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ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS U.S. NRC

College of Engineering, FH-10
Office of the Dean

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1979

Dr. Milton Plesset
Chairman
ECCS Subcommittee, ACRS
Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Dr. Plesset:

This is a written consultant's report on the June 19 & 20 meetings of the ECCS Subcommittee, concentrating on the B&W small break analysis and the NRC water reactor LOCA/ECCS research, respectively. The report consists primarily of recommendations concerning priorities for research conducted by the NRC.

As a preamble, I would like to emphasize that I think it would be presumptuous for anyone not involved with the research programs on a full-time basis to propose a complete, priority-ordered list of research areas. The NRC management and staff have obviously given this much attention and thought and have sought input from a variety of sources in developing a program budget. I do have opinions, however, about areas I believe should receive increased priority and others that could be given lower priority without adversely affecting reactor safety.

From the presentations both days it was evident that small breaks will receive much more attention than they have in the past. This is strongly recommended and appears overdue. It is also in line with the recommendations of WASH-1400. The rescheduling of LOFT experiments and the proposed modifications of some computer programs to include more system analysis following small breaks are two examples of the re-ordering of priorities that has occurred in this area.

There are other areas, however, that I think should also receive more attention and should be placed higher on a prioritized list. Although there was no presentation of a priority-ordered list as such, areas being given most attention could be inferred from the items included on the agenda. Projects that I believe should be moved up are:

1. Research on eliciting possible failure scenarios, using as researchers individuals who have a combination of scientific/engineering knowledge and extensive experience with power reactor systems

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A general characteristic of most of the research now being conducted is that it involves highly sophisticated researchers and equipment, resulting in large quantities of reports, data and computer output. While most of these efforts can be neatly placed on an information matrix of one sort or another, it is not obvious how (or whether) the information obtained is being integrated, distilled, or disseminated. The various research projects often acquire momentum of their own, and because the principal investigators are productive, talented and persuasive, the projects continue to be funded, perhaps with slight modifications to conform with changing concerns. The investigations are also complex and intriguing. Complexity and intellectual challenge, should not, however, be the main criteria in setting priorities for what needs to be done.

There is a need for some of the information these projects generate, but there should be additional emphasis on a different kind of research--one that would attempt to integrate knowledge about all aspects of nuclear power systems and, taking a more heuristic approach, look carefully for weaknesses in the system as a whole. An example is the Michelson analysis, which is now receiving much attention, but was virtually ignored until TMI-2. Such research should use the expertise of individuals who have both scientific knowledge and familiarity with the operation and instrumentation of power systems. By invoking conservation laws and engineering judgement and applying them to actual systems under duress it may well be possible to identify critical situations that have been given too little attention in the past.

The TMI-2 sequence will obviously receive a lot of attention, but it would be a mistake to concentrate on the details of that particular sequence and ignore some of the more basic lessons that can be learned from it. One of these is that more attention needs to be given to thoughtful analysis of complete reactor systems including the way in which information is provided to operators. Another is that simple calculations may be very useful in identifying problem areas. While back of the envelope analyses of systems may not be <u>sufficient</u> to describe an accident sequence, some conditions are certainly <u>necessary</u> if safety is to be assured and these can be checked quickly.

This type of research need not involve large experiments or long computer runs and would not have great impact on the overall budget, but could have significant impact.

2. Research at the "interfaces" between systems

In an excellent small book called The Selection of Design, one of Gordon L. Glegg's recurring themes is the care that must be given in design to 'frontiers' at which energy or material is changed from one form to another. He believes that it is at these frontiers or interfaces that the design process should begin and that ignoring them is courting disaster.

I believe there is an analogy in the safety design of nuclear power systems. Much effort has been concentrated on the design of the principal components to achieve safety in depth, but too little attention has been paid to the interfaces at which information or energy is transferred or transformed. This is particularly true at the interface with the control room and the reactor operators.

Research directed at the interfaces should receive higher priority. It should aim first at identifying sensitive interfaces and second at establishing standards to guide the NRC licensing and enforcement processes. If one examines the lists of "Occurrences Resulting in Reactor Shutdown" reported in NUCLEAR SAFETY and the incidents reported in NUCLEAR NEWS' "On Line with Verna," many can be attributed to lack of attention at the interfaces, and NRC could, through its research, identify and correct weaknesses.

3. Research on human factors in engineering and on decision-aiding techniques

An article on "Human Factors in the Nuclear Control Room" in the Nov-Dec 1977 issue of NUCLEAR SAFETY "Paints," in the authors' words, "a rather negative picture," and indicates the need for additional research in this area. Many of the symptoms described in that article were present in the control room at TMI-2. The authors cited several specific topics that needed research and attention. Some may be outside the scope of NRC, but several are clearly within the area needed to establish standards for operating license approval. Peter Drucker has written a great deal about the importance of this area in terms of effective management, and it seems to apply to safety as well as to economic enterprises. He has written, "We have barely scratched the surface here (In human engineering); yet we know already that these studies are leading us to major changes in the theory and design of instruments of measurement and control, and into the redesign of traditional skills, traditional tools, and traditional processes." And also, "Psychology tells us that the

.. one sure way to shut off al perception is to flood the senses with stimuli. That's why the manager with reams of computer output on his desk is hopelessly uninformed. That's why it's so important to exploit the computer's ability to give us only the information we want--nothing else. The question we must ask is not, 'how many figures can I get?' but "What figures do I need? In what form? When and How?' We must refuse to look at anything else."*

*Drucker, Peter F., Technology, Management & Society, Harper & Row, 1970.

4. Research on improved ECC Systems

Much of the experimental research conducted by NRC is used for code assessment or to help evaluate designs that are used or proposed by vendors for safe cooling of reactor cores. Little has been done on developing totally new concepts of emergency core cooling and energy transfer to an ultimate heat sink. Exploration of new concepts should be placed somewhere on the list of research priorities.

Areas that could tolerate a lower priority are:

1) Detailed analysis/reconstruction of the sequence of events at TMI-2

The broad outlines of what occurred at TMI are known, and so are the immediate consequences of the accident. Substantial effort at trying to reconstruct the exact sequence has been undertaken, but with only mixed results. One reason for the mixed results is that the sequence depends strongly on boundary conditions and operator actions whose timing or magnitude are unknown. Extensive efforts to try to deduce the sequence from sparse evidence seem fruitless since the results will be too uncertain to be useful either for verifying computer code analysis or for accurately reconstructing the accident sequence. Although it is of academic and professional interest to reconstruct all phases of the event, it is not as important as a deeper examination of the underlying causes and of the need for changes in standards.

2) Comparative Analysis of Standard Problems

I believe this program's priority should be substantially lower, especially if it remains in its present form.

3) Certain Small-Scale Separate Effects Tests

Such experiments are needed for development of thorough understanding from first principles, but probably not for safety assurance.

Sincerely yours,

Kermit L. Garlid
Associate Dean

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