last sentence of category 4 might read: "Once the above objective is accomplished, one of the preceding criteria becomes applicable." Category 5 should refer to the first four categories.

Response

Agreed.

Comment 2

As noted above, the traditional modes of identifying R&D needs should not be debased at this time. Category 6 might read: "It is recognized that for GCRs risk assessment methodology may not assure completeness in the identification of safety R&D topics. Consequently, the method should be used as a supplement to, and in conjunction with, the more traditional bases for identifying and assigning emphasis to safety R&D topics.

Response

Much of the discussion presented in the response to the introductory paragraph is applicable to this particular comment as well. A few additional thoughts are presented below.

It has long been recognized that the use of probabilistic risk assessment techniques provides insights and perspectives not otherwise attainable. The use of such techniques spans a broad spectrum of practical applications, ranging from high-level management decision-making to such technically oriented tasks as establishing optimum policies for stocking spare parts. It is believed that the same qualities that have made the probabilistic approach so effective in these other areas will prove to be indispensable in providing a practical, objective, and realistic basis for identifying R&D needs.

Of course, the use of probabilistic techniques in identifying R&D needs is currently in the early stages of development. For this reason, these techniques may not assure completeness in the identification of safety R&D topics. It was the recognition of this fact that was responsible for the inclusion of category 6 in the Newby letter of March 18, thereby including specific provisions for the use of more traditional techniques. (It is not entirely clear from the comment what the "more traditional bases" are, but they certainly are not risk. They are probably more closely linked to an understanding of accident phenomena and an assessment of the potential for

improvements, based on the judgments of experts. It is within this context that category 6 provides for the use of more traditional techniques.) However, it is anticipated that the AIPA methodology will be improved to the point that supplementary ideas will be provided by traditional methods and, ultimately, that the AIPA approach will stand alone as the sole source of identification of R&D needs. Thus, while the proposed change in wording of category 6 may be appropriate today based on the current status of the probabilistic risk assessment methodology, it is anticipated that such wording will become obsolete in the future as improvements in the technology take place. It was the intent of the authors of the criteria that they provide both short-term and long-term guidelines without the need for modification in the future.

While it is true that the risk assessment methodology may not be all-encompassing at this stage, it is recognized that the AIPA methodology offers now (even at this early stage of development) some unique characteristics and capabilities not otherwise available. For example, it provides a quantitative (rather than qualitative) measure of importance, and the numerical results are displayed in such a manner as to encourage objectivity (rather than subjectivity) in the overall review of the results and in the derivation of R&D recommendations. Also, the AIPA methodology is exploring a quantitative basis for establishing the relative importance of individual parameters.

Another important point that should be made is that the decision criteria as presented in the Newby letter are intended for use primarily by ERDA, but can be of significant importance to NRC, too. NRC interests in R&D are presumably focused in two areas: (1) the development and updating of safe (conservative), enforceable licensing and regulatory positions, and (2) efforts devoted to the demonstration of compliance (or noncompliance) with licensing and regulatory positions. On the other hand, ERDA attention is focused on several areas of interest and concern: (1) identifying areas of potential key public safety and environmental concern, (2) determining a priority ranking of the areas of potential R&D interest, (3) investigating

areas of highest ranking to identify the important contributors to safety and environmental concern, and (4) developing the technology necessary to resolve these concerns. Implementing this last item entails the development of a broad, realistic base of technical knowledge (by testing and analysis) to provide necessary design feedback information and data. Also included is the investigation of human operator responses, particularly under the higher stress conditions that result from accident occurrences. Thus, while the AIPA approach may not be perfectly suited to the identification of R&D needs for NRC, it is deemed appropriate for the needs of ERDA. Moreover, it is deemed possible to adopt the basic quantitative AIPA methodology, with some appropriate modifications, for use in identifying R&D recommendations for NRC.

Comment 3

The criteria are quasi-quantitative and, as written, are not unambiguously interpretable. In category 1, the meaning of the adverb "clearly" should be quantified, and the definitions of key concerns should be explicit in the interpretation of the phrase "could potentially constitute a public risk on the order of or greater than . . ."

Response

The AIPA methodology constitutes an evolving technology that is currently being developed to enable the identification of R&D recommendations for ERDA. In developing this technology, it was decided to proceed in several steps, starting at a broad, general level and proceeding toward greater detail. The development of the six decision criteria represents one of the steps in this evolutionary process. These criteria were agreed upon by ERDA-NRA, ANC, and the AIPA study team and provide a basic, fundamental framework for the application of the risk methodology. By virtue of the intended fundamental nature of these criteria, it was deemed appropriate to express them in a qualitative (or "quasi-quantitative") format to provide overall guidance. They should not be confounded by including potentially complex (but complete) quantitative rules that may not be widely understood. It was deemed wiser to obtain concurrence regarding the general criteria and then proceed to develop and obtain approval of detailed quantitative guidelines for implementing those criteria as a separate extension of the more general decision criteria. The step involving the development of quantitative guidelines was initiated several months ago and entails the quantification of such terms as "clearly" and "on the order of." The overall intent of the general decision criteria seems perfectly clear, at least within the framework of the general guidance intended. Any ambiguities that are felt to exist are most assuredly associated with the details of quantitative application of the criteria, a situation that will be remedied as the quantification guidelines evolve.

Comment 4

The use of WASH-1400 (or other such overall reactor safety risk assessments) as a reference for R&D decisions should require that comparison values be based upon quantitatively equivalent analyses. There is no such requirement in the criteria and, as yet, no firm basis for believing that the AIPA study in its present form constitutes such an analysis.

Response

It is agreed that there is a need to base any comparison with WASH-1400 on quantitatively equivalent analyses. However, the meaning of "equivalent" has to be developed. One consideration is that specific HTGRs can be expected to deviate from the reference plant design used as the basis for the AIPA study, just as specific BWRs and PWRs are known to deviate from the one BWR (Peach Bottom, Unit II) and the one PWR (Surry Power Station, Unit I) used as the bases for the WASH-1400 study. Another consideration is that the consequence uncertainties evaluated in WASH-1400 include the effects of weather variations, whereas weather variations were not included in the consequence uncertainties reported in Volume IV of the AIPA Status Report (because the added effort of including weather variabilities would not have aided in developing R&D recommendations, and their exclusion did not hinder those developments).^{*} Of course, WASH-1400 is the only study available that is comparable to the AIPA study. The whole subject of assuring quantitative equivalence falls under the general heading of quantification of the criteria, which is discussed in the response to comment 3. It should be pointed out, though, that there is reason to believe that the AIPA and WASH-1400 results are reasonably comparable now and that they can easily be transformed to facilitate direct comparisons, as briefly discussed in the next paragraphs. This comparability of the results of these two studies served as an impetus for expressing the six decision criteria in the form transmitted by the Newby letter of March 18.

^{*}These differences are associated with differences in the AIPA and WASH-1400 study objectives, as discussed in Section 1 of Volume I of the AIPA status report.

However, the specific efforts entailed in performing direct comparisons with the WASH-1400 results were considered to be a part of the future evolution of the AIPA technology.

With regard to the previous comment that the AIPA and WASH-1400 study results are reasonably comparable now (the discussion of transformations to facilitate direct comparison is given later), it is generally recognized that the two studies employed the same theoretical and practical bases to perform probabilistic risk assessments. In spite of these elements of commonality, there are certain aspects of the analyses that tend to raise the question of comparability of the two studies. One of these is that WASH-1400 was based on two completed LWRs (namely, the PWR Surry Power Station, Unit 1, and the BWR Peach Bottom Atomic Power Station, Unit 2), while the AIPA study was based on the HTGR reference design of General Atomic as of the summer of 1975, which has not been built. It should be noted, however, that both studies deal with specific designs. The failure rates and other data used in analyzing the HTGR are based upon systems that will have been tested and checked out with an assumed success comparable to the testing and checkout of systems in the LWR plants. The AIPA analysis therefore does not require that the paper design be perfect but, rather, only requires that its checkout and resultant adjustments and corrections be as successful as has been the case at the LWR plants.

The greatest difference between the results of the two studies is in the predicted maximum consequences or radiation doses to the public. (There is a difference between these two concepts in the probabilities of getting the maximum doses, but not a major difference.) No directly applicable data exist for either concept to evaluate the maximum doses because the accidents leading to such doses have not occurred. Thus, the evaluation of the maximum doses was based on an extrapolation of existing experimental and operational data. The relatively low doses resulting from potential HTGR accidents are strongly influenced by the relatively slow heatup of the large graphite core and the significant plateout of many radionuclides inside the pressure vessel and the containment. The relatively large doses associated

with the LWRs are strongly influenced by the relatively fast melting of the core and the ability of the molten core to melt through the pressure vessel, coupled with the large energy and mass releases into the containment which result in a relatively high probability of major containment leakages. There is need for further study of the low-probability accidents that can lead to the maximum dose levels, but the difference in these doses for the two concepts appears to be the result of inherently large differences in the ease and speed with which the fission products can reach the public.

An associated element in predicting the probability of such severe events is the confidence with which the successful functioning of the safety equipment can be assessed. For a gas-cooled, single-phase cooling system during an accident, the confidence in the capability of the equipment to function according to design is high. In the case of the LWRs which have to provide cooling in a two-phase system during severe accidents, the confidence in the success of the system cooling function is reasonable, but significantly less than in the case of the HTGR. This is reflected in the WASH-1400 study in some of the probability numbers associated with the successful functioning of the emergency core cooling system. It may be noted that the problem of successful functionability is not eased by being able to tour a plant that has been constructed, but instead hinges upon tests in two-phase flow systems.

Design errors can cause failures of equipment under accident conditions. The approach to this problem in WASH-1400 was to examine equipment and drawings in relatively great detail to determine if there was a design error in the plant. The AIPA study team was told that there was one place, a low-pressure coolant injection system, in which there was a design error because both a logic "a" and a logic "b" had to make the same decision, although the design intent was that only one of those logics had to make the proper decision in order to have proper valve action. This approach is very close to a design review approach, which has the merit of finding specific problems and correcting them before the accident occurs. However, there is no accounting in the WASH-1400 study for those design errors which

were either missed by the analysts (a form of human error) or which could not be caught by the analyst in any event because only the conditions of an accident could reveal the deficiency. The AIPA study, of necessity, had a different approach to the problem of design error. It came about by means of developing an approach to common-cause failures which does not depend on highly detailed studies of plant systems. This approach is called the beta factor method, and the material presented in the AIPA report shows that about one-half of the common mode failures handled under the beta factor method are what could be termed design errors. Therefore, in this sense, all of the redundant systems in the AIPA study are given a decrement in their probabilities of success because of a generic possibility of design error, even though the specific errors are not identified by this technique. In this sense, then, the AIPA study is more complete and more general. The AIPA method provides a better evaluation technique but not as good a design review technique.

There are details of the analyses of probabilities which benefit from detailed examination of actual plants. An example of this is the analysis dealing with the reactor water storage tank on page II-114 of WASH-1400. The probability of vent plugging, which is given as 4×10^{-7} , was based on details that were only available from the specific actual plant. Before these details were known, the estimate was higher by an unknown amount. However, it is not apparent that this would have caused a significant change in the results or conclusions of the analysis because this probability would have to have risen by a factor of 1000 before the effect would be noticeable, even in the local portion of the fault tree where it appears. Additional examples could undoubtedly be found as to how the details of the analysis affected the probability predictions. However, no case is known in which these details caused a significant or major change in the risk envelope points or conclusions of the study.

Another major consideration that raises the question of compatibility of the two studies concerns the role of uncertainty bands. The comparison of the assessed risk between the two concepts is not precise because the

risk assessments have uncertainty. Estimates of the uncertainty have been made in each case. In typical cases of interest, uncertainties in risk may be roughly factors of 10, yet the ratio of risks between the two concepts can be 10^4 , which is sufficiently greater than the uncertainty to be meaningful.

There are cases where the uncertainty in the prediction of physical events is believed to be unusually large. Examples are the possibilities of steam explosions for LWRs and the major influence of plateout of many isotopes in HTGRs. The RSS will sometimes handle these as possible branches of event trees, while the AIPA study generally handles them as uncertainty bands on the dose predictions. But major risk contributors, such as the HTGR-LOSP-BD and the BWR-TW- γ , are not governed by such uncertainties. Prediction of earthquakes and their effects is another example of physical uncertainty; in this case, the RSS provides what appear to be conservative frequency predictions with only modest uncertainties, while AIPA attempts more realistic frequency estimates but derives much greater uncertainty bands. Both studies conclude that earthquakes are not governing in risk estimates.

Uncertainties in the probability predictions are included in both studies by considering uncertainties in failure rates. However, in AIPA fault trees, the common mode beta factors are additionally assigned uncertainties. This additional consideration may be one of the reasons that AIPA sequence probability uncertainties are greater than the uncertainties for rather comparable RSS cases. This is believed to result in a proper increase in uncertainty.

Resolution of uncertainties in the future may cause predictions of both studies to fall as well as to rise. Both studies have some elements of conservatism which can counteract some uncertainty effects which might otherwise raise risk estimates.

Another major consideration that gives rise to questions of comparability of the two studies concerns their completeness. The RSS tries to encompass all events that could lead to major releases of radioactivity, while the AIPA study includes the major kinds of events that the RSS does, such as pipe and vessel failures and transients. However, there may be a few other transients that yield a small additional contribution to the probability of a major release of radioactivity. Neither study has done extensive work on the failure of pressure vessel internals, though the AIPA study examined failure of the core support, subsequent to publishing the Status Report, and found it difficult to evaluate but not demonstrably important.

A much more explicit and extensive set of analyses has been done for the HTGR in the region of high probability and low consequence than has been reported for the LWR. This is important for the insight it provides into protection against those accidents which are expected to occur in the life of the industry.

Another question that arises with regard to study comparability is that of representativity. The studies tend to be used for representative reactor concepts. For example, the RSS multiplies an average assessment for the Surry and Peach Bottom plants by 100 to estimate the risk of 100 actual plants. The real plants are somewhat different, but the trends as well as many specifics of the study are believed to hold reasonably well.

Another difficulty with a representative plant, whether it be a design or an actual plant, is that it changes with time. The plant which is in the design stage would be expected to change more, but the actual plant will change from maintenance, improvements, and back-fitting. So the changes in both plants are relied upon to be properly handled by the designer, the utility, and the Regulatory reviewer, even though the design gradually departs from that given in the original PSAR or FSAR.

It seems clear that, within the understandings and insights discussed above, the AIPA and WASH-1400 studies are reasonably comparable now. However, the study results are presented in somewhat different forms. The P-C

(probability versus consequence) plots used in the AIPA analysis display points (with uncertainty bands) for each individual event sequence of importance. This form of display was adopted as being most compatible with the primary study objective of identifying specific areas where R&D might be appropriate. WASH-1400, on the other hand, displayed cumulative probability distributions for complete plants on their P-C plots. This was done in line with their basic objectives of assessing the risk associated with LWRs and of comparing that risk with the risks from other man-made and natural sources. Since the final results from the two studies are presented in somewhat different forms, it will be necessary to transform them into the same format to facilitate their direct comparison.

Comment 5

While the criteria, per se, do not address the determination of R&D priorities, discussions of their application have suggested that the figures of merit developed in the AIPA study for reduction of risk uncertainties would be used. Consideration should be given to the incorporation of a quantitative measure of the prospects for success of the research and development effort as a function of the magnitude of the reduction in the uncertainties.

Response

It is true that the subject of establishing R&D priorities is not specifically included in the five decision criteria as presented in the Status Report.* It is also true that some preliminary, exploratory forms of priority-type ranking were performed in the AIPA study to gain some needed insights in this area. (Evidence to this effect is given by the listing of accident sequences in order of decreasing median risk and of decreasing 95% risk level**; also, a listing of fission product and core chemistry parameters is given in order of decreasing values of the R&D index.***) It is likewise true that other aspects of the details of implementing the R&D recommendations presented in the Status Report were not discussed in that report. There are two basic reasons for this.

1. ERDA management prerogatives were involved in that certain aspects of the total study are to be performed by GA while others are reserved for ERDA.
2. It was recognized that there are a number of other factors, one of which is the potential for success of an R&D effort (mentioned in

* Volume I, pp. 8-2, 8-3.

** Volume IV, Table 5-7.

*** Volume VI, Table 6-6.

the comment), that should be included in the process of establishing R&D priorities. The inclusion of such considerations in an appropriate quantitative manner is regarded as part of the further evolution of the basic AIPA technology.

DECISION CRITERIA ON GCR SAFETY R&D

Six Categories

1. The risk is small enough that the accident can clearly be identified as not being of potential key public safety* or environmental concern.** NRA effort may be orientated towards evaluation of risk sensitivity in various safety-related plant options.
2. The risk appears to be large enough that such accidents would be of potential key public safety concern. Additional NRA effort should be expended on design options which would reduce either the likelihood and/or the potential consequences, and which can be practically implemented.
3. The risk appears to be large enough that such accidents would be of potential key environmental concern. Additional NRA effort should be expended to assure that sufficient safety information or design options exist to meeting licensing requirements.
4. The uncertainty in the risk is large enough that one cannot determine conclusively if the accident would be of potential key public safety or key environmental concern. Further NRA effort should be expended to investigate the primary contributors to uncertainty with an objective of reducing the uncertainty to an acceptable level. Design areas which might be changed to significantly lower the risk should be identified. Once the above objective is accomplished, either criterion 1 or 2 becomes applicable.
5. Certain additional Safety R&D efforts which do not fit into the first three categories but nonetheless have general application to risk analysis will be indicated and may be performed. The further development of common mode failure methodology is an important example.
6. It is recognized that, at this state of development, risk assessment methodology is not all encompassing. Hence, some additional Safety R&D may be identified on other bases such as judgement of experts.

*GCR accidents of potential key public safety concern should be taken in this context as including those accidents which could potentially constitute a public risk on the order of or greater than the current upper limits of the envelope risk values as given in WASH-1400 (or other such reputable overall reactor safety risk assessments).

**GCR accidents of potential key environmental concern should be taken in this context as including those accidents which could potentially constitute a public risk on the order of or greater than extrapolation of the current upper limits of the envelope risk values as developed in WASH-1400 (or other such reputable overall reactor safety risk assessments) to the low consequence region (exposures less than $\sim 10^4$ man-rem) where environmental rather than human effects are of primary concern.

4. AEROJET NUCLEAR COMPANY

The Aerojet Nuclear Company (ANC) has been under contract with ERDA-NRA to perform a continuing independent review of the methods and results developed during the course of the AIPA study. The following paragraphs are based on a report documenting the results of the ANC review (Ref. 1). Section 4.1 is concerned with their review of AIPA methods and technical work, Section 4.2 with using the AIPA results for absolute comparisons of risk, and Section 4.3 with their comments on the R&D decision criteria.

4.1. REVIEW OF AIPA METHODS AND TECHNICAL WORK

This section is subdivided as follows. Comment set A consists of responses to comments quoted from Section 2 of the ANC report. Comment sets B, C, and D are quotations of comments and resolutions as given in Appendix A of that report based on reviews of Volumes II, III, and IV, respectively, of the AIPA Status Report.

A. Comments from Section 2 of ANC Report

Comment A-1

General Atomic has also given considerable attention to an earthquake model to describe the probabilistic response of plant equipment to earthquakes. There is a considerable lack of data to input to the models developed by GA; therefore, considerable engineering judgment is necessarily applied in the use of those models.

Response

While modest attention was devoted to the modeling of earthquake occurrence rates as a function of earthquake size and to evaluating those rates based on experience data, it is not deemed appropriate to state that "considerable attention" was devoted to developing a model of equipment survival as a function of earthquake size. It was not practical to develop equipment survival models with the same level of sophistication possible for the earthquake occurrence rate model due, primarily, to a lack of the requisite data. Thus, it was necessary to develop a rather simplistic model of equipment survival that relied heavily on the judgment and experience of experts.

Comment A-2

The areas recommended for R&D are not a direct output of the analysis but are information voids, reflected as uncertainty values, encountered in the course of calculating plant risks.

Response

Two areas of clarification are worthy of mention in conjunction with the above comment. First, the development of R&D recommendations involved two considerations: (1) the identification of the event sequences having

the relatively highest values of median and R_{95} risk,* and (2) the identification of the parameters having the highest uncertainty contribution to the risk assessments developed for the sequences so identified.** The high contributors to risk as well as risk uncertainty identified in this manner were the subjects of the R&D recommendations.

The second area of clarification concerns the terms "uncertainty" and "information voids." The uncertainty values used in developing R&D recommendations provide a measure of the degree of incompleteness of the informational base entailed in the risk assessment process. The term "information void" carries with it the connotation of a complete absence of information, which is not appropriate for the AIPA study.

Comment A-3

The risk values derived from the AIPA thus far allows one to judge the relative importance of accidents associated with the HTGR. It has not yet been demonstrated that the AIPA results can be compared with WASH-1400 results without a clear understanding of the differences in the two studies. ANC has compiled a few examples from the WASH-1400 study group experience where meaningful comparisons of the two studies might be doubtful or compromised, but it is not yet known how significantly these examples contribute to the WASH-1400 risk values (see letter attached as Appendix B). Subsequent work will be done by ANC to determine whether limitations exist which preclude meaningful comparisons.

Response

The validity of comparing the AIPA risk assessment with other risk assessments (specifically WASH-1400) was questioned by another reviewer.***

* See the ranking of event sequences in order of decreasing risk in Table 5-7, Volume IV.

** See the identification of R&D recommendations by parameter uncertainties in Tables 5-2 to 5-6, Volume IV.

*** See Comment 4 in Section 3.6.

The response to that comment seems appropriate for the present ANC comment also.

It would appear that the ANC concerns about comparing the AIPA risk assessment with that of WASH-1400 are not too great. This observation follows from two considerations. First, in the two areas of the ANC report that deal with ANC comments and suggestions concerning the ERDA-NRA R&D decision criteria (namely, ANC report Section 4 and Appendix C, as covered in Section 4.3 below), no concern is raised with regard to the use of "... the current upper limits of the envelope risk values as given in WASH-1400 or other such reputable overall reactor safety risk assessments ..." as a basis for comparison and evaluation of "GCR accidents of potential key public safety concern"* Second, as noted in Section 4.3 below (which addresses Appendix C of the ANC report), ANC actually compares the AIPA results with an upper risk envelope developed from WASH-1400 to support ANC's contention that the safety R&D recommended in the Status Report is not justified.** Thus, it would seem that ANC regards the AIPA and WASH-1400 risk assessments to be reasonably comparable.

With regard to the examples cited by ANC (in Appendix C of their report) to illustrate the basis for their concern regarding the meaningful comparison of AIPA and WASH-1400 results, responses are given in Section 4.2 below.

* Volume I, Section 8. See also an updated set of R&D decision criteria (presented as an attachment to Section 3.6 herein).

** A direct response to the specific ANC comment is given in Section 4.3.

B. Comments and Resolutions from Appendix A of ANC Report
(on Volume II of Status Report)

The following set of comments and resolutions is quoted from Appendix A of the ANC report and deals with Volume II of the AIPA Status Report.

Comment B-1

Page 2-4, logic diagram: The lowest gate shown should be an AND instead of an OR. The OR implies only one segment barrier. Multiple barriers usually infer backup barriers; in the event one barrier doesn't hold the next one will. Multiple barriers can fail due to single causes, but these causes would be identified below the AND gate.

Resolution^{*}

GA believes that either type of gate can be useful. If their logical diagram were more detailed the AND gate would appear. The tabulated OR gate would then include those events that fail multiple barriers.

Comment B-2

Table 4-3 presents β and γ factors which, presumably, apply to redundant pairs of like components. An examination of 205 abnormal occurrence reports by ANC shows that 67% of those abnormal occurrences involved hardware failures, 5.3% were hardware failures involving multiple pairs of like components, and 5.3% were hardware failures involving unlike components. The GA method for treating common mode parameters appears to apply to non-like as well as like components, but the discussion and table are addressed to like components only.

Table 4-3 lists only five component types. Is GA striving to expand the list?

^{*}While the term "response" is used elsewhere in this volume, the term "resolution" is used here as a part of the material quoted from Ref. 1.

The discussion on β and γ factors is relatively simple and straightforward when applied to only two or three components as GA has done in the examples. In the fault modeling of real systems there are equally several hundred components which must be considered. Has GA considered the problem of selecting only important β and γ factors for inclusion in fault models in order to make the analyses manageable?

Resolution

β -factors have only been compiled for like components, not unlike components. GA recognizes the potential contribution to system failure probability by non-like components in redundant subsystem, but thus far has not done much with the methodology for treating them. Limited exposure to the problem by GA indicates that the non-like components may be relatively minor contributors. The pairs of unlike components would have to represent "cut sets" of the system before their inclusion into the models would significantly affect the results. It would be virtually impossible to compile β -factors for non-like components because the large number of possible combinations of components. GA is continuing to strive for more common mode data and a more extensive breakdown of components. β -factors are manageable in relatively large systems because the factors are only applied for a few of the active components, or a single composite β -factor is applied to represent several components. The application of the β -factor to large systems is demonstrated in Volume IV.

Comment B-3

Covert Dependencies (page 4-95): GA has developed a method for accounting for sequence dependencies based on β -factors because "it is inefficient and impractical to develop the models in sufficient detail to identify all the commonalities among branch point events explicitly." How are β -factors determined other than by analysis (explicit modeling)? Presumably if β -factors can be obtained at a system level, failure data at the system level are also obtainable. If data are available at the system

level, there is no need to evaluate the system to begin with. Although the methodology for handling covert dependencies among event sequences is logical, practical application of the methodology is doubtful.

Resolution

General Atomic and ANC agree that there are three basic methods of accounting for intersystem dependencies: (1) explicit modeling, (2) the use of β -factors, and (3) a combination of both explicit modeling and β -factors. GA observes that as the resolution of the analysis becomes finer and finer the analyst reaches a point at which even explicit modeling entails significant engineering judgment. ANC agrees with this observation. General Atomic's position is that at this point, the β -factor approach becomes more efficient and practical. Thus, the intersystem β -factors are not used when it is possible to evaluate dependencies by explicit modeling (e.g. common power supplies shared by two or more systems) but should be used to identify commonalities such as "operator performs incorrect maintenance on two or more components or systems." The β -factor at the system level was used in only one place and that was with the shutdown systems. This β -factor was determined using engineering judgment. Very little data exist to enable compilation of β -factors at the system level. General Atomic has recognized that the exposition in Volume II is easily misinterpreted and ANC feels the above explanation satisfactorily clears up the misunderstanding concerning the use of intersystem β -factors by GA.

Comment B-4

What is the ordinate of Fig. 4-11 on page 4-51?

Resolution

The ordinate of Fig. 4-11 is "cumulative number of estimates."

Comment B-5

How does one select the correct P_S in the discussion in the first paragraph, page 4-45? Apparently a value between 0.99 and 0.9999 is selected; then, the value is diminished according to the response model.

Resolution

ANC's interpretation is correct. The maximum P_S is assigned by engineering judgment.

Comment B-6

The guesses by "several experts" (ref: paragraph 4.4.5), which is the best that can be done without substantial data, will shunt out the mathematical rigor and reasoning of the preceding discussion. Also, there appears to be no basis for increasing those estimates by 10%. Why not 20% or 30%?

Resolution

Operator response data and the 10% increase for stress are based on judgment. However, judgment can provide good estimates, as shown in the Rasmussen Study (WASH-1400) and in a study conducted in England in 1971 (see Volume II, reference 4-22).

Comment B-7

The human operator discussion is devoted to the operator's responses in an accident situation. Nothing is said about systems that won't work because an operator or maintenance man left a couple of valves in the wrong position, miscalibrated instruments, etc.

Resolution

Human factor events such as valves being left in the wrong position, the miscalibration of instruments, etc., are not shown explicitly as events in the models; rather these types of events are considered in the assignment of data to component failure events and in the assignment of β -factors to redundant components.

Comment B-8

The discussion on earthquakes leads one to believe that the only earthquake damage to the plant will occur when the magnitude exceeds the safe shutdown earthquake (SSE). Although the plant may be designed to withstand the SSE, there are components which have failure probabilities which are a function of earthquake magnitudes less than the SSE. If the plant is designed to the SSE, then the region greater than the SSE represents a region where it is impossible to predict what will fail as a result of the large earthquake.

Resolution

GA stated their interest in component failure probabilities which are a function of EQ magnitude. They feel the phrase "impossible to predict" is not clear and that they do in fact predict failure probability as a function of EQ magnitude (including above SSE).

Comment B-9

Page 5-4, next to last paragraph. Shouldn't the second sentence read: "The path directly through the PCR V . . ."

Resolution

"The path through the secondary coolant" is the correct phrase.

Comment B-10

Page 5-8, first paragraph. It is not clear why "Significant doses occur before significant release of the fission product inventory."

Resolution

Only a small fraction (~1%) of the release need leak through containment before a "significant" dose occurs. Therefore, significant doses can occur before significant release of the fission product inventory occurs.

Comment B-11

Page 6-3, first paragraph in Section 6.2. Shouldn't "consequence plot" be changed to "risk plot" or "probability-consequence plot?"

Resolution

"Risk plot" is indeed the correct term. "Consequence plot" was erroneous.

Comment B-12

Pages 4-7 and 4-13. "Uncorrelated" is not equivalent to "independent." For example, the sum and difference of two positive, independent, identically distributed random variables is uncorrelated but not independent; knowledge of the sum provides an upper bound for the difference.

Resolution

General Atomic agrees, "Independent" is the correct term. Of course, this does not affect the results of the AIPA study.

Comment B-13

Pages 4-15 through 4-20. If two events are independent, they are not mutually exclusive; and vice versa. Equation 4-25 holds exactly under independence since the failure rate for minimum time to failure is the sum of the individual failure rates when the failures are independent. The error in equation 4-35 from neglecting simultaneous independent failures is small since the probabilities are small.

Resolution

GA agrees. The common mode model has wider applications since it does not need to assume mutually exclusive events.

Comment B-14

Figures 4-9 and 4-11, pages 4-48 and 4-51.

- a. What curve-fitting techniques were used to obtain the lognormal distributions from the points plotted?
- b. The "mean MTOR" label on Fig. 4-9 is incorrect. The mean of a lognormal distribution (as opposed to exponential) is rarely at the 63% point. The lognormal distribution describes the uncertainty in the mean time to operator response (MTOR); the median of the uncertainty distribution is the best point estimate of this parameter. Since the probability scale is not labeled on Figure 4-11, it is not possible to distinguish whether the median or the 63rd percentile was used to obtain the actual MTOR estimates 13.8 and 25.7.
- c. In view of the inaccuracy just mentioned, and the data value 300, the data point 3.25 and the MTOR estimates have too many significant figures.

Resolution

The curves in Figures 4-9 and 4-11 are an "eyeball" fit to the points plotted. GA concurs with comments (b) and (c).

Comment B-15

Figure 4-10, page 4-49. The notes on the lower part of the figure are not clear.

Resolution

The X-axis in Figure 4-10 is labeled in MTOR units, not in seconds. The ".693 X MTOR" note refers to the median of the exponential distribution -- the time required for half the operators to respond. When several actions are required from the operator to prevent an accident, the median response times and delay times provide an estimate of the total time required.

Comment B-16

Page 4-57. There is a minor units mistake in shifting from earthquake magnitude to log magnitude. If eq. 4-76 redefined F so that $F(\log m)$ is the distribution function for magnitude, Eq. 4-77 should read $G(e^x) = \exp(-\lambda[1-F(x)])$.

Resolution

GA agrees. This error has no effect on the results of the AIPA study.

Comment B-17

Page 4-69. The lognormal assumption for earthquake occurrence rates is questioned. The within-site lognormal assumption is a direct contradiction to the assumption on which the earthquake occurrence rate estimates

at specific sites were derived, namely, the assumption that the maximum earthquake in a given year at a given site is lognormal, since equation (4-80) defined a 1-to-1 relationship between the occurrence rate and the maximum earthquake distribution. Furthermore, there is a formal similarity between the final equation producing a smooth earthquake occurrence model (eq. 4-152 on page 4-78) and the equation giving the earthquake occurrence rate at a specific site (eq. 4-81 on page 4-59); eq. 4-81 takes on the same form as eq. 4-152 under the assumption that the maximum earthquake per year at a given site divided by the safe shutdown earthquake magnitude at that site is lognormally distributed with parameters M and Σ . The assumption just described is equivalent to the original lognormal maximum earthquake magnitude assumption if M and Σ are suitably defined. Hence, ANC questions whether there is some direct way to go from the within-site occurrence rate described by eq. 4-81 to the global rate described by eq. 4-152 without requiring the contradictory lognormal earthquake occurrence rate assumption.

Resolution

GA agrees that the within-site occurrence rate derived from the lognormal assumption on maximum earthquake magnitude per year is not lognormal (see the note on page 4-69). However, they regard the lognormal earthquake magnitude distribution solely as an approximation, and do not feel that further lognormal assumptions on the occurrence rate are contradictory. Instead, such assumptions are reasonable, for the reasons listed in the response to comment 13^{*} of the letter on Volume IV. Equation (4-152) was derived to smooth the data and to provide a check on the computations, and had no probabilistic interpretation.

* Comment D-13 below.

Comment B-18

Pages 4-70 and 4-74. ANC questions the earthquake occurrence rate variance estimates defined by equations 4-113 and 4-129. The N's in equation 4-113 are percentiles of the assumed lognormal occurrence rate distribution, yet they are obtained from equation 4-109 which just gives one-sided upper confidence limits for this rate. However, a more fundamental objection is that, since the earthquake occurrence rate estimate at each site was calculated from a knowledge of the maximum earthquake magnitude at that site over a period of "n" years, the sampling variance in that estimate should be solely a function of n and the variance of maximum earthquake magnitudes. The AIPA model assumes that the variance in that estimate should be solely a function of n and the variance of maximum earthquake magnitudes. The AIPA model assumes that the variance of the log of the maximum earthquake magnitude per year at a given site is known. Therefore, it should be possible, using Monte Carlo techniques, to propagate the variance through and observe its effect on the estimated occurrence rates.

Resolution

GA's approach to finding the variance estimate is based on the within-site and site-to-site lognormal earthquake occurrence rate assumptions. GA explains that using confidence limits for distribution percentiles is justified by a Bayesian approach to the problem, regarding the observed quantities as fixed and the occurrence rate as a random variable.

ANC has not been able to successfully apply the Bayes approach to the confidence limits given in (4-109), because of the lack of symmetry between the observed quantities and the occurrence rate in the distribution defined by that expression. ANC feels that it would be instructive to try the Monte Carlo approach.

Comment B-19

Page 4-77, equations (4-143) through (4-149). A t-test rather than a normal theory test should possibly have been used to obtain confidence bands from the mean and standard deviation estimates. This would result in smaller confidence intervals.

Resolution

GA feels that there is no definite sample of occurrence rates. ANC regards the four occurrence rate estimates obtained as a sample. In any case, GA's approach is more conservative.

Comment B-20

Page 4-107. What assumptions are necessary to justify equation 4-234 on the error factor of a sum of lognormal variables?

Resolution

Equation 4-234 is derived by assuming that the sum is lognormally distributed. The quantity in brackets is z times the standard deviation of the natural log of the sum, obtained by propagating the variances of the input probabilities using a Taylor series expansion. GA recognizes that the formula is only an approximation; the sum of lognormal variables is not lognormal.

Comment B-21

Page 5-9, Figure 5-3. What is "Q"? "W"?

Resolution

Q is the flow rate of helium escaping from the containment. Integrating Q with respect to time yields the fraction of containment

volume released, which, multiplied by radioactive concentration, yields the curies release rate. W is the flow rate through the open relief valve.

C. Comments and Resolutions from Appendix A of ANC Report
(on Volume III of Status Report)

The following set of comments and resolutions is quoted from Appendix A of the ANC Report and deals with Volume III Preliminary Results, of the AIPA Status Report.

Comment C-1

Many of the ANC comments expressed in a letter on March 5, 1975, to ERDA still hold. Sequence probability values have been generally increased; however, dependencies between event tree headings still exist and would increase sequence probability values even more. For example, the event tree for the PCRV purge header break (Figure 4-7) shows a probability of 8×10^{-3} for failure of the purge header isolation valves to automatically close and 7×10^{-3} for failure of the same valves to close manually. The combined probability for the two events is 6×10^{-5} , yet since they are the same valves the probability value can't be better than the failure probability value for the valves themselves which is about 10^{-4} (may vary somewhat because of demands on the valves). Similarly, electrical power dependencies exist in the column heading for that event tree and other event trees.

The danger in ignoring the above dependencies, even on the coarse cut evaluations, is the possibility of throwing out initiating events as being unimportant when indeed they may be more important than others which are retained. It is obvious that the purge header break is relatively unimportant because of the probability of the initiating event itself when compared to the BD sequence probability on the LOSP event tree. This can also be said about the other probability values of initiating events discarded, assuming the consequences are no larger than LOSP sequence BD (total loss of core cooling).

Resolution^{*}

ANC and GA agree that a complete evaluation of all inter-event dependencies in the preliminary analysis would have improved the analysis by which the three initiating events were selected for further, more detailed evaluation. GA states that the reasons that dependencies were not fully considered at this point in the analysis were based upon questions of scheduling, man-power, and budget. Aerojet's position is that inter-event dependencies are a very important part of prioritizing events. While ANC appreciates the difficulties involved in coordinating a large program under the various restrictions of funds, etc., it is felt that this omission could have important consequences on final R&D recommendations. Aerojet views its responsibility as review of the published documents and not of the AIPA program in all its aspects and thus feels that its comment must still be noted. ANC agreed with GA that for the particular systems under consideration intersystem dependencies do not obviously affect the final results of the work undertaken in Volume III. ANC's comment concerns the methodology in general and is recognized by GA as significant. Should this methodology be accepted as valid and applied to other systems this omission could appreciably affect the analysis. It is this concern that leads ANC to record its comment.

Comment C-2

Page 1-1, paragraph 2. The statement ". . . in the detailed studies described in Volumes IV and VI, that the preliminary results are generally within the range of uncertainties of the more rigorous analyses." In comparing the preliminary and Phase I event trees for the reheater leaks and LOSP, it appears that this statement is questionable. One should not infer that preliminary analyses suffice for the more rigorous analyses because a single oversight can change the results appreciably.

^{*} While the term "response" is used elsewhere in this volume, the term "resolution" is used here as a part of the material quoted from Ref. 1.

Resolution

General Atomic states that no such inference was intended. The term "generally" qualified this statement to a sufficient degree to make it not only innocuous but superfluous. ANC feels that GA's work in Volume III stands well enough on its own and statements like this are unnecessary, adding only confusion.

Comment C-3

Page 2-23, first paragraph. The statement "This (referring to rapid PCRV depressurization) is very unlikely because it is equivalent to the breach of an ASME Class 1 Pressure Vessel" is inconsistent with the Risk Summary Figure 2-14. The plot shows the probability of about 10^{-7} (much higher than most of the other events) yet with a relatively low consequence (much lower than most of the other events).

Resolution

GA said that "very unlikely" was meant in absolute, not comparative, sense. They agreed that an additional sentence making this clear and noting that the low risk associated with this event (not low probability of occurrence alone) was the important measure would have been appropriate.

Comment C-4

Page 2-25, first paragraph. The statement is made about ". . . deriving initiating events . . ., at a lower priority, to consider the failure of passive structures . . ." Although the structures are passive there are numerous potential credible single events that could because of their singularity dominate over redundant active component probability values.

Resolution

GA stated that they recognize the importance of passive structures in determining initiating events. ANC was informed by GA that subsequent studies are being considered to determine the behavior of the PCRVR and internals in order to help answer those questions raised in the comment.

Comment C-5

Page 2-20, Fig. 2-14. Events related to PCRVR structural failure (17) are not shown on the risk plot. Is this because of the uncertainty of this event?

Resolution

This event was inadvertently left off the plot. No significance should be inferred from this omission.

Comment C-6

Page 4-3, first paragraph. The 1/64 inch diameter line must be in error.

Resolution

GA claims dimension is correct.

Comment C-7

References 4-2, 4-3, and 4-4 (page 4-154) have been cited as sources of data for the AIPA. At this time these documents were published the data were, perhaps, the best that could be had. Generally there are now better data available, and some of these subsequent data differ appreciably from the earlier data.

Resolution

GA noted this opinion and agreed that in subsequent work they would attempt to utilize the newer sources for their data in these particular areas (notably, human operator response).

Comment C-8

Page 4-66, Figure 4-66. Consequences of branches X and Y; AA and AB; AK and AL; AM and AN are listed as identical. Does success or failure of containment isolation have any effect on consequences? The event "cooldown on CACS loops" has a probability of 1 associated with it in all cases. Does this always work? The consequences of branch AI are lower than the consequences of branch AC. This seems to imply that the consequences are greater if the main loops work.

Resolution

- a) GA made the conservative assumption that local burning of hydrogen will occur which would lead to containment leakage similar to that resulting from the failure of containment isolation. This conservatism was found to be invalid in the more detailed analysis of Volume IV.
- b) The branches associated with the failure of CACS were not included in the event tree since this would result in the state of loss of forced cooling. This state was considered in other event trees with more probable event sequences and thus was considered unnecessary to evaluate here.
- c) The consequences are indeed greater if cooldown is accomplished using the main loops as opposed to the CACS. However, using the main loops leaves the option of using the CACS open should the main loops fail. If the main loops are shutdown and cooldown attempted on CACS this redundancy no longer exists. The result is that while the consequences are higher the overall risk is lower.

Comment C-9

Page 4-38. The conversion was made from 2×10^{-8} failures/hr to to 2×10^{-5} failures/year. This should be 2×10^{-4} failures/year and results in the elevation of Branch C in Figure 4-16 into a risk level comparable to reheater leak branches.

Resolution

GA included an intermediate step in their conversion that was not noted in the test. The failure rate was reduced by GA to 2×10^{-9} failures/hr before the conversion was made in order to reflect the superior reliability of nuclear vessels over those non-nuclear vessels from which the failure data were obtained.

D. Comments and Resolutions from Appendix A of ANC Report
(on Volume IV of Status Report)

The following set of comments and resolutions is quoted from Appendix A of the ANC report and deals with Volume IV of the AIPA Status Report.

Comment D-1

The reasoning used to construct the LOSP event tree and particularly the relatively high probability of sequence BD suggest that considerable attention might be given to the identification of potentially new accident initiating events in the primary cooling circuit. The time before the upper thermal barrier failure (about 1-1/4 hours) is an important consideration in the sequence. If the barrier were to fail sooner (e.g., due to causes other than high temperature) credit might not be given for normal loop cooling, CACS cooling, etc. That is, sequence BD could be short-circuited by potentially more probable sequences. R&D should be addressed to answering questions such as what are all the stresses and limitations associated with the thermal barrier fasteners, can initial thermal conditions exist which might cause the thermal barrier fasteners to fail sooner, are there other potential sources of debris that could plug the core or circulator channels, etc.?

The R&D recommendations resulting from the AIPA (Ref: Item 1, Page 5-14) regarding the "Probability Model of Core Cooling" are not explicit, but the discussion in Paragraph 5.1.1, Page 5-10, suggests that the R&D be directed toward improving the prediction of CACS startup. Item 1 R&D should encompass the identification of new accident initiating events in the primary cooling circuit as discussed above.

Resolution^{*}

GA concurs with the need for completeness in the AIPA and has plans for doing more work pertaining to the cooling circuit in conjunction with the PCRV integrity work. GA acknowledges (see Volume I, Section 8) that identification of new initiating events is required.

Comment D-2

The two, three, and four significant figure uncertainty values shown throughout the report lead the reader to believe that the uncertainties for the sequences are well known. Many of the input uncertainties are engineering judgments which, at most, can be quantified to one significant figure. The sequence uncertainty factors should be rounded off to the significant figures known for the model input uncertainties.

Resolution

GA agrees with ANC's concern about the significant figures retained in uncertainty values.

Comment D-3

Page 4-34, paragraph 2. The statement about having sufficient time to repair critical primary system components because of the less than 10°F/min core temperature rise implies that all primary system components have been treated as repairable. Actually the circulators and their associated components would have to be considered non-repairable because of their inaccessibility.

Resolution

GA agrees that the statement referred to in Volume IV may be misleading. GA did not consider the repair of failed equipment within the

^{*}While the term "response" is used elsewhere in this volume, the term "resolution" is used here as a part of the material quoted from Ref. 1.

containment building during the period of "no forced cooling." Many of the active components which contribute the most to CACS unreliability are found outside the containment building (e.g. CAHE cooling water pumps, air blast heat exchangers, diesel generators, etc.)

Comment D-4

Page A2-13, paragraph 2. The paragraph describes briefly the equipment required in the start-up of the core auxiliary cooling system. It would appear that closure of the normal cooling loop shut-off valves would have to be included in the analysis; otherwise, significant CACS coolant would bypass the core through the inactive normal cooling loops.

Resolution

The main loop shut-off valves were included in the analysis of the CACS but were pruned from the resulting fault tree because of their relative unimportance. The probability of two or more valves failing to close on demand was assessed at 2×10^{-5} per demand, including the consideration of common modes. This value is a factor of 100 lower than that of the highest, and a factor of 5 lower than that of the lowest cut-set probability found in the CACS fault tree in Volume IV. ANC's only concern was that the shut-off valves were considered.

Comment D-5

Page A2-18, Table A2-3. The probability value given for a diesel generator failing to start is 2×10^{-3} . A comparison with WASH-1400 shows this value to be an order of magnitude low. BNWL-813 is cited as the source for the data (not listed in report references). Why the difference?

Resolution

GA believes the BNWL-813 data pertaining to diesel failure are more applicable to the HTGR situation because of the time available to utilize the manual startup capability in the HTGR plant. ANC agrees.

Comment D-6

Over the years standard fault tree symbolism has evolved which isn't being used by GA. The GA fault trees are thus confusing to outside organizations (see WASH-1400, Appendix II, page 18).

Resolution

On subsequent analyses GA will utilize more of the industry standards.

Comment D-7

Page A2-46, Table A2-8. A value of 2×10^{-4} is listed as the probability of common mode failure of the containment recirculation fan and motor. The Fulton FSAR indicates that these fans will not operate above 200°F. Can the release of primary coolant to the containment cause the containment temperature to rise above 200°F? If so, failure could be almost certain.

Resolution

The containment temperature can, as a result of primary coolant release to the containment, exceed 200°F. However, the fans won't actually fail due to temperatures in excess of 200°F. Thermal cut-off devices are used to shut the fans off at 200°F to protect the associated filters, but the fans will respond again once the temperature drops below 200°F. Sensitivity analyses reported in Volume VI show that the consequences are not affected by interruptions of filter system operation for several hours. ANC believes the GA explanation to be reasonable.

Comment D-8

Tables with earthquake occurrences per year express numbers such that the second digit beyond the decimal point is a subscript. We presume the subscript refers to a minus power of ten. Enough people have questioned its meaning that it should be explained below each table where it appears.

Resolution

The method used by GA to express earthquake occurrences per year is commonly used to express numbers which have a large number of repeating digits beginning at the right of a decimal point. GA believes that since the method is commonly used no explanation is needed. ANC agrees.

Comment D-9

R&D Item 5 on page 5-14 indicates that R&D should be applied to determine "...the operator's ability to correctly deduce the cause of the anomalous plant condition...". If there is some question about the operator's response, the possibility of automatic reheater isolation should be considered. The problem could be more related to what the instrumentation does than what the operator does.

Resolution

As regards to the question of whether this aspect of reheater isolation should be automatic, the first step is to consider the absolute value of the dose. A person would receive 4 millirem over a period of two months from this event. This is comparable to the doses from normal releases allowed in Appendix I of 10CFR50, and may be compared to the exposure from background radiation received by this same person in this period of about 20 millirem.

In addition, the small reheater leak is an example of a condition where the measurable variables may be within expected variations in background levels, and therefore it provides an excellent example of the need for human operators in the "control loop." To adopt automatic isolation using today's instrumentation would result in a considerable number of spurious isolations, which would then result in undesirable thermal stresses on the reheater and piping (decreasing safety).

The conclusion is that automatic isolation for small reheater leaks is neither necessary or desirable.

Comment D-10

Page A1-49. GA states "...When the intersystem dependencies are calculated based on common equipment, for example, the branch failure probabilities decrease." ANC commented that while this statement might be true for this specific event tree, branch failure probabilities often increase after intersystem dependencies are calculated.

Resolution

GA agreed that the statement could be misleading and does not adequately demonstrate GA's understanding of the problem. They recognize the limited applicability of this statement and ANC is satisfied that the statement did not result from faulty analysis. GA and ANC agree that the statement might be improved if the wording were "...when the intersystem dependencies are calculated based on common equipment, for example, the branch failure probabilities can decrease".

Comment D-11

Chapter 5. By GA's own definition, the risk median points of all the branches considered fell into a "safe" or "acceptable" area and thus none of their safety recommendations included studies of safety equipment

improvement. The R&D suggestions therefore included only ideas that reduce the uncertainty associated with the prediction of probabilities and consequences in those branches that had survived the preliminary screening based on median values (p. 5-3 of Volume IV). However, the error, or uncertainty, bands associated with these median points do not extend into a region on the risk plot significantly different from that already deemed "safe" by GA. Thus, reduction of these uncertainty bands would seem to result in little reward. The act of proving that a system is "safer than safe" is not a legitimate justification of funding. For example, the upper 95% uncertainty limit on risk for an earthquake branch is three orders of magnitude lower than the median point for their LOSP branch, which was defined as "safe."

Resolution

GA states that in some cases there are significant unresolved questions concerning the magnitude of the uncertainties presented in Volume IV. It is this "uncertainty on the uncertainty" that leads to the possibility that the risk of certain branches might extend into areas where R&D is clearly desirable. ANC agrees that some of the uncertainties are in fact greater than those cited (which often do not include uncertainties involved in methodology, etc.). ANC feels that some development in the future of these additional uncertainties would help justify the R&D recommendations made by GA.

Comment D-12

The use of the lognormal distribution to describe uncertainties in probabilities and rates throughout this study is open to question. Table 1 below lists a few examples.

TABLE 1

Item	Page Reference	Comments
LOSP Occurrence Rate	A2-3	Use of the χ^2 distribution to obtain an upper bound for λ_1 assumes λ_1 is constant.
$P(\bar{3} 2, 1)$	A2-7	SAMPLE's results show that $P_{50\%}/P_{5\%} \neq P_{95\%}/P_{50\%}$. Thus, the 90% interval cannot be precisely represented by a constant multiple and divisor of the median.
CACS Failure	A3-41	
Probabilities		
$Q(8 j)$ for branch AL	A2-37	Probabilities from .51 to .91 can be obtained, yet the uncertainty factor implies that the probability is close to .68.
$Q_6 = P(\bar{5}) + Q_E$	A3-35	In Table A3-19, earthquake uncertainty factors are used. However, if Q_E varies between Q_E/f and $Q_E \cdot f$, Q_6 varies between $P(\bar{5}) + Q_E/f$ and $P(\bar{5}) + Q_E \cdot f$, not $(P(\bar{5}) + Q_E)/f$ and $(P(\bar{5}) + Q_E)f$.

Resolution

GA explains that error propagation codes such as SAMPLE, together with the constraints of time and funding, suggested the use of a single family of distributions in the AIPA study. The lognormal family is a natural

choice - it lends itself well to describing small positive quantities, as a two-parameter distribution it is flexible enough to give reasonably accurate results, and it was used in the WASH-1400 study.

GA recognizes that the indiscriminant application of lognormal uncertainties can lead to problems, but maintains that any errors introduced by using the lognormal distribution are insignificant. Regardless of the true distributions of input branch point probabilities, the resulting branch sequence probabilities tend to be lognormal since the logarithm is the transformation that turns sequence probabilities into sums, and sums asymptotically tend toward normality. Furthermore, WASH-1400 researchers found that the 90% error spreads of output parameters were generally independent of the distribution choices for inputs due to the large data ranges of the input variables. GA is, however, working on modifications of the SAMPLE code to make it accept empirical distributions.

Comment D-13

Page A2-36, second paragraph. In the LOSP "event 6" analysis, the time to CACS failure is broken into several intervals (to permit the consideration of different consequences from different periods of core cool-down). However, this requires the use of a constant allowable outage time for each interval in event 8, in contrast to the continuous outage time function given in Figure A2-9. This leads to two questions:

- (a) Going to a discrete outage time function introduces error; to what extent does this error depend on the initial selection of intervals? What considerations went into the selection of intervals? (It is stated on page 4-35 that they are "arbitrarily selected").
- (b) Forcing a lognormal distribution to account for the error seems artificial. The allowable outage time model is deterministic, and, given that the CACS failed in a time interval (t_1, t_2) , one can be "100% confident" (given the assumptions of the model for events 7 and

8) that the true failure probability for event 8 is in the interval obtained by evaluating (A2-37) at the T_j corresponding to t_1 and the T_j corresponding to t_2 . Furthermore, since this "uncertainty" seemingly lacks a probabilistic interpretation, what is the justification for combining it with other probabilistic uncertainties (using, e.g., 4-233 for Vol. II) to obtain 90% uncertainty bands for branch sequences?

Resolution

- (a) GA was faced with the problem of selecting a finite, representative sample of events from the infinite number of possible outcomes. The intervals selected are evenly spaced on a log scale, resulting in comparable probabilities of CACS failure in each interval. GA recognized that the introduction of discrete intervals introduces some errors into the analysis. However, the degree of this error (or uncertainty) is directly measured by the associated uncertainty factors. If smaller (or larger) intervals had been selected, the associated uncertainty factors would have been correspondingly smaller (or larger).
- (b) GA explains that, although the allowable outage time is a deterministic function of the time of CACS failure, it is a random quantity since the CACS failure time is random. Thus, the probability of failure to repair the system within that allowable time is random, and it is meaningful to define conditional distributions for that probability given that the allowable outage time is in intervals corresponding to CACS failure time intervals. The lognormal distribution was used for these conditional distributions for the reasons explained in item 13 above.

Two formulas are used throughout the study to obtain error factors: $\sqrt{P_{.95}/P_{.05}}$ and $P_{.95}/P_{.5} \cdot P_{.5}/P_{.05}$ could also be used. These produce different results in non-lognormal distributions, and it is not always clear how the choice between formulas is made in particular cases. Furthermore, there is no consistency in selecting medians. In some cases (example: CACS failure probabilities, page A2-37) the median is taken to be the geometric average of the upper and lower limits, while in others it is arbitrarily selected as, for example, the midpoint of an interval.

In some cases, the combination of median and error factor choices used in going from continuous to discrete models produces intervals which differ completely from the original continuous model. For example, in the reactor trip and containment isolation responses to earthquakes (pages A3-28 and A3-55), the "median" cited is not the geometric mean of the interval whose endpoints determine the uncertainty factors. Thus, the model implies that with probability 1

$$P(1.2) \leq P_f \leq P(1.4) \quad (1)$$

for an earthquake between 1.2 and 1.4 SSE magnitudes (where P_f is the probability of reactor trip failure and $P(\alpha)$ is given by Figure A3-3), yet the "lognormal" approach says that with 90% confidence

$$a \equiv P(1.3) / \sqrt{\frac{P(1.4)}{P(1.2)}} \leq P_f \leq P(1.3) \cdot \sqrt{\frac{P(1.4)}{P(1.2)}} \equiv b \quad (2)$$

As the figure below shows, these intervals are not related. The geometric mean would have to be used in relation (2) to produce interval (1).

Resolution

Medians and error factors are chosen on a case-by-case basis, depending on the information available. In each case the goal is to select numbers that most accurately describe the quantity being estimated. In some cases the lower confidence bound is ignored in determining error factors because it might over-estimate the uncertainty. (The upper bound is the most important in identifying high-probability events.) Similarly, the geometric mean is not used in some cases because it is near the lower of the two values averaged (for numbers less than one) and hence might underestimate probability values. The earthquake model discussed above is an example; the interval $[a, b]$ is conservative since it over-estimates the failure probability, yet the error factor reflects the magnitude of variation over the interval fairly well. In any case, GA maintains that the magnitude of any errors introduced in these decisions is small. ANC feels that an effort should be made to assess the magnitude of these differences. GA believes that this issue can be resolved when improvements in the SAMPLE code are made.

Comment D-15

Page A1-14. The failure probability for the intermediate reheater leak event #2 is the sum of hardware failure probabilities (Q_2) and the probability that the leak would not be detected with functioning equipment (X_1). The uncertainty for this sum should include uncertainty in X_1 as well as that of Q_2 .

Resolution

GA recognizes that the uncertainty in X_1 should be included in the final uncertainty analysis. However, GA stated with ANC's concurrence that the effect upon branch failure probabilities would not be significant in this case. In addition, Eq. A1-4 should read

$$\begin{aligned} Q_2 = & 3.0 [X(1) + X(2) + X(3) + X(4)]^2 \\ & - 2.0 [X(1) + X(2) + X(3) + X(4)]^3 \\ & + X(5) + X_1 \end{aligned}$$

Comment D-16

Page A2-37. How were the uncertainty factors obtained? Also, why do the values given in the table differ from those on the LOSP event tree under event #8?

Resolution

The uncertainty factors were obtained by computing the ratio of the upper limit by the median values. The values listed in the table are correct except for the last two columns on the right, which differ, to a small extent, with those values listed in the event tree, which are correct. The method used to compute the median values listed in the table was to compute the central estimate of the time of CACS failure as $t_2/2$, where

the interval of CACS failure is (t_1, t_2) . A more realistic method used to compute the values in the event tree was to compute the central estimate of the CACS time of failure using $(t_2 - t_1)/2$. Unfortunately, the information in the table did not reflect this modification to the analysis.

Comment D-17

Page A2-36, last sentence. The fact that uncertainty factor calculations had to be changed to avoid dividing by zero illustrates again the difficulties involved in trying to apply geometric means and error factors to describe certain intervals. In any case, changing the right endpoint of the interval to 100 seems arbitrary - a larger "median" probability value for ranch AV would be obtained by changing it to 20, or 11, or even 10.

Resolution

Originally, the branches in the lower half of the event tree were like those in the upper half, and there were branches for CACS failure in the intervals (10, 100) and (100, 5000). However, since the probabilities were so low ($\sim 10^{-14}$) for the (100, 5000) branch, that branch was combined with the (10, 100) branch. The probability values for the (10, 100) branch were retained in the final analysis.

Comment D-18

Page A3-4. If M is the maximum earthquake per year at a site, and M is assumed to be lognormal and σ is the standard deviation of $\log M$, then

$$\text{stdv. } (M) \neq e^{\sigma} \equiv \sigma_N \quad .$$

Rather

$$\text{stdv. (M)} = [\exp(2\mu + \sigma^2) \cdot (\exp(\sigma^2) - 1)]^{1/2}, \quad (1)$$

where μ is the mean of $\log M$.

Thus, if the Newmark paper only gives standard deviation data for M , the techniques used to obtain earthquake occurrence rates are invalid (one must know the mean μ in order to solve equation 1 for the standard deviation σ).

Resolution

GA explains that the Newmark paper does in fact give data for the desired standard deviation, namely that of $\log M$. Newmark supplies this data in the form of the one-standard-deviation error factor, e^σ . ANC acknowledges that the parameters were properly interpreted in the AIPA study.

Comment D-19

The technique for combining earthquake hardware probabilities with other probabilities is exactly the same for the CACS start-up as for the reactor trip; simplification of equation (A3-17) on page A3-31 shows that

$$Q_6 \equiv P(\bar{5}) + Q_E$$

where $P(\bar{5})$ is given on page A2-16. Therefore,

- a. Why aren't the uncertainties combined in the same way (using A3-14, for example); and
- b. Why is the median CACS start-up failure probability with $\alpha = 1.1$ lower than it is in Appendix 2 where no earthquake is assumed? [Compare the value in Table A3-19, p. A3-36, with that given on p. A2-16].

Resolution

GA did not recognize the similarity in the models when they were derived. The uncertainties should have been combined using equation A3-14; however, using the maximum uncertainty produces a conservative result. The results of SAMPLE given on page 2-16 should have been used instead of point values to obtain CACS start-up failure probabilities.

Comment D-20

The "engineering judgment" α_1 and α_2 values are the same for containment isolation as for CACS start-up. Why is Newmark's data applied to CACS start-up and not to containment isolation? Using the geometric mean of the two curves given as upper and lower bounds for CACS start-up produces very low estimates, which are anticonservative if the engineering judgment is correct. The difference in the final answers produced by the two methods can be seen by comparing Tables A3-18 and A3-27.

Resolution

GA states that the engineering judgment values given for CACS start-up showed that these engineers, while knowledgeable in their field, lacked sufficient background and experience in probability to provide reasonable estimates of the α -values. Thus, lacking α -values directly applicable to CACS start-up, the Newmark data on equipment and structural resistance was adopted to reflect the structural integrity of the CACS. This data was modified by the additional use of PCRV α -values to reflect the potential interaction between the CACS and the earthquake induced motions of the PCRV. GA feels that this combination best describes the CACS start-up failure probabilities, but recognizes that the results are only approximations with large (unmeasurable) uncertainties. Thus, further research in this area is a part of the R&D recommendations.

Page A3-40. The calculations on this page seek the probability that the first CACS failure occurs in each time interval. Like the LOSP analysis on page A2-20, these probabilities are unconditional; their sum over the time intervals (0, 10), (10, 100), (100, 1000), (1000, 5000), and (5000, + 00) should be one. ANC has three objections to the way in which these probabilities were evaluated:

- (a) $R_c(t)$ is the probability that the CACS does not fail in the interval (0, t). Let $F(t) = 1 - R_c(t)$, the probability that the first failure occurs before time t. Then the probability of failure in a time interval (t_1, t_2) is

$$F(t_2) - F(t_1) = R(t_1) - R(t_2) \quad . \quad (1)$$

Instead, the Q values calculated on page A3-40 are obtained from

$$Q = R(0) - R(t_2 - t_1) \quad . \quad (2)$$

This would be the conditional probability of failure, if $R(t_2 - t_1)$ were equal to $R(t_2)/r(t_1)$. However, it is not since R is not exponential.

- (b) Neglecting terms with $1/48$ in the exponent amounts to using the third equation in (A3-23) to evaluate the reliability. That is, it amounts to assuming $P_{osp}(t) = 1$. While this is nearly so for large values of t, it is not insignificant for the time interval (10, 100).
- (c) The beta values given on page A3-39 differ from those in the LOSP analysis on page A2-31. The smaller values used in the earthquake analysis have little effect on the results for the time interval (0, 10), but produce numbers roughly 30% smaller than what would be obtained with the LOSP beta values for the other time intervals.

Resolution

GA agrees that equation (1) is correct, and that the larger beta values should have been used. However GA does not agree that the impact on the results (maximum of 30% in the median assessment) is significant compared with the uncertainties which were assessed. In terms of percentage, the uncertainties in the CACS unreliabilities had a range of 1,000% to 3,100%. Also, it is stated on pages A3-48 through A3-50 that the CACS unreliability calculations are probably optimistic since there was no objective method available to assess the increase in the equipment failure rates and repair times affected by the earthquake (although an assessment was included in the CACS startup probabilities). The conclusions and recommendations made resulting from the earthquake analysis reflect the recognition by GA that the risks of earthquakes are not as well defined as those of the other events analyzed in the study due to the lack of understanding of how large earthquakes affect equipment. ANC agrees that the uncertainties involved overshadow the questions raised concerning the mathematical rigor.

4.2. USE OF AIPA RESULTS FOR ABSOLUTE COMPARISONS OF RISK

The first comment is quoted from Section 3 of the ANC report, and Comments 2 through 6 (the last four of which are examples cited from WASH-1400 to illustrate the point made in Comment 2) are quoted from Appendix B of the ANC report, as is Comment 7.

Comment 1

Reliability engineering analytical methods which are being used in the AIPA have been found to be useful tools in making decisions regarding the reliability and safety of various design options. These methods, except for the limited corroboration of results with experience by the Reactor Safety Study, have not been demonstrated for making absolute comparisons, that is, for making suitable comparisons with actual data. The purpose of the probabilistic methods verification subtask is to determine whether the reliability methods used by GA produce results which are in statistically measurable agreement with observed data. The object of the subtask, then, is to test the systems analysis methods used by GA by applying them to the evaluation of a system for which sufficient experience information has been recorded to make meaningful comparisons.

Response

The AIPA study team is pleased that ANC concurs with the usefulness of the probabilistic risk assessment techniques employed in the AIPA study for making decisions with regard to the reliability and safety of alternative design options. We further believe that these techniques provide a practical basis for the identification of safety R&D needs.

A review of the operating history for nuclear power plants accumulated thus far reveals that the accident sequences that dominate the AIPA and WASH-1400 risk assessments have not occurred. It is for this reason that there exists very limited corroboration of such assessments of directly

applicable experience data, as noted by ANC. Thus, it would appear that a primary consideration with regard to the making of absolute comparisons is a concern for the adequacy of such models. The probabilistic methods verification subtask mentioned in the comment quoted above was proposed by ANC as a means for providing a meaningful check on the basic systems analysis methods employed in the AIPA and WASH-1400 studies. Such an undertaking is wholeheartedly supported as a means of demonstrating the adequacy of the methodology used in these studies or of making improvements where such a need is found to exist.

It is considered that the probabilistic risk assessment methodology as employed in the AIPA and WASH-1400 studies is reasonably accurate from the standpoint of absolute risk. It is this consideration that provides the fundamental basis for comparing LWR risks with the risks of other man-made and natural hazards, as was done in WASH-1400.

Comment 2

Below is a summary of our discussions with you and Vojin on March 12 concerning our contention that one must be careful in making comparisons between the AIPA and WASH-1400 results. Our arguments were based on the fact that AIPA is an attempt to develop a risk model of a design concept (of a plant not yet built); whereas WASH-1400 was an in-depth investigation of two completed plants which became operational during the course of that study. We believe that a large number of risk factors can be identified in a completed plant; but these risk factors can only be subjectively conjectured from design drawings. We would like to include differences in depth (or perhaps better described as coverage) which would limit the validity of conclusions that can be drawn by comparing results of the two studies. In an attempt to prove our point we cited several examples in WASH-1400 of events that would be difficult if not impossible to identify from design drawings and important events for which data could not be appropriately assigned without knowledge of the actual physical situation.

Response

If this comment were correct, then the results of the WASH-1400 study would be applicable only to the two plants used as the basis for that study, and then only up to the time that plant modifications are made involving equipment retrofits or changes in operating or maintenance procedures. The study results could not be considered to be "representative" of 100 plants.

It is acknowledged that the WASH-1400 analysis was based on two completed plants, whereas the AIPA analysis utilized a reference design (on paper). However, it should be noted that both studies deal with specific designs. The failure rates and other data used in analyzing the HTGR are based upon systems that will have been tested and checked out with a success assumed to be comparable to the testing and checkout of systems in the LWR plants. The AIPA analysis, therefore, does not require that the paper design be perfect but, rather, only requires that its checkout and resultant adjustments and corrections be as successful as has been the case at the LWR plants. It is the recognition of this that provides a basis for accepting the WASH-1400 results as representative of 100 plants. Of course, it seems clear that there are major, generic characteristics of a plant design that do have a major impact on plant risk, such as the high heat capacity of the HTGR core, its negative power and temperature coefficient, the use of a single phase coolant, and the reliance on graphite in the core for its high-temperature structural integrity. Such characteristics tend to increase the amount of time available for responding to accident conditions in such a way as to mitigate their consequences. The probabilistic risk assessment methodology as applied in the AIPA study provides an adequate basis for incorporating these highly significant considerations into the analysis without necessitating an evaluation of the construction details of a plant that has been completed.

While major, overall plant characteristics can (and do) have a significant impact on plant risk, the effects of the more detailed plant characteristics tend to be minor. The results of the AIPA analysis provide some insights that tend to substantiate this claim. In the Preliminary Analysis, the LOSP-initiated core heatup sequence of greatest relative importance (branch BI in Figs. 4-20 and 4-22 of Volume III) had an occurrence rate of 1×10^{-7} per year. In the more detailed Phase I analysis, the corresponding sequence (Branch BD in Figs. 4-14 and 4-19 of Volume IV) had an occurrence rate of 1×10^{-6} per year. The current estimate, based on an even more detailed analysis being performed in support of the Phase II analysis effort, is 3×10^{-7} per year. Thus, it is clear that the inclusion of more and more detail in the analysis has had, thus far, only a small impact on the plant-level assessment of LOSP-initiated core heatup.

Further detailed discussions pertaining to the subject of this comment can be found in the responses to Comment A-3 in Section 4.1 and, particularly, Comment 4 in Section 3.6.

Comment 3
Low Pressure Injection System Valve Selection

In the two-loop BWR evaluated in WASH-1400, low pressure injection fluid is injected into loop A if a break occurs in loop B or into loop B if a break occurs in loop A. Injection into the wrong loop or injection into both loops constitutes failure of the system. Two, supposedly redundant, logic control networks have been provided to sense a break location and to open the proper valve for emergency coolant injection. A detailed fault tree analysis of the emergency coolant system revealed that some of the components within the logic networks were not redundant, but instead were single potential fault events. In other words, faults in either network could fail the low pressure coolant injection system. The provisions of a second logic system actually increased the likelihood of the failure-- the burden for proper loop selection was on both logic networks, not just one.

These components in the logic networks are, therefore, in the list of dominant contributors to system failure (Ref. WASH-1400, Appendix II, Table 6-13).

The point to be made here is that although the design concept was intended to be redundant, an actual evaluation of the detailed design revealed that was not the case. The AIPA is being performed using schematic diagrams and other information generally contained in the FSAR's and not the kinds of detailed design drawings which can be used to construct a facility.

Response

The point made in this comment is quite clear and was certainly recognized during the course of the AIPA study. As a matter of fact, the common mode analysis methodology was developed to take into account the type of situation identified in this comment -- that is, situations in which equipment that is intended to be redundant does not completely achieve that objective in actual operation because of a variety of factors that cause unwanted hardware interdependencies. While the AIPA common mode methodology includes the consideration of single fault events of the type identified in this comment, it includes other considerations as well, such as common adverse environments, design defects, and maintenance errors. Thus, the AIPA approach tends to pick up the specific point of concern in this comment plus others not specifically addressed therein.

It is true that extensive use was made of information and drawings contained in Safety Analysis Reports. However, it is also true that more detailed design drawings were used as the bases for the analyses of such safety systems as the reactor trip and core auxiliary cooling systems. Detailed block diagrams for the operations of these systems were developed from those drawings, and Monte Carlo simulation techniques (using the MONSIM computer code) were used for the detailed numerical analyses based on those diagrams.

Comment 4

Low Pressure Injection System dP Transducers

In order to identify the break location for proper emergency coolant injection, as discussed in the above example, four transducers have been provided to sense the differential pressure between the two loops. If a break occurs in loop A the output signal from the transducers will be in one direction; if the break occurs in loop B the signal will be in the opposite direction. The pressure transducer measurement range is 0 ± 10 psid with a case pressure rating of 1500 psig. The logic system selects the proper loop for injection within a $1/2$ second time frame which occurs 2 seconds after a loop pipe break occurs. (See WASH-1400, Appendix II, Figure 6-33.) If a pipe break occurs in one of the two coolant loops a 900 psi pressure transient can occur across the pressure transducers (a break location near one of the transducer sense lines would reduce one side of the transducers to near atmospheric pressure; the other side would be at reactor operating pressure). Questions immediately arise: Will the pressure transducers fail because of the pressure transient since the transient appears across the transducer diaphragm and the case rating does not necessarily apply to the diaphragm? If the measurement range of the pressure transducers is 0 ± 10 psid what is the output signal 2 seconds after the break occurs with several hundred pounds of pressure applied? These are important questions because improper loop selection might result from the pipe break itself. This is to say, that a dependency could exist between column headings on the event tree; i.e., the initiating event (pipe break) and the emergency cooling systems, thus significantly increasing the occurrence probability of some event tree sequences. The data used in WASH-1400 for these particular transducers were derived from reported failures of that type of transducer in a similar installation in another plant. The point to be made here is that important questions arise in the detailed evaluation of a more defined installation that don't arise in evaluation of conceptual designs. In this example information was needed on a specific type of transducer in a specific application. The information base used in the AIPA has not yet advanced to a stage where the above kinds of information are defined (nor do the questions arise), nor can the B-factor be

applied to account for the potential dependency between the initiating event and the low pressure coolant injection system.

Response

The response to Comment 3 above is applicable to this comment as well. In addition, it should be pointed out that interdependencies between event tree columns were taken into consideration during the AIPA study as deemed appropriate. Several examples can readily be identified in the event trees presented in Volume IV. In the case of reheater leaks, for example, the ability of the automatic isolation system to identify the leaking reheater depends on the size of the leak as identified in the initiating event (see Figs. 4-4, 4-6, and 4-7). In the LOSP analysis (see Fig. 4-14), the ability of the CACS to continue operating until main loop cooling can be restored depends on whether or not a trip of the turbine-generator occurred. Additional examples of such interdependencies can be found in the event trees in Volume IV.

The beta factor methodology developed during the AIPA study provides a broad range of application for the numerical evaluation of common mode interdependencies. While the application of that methodology was restricted primarily to the consideration of intrasystem dependencies, that methodology can also be applied to intersystem dependencies. Thus, contrary to what is indicated in the comment, the beta factor methodology can be used to account for interdependencies between initiating events and other events displayed in event trees.

Comment 5 Refuelling Water Storage Tank

The refuelling water storage tank at the Surry Plant is used as a source of water for the containment spray, the low pressure emergency core cooling, and the high pressure emergency core cooling systems. The probability of tank failure is potentially important because the tank is common

to the three systems (the tank failure probability is the limiting probability of failure for the combined systems). Data can be obtained for tank failures which occur relatively infrequently (considered " ϵ " in WASH-1400). Usually not considered in analyses, because schematic symbols ordinarily don't show them, are tank vents. Plugging of the refuelling water storage tank vent can be as disastrous as rupture of the tank itself; so what kind of probability value can be assigned to plugging of the vent? An engineering judgment is required because vent plugging data have not been reported. During the testing of systems which draw water from the tank, water is recirculated back to the tank; therefore, the vent is never subject to test. One might postulate plugging of the vent by birds nests over a period of time or by debris from nearby sources becoming sucked into the vent when needed (the vent will act like a big vacuum cleaner when water is rapidly drawn from tank). Only can good engineering judgment be made about the vent becoming plugged if the analyst has the opportunity to see whether debris exists or can exist to plug the vent.

Response

The response to Comment 6 is applicable to this comment as well. In addition, it is noted that, in conjunction with the discussion of the reactor water storage tank on page II-114 of Wash-1400, the probability of vent plugging (which is given as 4×10^{-7}) was based on details that were only available from the specific actual plant. Before these details were known, the estimate was higher by an unknown amount. However, it is not apparent that this would have caused a significant change in the results or conclusions of the analysis because this probability would have to have risen by a factor of 1000 before the effect would be noticeable even in the local portion of the fault tree where it is entered. This provides another example in which the inclusion of analysis detail has no significant impact on the plant risk assessment.

Comment 6

Engineered Safety Feature Dependency on Diesels

Perhaps one of the most interesting situations, and potentially a situation which could have had an important impact on the sequence probability values obtained for the BWR evaluated in WASH-1400 was an error found on the detailed design drawings. The situation is described as follows:

In order not to overload normal power sources (Ref. Peach Bottom FSAR, Page 7.4-18, Second Paragraph), two RHR pumps, A and B, start immediately and the other two, C and D start five seconds later. In the evaluation of the detailed drawings it was found that pumps A and C start immediately when required and B and D start five seconds later. This starting lineup would not prevent overloading, but assures overloading of the power sources. Pumps A and C would start, overload one commercial bus, and trip the feeder breaker to that bus. Five seconds later pumps B and D would start, overload the second commercial bus, and trip the feeder breaker to that bus (actually A and C could also trip the second commercial bus because provisions in the design automatically transfer the loads from the first commercial bus to the second). In both cases the burden for electrical power is placed on the diesel generators. The probability, however, of diesel failure is relatively high (about 10^{-2}). Thus, the situation places an upper bound on the likelihood of success for all engineered safeguard systems (all derive power from the same power sources as the RHR pumps).

An examination of the actual hardware revealed that the system was installed as indicated on the drawings, not as specified in the FSAR. But the FSAR statement about overloading was also wrong, because each of the two commercial buses can start up to three RHR pumps simultaneously without tripping.

There are several points to be made here. First, the analyses made thus far in the AIPA have not been to the level of detail which could identify these kinds of problems (nor do the AIPA objectives require that the analyses need to be to that depth). Second, obscure dependencies, if not found, can overwhelm any mathematical finesse in modelling; therefore, a risk analysis insofar as absolute probability determination is concerned, is not complete until performed at a level needed to identify these potential risk contributors. Risk factors can not be judged to be unimportant unless identified, examined and then judged to be unimportant. Finally, data cannot be applied to account for these kinds of problems. The problem exists or it doesn't. Explicit investigative failure modelling must be applied to determine whether data is applicable to the system.

Response

The response to Comment 3 is also applicable to the first point made in this comment. With regard to the second point, it should be noted that the potential for obscure dependencies always exists, regardless of the level of analysis detail. The main concern in this regard would seem to be to perform the analyses to that level of detail needed to enable the evaluation of the impact that the significant dependency contributors have on plant risk. Notice that it is not really necessary to identify the specific dependency contributors, per se; it is only necessary to be able to evaluate their effect on plant risk. This raises a question as to which of two basic analysis philosophies is, in reality, more comprehensive: (a) perform analyses in great detail and presume that the detail will assure the identification of all important dependencies, or (b) perform analyses in somewhat less detail but provide for the effects of the important dependencies in a generic sense based on data accumulated from past experience. In the former approach there seems to be too great a potential for important dependencies to slip by without being found. On the other hand, the latter approach provides a sort of "catch-all" process for accounting for the effects of all of a wide variety of potentially important dependencies whose occurrence would be consistent with the current state of

technology as evidenced by prior experience data. The AIPA study relied more on the latter philosophy, with the beta factor accounting for the current state-of-the-art interdependencies as determined from existing data.

With regard to the final point of the comment, the AIPA study team disagrees. The potential common mode dependency problems of subsystems and systems are analogous to the variety of potential problems in components and parts, such as

1. Poor weld quality in a heat exchanger, causing leakage.
2. A crack in a turbine blade, causing premature fatigue failure.
3. The presence of dirt in a valve, causing actuation failure.

In each of these cases, the problem exists, or it doesn't -- the same situation as identified in the comment with regard to common mode dependency problems. However, it is accepted practice to rely on data to evaluate the occurrence probabilities for these various component and part problems since those data reflect the current state-of-the-art of technology in those areas. The same is believed to be true with regard to the use of data to reflect the current state-of-the-art with respect to common mode dependency problems.

Although this comment (as well as others in this subsection) deals with specific, detailed aspects of probabilistic risk assessment analyses, a few general observations may provide some insights and clarity not otherwise apparent. It seems worthwhile to make a distinction between two different approaches to the analysis:

1. Design review.
2. Probability evaluation.

While both types of analysis overlap in their use of some analysis methods, their results may contain significant differences.

The design review approach tends to utilize a very detailed analysis, the objective of which is to attempt to identify all significant cases in which the current state-of-the-art is compromised because of deviations from accepted government and industry codes, standards, and/or successful design practice as a result of errors in design, construction, or operating or maintenance procedures. An original, historical objective of such analyses was to find and correct deficiencies. This approach has been applied to the common mode failure problem by attempting to find all the specific common modes of failure for the specific system and accounting for them. However, this approach does not account for any oversights by the analyst in finding every design and operating error, and it generally does not make allowance for those design errors which can only become evident with experience.

On the other hand, the probability evaluation approach tends to be a less detailed analysis performed with the objective of assessing system probabilities attainable with the current state-of-the-art, including assessments of the probability of design errors, especially common mode design errors on redundant systems. It does not restrict itself to only those failure modes which the analyst finds. Rather, it includes some provision for failures based on general previous experience without being limited to the precise definition of the mechanism.

The AIPA probability analyses utilize the probability evaluation approach. The ANC comments seem to be addressing probability analyses based on the design review approach. The problem with using the design review approach is that, in the main, it does not account for unknown design and operating error. The problem with the probability evaluation approach is the somewhat limited data base with which to probabilistically assess design error. But it is believed that the more complete assessment of design error is the preferred route, as is done in AIPA.

Comment 7

The Reactor Safety Study was a detailed investigation of a specific BWR and a specific PWR. Although believed to be representative of 100 plants, whether the WASH-1400 results are applicable to other plants is not known. This extrapolation, as I understand it, will be a subject of subsequent study by the NRC-RSR. Another follow-on activity by that study group will be to identify and categorize the important contributors to the risk values reported in WASH-1400. The results of this activity should establish whether those major contributors are the kinds of events that can be identified in a "Phase I" AIPA effort.

Response

This comment brings forth the question of the applicability of the WASH-1400 results to 100 plants, a consideration discussed in the response to Comment 2 above. As mentioned therein, it is believed that the WASH-1400 results are reasonably representative of 100 plants based on the reasons presented there.

4.3. THE ERDA-NRA R&D DECISION CRITERIA

Section 4 of the ANC report contains no ANC comments on the subject decision criteria; it serves only as an introduction to the comments presented in Appendix C of that report. The following are responses to comments quoted from Appendix C.

Comment 1

In regard to the agreements and commitments made in the GCR-AIPA meeting in Germantown on June 27, I have reviewed the "ERDA-RRD Decision Criteria on Gas Reactor Safety R&D." It is suggested that the sentence below be added to category 3 of the criteria in order to account for potentially important R&D work:

"....Category 3 would necessarily include areas where insufficient knowledge exists to calculate either the likelihood or consequence actors in the risk expression."

Response

The intent of category 3 seems quite clear. If an area is so poorly defined and, therefore, the risk uncertainty is so high that "...one cannot determine conclusively if the accident would be of potential key public safety or key environmental concern," then category 3 would be applicable.* Thus, the suggested additional wording seems superfluous.

Comment 2

Since little or no actuarial data exist regarding nuclear accident risks, these risks must be calculated. This calculation necessarily presumes that sufficient knowledge exists about accident probabilities and

* Volume I, page 8-3.

their consequences to make the calculation. This may not be the case, and therefore R&D would be justified to not only provide sufficient knowledge to make the risk calculation but also to identify all of those important accident initiating events upon which the risk calculations are based and which are needed to complete the risk spectrum.

Response

Some of the ideas presented in the response to Comment 1 of Section 4.2 are appropriate for this comment as well. In addition, it should be noted that the greatest differences between the WASH-1400 and AIPA studies are in the predicted maximum consequences or radiation doses to the public. The differences between these two concepts in the probabilities of getting the maximum doses are different, but not a major difference. The data forming the basis for the maximum dose predictions for each concept are not extensive in either case. The relatively low dose resulting from the HTGR is strongly influenced by the relatively slow heatup of the large graphite core and the significant plateout of many radionuclides inside the pressure vessel and the containment. The relatively large dose associated with LWRs is strongly influenced by the relatively fast melting of the core and the ability of the molten core to melt through the pressure vessel, coupled with the large energy and mass releases into the containment which result in a relatively high probability of major containment leakage. These potential events need further study, but they appear to display inherently large differences in the ease and speed with which the fission products can reach the public.

An associated element in predicting the probability of such severe events is the confidence with which the analyst knows whether the safety equipment will function successfully. For a gas-cooled, single-phase cooling system during an accident, the confidence in the capability of the equipment to function according to design is high. In the case of LWRs, which have to provide cooling in a two-phase system during severe accidents, the confidence in the success of the system cooling function is

reasonable, but significantly less than in the case of the HTGR. This is reflected in the WASH-1400 study in some of the probability numbers associated with the successful functioning of the emergency core cooling system. It may be noted that the problem of successful functionability is not eased by being able to tour a plant that has been constructed but, instead, hinges upon tests in two-phase flow systems.

Comment 3

Provisions should also be made in the criteria to allow for general research related to risk but not necessarily to any particular accident. For example, human factors are important contributions to risk but can be related to a lot of different accidents. Fission product studies is another example.

Response

Such provisions are included in the decision criteria via category 4.* The particular example cited therein is the case of common mode methodology, which most certainly spans many accident sequences. The two cases cited in the comment are also examples spanning many accident sequences.

Comment 4

The statement in category 3 regarding "key areas of uncertainty" is good. Any justification for research based on the uncertainty of risk values should be directed at key contributors to the risk values. This is to say that one should embark on a research activity related to the principal contributors. Research is not justified where the principal contributors to risk are readily correctable potential faults. Usually in the evaluation of a system there are only a handful of components and other

*Volume I, page 8-3.

events which clearly dominate the system failure probability, and in most cases the faults are readily correctable by design or procedure changes. These evaluations also usually suggest design changes which are easily implemented to reduce the system failure probability.

Response

It is agreed that R&D that is based on areas of high risk and high risk uncertainty should be directed at the key contributors to those risk and risk uncertainty values. It is also agreed that R&D associated with readily correctable potential faults is not really justifiable. However, it should be noted that this type of fault tends to be a deviation from the current state-of-the-art and, thus, tends to be identified by the detailed analyses performed in support of design reviews, as discussed in the response to Comment 6 in Section 4.2. Moreover, as noted in that response, the AIPA study utilizes a probability evaluation approach and, as such, tends to provide evaluations of state-of-the-art systems. As a result, the areas identified for R&D by the AIPA type of analysis tend to be those that are not readily correctable and, hence, may be appropriate for R&D. It should also be noted that risks for some events are already low enough, so that R&D directed at reducing the risk or the risk uncertainty in those cases would not be justified.

Comment 5

The General Atomic Company (GA) in their review of the AIPA task with ERDA-RRD on June 27 suggested several areas where safety R&D funding could be channeled based on risk values determined for the reheater leak, loss of off-site power, and earthquake accidents. As requested by Don Emon, Reference (a), ANC has reviewed the GA recommendations, and we believe that, based on the recent R&D criteria, Reference (b), the R&D suggested by GA is not justified. The attached graph shows the risk values for those accidents in relation to risk results obtained in the Reactor Safety Study, Reference (c) and a conservative threshold for biological damage (per the

AIPA risk values versus WASH-1400

BEIR Report, 2200-11,000 man-rem will produce one latent fatality due to cancer). The probability values derived by GA for the large (RH-LA) and small (RH-SM) reheater leak accidents would exceed probability values obtained in the Reactor Safety Study if the latter were extrapolated over lower consequence values. However, the consequence values derived for those accidents by GA are sufficiently low in relation to thresholds of damage presented in the BEIR Report that they should be of little concern no matter what their probabilities of occurrence might be.

The consequence values calculated for all accident sequences determined thus far in the AIPA suggest that GA should not devote time to the determination of probability values for accident sequences unless sequences are identified which have radiological consequence values that exceed the BEIR Report biological damage sensitivities. Exceptions would be the probabilistic evaluation of systems (e.g., reactor shutdown, core auxiliary cooling, and auxiliary power) which would likely appear in accident sequences not yet identified.

The attached graph and the conclusions drawn therefrom were discussed with GA on August 11.

References: (a) Agreements and Commitments HTGR-AIPA Meeting, June 27, 1975
(b) RRD Decision Criteria on Gas Reactor Safety R&D
(c) Reactor Safety Study (WASH-1400)
(d) "The Effects on Populations of Exposure to Low Levels of Ionizing Radiation," Report of the Advisory Committee on the Biological Effects of Ionizing Radiations (BEIR Report), 1972.

Response

Perhaps ANC is correct in observing that HTGRs are so safe that no safety R&D is needed. However, it should be noted that the safety R&D recommendations that were presented in the AIPA Status Report were developed without the benefit of the ERDA-NRA R&D decision criteria. The application of those criteria was identified in the report as being a part of future work.*

With regard to the validity of the R&D recommendations, it is necessary to consider several points. First, the uncertainty bands computed for the probability, consequence, and risk assessments covered most of the analysis uncertainty, with two notable exceptions: the integrity of the PCRV in core heatup events, and the earthquake analysis. It is necessary to overlay the unquantifiable uncertainties in these areas on the uncertainties quantified in the Status Report. Second, comments by other reviewers have raised questions as to the validity of the plateout fractions and earthquake occurrence rates presented in the Status Report.** Compromise numerical values have been selected in both cases.*** These interim modifications have caused changes in some of the medians and uncertainties as given in the Status Report. Third, one of the tasks included within the scope of the AIPA study is the consideration of alternative design options in evaluating the effects of various options on such parameters as plant economics. Such efforts could lead to the identification of R&D needed to assure that the options of interest satisfy the appropriate criteria of safety acceptability. Thus, there are several considerations that point to potential needs for safety R&D, particularly in the areas of PCRV and containment integrity, fission product plateout

* Volume I, Section 8.

** Comment 1 in Section 3.2, Comment 5 in Section 3.3, and Comments B-4 and B-12 in Section 3.5.

*** Responses to Comment 1 in Section 3.2 and Comment B-4 in Section 3.5.

within the PCRV during core heatup events, the assessment of earthquake occurrence rates, and an evaluation of the effects of earthquakes on equipment integrity -- ideas that were included among the R&D recommendations presented in the AIPA Status Report.

Regarding the "exceptions" for R&D study suggested in the comment, those systems do, of course, appear in accident sequences in the report. The reliability aspects of core auxiliary cooling and auxiliary power also appear in the first R&D recommendation of Volume IV. The risk importance of the reactor shutdown system is small and the system does not appear in the R&D recommendations. In any event, subjective choices based on experience with other concepts, such as LWRs, is not believed to be as strong a basis for R&D choices as is probabilistic risk assessment.

REFERENCE

1. Stewart, M. E., "GCR Accident Review -- FY 1976 Report," RE-S-76-140, Idaho National Engineering Laboratory, Aerojet Nuclear Company, July 1976.

5. COMMENTS BY DR. J. B. FUSSEL,
UNIVERSITY OF TENNESSEE,
CONSULTANT TO ORNL

All comments by Dr. Fussel pertain to Volume II of the Status Report.

Comment 1
(Section 1.1, Page 1-2)

The last half of the last paragraph is misleading. First, event trees do not treat "all plant response sequences," but rather treat "all envisioned plant response sequences." Secondly, "margins of safety" are not known quantitatively from the proposed uncertainty analyses. No account is taken during these uncertainty analyses for a less than perfect logic model (fault tree/event tree). In fact, the chance of oversights and omissions in the logic model often introduces outstanding uncertainties in the computational results.

Response

It is agreed that the wording of this paragraph could be improved so that it does not overstate the scope and capabilities of the methodology. The point made here is that the methodology lends itself to the analysis of a much wider spectrum of accident sequences when compared to what is termed the deterministic methods, i.e., those used in the support of reactor siting and licensing in the U.S. For example, the event tree typically includes many sequences involving multiple-component and multiple-system failures, which are usually dismissed as "incredible" in the deterministic approach. Of course, the method is not all-encompassing; no engineering analysis method is.

Although no attempt was made to quantify the uncertainty associated with a less-than-perfect logic model and other types of models, the recognition that imperfections indeed exist is clearly reflected in the manner in which the results of the study have been interpreted (see Volume IV, Table 5-7, p. 5-9). R&D Problem No. 2 arises from an uncertainty which could not be quantified, but nevertheless was recognized as an important area for further research. Also, the recognition of a less-than-perfect operator reliability model led to R&D Problem No. 5. The entire discussion of the earthquake analysis in Volume IV reflects the awareness of uncertainties which could not be quantified systematically, most notably the uncertainty in predicting earthquake-induced failure probability of hardware. Another limitation of the event tree/fault tree methods reflected in the results is the ability to analyze sequences having a very low frequency. Event tree sequence probabilities calculated at less than 10^{-10} per reactor year are reported as " ϵ " in recognition of a general disbelief in the method at very low frequencies.

The point brought out by this comment is well taken in that any type of engineering analysis possesses unquantifiable uncertainties associated with a less-than-perfect understanding of what is being analyzed. On the other hand, the ability to objectively account for this in the quantitative analysis of uncertainty would require perfect knowledge, and this will never happen. The important thing is that these uncertainties be recognized in the interpretation of the results.

Comment 2
(Section 1.2 , Page 1-3)

Deterministic safety analysis tools are not known to usually give ultraconservative results. In fact, probabilistic uncertainty analyses show that a chance (sometimes a substantial chance) exists that all deterministic results are not necessarily "conservative."

Response

It is agreed that the referred statement may have been somewhat exaggerated; perhaps the word "ultraconservative" should be replaced by "conservative." The purpose of this statement is to provide the reader with an explanation as to why the consequences of the design basis accidents described in Safety Analysis Reports are substantially greater than the consequences of the same accidents evaluated under risk assessment ground rules. The purpose of SAR-type analyses is to show that an adequate margin of safety exists in the design of the plant so that substantial chances do not exist for consequences to exceed regulatory limits for specified accidents. An acceptable margin of safety is usually arrived at, in part, with the use of assumptions which are known to be conservative. Some important examples in the case of the HTGR are assumed values for meteorological dilution, breathing rate, MHFPR release fractions, no radioactive depletion enroute to the dose receptor, no radioactive decay during transit from the fuel to the containment, and the arbitrary specification that certain systems do not function during the accidents. Although there may be a substantial chance that one or more of these assumptions is not necessarily conservative, the "stacking" of many such assumptions in estimating the consequence of a single accident usually, if not always, results in a conservative result.

Comment 3
(Table 1-1, Page 1-4)

The practical capability of probabilistic methods is categorically overstated.

Response

To the extent with which the AIPA study team concurs with Comment 1, it is also agreed that there is nothing magic about risk assessment which makes it all-encompassing. However, it is felt that the probabilistic approach has already increased the level of understanding of HTGR safety to a significant extent with a moderate commitment of resources in a relatively short period of time.

Comment 4
(Section 2)

Excellent!

Response

This complimentary remark is appreciated.

Comment 5
(Section 3)

The otherwise excellent section is, unfortunately, garnished with statements such as "All possible event sequences are identified."

Response

Again, it is agreed that only "all envisioned" sequences can be analyzed.

Comment 6
(Section 3.2, Page 3-2)

Risk is actually more appropriately defined as the product of the expected frequency of occurrence and the consequence of the occurrence. When this frequency is small, its value is nearly equal to the occurrence probability.

Response

It is agreed that risk is more appropriately defined in terms of the expected frequency of occurrence. It was elected to use the terminology most often found in the literature, e.g., WASH-1400. It is noted that the mentioned approximation is very good for events "less probable" than anticipated transients. Of course, this has negligible impact on the results of this study.

Comment 7
(Section 3.3, Page 3-7)

One important limitation of the event tree technique is not mentioned. During event tree evaluation, events occurring "early" in a sequence are assumed to be independent of those occurring "later" in the sequence. This required assumption causes problems when (1) the sequence is not well defined, (2) the events overlap in time, and/or (3) an "early" event can possibly reverse its state from success to failure at a "later" time. Never think that because the mathematics "looks right" the problem does not exist. The problem is that the fault trees used to evaluate the probabilities are in error. Dependencies of "later" events on "earlier" events are, however, almost inherently treated correctly, as is pointed out in the Section.

Response

Some of the problems alluded to in this comment were encountered when constructing the event tree for loss of offsite power, which is discussed in detail in Volume IV of the Status Report. The problem of "events overlapping in time" was encountered (see column 3, LOSEP event tree, Volume IV, p. 4-33) and treated using the joint density function approach outlined in Section 4.2.3 of Volume II. A similar problem was encountered in the same event tree in evaluating the interdependency between the time of CACS failure and the maximum safe outage time of the cooling systems (see columns 6 and 8 of this event tree). This dependency was treated by "discretizing" the time of failure range into a number of intervals, a procedure which resulted in an increase in the number of branches to be analyzed.

The general problem of constructing an event tree which includes events distributed over a long period of time seems to be a good area for further development of the methodology. A new approach which is currently being investigated by the AIPA staff consists of constructing different

event trees for different time intervals following the initiating event. By appropriate selection of the time intervals, it appears that some of the mentioned problems can be minimized. As pointed out in Volume II, Section 3, event tree construction is a crucial step in the methodology and also appears to be the least formalized of all the major elements. The technical soundness of any given tree is dependent, therefore, on the expertise of the analyst and the extent to which the work is reviewed and checked. Reliance on extensive reviews is probably more important for the event trees than for the fault trees since there seems to be much more engineering experience with fault tree construction.

Comment 8
(Figure 4-3, Page 4-6)

The diagram shown in this figure is not a reliability diagram. A reliability diagram is a logic model. The descriptions in the boxes (or whatever) in a reliability diagram must describe events, not system components. To describe an event, both a component and a failure mode must be given.

Response

The purpose of this figure is to show how the reliability diagram is used to derive an expression for the system reliability in terms of the component reliabilities for simple series-parallel systems. This comment, however, appropriately points out that the component failure modes must be considered in performing the quantitative evaluation of the system. The failure modes applicable to a given problem will depend on the precise definition of "system success" or mission which the reliability analyst must also specify. It is agreed that this point would have been more clearly illustrated if the diagram had included specific failure modes for an example problem.

Comment 9
(Section 4.2.1, Page 4-7)

Equation 4-4 requires no assumption with regard to failure independence. A tacit assumption does exist insomuch as the success of a component does not enhance the chance of failure of another.

Response

The AIPA study team disagrees that Equation 4-4 is valid in the case of failure dependence and gives the following example to prove the point. Consider a reliability diagram consisting of N identical components in series. Assume that the components have both independent and common failure modes, i.e., sometimes all N components fail simultaneously due to some common cause and sometimes the components fail independently of each other. One can write an expression for the reliability of any particular component as

$$r_j = P(A) \times P(B_j|A) \quad .$$

where $A \equiv$ the event no common mode failure occurs,

$B_j \equiv$ the event no independent failure occurs in event j.

The system reliability R_s can be written as

$$R_s = P(A) \times R_{S|A} + [1 - P(A)] R_{S|\bar{A}}$$

where $R_{S|A} \equiv$ conditional system reliability given no common mode failures occur,

$R_{S|\bar{A}} \equiv$ conditional system reliability given a common mode failure occurs.

Given no common mode failures occur, Eq. 4-4 in Volume II can be applied so that

$$R_{S/A} = \prod_{j=1}^N P(B_j | A)$$

And since

$$R_{S|\bar{A}} = 0 \quad ,$$

R_S becomes

$$R_S = P(A) \prod_{j=1}^N P(B_j | A)$$

or

$$R_S = \frac{\prod_{j=1}^N r_j}{[P(A)]^{N-1}}$$

which is not the same as Eq. 4-4. It is interesting to note that in the special case where all component failures are common mode (and therefore no independent failures occur), the system reliability R_S is better than that predicted by Eq. 4-4; i.e.,

$$R_S = P(A)$$

since $r_j = P(A)$, $j = 1, 2, \dots, N$.

The AIPA team agrees with the second point raised, that Eq. 4-4 also requires an assumption that success of one component does not enhance the chance of failure of another.

Comment 10
(Figure 4-4, Page 4-9)

Describing a basic event as "component X fails" is inadmissible. The failure mode must be specified, such as "component X fails to low signal" or "component X fails shorted."

Response

Agreed (see response to Comment 8 above).

Comment 11
(Section 4.2.1, Page 4-11)

The statement, "Reliability diagrams bear a greater resemblance to system schematics than to fault trees and therefore are somewhat simpler to use and construct" reflects a popular misconception. Many erroneous logic models have resulted from assuming that reliability diagram resembles the system schematic. As an elementary example, a system composed of three fuses wired in series and with success defined as "no overload through the system," has the following schematic and reliability diagram. (On following page)

Response

The AIPA study team is aware of the popular misconception referred to here and still maintains that the reliability diagram provided in the example bears a greater resemblance to the system schematic than does the corresponding fault tree. The discussion on page 4-11 was intended to point out some of the advantages and disadvantages of each method. The statement "greater resemblance" was not intended to mean "equivalence." It is agreed, however, that this discussion could have been improved by pointing out this common pitfall of model construction and thereby avoiding the misunderstanding that resulted. In light of the discussion on page 4-11 and the example given in this comment, the novice reliability analyst would be well advised to use the reliability diagram approach and Eq. 4-19 as a means of checking out the fault tree, or vice versa. Such a procedure would tend to uncover potential errors such as the one pointed out here; however, this may be impractical when analyzing large systems.

RELIABILITY DIAGRAM

Schematic

Comment 12
(Section 4.3)

The method outlined here results in additional, usually exorbitant, numbers of minimal cut sets being generated during analysis of systems encountered in practice.

Response

It is true that the method referred to results in an additional, but, by no means an exorbitant, number of minimal cut sets when put into practice in the analysis of large systems. In fact, it has been found that this method is significantly more efficient when compared with, say, the methods used in WASH-1400. The principal reason why the additional cut sets do not make the method impractical is that first-order approximations, such as Eq. 4-47, can be applied in the vast majority of the analyses encountered in practice. Several hundred event tree probability calculations were performed and reported in Volumes III and IV of the Status Report. These probability evaluations together with the corresponding uncertainty analyses were performed quite easily within the moderate manpower and funding requirements for this study.

Comment 13
(Section 4.3.5, Page 4-38)

Of interest is the fact that Epler's $P^{\sqrt{2}}$ rule of thumb gives (0.982) almost the same answer for the sample problem as is obtained by the detailed method presented in the report (0.989).

Response

The AIPA study team agrees it is interesting that Epler's rule of thumb gives the same result. It is also noted that Epler's paper on common mode failures at ORNL (Ref. 4-12 in Volume II) had considerable influence on the thoughts during the early stages of developing the AIPA methodology for quantitative common mode analysis. Of particular interest is Epler's observation that the unavailability of a redundant system does not continue to improve when the level of redundancy is increased beyond one-out-of two.

Comment 14
(Section 4.4.3, Page 4-44)

Is the assumption (Assumption No. 2) made that the operator will never increase consequences or not?

Response

With regard to the time-dependent operator response/reliability model described in Section 4.4, it is assumed that the operator will either succeed or fail to perform a specified corrective action. A third state for taking incorrect action which increases the consequences is not included in this model. The R&D recommendations in Volume IV (Table 5-7, p. 5-9) reflect an awareness that improved methods are needed for the treatment of human operator reliability. Although there is no allowance for increased consequence operator actions in the model described here, any particular operator action which is envisioned by the analyst can be easily incorporated directly into the analysis as a basic event in the event tree. This includes any envisioned action which makes the consequences worse.

It is noted, however, that the slow response of the HTGR in nearly all the transients which have been postulated involving the reactor core and primary coolant system eliminates the need for the plant operator to make crucial decisions within short periods of time. In the vast majority of the accident sequences which have been analyzed in the study, there is sufficient time for the operator to observe an anomalous condition in the plant, take some sort of corrective action, observe the plant's response to his corrective action, and, if the action should be incorrect or actually make the situation worse, there is still sufficient time for the operator to correct his mistake and take the proper course of action.

As pointed out in the response to Comment B-5 in Section 3.1, the operator response model discussed here was found to be of secondary importance in all but a few of the accident sequences analyzed in the study.

The most important exception is the small reheater leak, Branch SM. Both the probability and consequence predictions for this sequence were sensitive to the assumptions associated with the operator response model. The recognition that more development is needed in the human reliability area is reflected in the rather large uncertainty assigned to the probability of this sequence, and in the recommendation for additional R&D discussed in the first paragraph above.

Comment 15
(Figure 4-9, Page 4-48)

I do not yet understand how the data points or estimates can appear on the cumulative distribution curve.

Response

The referenced figure is in error. The cumulative distribution curve was arrived at by normalizing a distribution of estimates curve (see Fig. 4-11), which is simply a lognormal fit of the data points (estimates). The data points should not appear in this figure.

Comment 16
(Section 4.6.2)

A much more straightforward approach to treatment of "overt dependencies" is to couple all the fault trees in a particular event sequence together with an AND logic gate and then use standard techniques to obtain the minimal cut sets for this new logic model. After obtaining a new list of cut sets, standard quantitative techniques can be used for evaluation. The problem with my approach is that the number of minimal cut sets can be very large.

Response

It is agreed that the suggested approach could have been used since it is mathematically equivalent to the referenced approach (i.e., both approaches would yield the same results for the event sequence frequency). The primary objective of this study is to provide recommendations of R&D in gas-cooled reactor safety. The referenced method is preferred over the suggested method since it lends itself to the identification of key events along a particular sequence which make significant contributions to both the sequence frequency and uncertainty. As can be seen in the event tree format used to present the results (Volumes III and IV), each of the branch point probabilities and uncertainties is listed in addition to the sequence probabilities. It seems that the best method to use depends on how much and what kind of information is sought.

Comment 17
(Section 4.6.3, Page 4-94)

The stated objective of logic model construction is not always quantitative evaluation.

Response

It is agreed that logic models are constructed to meet objectives other than quantitative evaluation. The statement should have been qualified to read, "the objective of logic model development in the AIPA study is"

Comment 18

(Section 4.7, Page 4-100)

Logic model inadequacies (Factor No. 4) is addressed as a source of uncertainty but is not accounted for in the uncertainty analysis methodology. This is misleading.

Response

In accordance with the response to Comment 1 above, the uncertainty analysis methodology does not explicitly address modeling uncertainties. The awareness that such uncertainties indeed exist is reflected in the manner in which the results of this study have been interpreted. Also, it should be noted that some allowance has been made to account for possible model deficiencies and other possible sources of error in the detailed risk analysis reported in Volume IV. The uncertainty factors which were specified as input for the SAMPLE code were rounded up to the nearest half order of magnitude.

Comment 19
(Section 4.7.2, Page 4-104)

The variables P_i represent system failures and are never distributed lognormally if the assumption is made that component failure rates are distributed lognormally as is shown in the Reference 4-10. The results of this section are then subject to serious question. Fortunately, the re-use of Monte Carlo methods (SAMPLE) is not expensive at this point in the overall analysis.

Response

The procedure outlined in Section 4.7.2 arises from a number of practical considerations. As pointed out in the response to Comment 16 above, it was desirable from the standpoint of the study objectives to be able to analyze each branch point probability along each sequence separately. The SAMPLE code was used to provide an uncertainty distribution for each system failure probability P_i . The most straightforward way to produce the sequence uncertainty distributions from this point in the analysis would be to reuse the SAMPLE code with Eq. 4-214 and specify the input distributions for the P_i as the output from the previous SAMPLE runs. Unfortunately, the SAMPLE code does not permit the input of any general distribution, only certain specified parametric distributions (e.g., normal, lognormal, log-uniform, etc.). As described in this section, this final step in the procedure can be performed analytically if it can be assumed that the P_i are distributed lognormally. Fortunately, most of the uncertainty distributions for the P_i analyzed in the study turned out to be reasonably close to lognormal, and, for this reason, the analytical formulas were used. It is noted that, had the procedure suggested in Comment 16 above for direct evaluation of the sequence been used, the analytical procedure and lognormal assumption would not have been necessary. Since publication of the Status Report, an improved version of the SAMPLE code has been developed, which accepts any nonparametric distribution which can

be specified with a table. This additional capability eliminates the need for the analytical procedure referred to along with the lognormal assumption. A reevaluation of the key accident sequences (e.g., LOSP-BD and LOSP-BF) has shown that the procedure and assumptions alluded to here very closely approximate the results reported in Volume IV for these cases. However, for current and future AIPA analyses, event sequence probability distribution can be evaluated via Monte Carlo simulation.

Comment 20
(Section 5.1, Page 5-2)

Uncertainty boundaries most assuredly do not define limits of results since a perfect model is assumed during the uncertainty analysis but does, in fact, not exist.

Response

Agreed (see response to Comment 1 above).