BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

P.O. Box 5000
Upton, New York 11973-5000

TEL (516) 282-2617
FAX (516) 282-2293
E-MAIL

Department of Advanced Technology

November 10, 1993

Mr. Mark Cunningham
RES/PRAB
Mail Stop NLS-372
U.S. Nuclear Regulatory Commission
Washington, DC 20555

Dear Mr. Cunningham:

**Subject: PRA ERG Final Report**

Enclosed is a copy of the PRA External Review Group's (ERG) Final Report, reviewing the October 8, 1993 report of the NRC PRA Working Group. Following your instructions and those of Dr. Beckjord, I have made no attempt to summarize the findings or combine them into a consensus report.

With the transmission of this report, all planned activities of the ERG have been completed.

Sincerely yours,

John R. Weeks
Sr. Metallurgist

JRW29/jmf
Enclosures
Copies to: (w/Encls.)
B. J. Garrick
B. Harris
R. L. Keeney
H. Kouts
W. T. Pratt (w/o Encls.)

# Final Report of the NRC PRA External Review Group

**John R. Weeks**

November 10, 1993

In the spring of 1992, Brookhaven National Laboratory (BNL) received a request from the NRC office of Nuclear Regulatory Research to assist that office in establishing an External Review Group to review and comment upon the activities of NRC's Working Group on probabilistic risk analysis (PRA).

The following persons were selected for this PRA External Review Group (PRA ERG), were invited, and agreed to serve as members:

- Dr. B. John Garrick, President, PLG, Inc. (a subcontractor to BNL)
- Dr. Bernard Harris, Professor, University of Wisconsin (a consultant to BNL)
- Dr. Ralph L. Keeney, Professor, University of Southern California (a consultant to BNL)
- Dr. Herbert J. Kouts, Member, Defense Nuclear Facilities Safety Board (serving on PRA ERG at the discretion of the Board)

Dr. John R. Weeks, BNL, agreed to coordinate the activities of the PRA ERG and chair its meetings. The PRA ERG met with NRC management, staff, and consultants on October 20, 1992 to review NRC's activities in PRA and to discuss its charter and future activities. At this meeting, it was emphasized that the NRC PRA Working Group was formed in response to concerns raised by the ACRS on how the staff was utilizing PRA techniques in decision making, and that one of the PRA ERG activites would be to review the NRC's response to the ACRS letter. The charter of the PRA ERG is:

> "To review the technical adequacy of the guidance and recommendations of the PRA Working Group with respect to:
>
> - The associated intended uses, and
> - The state of technology of risk assessment and related technical disciplines."

The PRA ERG received a predecisional draft of the NRC PRA Working Group's report, "Guidance for NRC Staff Uses of Probabilistic Risk Analysis," early in February 1993 and met with representatives of the Working Group and their consultants on February 17 and 18, 1993 to discuss this predecisional draft report, in the light of the PRA ERG charter and the ACRS letter. Each member of the ERG was asked to prepare individual comments on this document. These were provided on March 10, 1993.

The PRA ERG then received and reviewed a revised draft of the Working Group's report, dated April 16, 1993 and met with representatives of the Working Group and their consultants on June 29 and July 7, 1993 to review and resolve their findings.

The PRA ERG then received and reviewed a final draft of the Working Group's report, dated October 8, 1993. Each member was again asked to prepare individual comments on this draft. These are provided, as received by BNL, as Appendices A, B, C, and D of this report.

## APPENDICES

PLG

ENGINEERS • APPLIED SCIENTISTS • MANAGEMENT CONSULTANTS

PLG, Inc., 4590 MacArthur Boulevard, Suite 400, Newport Beach, California 92660-2027
Tel. 714-833-2020 • Fax 714-833-2085

Washington, D.C., Office
Tel. 202-659-1122 • Fax 202-296-0774

November 2, 1993
BNL-7629-PLG-11

Dr. John R. Weeks
Department of Nuclear Energy
Brookhaven National Laboratory
Associated Universities, Inc.
29 Cornell Avenue, Building 197C
Upton, Long Island, NY 11973-5000

Dear John:

A REVIEW OF NRC STAFF USES OF PROBABILISTIC RISK ASSESSMENT
OCTOBER 8, 1993

The revision addresses my comments satisfactorily. I have only a few additional comments, most of which are minor. Additions to existing text are highlighted.

**Main Report, Page 1, Lines 33-34:** Eliminate the last sentence of the footnote and substitute the following three sentences:

> The term "risk" should be restricted to either the risk triplet definition widely used in PRA work or the more simplified "aggregate risk," defined as the sum (over all i) of the product of $f_i$ and $x_i$. While the use of aggregate risk estimates provides a simple message, it can also provide a misleading message. The use of aggregate risk can mask information conveyed by the specific numerical values of the frequency and consequence. (See Appendix C, Pages C.118-C.126, Section C.4.4.4, Risk Calculation, for a more complete discussion.)

This makes the main report more consistent with the detailed guidance in Appendix C.

**Appendix B, Page B-10, Essential PRA Elements:**

An essential PRA element is consideration of the uncertainty introduced by our state of knowledge regarding the problem at hand. The guidance in the third bullet on Page B-21 and the discussion on Appendix C, page C.173, lines 1-18 make a clear statement of the importance of uncertainty analysis and sensitivity analysis in the decision-analysis process. I strongly recommend that this attitude be reflected in the discussion of uncertainties in the paragraph on Page B-10 as well. There is a difference between doing formal uncertainty

analysis and being cognizant of the uncertainties. I agree that formal uncertainty analysis need not be done when operational events are screened. However, a statement of the uncertainties involved and a discussion of how the sensitivity analysis addresses those uncertainties are certainly appropriate. I recommend that the paragraph be reordered and modified to read:

> Essential PRA Elements: Point estimates (see Appendix C) are satisfactory for screening and prioritization uses. Class-specific event trees and fault trees, with generic recovery actions, are used to provide point estimates of conditional core damage probability. For screening with PRA, it is important that uncertainties in our state-of-knowledge relative to the specific operational event being studied be explicitly delineated and discussed. Sensitivity studies illuminate the importance of key assumptions, uncertainties, and other factors, and they should be incorporated into the screening analysis. However, formal uncertainty analysis is not necessary.

**Appendix B, Page B-10, Essential PRA Elements:** Following this same line of reasoning, the paragraph on page B-11 should be changed to read:

> Essential PRA Elements: The essential PRA elements used in this program are the calculation of consequences and risk to the public (in terms of person-rem averted) in addition to core damage frequency and the calculation of point estimates (see Appendix C) with sensitivity studies on key variables. These sensitivity studies are intended to ensure that the overall ranking given to an issue is not sensitive to uncertainties in our state of knowledge and key assumptions made in the analysis. An formal uncertainty analysis is currently not considered to be necessary for these studies.

**Appendix B, Page B-23, last paragraph:** Starting at the 5th line from the bottom, change to read:

> ...and the two figures (frequency and consequences) are multiplied together to obtain an estimate of the "aggregate risk" of the event. (See Appendix C, Page C.118, for a definition of aggregate risk.) In other cases, there may be a continuous release, and the "frequency" parameter becomes a release rate ~~unity~~. Issues governing normal effluent release would come under this category. These cases are not event-oriented, but it is still possible to estimate risk.

The first change makes this page consistent with the definition of risk in the first comment. The term release rate provides a more clear description of what is happening

**Appendix B, Page B-27, Use of Judgmental "Data"**

Add after the sentence, ending on Line 11:

> When conservatism is used, it can be put into perspective by stating the analyst's realistic estimate, the conservative estimate, and the confidence that the parameter in question will be no worse than the conservative value used.

**Appendix C, Page C.12, Line 16-31, Concept of Probability of Frequency**

I think that the writers are confused about my definitions of frequency and probability. I propose the following paragraph to replace the paragraph on Page C.12, Lines 16-31:

> The concept of probability of frequency, which is an integration of both notions of probability, was introduced in Reference C.2.10. Very simply, frequency refers to the outcome of any experiment or observation that can be repeated. As such, it is, in principle, a quantity that can be measured. The units of the denominator can be any repeatable quantity, be it time, starts, application of shocks, or events. Probability, on the other hand, relates to our state of knowledge regarding the uncertainty of that observable quantity. The concept is applied when we state that our interpretation of the evidence leads us to believe that there is a 95% percent probability that the frequency is less than a specific observable value. Thus, the probability of frequency concept invokes a broader definition of frequency than the traditional "per unit time" usage, and imposes a more definitive definition of probability.

**Appendix C, Page C.25, Lines 5-14**

The first four disadvantages simply state that a Bayesian analysis can sometimes be done poorly. We agree with this entirely, and suggest that the lead-in to the advantages and disadvantages have the following statement added just before Line 27 of Page C.23:

> To use the Bayesian method, one must do the analytical work necessary to provide a credible basis for applying the indirect evidence available. The Bayesian approach cannot be used by an analyst to simply "put forth" a prior distribution. The prior distribution represents the interpretation of available evidence to produce a state of knowledge, but this interpretation also provides the challenge to communicate the reasonableness of the evidence to those who would question the judgment. It is through this consensus-building process that the Bayesian method can provide added value to the decision-analysis process. If the process is not followed, there are opportunities for misinformation and therefore miscommunication.

**Appendix C, Page C.25, Lines 26-27**

I suggest that this "advantage" be deleted. Discussions of both classical and Bayesian methods need to caution that sample data may not be taken under all the conditions that the equipment may be expected to operate. In both methods the applicability of the sample data must be addressed. The statement regarding Bayesian samples on Page C.24, Lines 26-27, could especially be misinterpreted. One needs to understand both the way the sample data were obtained and their applicability to the parameter being estimated in order to make proper use of it.

**Appendix C, Page C.26**

I suggest that the following be added after Line 8 of Page C.26:

> Since Bayesian likelihood functions of standard failure data use Poisson and binomial models, when there is strong direct evidence, both approaches will produce very similar results.

**Appendix C, Page C.32, Lines 27-32**

Delete. This paragraph essentially duplicates Page C.31, Lines 30-37.

**Appendix C, Page C.198, Lines 7-8**

Again, this recommendation needs to distinguish between considering uncertainties and doing formal uncertainty analysis. I suggest that it be changed to read:

> For PRA uses in screening issues, uncertainties that could influence priorities need to be identified and addressed as part of the sensitivity analysis. However, formal uncertainty analysis is generally not necessary.

If you have any questions, please let me know.

Very truly yours,

B. John Garrick

APPENDIX B

1

COMMENTS ON

PRA WORKING GROUP REPORT

Draft Final Version October 8 1993

A REVIEW OF NRC STAFF USES OF

PROBABILISTIC RISK ASSESSMENT

Bernard Harris

1 November 1993

General Comments. The document under review is a revision of the draft report dated 16 April 1993. The revisions have been made following discussions with the External Review Group, which were held in Denver, Colorado and Albuquerque, New Mexico on 28 June 1993 and 7 July 1993 respectively. Overall, the report, as before, conveys the feeling that the NRC staff is not very adept in the application of PRA methodology and is lacking in the training necessary to properly utilize PRA methodology in the regulatory process. These shortcoming needs to be remedied.

It is apparent that much of the PRA methodology will employ various software packages. Many of these will be proprietary. It is clear that validation of these software packages is needed and that comparisons between the various results obtained using different software packages should be investigated. Also, I suggested that it may be desirable to have some type of certification procedure for individuals and/or organizations that perform PRA analyses.

On reading the present draft, almost all of my previous comments have been dealt with satisfactorily. However, some of the revisions that have been made have introduced some additional problems that need to be addressed. Most of my specific comments will refer to the sections on probability and statistics and uncertainty.

Detailed comments follow.

Specific comments.

p. C.11, l. 10. change "is" to "are". Also, there is substantially more to "classical" statistical inference than confidence intervals and testing of hypotheses. This would include point estimation, analysis of variance and the design of experiments. (Actually testing of hypotheses and confidence intervals are the same topic, since there is a fundamental duality between them.)

p. C 12, l. 16 - l. 31. I feel that a typical reader will find this paragraph very confusing. Substantial rewriting is needed.

p. C 13, l. 9 - l. 14. The statement concerning the validity of the "rare event approximation" has been substantially improved, but is not yet completely accurate.

If the n events are mutually exclusive, there is no error as noted in the Working Group Report. However, the probabilities of the n events can be very small and there may still be a

substantial error. An upper bound for the error is n*n*maxP(Ai) and this is valid whether or not the events are independent. This can be verified by using the "Bonferroni inequalities" and perhaps a reference to them might be in order. The last statement - referring to 3 significant figures - does not seem to be needed ( and is suspect, in any event, since the qualification given, is not precise) if the error bound is given. Comment [183] is not correct - the error can be very large, even in the case of "rare events".

p. C 14, l 11-24. The formula on line 13 is correct, if the events are independent and is equivalent to the inclusion-exclusion formula in that case. However, if the events are not independent, then it is not necessarily an upper bound for the left hand side. It is easy to construct examples for which it is an upper bound and also to construct examples for which it is not an upper bound. As noted before (see [185], this can fail badly as an approximation.

p C. 15, two typos. l.5, change "uncountable" to "uncluntably".
l. 17, change "variable" to "variables".

p. C. 16, l. 15. It is conventional to denote the mean by "mu" not "m". also p. C. 17, l. 5.

p. C 17, l. 14. Consider f(X|Y). This is unusual notation. It ias customary to use upper case letters for the random variables and lower case letters for the values they assume ( which are also

the arguments of the probability density function). Also, it appears that independent random variables have never been defined, although independent events have been.

p. C. 17, l. 25. change " the value" to "a value" and " a probability" to " the probability".

p. C. 18, l. 5. last word in line should be "a", not "the", since percentiles need not be unique.

p. C. 20, l. 7. 95% is too specific, a tolerance limit could be an 80%, 60%, 90%, etc., tolerance interval.

p. C. 21, l. 14. I do not regard testing of hypotheses as "estimation" .

p. C. 21., l. 25. Delete "as much as possible".

p. C. 24, l. 35-39. I concur in part with your comment [214]. There exists no exact confidence interval procedure for the "Behrens-Fisher" problem. For point estimation, there is "always" a classical procedure. Also, there is a "problem" with "exact", since "all models are approximate", whether Bayesian or frequentist. However, I feel that such philosophical discussions are beyond the scope of the document. I suggest clarifying the discussion by changing "estimation" to either "interval estimation" or "confidence intervals" and dropping or modifying "exact".

p. C. 25, l. 1-3. Here a frequentist would presumably use "empirical Bayes".

p. C. 25, l. 15. change "precedences" to "precedents".

p. C. 28, l. 25 - 35. I agree that the Poisson process is inappropriate. I can not recall what I had in mind, except that is should be also feasible to model the denominator in a logical way. At the moment, I do not have any reasonable specific proposal, but will continue to ponder possible alternatives.

p. C. 36, ref. 2.14. Is there a later edition of Mood et al?

p. C. 39, l. 26. Clumsy word order.

*p. C. 42, l. 8. Here and in many other places, approximations are denoted inappropriately by "=". In some instances noted later, it is not indicated that an approximation is being used and the limitations on the validity of the approximation is not noted. In most of these instances, the approximation is obationed assuming an exponential distribution and small values of "t". It would also be desirable to obatin error bounds on the approximation.

p. C 42, l. 3,4. Here and in several other places, the statements are not punctuated appropriately. (This minor problem did not appear in the earlier version of this chapter).

p. C 42, l. 13. Rewrite as "In a typical case, a component has both ....."

p. C 42, l. 24. sign error in equation.

p. C 42, l. 27. see *.

p. C 43, l. 6. see *.

p. C 43, l. 18. Components in a system need not be "physically" interconnected.

p. C. 43, l. 35 contradicts l. 27.

p. C. 44, l. 23. The reliability of standby components need not be (stochastically) dependent on the reliabilities of other components.

p. C. 46, l. 3. Typo change FME to FMEA.

p. C. 46, l. 7. Identifying causes from observbed events is "inductive", not "deductive".

p. C. 47, l. 7. If a system is a two-out-of-three system, in my usage, it is not parallel. Parallel is one-out-of-n.

p. C. 49, l. 10. Proposed editorial change. Replace " reasons for

this dependence" by "nature of the dependence".

p. C. 49, l. 17-18. see *.

p. C. 53, l. 11-13. A useful alternative is to introduce utilities.

p. C. 53, l. 28-29. Grammatical problems.

p. C. 53, l. 30 insert "the" between of and occurrence.

p. C. 54, l. 38. In the usual frequentist model, if you assume that the two components have the same failure rate, you obtain a "better" estimator if you pool the test data on both components and estimate by q2.

p. C. 55, l. 3-4. I would presume that this should depend on the loss function utilized.

p. C. 57, l. 1. Layout and punctuation are not satisfactory. This was better in the previous version.

p. C. 58, l. 20. layout, punctuation.

p. C. 60, l. 1-4. I like the previous version better.

p. C. 66, l. 24 ff. It appears that my previous comment [247] was misunderstood. I was not referring to software reliability and/or

codes, but to "Time Dependent Modeling".

p. C. 119, l. 13. The introduction of utility permits "quantitative" not "qualitative" comparisons, since that creates a linear ordering. The risk triplets provide only a partial ordering.

p. C. 120, l. 1-6. Aggregate risk is an expected value. Let X(i) be the number of occurrences of accident i in [0,T]. Then E(Sum C(i)*X(i)/T) is algebraically equal to Sum C(i)*F(i).

p. C. 126, l. 33 ff. This is a mathematically delicate topic, due to lack of robustness. No changes should be made at this time, but I feel that this is an important topic for detailed investigation in the future.

p. C. 148, l. 27. Suggested rewriting. This section focuses on the methods used for formally obtaining and processing .....

p. C. 173, l. 15. "Complementary" seems to be the wrong word. Two items are complementary if together they constitute the "whole". This applies not only in the mathematical usage, but also in the literary usage. The point that one wishes to make is that sensitivity analysis provides additonal information, which is not included in the uncertainty analysis.

p. C. 178, l. 19. "Classical" methods do result in probability distributions for the output.

p. C. 179, l. 9. It is preferable to utilize a joint distribution for all variables, since this will permit identification of interactions. Selecting a distribution for each input variable separately is adequate only for linear systems.

p. C. 179, l. 29 - 40. This is a big improvement over the previous version. The empirical CDF is always a sufficient statistic and this will not cause the same loss of information as restricting attention to confidence intervals. The sampling distributions of point estimators for various models is also a worthwhile adjunct to the analysis.

p. C. 180, l. 10. It is always difficult to get accuracy in the tails, since observations are rarely obtained in the "far tails".

p. C. 181. l. 10 ff. Latin hypercube sampling permits joint sampling as well as individual sampling of variables. As noted before, joint sampling is to be preferred.

p. C. 190, l. 5. One-at-a-time methods are risky to use, since they obscure "interactions".

p. C. 197, l. 31-35. It should be possible to devise methods to study the diferences between plants and I have some preliminery thoughts on the possibility of doing so. Again, this is in the realm of future activities and not relevant to revisions of the present report.

APPENDIX C

**Comments on the October 8, 1993 Draft Report**

**"A Review of NRC Staff Uses of Probabilistic Risk Assessment"**

**by**

**Ralph L. Keeney**

**October 29, 1993**

I have read the main report, Appendix, and the chapters on "Expert Judgment" and "Uncertainty and Sensitivity Analyses" of NRC. The authors did a very thorough job of incorporating my comments on the previous review of this draft. I agree with the positions taken in the report and believe that it should be beneficial to the NRC to help address the comments in the ACRS letter dated July, 1991. I would like to mention, however, that if the recommendations referred to in the "Reply to Comments from the External Review" are followed, the concerns of the ACRS will more likely be significantly reduced. Specifically, the "Reply" makes recommendations "that follow-up reports be written that discuss in one place present risk assessment and risk management practices as well as an evaluation of how else risk assessment should be used at NRC" and "that the concept of decision analysis be pursued by the staff for possible future use in the staff's decisionmaking process".

In closing, let me make two minor comments:

1. Appendix A does not provide a copy of the survey distributed to the staff. It should provide this and this would be easy to do.

2. On page 182 (line 4), one change of "precision" to "accuracy" did not get included on this draft.

APPENDIX D



# DEFENSE NUCLEAR FACILITIES SAFETY BOARD

625 Indiana Avenue, NW, Suite 700, Washington, D.C. 20004
(202) 208-6400

November 1, 1993

Dr. John Weeks
Department of Advanced Technology
P.O. Box 5000
Brookhaven National Laboratory
Upton, New York 11973-5000

Dear John:

I have received the Draft Final Version (dated October 8, 1993) of the NRC PRA review and have reviewed it in the light of the comments I had previously rendered as a member of the External Review Group.

I find that this Final Version is greatly improved over the previous one from the standpoint of its literary and technical content and that it adequately addresses the comments I had submitted at the Albuquerque meeting of the External Review Group. It is my opinion that the Draft now provides good guidance to the NRC staff on the means and extent of use of PRA in regulation, up to the points where the Draft makes recommendations as to further analysis that should now be done.

Sincerely,

Herbert Kouts

Herbert Kouts