First Report of the NRC PRA External Review Group

John R. Weeks

March 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 ACRS letter is given as Appendix A in this report. 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 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 are provided, as received by BNL, as Appendices B, C, D, and E of this report.

APPENDICES

A	Letter, D. A. Ward, Chairman ACRS, to I. Selin, July 19, 1991
B	Comments by Dr. B. John Garrick, dated March 5, 1993
C	Comments by Dr. Bernard Harris, dated March 7, 1993
D	Comments by Dr. Ralph L. Keeney, dated March 3, 1993
E	Comments by Dr. Herbert J. Kouts, dated March 10, 1993

APPENDIX A

UNITED STATES
NUCLEAR REGULATORY COMMISSION
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
WASHINGTON, D. C. 20555

July 19, 1991

The Honorable Ivan Selin
Chairman
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Chairman Selin:

SUBJECT: THE CONSISTENT USE OF PROBABILISTIC RISK ASSESSMENT

During the 375th meeting of the Advisory Committee on Reactor Safeguards, July 11-13, 1991, and in earlier meetings, we discussed the unevenness and inconsistency in the use of probabilistic risk assessment (PRA) in NRC. PRA can be a valuable tool for judging the quality of regulation, and for helping to ensure the optimal use of regulatory and industry resources, so we would have liked to see a deeper and more deliberate integration of the methodology into the NRC activities. Our recommendations to this end are directed at problems that took time to develop, and are likely to take a long time to solve.

PRA is not a simple subject, so there are wide variations in the sophistication with which it is used by the various elements of NRC. There are only a few staff members expert in some of the unfamiliar disciplines -- especially statistics -- that go into a PRA, so it is not surprising that there are inconsistencies in the application of the methodology to regulatory problems.

To illustrate the problems, let us just list a few of the fundamental aspects of the use of PRA, in which different elements of the staff seem to go their own ways. These are just illustrations, but each can lead to an erroneous regulatory decision.

1. The proper use of significant figures is in principle a trivial matter, but it does provide a measure of a person's understanding of the limitations of an analysis. Yet we often hear from members of the staff who quote core-damage probabilities to three significant figures, and who appear to believe that the numbers are meaningful. It is a rare PRA in which even the first significant figure should be regarded as sufficiently accurate to play an important role in a regulatory decision, but there is something mesmerizing about numbers, which imbues them with misleading verisimilitude.

They deserve respect, but not too much, and it is wrong to err in either direction.

2. Closely related is uncertainty. There is no way to know how seriously to take the results of a PRA without some estimate of the uncertainty, yet we often hear thoroughly unsatisfactory answers (some perhaps invented on the spot) when we ask about uncertainty. One of the advantages of PRA is that it provides a mechanism for estimating uncertainty, uncertainty which is equally present, but not quantified, in deterministic analyses.

3. Conservatism. A PRA should be done realistically. The proper time to add an appropriate measure of conservatism is when its results are used in the regulatory process. If the PRA itself is done with conservative assumptions (more the rule than the exception at NRC), and is then used in a conservative regulatory decision-making process, self-deception can result, or resources can be squandered.

   The inconsistent use of conservatism was illustrated by a pair of briefings at our April 1991 meeting, which included updates on proposed rules on license renewal and on maintenance. In the former case, we were told that a licensee could use PRA to add an item for later review, but never to remove one -- a one-way sieve. In the latter case we were told that PRA could be used to justify either enhancement or relaxation of maintenance requirements. Foolish consistency may be a hobgoblin, as Emerson said, but there is nothing foolish in seeking consistency in regulation.

4. The bottom line. It has been widely recognized since WASH-1400 that the bottom-line probabilities (of either core melt or immediate or delayed fatalities) are among the weakest results of a PRA, subject to the greatest uncertainties. (That doesn't mean they are useless, only that they should be used with caution and sophistication.) Yet we find staff members unaware of these subtleties, often dealing with small problems, justifying their actions in terms of the bottom-line probabilities. This is only in part due to the Backfit Rule, which almost requires such behavior; it is also inexperience and lack of sensitivity to the limitations of the methodology.

   A number of staff actions and proposals use bottom-line results of a PRA as thresholds for decision making, often with the standard litany about the uncertainty in the reliability of these results. In fact, the quantified uncertainty in the bottom-line results of a PRA is just as important a number as the probability itself. It would be straightforward to employ a decision-making algorithm that <u>prescribes</u> a confidence level for the decision, and uses both the bottom-line probability and the uncertainty to achieve this. A further improvement would be to incorporate the consequences of erroneous decisions, what statisticians would call the loss function, into the decision-making process. The Commission has come close to this approach in its recent instructions to the staff on the diesel generator reliability question.

These are just a few examples of problems with the use of PRA in NRC, all common enough to be disturbing, and increasing in frequency as the use of PRA increases. It has been more than fifteen years since the publication of WASH-1400, a pioneering study which, despite known shortcomings, established the NRC at the forefront of quantitative risk assessment. One could have hoped that by now a coherent policy on the appropriate use of PRA within the agency, on both large and small problems, could have evolved.

We recommend that:

A. A mechanism be found (perhaps a retreat) through which the few PRA and statistical experts now scattered throughout the agency (and generally ignored) can be brought together with the appropriate senior managers and outside experts, to work toward a consistent position on the use of PRA at NRC. It could be worth the time expended. (Among other long-term benefits, such an interaction would add an element of horizontal structure to the NRC's predominantly vertical organization.)

B. The Commission then find a way to give credence and force to that position.

C. The Commission emphasize recruitment of larger numbers of professionals expert in PRA and statistics.

D. The Commission consider some kind of mandate that any letter, order, issue resolution, etc., that contains or depends on a statistical analysis or PRA, be reviewed by one of the expert PRA or statistical groups.

We do not pretend that this is an easy problem. The solution involves not only a cultural shift, so that those few experts already at NRC have some impact, but also substantial enhancement of the staff capabilities. That will require incentives that only the Commission can supply. It is interesting that the Commission's Severe Accident Policy Statement, dated August 1985, stated that "within 18 months of the publication of this severe accident statement, the staff will issue guidance on the form, purpose and role that PRAs are to play in severe accident analysis and decision making for both existing and future plant designs...."

Additional comments by ACRS Members Harold W. Lewis and J. Ernest Wilkins are presented below.

Sincerely,

David A. Ward
Chairman

Additional Comments by ACRS Members Harold W. Lewis and J. Ernest Wilkins

We thoroughly endorse this letter, and regret only that the Committee chose to ignore the parallels between the PRA problems and those in a number of other newer technologies significant to nuclear safety. Recommendation C should have included mention of some of these -- electronics and computers, for example -- which are of increasing importance. Weaknesses in those areas also need correction. Computerized protection and control systems, in particular, require the kind of sophisticated review that NRC is in no position to provide.

APPENDIX B

REVIEW COMMENTS

by
B. John Garrick

Member, PRA External Review Group
U.S. Nuclear Regulatory Commission

A Review of the Draft Predecisional Document

GUIDANCE FOR NRC STAFF
USES OF PROBABILISTIC RISK ANALYSIS

March 5, 1993

PLG

ENGINEERS • APPLIED SCIENTISTS • MANAGEMENT CONSULTANTS

Review Comments

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REVIEW COMMENTS

by
B. John Garrick

GUIDANCE FOR NRC STAFF USES OF PROBABILISTIC RISK ANALYSIS

INTRODUCTION

The U.S. Nuclear Regulatory Commission (NRC) is to be commended on trying to come to grips with the direction to take in the use of the important discipline, probabilistic risk assessment (PRA). This review, while quite critical in tone, is nevertheless done with a great deal of optimism about the initiatives being taken by the NRC to employ PRA in the most effective way possible.

The charter for our review was defined in the subject document, as follows:

> "To review the technical adequacy of the guidance and recommendations of the PRA Working Group with respect to the associated intended uses and, as appropriate, to the state of technology of risk analysis and related technical disciplines."

The review comments are organized into basically three groups:

- General Comments
- Comments on Appendix B
- Comments on Selected Sections of Appendix C

Since the meat of the guidance document is Appendices B and C, the review concentrates on these areas. However, the General Comments do provide a broad brush of the entire document. With respect to Appendix C comments, the reviewer has consulted specific experts within PLG's staff for assistance.

GENERAL COMMENTS

1. The fundamental questions of why and how NRC should use PRA have not been answered. Thus, the "consistent position" question asked by the Advisory Committee on Reactor Safeguards (ACRS) remains an open issue. As it is now, the burden of how to use PRA is on members of the staff and their individual preferences and opinions. The results are inconsistencies in the use of the PRA language, lack of definition of terms and concepts, and opportunities for miscommunication and general confusion.

   There is little doubt that the fundamental goal of PRA at the NRC should be to support regulatory decision making under uncertainty. This is the strength of PRA.
   To address the principles under which this can be done, and thus provide more

consistency, I suggest that the introduction to the guidance document emphasize in a positive way that a PRA can provide an NRC staff member with valuable insights that cannot be easily obtained from other tools. I consider the following three characteristics to be of prime importance for guiding the staff member's use of PRA as a regulatory tool:

- The PRA represents a comprehensive expression of our understanding how the various systems in the plant function, interact, and support each other to mitigate against initiating events to prevent a negative consequence (for now, core damage, release of radioactive materials, and their dispersal to produce a health effect).

- The structure of the PRA model permits us to describe the physical conditions under which each system must accomplish its functions, and make those circumstances conditional on both the initiating event and the success or failure of other systems. In essence, it permits us to define system functional reliability assessments of physical response calculations in the context of event sequence categories in which they could be expected to occur. It then enables us to prioritize the need to accomplish them based on the frequency we can expect to encounter those conditions and the potential consequences of an unfavorable outcome. Finally, it provides us a place to quantitatively use the results. In other words, it provides a practical context for all safety evaluations.

- When the model is quantified, it provides us with a "bottom line" set of accident scenarios that can be ranked, decomposed, and tended to determine where plant vulnerabilities exist and what options are available to mitigate against them. Because the accident scenarios arise from the combination of our understanding of the plant response and the values of the individual functional failure rates, we can gain insights into specific changes to plant procedures or systems that could improve safety. Conversely, the PRA can be used to evaluate the risk effectiveness of regulations and proposed changes to plant operating procedures or hardware.

- Truly effective application of the PRA will come only from people who are thoroughly familiar with how the plant systems respond to an accident initiator and the fidelity of the PRA model to that response. They must also be cognizant of the limitations of the PRA methods. In other words, the PRA results need valid interpretation in the context of realistic plant event sequences.

With these ideas in mind, the NRC's use of PRA should be keyed to verifying that the PRA reflects an accurate understanding of how the plant functions, and once that is accomplished question new evidence in terms of how it impacts the ability of the plant to successfully mitigate against new events or issues. The guidance for individual applications would then expand on how the staff can apply above principles to specific types of problems.

2. The guidance document places much emphasis on such PRA-related disciplines as statistics and reliability, and way too little emphasis on knowing how plants actually

work and how they can get into trouble (accident scenarios). It is the latter that determines the quality and credibility of any PRA. An analyst who can walk through an accident sequence while explaining details on plant response is ever so much more a PRA expert than one that knows what a minimum cut set is.

It is time that we broke down the knowledge barriers between the regulators and the licensees. This industry would benefit beyond what we can imagine if there were sincere technology exchange between these two groups. Every PRA expert who is a regulator should have the experience that comes from first working at a facility of the type he is trying to regulate or with outside consulting organizations clearly established as experts in the field. Lack of in-depth knowledge of plant and facility details (operations, maintenance, and engineering) and their relationship to the PRA will be the overpowering deficiency in the skill level of the NRC staff to use the guidance document effectively. This requirement must be addressed in the NRC's training program.

3. Related to comment (2) but worth its own space is the matter of the role of industry efforts in PRA in relation to the NRC. For reasons that are partly understood but clearly unjustified in my mind, the NRC functions, in a PRA technology sense, as if there were no industry out there. Maybe it's the regulator-driven obsession with the well-known "conflict of interest" bogeyman. Is this the reason why major works by industry are hardly ever referenced in NRC reports? Whatever it is, it is a disaster in terms of efficiency and quality control of the nuclear power industry. It is at the very heart of our ability to be competitive, especially in the international marketplace.

   In a more narrow view, it is clear that if NRC were much more in tune with the industry work in PRA, a lot of the deficiencies noted in this review would simply not exist. And, of course, industry and the public would benefit as well. The current efforts by NUMARC and EPRI to integrate PRA into consideration of the maintenance rule and issues marginal to safety present an excellent opportunity for the NRC to actively cooperate with industry to achieve guidelines that both can live with.

4. There are a number of opportunities for improvement in the use of PRA at NRC that are repeated several times in the specific comments that follow. Besides those identified in (2) and (3) above, they appear throughout the document, and relate to such matters as:

   - The issue of applying generic safety issues resolution without full consideration of site specific mitigation capabilities.

   - Inexperience with full-scope PRAs and therefore all of the analysis activities associated with how to tie together the three levels of PRA into an integrated package, including the use of event sequence diagrams.

   - The treatment of uncertainty and the sense that it is synonymous with elaborate statistical analysis.

The guidance appears to recognize that fixes to address so-called generic safety issues in some cases have been inappropriately applied to plants that have already had alternate mitigation capability in place. This can occur when regulatory guidance is applied based on assumptions of plants different from those to which the rule will be applied. The guidance in B.4 suggests that the NRC will make its assessments apply to an explicitly defined class of plants, down to one site if necessary. (p. B-30, lines 6-15) I certainly agree with this approach. In fact, it might even be appropriate to take this idea to the limit and deal only with individual plants.

Since the guidance also recognizes that there is both a positive and a negative aspect of any proposed change, we encourage the working group to maintain a flexible attitude towards any type of grouping. With the existence of the IPEs, it may be more efficient to deal with the individual plants. While this may sound time consuming, in the end it probably will save time (we don't have to worry about the boundaries of the group changing as the issues change) and will certainty enhance the likelihood of defining meaningful options for meeting the concern in terms of the resources available to the utility. This approach will have the added advantage of enabling one or more staff members to become the NRC expert on each specific plant. It is through this thorough understanding that truly effective and efficient regulation of nuclear power will evolve.

On the matter of inexperience with full scope PRAs, the NRC would greatly benefit from more extensive use of tools that keep risk issues in context. Such practices aid the discussions with licensees, ACRS, the public, etc., as well as convey a clearer understanding of the issues involved. An example is to use event sequence diagrams that preserve plant response language and clearly indicate the sequence and structure of events as they might logically occur. Such presentations do not require an understanding of esoteric logic diagrams and yet tell it like it really is.

While it is very true that the NRC has made great progress in recognizing the importance of quantifying uncertainty, there is still confusion on how and when to do it. If the simple view is adopted that uncertainty is admitting what we don't know, it is always possible to quantify it and to do so without the necessity of elaborate statistical analysis. We may not like the result but the point is we can always quantify the uncertainty. Therefore, in all safety management activities, screening, prioritizing, special assessments, etc., uncertainties should be considered if not analyzed. It is illogical to not treat uncertainties in the front end of an analysis or for so called screening or preliminary analysis when in fact the results often determine the scope of the more detailed analysis.

5. Finally, with respect to what might be considered a "nit," I strongly suggest substituting the word "assessment" for the word "analysis" in the title and text of the guidance document. This, of course, follows the long-established tradition started by the NRC of referring to this business as probabilistic risk assessment as opposed to "analysis." Besides, "assessment" is a better descriptor of the scope of what is actually done in a PRA.

Review Comments by B. John Garrick

Appendix B

Review of and Recommendations on
Agency PRA Uses

INTRODUCTION TO SPECIFIC COMMENTS

The specific sections reviewed are identified in the Table of Contents. The specific comments are generally made by page and line number. A different approach was taken for Section C.2.4 Here, a modification of the entire section is presented. The deletions of the old section are shown as ~~line-throughs~~, and the additions are shaded to enable the reader to see exactly what the proposed changes are.

Page B-3, Lines 6-20:

It is not clear that the eight bullet items include discussions on the following important disciplines:

1. Dependency Analysis in General
2. Common Cause Analysis in Particular
3. Human Response Analysis
4. Knowledge of Nuclear Plants in Terms of Design, Operations, and Maintenance.

The reviewer observes that the first three disciplines are, in fact, explicitly addressed in Appendix C.3. However, the fourth is critical if the PRA is to represent our understanding of how the plant responds to mitigate against core damage.

Page B-4, Lines 1-23:

In spite of the referenced effort by RES to develop guidance on advanced reactor PRAs, there would still be considerable value in the working group addressing matters of scope until such guidance is available. The concern is that the advanced design PRAs are not considering important contributors to risk such as support system dependencies, human actions, and common cause. Since these are the principal contributors to most full scope risk assessments, and since there is a tendency to compare advanced design risk with current light water reactor risk, it would appear that some near-term guidance is crucial.

Page B-4, Line 30:

While "license issuance" is identified as a purpose, there were no specific examples of such nor was there any connection made to the IPEs — why?

Page B-5, Lines 45-58; and Page B-6, Lines 1-16:

The specific examples of the licensing actions on B-5 and B-6, while interesting, are not nearly so profound as might be expected. Maybe this is because these are actions that have been taken as opposed to actions that are in review. It is known that more major applications of changes to technical specifications are under consideration (e.g., South Texas Project) that might better demonstrate the impact of PRA. The point is that PRA has much greater potential for impacting licensing actions than is conveyed by the examples cited.

Page B-6, Lines 41-43:

Experience indicates that the most meaningful importance ranking is with respect to event sequences (scenarios) because context is preserved. The high emphasis of the guidance document on hardware runs the risk of unrealistic rankings and loss of perspective.

Page B-7, Lines 30-32:

A real risk management opportunity is how to get the most benefit out of the IPEs. Considering the resource base generated by the IPE program there appears to be a now in place a capability for each utility to evaluate the risk impact of some requirements imposed by the NRC, this capability should be exploited.

Page B-10, Lines 29:

Suggest that the phrase "uncertainty analysis is not necessary" be deleted from the first line of this paragraph. While it may be true that for some situations it is not necessary to require elaborate uncertainty analysis, it is never true that uncertainties should not be considered. The conflict here is that the guidance could be misinterpreted as suggesting to not consider uncertainty for applications where the uncertainties are the greatest. This problem can be eliminated by changing the sentence beginning of Line 35 to read "While formal uncertainty analysis may not be warranted at this stage, the uncertainties involved in the point estimates are important and sensitivity studies to illuminate the importance of key assumptions, uncertainties, and other factors must be accomplished.

Page B-11, Line 28:

Person-rem as a consequence performance parameter obscures the real risk to individuals and therefore the public. The maximum offsite individual risk ought to be calculated as well over a very large population. Because of the relatively high threshold dose levels for detectable biological damage, large person-rem values resulting from the integration of very low doses may not be the best means to communicate individual risk. One always knows the score with individual dose levels. (Applies to Page B-18, Line 28 as well.)

Page B-11, Lines 30-31:

Change the sentence to read: "Although detailed, formal uncertainty analysis is not considered necessary for these studies, the evidence regarding uncertainties of the key assumptions and variables that bounds the extent of the sensitivity study should be documented."

Page B-12, Line 1:

The idea of a "risk index" is a powerful concept. However, it is a concept that can only be based on plant-specific risk assessments and not on general analyses.

Page B-12, Line 34:

Since LERs and NPRDS data have not been a principal source of data for PRAs, there is concern with the suggestion that these databases are the principal sources for the NRC calculation of core damage frequencies. For a PRA to have any quantitative meaning as indicated elsewhere in this review, most of the data has to come directly from the plants or facilities involved. However, the LER and NPRDS data can be evaluated using plant specific PRAs. This can be done in two ways:

First, the circumstances of LER or NPRDS data may indicate that a class of interactions or dependencies may exist in the plant's response to potential accident initiators that should have not been considered. In other words, the data can generate a check of the completeness of our understanding of the plant's mitigation capabilities. The act of addressing the sequence of events via the PRA model can provide insights into procedure changes, training improvements, or design modifications that will allow the plant to successfully mitigate against the problem identified by the LER or NPRDS data.

Second, if the LER or NPRDS data applicable to a given plant system will change the generic distribution against which the plant specific data is updated. For example, if the data indicates that a failure mechanism may exist that plant specific testing does not address, the analyst may want to increase the plant specific failure rate to reflect the probability that the additional failure mechanism will occur. In this case the PRA can then provide information regarding how important the increase in the failure rate due to the untested failure mechanism is, and thus whether an engineering investigation is warranted.

It is in the above two contexts that the PRAs can assist the NRC analyst to put LER and NPRDS data into a reasonable context.

Page B-13, Line 12:

The comments regarding operational data analysis also apply here, as trending of equipment failure rates could be considered a subset of operational events. For these analyses, the NRC analyst would most likely use the LER or NPRDS data to change the generic distribution against which the plant specific data is updated and requantify the PRA. In most instances the dominant sequence model will be an adequate tool for this application.

One cautionary note: Using only NPRDS and LERs to identify safety and risk concerns could be misleading. These data bases are limited for PRA applications and, in fact, are used only slightly in large scope PRAs and IPEs. In many cases they are not used at all. Please see comments covering C.2.4.

Page B-13, Maintenance Rule:

The NUMARC guidance, while emphasizing risk-based approaches for ranking SSCs, permits the use of expert judgement as either a supplement to or in place of PRA models. Certainly, expert judgement will be used initially by many utilities for estimating risk significance during shutdown.

NRC guidance, once developed, should consider checking the ranking results presented by the utilities against the risk implications of their IPE and insights gained from the NRC's examination of shutdown events, as well as industry experience data. In addition, the guidance should also detail how the inspections will insure that the current understanding of plant safety, including consideration of design changes, are reflected in updated SSC rankings.

Page B-14, Generic Issues, Line 17:

Suggest that the following sentence be added to the end of the paragraph:

"Only those plants for which the issue has been identified as being applicable and producing a 'significant' increase in risk (as defined by the screening decision criteria) will be subjected to this evaluation."

We believe that an important result of the PRA process is an ability to put a safety concern into perspective on a plant-by-plant basis. The screening process should eliminate from further consideration those plants whose specific design features make some initially "generic" issue insignificant or inapplicable.

Page B-15, Line 18:

Suggest adding the following after the sentence ending with "be Level 2 studies,":

"As the Level 1 portion of the PRA provides plant damage states and frequencies under which the severe accident physical processes are evaluated, it is important that the analyst have a good understanding of those results when establishing and evaluating the physical process research."

Page B-15, Lines 42-44:

Don't really understand this comment. PRA applications are one thing and methods development another.

Page B-16, Individual Plant Examinations, Lines 28-29:

The working group's desire to put the IPEs to use is extremely timely. Positive guidance should be developed as soon as possible so that the capability to use the IPE will not atrophy. Perhaps the working group should take a lead in this area.

Some specific applications that the IPEs could be used for include:

- Take advantage of the plant specific nature of the IPE's to assist in screening new issues. Plants designed to respond in a manner that keeps the risk low can be eliminated from further requirements.
- Provide a basis for performance based regulations.
- Provide a mechanism for cross checking the risk-significance of maintenance program proposed under the maintenance rule.

Page B-17, Lines 47-48 and B-18, Lines 4-7:

The NRC staff should be encouraged to push hard for the adoption of risk based analysis as the underpin of the backfit rule. Even though the backfit rule does not require a risk based safety analysis, it is a perfect application for PRA. PRA offers the opportunity in this case to bring genuine order and logic to an otherwise complex, expensive and confusing rule.

Page B-18, Line 10:

See comment for Page B-11, Line 28.

Page B-18, Lines 42-45:

The performance assessment work for WIPF is well advanced and a very good application of the scenario based approach to risk assessment. The NRC would be well advised to use the WIPP results as source material for developing guidance. I say WIPP rather than Yucca Mountain only because their work is much more advanced from a methodology and results standpoint.

Page B-21, Lines 21-22:

Suggest the following rewrite for the bullet item consisting of lines 21 and 22:

> The analysis should make use of modern PRA logic diagrams (such as ESDs, fault trees and event trees) and other risk performance displays such as dependency matrices. Valuable references in this regard are the NUREG-1150 studies and the reviewed industry PRAs.

Page B-21, Line 31:

See comment on B-10, Line 29. Suggest that the wording of this bullet be changed to be consistent with that comment.

Page B-23, Lines 27-31:

While it is true that PRA has been used primarily to document the plant's ability to minimize core damage frequencies, PRA is a fundamental thought process applicable to any problem including releases of primary coolant to the environment. As a matter of fact, if we want to attempt to calculate "partial core damage", PRA is a very logical approach to take. The point of confusion here is deterministic <u>versus</u> probabilistic, as opposed to the way it should be thought of, namely, deterministic <u>and</u> probabilistic.

Page B-24, Lines 33-34:

Important guidance here is the observation that the thought processes employed by the analysts are much more important than the modeling assumptions. It was appropriate to call attention to the "black box" syndrome.

Page B-24, Line 47:

Definitely NUREG-1150 should be an important basis for generic issue prioritization. However, it has its limitations--it is complex and, of course, only covers 5 plants. Equally important references are the reviewed industry PRAs.

Page B-25, Lines 10-12:

Off-site consequences are not only plant-specific but site-specific as well. We need to be extremely careful with such transformations as described in NUREG-0933.

Page B-26, Line 7:

The phrase "educated guesses" is not an appropriate expression in the logic business. We should not be guessing or conveying that we are guessing. Expert judgement is a very different concept, especially if its done in context with the uncertainties and evidence involved.

Page B-27, Lines 2-3:

Again, uncertainty "analysis" is always most important when there is too little time to do it or too little information. When engineering judgement is all that there is to go on, it is especially important to try to quantify the uncertainty. This is done by considering the evidence behind the "judgement". It does not mean that an elaborate statistical analysis is necessary. There appears to be tremendous confusion on the whole uncertainty analysis issue.

Page B-27, Figure B.1:

The criteria would be better if it were based on health risk rather than core damage frequency. A low priority CDF may, in fact, be a high priority health risk, at least relatively speaking. That is, if the CDF was relatively low, but containment bypass scenarios figured heavily into the risk then from a real risk standpoint these scenarios should have a high priority. Also, uncertainties could really cloud the decisions if the uncertainties are not known--at least to a first level of approximation.

Page B-28, Line 16:

Techniques for performing PRA are a required area of proficiency, but, what may be even more important is training and knowledge on how plants actually work.

Page B-30, Lines 31-32:

Should add the following to the sentence that ends with analysis--"and familiar with the plant involved."

Page B-30, Line 46:

This is almost impossible given that such plant-specific factors as support systems, other dependencies, procedures and human actions tend to drive the risk.

Page B-31, Lines 21-23:

In the long run it would be much cheaper to just do a Level 3 analysis. A Level 3 analysis is only a fraction of the effort of a Level 1 and it closes the loop on many otherwise open questions. That is needed to avoid the "black box" syndrome.

Page B-32, Lines 13-14:

This is an extremely important observation and is probably worth repeating several times in a document such as this. There is almost always a compromise as a result of a fix.

Page B-34, Section B.4.6

The brevity of this section suggests that QA is not a big thing, when in fact it is. Much more discussion and guidance is needed on QA and review.

Review Comments by B. John Garrick
(Assisted by Shobha B. Rao)

Appendix C.2.4

Sources of ~~Reliability~~ Data

C.2.4 Sources of ~~Reliability~~ Data

In order to apply the reliability and unavailability models to be discussed in Section C.3, the analyst must first estimate the parameters of such models using appropriate statistical estimation methods, such as those discussed above, in conjunction with available ~~reliability~~ data. Parameters generally considered for such models are:

- Component Failure Rates
- Component Maintenance Rates and Mean Durations
- Common Cause Failure Rates
- Initiating Events

Four basic categories of reliability data or information about a parameter of interest (such as a demand failure rate) are often available: (1) engineering knowledge about the design, construction, and performance of the component; (2) past performance of similar components in similar environments; (3) past performance of the specific component in question; and (4) expert opinion regarding the parameter. The first two types constitute generic data or information and may include varying degrees of subjective judgment. The third type, consisting of observed sample data, is the plant- or application-specific data. The fourth type, expert judgment, is appropriate for rare events when there exist little or no generic or plant-specific data (such as for the frequency of occurrence of a severe earthquake in a region for which there has never been an earthquake of the magnitude under consideration). The elicitation and use of expert opinion is discussed in Section C.8; thus, it is not discussed here.

Some of the more common sources of generic and plant-specific reliability data are described below. In performing PRA, the quality of the reliability data is of tantamount importance and cannot be overemphasized. The analyst should ensure that the data conform to the statistical model and accurately represent the situation for which statistical inference (such as parameter estimates) are desired. The sources of data described below very tremendously in both quality and applicability. Care and caution must be exercised in both collecting and using data from these sources. For example, many generic data sources utilize the same basic failure event data and thus cannot be assumed to be independent sources. Also, industry participation in a given data base effort may be voluntary. Consequently, parameter estimates calculated from these data sources may be nonrepresentative (that is, biased). Section 5.9 of Reference C.2.17 discusses data quality.

C.2.4.1 Generic Data

Useful sources of generic ~~reliability~~ component data (failure and maintenance data) are found in the reliability databases supporting current (modern) PRAs. These PRAs are often performed for recent or current NRC-sponsored PRA programs, such as NUREG-1150 (Ref. C.2.29). NUREG-1150 developed and used a generic component database for PRAs at 5 commercial nuclear power plants. The reliability database was developed for Reference C.2.?.

An older publication from the NRC is the Reactor Safety Study (Ref C.2.15), which contained much information on methods of data collection, failure rate data, and model development for risk analysis. The Reactor Safety Study summarizes generic (and some specific) component failure rate data for nuclear plant PRAs. This data source is largely compiled using expert judgment based on non-nuclear operating experience. In spite of this and the fact that this data source is nearly 20 years old, it continues to be used as a source of generic failure rate data and is the basis of a number of other generic sources of data. However, it has now been superseded by Reference C.2.29.

The Institute of Electrical and Electronics Engineers (IEEE) publishes ANSI/IEEE Std 500 1984 (Ref. C.2.?) a manual which gives useful data on failures modes, failure rate ranges, and environmental factors on generic components actually or potentially used in nuclear power plants. It contains information on over 1,000 electrical, electronic, sensing, and mechanical components and is arranged for easy data access. The Industrial and Commercial Power Systems Committee of the IEEE conducts a survey of 68 industrial plants in 9 industries and reports on equipment failures, cost of outages, loss causes, and types of failures.

Another source of reliability data is the Nuclear Computerized Library for Assessing Reactor Reliability (NUCLARR) (Ref. C.2.9). The primary goal of NUCLARR is to establish and operate computerized data base management tools for use in estimating human error probabilities and hardware component failure rates in the nuclear power industry. It is implemented as a user-friendly, menu-driven system for retrieving and manipulating data obtained from other sources.

The International Atomic Energy Agency (IAEA) developed a component reliability database which is a good source of generic data (Ref. C.2.21). It consists of approximately 1,000 records compiled from 21 different data sources around the world. It includes data for all components usually modeled in nuclear power plant PRAs. It was compiled using DBASE III commercial software.

~~Many private consulting firms.~~ In addition to NUREG-1150 (Ref. C.2.29), general data for maintenance can be derived from the NERC/GADS reports. The National Electric Reliability Council (NERC) operates the Generating Availability Data System (GADS), formerly called the Edison Electric Institute Equipment Availability Data System. Its major concern is with summary performance data on all types of electric power generating equipment. It is the primary means of collecting, processing, analyzing, and reporting data on outage, availability, and maintenance of systems and components.

Maintenance data for component outage frequencies and durations compiled from nuclear plant experience are also available from the proprietary database (Reference A). PLG and other private consulting firms, such as Science Applications International Corporation (SAIC), and Westinghouse Corporation, ~~and PLG, Inc.~~ have compiled and maintain proprietary generic ~~component failure rate~~ databases for use in their own PRAs and for other commercial uses. In addition to component data, these contain common cause failure data, initiating events data, generally in a format that is compatible with the reliability and unavailability models defined for a PRA. The Electric Power Research Institute has been regularly funding projects for compilation of common cause events. The original report from this effort, NUREG/CR-4780 (Reference B) documents data and provides detailed methodologies for common cause failure models. This report (Reference C) has been recently updated.

There are many other less widely used sources of generic reliability data, some of which will now be described. The Government-Industry Data Exchange Program (GIDEP) seeks to reduce the costs in time and money of gathering data by providing for an exchange of data which is essential in the research, design, development, production, and operational life-style phases of systems and equipment. There are four major areas for exchange of data: (1) Engineering, (2) Metrology, (3) Reliability-Maintainability, and (4) Failure Experience. A major program sponsored by GIDEP is the Failure Rate Data Program (FARADA) which involves the joint collection, analysis, compilation, and distribution of failure rate data and failure mode data by the Army, Navy, Air Force, and NASA.

The United Kingdom Atomic Energy Authority Data Program (UKAEA) is another agency which is a comprehensive source of reliability data on nuclear power reactors. The data classification and coding format are similar to that used by the FARADA and GIDEP programs. The data come from a long-standing incident reporting system on some 900 components. There is also a reliability data bank called the National Center of Systems Reliability (SYREL). It has information on performance availability and generic reliability data, some of it from industries other than nuclear power plants.

~~The National Electric Reliability Council (NERC) operates the Generating Availability Data System (GADS), formerly called the Edison Electric Institute Equipment Availability Data System. Its major concern is with summary performance data on all types of electric power generating equipment. It is the primary means of collecting, processing, analyzing, and reporting data on outage, availability, and maintenance.~~

The publication, Nonelectric Parts Reliability Data (NPRD-91), provides data from military and space applications in four sections: (1) Generic Level Failure Rate Data, (2) Detailed Part Failure Rate Date, (3) Part Data from Commercial Applications, and (4) Failure Modes and Mechanisms.

The Energy Systems Group of Rockwell International operates the Energy Technology Engineering Center (ETEC), formerly called the Liquid Metal Engineering Center (LMEC). This program was concerned with equipment and parts used in tests of liquid metals and experimental reactors. They did include general reactor components in the program, but it is now discontinued. However, information from it can still be obtained.

C.2.4.2 Plant-Specific Data

~~Perhaps the best source of plant-specific failure data are the plant operating and maintenance logs themselves. However, it is a time consuming task to go through such logs in order to extract the desired data. Another source of plant-specific data, such as the number of demands on a component at times other than periodic tests, is the control room log. It is useful in supplying operating time data.~~

~~Licenses report off-normal events to the NRC as Licensee Event Reports (LER). The report consists of monthly computer listings which describe the facility, the events, the significance of the events, the operating conditions, etc. In 1990, a total of 2,128 LERs were reported, while for 1991, there were 1,858 LERs. The NRC also issues the Gray Book (Ref. C.2.7) a monthly report which is a source of data on all operating commercial nuclear power plants in the U.S. It summarizes inspection status, operating status, reports from licensees, average daily power levels, unit shutdowns, and power reductions. Also at the NRC are immediate notification reports and plant operating data contained in licensee monthly operating reports.~~

~~The Sequence Coding and Search System (SCSS) at the Oak Ridge National Laboratory is designed to facilitate the storage and retrieval of LER data. On average, the SCSS contains 150 pieces of related information for each LER. Because the LeRs are not primarily designed for use in calculating failure rate estimates, it can only be used to infer failure rates. Consequently, this restriction reduces the usefulness of LER data as a plant-specific data source.~~

~~The Nuclear Plant Reliability Data Systems (NPRDS), maintained and managed by the Institute of Nuclear Power Operations (INPO), is another source of plant-specific data. The NPRDS accumulates, stores, and reports failure statistics on systems and components in nuclear power plants. The data is limited to certain safety categories. INPO solicits data from all U>S. organizations which operate nuclear reactors for the purpose of generating electrical power. The data base currently contains approximately 140,000 component failure records and approximately 565,000 engineering records. These records are related to 35 major categories of components in 112 nuclear units. However, because plant participation in this effort is voluntary, a question of completeness and accuracy arises.~~

~~The American National Standards Institute has another subcommittee which publishes the Failure Incident Report Review (FIRR). It reviews problem areas and determines the need for changes in standards.~~

Different plants have different means of keeping logs on component failures and component maintenance events. In general, no one source provides all of the data necessary for estimation of any of the parameters being considered for the PRA. Various sources must be consulted and similar sources may be called by different names in different plants.

Control room logs generally track all systems that are necessary for continued plant operation and all of the safety-related equipment. The amount of information entered, however, is generally very brief. Data for an event may be spread out over days, making this source quite cumbersome to use for failure and maintenance events. The control room logs, however, are possibly the best source of data for system switchovers. Every time the trains of an operating system are switched from operating to standby train, the information is logged in. A survey of a few months of logs can give a very good estimate on the number of actuations of these types of components.

Every plant maintains a system into which component failures are reported. These reports are created when failures or degraded states are noticed during plant walk-throughs or during surveillance tests. These may be called Action Reports, Failure Reports, Discrepancy Reports, or Trouble Reports.

All maintenance work performed in a nuclear plant is cleared by the operations staff and is logged as Maintenance Requests, Clearance Requests, Work Requests, or Tag-out orders. The advantage of using this source is that all of the information for a given event is on one record, unlike the control room logs.

Other sources that may be available at some plants are component history logs which will contain all information about a specific component: failures, maintenance events, operating time, and actuation demands. Data from run meters and cycle counters may be available at some plants, which would simplify considerably the task of estimating failure rates.

Licensees report off-normal events to the NRC as Licensee Event Reports. These reports describe the events in great detail, in addition to providing other relevant data about the plant at the time of the event. These are an excellent source for initiating events data. The LERs also report component failures, but the reporting is limited to safety-related equipment. The requirements for reporting failures are also subject to interpretation by different licensees; in addition the requirements have changed a few times over the last 15 years. All of these parameters must be taken into consideration before using the LERs as a source for component failure data.

The monthly reports at a nuclear plant provide information on the power history. This is useful for identification of the shutdown periods. This information is also provided to the NRC and is published in the Gray Book (Reference D).

The licensees also report failure incidents on a selected number of components to the Institute of Nuclear Power Operations (INPO) which manages the Nuclear Plant Reliability Data Systems (NPRDS). The reporting, in the early years, was voluntary and the database therefore not complete and accurate. Since the middle 1980s, the reporting has been mandatory. The number of components reported above the minimum requirements still varies from plant to plant. The information for the failure generally is quite detailed, and since the information is computerized, the retrieval is quite simple.

References:

A. PLG, Inc., "Database for Probabilistic Risk Assessment of Light Water Nuclear Power Plants," PLG-0500, November 1991.

B. Pickard, Lowe and Garrick, Inc., "Procedures for Treating Common Cause Failures in Safety and Reliability Studies — Procedural Framework and Examples," prepared for U.S. Nuclear Regulatory Commission and Electric Power Research Institute, NUREG/CR-4780, EPRI NP-5613, Vol 1., 1991.

C. PLG, Inc., "A Database of Common-Cause Events for Risk and Reliability Applications," prepared for Electric Power Research Institute, TR-100382, June 1992.

D. "Licensed Operating Reactors, Status Summary Report," NUREG-0020, updated monthly.

Review Comments by B. John Garrick
(Assisted by R. Kenneth Deremer)

Appendix C.4

Accident Progression Analysis

General Comments:

1. This section is generally lacking in terms of specific guidance.
2. This section should contain more examples, including graphics.
3. This section contains numerous typographical and grammatical errors (e.g., incomplete sentences).

Specific Comments:

Page C.67, Line 12:

It should be noted that NRC has also referred to these types of studies as "containment performance analyses" and "backend analyses" in the IPEs.

Page C.67, Line 15:

It should be noted that the analysis of the "transport of radioactive materials" referred to here does not include transport in the environment.

Page C.67, Line 29:

The sentence beginning on this line should begin with "The discussion in this section" draws ....

Opening paragraph, Section C.4.2:

It should be noted that from this point on only the term APET will be used and discussed. The APET which is discussed reflects those used for Level 2 quantification of the plants analyzed in NUREG-1150.

Page C.68, Bullet beginning on Line 30:

In NUREG-1150, the status (e.g., recovery) of ac power was extremely important in the Level 2 analysis. This observation is somewhat contradictory to this bullet.

Page C.69, Line 9:

The authors might want to consider replacing the phrase "beyond core damage" with the phrase "beyond the time of core uncovery" since, in NUREG-1150, some of the APET pathways did not involve core damage.

Page C.69, Line 16:

Don't the probabilities (frequencies) of the endpoints sum to the core damage frequency?

Page C.69, Line 30:

The term "size" is ambiguous.

Page C.69, Line 44:

The term "large scale APET" is relative, but here it seems to refer to those used for NUREG-1150. Does this imply that the APETs used in NUREG-1150 are indeed "examples" of what the NRC deems a large scale APET?

Page C.70, Line 9:

I doubt that the quantification of an APET (i.e., one for Level 2 quantification) with only 10 top events would provide many useful insights. This should be noted (perhaps under "Disadvantages").

Page C.72:

One advantage of large APETs that is not mentioned is the increased ability to track dependencies (e.g., hydrogen release and subsequent disposition).

Page C.72, Line 10:

It is not clear why it is easier to visualize a small APET. The APETs used in NUREG-1150 were difficult to visualize and they were indeed large scale. However, a small scale APET of the same format would also suffer from the same problem. Is the author really referring to a CET at this point?

Section C.4.3.2:

This section should contain some discussion relative to time phasing of the accident progressions.

Page C.73, Bullet starting on Line 11:

It is possible to link CETs directly to the Level 1 model, allowing pathways to be traced back to the initiating event, thus providing the much fuller picture of accident progres[illegible]n.

Section C.4.3.2:

Specific examples of the "unsupplemented" and "supplemented" trees should be provided.

Section C.4.3.2.2:

No general guidance is given as to what uncertainties should be addressed (i.e., What APET events are most likely to require such evaluations? or How does one decide which events require uncertainty analysis? Only a limited number of NUREG-1150 APET events were sampled.)

Page C.77, Lines 39 & 40:

The term "fair" should be replaced, perhaps with "significant."

Page C.78, Bullet starting on Line 1:

Examples of "accident progression via other models" should be identified.

Page C.78:

The guidance given on this page is extremely general. Specific guidance should be provided where possible. The general guidance should also stress the limitations of the accident progression codes. For example, does a particular code provide acceptable predictions of source terms even though in-vessel thermal-hydraulic calculations are not acceptably accurate.

Page C.79:

It is not clear why the paragraph at the top of the page is "boxed," where it is cited, or whether it is the correct order in the text. While this author has first hand knowledge of the type of problem used as an example in this paragraph, it is not a good example for understanding the calculations performed by accident progression codes.

Page C.79:

Some general statements/recommendations regarding the use of MAAP or MELCOR would be useful additions to the guidance listed here. A summary statement regarding the conclusions reached in the comparison of MELCOR and MAAP would also be a useful addition.

Page C.80:

It is not clear as to where the material in the boxes has been cited in the text. It appears between two bullets.

Page C.81:

The "figure" on this page has no figure number and no obvious citation. The material on this page is very confusing and needs additional explanation.

Review Comments by B. John Garrick
(Assisted by R. Kenneth Deremer)

Appendix C.5

Source Term Analysis

General Comments:

1. This section is generally lacking in terms of specific guidance.

2. Much of this section is related to work and terminology associated with NUREG-1150. With respect to source terms, NUREG-1150 left a little to be desired in terms of "scrutability." The NRC should ensure in their guidance that this deficiency in scrutability does not reappear.

3. This section contains a number of typographical (e.g., CORRAL is written as CORREL in several places) and grammatical errors (e.g., incomplete sentences, missing words).

Specific Comments:

Page C.84, Line 21:

The statement implies that the accident progression analysis and the source term analysis are two distinct entities when in fact they are very closely coupled and are performed simultaneously with the integrated codes. (See Page C.85, Line 21)

Page C.84, Line 39:

Reference is made to a "collection of parameters" for the source term. Specific examples of these parameters should be given at this point.

Page C.85, Line 12:

The text notes that "the accident progressions are typically collapsed into groups, referred to as accident progression bins." On Page C.88 (Line 20), it is noted that "the group of accident progressions is referred to as a release category." Some additional explanation may be required to establish the similarity or distinction of the terminology.

Section C.5.3.3:

Based on my understanding, existing source term parameter codes are "hardwired" for various types of plants (e.g., PWR large dry, BWR Mark I, etc.). If indeed this observation is true, the disadvantages list should contain this observation.

Page C.92, Lines 16 & 17:

The sentence contained on these lines should be revised as follows:.....first and third methods in PRAs, <u>respectively</u>.

Tables and Figures near the end of this section were "garbled" in my version of the draft report.

Page C.96, Line 33:

Some specific examples of the "other detailed mechanistic codes, too (sic) many to list here, that address specific phenomena" should be provided.

Page C.97, Line 30:

What PRAs are being referred to here?

Review Comments by B. John Garrick
(Assisted by Keith Woodard)

Appendix C.6

Consequence Analysis

General Comments:

It may be of interest to NRC reviewers to provide a discussion of the history and application of "other versions of CRAC....." used by non-government consequence assessments. In 1976, shortly after the RSS was released, a group of industry and government representatives met to discuss the shortcomings of CRAC. A list of about two dozen problem areas were identified. PLG addressed most of these and developed the CRACIT (Calculation of Reactor Accident Consequences Including Trajectories) to address some of the then-perceived, most important deficiencies (e.g., terrain dispersion, variable direction of plume, multiple release (long-term release), realistic evacuation trajectory, etc.). The CRACIT code has been used in about 10 full scope PRAs. In more recent times, this code has been improved to use particle-in-cell dispersion (for terrain or seabreeze) and highway models to improve the evacuee dose assessments.

We like to think that improvements in the models help to reduce the uncertainties and that the displays generated by the suite of post-processors added tend to increase the level of understanding and usefulness in applying the results to the questions being addressed.

Specific Comments:

C.6.1 Learning Objectives

1. In the first bullet, replace "types" with content and organization.

2. In the second bullet, after "of" ad input information required and the. Also, delete "the" at end of first line.

C.6.2 Concepts

1. Second paragraph, third sentence (and in the third paragraph). There seems to be considerable emphasis on "onsite consequences." It is noted in the text that "onsite consequences" have not historically been a part of consequence assessment. Why is there so much emphasis on it in an NRC Guidance document? Perhaps, citing new regulatory requirements or documents would be helpful. The

DOE has recently been considering "onsite" consequences in its directives. Is there an effort (or requirement) to develop a "robust methodology" for onsite consequences?

2. Same cite as No. 1 above. I would provide a more common example related to the different display products used for siting versus emergency planning.

C.6.3.1.2 Performing A Consequence Analysis

1. In the second paragraph, Line 3, after "(2) the....." add time dependent.

2. Same paragraph, Line 5, after "windspeed....." add wind direction.

3. Third paragraph, Line 10, sentence starting with "CRAC allows....." and the following four sentences. The "leading edge" statement is misleading. Also, "puff" and "tail" discussions are confusing. They should be rewritten, perhaps as follows:

   "CRAC models the release as if it were made in a single "puff." Particulate material is deposited along the straight-line plume track in proportion to the time-dependent wet or dry deposition rates dictated by the weather."

   "This methodology does not properly model the effects from longer term releases and particularly, the effect of wind shifts during such releases. MACCS, on the other hand, allows the time-dependent release to be separated into several releases ("puffs"), thus, accounting for possible wind shifts and other weather changes affecting the isotope distribution in the "puff." The end of a release or "tail" would be included in one of these separate releases." (This whole paragraph should be rewritten.)

C.6.3.1.3 Treatment of Uncertain. /

1. First paragraph, Line 10, after "(e.g.,....." add plume rise,".

2. Same paragraph, Line 13, after "evacuated" add evacuee location with respect to plume and deposited material"

3. Second paragraph, first sentence. I am not sure what this means. The author seems to be indicating that the process of using weather scenarios to develop the CCDF is an expression of weather induced uncertainty. I recall looking at it differently. It seems that we asked the uncertainty question related to weather, more like — given a measured weather condition, what is the chance it could have been different for that measurement.

4. Same paragraph on uncertainty — next to last sentence. Uncertainties in Level 3 parameters have been propagated in PRA results. These studies did not include all Level 3 parameters but did include the more sensitive parameters like dispersion,

delays in evacuation, etc. See, for example, Indian Point, Zion, Seabrook, and others, where a family of risk curves in the CCDF format included propagation of uncertainties in all three levels of the PRA.

C.6.3.2 Onsite Consequence Assessment

1. General Comment: Onsite consequences have always been considered in SAR analyses. They were generally restricted to control room doses; however, dispersion in building wakes was estimated. In several commercial plant licensing cases, building wake dispersion was estimated based on wind tunnel measurements with scale models of the plant.

C.6.4 Codes

1. General Comment: It may be useful to list CRACIT (and maybe CRACEZ) as potentially useful tools in terrain or for complex evacuation scenarios. These codes probably should be referenced given their role in Level 3 analyses. See attached reference (Attachment 1) for NRC consideration.

2. Page C.107, Line 10, remove "guides" and put an s on actions.

C.6.5 Products of Analysis

1. Figure 16 (probably Figure C.6.1). "Exceedance" is not found in the dictionary; however, it probably has the appropriate meaning for the reader.

2. First paragraph, starting with the sentence on Line 6. If this is referring to Figure 16, some rewording is necessary to avoid confusion between frequency wind "probability" and "individual" and "uncertainty." The author is referring to the CCDF as expressing uncertainty in weather. I never looked at it that way. A given weather scenario was chosen by a sampling method and represents the weather at that time (with some uncertainty in each parameter). I would rewrite the sentence to read ".....the CCDF gives the frequency that the number of health effects being evaluated is exceeded. If a distribution (family of curves) is incorporated into the CCDF, this should be represented as "uncertainty" and expressed as probabilities (e.g., 5% probability that the frequency of exceeding 200 Latent Cancer Fatalities is 10^{-7}).

3. Figure 17 (probably C.6.2). The label on the vertical axis should read "Conditional Frequency of Exceeding X."

4. The last paragraph on Page C.110, before Section C.6.6 is confusing. I wonder if the two figures are reversed. "The variation along a curve in Figure C.6.2.....different types of accidents.....". There is no variation in Figure 17. The "variation along the curve....." we refer to as uncertainty or probability (see above).

5. On Page C.109, Line 10-11, correct to read "the CCDF gives the frequency with which a particular consequence will be exceeded given the variability in the weather conditions which may exist at the time of an accident."

Review Comments by B. John Garrick
(Assisted by Donald J. Wakefield)

Appendix C.7

Risk Integration

General Comments:

1. This section is more of a discussion of risk rather than risk integration. The notion of integrating scenario probabilities and consequences should be included even if the results of a Level 3 analysis are not available.

2. The notion of comparing risks from two evaluations, considering the uncertainties in each evaluation, should be presented.

Specific Comments:

Page C.115, Lines 21-27:

This paragraph should not leave the reader thinking that integration applies only when the results of a Level 3 analysis are available.

Page C.115, Line 31:

Reference C.7.15 is the appropriate reference.

Page C.115, Line 33:

This line should read "low-probability/high-consequence scenarios and high-probability/low-consequence scenarios;"

Page C.115, Line 36:

Suggest rephrasing to say.....makes use of the triplet definition of risk, but also defines a single risk measure obtained by summing the product of scenario probabilities and consequences over all scenarios.

Page C.116, Line 1:

The index of the summation (i.e. i = 1→n) should be defined as the list of all scenarios n. The term frequencies should be changed to match the term probabilities on the previous definition of the risk triplet. The point should be made that there are many measures of consequences which can be used in the definition of risk.

Page C.116, Line 21:

This sentence is unclear.

Page C.116, Line 35:

Suggest rephrasing to say....."The distribution represents uncertainty in the model parameters and in the modeling assumptions used in the PRA." This paragraph should make clear that this risk distribution is defined for the risk as a product of probability and consequence.

Page C.118, Line 23:

The term reactor-year should be defined. It usually only is used for the risks from power operation. In this context it means the risks from power operation per generating unit per calendar year. This definition accounts for the fact that plants do not, on average, operate for the entire year. A more general definition is needed to cover the risks from plants while at shutdown.

Page C.117/118, Lines 27 to 5:

These definitions should be fixed; i.e., move reactor year to the end, and give the units of time in the final two bullets.

Page C.117, Line 21:

Suggest that the sentence be rephrased to read....."that the constituents of the risk product be explored to determine the key sequences and the underlying causes for their contribution to the total risk."

Page C.119, last line of third bullet in the example:

.....can be outside of the 5% or 95% bounds when the distribution is very broad.

Page C.119, Lines 6-end:

These paragraphs appear out of place in the summary. The idea of combining the results from surrogate PRAs to the Level 1 of a specific plant should be covered in a separate section.

Page C.120:

Many of the references listed are not called out in the text.

Review Comments by B. John Garrick
(Assisted by Donald J. Wakefield)

Appendix C.8

Application of PRA Guidance

General Comments:

1. The relevance of Section C.8 to the overall purpose of Appendix C is not described in Section C.1. This section lacks a clear purpose.

2. Section C.8 seems to be directed toward the applications of a Level 2 analysis, but the learning objectives are stated much more broadly.

3. The concepts in Section C.8.2 highlight the size and structure of event APETs, but not other features which could characterize the scope of a PRA; e.g., selection of initiating events and scope of human actions considered. Again, there seems to be a focus on accident progression analysis, with little or nothing said about the other areas of a PRA.

4. I would have expected other issues to be covered in this section such as:

   - When a surrogate analysis is sufficiently similar to draw useful conclusions.
   - Which existing PRA models are accurate or accepted by NRC for use as surrogate models?
   - The types of results that PRAs of different scopes can provide.
   - The acceptability or significance of results of independent calculations versus verification of assumptions.
   - When reviewing a completed PRA, the merits of independent calculations versus verification of assumptions.
   - A how to for drawing conclusions from the results of an existing study.

5. This section needs to be written with a clearer purpose.

Specific Comments:

Page C.122, Lines 33-37:

A picture such as that developed for the Goesgen PRA, which highlights the scope of analysis, would be useful as an example. The Goosgen picture (Attachment 2) shows initiating event classes on one axis, ending in sabotage; and sequence stopping point on the other axis, ending in health effects.

Page C.123, Table C.7.1:

This table is misnumbered. It again focuses on the interface between Levels 1 and 2. Typical characteristics of plant damage states in the example check list should consider RCS integrity, location and amount of water in containment, and for PWRs, the availability of steam generator cooling.

Page C.123, Lines 1-5:

This paragraph is unclear.

Page C.124, Lines 19 and 20:

The numbered references listed are not provided.

Page C.124, Lines 29:

This section should clearly state that the existing models are from other plants and that the models are being changed to more closely resemble the design of the plant being studied.

Page C.125, Lines 10-12:

The success of a surrogate model also depends on the objectives of the review. In general, we do not support the use of surrogates.

Page C.125, between Lines 15 and 16:

First line: Change ~~priorities~~ to prioritize. Which decision is left to the NRC staff is unclear. The second paragraph is unclear and confusing. In the third paragraph, the subject "such analyses" referred to is unclear.

Page C.126, Line 6:

The title of this section is overly ambitious.

Page C.126, Line 15:

"Lacking specific guidance performing a PRA." This phrase is confusing.

Page C.126, Line 32:

Should add a sentence that notes that other tasks are required to integrate the Level 1 analysis with Level 2 and 3 analyses.

Page C.127, Lines 5-24:

It is unclear what an integrated PRA is being compared to. I disagree with the disadvantages listed. Suggest that this whole topic be deleted. Instead, the utility of each level of analysis for specific applications should be described.

Page C.127, Line 41:

Initiating events are also grouped according to functional impacts on the mitigating systems.

Page C.127, Line 47:

The "inputs" to what is unclear.

Page C.129, Line 4:

The section references itself. Page C.122 is not relevant.

Page C.129, Lines 18-25:

Plant damage states must be defined to discriminate between all the phenomena which are relevant to the APET response. Alternatively, individual sequences in the Level 1 models could be linked directly to individual sequences in the APET without the use of plant damage states. However, the Level 1 sequences must still then discriminate sufficiently to permit quantification of the APET.

Page C.129, Line 39:

Aren't the frequencies of the accident progression bins also passed to the source term calculations? Has "accident progression bins" been previously defined?

Page C.129, Lines 43-47:

The approach described is just one of several approaches to defining source terms. It is unclear why it is discussed here.

Page C.130, Line 29:

Risk is often defined in other ways. it is unclear why this definition is highlighted here.

Page C.131, Line 8:

The NRC staff should set its own objectives when initiating a review.

Page C.131, Lines 14-15:

It is unclear why the review of the GE study is important enough to highlight. The NRC has reviewed many PRAs.

Page C.131, Line 18:

This first bullet seems to discount the purpose of this whole section.

Review Comments by B. John Garrick
(Assisted by Stan Kaplan)

Appendix C.10

Sensitivity and Uncertainty Analysis

Specific Comments:

Page C.156, Line 20:

....."risk assessments (i.e., tools for making calculations)".....risk assessment is more than this, it is a fundamental thought process.

Page C.156, Line 25:

Disagree with the statement "By their nature, PRA estimates are imprecise." The purpose of PRA is to precisely express uncertainty.....a serious contradiction.

Page C.156, Line 34:

"For example, if decision-makers are comparing two results".....the result is the uncertainty.

Page C.157, Line 17:

Refers to a Section with a question mark (?).

Page C.160, Lines 5-21:

This is a meaningful, well-written section.

Page C.160, Line 36:

This section is wrong. ....."treating uncertain parameters as random variables".....should read fixed, but uncertain, not random.

Page C.160, Line 47:

....."could include".....all data should be used. The issue is how to use it correctly.

Page C.161, Line 11:

....."producing uncertainties".....the uncertainty was already there. The second calculation should reduce uncertainty.

Page C.161, Line 12:

....."are rarely treated in a comprehensive manner".....is a true statement.

Page C.163, Line 1:

....."as random variables".....should be deleted.

Page C.163, Line 2:

Change the last word in the sentence....."allows"..... to requires.

Page C.163, Lines 4 and 5:

The output variable is not a distribution. The distribution expresses our knowledge about the output variable.

Page C.163, Line 19:

Change the words....."analysis methods".....to propagation methods.

Page C.163, Line 28:

What is meant by....."or risk"?

Page C.164, Lines 3-5:

This should be reworded so that it is clearer.

Page C.165, Line 1:

Add <u>are</u> before the word....."included".....

Page C.165, Line 7:

Add <u>usually deemed</u> before the word....."sufficiently".....

Page C.169, Line 18:

This sentence is unclear.

Page C.170, Line 10:

It is important to be clear on what question you are asking.

Page C.170, Line 12:

This sentence should consider the state of knowledge distribution about the fixed, unique value. It is important to be clear on what question you are asking.

Page C.173, Line 9:

This sentence should be rewritten. The key point here is uncertainty about variability. See Reference A for clarification.

Page C.174, Line 5 and 6:

This sentence is unclear.

Page C.174, Line 21:

Add the following sentence: Or sometimes the analyst is not clear what he is doing.

<u>References</u>:

Kaplan, S., "Formalisms for Handling Phenomenological Uncertainties: The Concepts of Probability, Frequency, Variability, and Probability of Frequency," PLG-0855, *Nuclear Technology*, April 1993.

CRACEZ: A CONSEQUENCE MODEL WITH PARTICLE-IN-CELL DISPERSION AND DETAILED SHELTER/EVACUATION DOSE COMPUTATIONS

to be presented at the
Technical Committee Meeting on Guidelines on Probabilistic
Consequence Assessment PSA (Level 3)
Vienna, Austria
09-13 November, 1992

K. Woodard, J. Lewis and T.E. Fenstermacher
PLG, Inc., 1615 M Street, N.W., Washington DC 20036

ABSTRACT

The CRACIT (Calculation of Reactor Accident Consequences Including Trajectories) reactor accident consequences model has been extensively modified to obtain a more accurate assessment of the potential acute accident consequences near nuclear facilities and to evaluate countermeasure strategies in finer detail. The new model, referred to as CRACEZ (Calculation of Reactor Accident Consequences in the Emergency Zone), is capable of modeling dispersion at sites with sea breeze or complex terrain (using a particle tracking dispersion model in a 3-dimensional wind field), and incorporates detailed modeling of the time dependence of the release in 15-minute increments. Doses are computed for evacuee cohort groups with similar initial locations and staged evacuation start times. The position of each cohort as a function of time is determined by a link-node highway model. CRACEZ provides conditional frequencies of occurrence of health effects and specified dose levels in the form of risk curves as well as risk versus distance curves and risk maps. This information is used to support site-specific risk assessment and emergency planning activities. The program has been particularly useful in studies involving definition of the Emergency Planning Zone (EPZ) around nuclear facilities. Output processors facilitate examination of multiple protective action strategies.

1. CONSEQUENCE CALCULATIONS WITH EMPHASIS ON THE EMERGENCY PLANNING ZONE

In several recent applications, PLG has been requested to study complex terrain effects and countermeasure strategies within 10 miles of nuclear facilities. This required major changes to some areas of PLG's CRACIT program. The basic objective of the site consequence analyses are to estimate the receptor exposures in the area surrounding the plant due to a series of postulated atmospheric releases. Since environmental factors and countermeasures can cause the doses to vary substantially for any one release category, emphasis was placed in the areas of terrain dispersion and evacuation detail. The concept of cycling a given release scenario through many different weather conditions was retained even though the computer time increased. (Fortunately, the computer speed has increased more than an order of magnitude in the last 5 years, making use of a particle-in-cell terrain model feasible.) Results are presented as (conditional) frequency distributions of X/Q (dispersion) and health effects for each category. These are combined with the frequency of each particular release category to comprise a statement of risk in the form of risk curves as in previous analyses.

Other factors that can influence consequences include release timing and magnitude, as well as spatial and temporal population distribution (e.g.: numbers, location, protective action effectiveness). The CRACEZ program incorporates models designed to realistically simulate the above phenomena when estimating the doses particularly near the plant.

Sequential meteorological data are used to determine the position and intensity of the effluent plume for each simulated accident. Start times (and, therefore, the meteorological sequence) are selected randomly from site data to represent the spectrum of meteorological conditions. The meteorological data used in the accident simulation correspond to actual data measured for each hour (or quarter-hour, if available) during a typical year at or near the site. As a result, the plume will follow a different trajectory and will affect different locations for each simulation. Thus, doses will be different for each simulation.

2. NEW FEATURES

The program improves on many of the innovative features in CRACIT. The most important features in CRACEZ are (1) dispersion of released material is modeled in a 3-dimensional wind field with a particle-in-cell dispersion model, (2) location of evacuee groups can be varied with time (1-minute update) during the simulation, and (3) the input source term can be varied every 15 minutes. These features enable more realistic modeling of near-site consequences.

3. PARTICLE-IN-CELL DISPERSION MODEL

Many sites and release configurations are not amendable to analytical techniques that use straight-line or single plume geometries. CRACEZ incorporates a particle tracking model that can follow plumes in a 3-dimensional wind field specified for a given weather scenario. Wind fields can be significantly influenced by such factors as complex terrain, wind shear with height, and sea breeze effects. Hourly or 15-minute averages of wind speed, wind direction, and atmospheric stability are used in the wind field definition. A modified potential flow wind field model for complex terrain is built into CRACEZ.

Every 15 minutes, a group of particles is dispersed in the flow field. Each particle is tagged with its share of the released material. The particles are moved at each time step according to a resultant velocity from the mean velocity and a perturbation velocity. The perturbation velocity components are based upon a stochastic process involving the wind turbulence. The particle motion near terrain boundaries is calculated using a technique that ensures local velocities are used along the entire track of particle motion and that time steps can be sufficiently large to minimize calculation time. The particles are counted once every minute in a 3-dimensional polar grid and 15-minute average concentrations are computed.

From this information, dose rates on the polar grid are computed at local ground level for each time period. This process defines the spatial and temporal distributions of dose in the radial grid every 15 minutes from which evacuee exposures can be determined with more accuracy than previous models.

4. COUNTERMEASURES (EVACUATION AND SHELTERING)

Provisions are made in CRACEZ for the evacuation to be simulated dynamically. CRACEZ models the individual routes followed by evacuees in a more accurate manner than previous models. The actual evacuation network is first modeled by a network highway model. These results are processed using a cohort concept. A cohort is a group of people who leave the same area at the same time, and follow the same evacuation route. It is assumed that each cohort stays closely packed. For instance, when a cohort passes through an intersection, the entire cohort not only turns in the same direction, but no other cohort may enter until the current cohort clears the intersection.

In order to determine the dose to each cohort, the period of evacuation is divided into 15-minute time intervals. During each time interval, the evacuation model is given a picture of the cohort structure in that time interval. For each element in the radial grid, this picture shows which cohorts pass through that grid element in that time interval, how many seconds they spend there, and what sheltering factors are to be used. The dose is the same for all individuals within a cohort. CRACEZ is thus able to target for dose calculation only the cohorts which are exposed to radiation during each time interval.

When processing for the time interval is complete, the results are in the form of a group of cohorts, each of which is associated with it the number of the cohort, the original location of the cohort, and the whole body, thyroid and lung doses received by the individuals in that cohort.

Separate shielding factors can be applied to the cohort depending on whether they are at the start location, moving on the grid or out of the evacuation zone. Figure 1 illustrates the process.

5. EVACUATION MODELING

The CRACEZ program interfaces with results generated by a link-node evacuation model. Such models are available to aid emergency planners in the development and analysis of evacuation time estimates. A trip distribution and assignment model is utilized to develop traffic routing plans which make optimum use of the existing roadway system in times of emergency. An example of "nodes" on a typical site map is shown in Figure 2. A traffic model simulates the movement of evacuation traffic along the planned routes. The network "links" connecting the "nodes" are illustrated in Figure 3. The movement of vehicles on evacuation routes is responsive to such factors as highway capacity constraints, the effects of traffic control tactics, and the presence of traffic accidents embedded in the traffic stream. The simulation is dynamic in that the rate of trip generation may change over time.

6. VARIATION OF SOURCE TERM

Up to four releases could be run using the "multi puff" release treatment in CRACIT. This was shown to be a sensitive parameter in PRA studies. CRACEZ incorporates a more accurate representation of the source term. In the particle tracking dispersion model, a new dispersion sequence is started every 15 minutes. Since the group of particles associated with each 15 minute release is treated independently on the dose grid, the input source term can be redefined for each isotope every 15 minutes. This methodology accounts for the plume spreading effects of wind direction shifts, if they occur. Figure 4 illustrates the cohort dose process performed at 15-minute time intervals.

7. OUTPUT REPORTS

Results of the more refined calculations are available in several forms. Health effects due to multiple simulations of a given hypothetical release are displayed as conditional risk curves for each release category. Absolute risk curves can also be plotted using frequency of occurrence of the individual release category. A number of output features were developed specifically for use in Emergency Planning Zone (EPZ) studies as shown in Figure 5. They include comparisons with safety goals, risk versus distance plots, risk versus evacuation distance, and risk maps where the spatial distribution of risk is depicted as color coded contours on site maps (see Figure 6).

8. RECENT STUDIES AND CONCLUSIONS

Experience with the new model has led to a few new insights of importance to consequence analysis. First, use of staged evacuation results in considerable lowering of evacuee doses compared with earlier models. In two studies in complex terrain areas, it was found that consequences were lower when terrain was modeled compared with "flat terrain" cases. Finally, modeling of the plume shine dose in a more realistic way resulted in considerably lower doses for stable cases compared with the Gaussian model with the finite correction factors. This is probably due to a more accurate modeling of the anisotropic shape of the plume, with horizontal spread being almost a factor of four wider than the vertical spread. The finite correction factor commonly used in consequence models, assumes a symmetrical plume.

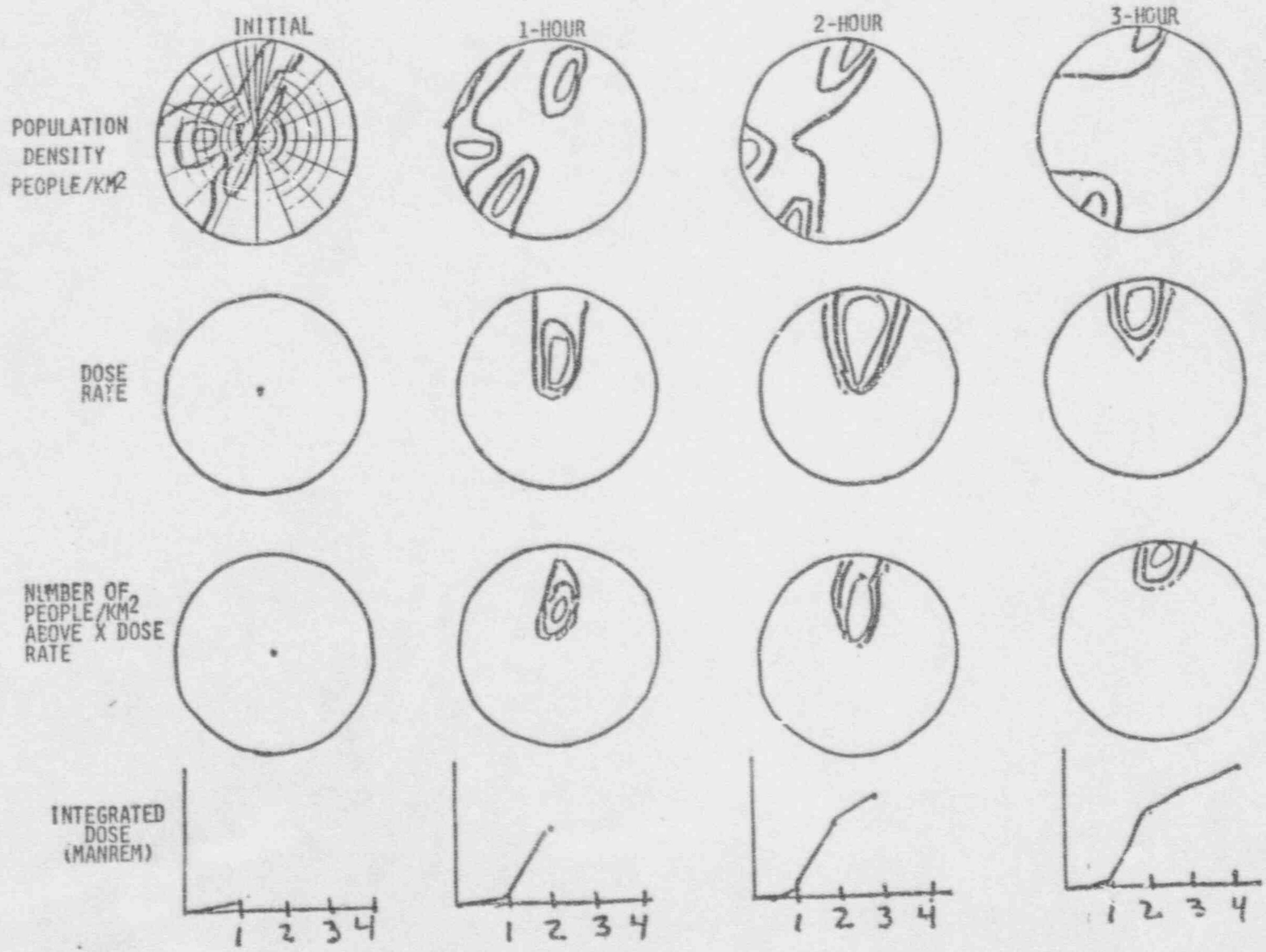

FIGURE 1. ILLUSTRATION OF CRACEZ FUNCTION

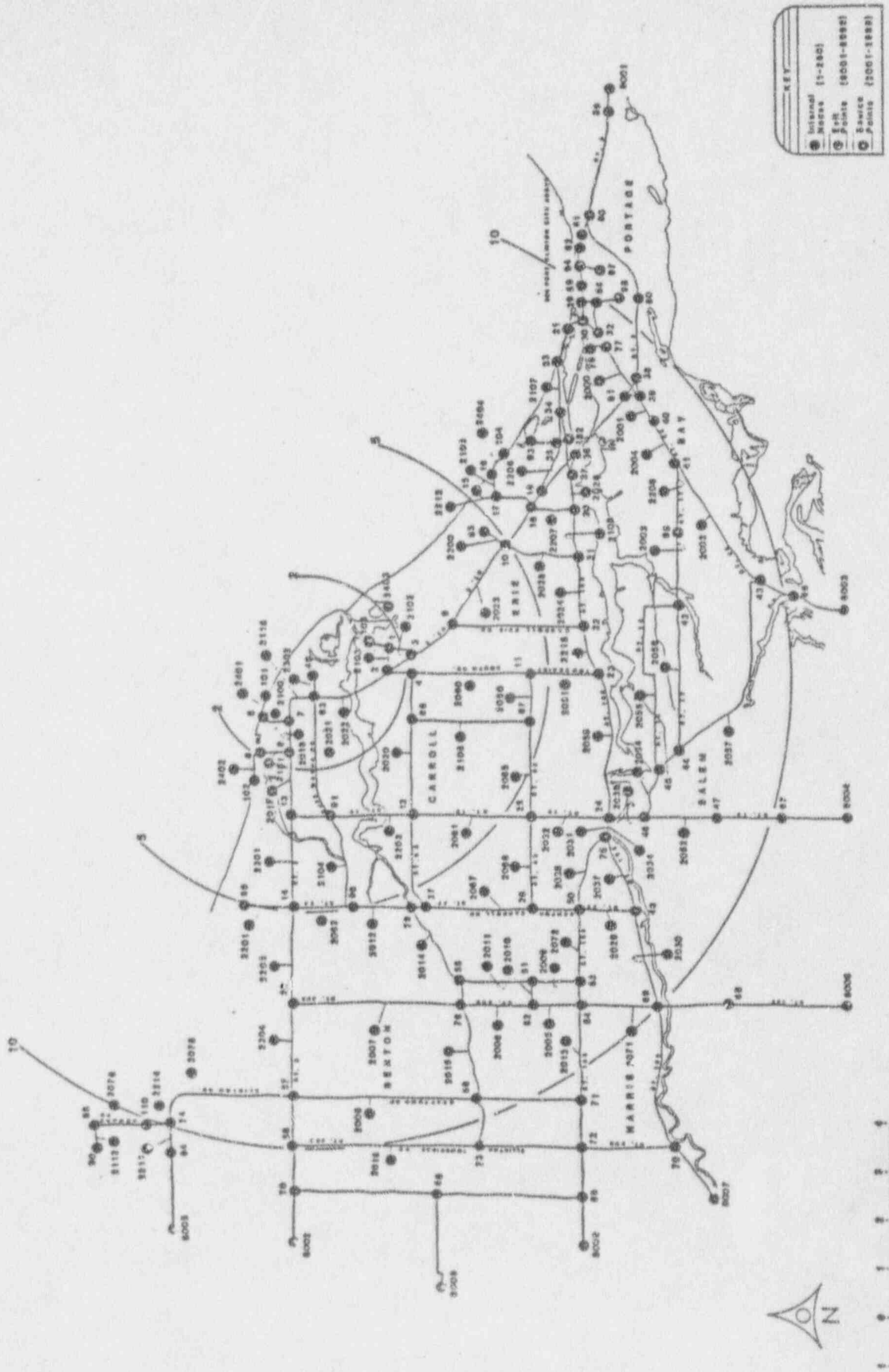

FIGURE 2. EVACUATION NETWORK SCHEMATIC

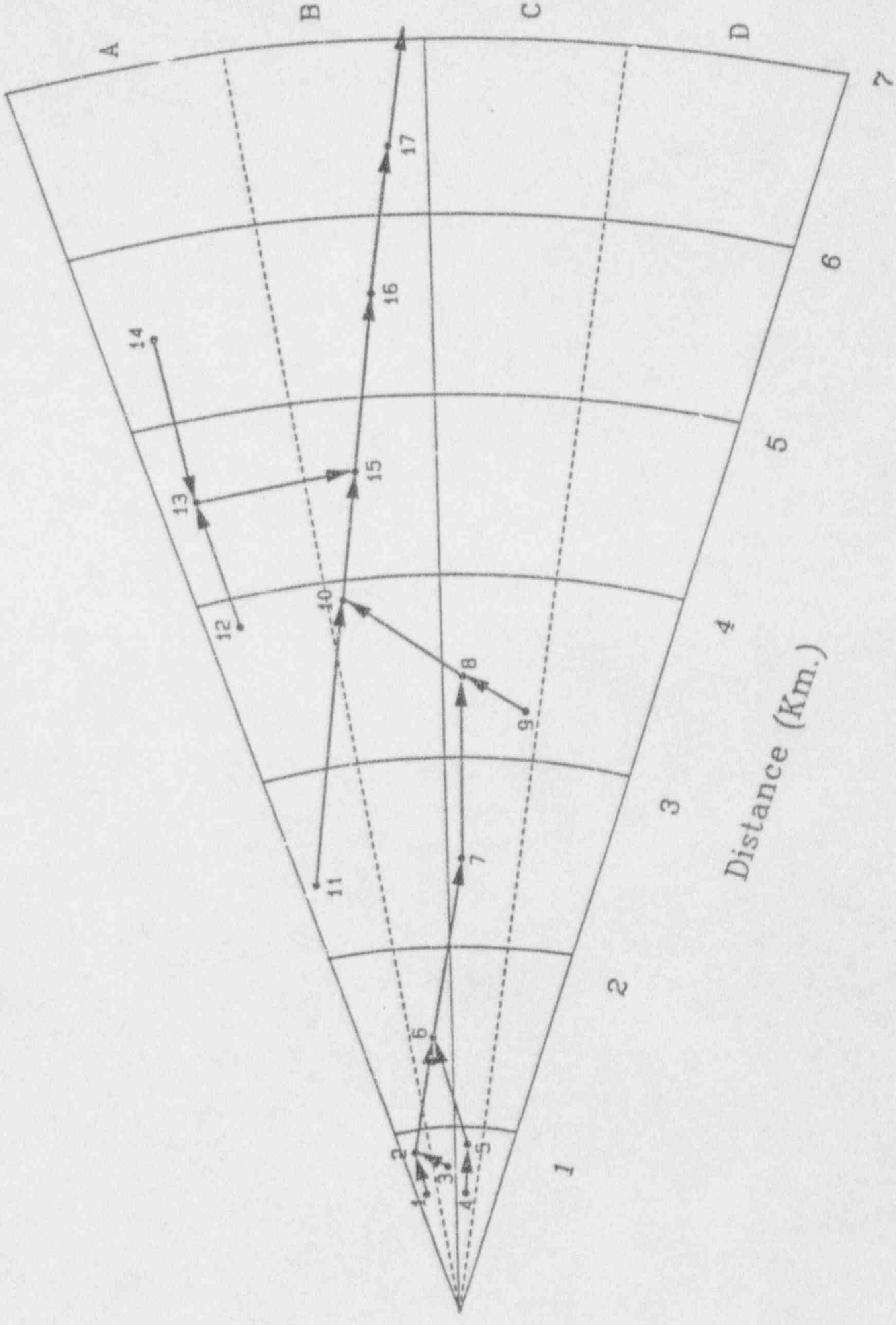

FIGURE 3. SCHEMATIC OF EVACUATION HIGHWAY NETWORK

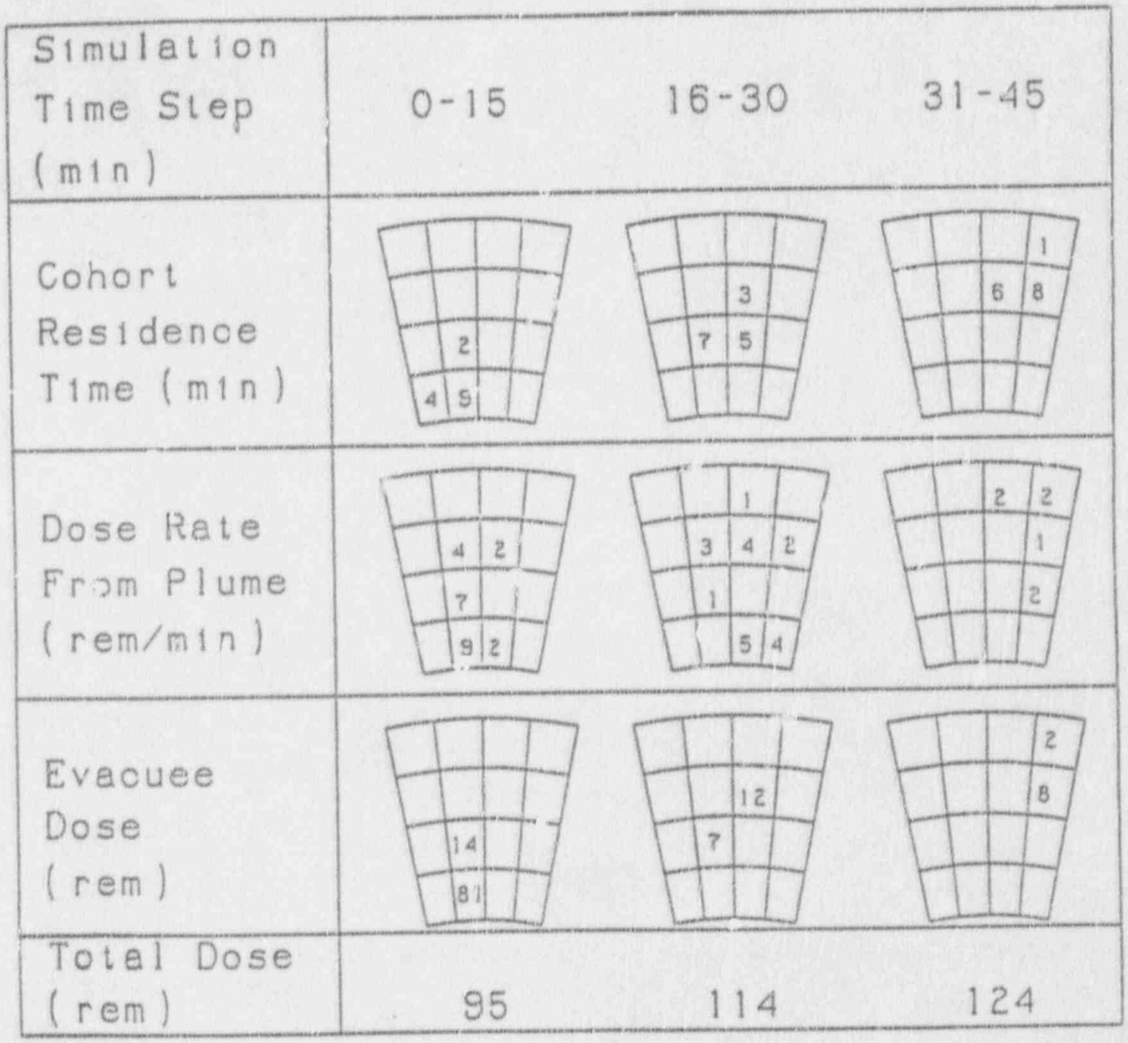

FIGURE 4. COMPUTATION OF THE DOSE FOR A SAMPLE COHORT OVER THREE TIME INTERVALS

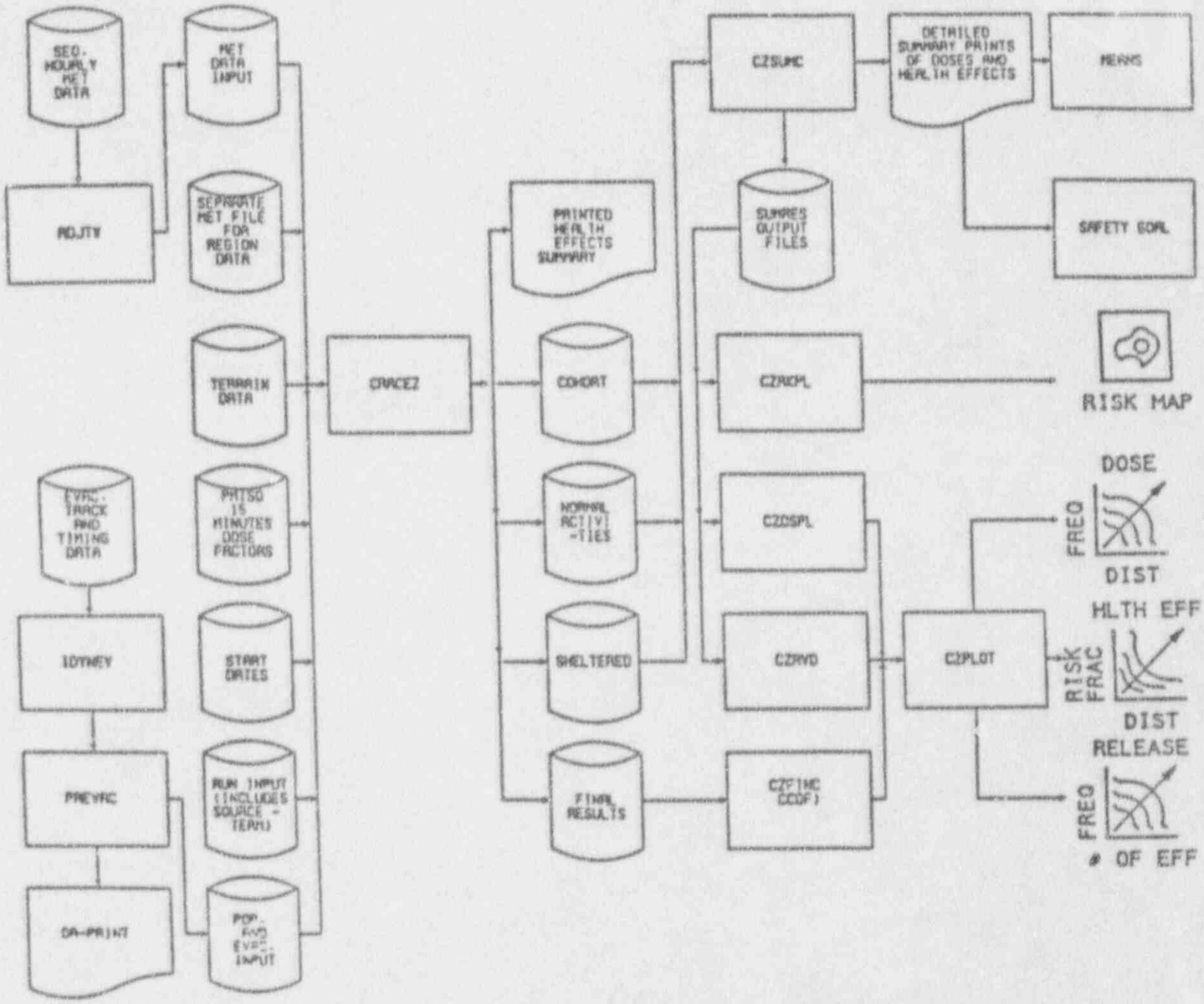

FIGURE 5. CRACEZ SYSTEM COMPONENTS

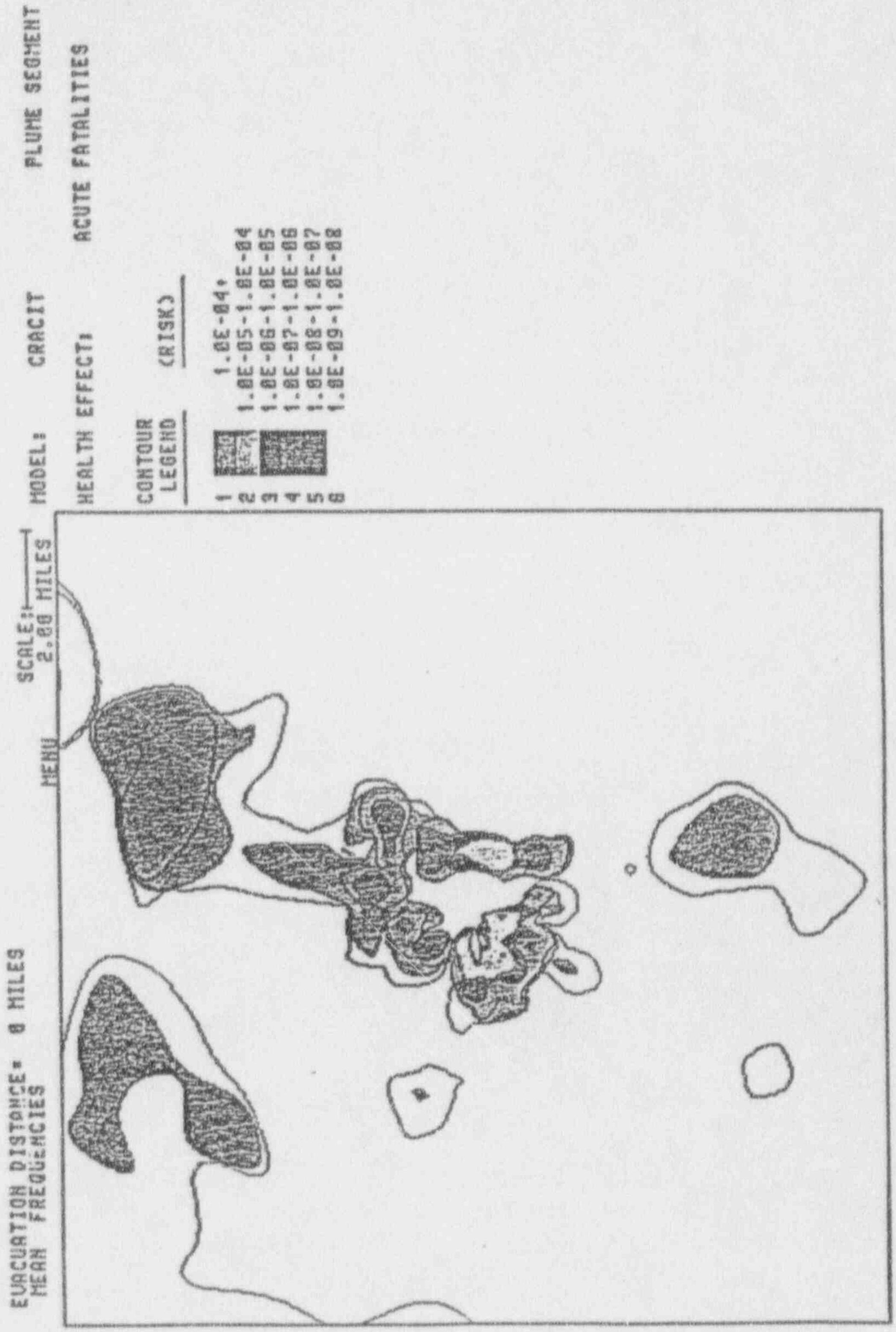

FIGURE 6. EXAMPLE OF RISK MAP

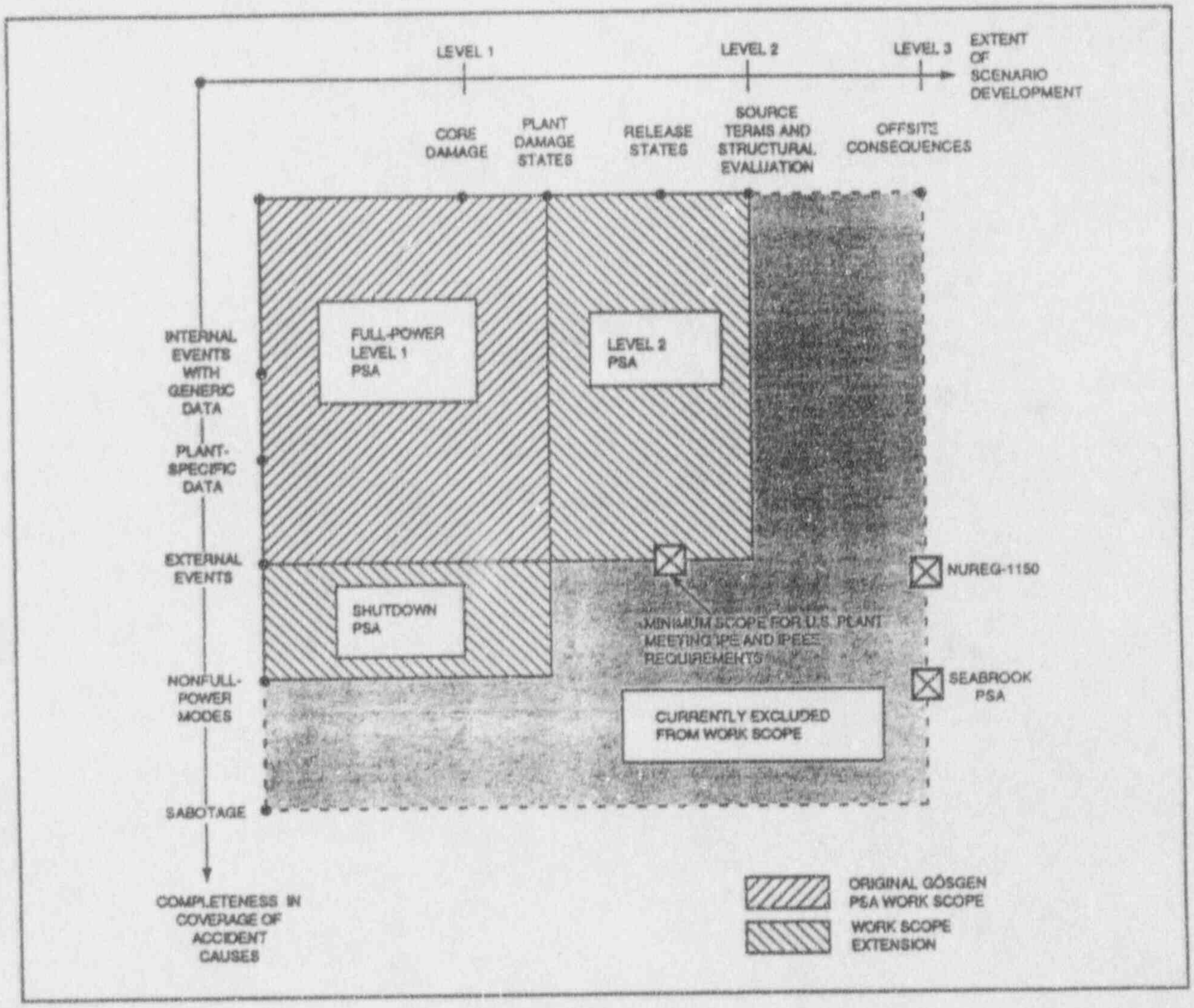

Figure **Characterization of *Gösgen* PSA Work Scope**

APPENDIX C

COMMENTS ON

PRA Working Group Report

Draft February 8 1993

GUIDANCE FOR NRC STAFF USES

OF PROBABILISTIC RISK ANALYSIS

Bernard Harris

6 March 1993

General Comments. The document under review responds appropriately to the July 19, 1991 letter by the Advisory Committee on Reactor Safeguards. It takes cognizance of the inconsistencies in the use of PRA methodology and in the inadequacies of training of the NRC staff in some of the specific skills needed by PRA practitioners. Despite the overall positive reaction to the report, there are many specific comments, which are given below. For the most part, these comments refer to the sections dealing with statistical methodology and uncertainty.

Specific Comments.

p.11, 2.3 It is evident from this section that the staff training is seriously inadequate. While formal university courses in statistics and reliability are highly desirable, "short courses" conducted at NRC facilities should also be considered. These have one advantage over university courses - namely, they can be more "focused".

It may also be desirable to have a certification procedure for individuals that perform PRA analyses. Such certification would provide administrators with guidance on the individuals training and competence in the subject matter areas utilized in PRA's.

p.B-25, B.3.4. In this section, there is the implication that the decision analysis should utilize loss functions, which are not symmetric, that is, they assign greater losses to underestimation (overestimation) than to overestimation (understimation). Such analyses are feasible for convex loss functions and this may be a research area which the NRC should study (or have studied by a contractor or through a research grant).

Appendix B. Presumably, PRA's will be carried out by means of software packages, some of which will be proprietary. The variability in the results utilizing different software packages should be established and the reasons for the divergent solutions should be established.

p. C.5, C.2.2.1. 1.23, {24<t< }. The event A is described by {24 t< }.

1.47, exhaustive means that the union of the events is the sample space, in which case the probability is unity, whether they are mutually exclusive or not. The given equation is correct with the sole condition - "mutually exclusive".

p. C.7, 1.8-12. J. Garrick raised some concern about the use of the word "classical". I concur with this objection.

1.41. Change "experience" to "knowledge".

p. C.8, 1.3. In the usual statistical usage, "the frequency of an event" is the number of times that the event occurs. Presumably, what is intended here is the "intensity" of a Poisson process.

p. C.9, 1.10-14. There are several concerns with the rare event approximation.

The inequality which appears in line 10 is always correct. If the right hand side is to be an "approximation" to the left hand side, some conditions are needed. First, using the "Bonferroni bounds", it is possible to estimate the error in "replacing" the left hand side of the inequality by the right hand side. Second, it can happen that the right hand side is "very far" from the left hand side, even in the case of "rare events". To see this, consider the following situation. If the events are "very similar" then the approximation will be poor. This is illustrated by the following numerical example. Assume that the n events on the left hand side are 106 events, each of which has probability 10-8. If these events are "almost identical", then the probability of the union of these events is "close" to 10-8, but the right hand side will give 10-2.

p. C.9, 1.33-37. The statement given in lines 36,37 is not completely correct. If all but one of the n events has probability zero, then the events are mutually exclusive and they also satisfy the definition of independence, given in line 23. Note that this also applies in the case P(A1)=0 in lines 33, 34.

p. C.10, 1.7-11. The comment for p. C.9, 1.10-14 applies here as well. also typo -replace "event" by "events".

1.33-34. Delete: then P(x) ...... to 1.

1. 40 delete "continuous".

1.46-47. Replace "positive" by "non-negative", delete "continuous". (It is easy to give examples of continuous random variables, whose probability density functions are not continuous).

p. C.11, 1.5-6. add "normal".

p. C.11, 1.17-22. There are also distributions which are neither continuous nor discrete. It may be desirable to merntion that this possibility can occur. The situation in which this might occur in a PRA analysis is in truncation. For example, let X is a continuous random variable. If $X>M$, then X is "reduced" to M. This may occur in some part being gaged and if it fails the gage, it is "remanufactured" to the limit imposed by the gage.

Also, what you are calling probabiltiy density functions here, were previously called probability distributions, see, for example, 1.46, p. C.10. This might possibly be confusing.

p. C.11, 1.37-38. The mean for the pump example is 0.5 only if the probabilities of 0 and 1 are each 0.5. Apparently this assignment was not made previously.

p. C.13, 1.12-22. There are a number of problems with the description of the "median". In lines 12, 13, it is claimed that the median is that value M for which ...... This statement fails for continuous distributions of the following type. Let the probability density function satisfy_

$f(x)=1/2,\ 0<x<1$
$f(x)=1/2,\ 2<x<3$
$f(x)=0$, otherwise.

In this case, M is any value in the interval [1,2].
A similar problem occurs with the pump. There are two ways around this difficulty. A precise definition of the median would lead to non-uniqueness of the median in the two situations above.
This would be:M is a median if P{X M} .5 and P{X M} .5.
However, this may be too complicated, despite the fact that this is the "mathematically correct" definition. A plausible compromise, which is often used in practice, is to use the midpoint of the median interval, when the median is not unique. This would make the median of the pump example .5.

1.16-19. Which "average" is more appropriate depends on the intended usage. I agree that it is desirable to point out that the mean may be substantially influenced by "extreme values" in a "highly skewed" distribution. Examples may be useful here.

1. 19-22. The mode need not be unique. For the extreme case, consider the uniform distribution on [0,1]. Also most likely may not be meaningful for continuous distributions (without introducing suitable limiting arguments).

p. C.14, 1.4-16. The parameters of a distribution constitute an index set for the family of distributions. The assignment is arbitrary, except that each distribution in the family must have a distinct index. In 1.11-16, it is possible to parametrize many families of distributions, such as the beta distributions, so that the parameters will have a simple and useful natural physical interpretation. There are "traditional" parametrizations, which are the ones that you tend to find in text and reference books.

C4

p. C.15, 1.11. Replace "However" by "Despite this".

p. C.16, 1.4. Replace "conjecture" by "statement". (Conjecture implies belief).

p. C.17, 1.7-8. The sentence "However, classical ....model" appears to be misplaced.

1.34. This seems to me to be a vacuous statement. Data is, of necessity, always limited. The imprecision (even under the most favorable circumstances) is a consequence of sampling errors. These comments apply to Bayesian methods as well.

1.36-38. I also find it hard to buy this "disadvantage". In general, probab ilities, confidence coefficients, sample variances, etc., are meant to be interpreted in terms of the population (by means of the sampling distributions) and not with respect to the "single sample"

1. 40-46. I agree in part with this statement. However, classical estimation can often give an upper bound on how rare the rare event is.

(It is only fair to note that the above statements need to be interpreted in the light of my not being a Bayesian).

p. C.18, 1.1-2. It is not correct to say that parameter uncertainty can only be characterized by confidence intervals. The sampling distribution of the estimator is a very good way to study parameter uncertainty.

1.2-6. It is also not correct to say that "It is practically impossible ....". It isa certainly difficult, but computer simulation does provide a mechanism to do so (albeit a tedious method).

1.10. I think "more-or-less" can be deleted.

1.18. Delete "quite".

1.27-28. Section within bounding lines. I have always regarded the lognormal distribution as an inappropriate choice of a prior distribution for this problem. (I know that this was used in WASH-1400).

p. C.20. 1.1-43. An advantage of Bayesian methods which is not in the list is that the mechanics of obtaining the "answer" is easily described and easy to carry out.

p. C.21. 1.11-12. Bayesian inference has long historical precedents, which include Laplace, in particular .

1. 1-17. The sensitivity to the prior is of distinct concern in the use of Bayesian methods. Also, another significant

C5

problem which may occur, is when the data contradicts the prior, resulting in a posterior distribution, which is incompatible with both the prior and the data.

l. 31-44. This discussion is highly debatable. That is, I regard it as far from impartial.

p. C.22. l.41-43. This situation is very risky. See C.21, l.1-17 above.

p. C.23, l.11-16. The principle of invariance is also a common method used in choosing an "uninformative prior".

l. 28. There are also situations worth mentioning in which n is a random variable. (In many instances, this need not affect the inferential procedures, in that they may be conditional on n).

p. C.26, l.6-11. There are some possible alternatives to expert judgment, which are sometimes feasible. These include theoretical mathematical models and physics of failure models. It may even be possible to gain insight into the frequency of magnitudes of earthquakes by historical studies.

p. C.29, l.1. I have a general mistrust of the reliability and accuracy of LER's and would tend to use them as a data source with great caution.

l.24. Replace "simplest" by "simplest non-trivial". The simplest case is an experiment with only one possible outcome.

l. 28-31. Some changes in terminology are desirable here.

p. C.34, l.40. The phrase "small values" needs clarification, or it can be omitted. Clarification can be accomplished by providing information on the extent of error using the approximation as a function of (lambda)t.

p. C.35, l.1-11. Perhaps some additional details would be useful here in describing "instantaneous repair" and "failures between inspections".

p. C.44, l.12-13. There is no logical reason to assume that identical components are statistically independent. They would, however, have identical marginal distributions.

l.13-15. If the data sources are the same, then if the components are identical, the data can be used for all such components. Independence is a property of the population and does not depend on the data.

l.25. Presumably q1 and q2 are parameters, since they are used to define Q. In this case, independence and distribution do not have a meaning in the frequentist analysis.

p. C.46., l.24. The formula for Qs should be identified as an approximation.

p. C.47, l.25-27. and are estimates, not probabilities.

p. C.48, l.12. " scarce" should be replaced by some other word, such as "uncommon".

p. C.51, l.45. "PSF" should be defined.

p. C.53, l.2-10. line 6-- add "of this school" after argument. Also, I do not see why operators can not be treated as components. Clearly, their stochastic behavior may not be as simple as is usually attributed to "mechanical or electronic components". I also do not feel that assessing HER's is fruitless, but I do think that it is vbery difficult and that we are a long way from being able to do this successfully.

p. C.133, l.27. Change "is stated on when the expert use judgment" to "should be kept in mind on when to use expert judgment".

p. C.135, l.29-47. One concern that I have with such procedures deals with the "Group Dynamics". In the past, I have observed a number of instances in which a group of individuals prefers alternative A to B overwhelmingly. Then an individual with a dominant personality says he prefers B and the group shifts its preference to B.

p. C.138, l.18. Change "beliefs" to "knowledge".

l.22. Change "elucidations" to "elicitations".

l.46. Various situations need to be checked for consistency. These include a lack of transitivity, or inconsistent probabilties, such as $P(A)<P(A \text{ and } B)$.

p. C.139, l. 10-13. These methods also involve possible inconsistencies.

l. 21-28. I can also envision that if a specific problem is decomposed in two different ways, one might easily obtain two different answers.

p. C.140, l.33. "consensus" has not been defined.

l. 42-46. In general, I have the following anxiety about "expert opinions". I believe that experts have fairly good judgements regarding central values, such as means, medians and presumably, percentiles in the range 25-75%. However, in PRA analyses, one is often concerned with values in the extreme tails and here I feel that expert judgment is highly unreliable (which

C7

is logical, since the experts have never seen events with pobabilities as low as 10-6.

p. C.141, 1.34. Change "of zero or one" to "near zero or one".

1.35. Typo. Delete "a".

p. C.143, 1.5, 1.15 and many other places. The labeling of Tables, Figures, and References needs revisions.

p. C.145, 1.17. "Refinement" has not been precisely defined.

1.36 "Sharpness....". A revision is necessary.

p. C.146, 1.14. Change "multiple" to "several".

1.18-19. "Better" is relative to ?

p. C.147, 1.16. Replace "no difference between the methods" by "no difference in the results between the two methods".

1.43-47. I believe that geometric averaging is not appropriate.

p. C.158, 1.15-18. I find this paragraph confusing.

p. C.162, 1.47. Typo, change "of" to "or".

p. C.167, 1.17-32. The purpose of the method of moments is to select (estimate) a specific distribution from a given parametric family. The number of moments that need to be generated is usually equal to the number of unknown parameters. For example, if you are trying to "choose" an exponential distribution; this is a one-parameter famuily and if you try to "fit" both the mean and the variance, there will generally be no exponential distribution which meets both conditions. Also, choosing different moments will give different answers.

p. C.169, 1.26. Decisionmaking should be two words.

p. C.170, 1.36-42. The CCDF is the probability .... greater than (not greater than or equal to). Otherwise the equation in line 42 will not hold.

p. C.186, 1.30 typo- replace "is" by "in".

p. C.187, 1.26. delete "be".

1.36. I believe "straightforward" is one word.

p. C.188, 1.33. typo- replace "affects" by "effects".

p. C.189, 1.12. nevertheless.

C8

p. C.191, 1.1-11. It will generally not be possible to simultaneously optimize all of these quantities. In general, you will need to define an "objective function".

p. C.193. 1.32. The word "is" is missing.

1.37. Insert "the" before logarithmic.

APPENDIX D

Comments on Draft Predecisional Report "Guidance for NRC Staff Uses of Probabilistic Risk Analysis"

by

Ralph L. Keeney
March 1993

The comments here are on the main body of the report and Chapters 9, 10, and 11 of Appendix C. In each case, I will first present a few general comments. These will be followed by several detailed comments.

Throughout my comments, I will use the notation p6, 13, for instance, to mean page 6, line 13. Sometimes, I will simply use the notation p6 if I am referring to many lines on a page.

Before proceeding, it should be mentioned that the sections of the report commented on below have many strengths and many aspects are explained very well. It is the nature of constructive criticism not to mention these parts. Consequently, the fact that the comments are all suggestions for potential improvements, does not really reflect the fact that many things are done well.

Comments on the Main Report

General comments

1. Spend some time motivating the use of probabilistic risk assessment. This motivation includes that the problems are complex, addressing them requires many disciplines, there are significant uncertainties about phenomena related to possible consequences, and judgment is necessary in addressing any of the problems. The point is that we use and want to use PRA because it is the best way to address difficult problems. To contrast this with what one might perceive from the report, PRA is not in use simply because there was an accident at Three Mile Island.

2. The document should be much better related to suggestions in the letter from the ACRS to what is done by the working group. Specifically, the document

should mention how recommendations A through D in the ACRS letter are addressed.

3. The entire document is technique-focused, rather than problem-focused. Also, the document refers to how PRA is being used in the NRC. I believe it also should mention how PRA should be used to better address problems of the NRC. Indeed, I think that is one of the concerns that comes through in the ACRS letter.

4. Throughout the document, there are comments referring to improving the ability of NRC staff to <u>use</u> PRA well. That task would take years and years of effort, and I think it is essentially not possible in many cases. However, the message in the ACRS letter is that the NRC has many members that are unsophisticated consumers of PRA. What needs to be taught is how individuals should appraise PRA studies. This requires that they understand what was done, why it is done, and make substantial suggestions based on the answers to these questions.

5. Key item not found in the report is the essential role that structuring any problem has for the analysis that follows. All quantitative analysis is based upon a qualitative foundation that should be built systematically based on logic and good reasoning. The fundamental role of structuring should be mentioned and clearly acknowledged in the report.

<u>Detailed comments</u>

p1, 6: It would be appropriate to define PRA early in this document.

p1, 10: What are risk benefits.

p1, 13: The document refers to balance, but does not mention what (prevent and mitigate) are balanced against.

p1, 31-32: Risk identification is a key initial step in risk analysis. Also, what is referred to as risk characterization is often considered a part of risk communication.

p3, 1: Improvement in PRA methods is not particularly needed. What is needed is improvement by NRC staff of the knowledge and appropriate use of such methods.

p3, 29: Again, the NRC staff need not apply PRA, but rather appraise in a sophisticated manner.

p3, 37: Again, it is not improvements in PRA, but improvements in the ability to use PRA.

p4, 34-36: In order to refer to limitations of present staff practices, one cannot simply review what practices are currently being followed. One must also address where should PRA be used, where it is not now used.

p6, 48: Refer to the individuals by name who are now called et al.

p7, 6: Decision theory is more appropriately referred to as decision analysis.

p7, 22: Please include the affiliation and initial to read Ralph L. Keeney, Professor, Department of Systems Management, University of Southern California.

p9, 6: Addressing anticipated PRA uses requires focusing on problems that are addressed by NRC, rather than simply where PRA is used currently by the NRC.

p11, 8-10: Performance assessment models are risk analyses, rather than similar to risk analysis.

p11, 21-36: A little more detail here would be useful. What does it mean to have less than one year experience and familiarity with PRA techniques?

p11, 43: The use of "best estimate" is ambiguous here as different people use the term differently.

p11, 47: Is it that this sentence refers to formal decision analysis and not formal expert judgment, which one can consider to be used sometimes in formal decision analysis. I assume this is the meaning as someone within NRC certainly has familiarity with NUREG-1150.

p12, 25: The training needs to be on how to understand, interpret, and appraise PRA uses rather than how to use PRA.

p14: This table does not communicate very effectively. It should be possible to look at this table and understand what is meant by it. As one example, the notation x is not clarified.

p15, 23-25: These lines indicate one of the points about lack of sophistication with regard to understanding PRA in the NRC. Judgment is always used in every analysis that is ever done. It therefore makes no sense to write "based on judgment" when judgment is explicitly used. In this case it is completely clear. Perhaps we should state "based on judgment" on every report that is ever produced by the NRC.

p15, 27-29: If conservatism is used, this should also be made very explicit in any documentation.

p15, 31-32: I think it is a very bad idea to use qualitative terms such as high, medium, and low. A tremendous amount of research has indicated that people have very different definitions of such terms. Meaning is lost when numbers are arbitrarily translated into qualitative terms. As mentioned in the ACRS letter, uncertainties about numbers does not mean that they are useless but only that they should be used with caution and sophistication.

p16, 21: A fundamental point for general guidance is that problems should be explicitly structured for PRA use.

p16, 32-34: It should be recommended that the decision criteria always be made explicit.

p16, 35: Somewhere around this blank, it should be mentioned that thorough documentation is important for PRA use.

p17, 4-16: Three important topics left off of this list are structuring problems for PRA analysis, the quantification of judgments, and documentation.

p18, 10-13: As mentioned above, performance assessment is a risk analysis; it is not something similar to risk analysis.

p19, 38 to p20, 3: These four recommendations never refer to what the purpose of changes in training should be. It is oriented toward what can be done, not what one hopes to achieve by doing it. That purpose should be that NRC staff members gain the sophistication to structure, appraise, and appropriately use the results of PRA analysis.

p21, 33-45: Same comment as on page 17 above.

Comments on Chapter 9, Appendix C Titled "Expert Judgment"

General comments

1. The context of this chapter could be said much better. Take more time than on lines 20 through 25 on page C.133. The context should indicate why expert judgment (in the explicit form) is used, that the alternative is using expert judgment implicitly, and that judgment is used for many types of questions (such as scenarios, values, and estimates as stated). It should

then refer to the fact this chapter mainly deals with estimates. Then the chapter should narrow to the specific concern of assigning probabilities for estimation.

2. There could be more elaboration on the motivation for what is hoped for the reader of this section than given on the top of page C.133. It should be more detailed and focus on providing individuals greater understanding of how to appraise whether or not expert judgment should be used explicitly and how well any explicit use is carried out.

3. Somewhere in the chapter, there should be a discussion about the advantages and disadvantages of using an explicit procedure to specify expert judgments. The advantages and disadvantages should address the economic costs, time, and effort required. It should stress that when explicit expert judgment is not used, it is sometimes the case that projects will have to go back and repeat the elicitation of judgments more carefully than previously done. The cost in terms of time and dollars of repeating can be very substantial, even relative to the cost of doing explicit elicitation.

4. The chapter should always make it clear that judgment is always used, the only issue is whether it should be used explicitly. This is only made at the end of the chapter, and then very briefly.

Specific comments

pC.134, 20: Another reason for making judgments explicit is to provide a basis for much better appraisal of them.

pC.134, 23-25: This is one of the places where it should be clear that judgment is always used, that the question is whether to use it explicitly or not.

pC.135, 16: This should be about motivational biases, not simply those due to economic stakes. Other motivational biases include a desire for particular alternatives to essentially be chosen or a desire to remain consistent with previous estimates made with less information in the task.

pC.135, 29-35: It should be made clear that isolating experts is not the same thing as not sharing "publicly available" information.

pC.135, 37-42: Multiple experts are not redundant. The reason for using them is that one expects additional information may be gathered because the different experts have different knowledge.

pC.135, 45-47: The decomposition should not necessarily be on independent issues, but rather along disciplinary lines.

pC.136, 16-17: This should say "with all assumptions explicitly stated."

pC.136, 26-37: Define what is meant by observable. By context, it seems like it means measured directly. Is the standard deviation really observable?

pC.136, 41-45: There are two issues first, pressure and location, followed by only one issue, pressure.

pC.137, 5-6: The term subjective probability is a poor one and not used earlier. Why not delete it here.

pC.137, 10-16 and C.137, 1-5: This paragraph seems out of context referring to expert training in one previous project rather than to technical issue selection and definition.

pC.138, 12: Expressing their "knowledge" rather than "beliefs."

pC.138, 17-18: The fundamental objective of elicitation training is to motivate the expert and enhance the likelihood that they can express their "knowledge" well. It is the fundamental objective of the elicitation to help them express that knowledge.

pC.138, 22-23: Elicitations, not elucidations (typos).

pC.138, 36-43: Ranking of the likelihood of outcomes is a very good thing to do in probability assessment, although it is referred to as an indirect technique here which receives a poor evaluation here.

pC.139, 15-17: Make it clear that the decomposition should be on the basis of knowledge (i.e., along interdisciplinary lines).

pC.139, 38-39: The expert should always be presented with a summary of findings to review, discuss, and revise as appropriate.

pC.140, 1-9: The concept of over-confidence is not clear from this paragraph.

pC.140, 11-17: The notions of anchoring and availability are not clear here. More words are needed. Anchoring, for instance, need not be on a single computer model or experiment, it can be on a single event or a few events.

pC.140, 33: The word consensus does not have a common definition. Please define it if you choose to use it.

pC.140, 42-46: Clearly the extremes are an important part of risk analysis, and therefore an important part of explicit expert judgments. More than a couple of sentences is needed to discuss this complex problem in a manner to provide something useful to readers of the document.

pC.141, 3-4: Adequate documentation is always required so this sentence seems superfluous.

pC.141, 29-36: A little more detail would facilitate understanding why a single event does not verify the goodness of a probabilistic estimate. An example may help here.

pC.143, 7 & 15: Different references to the same table are used on these lines.

pC.144: The table should clarify its source.

pC.145, 36: A sentence begins with one word but does not end.

pC.145, 38-46 and pC.146, 1-9: Scoring rules are a very advanced topic and of very little practical use. I would suggest deleting them.

pC.146, 14-19: As mentioned above, multiple experts are not redundant. Also, what is meant by a better prospective or better quality distributions?

pC.147, 15-16:	What is meant by "no difference between the methods." Certainly the methods are different.
pC.147, 18-47 and pC. 148, 1-5:	There seems to be more technical detail here than appropriate for the readers.
pC.149, 37-39:	I think one could say there is a consensus that more than one aggregation of the judgments of various experts should be utilized. One can also say there is a consensus that the information embodied in the individual assessments should not be lost (i.e., analysis with each set of individual judgments could be insightful).

Comments on Chapter 10, Appendix C Titled "Sensitivity and Uncertainty Analysis

General comments

1. A simple example at the beginning of this chapter could be very useful. With such an example, both uncertainty analysis and sensitivity analysis could be illustrated. Perhaps the model should have two input variables that combine in the model to provide an output variable.

2. The use of imprecision in a place where uncertainty would be better is often misleading. Imprecision about estimates is not the same thing as imprecision about knowledge. For example, on page C.157, lines 21 and 22, consider uncertainty about the flip of a fair coin. An expert would say the probability of heads is 0.5. This is very precise from my perspective.

3. The organization of all the "types" of uncertainties is difficult to follow. Figure C.10 does not help much. Perhaps a hierarchical structure of the types of uncertainties referred to would be helpful.

4. Many assertions are made regarding the display of information about uncertainties, but there is no basis for the assertion provided. Examples are given below.

Specific comments

pC.156, 45-46: Sensitivity analysis need not address only the sensitivity to the overall uncertainty, but to components of the uncertainty (as referred to later in this chapter).

pC.158, 11: Referring to a "correct" model seems inappropriate. Different models are for different purposes and none is precisely the same as the "real world." I would suggest leaving out the conundrum of the "correct" model from this chapter.

pC.160, 11: The notion of requiring two or three pumps is not clear. Required for what?

pC.161, 17-18: Since the model is built using judgments of the analysts (always), why refer to these uncertainties as subjective, when in fact all uncertainties are subjective.

pC.160, 38-39: The Bayesian approach need not assume a particular distribution. As mentioned later in the chapter, simulation can be used with a distribution of any shape.

pC.164, 5: I do not understand why the frequency estimates are equally probable. Suppose you had discrete sampling and each combination of input variables led to a different output value. Then, some of the sampling points (combinations of input variables) could be much less likely than others, and yet they had a chance to come up in 20 samples. It seems to me the text on this page should be much simpler and state that with more samples, we would get a better representation of the output distribution given our collective judgment on input.

pC.165, 9-29: It is not clear what the advantages and disadvantages are measured relative to. Are costs of simulation really high relative to other approaches (except neglect) when the problems are large?

pC.167, 6-7 and 12-13: Almost the same sentence is repeated.

pC.169, 5-6: Why should such displays be avoided?

pC.169, 18: What does the "it" refer to? The sentence is ambiguous to me.

pC.170, 31-32: This sentence contributes little. What is the place CCDFs in risk assessment and why are they often confusing?

pC.172: Label the x-axis in the figure and give an example with the mean and another example with the median displayed as referred to on lines 1-3.

pC.172, 5-6: Why are box plots [illegible] effective for communicating?

pC.174: Label the x-axis in the figure as hours (I presume).

pC.174, 10-12: Indicate how you "process" the family of curves.

pC.174, 15: Why should the meaning depend on the purpose of the analysis as opposed to simply the method of the analysis, which of course may depend on the purpose. The way it is now read is that you can interpret things the way you want depending on whether you are for or against something, for instance.

pC.175, 15: Eliminate arbitrary and simply say the groupings depend on the discretion of the analyst who wishes to understand certain relationships.

pC.177, 16-18: Such failures, errors, and events should not necessarily be the focus of efforts to improve reliability and reduce risk. This is into the realm of decisionmaking which depends on the cost and likely changes that alternatives will have on the likelihood of those failures, errors, and events.

pC.178-pC.180: The section on other sensitivity methods does not clearly tie those methods to the topic of the chapter.

Comments on Chapter 11, Appendix C Titled "Decision Analysis"

General comments

1. I think this chapter would be much better if it began focused on the types of problems that decision analysis is best suited to address. It should motivate spending time on problems that are complex, too complex to carefully think through all the implications in one's mind without the aid of some method. Typically this problems involve multiple objectives, significant uncertainties, intangible as well as tangible consequences, several interested stakeholders, sequential decisions, and opportunities to gather information. The current version of the chapter is much more technique focused. To illustrate the spirit that I am suggesting, I have enclosed a reprint of an earlier article titled, "Decision Analysis: An Overview."

2. The first ten pages of the chapter jump around and many comments are repeated, or close to repeated, on successive pages. For example, preferences are measured with utility functions (pC.187, 31 and pC.188, 31); utilities incorporate risks, (pC.187, 32 and pC.188, 33 and pC.190, 44); outcomes have probabilities, (pC.188, 23-27 and pC.190, 23-27 and pC.192, 3).

3. There is too much detail on certain fairly sophisticated techniques. Most of these could be deleted with no loss of information, which would facilitate understanding the rest of the chapter. The material that could be dropped concerns risk premiums on pC.193, influence diagrams beginning on pC.199, conjugate distributions and sampling on pC.203, and stochastic dominance on pC.203.

Specific comments

pC.186, 19: Sensitivity of the decision to inputs (not the decision model).

pC.186, 38-40: I think the goal of decision analysis is to help make informed decisions. Part of this is identifying the best of the alternatives available, but there are many other parts. These include getting rid of the particularly bad alternatives and providing the knowledge about why certain alternatives are good and why others are bad.

pC.186, 41: Here it should stress that decisions that should be made refer to should be made in order to be consistent with one's information and values.

pC.187, 16-17: I think the key thing about decision analysis is that it addresses the complexity of complex decision problems in a logical manner. It is certainly nice that decision analysis also has a rigorous axiomatic foundation.

pC.187, 35-36: State that the axioms of utility theory should be verified for a potential user of decision analysis. At least the axioms should be implicitly verified by discussion.

pC.188, 5-6: 'Mutually exclusive actions' does not have a clear meaning. Certainly one can choose sets of actions.

pC.188, 12: Values are not the same thing as consequences. Here, "Values or" should be eliminated. Also eliminate values in lines 15 and 16 and clarify the use of values in line 18.

pC.188, 45-46: The decision tree in figure 1 was not from the site decision for nuclear waste, but rather from an analysis of the portfolio of sites to characterize.

pC.189: The figure should have a title with the reference from which it came stated.

pC.191, 10: Man-rems should be changed to person-rems.

pC.191, 13-21: In [illegible]e, more than four attributes can be logically assessed for a [illegible]ity function. The nuclear waste siting study had 14 attributes in preclosure. In these cases, simplifying assumptions are utilized, but the appropriateness of these assumptions is first verified.

pC.192, 14-15: Much data is utilized in the decision analysis without Bayes. It is that the prior judgments are used or that judgments are only assessed after information is gathered.

pC.192, 23-33: Most of this repeats earlier material in the section.

pC.192, 47: The term "prosp[illegible]." is not defined. Also, u(x) is not a function, it is the utility of the consequence x.

pC.194, 4-5: I disagree that utility functions cannot be aggregated across individuals. However, to do this requires value judgments as discussed in Chapter 10 of Keeney and Raiffa's <u>Decision With Multiple Objectives</u>.

pC.193, 9: A prime sign is mistyped as an apostrophe.

pC.193, 11: It should state the expected value of X, when X is a non-degenerate lottery.

pC.193, 20: The $40,000 should likely be $400,000.

pC.193, 31-47: Most of this should be deleted. On line 44 there is a C and c inappropriately used to mean the same thing.

pC.194, 6-8: Certain comparisons of utilities can be made. For instance, one might be able to claim that individual 1 would be willing to spend much more money in order to reduce a given environmental impact than would individual 2. This certainly is a comparison.

pC.194, 11: A new word numeraire is introduced and not defined.

pC.194, 10-19: Much of this repeats earlier material.

pC.195, 14-15: These should be X's.

pC.196: There needs to be some work on this table to line up several items. Also, the reference for the table should be indicated.

pC.197, 33-39: This essentially repeats earlier material.

pC.197, 48-54: I would probably delete the material on the multiplicative utility function and just cover the additive utility function on the next page.

pC.198, 13-18: If the multiplicative utility function is maintained, then it would be worthwhile to indicate that the additive utility function is essentially a special case of the multiplicative utility function.

pC.199, 34-47 and pC.200: I would delete the material here on influence diagrams and the two figures. I don't think these communicate any new ideas to the audience, who will not be expected to do decision analysis.

pC.201, 19-21: Certainty-equivalence also simplifies an analysis in the way referred to here because it also provides natural units.

pC.202, 27: The units of the utility function must be adapted as we have man-rems minus costs inside the parenthesis. Also, man-rems should be changed to person-rems.

pC.203, 4: The utility function u previously had one argument and now it has two arguments. Something should be changed to eliminate the inconsistency.

pC.203, 11-15: This detail could be eliminated.

pC.204, 16: Weights and scaling factor are the same thing. You also may wish to indicate how these relate to value tradeoffs which you refer to in this chapter.

pC.206, 16-18: I would not use this reference as it is not generally available. Many of the specific papers probably have appeared in the refereed literature. If not, perhaps other examples from the same people that have appeared in the refereed literature would be better.

pC.207, 3-5: As stated earlier, the decision tree was not used in the Department of Energy study. It was done in an independent study to look at the portfolio of sites to be characterized for a repository.

pC.207, 6-7: I don't think one can say that the recommendations of the DOE study were not adopted. The DOE study ranked five sites but did not provide guidance for which three of the ~~three (3)~~ five should be chosen for characterization. It does not automatically follow that the best three sites for characterization are the three sites individually ranked in the order 1, 2, and 3.

pC.207, 32-33: Again, from my perspective, the fundamental strength of decision analysis is that it logically addresses the complexity of complex problems. It is secondary, although important, that it has an explicitly stated axiomatic foundation.

pC.208, 6-8: I don't see how it is a limitation of decision analysis that it requires the decisionmakers to participate.

pC.208, 10-12: Decision analysis can certainly be logically used for "group decisions" which includes organizational decisions such as the repository decision discussed throughout this chapter.

pC.208, 25-26: Utility functions can also be organizational, and they can be examined as to whether they meet certain standards, although one may not wish to call these "objective" standards. For instance, one can determine whether a utility function incorporates the basic objectives and values stated by an organization.

pC.214: I think it would be appropriate to refer to the source from where these axioms are taken.

pC.214, 18: The second p and q are switched.

pC.216, 3: I presume the C should be $\succeq$ B, otherwise C has different uses on lines 2 and 3.

Referenced in review of Appendix C, Chapter 11.

Feature Article

Decision Analysis: An Overview

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(Received February 1981; accepted June 1982)

This article, written for the nondecision analyst, describes what decision analysis is, what it can and cannot do, why one should care to do this, and how one does it. To accomplish these purposes, it is necessary first to describe the decision environment. The article also presents an overview of decision analysis and provides additional sources for its foundations, procedures, history, and applications.

1. THE DECISION ENVIRONMENT

THE ENVIRONMENT in which decisions must be made is more complex than ever before. Governmental regulations, such as the National Environmental Policy Act and the Occupational Safety and Health Act, require corporations and governmental agencies to consider and justify the impact of their actions. Informed consumers, employees, and shareholders demand greater public consciousness, responsibility, and accountability from corporate and governmental decision makers. For example, executives evaluating potential mergers or acquisitions must consider antitrust suits and other legal matters, social impact, and political issues in addition to financial aspects. In appraising potential public programs or the elimination of existing programs, a governmental agency should consider not only the multifaceted costs and benefits of its options but also the diversity of the population and its sometimes conflicting viewpoints and political concerns.

Such examples suggest several factors contributing to the complexity of decision problems. Because the purpose of analysis is to illuminate complexity and provide insight, it is worthwhile to summarize these intertwined features.

1. *Multiple objectives.* It is desirable to achieve several objectives at once. In evaluating routes for proposed pipelines, one wishes simultaneously to minimize environmental impact, minimize health and safety hazards, maximize economic benefits, maximize positive social impact, and please all groups of interested citizens. Because

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all this cannot be done with a single alternative, it is important to appraise the degree to which each objective is achieved by the competing alternatives.

2. *Difficulty of identifying good alternatives.* Because many factors affect the desirability of an alternative, the generation of good alternatives for careful analysis involves substantial creativity. In some problems, a good deal of soul-searching is required to identify even a single alternative which seems possible, let alone reasonable, for achieving the problem objectives.
3. *Intangibles.* How should one assess goodwill of a client, morale of a work force, distress at increasing bureaucracy and governmental regulations, or the aesthetic disruption of a telecommunications tower? Although it is difficult to measure such intangibles, they are often critical factors in a decision.
4. *Long-time horizons.* The consequences of many decisions are not all felt immediately, but often cover (by intention or otherwise) a long time period. For example, the projected lifetime for most major facilities is 25–100 years and research and development projects routinely require 5–20 years. Future implications of alternatives now being considered should be accounted for in the decision-making process.
5. *Many impacted groups.* Major decisions, such as constructing canals for crop irrigation or legislation regarding abortions, often affect groups of people whose attitudes and values differ greatly. Because of these differences, concern for equity contributes to the complexity of a problem.
6. *Risk and uncertainty.* With essentially all problems, it is not possible to predict precisely the consequences of each alternative. Each involves risks and uncertainties—an advertising campaign may fail, a large reservoir may break, a government reorganization may result in an unwieldy bureaucracy, or a new product could turn out to be an Edsel. The major reasons for the existence and persistence of these uncertainties include: (1) little or no data can be gathered for some events, (2) some data are very expensive or time-consuming to obtain, (3) natural phenomena such as earthquakes and droughts affect impact, (4) population shifts affect future impact, (5) priorities, and hence perceived effects, change over time, and (6) actions of other influential parties, such as government or competitors, are uncertain.
7. *Risks to life and limb.* A general class of critical uncertainties concerns the risks to life and limb. Numerous personal and organizational decisions affect the likelihood that accidents or "exposure" result in fatalities or morbidity. Examples include decisions about

highway maintenance, foods and drugs, toxic or hazardous materials, birth control, leniency toward criminals, and whether to walk or drive somewhere. It is not an easy task to include such dire consequences in an analysis, but it is certainly a part of many decision problems.

8. *Interdisciplinary substance.* The president of a multinational firm cannot be professionally qualified in all aspects of international law, tax matters, accounting, marketing, production, and so on. Qualified professionals should supply the relevant inputs on these key factors in a major decision.
9. *Several decision makers.* One player rarely holds all the cards with respect to a major decision. Several players, who may or may not be on the same team, control crucial aspects in the overall decision-making process. To begin production and marketing operations in a new geographical area, corporate management may require approval from stockholders, several regulatory agencies, community zoning boards, and perhaps even the courts. The potential actions of other players must be considered when a corporation evaluates its strategic policy.
10. *Value tradeoffs.* Important decisions involve critical value tradeoffs to indicate the relative desirability between environmental impact and economic costs today, immediate social costs versus future social benefits, negative impact to a small group versus smaller positive impact to a larger group, and sometimes the value of a human life versus the benefits generated by a hazardous technology.
11. *Risk attitude.* A firm operating with the status quo strategy may forecast small and declining profits in the next few years. Changing to an innovative strategy may have a chance of resulting in substantially higher profits, but have a risk of losses or even bankruptcy. Even if the likelihoods of the various consequences are known, crucial value judgments about an attitude toward risk are essential to appraise the appropriateness of accepting risks necessarily accompanying each alternative.
12. *Sequential nature of decisions.* Rarely is one decision completely uncoupled from other decisions. Choices today affect both the alternatives available in the future and the desirability of those alternatives. Indeed, many of our present choices are important because of the options they open or close or the information they provide rather than because of their direct consequences.

Collectively, these features describe many complex decision problems. Although the features causing the complexity in specific problems may differ, the bottom line is that many of today's decision problems have the following characteristics.

High stakes. The difference in perceived desirability between alternatives is enormous. It may involve millions of dollars or severe environmental damage, for instance.

Complicated structure. Numerous features (discussed above) make it extremely difficult to appraise alternatives informally in a responsible manner.

No overall experts. Because of the breadth of concerns involved in most important decision problems, there are no overall experts. Different individuals, however, have expertise in disciplines such as economics, engineering, and other professions which should be incorporated into the decision process.

Need to justify decisions. Decisions may need to be justified to regulatory authorities, shareholders, bosses, the public, or oneself.

Complexity cannot be avoided in making decisions. It is part of the problem, not only part of the solution process. There are, however, options concerning the degree of formality used to address the complexity. Near one extreme, this may be done intuitively in an informal manner. Near the other extreme, formal models can be used to capture as much of the complexity as possible. In any case, the process of obtaining and combining the available information is a difficult task that requires balancing all the pros and cons as well as recognizing the uncertainties for each alternative.

2. WHAT IS DECISION ANALYSIS?

Decision analysis can be defined on different levels. Intuitively, I think of decision analysis as "a formalization of common sense for decision problems which are too complex for informal use of common sense." A more technical definition of decision analysis is "a philosophy, articulated by a set of logical axioms, and a methodology and collection of systematic procedures, based upon those axioms, for responsibly analyzing the complexities inherent in decision problems."

The foundations of decision analysis are provided by a set of axioms stated alternatively in von Neumann and Morgenstern [1947], Savage [1954], and Pratt et al. [1964], and the Appendix of this article. These axioms, which provide principles for analyzing decision problems, imply that the attractiveness of alternatives should depend on (1) the likelihoods of the possible consequences of each alternative, and (2) the preferences of the decision makers for those consequences. The philosophical implications of the axioms are that all decisions require subjective judgments and that the likelihoods of various consequences and their desirability should be separately estimated using probabilities and utilities respectively. The technical implications of the axioms are that probabilities and utilities can be used to calculate the expected utility of each alternative and that alternatives with higher expected utilities should be preferred. The practical implication of the decision analysis axioms is the provision of a sound basis and general approach for including judgments and values in an analysis of decision alternatives. This permits systematic analysis in a defensible manner of a vast range of decision problems.

Decision analysis focuses on aspects fundamental to all decision problems, namely

1. A perceived need to accomplish some objectives,
2. Several alternatives, one of which must be selected,
3. The consequences associated with alternatives are different,
4. Uncertainty usually about the consequences of each alternative,
5. The possible consequences are not all equally valued.

The decision problem is decomposed into parts, which are separately analyzed and integrated with the logic of the decision analysis axioms to suggest which alternative should be chosen. This "divide and conquer" orientation is almost essential for addressing interdisciplinary problems. The methodology of decision analysis provides a framework to combine traditional techniques of operations research, management science, and systems analysis with professional judgments and values in a unified analysis to support decision-making. With the procedures of decision analysis, models (e.g., economic, scientific, operations research), available data, information from samples and tests, and the knowledge of experts are used to quantify the likelihoods of various consequences of alternatives in terms of probabilities. Utility theory is used to quantify the values of decision makers for these consequences.

3. THE METHODOLOGY OF DECISION ANALYSIS

This section presents an overview of the methodology of decision analysis without delving into too much detail. Books by Raiffa [1968], Schlaifer [1969], Tribus [1969], Winkler [1972], Brown et al. [1974], Keeney and Raiffa [1976], Moore and Thomas [1976], Kaufman and Thomas [1977], LaValle [1978], and Holloway [1979] provide details on various aspects of the methodology. My purpose is to indicate its general thrust, with emphasis on those aspects unique to decision analysis.

For discussion purposes, the methodology of decision analysis will be decomposed into four steps:

1. Structure the decision problem,

2. Assess possible impacts of each alternative,
3. Determine preferences (values) of decision makers, and
4. Evaluate and compare alternatives.

Figure 1 illustrates the interdependencies of the steps and indicates where the features of complexity introduced in Section 1 are addressed. To interpret the implications of these steps, it is important to keep two facts in mind. First, one iterates among the various steps. Not only what should be done in one step but how it should be done can be affected by preliminary results from another step. Second, decision analyses concentrating on some steps almost to the exclusion of others are often appropriate and useful. Such considerations are mentioned in more detail in Section 4 on the practice of decision analysis.

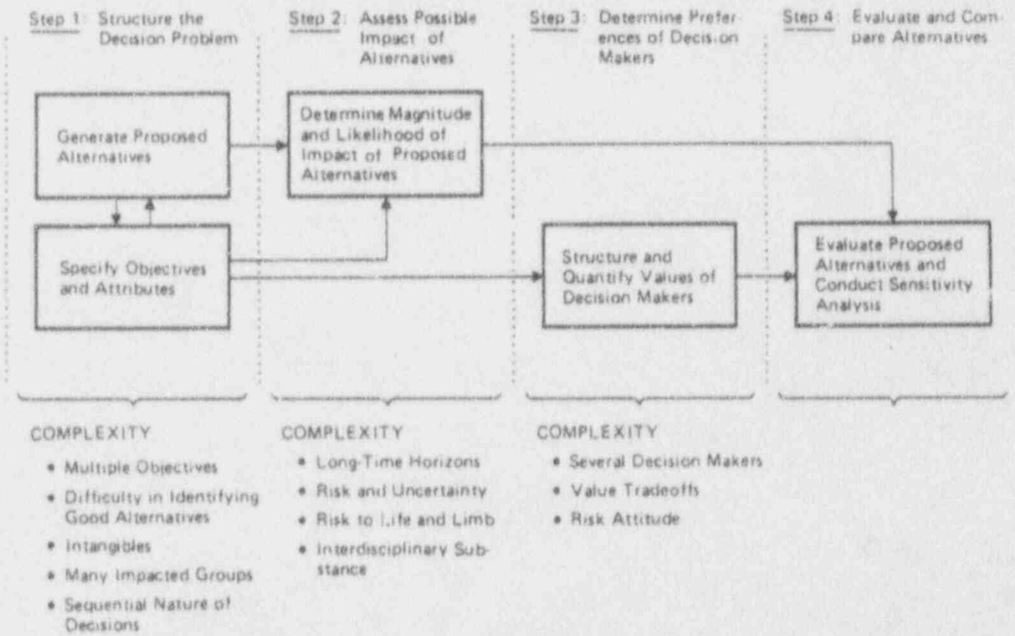

Figure 1. Schematic representation of the steps of decision analysis.

Step 1—Structure the Decision Problem

Structuring the decision problem includes the generation of alternatives and the specification of objectives. The creativity required for these tasks is promoted by the systematic thought processes of decision analysis.

Decision analysis captures the dynamic nature of decision processes by prescribing a decision strategy that indicates what action should be chosen initially and what further actions should be selected for each subsequent event that could occur. For instance, a decision strategy might suggest an initial test market for a new product and then, based on the results, either cancel the product, initiate further testing, or begin a full scale marketing and sales effort. Thus, in describing the alternatives, one must simultaneously specify the decision points, events that may occur between them, and the information that can be learned in the process. This dynamic structure can be conveniently represented as a decision tree (Raiffa [1968]).

Two major problems are associated with generating alternatives. First, there may be a large number of potential alternatives, many of which are not particularly good. However, early in the investigation of the decision problem, it may be difficult to differentiate between the good alternatives and those which are eventually found to be inferior. In such circumstances, inferior options can be identified by screening models which use assumptions too crude for a final evaluation but sensitive enough to weed out the "bad" alternatives. These models analyze a simplified decision problem by using deterministic rather than probabilistic impact, dominance or "almost dominance" rather than a complete objective function, and constraints. This has the effect of eliminating alternatives so the decision tree is pruned to a manageable size. Consequently, more time and effort can be expended to carefully appraise the remaining viable alternatives.

A second major problem associated with generating alternatives is that sometimes there seems to be a complete lack of reasonable alternatives. In this case, it is often worthwhile to utilize the objectives of the problem to stimulate creativity. If the objectives are clearly specified, one can describe possible consequences of the problem which are particularly desirable. Working backward, one asks what type of alternatives might achieve such consequences. The process of quantifying the objectives with an objective function (i.e., a utility function as discussed in Step 3) promotes additional thinking about worthwhile alternatives. The result of such a process is often a broadening of alternatives, which is actually a broadening of the decision problem. For instance, a significant increase in local crime might result in a "premature" decision that more police are needed. An analysis might be initiated of alternatives differing only in the number of additional police, however, the problem is presumably much broader. The objective would likely be to minimize crime. From this perspective, one may create alternatives involving additional police equipment (e.g., cars, communications), different operating policies with existing personnel and equipment, community action programs to report "suspicious" activity, or the reduction of early release programs for hard-core criminals in jails. A critical change is the introduction of dynamic alternatives rather than reliance on static alternatives alone. The difference is that a dynamic alternative is designed to be adapted over time based on external circumstances and new information.

The starting point for specifying objectives is the creation of an unstructured list of possible consequences of the alternatives. The consequences must be organized into a set of general concerns. For instance, with many problems involving siting large-scale facilities, the general concerns may be environmental impact, economics, socioeconomics, health and safety, and public attitudes. To determine specific objectives, the question is, for example, what is the environmental impact of a particular problem. The process of answering such questions is essentially a creative task. Previous studies on related topics and legal and regulatory guidelines should be of significant help in articulating objectives. For problems requiring external review, the potential reviewers (i.e., intervenors, shareholders, or concerned citizens) may contribute useful ideas for objectives.

From all of this information, an objectives hierarchy should emerge with broad objectives pertaining to general concerns at the top of the hierarchy and more detailed objectives further down. The lower-level objectives essentially define the meaning of higher-level objectives; the lower-level objectives are means to the higher-level ends. Holes in the hierarchy can be identified and filled by the following means-ends relationships.

For each of the lowest level objectives in the hierarchy, we must identify attributes to measure the degree to which the objective is achieved. Sometimes this is easy. For example, an obvious attribute for the objective "maximize profits" is millions of dollars (why not think big?). However, it is more difficult to determine an attribute for an objective like "minimize visual degradation." This often requires constructing an attribute to measure the objective using procedures such as those in Keeney [1981].

Let us now introduce notation to concisely describe our problem structure. We have generated a number of alternatives A_j, $j = 1, \cdots, J$, and an objectives hierarchy with n lowest-level objectives O_i, $i = 1, \cdots, n$, where n may be one. With these lowest-level objectives would be associated attributes X_i, $i = 1, \cdots, n$. Furthermore, define x_i to be a specific level of X_i, so the possible impact of selecting an alternative can be characterized by the consequence $x \equiv (x_1, x_2, \cdots, x_n)$. An example of an objective O_i is "maximize the local economic benefit" and an associated attribute X_i may be "annual local tax paid." A level x_i could then be \$29 million.

The first step of decision analysis addresses several complexities discussed in Section 1. The multiple objective feature is addressed by specifying O_1 to O_n. Some of these objectives concern the effect on various groups so this feature is also considered. The intangibles are included by using objectives such as "minimize aesthetic disruption" and, of course, significant effort is focused on the complexity of generating viable dynamic alternatives.

Step 2—Assess the Possible Impacts of Different Alternatives

In this step of decision analysis, we wish to determine the impact of each alternative. If it were possible to precisely forecast impact, we could associate one consequence with each alternative. Then the evaluation of alternatives would boil down to a choice of the best consequence. Unfortunately, the problem is usually not so simple because of uncertainties about the eventual consequences. Therefore, for each possible alternative, it is desirable to determine the set of possible consequences and the probabilities of each occurring. This can be done formally by determining a probability distribution function $p_j(x)$ over the set of attributes for each alternative A_j. In some cases the uncertainty associated with an alternative may be small. Then, an appropriate simplification is to omit the uncertainty for that alternative. Because one can treat p_j in general to include cases with no uncertainty (where $p_j(x)$ assigns a probability one to a particular x and zero to all others), we will use p_j throughout.

When feasible (meaning that both general knowledge about the problem structure and the scope of the project allows it), it is desirable to determine probabilities of possible consequences with the development and use of formal models. These models typically utilize the traditional methodologies of operations research, management science, systems analysis, simulation, planning, and the sciences and engineering. Complex models can often be constructed to have several components, each pertaining to knowledge associated with a single discipline or organizational unit. For instance, in a decision analysis by Smallwood and Morris [1980] examining whether to build a new manufacturing facility, a model had components concerning the market for the proposed product, maintenance, production, capital costs, the competition, and the financial impact on the company. Experts in each of these substantive areas could provide information on their part of the problem. Hence, these models allow one to break the assessment into manageable parts and combine the parts to determine p_j.

When a model is utilized, either deterministic or probabilistic information is required to specify model inputs in order to determine appropriate probability distributions over model outputs (i.e., consequences). When a model is not appropriate, information is necessary to directly determine possible consequences. In both cases, such information must be based on the analysis of existing data, data collected specifically for the decision problem, or professional judgment. Data analysis is common to many disciplines other than decision analysis so, although it is important, it will be passed over here. The quantitative assessment of professional judgments or probabilities is a unique aspect of decision analysis discussed below.

There are several methods for quantifying probabilities (see Winkler [1967a], Spetzler and Staël von Holstein [1975], and Staël von Holstein and Matheson [1979]). One method is to use a standard probability distribution function and assess parameters for that function. For example, the parameters of a normal distribution are the mean and standard deviation. Another technique, referred to as a fractile method, involves directly assessing points on the cumulative probability density function. Suppose y is the single dimensional parameter of interest and we wish to assess the probability density function $p(y)$. One is asked for a level such that the probability is p' that the actual level is less than y'. questioning is repeated for several probabilities such as $p' = 0.05, 0.25, 0.5, 0.75$, and 0.95. Alternatively one can ask for a probability p'' that the y-level is less than y''. By fitting a common probability distribution to the assessed data, one obtains $p(y)$. A third procedure for assessment is appropriate when the possible impact is categorized into distinct levels. The professional familiar with the subject is asked to specify the probability of each level. These assessments may sound easy, but in practice they are involved processes with many potential sources for error (see, for example, Tversky and Kahneman [1974, 1981]). However, recent experience suggests that professionals with training can formulate probabilistic forecasts in a reliable manner (see Murphy and Winkler [1977]).

A factor which can increase the complexity of impact assessments is probabilistic dependencies among attributes for given alternatives. If two attributes are probabilistically dependent, the impact specified for one will affect the assessed impact on the other. When there are conditional dependencies, it is essential to either model the dependencies and develop probabilistic assessments using the output of the model, or to bound the possible probability distributions utilizing logic and understanding of the problem (see, for example, Sarin [1978] and Kirkwood and Pollack [1980]). Then investigate whether and how the dependencies influence the evaluation of alternatives. If such dependencies are important, additional effort to better characterize them may be appropriate.

A host of additional difficulties can occur when more than one expert is asked for professional judgments about the same events. These experts may have different opinions, yet it may be almost impossible to discover the reasons for the differences. Furthermore, it is likely that the experts formulate their judgments based partially on the same experiments and data sources, so they are not independent. Still, the decision maker may desire a single coherent representation of the uncertainty in the problem. Recent contributions by Morris [1977] and Winkler [1981] address this problem, which is one area of current research in decision analysis.

Specifying probability distributions addresses the risk and uncertainty aspects of the decision problem. In describing the possible impact, the time in which consequences might occur should be indicated. Thus, the feature of long-time horizons is addressed in this step. The interdisciplinary substance is also included by utilizing the skills of the various disciplines to develop and structure models, provide information and professional judgments relevant to the discipline, and appraise the results of the model about possible consequences concerning the disciplinary substance.

Step 3—Determine Preferences (Values) to Decision Makers

It is probably impossible to achieve the best level with respect to each objective in a decision problem. The question is, "How much should be given up with regard to one objective to achieve a specified improvement in another?" The issue is one of value tradeoffs. For decision problems with either single or multiple objectives, it is rarely the case (except in simple problems) that one alternative is guaranteed to yield the best available consequence. There are usually circumstances that could lead to undesirable consequences with any given alternative. The question is, "Are the potential benefits of having things go right worth the risks if things go wrong?" This issue is about risk attitudes. Both value tradeoffs and risk attitudes are particularly complicated because there are no right or wrong values. Basically, what is needed is an objective function which aggregates all the individual objectives and an attitude toward risk. In decision analysis, such an objective function is referred to as a utility function, symbolically written u. Then $u(x)$, the utility of the consequence x, indicates the desirability of x relative to all other consequences. As mentioned in Section 2, following directly from the axioms of decision analysis, alternatives with higher expected (i.e., average) utilities should be preferred to those with lower expected utilities.

This step, unique to decision analysis, involves the creation of a model of values to evaluate the alternatives. This is done in a structured discussion between a decision analyst and the decision makers to quantify value judgments about possible consequences in the problem. The procedure systematically elicits relevant information about value tradeoffs, equity concerns, and risk attitudes with provision for consistency checks. In addition to the obvious advantage of providing a theoretically sound manner to evaluate alternatives, the explicit development of a value model offers several other advantages, including indicating which information is of interest in the problem, suggesting alternatives that may have been overlooked, providing a means to calculate the value of obtaining additional information, and facilitating concise communication about objectives among interest parties. In addition, a sensitivity analysis of the value judgments can be conducted to appraise their importance for the overall decision.

The process of determining the utility function can be broken into five steps: (1) introducing terminology and ideas, (2) determining the general preference structure, (3) assessing single-attribute utility functions, (4) evaluating scaling constants, and (5) checking for consistency and reiterating. For decision problems with a single objective, only Steps 1, 3, and 5 are relevant. In practice there is considerable interaction between the steps although each will be separately discussed.

Introducing Terminology and Ideas. The basic purpose of this step is to develop a rapport and an ability to communicate between the decision analyst and the decision maker or decision makers. It should be stated that the goal of the assessment process is to end up with a consistent representation of preferences for evaluating alternatives. The analyst should make sure that the decision makers are comfortable with the assessment procedure and understand the meaning of each attribute and the objective it is meant to measure. If the decision makers have not been closely involved in defining the attributes or describing the impact of alternatives, this phase of communication is particularly important. The decision makers should understand that there are no correct or incorrect preferences and that expressed preferences can be altered at any time.

Determining the General Preference Structure. Here, one structures preferences with a model indicating the general functional form of the utility function $u(x_1, \cdots, x_n)$. To obtain the structure for multiple objectives, one uses value independence concepts in the same way that probabilistic independence is utilized in structuring models of impacts. Most of the independence concepts concern relative values for consequences with levels of a subset of the attributes fixed. The independence concepts are used to derive a simple function f such as

$$u(x_1, \cdots, x_n) = f[u_1(x_1), \cdots, u_n(x_n), k_1, \cdots, k_m, \cdots, k_R] \quad (1)$$

where the u_i are single-attribute utility functions and the k_m are scaling constants. Specific functional forms following from various assumptions are found in Fishburn [1964, 1965, 1970], Meyer [1970], Farquhar, Keeney and Raiffa, Bell [1977b, 1979b], Tamura and Nakamura [1978], and Farquhar and Fishburn [1981]. Using (1), the overall utility function is determined by assessing the single-attribute utility functions and the scaling constants which weight various combinations of single-attribute functions.

A related approach to model values for multiple objectives involves building a value functin $v(x_1, \cdots, x_n)$ which assigns higher numbers (i.e., values) to preferred consequences. This is done in a spirit akin to (1) using either single-attribute value functions or indifference curves together with scaling constants. A utility function is assessed over value providing $u[v(x)]$ which incorporates value tradeoffs in v and an attitude toward risk in u. Models of value functions addressing multiple objectives are found in Debreu [1960], Koopmans [1960], Luce and Tukey [1964], Krantz [1964], Krantz et al. [1971], Dyer and Sarin [1979], Kirkwood and Sarin [1980], and Keelin [1981]. A commonly used value function is the discounting of cash flows over time at a fixed rate. Boyd [1973] and Keeney and Raiffa discuss procedures to obtain both $v(x)$ and $u(v)$.

Assessing Single-Attribute Utility Functions. Procedures for assessing single-attribute utility functions are well developed. In summary, one wishes to first determine the appropriate risk attitude. For instance, for consequences involving profits, one is said to be risk-averse if profit level $(x_1 + x_2)/2$ is always preferred to a lottery yielding either x_1 or x_2 each with a probability of 0.5. In this case, the average of profits x_1 and x_2 is preferable rather than risking a half chance of the higher and a half chance of the lower. When one is risk-averse, the corresponding single-attribute utility function is concave. As discussed in Pratt [1964], special risk attitudes restrict the functional form of single-attribute utility functions. A common utility function is the exponential utility function

$$u(x) = a + b^{-cx} \tag{2}$$

where $a, b > 0, c > 0$ are scaling constants. This utility function is referred to as constantly risk-averse since it is the only one consistent with the following property. If x_3 is indifferent to a 0.5 chance at either x_1 or x_2, then $x_3 + \epsilon$ must be indifferent to 0.5 chance at either $x_1 + \epsilon$ or $x_2 + \epsilon$ for all possible ϵ.

To specify the scaling constants a and b in (2), one arbitrarily sets the utility corresponding to two consequences. This is similar to defining a temperature scale by selecting a boiling and a freezing point. The utilities of all other consequences are relative to the two chosen for the scale. To specify the appropriate numerical value for a constant c in (2), one can identify both a lottery and a consequence which are equally preferred by the decision maker. For instance, suppose the decision maker is indifferent regarding the certain consequence x_3 and a lottery yielding either x_1 or x_2 with equal chances of 0.5. Then, to be consistent with the axioms of decision analysis, the utility of x_3 must be set equal to the expected utility of the lottery. Hence,

$$u(x_3) = 0.5u(x_1) + 0.5u(x_2). \tag{3}$$

Substituting (2) into (3) and solving gives us the value for parameter c.

Evaluating Scaling Constants. With multiple objectives, the same concept is utilized to determine scaling constants, which relate to the relative desirability of specified changes of different attribute levels. To illustrate this in a simple case, consider the additive utility function

$$u(x_1, \cdots, x_n) = \sum_{i=1}^{n} k_i u_i(x_i), \tag{4}$$

where k_i, $i = 1, \cdots, n$ are scaling constants. For this additive utility function, the values of the k_i indicate the relative importance of changing each attribute from its least desirable to its most desirable level. To assess these scaling constants, one generates data representing stated value judgments of the decision maker. For instance, the decision maker may be indifferent between $(x_1, \cdots, x_n)$ and $(x_1', \cdots, x_n')$. Then the utility of these two consequences, since they are indifferent, must be equal. They are set equal using (4) which yields an equation with the scaling factors as unknowns. Using such indifferences, one generates a set of n independent equations which is solved to determine values for the n unknown scaling factors. The equations can be generated by sequentially considering consequences which differ in terms of the levels of only two attributes. This significantly simplifies the comparison task required of the decision makers. More details about the assessment of utility functions can be found in Fishburn [1967], Huber [1974], Keeney and Raiffa, Bell [1979a], and many other sources.

Checking Consistency. It has been my experience that invariably there are inconsistencies in the initial assessments. In fact, this is one of the main reasons for the procedure, because once inconsistencies are identified, decision makers can alter their responses to reach consistency and better reflect their basic values. Furthermore, decision makers usually feel better after having straightened out their value structure in their own mind. Thus, it is essential to ask questions in different ways and to carefully reiterate through aspects of the assessment procedure until a consistent representation of the decision maker's values is achieved. Conducting sensitivity analysis of the evaluation of alternatives (Step 4 of decision analysis) may reveal if the utility function is a good enough representation of decision maker values.

With multiple decision makers, as discussed in Harsanyi [1955], Fishburn [1973], or Keeney and Raiffa, additional value judgments are required to address the relative importance of the different decision makers and the relative intensity of the potential impact to each in order to determine an overall utility function. Alternately, the decision problem can be analyzed from the viewpoints of the different decision makers by using their own utility functions. It may be that the same alternative is preferred by each decision maker, possibly for different reasons. In any case, it might be helpful to eliminate dominated alternatives, identify the basis for conflicts, and suggest mechanisms for resolution.

This third step of decision analysis uses value judgments to address the complexities concerning value tradeoffs and risk attitude outlined in Section 1. The value judgments are made explicit in assessing u for each decision maker. This process of building a model of values corresponds precisely with that used for any model. We gather some data (the decision maker's judgments), and use the data in a generic model (the utility function u) to calculate its parameters (e.g., the k_m's in (1) and c in (2)). Additional value judgments are necessary to structure values of multiple decision makers into one coherent utility function.

Step 4—Evaluate and Compare Alternatives

Once a decision problem is structured, the magnitude and the associated likelihoods of consequences determined, and the preference structure established, the information must be synthesized in a logical manner to evaluate the alternatives. It follows from the axioms of decision analysis that the basis for this evaluation is the expected utility $E_j(u)$ for each alternative A_j, which is

$$E_j(u) = \int p_j(x)u(x)dx. \quad (5)$$

The higher $E_j(u)$ is, the more desirable the alternative. Thus the magnitudes of $E_j(u)$ can be used to establish a ranking that indicates the decision maker's preferences for the alternatives. It should be remembered that the expected utility associated with an alternative is directly related to the objectives originally chosen to guide the decision and reflects the degree of achievement of the objectives. One can transform the $E_j(u)$ numbers back into equivalent consequences to obtain information about how much one alternative is preferred over another.

It is extremely important to examine the sensitivity of the decision to different views about the uncertainties associated with the various consequences and to different value structures. This is conceptually easy with decision analysis, since both impact and values are explicitly quantified with probability distributions and the utility function, respectively. Without quantification it would be difficult to conduct a thorough sensitivity analysis. A useful way to present the results of a sensitivity analysis is to identify sets of conditions (in terms of uncertainties and preferences) under which various options should be preferred.

4. PRACTICE OF DECISION ANALYSIS

The ultimate purpose of decision analysis is to help decision makers make better decisions. The foundations, provided by the axioms, do not "assume the problem away." Even though the theory and procedures are straightforward, a price is paid for attempting to address the complexities of a decision problem explicitly. The implementation phase, that is putting the methodology into practice, is more involved compared to other forms of analysis. A significantly greater portion of the overall effort in decision analysis is spent generating alternatives, specifying objectives, eliciting professional and value judgments, and interpreting implications of the analysis. Each of these requires interaction between the decision analyst and the decision makers or individuals knowledgeable about the problem substance. Structured creative thinking is demanded and sensitive information is elicited.

In this section, we suggest how to conduct a decision analysis and the art of interaction necessary to elicit information. The usefulness of decision analysis and several uses in addition to evaluating alternatives are indicated. Finally key potential pitfalls are identified.

Conducting a Decision Analysis

A careful definition of the decision problem is essential. For complex problems, an adequate definition is rarely available at the time the analysis is to begin. Yet, it is tempting to begin analyzing the problem immediately. What is available at the *beginning* is a somewhat vaguely perceived notion of problem objectives and possible alternatives. Defining a problem means: generating specific objectives with appropriate attributes and articulating dynamic alternatives including possible information to be learned in the decision process. The attributes indicate what information is wanted about the alternatives, namely the degree to which the alternatives measure up in terms of the attributes.

Given the set of attributes, the utility function can be assessed to quantify the decision maker's values. That is, Step 3 of a decision analysis can proceed before Step 2 (see Figure 1). The utility function can then be used to indicate the relative importance of gathering different information. This is important because assessing values requires only personal interaction which is much less expensive than the field tests, equipment, and surveys often necessary to quantify the impact of the alternatives. Knowing what information to collect may reduce this burden or at least focus it on the information desired. One other point is worth mentioning in this regard. There is one value structure for a decision problem since each alternative is to achieve the same objectives. There is a possible impact to be assessed for each alternative. Thus, concentrating thoroughly on the values at the outset may save time, effort, and money on a decision analysis, and result in more useful insights for the problem.

Once the decision problem is well-structured, the collection of information should proceed as indicated in Step 2 of Section 3. The process may be complicated because of problem substance or required personal interaction. The former situation is not unique to decision analysis and will not be discussed further.

The Art of Decision Analysis Interaction

A key to successful decision analysis is the interaction of decision analysts with the decision makers and other professionals working on the project. As with all forms of personal interaction, there is a great deal of art and skill required. Most of the skills required to be a successful member of any group are also necessary to be a successful member of the team analyzing the decision process. However, the nature of decision analysis gives rise to a few special aspects of that interaction process.

Decision analysts obtain clearly articulated (often quantitative) information about the problem structure, possible impact, parameters for a model, and value judgments. In addition to the complexity of the problem substance, obtaining such information can be difficult because:

1. The information may be sensitive.
2. The natural procedures to process the information in one's mind often result in biased judgments.
3. The respondent may have vested interest in misrepresenting information.

The decision analyst should be aware of any of these three possibilities.

In a recent article, Fischhoff [1980] draws an analogy between decision analysis and psychotherapy. Decision analysts try to formalize the thinking and feelings that the decision maker wishes to use on the problem. By clarifying and even quantifying the process, these thoughts and feelings are potentially opened for review by others (e.g., bosses, regulators, courts). In any assessment process, one should take the time and use any available devices to establish a rapport with the respondent and to make him or her feel comfortable. I always point out that the reason for the analysis is that the problems are too difficult to informally analyze consistently. Hence, a major purpose of these processes is to identify inconsistencies in the unassisted thinking of the respondent. It is critical to assure these individuals that they will have a first right to adequately review your work. Furthermore, they should have the option of changing their responses. This helps to ensure that no misrepresentation of their judgments occurs. What this boils down to is the need to build trust between the decision analyst and all respondents working on a decision problem. The establishment of this trust is the responsibility of the decision analyst.

Tversky and Kahneman [1974, 1981] have identified many biases that individuals may inadvertently utilize in providing professional or value judgments. These biases probably occur with any procedure, formal or informal, to assist in the decision-making process. Since decision analysis focuses on such issues, reasonable procedures have been developed with consistency checks to avoid, or at least identify, the major biases which may be influencing the particular analysis. Many professionals, including Winkler [1967b], Slovic and Lichtenstein [1971], Hogarth [1975], Spetzler and Staël von Holstein, Fischer [1976, 1979], Seaver et al. [1978], and Alpert and Raiffa [1981], have compared various approaches to examine their strengths and weaknesses for such assessments.

A more difficult issue for the analyst might be that of potential conflict. A decision maker who wants a particular product to be produced and marketed may be motivated to overestimate its potential sales. A product manager whose evaluation depends on meeting a specific goal might underestimate the potential sales during the goal setting process. To assist in identifying such conflicts, aside from one's knowledge of the position of individuals with respect to the problem, several techniques are used to reduce conflict.

Effects due to the sensitive nature of decision information, inherent conflicts, and unconscious biases can be reduced by using four devices: iteration with consistency checks, assessments with different individuals, decomposition, and sensitivity analysis. Information should be gathered using redundant lines of questioning, and resulting inconsistencies should be investigated until consistency is achieved. Then, there is some comfort that the major discrepancies are eliminated. Use of judgments about the same factor obtained from different qualified individuals has obvious virtues. Decomposition involves dividing the assessment into component parts and obtaining judgments on the components. For instance, in addition to asking the product manager about profit from the product, ask component judgments about product manufacturing costs, distribution costs, potential sales at various prices, pricing policy, and competitor actions. Different individuals should provide these inputs which would then be utilized to provide estimates of profit. Sensitivity analysis can identify problem elements which are crucial for the evaluation of the alternatives. It is only for these that significant effort is necessary to appraise the recommendations of the analysis.

The Usefulness of Decision Analysis

There are some important misconceptions about the philosophy and usefulness of decision analysis. It is worthwhile to mention them since some individuals conclude that they limit the applicability of decision analysis. The misconceptions are:

1. Objective, value-free analysis is needed, whereas decision analysis is too subjective and value laden.
2. Decision analysis does not apply to many decision makers who, by their choices of alternatives, violate the axioms of decision analysis.
3. The purpose of decision analysis is to solve decision problems, but this is rarely achieved since important factors are always left out of the analysis.
4. Decision analysis requires a single, identifiable decision maker, yet most decisions involve groups of decision makers, some of whom may not be clearly identified.

This subsection presents my viewpoints about these misconceptions. Although some decision analysts may have a different perspective, I believe that the circumstances giving rise to these misconceptions actually enhance the usefulness of decision analysis rather than limit its applicability.

Objective, value-free analysis is not possible or desirable. As complexity increases, the percentage of the problem which can be be captured by "objective" data typically decreases. Simultaneously, the role that values, professional judgment, and experience must necessarily play in the decision process increases. We do not have data bases for the possible consequences of a particular merger, the overall impact of "rescuing an industry," the "true" probability of low probability-high consequence events, the price and availability of oil in 1990, or the value of the environmental, economic, and social consequences of an oil shale program. Yet decisions involving such factors will necessarily continue and are crucial to everyone. What is needed is logical, systematic analysis that makes the necessary professional and value judgments explicit and combines these with the "objective" data for the problem. Decision analysis provides the theory and procedures for such analysis.

Many decision makers prefer to act in accord with the decision analysis axioms and yet seriously violate them in selecting alternatives without the benefit of a decision analysis (see, for example, Kahneman and Tversky [1979]). This is a strong motivation for the prescriptive appeal of the approach. The purpose of prescriptive decision analyses is to provide insight about which alternative should be chosen to be consistent with the information about the problem and the values of decision makers.

Decision analysis will not solve a decision problem, nor is it intended to. Its purpose is to produce insight and promote creativity to help decision makers make better decisions. In selecting an alternative, the decision makers should jointly weigh the implications of an analysis together with other factors not in the analysis. This orientation simultaneously implies that decision analysis is not up to the task of solving any decision problem, but that it is appropriate to all. Of course it is not worth analyzing every problem. More difficult decision problems are naturally more difficult to analyze. This is true regardless of the degree to which formal analysis (i.e., use of models as a decision aid) or intuitive appraisal (i.e., in one's head) is used. However, as complexity increases, the efficacy of the intuitive appraisal decreases more rapidly than formal analysis. Thus, roughly speaking, it may be more useful to analyze 60% of a difficult problem than 90% of a simpler problem.

Decision analysis does not require either a single decision maker or identifiable decision makers. It requires an orientation to the decision to be made and individuals able and willing to provide information essential to that decision. The essential assumptions in this regard are Axioms 1, 2, and 3 given in the Appendix. What is assumed is that the information required by those assumptions can be obtained in a useful manner. A decision analysis which structured and analyzed a decision problem without any interaction with or knowledge of the "decision makers" could provide a tremendous amount of insight—the product of decision analysis—to anyone aware of the analysis. While it may be easier to structure a decision problem and provide critical information by interacting with the identified decision makers, many critical problems do not afford this luxury, and it is not essential for constructive decision analysis to occur.

Uses of Decision Analysis

One key manner of deriving insight from a decision analysis is to evaluate the alternatives. This is of course common to most prescriptive analytical approaches. However, decision analysis has other crucial uses to provide insight.

A strength of decision analysis is that one can readily calculate the value of additional information (see LaValle [1968] and Merkhofer [1977]). This is done by defining and evaluating alternatives which include the costs of gathering specific information and the likelihood of what that information will be. For example, a test market for a proposed new product may cost one million dollars and the results may indicate potential annual sales anywhere between 20,000 and 500,000 sales per year. If the "test market" alternative has a higher expected utility than the "no test market" alternative, it is worthwhile. By raising the cost of the test market, we can find the cost where these two alternatives are indifferent. This cost is referred to as the value of the test market information and indicates the maximum one should pay for that information. Using this basic idea, Gilbert and Richels [1981] analyze the value of uranium resource information for U.S. energy policy decisions.

Because of the focus on problem complexities, there are many useful by-products of decision analysis. The framework of decision analysis promotes honesty by providing the opportunity for various independent checks and centers communication on crucial problem features. For instance, one often develops a clear understanding of the substantive issues of a problem in the process of structuring the objectives hierarchy. This also has the effect of sensitizing different individuals to the issues and perhaps bringing about a commonality of understanding of what the problem is or at least a common set of terms to discuss the problem. Also, creative alternatives can be generated by stimulating thinking based on the problem objectives.

Finally, decision analysis can be very important in conflict identification and resolution. It should indicate whether conflicts among various individuals concern the possible impacts or the values for these impacts. Furthermore, conflicts may involve only certain objectives in either case. Once conflicts are identified, attention can be concentrated on their resolution by examining the bases for judgments of each individual concerned. It may be that only parts of the individuals' bases differ and those parts are the reason for the conflict. Information might be gathered which would resolve such a conflict. However, there are irresolvable conflicts, such as justifiable differences in values. For these cases, identification of the basis for the conflict may in itself by an important contribution toward a more responsible decision.

Many decision analyses do not need to be complete. Partial decision analyses which give cursory qualitative attention to some steps in Section 3 are definitely appropriate for many decision problems. These partial analyses should focus on the aspects of the overall problem where insight might be most fruitful to the decision makers. Once the problem is structured, or the impact of alternatives clarified, or the values articulated, the rest of the analysis may be easy or even unnecessary. In these partial analyses, the unique contribution of decision analysis is often the procedures to address explicitly the softer parts of the problem—its structure and professional and value judgments.

Pitfalls of Decision Analysis

Decision analysis is subject to the same pitfalls as other approaches designed to assist decision makers. These pitfalls can be categorized as follows:

1. Weak or no logical or theoretical foundations,
2. Lack of consideration of subjective and value components of the decision problem,
3. A claim that analysis provides a solution to the decision problem,
4. Poor analysis,
5. Weak personal interaction skills.

As previously stated, the foundations of decision analysis are strong and the subjective and value aspects of the decision problem are addressed. Hence, specific pitfalls under Categories 1 and 2 are rarely the downfall of a decision analysis.

Category 3 represents a pitfall often more common to decision analysis than other approaches. Because decision analysis does try to capture a bigger share of the "real problem," there is a tendency to assume the entire problem is addressed. Worse though is the misrepresentation that such an analysis provides a solution to the decision problem. Decision analysis, indeed any analysis, only focuses on part of a problem and this should be understood.

Poor analysis or poor personal interaction can, of course, render the best conceived decision analysis worthless. Rather than repeat all the things that could go wrong here, it is more appropriate to refer to Howard [1980] for a short appraisal of decision analysis or Majone and Quade [1980] for an entire volume on pitfalls of analysis.

5. APPLICATIONS OF DECISION ANALYSIS

Discussions of early applications of decision analysis in the oil and gas industry are described in Grayson [1960] and Kaufman [1963]. Applications also occurred in other fields. However, for proprietary reasons, many of the completed decision analyses do not appear in the published literature. Fortunately, the essence of some of these analyses appears in the form of "fictitious" analyses or case studies. Magee [1964a, b] describes applications to captial investment decisions, Howard [1966] discusses a product introduction, and Schlaifer [1968] presents cases representing analyses from the early 1960s. Papers by Brown [1970] and Longbottom and Wade [1973] surveyed applications of decision analysis through the 1960s.

The 1970s saw an expansion in applications of decision analysis in both private industry and government. They involved new product decisions, research and development efforts, medical problems, energy problems, environmental alternatives, and the setting of standards to name a few. In this article, it would not be possible to survey all of these applications. Hence, we will simply attempt to indicate sources of some applications which are readily available. Many of these sources describe other applications.

There have been many applications of decision analysis addressing various corporate problems. Although many of these are proprietary, there are some published examples of these corporate decision analyses. Spetzler [1968] describes the procedure of assessing a utility function for a corporate board. Matheson [1969] summarizes an application concerning the introduction of a new product. The book by Brown et al. describes several applications. Keeney [1975] discusses the assessment of a multiple objective corporate utility function to examine corporate policies. Keefer and Kirkwood [1978] discuss an application to optimally allocate an operating budget for project engineering. A recent application described in Smallwood and Morris considers whether Xerox Corporation should construct new manufacturing facilities for a new product and when this should be done. Stillwell et al. [1980] report the evaluation of credit applications.

Rather than discuss selected applications in medical fields, it is simpler to refer readers to a recent annotated bibliography of decision analysis applications by Krischer [1980]. The applications address such diverse problems as evaluating governmental programs to save lives, the evaluation of new drugs, the selection of medical technologies for advanced medical systems, analyses to select treatment strategies for diseases or ailments, the development of on-line computer systems to assist physicians in decision-making, and the development of various health industries.

There have been numerous applications of decision analysis to problems faced by various branches of government over the last decade. Examples of these include the possibility of seeding hurricanes threatening the coasts of the United States (Howard et al. [1972]), metropolitan airport development in Mexico City (de Neufville and Keeney [1972]), protection from wildland fires (North et al. [1975]), trajectory selection for the Mariner Jupiter/Saturn project (Dyer and Miles [1976]), and the evaluation of busing alternatives to achieve school integration (Edwards [1980]). Several recent applications of decision analysis to governmental problems concern selection of standards. Examples include emission control strategies by North and Merkhofer [1976], chronic oil discharge standards by von Winterfeldt [1982], and the negotiation of international oil tanker standards by Ulvila and Snider [1980].

Significant environmental problems concern both government and industry. In the recent past, many decision analyses addressed such environmental problems. Examples are the work of Gardiner and Edwards [1975] concerning development within the areas under the jurisdiction of the California Coastal Commission, work involving Bell [1977a] and Holling [1978] concerning control of a forest pest, the analysis of marine mining options by Lee [1979], and the evaluation of regional environmental systems by Seo and Sakawa [1979].

The area with the greatest number of applications in recent years has been energy. There have been decision analyses of the United States' synthetic fuels policy (Synfuels Interagency Task Force [1975]) and nuclear reactor program (Manne and Richels [1978]), expansion of the California electrical system capacity (Judd [1978]), management of nuclear waste (Lathrop and Watson [1982]), and commercialization of solar photovoltaic systems (Boyd et al. [1982]). There has been considerable effort focused on the alternatives facing the utility industry. These include the selection of technological alternatives for specific projects such as transmission conductors (Crawford et al. [1978]), the examination of the implications of both over- and undercapacity (Cazalet et al. [1978]), the siting of energy facilities (Keeney [1980b], and Sarin [1980]), and the choice between coal and nuclear technology for large-scale power plants (Beley et al. [1981]).

6. HISTORY OF DECISION ANALYSIS

It is difficult to trace decision analysis from its beginning to the present because of the evolutionary nature of both its content and its name. The foundations of decision analysis are the interwined concepts of subjective probability and utility, and Ramsey [1931] was the first to suggest a theory of decision-making based on these two ideas. Two centuries earlier, Bernoulli [1738] wrote a remarkable paper on the motivation for the concept of utility and on a possible form for a utility function. For a historical discussion of the early development of subjective probability and utility theory, see Fellner [1965]. On the uncertainty side, DeFinetti [1937] contributed greatly to the structure of subjective probability. Modern utility theory for decision-making under uncertainty was developed, independently, by von Neumann and Morgenstern. They postulated a set of axioms similar to those in the Appendix (using only objective probabilities) and demonstrated that a utility could be assigned to each consequence in a manner such that the decision maker should prefer the alternative with the highest expected utility in order to act in accord with the axioms. This result is often referred to as the expected utility hypothesis.

Wald [1950], in his classic work on statistical decision problems, used theorems of game theory to prove certain results in statistical decision theory. Although he used an expected-loss criterion instead of utility theory, it was only a minor modification to introduce utility into the Wald framework. This work highlighted a critical problem, namely, how to account for informal information about the states of the world in his model. The school of statisticians and decision theorists, including J. Marschak, H. Chernoff, and H. Rubin, advocated the use of judgmental probability as one method of tackling the statistical decision problems proposed by Wald. The pioneering work of Blackwell and Girshick [1954] contributed to the integration of utilities and subjective probabilities into a coherent program for handling these problems. Then Savage [1954], in a major contribution, provided a rigorous philosophical foundation and axiomatic framework for the approach.

Once the theory was developed, many individuals began applying it to mathematically well-structured problems involving uncertainties and possibilities for sampling or experimentation. These results, building on the work of others, formed a body of results known as Bayesian or statistical decision theory (Schlaifer [1959], Raiffa and Schlaifer [1961], Pratt et al. [1965]). When in the early 1960s these same individuals and their associates, mainly at the Harvard Business School, began using these theories on real business problems involving uncertainties, whether or not sampling and experimentation were possible, an adjective was added to yield applied statistical decision theory. However, since applied statistical decision theory was relevant to broad classes of complex decision problems (see Schlaifer [1969]), it was better to have a more application-oriented name, and the term decision analysis appeared in the literature (Howard [1966]).

Over the past 30 years, the contributions of many people concerned with the behavioral aspects of decision-making have had a significant impact on prescriptive decision analysis. Mosteller and Nogee [1951], Friedman and Savage [1952], Edwards [1954], and Davidson et al. [1957] made early contributions to the assessment of preferences and judgments. Excellent sources for this work are Slovic and Lichtenstein, Tversky and Kahneman [1974, 1981], Hammond et al. [1980], and Einhorn and Hogarth [1981].

Procedures have been developed to better account for specific characteristics of decision problems mentioned in Section 1. Examples include work by Pratt [1964] and Schlaifer [1969] assessing utility functions; by Winkler [1969, 1981], Edwards [1968], Schlaifer [1969], Spetzler and Staël von Holstein [1975], and Morris [1977] assessing probability distributions; by Arrow [1963], Harsanyi [1955], and Keeney and Kirkwood [1975] on group preferences; by Fishburn [1964, 1965, 1974], Pollak [1967], Raiffa [1969], and Boyd [1973] on multiattribute preferences; by Koopmans [1960], Lancaster [1963], Meyer [1977], and Bell [1977b] on preferences over time; by Rousseau and Matheson [1967], Schlaifer [1971], Seo et al. [1978], and Sicherman [1982] developing software systems for structural and computational assistance; by Miller et al. [1976], Jungermann [1980], and von Winterfeldt [1980] on structuring decision problems; and numerous contributions by people in statistics, stochastic processes, systems analysis and computer science to develop better probabilistic models. In many analyses concerning, for example, liquefied natural gas, nuclear power, and hazardous wastes, a critical issue is the value of lives which may be lost due to either an accident or "normal" use. Some of the current methodological development in decision analysis concerns value judgments such as the value of human life (see, for example, Bodily [1980], Howard [1979], Keeney [1980a], and Pliskin et al. [1980]).

7. RESEARCH

The techniques and procedures of decision analysis are sufficiently developed to make substantial contributions on many complex decision problems. Compared to other approaches, both formal and informal, decision analysis has much to offer. Compared to "providing all the insight a decision maker could possibly want at a price too low to refuse," there are significant improvements which could be made. Research on the following topics, categorized by the steps of decision analysis outlined in Section 3, will help lead to improvements. Winkler [1982] provides a much more comprehensive survey of potential research topics in decision analysis.

Regarding the structure of the decision problem, better approaches to develop objective hierarchies, create alternatives, and interrelate the two are needed. The approaches should be systematic to increase the likelihood that important parts of the problem are not omitted. Procedures are needed to help identify relevant stakeholders—individuals or groups with an interest in the problem—and ensure that their objectives and advocated alternatives are recognized by the analysis. Likewise, more systematic procedures are needed to identify exogenous events not under the decision makers control which could significantly influence the consequences of alternatives. This should reduce the likelihood of unanticipated consequences. Any analysis excludes many aspects felt to be less relevant than included ones. Yet, many decision problems are interrelated even though at some level it is impractical to include these interrelationships in detail in an analysis. Improved means of addressing the interrelationships are needed in decision analysis. As a specific example, a carefully defined attribute to measure flexibility in a decision problem might address the degree to which alternatives in a given problem might foreclose options in other decision problems.

Regarding possible impact, two major problems deserving additional research concern probabilistic dependencies and multiple experts. Better methods to characterize probabilistic dependencies and to elicit subjective probability distributions with probabilistic dependencies would be helpful. On many important problems, different experts disagree about the impact expected from various alternatives. Research is needed to provide methods to reduce such discrepancies when appropriate and to produce responsible representations of the "collective judgment" in cases where discrepancies persist.

Models of values could be improved in three significant respects. First, models to better characterize the values of a group are needed. The process requires judgments about the intensity of preferences for group members and their relative importance in the group. In addition, better models to evaluate morbidity and mortality consequences of decision problems would be helpful. Additional research on structuring and eliciting preferences for impact over time, especially for nonmonetary impact, could make a substantial contribution.

Regarding analysis of alternatives, the main research involves developing better computer programs to assist in completing the tasks of decision analysis and better presentation materials and procedures to realize the full potential benefits of decision analyses.

8. SUMMARY

Decision analysis embodies a philosophy, some concepts, and an approach to formally and systematically examine a decision problem. It is in no way a substitute for creative, innovative thinking, but rather it promotes and utilizes such efforts to provide important insights into a problem. Philosophically, decision analysis relies on the basis that the desirability of an alternative should depend on two concerns: the likelihoods that the alternative will lead to various consequences and the decision maker's preferences for those consequences. Decision analysis addresses these concerns separately and provides the logic and techniques for integrating them.

The foundations of decision analysis are a set of axioms which define the basic constructs, judgmental probability and utility, and the logic of the approach. From these axioms, the fundamental result of decision analysis is derived: the alternative with the highest expected utility should be the most preferred. I, and many other individuals, find these axioms compelling for analyzing decision problems to *prescribe* what alternative a decision maker should choose. It is important to note that any decision-making approach which is not consistent with all of the axioms of decision analysis must, by definition, violate at least one of them.

To one who believes in the axioms, the standard for correctness in formally analyzing decision problems must be decision analysis. This does not mean other approaches are not more appropriate in some cases, but that the additional assumptions necessary for these other approaches are important to understand and appraise. Often, however, other seemingly competitive approaches are not in conflict with the axioms of decision analysis. For instance, in cases where there are no uncertainties in describing the possible implications of each alternative and where the utility function (i.e., objective function) is linear, linear programming does not violate the axioms of decision analysis. In such cases, it could be considered a tool of decision analysis, with the big advantage of effectively evaluating an infinite number of alternatives and selecting the best one.

A unique aspect of decision analysis is that its theory and procedures were developed to formally introduce and process subjective judgments in the evaluation of alternatives. Professional and value judgments are clearly an important part of the major problems facing our society. With problems concerning abortion, the desirability of capital punishment, or the treatment of terrorists, professional judgments about the likelihoods of various consequences resulting from each alternative and the value judgments required to evaluate such alternatives must be made. In decisions concerning inflation or the energy situation of the country, judgments must somehow be formulated about the likely effects of various policies. Value tradeoffs must be made between inflation rates and unemployment or between the energy available for personal use (i.e., comfort) and national dependence on foreign fuels. In many cases, to neglect such features misses the essence of the problem altogether.

Experience in using decision analysis indicates that knowledgeable professionals, industry executives, and government officials are willing to address the difficult professional judgments and value questions necessary to focus meaningfully on the characteristics of complex decision problems. However, most analyses of important decision problems have left the incorporation of judgments and values to informal procedures with unidentified assumptions and to the intuition of the decision makers. What has been lacking is not information, but a framework to articulate and integrate the values and professional judgments of decision makers and experts with the existing data to examine the overall implications of alternative courses of action. Decision analysis provides this framework.

APPENDIX. THE AXIOMS OF DECISION ANALYSIS

An important feature of decision analysis is that it has an axiomatic foundation. The axioms provide the rationale and theoretical feasibility for the "divide and conquer" approach of decision analysis. In Section 3, decision analysis was simplified to four steps:

1. Structure the decision problem;
2. Assess the possible impact of each alternative;
3. Determine preferences (values) of decision makers; and
4. Evaluate and compare alternatives.

Axioms corresponding to Steps 1 through 3 state conditions under which it is feasible to obtain the necessary information for a decision analysis organized in this manner. Axioms corresponding to Step 4 provide the substance for aggregating the information in the preceding steps to evaluate the alternatives.

To facilitate understanding, the axioms of decision analysis are stated here in an informal and intuitive manner. The complete sense of the axioms is preserved although they are not technically precise. A formal statement of the axioms is found in Pratt et al. [1964]. In the following, Axioms 1a and b pertain to Step 1 of the decision analysis methodology, Axiom 2 pertains to Step 2, and so on.

AXIOM 1a (Generation of Alternatives). *At least two alternatives can be specified.*

For each of the alternatives, there will be a number of possible consequences which might result if that alternative were followed.

AXIOM 1b (Identification of Consequences). *Possible consequences of each alternative can be identified.*

In identifying consequences, it may be useful to generate an objectives hierarchy indicating the domain of potential consequences in the problem. Attributes can be specified to provide evaluation scales necessary to indicate the degree to which each objective is achieved.

AXIOM 2 (Quantification of Judgment). *The relative likelihoods (i.e., probabilities) of each possible consequence that could result from each alternative can be specified.*

As discussed in Section 3, there are a number of procedures to assist in specifying relative likelihoods. Such probabilistic estimates are based on available data, information collected, analytical or simulation models, and assessment of experts' judgments.

AXIOM 3 (Quantification of Preference). *The relative desirability (i.e., utility) for all the possible consequences of any alternative can be specified.*

The preferences which should be quantified in a decision problem are those of the decision makers. It is very helpful if one can assess these preferences directly from the decision maker or decision makers. However, for many problems other individuals have a responsibility for recommending alternatives to the decision makers. In such problems, those individuals may have a responsibility for articulating an appropriate preference structure.

AXIOM 4a (Comparison of Alternatives). *If two alternatives would each result in the same two possible consequences, the alternative yielding the higher chance of the preferred consequence is preferred.*

AXIOM 4b (Transitivity of Preferences). *If one alternative is preferred to a second alternative and if the second alternative is preferred to a third alternative, then the first alternative is preferred to the third alternative.*

AXIOM 4c (Substitution of Consequences). *If an alternative is modified by replacing one of its consequences with a set of consequences and associated probabilities (i.e., a lottery) that is indifferent to the consequence being replaced, then the original and the modified alternatives should be indifferent.*

Axiom 4a is necessary to indicate how various alternatives should be compared. Axioms 4b and 4c are often referred to as consistency axioms. Axiom 4c allows one to reduce complex alternatives involving a variety of possible consequences to simple alternatives referred to in Axiom 4a. It is then easy to compare alternatives. Axiom 4b is necessary to include comparisons of more than two alternatives.

The main result of these axioms is that the expected utility of an alternative is the indication of its desirability. Alternatives with higher expected utilities should be preferred to those with lower expected utilities. The probabilities and utilities necessary to calculate expected utility emerge as distinct items. Information about each must be gathered in conducting a decision analysis. However, the axioms themselves provide little guidance about how to obtain this information. This is discussed in Sections 3 and 4 on the methodology and practice of decision analysis.

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APPENDIX E
COMMENTS BY H. J. C. KOUTS

GENERAL

1. The document is well structured, and it presents the principal features of PRA as they are pertinent to use by the NRC. It contains the major components of guidance to NRC staff on importance, usage, strengths, and limitations of PRA in such applications. Therefore it appears to be directed to the objective established by the ACRS.

2. The draft still requires a substantial amount of improvement, especially editing. It is uneven in quality and content, principally in sections of the very important Appendix C. Apparently it was necessary to send the draft to us at a time when some of the sections had just come out in their first versions. I have included a lot of editing among the attached detailed comments.

3. Though the draft proposes to deal with application of PRA to NRC's problems of all kinds, it is definitely oriented to use in connection with nuclear power plants. In fact, however, there are fundamental distinctions between applications of PRA to very different kinds of regulatory problems. In application to nuclear reactors, PRA can be relatively reliable (even though its estimates have large margins of uncertainty). Although much engineering judgement goes into the PRA of a nuclear reactor, especially in the step from a Level 1 to a Level 2 PRA, it is reasonably well informed engineering judgement, generating a fair degree of confidence in its application. There will be less reliability attached to a PRA analyzing health effects from accidents in operation of a linear accelerator for food sterilization, because of importance of the contribution of human factors, and a smaller base of engineering experience. At an opposite pole from use with nuclear reactors one finds application of PRA to such problems as theft of fissile material by an armed adversary, or failure of a repository for high level nuclear waste. Here the judgement approaches guesswork.

I believe that guidance is needed on the reliance to be placed on PRA in its applications to such diverse problems.

4. The draft has a tendency to treat PRA as an analytical regime, and it only mentions systems thinking as a process followed by some practitioners on the NRC's staff. I believe that some of the greatest successes of PRA have come as a result of application of engineering insight, especially systems insight, to the analytical part of PRA. Development of the skill of seeing problems from a full systems basis must be included as an essential part of any training program. The need for systems engineering should be woven intimately into the draft.

5. The use of cutoffs should be presented early. The principal reason for cutoff at low probability is the minute contribution that is lost. But another reason is the high uncertainty attached to events at very low probability. The draft mentions

cutoff in a few places, sucn as in connection with decision criteria. But guidance is needed early in connection with input to the analysis. One important reason at this point is to eliminate far-fetched contributors that only consume analytical time and resources.

6. Cannot regulatory guidance be given in the main body of the draft on reconciling PRA with the conventional deterministic concepts based on defense in depth? I do not mean the use of deterministic methods in constructing event trees (some views on this as developed in Appendix C are given among the detailed comments, see later), but the relative reliance on deterministic and PRA methods in actual regulation.

DETAILED COMMENTS

I include editorial suggestions. Some of these may have originated from our meeting in Orange County.

THE MAIN REPORT

- The relation to the Appendixes could be closer. The text could profit in this way from such references.

- This main text is well written and I believe it will be helpful.

P. 1, line 10: *risk vs benefits*
line 24: Comma following *July*.

P. 2, line 13: *other*, not *another*. The last part of the sentance needs cleaning up.

P. 4, line 21: *representatives*
line 45: replace *and in* by *reflecting*.

P. 5, line 20: *organizations*

P. 7, line 38: insert **was** following *plan*.

P. 9, lines 12-14: The sentence is unclear.
line 37: should the colon be a comma?

P. 11, line 43: Appendix C says that "best estimate" is a usage that should be avoided.

P. 13, line 47: replace first *in* by *at*.

P. 15, last bullet: I believe that at this point the importance of a sanity check should be discussed. Sanity checks are covered in the recommendations in lines 28-36 of P. B-7.

P. 16, line 8: *data information* ?
line 9: *these*, not *this*.
line 13: *issues*
Add to the bullets items on the range of probabilities that analyses should take into account, and the importance and implications of uncertainties.

P. 19, line 15: *organizations*
lines 41-42: include participation in the conduct of PRAs.

P. 21, line 2: I doubt that much should be done directly by NRC in developing new PRA methods. Most of that advance will be the work of contractors who are relatively

unencumbered by the red tape of government.
line 20: ***hands-on***

P. 22, line 6: insert ***of*** following ***example.***
line 9: ***example analysis*** ?

P. 23, lines 18-20: I doubt that reliable methods will be found to convert the results of a Level 1 PRA to Level 3 status.

APPENDIX A

•Appendix A is well written. Detailed comments follow.

Section A.3 No research uses are broken out. Should the list not include guidance on what research might be useful?

Bullet on lines 35,35: Did these staff members recognize their deficiency and seek help from others who understand the methodology? In other words, how much interaction exists in the staff on matters in which PRA is used?

Fourth bullet on P. A-7: Presumably this means experience in PRA.

APPENDIX B

- This Appendix is very well written. I regard it as the heart of the report. It is a pity that guidance has been relegated to an Appendix.

- Detailed comments follow.

P. B-1, line 48: *provide*, not *provided*.
line 49: *understand<u>ing</u>*.

Table B.1 should be given a little more clarification. It is hard to understand.

P. B-4, lines 16-23: Of course, the problem is to generate a Level 3 analysis in the absence of a specific site with its specific population distribution. Elsewhere in the report there is mention of use of the Braidwood population distribution as a kind of generic distribution. The problem and the form of its resolution should be mentioned here.
line 32: delete ***the***
line 35: ***are***, not ***is***.
line 42: ***Plan***, not ***Plant***.

P. B-6, line 11: ***Turbine***, not ***Trubine***.
lines18-22: It may be undesirable to generate such a list, because it could become a straightjacket.
line 31: ***ensure*** is better than ***assure*** in this usage.

P. B-10, line 30: ***provide***, not ***provided***.
line 39: ***prioritization***, not ***prioritizations***.

Section on Operational Events, Pp. B11-12: Some current examples of this usage would be helpful.
lines 28-31: The sentence needs rework.

Section on Operational Data Trending on P. B-13: The converse of this process is also important and worth mentioning: that is, the use of operational data to improve the data input to PRAs.

Section on the Maintenance Rule, same page: Should make it clear that what is in question here is performance indicators for maintenance.

P. B-14, line 22: insert ***eliminating*** following ***with***.
lines 37-46: this should be edited to state the items in the list in a parallel fashion, i.e. all as nouns, all as actionnsures, or at least all the same thing.

P. B-15, Section on Severe Accident Issue Analyses: Should also say that this work on features of severe accidents is done largely to be able to replace uncertainties and engineering judgment by data.

line 46: *guid<u>es</u>*, not *guid<u>ance</u>*.

Section on Facility Analyses on P. B-15 should also take up the matter of bottom line usage--when this may and should be done, with what precautions, and when it should be avoided.

P. B-16, line 2: ***are***, not ***is***. Insert ***the*** following ***by***.

lines 5,6: replace ***of a more*** by ***more of the nature of***.

lines 23 and 25: I believe the word should be ***probabilities*** rather than ***frequencies***, for events at very low likelihood.

lines 41-43: states there are three purposes and then lists four.

P. B-17, line 3: replace ***is principally*** by ***principally calls for***.

lines 20,21: close the parentheses.

line 41: ***justif<u>ies</u>***, not ***justif<u>y</u>***.

P. B-18, lines 16-20: this should be edited to state the items in the list in a parallel fashion, i.e. all as nouns, all as actions, or at least all the same thing.

line 50: close quotation marks.

P. B-19, line 41: ***ensure*** is better than ***assure*** in this usage.

Section B.3 and the following is strongly reactor-oriented. It would help if the text said so.

P. B-21,lines 32-33: replace ***The analysis write-up*** by ***Write-up of the analysis***.

lines 41-45: It would help to state a precaution, such as to the extent possible".

line 48: insert ***used*** following ***be***.

P. B-25, line 23: ***CDF***, NOT ***cdf***.

lines 28-30: what is meant?

lines 43,44: The error would be diversion of funds from a more deserving problem.

P. B-26, line 6: delete ***make***.

lines11-15: This is a repetition of the thought developed at the top of the page.

P. B-28, line 2: insert ***and experienced*** following ***knowledgeable***.

line 49: ditto.

P. B-32, lines 3-5: what is meant?

<u>Appendix C</u>

•There is no uniform practice for words being defined. Sometimes they are shown in italics, sometimes in bold. I suggest a single procedure.

•I have caught a number of uses of the word "data" as a singular noun, whereas of course it is a plural noun. I must have missed some. I suggest a word search on "data" to ensure all such mistakes are found.

•I firmly believe in use of the conditional tense rather than the present tense in discussing hypothetical accidents that are only postulated for the purpose of analysis. Use of the present tense gives the reader the idea that the accident is real. Such statements are readily available for excerpting and use against the technology. I have caught a number of such uses and corrected them, but no doubt there are more. Please be on the lookout for them.

•In the following detailed comments, editorial suggestions are mingled with substantive comments.

Section C.2

•Section C.2 is well written.

P. C.5, line 26: add ***now defined to be A*** after $A_1 \cap A^2$.
line 47: delete ***and exhaustive***.

P. C.7, line 28: insert ***with the outcome A occurring*** n_A ***times*** following ***n times***.
lines 41 and 42: change ***experience*** to ***knowledge***.

P. C.8, line 3: insert ***explicitly*** after ***often***.
lines 16, 17: Begin the sentence as follows: ***This notion is defined and used there to represent...***
line 29: add at the end of the last sentence of the paragraph: ***and which expresses the best wisdom as to the outcome of a number of trials if they were carried through to determine the classical frequency***.

P. C.9, line 22: change ***occurs*** to ***has occurred***.

P. C.10, line 7. The last A should be A_n.
line 8: the last bracket should be $[1-P(A_n)]$. Eliminate the bracket after the inequality sign.
line 14: something missing after the period.
line 22: add ***or a subset of them*** following the parentheses.

line 34: change *is said to* to *must*.
lines 46, 47: change *positive, continuous* to *non-negative, integrable*.

P. C.11, Second paragraph: The discussion has proceeded subtly from a range of 0 - ∞ to -∞ - ∞. The text should say so.
line 38: add *if the respective probabilities are both ½*.
Last full paragraph: There is another subtle point here. Should point out that P(x) is also the probability distribution for G(x).

P. C.13, First two sentences: it would help to point out that the rule follows from the fact that independence means factorability of the distribution function.
line 14, 15: delete the full sentence.
line 17: change *generally* to *sometimes*.
line 45: <u>continuous</u> *distribution*.

P. C.15, First paragraph: The term "best estimate" does have meaning for properties and quantities that are to be regarded as single valued, and are not assigned probability distributions on a Bayesian basis.
line 11: change *However* to *Despite this*.
line 12: change *a best* to *an*.

P. C.17, line 34: Delete as evident.

P. C.18, line 10: delete *more-or-less*.
line 18: delete *quite*.
line 42: *data <u>are</u>*.

P. C.19, line 7: replace *parameter value* by *value of the parameter*.
First full paragraph: Edit the last sentence.
line 34: *a <u>logical and</u> unified...*

P. C.21, lines 11-12: delete following the comma.

P. C.27, line 42: delete *agency which is a*.

P. C.28, line 19: change *metals* to *metal systems*.
line 20: change *it* to *that practice*.
line 47: change *it* to *the SCSS*.

P. C.29, line 7: *data <u>are</u>*.
line 47: append *in statistical usage*.

P. C. 30, line 10: *regarding <u>a</u> PRA*.

Section C.3

•This Section is well written.

P. C.35, lines 9-11: the expression is valid only for small λT.

P. C.36, Sentence on lines 4-5: This is not necessarily so, e.g. for a one-out-of-three logic system.
First full paragraph: somewhere here should be a discussion of diversity.

P. C.37, lines 40, 41: change *can* to *could*.

P. C.45, line 10: Something is wrong here. Missing words?

P. C.46, lines 15-23: Should make it clear that the example refers to a parallel linkage of the components.

P. C.47, line 6: *data are*.
Formulae following line 25: they are correct only if $Q_1^2 << Q_2$ and $Q_1^3 << Q_3$.
line 26: *are estimated conditional*.

P. C. 48, line 12: replace *scarce* by *rare*.

P. C.49, line 15: The handbook is not previously referred to.
line 18: Use of "best estimate" does not fit precautions stated in Section C.2.
line 38: *the professional judgment*.
Last full paragraph: Not very understandable. Rephrase.

P. C.51, line 25: Define PSF.

P. C.52, line 47: Replace *He* by *The author*.

P. C.53, lie 6: Insert *advanced by this school* following *argument*.

P. C.54, line 8: Replace *Simpler approaches* by *A simpler approach*.
lines 11-16: Restate.

Section C.4

•This section is not well written. It does not have much meat. It does not say that the matter discussed is an integral part of development of the source term as the objective of a Level 2 PRA. It does not give much guidance, and in fact repeats frequently that guidance is not available. I have edited the section heavily, and it needs rewriting.

P. C.67, line 38: *is*, not *are*.

P. C.68, line 4: *Level 1 analysis and itself* is awkward and should be fixed.
lines 9-10: something is missing after *according*.
line 12: Replace *in* by *leading to*.

line 15: Define *top event*.
line 16: The symbol for the bullet is wrong.
line 27: *course of*.
line 31: *is*, not *are*. Remove the comma.

P. C.69, line 2: rewrite as *less available information, which requires..*
line 6: *pronounced*, not *pronounce*. *for*, not *of*.
line 27: *PRA procedures guide* should be underlined or in italics or something, because it is a Title.
line 29: strike *the*.
lines 31-34: Clean up the sentence.
lines 34-35: restate as *whether an accident progression analysis is being prepared or is being reviewed*.
line 44: following *APET*, insert *where many facets of a severe accident must be considered explicitly,*.

P. C.70, line 1: a comma after the third word. Strike the remainder of the sentence following the word *logic*.
lines 6-7: NUREG-1150 covers five PRAs. Which one is meant?
lines 9-11: Restate as *In a small scale APET, a severe accident is presented in simple terms, with as many as ten top event questions forming a simple logic tree.*
lines 15-16: Restate as *study of risk associated with plant operation at low power and during shutdown*.
line 19: replace *such principles* by *it*.
line 20: replace *by establishing* by *through inspection of*.
line 22: *detailed*, not *detail*.
line 24: strike *an*.
line 25: replace *an* by *a preferred*.
line 26: *but may also*.
line 27: replace *stated a* by *a stated*.
line 33: replace *views* by *have viewed*. Insert *the standpoint of* following *from*.
line 35: replace *with* by *by*.

P. C.71, second line in the box: replace *such* by *the simple question*.
lines1-3: Meaning of the sentence is unclear.
line 3: replace *when* by *as*.
line 8: replace *in only* by *only in*. Strike *same*.
line 9: replace *with consideration of say*, by *by addressing*.
line 19: replace *be* by *lead to*.

P. C.72, line 2: replace *implied* by *including them by implication*.
lines 3-5: Redo the sentence.
lines 22-27: Strange. It says that the guidance is that there is no guidance.
lines 33 and following: This is very unclear. I do not

get an image of what supplemented and unsupplemented event trees are.

P. C.73, line 1: replace *give* by *lead to*.
line 6: replace *fixed into* by *embedded in*.
line 11: Replace *when* by *using*. Add *is used* after *method*.
lines 23-25: Not understandable.
line 31: *from*, not *form*.
line 35: after *established*, add *in any specific case*.
lines 36-38: Repetitive.
The footnote: This is not really true. Fault trees are used to evaluate the branch probabilities in the event trees. The relationship between fault trees and event trees is well presented elsewhere in this Appendix.

This whole section on large and small APETs misses the essential point that the APET must have a size consistent with the state of knowledge of the phenomena dealt with. If there are many branch points where the physics is poorly understood or not understood at all, the APET is too big.

P. C.74, line 22: *traced*, not *trace*.
line 43: replace *cease* by *are to be terminated*.

P. C.75, line 2: reword as *systems is similar for the plant damage states*.
lines 6,7: reword as *systems are drastically different for the plant damage states. This is generally the case for PRAs associated with low power operation and shutdown*.
lines 30,31: reword as *the discussion will deal with branches where branch probabilities are represented by distributions rather than point values*.
line 35: restate as *a truly variable likelihood of the outcomes*.

P. C.76, line 5: reword as *introduced by the extent to which structure of the APET may not be...*
line 11: *represents*, not *represent*.
lines 17-19: Completely non-understandable.
line 24: replace *having a fair amount detail* by *making use of a moderate amount of detail*.
line 25: reword as *the calculations do not include extensive detail on any one phenomenon*, (Note that phenomenon is the singular form; phenomena the plural form.)
line 26: replace *have* by *use*. *allow*, not *allowing*.
line 27: insert *over the full range* after *done*.
In any case, break up the sentence running from line 23 through line 29.
line 31: replace *the* by *these*. Replace *the codes* by *they*.

line 32: strike the first occurrence of *codes*.
lines 35-36: restate as *lack of operation of equipment*.
lines 46-47: What is the meaning of the full sentence?

P. C.77, The treatment of deterministic methods is inadequate. They are not used only in construction of APETs. Somewhere it is necessary that discussion be given to use of deterministic methods in best estimate calculations, to the use in bounding calculations, to the relation between deterministic methods and engineering, particularly of systems, and to the basis for choice between deterministic and probabilistic methods when confronted with a specific problem. This may not be the best place to give such a discussion, but the discussion here should recognize these points.
- lines 15-18: the above is especially pertinent to this section.
- line 20: *practitioner* rather than *practitioners*.
- line 23: replace *With* by *Following*
- line 29: reword as *calculations supplemented by other information*.
- line 34: What is the meaning of *as in the former*?
- lines 35-37: not understandable.
- line 39: reword as *codes usually include a substantial amount of detail*.
- line 40: insert *as much as* following *take*.

Page C.78, lines 1-2: reword as *phenomena they model; other modes*. Replace *via* by *requiring*.
- line 5: *stems* rather than *stem*.
- lines 8-11: this is a repetition.
- line 14: replace *a fair amount of* by *substantial*.
- line 15: reword as *areas. The phenomena occurring during...*
- line 18: replace *to make* by *in constructing*. Replace *known* by *understood*.
- line 20: replace *code* by *analysis*.
- line 27: insert *the* following the comma.
- line 28: reword as *and its constituent models will*
- line 31: insert *sometimes* following *will*.
- line 37: insert *for reasons of brevity* at end of line.
- line 38: delete *for brevity*.
- line 39: insert *found* following *is*.
- lines 39-40: Don't understand the sentence.
- line 43: End the sentence after *NRC*. Start new one with *However, it is*.
- line 44: reword as *The code currently used by NRC.*.
- line 45: reword as *The nuclear industry*

Page C.79, line 8: delete *or*.
- line 10: reword as *When codes and their manuals are used and compared,*
- line 11: *versions of the codes*.
- line 26: insert *listed and* after *are*.
- line 27: delete *and listed*.

Page C.81, fourth line of text in the box should say *it<u>s</u> contractors*.

Section C.5

• This section and the preceding one contain much in common. Many of the points are repeated. I wonder why the two are treated separately, since they are both parts of the Level 2 analysis, and they overlap in methodology.

• The section is reasonably well written.

Page C.84, line 44: replace *the release* by *a release*.
line 45: replace *is* by *would create*.

Page C.86, line 7: *specify*, not *specified*.
line 16: *release<u>d</u>*.
lines 28,29: reword as *the start of subsequent evacuation would..*
line 30: replace *begins* by *were to begin*.
line 31: replace *will* by *would*
line 34: replace *is* by *would be*.
line 44: delete *and*.
line 46: replace *are* by *would be*.
line 47: reword as *analysis, attention is directed to the transport..*

Page C.87, line 1: delete *is of concern*
line 2: replace *is of concern* by *are analyzed*.
line 19: replace *is* by *would be*.
line 21: replace *will* by *would*.

Page C.88, line 1:*release <u>would</u> affect*.
line 15: *perform* rather than *performed*
Section C.4 should refer ahead to Section C.5.3.1.
line 34: *model<u>led</u>*.
line 36: *detail<u>ed</u>*.

Page C.89, line 1: *computation<u>al</u>*.
line 4: insert *deterministic* following *the*.
line 8: replace *included in* by *made use of by*.
line 17: reword as *be known if the results are to be used in an...*

P. C.90, line 14: *tend<u>s</u>*
line 34: replace *to* by *of*.

P. C.91, line 46: replace *included* by *explored*.

P. C.92, line 1:....*inputs. However,....*
line 10: *in*, not *is*.
line 34: *CORRAL*, not *CORREL*.
line 40: reword as *source term<u>s</u> and consequence<u>s</u>*

suggested....

P. C.93, line 2: *progressions*
line 3: replace *together* by *into sets.*
line 21: **was**, not *is.*
line 23: **were**, not *are.*
line 29: insert *trees* following *progression.*

P. C.94, line 2: *CORRAL*, not *CORREL.*
line 9: **was**, not *is.*
line 43: *terms.*

P. C.96 Section C.5.5.1 is largely a repeat ofsections of C.4.
line 19: semicolon instead of comma.
line 33: *too*, not *to*
line 45: replace *are based on* by *analyze the.*

P. C.97, line 5: What is the meaning of "integral releases"?
line 8: *is*, not *are*
line 10: replace *is* by *would be.*
line 12: replace *are* by *would be.*
line 15: *...nature. However,....*
line 16: reword as *and therefore solves a set...*
lines 16-18: shorten the sentence to *The timing information is better than with XSOR.*
line 21: *plays*
lies 36,37: Is something missing here? If not, reword *evaluated multiple times.*
line 40: What is the meaning of "multiple model evaluations"?

P. C.98, line 2: Put a period at the end.
line 28: *suggests.*

Section C,6

- This section is also reasonably well written.

P. C.101, line 38: *result*, not *results.*

P. C.102, line 11: replace *transfer* by *translate.*
line 13: reword as *The consequences that would accompany...*
line 33: insert *might* following *that.*

P. C.103, line 1: replace *are including* by *include.*
line 28: *principal*, not *principle.*

P. C.104, line 12: *emergency*, not *emergence.*
line 18: reword as *if the containment were to rupture*
line 20: replace *is* by *would be.*
line 23: reword as *if the containment were to fail'*
line 24: replace *will* by *would.*

line 25: ditto
line 32: *overpredict*, not *over predict*.

P. C.105, line 27: comma following LD_{50}*'s*.
line 32: *suggests*

P. C.106, line 7: *ongoing*, not *on going*. Replace *are addressing* by *address*.
lines 17 and 18: replace *is* by *would be*.
line 22: replace *growing* by *becoming*.
line 24: *are*, not *is*. Replace *length* by *duration*.
line 26: insert *would* before *have*.
line 29: semicolon after the parentheses, not a comma.

P. C.107, line 6: *plume travels*, not *plumes travel*. Replace *may* by *would*.
line 8: replace *can* by *could*.

P. C.108, line 2 of text in the block: *principal*, not *principle*.
line 5: comma at the end.

P. C.109, line 3: replace *measured* by *expressed*.
line 9: a typeover.
line 11: replace *that exist* by *existing*.

P. C.110, line 1: rewrite as *calculation of the expected value of risk..*

P. C.111 The use of "chronic" in the box is not the customary meaning of the word. Is there another choice of word?

P. C.112, line 13: strike *of*.
line 18: *suggests*.

Section C.7

•This section needs much rewriting and editing.

•Introduction of the concept of risk triplets is good, but the other authors of this work seem to stick to the old definition. Somehow the overall editor of this work will have to reconcile the different usages.

P. C.115, line 22: insert *described* following *analyses*.
line 24: replace *to bring* by *bringing*.
line 29: Restate as *Risk may be prescribed as the set of...*
line 33: insert *and* following *scenarios*.
line 38: does not convey a meaning

P. C.116, line 9: *the*, not *to*.

line 14: Is "the expected values definition of risk" the quantity defined as R above? If so, the connection should be made more clear.

line 19: restate as *value of risk is an appealing concept.*

line 24: replace *expected vaalue risk estimates by estimates of the expected value of risk.*

line 21: words following *because the* do not seem to make sense.

line 25: end the sentence with *constituents.*

lines 24-27: I believe this, but the text does not say why.

lines 29 and following: This discussion is not very understandable because one would have to go to Reference C.4.1 to find out what it means. What are the methods in question?

P. C.117, line 4 in the box: replace *a* by *the.*

line 2: What is the meaning of the complete sentence?

line 10 replace *known* by *understood.*

line 13: ditto

lines 15,16: *Severity* is not the appropriate word. What is meant?

line 21: reword as *Risk estimates are commonly reported as follows:*

P. C.118, line 2 of the caption for the figure, replace *with* by *through.*

line 17: replace *Using* by *In.*

line 18: ditto

P. C.119, line 2 Of the third bullet in the box: strike *of.*

line 6: rewrite as *One method of obtaining risk estimates for a previously unanalyzed plant is through use of a surrogate PRA.*

line 9: *suggests*. Strike *that.*

P. C.120, line 4: insert *new* following *a.*

lines 4-8: This is a big step from use of a surrogate PRA. Perhaps the transition could be softened by beginning this paragraph with the phrase "Of course, another method..."

Section C.8

- This section will also need a great deal of editing and rewriting.

- As can be seen in the editing, attention is needed to avoidance of dangling participles and to making the number of the verb agree with the number of the subject of the sentence. E.g. they are, not they is.

P. C.122, lines24,25: reword as *The guidance in Sections C.3*

through C.7 provides no fixed and specific rules.
lines 25,26: reword as *rules would introduce unnecessary complication.*
lines 26,27: reword as *Several guidance principles that can be applied to use of a surrogate PRA, performance of a PRA, or review of a PRA are illustrated below:*
line 33: replace *using* by *use of*
line 34: *of*, not *if*.
line 36: replace *then using* by *use of.*
line 38: strike *performing*. strike *size of the.*
line 39: replace *such* by *formulated.*
line 41: strike *reviewing.*
line 42: reword as *are issues in determining the extent to which objectives are met.*

P. C.123, line 1: replace *using* by *use of.*
line 3: ditto
line 7: replace *performing* by *performance of.* Strike *have.*
line 23: *Use of an Existing PRA*

P. C.124, first line in the box: replace *are not* by *do not define.*
first two lines of the box: rewrite start of sentance as *One must also consider how design details...* In the same sentence, strike all after *accident progressions.*
line 4 in the box: strike *portions.* Insert *defined by* following *be.*
line 5 in the box: replace *instead* by *rather.*
line 6 in the box: replace *details* by *detailed design.*
line 7 in the box: replace *and that affect* by *which.*
line 4: rewrite as *volume and pressure capabilities.*
line 9: replace *will* by *may.*
line 12: *suggests*
line 14: rewrite as *therefore, surrogate studies are not fully satisfactory substitutes for plant...*
line 25: *updates*
lines 31,32: I do not understand the sentence.
line 34: *for*, not *of.*

P. C.125, line 2: replace *approach* by *method.*
line 3: rewrite as *same level of detail as was found in the PRA of the surrogate plant on which the ...*
lin2 6: *states.*
line 7: strike *characteristics* both times.
line 8: *will yield a plant-specific model.*
line 10: reword as *success of application of surrogate models will depend on ....*
line 11: reword as *..models match those of the plant to be analyzed, and the...*
line 12: insert *for the application* following *data.*
line 13: insert *on general methodology* follwing *such.*

line 14: insert *for particular cases* following *support*.
first line in the box: *is*, not *are*. *prioritize*.
second line in box: replace *done* by *carried through*.
first line of second paragraph in the box: strike *to*.
third line of this paragraph: *with*, not *within*.
The remainder of this paragraph is very unclear.
Last line of last paragraph in the box: insert *usually* following *are*.

P. C.126, line 1: replace *applying* by *translating*. Replace final *a* by *the*.
line 12: *operation*, not *operations*.
lines 15,16: Restructure the sentence.
line 27: replace *separate* by *its separate constituent*.
lines 32,33: reword as *had been carried through as a single complete action, and then had...*
line 33: insert *been* following *analyses*.

P. C.127, lines 15-23: These are not disadvantages, but are precautions to be observed.
line 28: *is*, not *are*.
line 37: comma after *frequency*.
Discussion starting with line 40: note that what follows concerns NUREG-1150.
line 43: strike *an*. Comma, not semicolon.

P. C.129, line 1: *latter*, not *later*.
line 13: replace *they* by *the remainder*.
line 18: insert *to* following *according*.
line 27: insert *analysis* following *2*.
line 29: *progressions*
line 35: *is*, not *are*.
line 43: replace *Having delineated* by *After*. Insert *have been delineated* following *progressions*. Delete *labeled*.
lin2 44: insert *have been labeled* following *progressions*.

P. C.130, line 3: *input*, not *inputs*.
lines 29-32: Note the departure from the risk triplet concept.

P. C.131, line 3: delete *made*.
line 6: replace *performed* by *accomplished*.
line 12: *establish*, not *established*.
line 13: delete *amounts of*.
What lesson is to be drawn from the content of the box. It is not clear.
line 18: *are*, not *is*. Replace *apply* by *be used in applying*.
line 21: insert *or before reviewing*.
lines 26,27: reword as *Either the results or the models from a PRA at an existing plant can be used for surrogate purposes to make...*
line 28: insert *being analyzed* following *plant*.

line 29: replace *making* by *based on*. Replace *accident* by *accidents analyzed*.

P. C.132, lines 1,2: delete *and..........plant*.
line 2: replace *applying* by *application of*.
line 3: reword as *the important issue to be resolved is...*
line 6: replace *performing* by *performance of*. Replace *integrating* by *integration of*.
line 9: insert *at each level* following *analyses*.
line 11: Insert *been* following *analyses*. Replace *mute* by *a moot*.
line 14: delete *Performing....PRA*. Replace *applying* by *application of*.
line 15: insert *of* following *review*.

Section C.9

- This is a well written section.

- I include editing done at the Orange County meeting, because I can't isolate other editing from my own.

P. C.133, line 42: insert *a* at the end.

P. C.134, line 11: *experts'*
line 19: *any more*
line 20: insert *better appraised and* following *be*.
line 27: delete *sources of*.
line 41: replace *may be* by *is*.
line 16: add more on motivational biases.
line 19: insert *at first* following *were*
line 32: insert *provide* following *experts*; *thus* following *viewpoints*.
line 35: replace *shared* by *interchanged*.
line 37: replace *multiple, redundant* by *several*.
line 45: *expert*, not *experts*.

P. C.136, line 10: comma following *used*.
lines 34-37: remove the sentence.

P C.138, line 18: replace *belief* by *knowledge*.
lines 22,23: *elicitations*, not *elucidations*.

P. C.141, lines 3,4: delete as self-evident.
line 34: replace *of* by *near*.
line 35: delete the last *a*.

P. C.144, State the origin of the Table.

P. C.145, Section C.9.7.2: An introduction is needed, including a definition of information.
line 36: Finish the sentence.

Delete Section C.9.7.3

P. C.146, line 14: replace ***multiple*** by ***several***.
line 18: delete ***obtaining a better***.

P. C.147, line 16: insert ***results obtained using the*** following ***the***.
lines 23,24: Why is it important?

P. C.149, lines 37,38: delete ***or even.....cases,***.
line 19 delete ***obtaining better***.

Section C.10

- This section is very well written.

P. C.157, line 42: insert ***if*** following ***because***.
line 43: insert ***of a specific kind were to occur, they*** following ***accidents***.

P. C.158, line 23: ***fault***, not ***faults***.
line 44: ***model***, not ***Model***.

P. C.161, line 10: replace ***for the....parameter*** by ***with the same input parameters***.
line 28: comma after ***model***, then insert ***a process***.

P. C.162, line 23: ***analysts*** (to make the statement similar to the preceding one)

P. C.164, line 16: delete ***of***.

P. C.165, line 1: insert ***are*** following ***observations***.

P. C.167: Note that the Propagation of Discrete Probability Distributions is closely related to the method of Latin Hypercube Sampling.

P. C.168, line 28: ***are***, not ***is***.
line 30: ditto
line 47: reword as ***There are many possible alternative displays***.

P. C.170, line 3: ***are***, not ***is***.
line 6: ditto.

P. C.178, line 15: delete the comma.
line 21: ***quantile***, not ***quantiles***.

P. C.180, line8: comma after ***new***.
line 19: comma after ***analysis***.

P. C.181, line 26: ***ensure***, not ***assure***.

line 28: replace *with* by *of*.
line 32: *on*, not *in*.

P. C.182, section C.10.8.3: It would help to mention human factors and external events as falling into the area of inadequately treated contributors to risk.

Section C.11

- This is a well written section, and I left it to be reviewed by those more versed in it than I claim to be.

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November 13, 1992
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Dear John:

EARLY REACTIONS TO NRC PRA WORKING GROUP ACTIVITIES

It occurred to me that it may be appropriate to offer some comments following the briefing and handouts that we received on October 20, 1992.

My impression is that, for the most part, the Advisory Committee on Reactor Safeguards (ACRS) wants the U.S. Nuclear Regulatory Commission (NRC) to:

- Clarify its position relative to PRA and its application to the regulatory process.
- Take a consistent position when presenting PRA technical material.
- Strengthen its capability in the PRA area.

In response, the NRC staff is developing guidance on consistent and appropriate uses of PRA within the NRC. Its approach is to examine NRC practices in using PRA, sort the PRA activities by category, develop guidance information for each category, perform case studies, and identify skills and techniques needed to overcome any limitations.

The following impressions are offered on what I have seen so far.

- The guidance material is good stuff but is slanted much more toward the textbook elements of certain parts of a PRA than toward how to use a PRA. While it is helpful to know something about the analytical content of a PRA, the emphasis in a regulatory environment should probably be on interpretation and application of the results. In this regard, the guidance effort would be greatly facilitated by including more "user group" expertise in the internal working group. Consideration should be given to tapping those users of PRA, the plant owners and operators, who have already taken major steps towards incorporating the PRA way of thinking into the plant decision-making process.

- The current breakout of topics lacks the top-down perspective that some PRAs have been so effective in providing. In particular, a great strength of a well-conceived PRA is that it is possible to start with the bottom line (for Level 3, health risk; Level 2, source term; Level 1, core melt) and unravel the results logically and systematically right down to the input data driving the risk. Along the way, it is possible to display and importance rank risk contributors in increasingly more detailed elements such as scenarios, initiating events, plant damage states, equipment, and basic causes. Consideration might be given to organizing the guidance document accordingly.

  The current breakout of Statistics, Reliability Methods, Decision Analysis, etc., runs the risk of getting issues out of context. For example, the highlighting of statistics as separate from sensitivity and uncertainty analysis presents a contextual problem. Statistics, sensitivity, uncertainty, etc. are all related to how the parameter of the risk model is defined and the form of the results. For example, some PRAs have adopted event frequency embedded in a probability distribution as the parameter of choice for describing risk. Such a definition makes uncertainty an implicit part of the fundamental risk parameter. In particular, issues such as means, medians, confidence levels, etc., become non-issues as they are explicitly and implicitly contained in the basic approach.

  The real point is the form of the results and the role that statistics, uncertainty, etc., have in those results--they are not stand alone disciplines. For example, statistics, per se, while important, enter into a PRA only in a very minor way. Most of what a PRA is all about is engineering and operations information that define event sequences that can get the plant into trouble. This is the part of a PRA that controls its quality and credibility.

  It is this top-down approach from the point of view of the parameter chosen to represent risk that would eliminate much of the confusion pointed out by the ACRS.

- The outline of the guidance document appears to omit a very important element associated with the use of PRA results: an integrated framework for defining accident sequences. A PRA can be viewed as a structured set of scenarios; i.e., accident sequences that provide an integrated picture of what can go wrong with a plant. The scenarios answer many questions about the fidelity of the PRA, including how systems interact, human/plant interfaces, interfaces with different phases of the PRA such as between Level 1 and 2, the connection between different plant accident conditions and emergency procedures, intersystem dependencies, and so on. The inability to communicate the context of specific disabling or recovery events in accident sequences may be part of the cause of the ACRS concerns.

Now, having said the above how can NRC move forward and take full advantage of the guidance material developed thus far? I believe that the crux of an approach has already been signaled. The steps might be somewhat along the following lines, with the guidance topics in capital letters:

- Establishing the Goal

  Even in the absence of a policy statement by the NRC, it should be possible to put forth a goal such that the loop can be closed for purposes of providing guidance. As it is now, the burden of how to use PRA is on members of the staff and their individual preferences and opinions. The results are inconsistencies in the use of the PRA language, lack of definition of terms and concepts, and opportunities for miscommunication and general confusion.

  There is little doubt that the fundamental goal of PRA at the NRC should be to support decision making under uncertainty. That is the strength of PRA and really what it is all about. Possible outcomes of such a goal are (1) the development of risk-based regulations, (2) the use of the PRA as a communication and monitoring vehicle on issues of safety with licensees, (3) detailed reviews to determine compliance with licensing requirements, (4) the use of the PRA as a basis for importance ranking of safety issues for specific plants, (5) assessment of the risk significance of operational incidents, (6) the use of the PRAs as a basis for safety research, and (7) all of the above or some combination thereof.

- Defining the Risk Parameter

  A clear definition of the risk parameter greatly enhances the whole issue of risk communication. Such a definition must embrace more than the usual reference to probability and consequence. Many years of PRA applications have demonstrated the value of embracing the concepts of UNCERTAINTY (by how probability is defined) and the scenario-based approach. Guidance in these areas would greatly facilitate the communication of the NRC staff with the ACRS and, for that matter, the industry.

- Structuring the Risk Model

  A delineation of the steps involved in the structuring of a risk model is critically important in putting PRA into proper context. It answers such questions as how accident sequences are developed and systems analyzed. It is the key step in developing an integrated model. It is in the context of developing the PRA model that consideration should be given to RELIABILITY ANALYSIS METHODS.

- Quantifying the Risk Model

  The quantification step involves identification and processing of all of the evidence that could in any way affect the outcome of the sequences that have been modeled. It is in this evidence-processing step where STATISTICS, Bayesian analysis, expert elicitation, common cause analysis, etc., take place. The initiating events, basic events, and other event data are input into the model in the form of probability distributions that quantify the event UNCERTAINTIES. These are then propagated through the model to quantify the risk parameter(s).

- Analyzing the Results

  This is probably the most important part of a PRA and is the most neglected. It is also the part most beneficial to the NRC and, following the NRC review for model credibility, where the regulatory effort really begins. Those PRAs where the opportunity existed to carefully examine and analyze the results have made the biggest contribution to both PRA practice and theory. It is where importance ranking can be examined and expanded, correlations with licensing requirements including technical specifications can be made, SENSITIVITY studies performed, plant modification and procedural change impacts quantified, plant-specific training practices considered, TRANSFORMATIONS OF CORE DAMAGE FREQUENCY RESULTS TO RISK RESULTS, etc.

  It is also where the real practice of DECISION ANALYSIS can take place. That is, consideration of alternative courses of action of risk management can be made in full view of the impact on risk, and with a little expansion of scope, other attributes such as cost and benefits can be examined, thus, moving into the domain of decision analysis.

  Another benefit to the NRC from extensive analysis of PRA results across many plants is the development of a knowledge base for assessing the credibility of an expanded role for PRA in the regulatory process. It is the kind of knowledge base necessary to make an efficient transition toward risk-based regulatory practices.

There are many other observations that could be made, but probably this is enough for now. The main thrust of my comments is to push for context of issues by taking full advantage of the lessons learned. I am looking forward to the draft material scheduled for December.

Very truly yours,

B. John Garrick

cc: Mark Cunningham
Eric Beckjord

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May 27, 1993
BNL-7629-PLG-07

Dr. John R. Weeks
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Dear Dr. Weeks:

PRA-ERG FIRST REPORT

I have reviewed the revised draft report, dated April 15-16, 1993, and have some additional comments, as follows:

- **SUMMARY**

The April 16, 1993, draft of the subject report has improved over previous drafts in a number of areas. This draft is better written and has incorporated important new information partly as a result of the previous review by the External Review Group. There remains some concern by this reviewer that the changes, while favorable, have been much more with respect to minor points than with major points. Thus, many of the March 5th comments carry over to this review and are highlighted below.

As before, the primary substance of the Guidance document is contained in Appendices B and C. The outline for Appendix B is essentially the same as in the previous draft. One exception is the inclusion of Section B.2.5, a one-page section that discusses regulation of medical devices. Appendix C has been revised to reflect new section numbering and the apparent deletion of the two old sections, C.8 (Application of PRA Guidance) and C.11 (Decision Analysis). A new section, C.4.9, on deterministic calculations for accident progression, has been added and is a good addition.

• **GENERAL COMMENTS**

The major points of concern are as follows:

The fundamental questions of why and how NRC should use PRA have still not been answered.

The guidance material beginning on page 22 is excellent in terms of possible applications of PRA but is not anchored to any particular policy or position statement. What is still lacking is a commitment by NRC regarding the use of PRA--a goal or policy statement from which all uses of PRA can logically flow. The feeling continues that NRC staff has the burden of coming up with its own list of PRA applications, hoping that there arises a consensus and direction for future uses. Such a shakeout process may work but appears to lack real vision and leadership (see March 5th comments, page 1, beginning with item 1).

The continued separation of PRA methods from plant knowledge in the NRC approach to staff training.

The quality and credibility of a PRA hinge on how well the models reflect how the plant works. The focus of the NRC training on methods at the exclusion of plant knowledge is a major deficiency. One approach to overcoming this problem would be to use plant-specific PRAs in the training sessions for tracing plant response to specific initiating events. In the process, the students would learn exactly what equipment and operator actions enter into specific accident sequences. This approach had been very effective in industry courses on PRA.

The difficulty NRC has in tapping industry resources for lessons learned and technology transfer on PRA work.

The current draft has made some improvement in recognizing the large knowledge base on PRA represented in industry. However, there remains a serious gap of information between government and industry that has greatly slowed the development of the risk-based safety management process. One solution would be more government-financed workshops, seminars, and just plain exchange sessions on the PRA work to date. This reviewer believes that we would be amazed by how much we both would learn from such an effort.

The feeling that the consideration of uncertainty is always something one does later.

While it is true that detailed uncertainty analysis is not always required at the screening stage, it is not true that uncertainty need not be considered. The treatment of uncertainty in risk performance parameters is an inherent requirement of the PRA thought process. Any prioritized list of issues, however preliminary, is authenticated only by some documentation of the analyst's confidence in the ranking of the individual issues.

• SPECIFIC COMMENTS

APPENDIX B

Some suggested changes have been incorporated. The importance of event sequence diagrams (ESD) and dependency tables as logic tools was noted.

The notion that it is important for the analysts to have knowledge of how plants work is briefly addressed in Appendix B.

The notion that uncertainties should always be a factor in any decision based on PRA is accepted for issue-specific analysis but is not embraced for screening and prioritization applications in the current draft.

Similarly, the notion that risk-based conclusions must be plant specific has not led to any wording changes. Much discussion remains concerning the use of generic studies for risk-based decisions; in particular, for issue screening.

The comment that the NRC does not adequately reference industry work when examples are offered appears to be still valid. However, in Section 2.3 of the main report (i.e. page 19), the Working Group states its belief that industry-sponsored PRAs are equally well cited. Ironically, Section 2 references (i.e. where this position is stated) do not refer to any industry studies.

The suggestion to provide greater guidance on QA was not heeded.

APPENDIX C

Section C.2.4 — Sources of Reliability Data

Few changes were made in this section, although an extensive rewrite was proposed.

Section C.4 — Accident Progression Analysis

This section was largely rewritten from the draft and is a major improvement. A number of our suggested changes were incorporated. The section was rewritten to downplay the discussion of individual computer codes. The notion of linking Level 1 and Level 2 event trees into a single sequence representation was not incorporated into the latest text. The guidance on uncertainties for this section was deleted. The later, more general section on uncertainties (i.e. C.6) would benefit from an example of the difficulties in quantifying and interpreting Level 2-related uncertainties.

Section C.5 — Source Term Analysis (New Section C.4.5)

Portions of the wording suggestions were incorporated. The section was rewritten to downplay the discussion of individual computer codes.

Section C.6 — Consequence Analysis (New Section C.4.6)

The treatment of consequence uncertainties was significantly scaled back. The reduction in scope of this section bypassed many of the wording suggestions made.

Section C.7 — Risk Integration (New Section C.4.7)

Many of our comments were incorporated into the current text. The section is now titled "Risk Calculation." The triplet definition of risk is adopted. The second item of the triplet is termed the probability of the scenario; however, the mathematical definition of total risk refers to accident frequencies. Both definitions should refer to accident scenario frequencies. The uncertainties in the accident scenario frequencies and consequences should be acknowledged.

Section C.8 — Application of PRA Guidance (Section Deleted)

This is probably, in part, due to the comment that the section lacked a clear purpose.

Section C.10 — Sensitivity and Uncertainty Analysis (New Section C.6)

Nearly all of the suggested wording changes were incorporated.

Please give me a call if you have questions.

Very truly yours,

B. John Garrick

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July 12, 1993
BNL-7629-PLG-10

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

Dear John:

ERG ACTION ITEMS

Enclosed are comments to the questions and text on CRACIT provided me at the Albuquerque meeting. The other action items that I have will follow soon.

Very truly yours,

B. John Garrick

Enclosure

cc: Mark Cunningham

To be Added to Table

One of the industry codes, CRACIT * (Calculation of Reactor Accident Consequences including Trajectories) was developed by PLG Inc., in the mid-1970's following release of the RSS. It addressed most of the shortcomings identified by industry representatives in the CRAC model. The MACCS code also corrected many of the early criticisms, however, there remain some basic differences between CRACIT and MACCS as follows. CRACIT is able to treat changes in wind direction as well as difficult atmospheric dispersion effects (caused by land-water interfaces and complex terrain) using a variable trajectory plume model while MACCS uses a straight-line model. CRACIT can utilize meteorological data from several locations to model more realistically long-range plume transport ~~more accurately~~. Finally, CRACIT calculates doses to evacuees that can travel along ~~more~~ realistic exit routes (variable trajectory) in a time-dependent manner. Adding these complexities tends to decrease the modeling uncertainty.~~some of the inherent uncertainties in such modelling efforts.~~

*Woodard, K., "Sensitivity of Consequence Results to More Sophisticated Atmospheric Dispersion and Other Modelling Enhancement," presented at the Workshop on Reactor Accident Offsite Consequence Modelling Assessment and Application, Tokai, Japan, April 1984.

Questions

Last sentence in text is unclear. What is meant by uncertainties?

Should *accurately* be *realistically*?

Regarding health effects:

- Was BEIR V information used?
- Was ICRP 26 (dose conversion factors) and 60 (for additional organs used?
- Were health effect models based on NUREG/CR-4214? What version?

Suggested Addition to Text

Appropriateness of the trajectory model will depend on the characteristics of the subject site, the problem (integral versus point effects), and the availability of data. A paucity of data may limit the benefits. With the trajectory model, more calculations are needed to converge on the final results. ~~Need to determine~~ The appropriateness of a particular code depends on ~~to~~ the specific problem being analyzed.

Regarding the Questions on Health Effects

CRACIT uses the Reactor Safety Study health effects models. This was intentional to provide a basis for comparing Level 3 results with WASH-1400 and with other Level 3 studies PLG was performing in the late 70's and early 80's. These were the only major Level 3 studies performed and preceded the release of such health effects information as NUREG/CR-4214, KRP 26 and 60, and BIER V. Studies indicate that differences in health effects models do not have a significant impact on results compared with the effects of variations in other parameters.

Comments on the April 16, 1993 Draft Report
"A Review of NRC Staff Uses of Probabilistic Risk Assessment"
By
Ralph L. Keeney
May 1993

The comments that follow pertain to the main body of the report and to Chapters 5 and 6 of Appendix C. In each case, I present a few general comments followed by detailed comments.

Before proceeding, I would like to mention four general comments:

1. The previous draft of this report had a chapter in Appendix C titled "Decision Analysis." What happened to that chapter? I felt that chapter was important to the topic of this report and should be included. At a minimum, there should be some explanation about why a large body of relevant material was deleted from this version of the report.

2. There are parts of this report that could not possibly have been proofread by anyone. When one finds two or three sentences repeated in two or three places, equations in the wrong place, and incorrect words (that were surely corrected by the authors from the previous draft) not included in this draft, one is somewhat suspicious of the care given to the entire manuscript.

3. Partly due to the concern raised in point 2, I would very strongly recommend that a technical editor review the entire manuscript at some stage after there is agreement on the content of the technical material and acceptance of the quality with which it is written.

4. On the previous draft, lines on the page were numbered, which greatly facilitated references for reviewers. If there is another draft version, I would certainly welcome that lines again be placed on the manuscript, as they were not on this version.

Comments on the Main Report

General Comments

1. Most of the inferences drawn in this report are based on a sample of 80 individuals at the NRC who responded to a survey. Little information is provided about the survey (what was asked, how it was asked, who all received the survey, what the response rate was) so it is difficult to appraise the implications.

2. This version does a better job motivating the use of probabilistic risk assessment than the previous version. However, I still believe that it should be substantially improved if one expects that people within the NRC would read this document any further than the first couple of pages.

3. Somewhere in the first chapter, there should be a clear response to the specific suggestions in the ACRS letter.

4. The document continually stresses improving the ability of the NRC staff to use PRA well. Perhaps the problem is that the term "use" is never defined. From context, it often seems as if one expects people on the NRC staff to conduct analyses using PRA. I do not think that will occur in the cases where the staff was not well-trained in universities or elsewhere to conduct PRA. Furthermore, I do not think that should be the intention of the NRC. The intention should be to make the staff members of the NRC responsible and sophisticated consumers of analyses that use PRA. In other words, they should know how to appraise PRA studies and decide what was done, why it was done, and what the implications are for doing the job of the NRC as well as they can.

5. It is essential for any PRA that the problem be carefully structured. This is an exercise based on logic, understanding the technical aspects of whatever is to be analyzed, and knowing what one hopes to achieve by the PRA to follow. Careful structuring provides the foundation for

any PRA and it should be discussed in this report. It currently is not considered.

Detailed Comments

p1: Define PRA somewhere early in this document.

p1: What is prevention and mitigation of core damage accidents "balanced" against?

p1: The document refers to risk benefits; what are these?

p4: (middle) Staff use of PRA rather than appraisal mentioned here (see General Comment 4).

p4: Here it says future uses that are not well defined are not included. On the other hand, anticipated uses are referred to on page 5. It would be nice to have a consistent terminology and to provide insight about future uses, especially when PRA should be used in such areas now.

p5: On your second and third bullets, staff uses are again stressed, rather than the ability to appraise analyses.

p5: No information is given about how the working group compiled the set of anticipated uses.

p6: Regarding task 2, one cannot simply review the practices that are currently being followed to identify limitations of present practices. Many limitations may refer to what is not present staff practice.

p8: Regarding external review, the expertise of your review group includes decision analysis which, as I suggested on the previous draft, would be a better phrase than decision theory.

p16: On the top, you cannot address anticipated PRA uses by referring to existing NRC staff activities only.

p17: There is a category in the table called Regulatory Action; isn't almost all of this stuff regulatory action?

p18: In the first bullet, performance assessment methods are risk analyses-they are not similar to risk analyses. Also, you now use the term risk analyses rather than risk assessment. Is this intentional?

p18: It certainly is not clear what it means to say that many staff had limited experience and familiarity with PRA techniques. It would be useful to be more specific.

p18: The use of the term "best estimate" at the bottom of the page is not clear. If it is to be used, please define it. However, this report says to avoid use of the term best estimate (see page C.167, bottom line).

p19: At the top is a reference to decision criteria. Such references occur again on pages 21, 22, 25, and 34 as well as probably other places. Given the important role that they seem to play in this introductory document, it would be nice to define what is meant by decision criteria.

p19: For needed improvements, it is crucial to mention that staff should learn about how PRA analyses should be structured and also about how to appraise PRA analyses.

p22: In the last bullet, there is a problem with English because it says the decision criteria should be similar to the guidance provided. Could it mean that the decision criteria should be developed in manners consistent with the guidance provided somewhere, or what does it really mean?

p25: In two of the top bullets, risk analyses and analyses are referred to rather than risk assessment. Was this the intention?

p27: It is useful to note that priorities should not be set according to years when the work will be completed. Rather, priorities should be set by the importance of the work. Then one should take this into account, as well as the cost and time needed to complete the work, in deciding when it should be completed. The logic of this beginning to Section 3.4 is weak.

p30: The footnote on this page is essentially a repeat of the footnote to the table on the following page. Do we need this redundancy?

p34: In referring to the skills that one would want for recruitment of staff, the list does not directly pertain to risk management. The terms risk analysis and decision analysis should appear along with probability, statistics, and reliability methods.

p36: I think simplified analysis methods for the staff are a bad idea if they are meant to be a substitute for understanding sensitivity and uncertainty analyses conducted by professionals familiar with PRA. It is not that the staff need be able to conduct such analyses. It is that the staff should be able to appraise whether those analyses make sense and what the implications of those analyses are. They should also be able to appraise which analyses should be done that were not done.

p36: For Section 4.6, again, priorities are not set according to years.

Comments on Chapter 5, Appendix C - "Expert Judgment"

General Comments

1. There are improvements in this chapter over the previous version. However, I still think it would be very important to set out the context of the chapter early in more detail. It should make clear what it is that the authors hope readers will learn from the chapter.

2. In clarifying the context of expert judgment, it should be made very clear that expert judgment is always used. The issue is only about how it is used, whose judgment is used, and whether this is done explicitly or not.

3. I also think there should be more discussion about the advantages and disadvantages of the explicit procedure to specify expert judgments. This is mentioned a bit more in this version at the end of the chapter. However, given the misunderstandings of many people about the appropriate role of the explicit use of expert judgment, it would be useful to cover this topic using at least one page.

4. It is apparent that many changes made by the authors were not included in this version of the document. Somebody simply did not proofread carefully. For example, the second-to-last sentence on page C.137 is essentially equivalent to the first sentence on page C.138. Other examples are included below.

Specific Comments

pC.131: Little attention, except for a sub-subsection (C.5.3.2), is given to the alternative forms of organizing experts mentioned as a key point for understanding. Also, "organizing experts" sounds awkward. Isn't it more organizing assessments to gain as much information as possible from a group of experts.

pC.131: In the middle of the page, within a parentheses, there is a reference to page C.9 and page C.9.

pC.131: On the following lines, flip the order of the two sentences. Also, is there really a continuum of expert judgment techniques?

pC.132: Expert is missing a p in mid-page.

pC.132: The motivation for the formal process should be re-thought and reorganized. The first point listed implies the other ones. Issues being complex and interdisciplinary are different, and either could be a basis for a formal process.

pC.132: Expert judgment is not necessarily a summary of a person's knowledge; it is perhaps a specific type of summary of an expert's knowledge about a particular topic or issue.

pC.132: For such a strong statement that the human mind is the best available mechanism for evaluating, weighting, and combining information, some references are required. In fact, for combining a great deal of numerical information, a computer program (of course, written by someone with a mind) may be much better to evaluate and combine the information.

pC.132: Near the bottom, another major plus to formal assessments is that the judgments are made explicit for appraisal.

pC.133: There is another odd reference on about the eighth line to C.5.C.5.3.

pC.133: Before one selects experts, I think it is very important to have some idea about the issues which will be addressed. Otherwise, how can you ensure that the experts know anything about the issues? This general point should be brought up in the chapter. This concern is why I made some revisions to the steps carried out in the assessment in NUREG 1150 for assessing expert judgments in the future. This change was to select issues first and then experts. The experts would then help refine the issues to ensure that they were appropriately stated. This revision is included in the report by Bonano et al., and is included in a paper by myself and Detlof von Winterfeldt ("Eliciting Probabilities from Experts in Complex Technical Problems," <u>IEEE Transactions on Engineering Management</u>, <u>Volume 38</u>, 1991, pp. 191-201). This paper is not referenced in the chapter and I think it would be appropriate to include it as it is much more accessible than the Bonano et al. report.

pC.133: Explain to whom the testimony was given at the bottom of the page.

pC.134: Regarding organizing experts, it seems that it is the desirability of various approaches that varies with respect to the scope of issues, etc. (see comment for pC.131).

pC.134: The isolation of experts in terms of where they work is not equivalent to isolating them from each other's information. One could hand each expert all the same information and yet require them to do their work in isolation.

pC.135: Not all important issues can be identified through sensitivity and uncertainty analyses. In order for anything to show up in sensitivity analyses, it must be first included in the analyses. This concerns the qualitative structuring of analyses that is so crucial.

pC.135: Define observable quantity.

pC.135: A few lines later, define x as it is used in this paragraph.

pC.135: Containment failure pressure and location sounds like two issues, one for pressure and one for location.

pC.135: The discussion in the last paragraph and the first full paragraph on page C.136 is not clear as it could be.

pC.136: Indicate what the X'S and the bars on the table indicate.

pC.136: Why refer to "subjective" probabilities in the first full paragraph? Is there another type of probability?

pC.137: You list four objectives so it should say the fundamental "objective<u>s</u>." The first of these should be to help the experts be able to express their beliefs--training is not to help them express their beliefs.

pC.137: Your last objective is essentially repeated two sentences following the list of objectives.

pC.137: On the next two lines, you refer to elucidations rather than elicitations.

pC.137: About seven lines from the bottom, thus and us should be this and is.

pC.137: In the italics and two lines above, you repeat the same sentence.

pC.137: On a line in between these, you repeat a sentence that is in italics at the top of page C.138.

pC.138: On about line 10, how do you know the specialist will inform the expert of incompatibilities? The sentence should say the specialist should inform the expert about incompatibilities.

pC.138: To state that decomposition leads to better quality probability distributions is not as complete as it should be. First, the decomposition needs to be done in a reasonable manner (which usually means along lines of knowledge, sometimes in different disciplines). Second, in order to compare probability distributions, the recombination of the decomposed assessments is necessary. This is not stated here.

pC.138: You cannot always get a consensus, yet the text makes it sound like one can choose to have one.

pC.138: Using a single decomposition does not necessarily force experts to take a single view of the issue. Each of them may take this view without being forced.

pC.139: In the second paragraph, if it is important to extrapolate beyond a given range, this chapter should give some insight about how to do it.

pC.139: The first paragraph under Biases is not clearly written. Also, these are biases in probability assessments, not in probability formation. They may also be biases in probability formation but that is not what has been examined.

pC.140: It would clarify the first sentence in Section C.5.5 to state quality of a single (or point) forecast.

pC.140: In the section on evaluation, in the last sentence one clearly needs a set of outcomes but not a set of probabilities to judge goodness. If you judge the probability of heads on a bent coin is 0.4, you may need a number of flips to decide whether your assessment was good or not. However, you do not have a set of probabilities.

pC.140: The 45-degree line reference on the bottom of the page is not a very good way to make the point. This assumes that the axes between 0 and 100 percent have the same length, for instance. Just refer to the plot of probabilities against observed frequencies and that they should match.

pC.141: Put more information on the table and its title so that one can understand where the data is from and what the reader is meant to understand from the figure without reading the text.

pC.144: The first two paragraphs are the same.

pC.144: On paragraph 3, calculation is typed and it should be calibration.

pC.144: In combining expert judgments, it is not necessarily that the multiple experts are "responding to the same questions" but providing information about the same variables.

pC.144: The sentence in italics makes a strong claim and should have support and references.

pC.144: The second-to-last paragraph says the behavioral approach allows experts to converge to a consensus distribution. This may not occur and there may not be a consensus.

pC.145: In the third paragraph, the use of consensus is again awkward here.

pC.145: In the middle paragraph on mathematical aggregation rules, the last sentence says that averaging values can lead to values of the variable that are not physically possible or believed to be possible by all the experts. Although I certainly agree that one should average probabilities rather than values, averaging probabilities can also lead to values that are not physically possible or believed to be possible by all the experts. I think you want to average probabilities to maintain the range of the distribution represented by all of the experts.

pC.146: It might be mentioned that one should choose experts so that they are all very competent. This selection property is one which suggests that equal weights may be reasonable.

pC.146: Define the notation used on the equation in the middle of the page.

pC.146: On the bottom of the page, you are missing an equation which then appears out of place on the top of the next page.

pC.146: The sentence that is on the third and fourth lines from the bottom is awkward with its two references to median.

pC.147: Another odd reference in the middle to C.5.C.5.3.

pC.147: The second- and third-to-last paragraphs do not use identical words but essentially state the same point.

pC.148: The first non-italic sentence needs work.

pC.148: The discussion of advantages and disadvantages should set the context where expert judgments should be used in an explicit manner. Clearly, we should not use expert judgment in an explicit manner if there is a great deal of information available. It is only a candidate for use when circumstances exist that you mentioned earlier in the chapter. The advantages and disadvantages should be discussed for these circumstances.

pC.148: Regarding disadvantages, it should be made very clear that the costs of not doing a formal expert assessment can also be substantial, both financially and in terms of time. An example where this occurred was in the first exercise with NUREG 1150. In fact, this experience lead to Steve Hora being asked to design a much more reasonable process, which he did and which was carried out.

pC.149: At the top of the page, one common error that should be avoided by any NRC applications is to assume that anybody can do the expert elicitations. There are a lot of skills required in these assessments and the assistance of specialists (experts in assessment) should be utilized.

pC.149: The possibility of repeating an analysis due to an unfavorable peer review is important. However, it is even more important when the analysis must be repeated because it was not done well the first time. This point should be mentioned.

pC.149: In the third line from the bottom, omit the sentence that has "faithfulness of probabilities" in it.

pC.150: I would not call the Delphi method a behavioral method. If anything, it is a mathematical combination, because it takes estimates from a number of people and mathematically aggregates them, usually by reporting the median.

Comments on Chapter 6, Appendix C - "Uncertainty and Sensitivity Analysis"

General Comments

1. This version is improved over the previous version. However, I still strongly believe that a simple example at the beginning of the chapter to set the stage for what follows would be extremely useful. With such an example, both uncertainty analysis and sensitivity analysis could be illustrated. See my comments on the previous version.

2. Many assertions are made in the chapter regarding the display of information about uncertainties, but there is no reasoning given for the assertions. It would be nice to present the judgments on which such assertions are based.

3. It is not clear what the comments in italics are meant to communicate. Some of them seem profound and others seem rather insignificant. As a collection, it would be worthwhile to carefully review them and make appropriate changes to communicate what is intended.

Specific Comments

pC.155: The opening statement in Section 6.2 that says estimates are "imprecise" due to "imprecision" seems awkward. Later in the same paragraph, the sentence refers to "an results," which requires some change.

pC.155: The last paragraph might usefully become the first paragraph of the chapter if general comment #1 is not followed. Somehow, it would be useful to describe what is to be talked about in the chapter at the very beginning.

pC.156: The term "issues" at the top of the page has a different meaning in Chapter 5.

pC.156: In the first full paragraphs, delete "and other imprecise" after vague. The statement itself is imprecise.

pC.156: Regarding the first arrow, "The uncertainty is stochastic because a large number of accidents would not all be expected to follow the same event tree path," seems awkward. This point is true for only two similar accidents, isn't it?

pC.157: The figure that follows certainly provides very little insight to facilitate the discussion. It does not make clear the two types of uncertainties referred to. The last paragraph of this page refers to uncertainty in basic assumptions and the word "assumptions" is not used on the figure, but yet referred to.

pC.158: The figure could tremendously benefit from additional thought about what is hoped to be communicated. Once that is clear, reorganization of the figure could be useful. A better title would be helpful also.

pC.159: The word "subjective" seems strange because the choice of a PRA model is always at the discretion of a PRA analyst.

pC.159: In the second paragraph there is a question mark and, again, the use of the term "subjective." Later in that paragraph, it says that the basic shape of a distribution must be assumed with the Bayesian approach. I do not think this is correct, as one could draw out any distribution and use a Bayesian approach. It may be simpler to do the calculations if a specific distribution is assumed, but that is not what the text says.

pC.160: In the third line, the use of a "correct" model is not the appropriate way to state things. What is a correct model? Shouldn't the analyst be thinking about which ones are more likely to provide useful insight?

pC.160: The bottom of the first paragraph refers to Section C.8 and should probably say C.5 in this version. In the next sentence, shouldn't sensitivity analyses always be used, and not just when model uncertainties cannot be treated using one of the three methods described above?

pC.160: Why isn't a "more complete model" simply a different model and therefore completeness uncertainty would just be a part of model uncertainty. A better discussion to clarify this, if you wish to keep them separate, would be useful.

pC.161: I would say one more very important point for uncertainty analysis is to clarify what is not done by the analysis, which is perhaps to be very clear about what is not in the scope of the uncertainty analysis. This is different than saying what is in the scope of the uncertainty analysis only, as others may not be able to figure out what is not there from what is there.

pC.161: Change the reference to Section C.9.4 near the bottom of the page, as this is from an earlier draft.

pC.162: In the sixth line from the bottom, add the word "lowest" to read "the next lowest estimate."

pC.163: Don't you use accuracy in the paragraph to be the same as precision in the second of the three points? If not, the distinction would be important to clarify. If the two words are synonyms, this might be clarified.

pC.163, pC.164: It is not clear what the advantages and disadvantages are measured relative to. Are the costs of simulation high relative to other approaches, or relative to the mythical "zero" cost?

pC.164: Don't imply that Figure 6.2 is from an LHS analysis when you have already said it is from an SRS analysis.

pC.164: Do you mean precision or accuracy for the first advantage at the bottom of the page?

pC.164: In the second advantage, do you want to use the word "nonrepresentative"? Each run through a model using SRS is a representative run. Perhaps what you need to do is define representative and nonrepresentative.

pC.164: In the disadvantage at the end of the page, you should add "to achieve the same precision" at the end of the sentence, or some similar set of words.

pC.165: Twice within three sentences you say almost the identical thing when you say that Latin hypercube sampling is the method of choice for PRA.

pC.166: In the middle paragraph, n need not be the same for each distribution. You may wish to point that out.

pC.166: Reference 6.12 in the last line is not correct.

pC.167: Reference 6.17 in the last line of the first paragraph is incorrect.

pC.167: Decisionmakers is sometimes spelled with a hyphen and sometimes without a hyphen on this page.

pC.167: The reference to Section C.9.5 near the middle is not correct.

pC.167: Give some reasons for saying that "displays that represent separation of different types of uncertainty and subtle probabilistic concepts should be avoided."

pC.167: The discussion of "best estimate" at the bottom of the page could be improved. You say avoid the term best estimate, but then say that it should be carefully defined when it is used. I would rather see the point made that it just should not be used. You may then give alternatives, such as the mean, the average, the median, and suggest that analysts should not give in to demands that often lead to misinterpretation and misrepresentation of the results of an analysis.

pC.168: In the second line under the italics in the middle, change "are being" to "should be."

pC.168: In the same paragraph, the discussion of the uncertainty distribution constructed from means is not particularly clear. For instance, what do you mean by "overall uncertainty"? And what do you mean by characterized as a distribution function "about the mean value" of the output variable? Isn't the mean always within the distribution?

pC.168: Fourth line from the bottom should eliminate "a."

pC.169: Give the reasoning for the assertions about effective communication in italics.

pC.169: To state the risk curves are not CCDFs because they are not bounded between zero and one is not the logical way to do it. It is certainly not the case that anything bounded between zero and one is a CCDF. Give a better reason for the fact that risk curves are not CCDFs.

pC.170: Give the reasoning for your assertion on communication in italics.

pC.170: On the figure, label the x-axis. Give an example with the mean and another example with the median displayed as referred to in the paragraph above the figure.

pC.171: Regarding the last sentence, if each interval had the sample numbers, the sample size could be calculated. As it now stands, the sample size is not listed on the figure.

pC.172: On the eighth line of text, do you mean to add the term "parameters of the" to read "represent the uncertainty in the parameters of the distribution"?

pC.172: Label the x-axis in the figure as hours, or whatever is correct.

pC.173: In the first paragraph, say something about how you "process" all of the curves.

pC.173: I do not see why the meaning should depend on the "purpose" of the analysis in the first paragraph. See my comments on the previous version on this.

pC.173: Give some basis for the assertion in parentheses.

pC.175: In line three, you have a question mark.

pC.175: In the first sentence in the section on "Risk Reduction," if one of the input variables is temperature, would you really set it equal to zero?

pC.175: Similarly, in the second sentence in the section on "Risk Increase," do you set all input variables to one? What if the input variables are temperature or a failure rate?

pC.175: The footnote on the bottom refers to Risk Integration, which is now renamed Risk Calculation.

pC.176: There is a question mark on a reference in the middle of the page.

pC.176: The section on other sensitivity methods does not clearly tie in with the topics of this chapter very well. It would be worthwhile to correct this.

pC.177: Concerning regression, the notation $x_1, x_2, \ldots$ is first multiple variables, then they are multiple observations, and then they are again multiple variables, all on the same page.

pC.177: The apostrophes on the page should likely be primes.

pC.177: The statement that "least squares" assures predicted values are "as close as possible to the observed values" just past the middle of the page is not correct. You would be as close as possible if you used the absolute distance between regression curve and the observed values.

pC.178: At the top, linear values are said to be easier to manage. Easier than what?

pC.178: The discussion on rank regression seems to implicitly assume that data are scaler valued. Perhaps this should be said.

pC.178: To say that regression analysis is only a tool in the third paragraph seems odd, as all of these models are only tools.

pC.178: There are question marks on references at the bottom of two paragraphs.

pC.178: In the last paragraph prior to Section 6.6, one might say that interactions among variables are identified many times through the knowledge of people familiar with the power plants being analyzed. In other words, it is not necessarily the knowledge of the PRA analyst that is important, but rather the knowledge of technical specialists and the interaction between the PRA analyst and the technical specialist.

pC.179: In the first comment in italics, clarify what you mean by "carefully selected."

pC.179: In the paragraph in the middle, what is "integral analysis"?

pC.180: The three items in parentheses could also be stated much more clearly and completely. Reading them alone, none of them are particularly clear. In addition, the third one about the "only legitimate use" seems incorrect. If two power plants are very similar, it would seem that some of the analyses could carry from one plant to another.

pC.180: Fourth line from the bottom, change screen to screening.

COMMENTS ON

PRA WORKING GROUP REPORT

Draft April 16 1993

A REVIEW OF NRC STAFF USES OF

PROBABILISTIC RISK ASSESSMENT

Bernard Harris

29 May 1993

General Comments. The document under review is a revision of the draft report dated 8 February 1993. The revisions have been made following discussions with the External Review Group, which were held at Newport Beach, CA at the offices of PLG, Inc. on February 17 and 18, 1993. Subsequently, in March 1993, the External Review Group submitted their comments to the PRA Working Group in a written report. Overall, the report, as before, conveys the feeling that the NRC staff is not very adept in the application of PRA méthodology 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 compartisons between the various results obtained using different software packlages 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 revision, I did not observe substantial changes from the previous version. As before, most of my specific comments will refer to the sections on probability and statistics and uncertainty.

Detailed comments follow. Some of these are correction of typos. Some others are of relatively minor consequence and may be regarded as "nitpicking". Also, some are queries and some are suggestions for possible future investigations which may improve the quality of PRA studies.

Specific Comments.

p. C.1. Two typos. 1st line two superfluous n's. Middle of page, C.11, incorrect reference; clearly C.1.1 is intended.

p. C.6. I have a small objection to referring to an experiment with only two outcomes as the simplest case. Clearly, the simplest case is an experiment with only one outcome. I feel that one should say "In the simplest non-trivial case .....".

p. C.7. top. The statement " In order to mathematically consider .... a number with each outcome must be ..... " is not accurate. Examples where this is not the case - vector valued outcomes of experiments, experiments whose outcome is a sequence, experiments whose outcome is a function (such as Brownian motion), or even "qualitative" outcomes, such as a drawing from an urn which contains green, red, or yellow balls.

p. C.7. 2nd paragraph. Delete interesting - you can also manipulate "dull" sets. Also , the statement that sample spaces are sets is correct, but somewhat awkward. The manipulations referred to in the sentence concern subsets of the sample space. I feel that it would be preferable to begin by defining an event as in the next line.

p. C.7. I will go along with "two basic" if necessary. However, there are "many" interpretations of probability. Perhaps one should say, "Of the many interpretations of the notion of probability, we will utilize two, the classical and the subjectivist interpretations."

p. C.9. P(all possible events) has the possible interpretation that "every event" has probability one. Why not say "P(S)=1." The grammar in the next line is incorrect. Revise to "Mutually exclusive events are ....." or "A collection of events is said to be mutually exclusive if ......".

p. C.9. I disagree with the definition of the frequentist approach to probability--- P(A) can be either unknown or known- for example, in tossing a coin, it is assumed to be known and equal to 1/2. The frequentist approach assumes that it is a "single fixed number."

p. C.9. End of third paragraph. Some assumptions are needed for Bernoulli's theorem (cited here) to hold. Since simplicity is needed, one should have the experiment repeated under "identical conditions" (or some similar statement). Also in the limit and as n becomes large are redundant. Delete as n becomes large, or change the statement to "is arbitrarily close to P(A) for sufficiently large n.

p. C.9. next to last line - delete thorough (or possibly replace by "comprehensive").

p. C.10. Delete "explicitly".

p. C.10. The usage of "frequency" is confusing to the reader. Throughout much of the document, frequency is employed as a rate, such as the intensity of Poisson process. On the other hand, the "relative frequency" of some event is the proportion of times that the event has occurred in some experiment and therefore a random variable. In this latter case, it can not be unknown. I presume that you mean frequency in the "first sense", which is not the usual textbook definition in "Statistics". In this case it is a "parameter" and could have a prior distribution for Bayesians.

p. C.11. The inequality - probability of the union of n events does not exceed the sum of the probabilities is always true. However, the difference between the left and right hand sides of this inequality can be very large. As an extreme case, let the n events be identical. Then P(A) <= nP(A) and the right hand side does not approximate the left hand side, regardless of the value of P(A), unless P(A)=0.

p. C.11. Delete last three line of this paragraph. The inequality above is "interesting" only when the n events are not mutually exclusive. Perhaps the Bonferroni bounds are what is intended here.

p. C.12. First complete paragraph. This also fails as a "rare event approximation" without some supplementary conditions, which can easily be obtained.

p. C. 12. add "random variables" after "Poisson".

p. C. 12. change to " area between the graph of the function and the abscissa". (There is also the possibility of a singular continuous distribution - I think that the mention of such should be avoided, but as the paragraph stands, it is not completely correct). One way to make it correct is to define a probability density function and then assert that for every probability density function, there is a continuous random variable.

p. C. 13. If x denotes a value ...... then the value o' the density function at x is denoted by f(x). (Why "often"?). Also, after beta, add the words "random variables".

p C. 13. Replace "almost all" by "many". In probability theory, almost all has a specific technical meaning.

p. C. 13. If X takes on only positive values, ... , the limits of integration are 0 to x. In a sense, this is unnecessary, since you can always take the limits of integration to be -(infinity) to x. (This is a very minor technical point).

p. C. 13. In Figure C.2.2, delete vertical lines in discrete cumulative distribution function (or perhaps replace by dotted or dashed lines).

p. C. 14. 2nd paragraph. Replace "do exist in practice" by "can occur in practice" (or delete "in practice").

p. C. 14. Presumably "contrbutions" is a typo and "combinations" is intended.

p. C. 14. Third paragraph, denote mean by "mu", since this is what is used consistently in the document.

p. C. 14. Fourth paragraph, sentence construction is awkward. Delete then and interchange phrases, getting "and the "integral"g(x)f(x)dx, when x is continuous."

p. C. 14. Fifth paragraph. The discussion of conditionally independent top events does not seem to relate easily to the statement that the expected value of the product is the product of the expected values. I feel that, as written, this is likely to be confusing to the reader.

p. C. 15. Replace "the" by "a" (end of first paragraph). "The" implies uniqueness of the "mode".

p. C. 15. next paragraph, delete "just".

p. C. 15. The definition of quantile is needlessly complicated, since you have postulated a continuous distribution. It suffices to define the pth quantile as a solution of F(u)=p. (It might possibly not be unique, with this definition, but will have all the needed mathematical properties). The definition given on page C.15 is not completely consistent with the definition of the median on the preceding page.

p. C. 16. Beginning -- write Parametric statistical inference. Note that in the next paragraph, you contradict the sentence as it is presently constituted by introducing non-parametric inference. Or possibly begin by stating that two forms of statistical inference will be discussed, namely, parametric and nonparametric inference.

p. C. 16, line 8 , add "being" at end of line.

p. C. 16. First sentence, third paragraph (beginning with "Two basic") is superfluous and can be deleted in view of the sequel.

p. C. 16. Third paragraph, delete "common".

p. C. 16."The range .... estimator" is awkward given the definition of estimator in the first paragraph on C. 16. Perhaps this should be modified to read "the range can also be used to describe the spread of a distribution". (Note that for many distributions being treated in this manuscript, the spread is infinite.)

p. C. 17. I have some question as to whether one should consider tolerance intervals and predictikon intervals to be intervals which contain a "parameter" - see first paragraph this page.

p. C. 18. "untrue" is improperly used. A statement is either true or untrue. It suffices to say that the operating characteristic is a function of the parameter.

p. C. 18. Minor grammatical problem. While pump is operating, there can be no failure time observed.

p. C. 18. Confidence or significance levels are chosen to reflect the risk you are willing to assume and do not necessarily depend on knowledge previously gained about a parameter.

p. C. 18. Change " The data speak for themselves" to something that says "The results depend only on the data".

p. C. 19. In classical estimation, it is possible to model information about the parameter, which is externsl to the data. The techniques for doing this are more complicated than the Bayesian techniques, however.

p. C. 19. One can propagate confidence intervals through fault trees or event trees. Again the process is more complicated than the corresponding Bayesian procedures.

p. C. 21. Whether Bayesian methods produce better parameter estimates or not, depends on the prior. As written, there is an implication that Bayesian methods produce "better" estimates in general.

p. C. 21. If sample data are scant, then Bayesian methods yield answers. That much is certainly true. However, the answers are substantially dependent on the prior and highly "uncertain" and therefore "dangerous" to use.

p. C. 21. I take some exception to the last two statements on this page. First, you can model classical methods to use generic data. Second if a Bayesian estimator can be found, then a classical estimator exists as well. All you have to do, is to take a Bayesian estimator, determine its sampling distribution, and then use this in a frequentist manner.

p. C. 22. Bayesian methods have a "very long" history. The statement of less historical precedent is misleading.

p. C. 23. Section C.2.3. Change to "which can have potentially ...... (potentially is apparently misplaced in the sentence order and it is not clear whether it is modifying can or undesirable.

p. C. 25. Whether the mean or median is the more desirable depends on the usage to which the estimator will be put and the loss function. I do not regard this as "controversial".

p. C. 25. Third paragraph. It is also possible to model the denominator, by a Poisson process, for example. The problem becomes analogous to a "pure birth process" in such a case and one can readily obtain point and interval estimates.

p. C. 26. The natural way of treating lognormal data is to take logs, use the normal distribution analysis and transform back.

p. C. 26. Some caution is needed with the rare event approximation. See earlier comments.

p. C. 29. C.2.4.1- Does EPRI also have suitable data bases?

p. C. 29. I have always had a great mistrust of LER's as a reliable data base. Is this mistrust justified?

p. C. 35. Section C.3.1 is excellent.

p. C. 35. The conditional probability is approximately lambda(t)delta(t) for small delta(t). i.e. d and delta are "switched" and lambda(t)dt is not necessarily a probability.

p. C. 36. Failures occur with increasing probability is not quite correct, increasing frequency would be better; or perhaps, the probability of a failure in (t, t+u), u>0, is an increasing function of t.

p. C. 36. "small values of lambda*t" is logically correct, but since lambda is a positive constant, it might be preferable to replace this by small values of t.

p. C. 36. The use of MTTF is described as "before a failure". In the exponential case, this is also the mean time between failures. Should this be noted? In particular, this is a renewal process in this case.

p. C. 37. Same comment about lambda*t as before.

p. C. 37. "known not to be constant" is not quite accurate, since the Weibull family of distributions includes the exponential distributions. Perhaps, one should say, "For cases in which one is not willing to assume a constant failure rate".

p. C. 37. C. 3.2.2., "discrete" seems superfluous.

p. C. 37. C.3.2.2., second paragraph. Minimize is not accurate. For example, one can always add more components, making the consequences smaller and thus there is no minimum. I would suggest replaciong "minimize" by "reduce".

p. C. 37. end of page. n-out- of-n is a k-out-of-n system and not redundant.

p. C. 38. Hot standby - the standby component failure rate need not be smaller than the operating component failure rate. In particular this can occur when one uses standbyu components of "poor quality". Also, it seems to me that in cold standby, failures can occur, while in stadby, but they will not be detected until the standby component is placed "on line". That is, I regard hot standby as having the standby component operating but not "on line" until switched in, and in cold standby, the standby component is not operating, and therefore, if it is defective, it will not be known until placed "on line". Also, I believe that one can have stochastic independence in both cases. (Presumably, it will depend on the specific events being considered).

p. C. 38. Parallel- the function is accomplished even if all but one of the individual components fail. The reliability of the standby components need not depend on the reliabilities of the other components. However, the system reliability does depend on the reliabilities of the other components and the standby components.

p. C. 39. I have a question about the two models. I feel intuitively that they are equivalent.

p. C. 39. I have the impression that indu:tive and deductive have been switched.

p. C. 41. Notational problem , line 9. As written, the impression is conveyed that the number of events is the same (namely n) in each cut set. It would be better to write nj.

p. C. 44. The technique given at the top of the page is that of Markov chains. Should a reference to such be cited?

p. C. 44. The term expected value is not being used in the usual way. What is being described is the expected value of the consequence?

p. C. 44. C.3.2.4. The definition of dependence is not quite rigorous. It may be defined by sayuing that the conditional probability of an event, given the occurrence of another event is not equal to the unconditional (or marginal) probability of the first event.

p. C. 45. Typo-dependencies.

p. C. 45. Perhaps one should note that in a frequentist analysis, one would pool the data in estimating Q and then q2 would be correct.

p. C. 51. The italicized paragraph is very good.

p. C. 52. The discussion of THERP and HCR/ORE suggest that this may be a place where a "good applied statistician" using methods such as ANOVA and analysis of covariance may make useful contributions.

p. C. 57. It appears that a useful way to cope with software reliability growth is to study the intervals between the detection of "bugs".

p. C. 58. Section C.3.3 seems tio be misplaced. I feel that it should precede and not follow software reliability.

p. C. 93. Disadvantages. A possible way to model complex phenomena is by treating it as if were a random process, even if it is deterministic.

p. C. 106. As tabulated outcomes A and B have the same contribution to risk. Introducing a "utility function" may be a satisfactory way to circumvent this difficulty.

p. C. 112. Sensitivity studies can be modified to treat simultaneous changes in several inputs.

p. C. 114 and following. The labeling of figures is in error. That is, Figure C. 4. 5 is followed by Figure C. 4. 7.

p. C. 124. The paragraph beginning "While results ...." is very good.

p. C. 134. line 2 typo - "that" should be "they".

p. C. 134. If you know in advance what a given expert will say, there is no need to include him in a panel. You can factor in his opinions. That is, he is superfluous to the panel.

p. C. 134. If you isolate experts, you may gain by eliminating problems due to "dominant personalities", etc.

p. C. 138. Italicized paragraph. There is a difficulty if you decompose a problem. Namely, how to return to the original problem.

p. C. 139. Here you have my usual concern about expert judgements. The most important values (those in the distant tails of a distribution) are precisely the ones with the greatest uncertainties in expert judgements.

p. C. 148. Add to the "Disadvantages", the problem is ascertaining extreme values.

p. C. 148. Cautions Typo, change "an" to "and".

p. C. 149. near bottom. Change "over confidence" to "overconfidence".

p. C. 155. Uncertainty analysis. The important uncertainty is the uncertainty in the output.

p. C. 156. Uncertainty analysis and sensitivity analysis are "confounded". Essentially, they need to be studied together.

p. C. 159. To resolve the problem of plant specific versus generic data, statistical analysis of homogeneity of plants may be appropriate.

p. C. 161. Typo, near bottom of page, change "of" to "or".

p. C. 161. In asserting that classical methods produce confidence intervals as their primary output, an important usage of classical methods is being overlooked. That is, the calculation of sampling distributions indexed by the input parameters. This would be a very useful adjunct to any sensitivity and uncertainty analysis.

p. C. 163. It should be noted that the quantiles that are given are estimates and not the population values.

p. C. 164. Disadvantages. Even in the case of iput distributions with highly skewed and long tails, substantial smoothing may take place in the calculation of the output distributions (such as if the calculation involves repeated convolutions) and the output distributions may be "well-behaved".

p. C. 166. I am not sure what is meant by "mean probability for that interval".

p. C. 177. "The predicted values will be as close as possible to the observed values" is not quite an accurate statement. It depends on how you measure "close".

p. C. 177. The notation in the equation near the bottom of the page is inconmsistent, that is the coefficient k and the subscript n are incompatible.

p. C. 178. It should be made clear that linear regression means linear in the coefficients and not necessarily in the x's.