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Advisory Commission on Reactor Safeguards
ATTN: John T. Larkins, Executive Director
U.S. Nuclear Regulatory Commission
Mailstop T-2 E26
Washington, DC 20555

Dear Members of the Committee,

Subject: ACRS Review of NRC/RES Proposal on Resolving Generic Issue 189

I have been involved, in one way or another, in the issue of the vulnerability of ice condenser PWRs to Station Blackout events (SBOs) for almost 20 years, beginning with work on NUREG-1150, followed by my managing Sandia's support to NRC's Containment Performance Improvement program in the late 1980s, and continuing with my involvement in the effort to resolve the Direct Containment Heating (DCH) issue for ice condensers in the late 1990s. In regard to this last effort, I was a co-author of "Assessment of the DCH Issue for Plants with Ice Condenser Containments," NUREG/CR-6427, and I carried out the CONTAIN code calculations for the final version of that report. Shortly thereafter, in 1999, I retired from Sandia (though I imply no cause and effect relationship there). Since then I have followed with interest NRC's efforts to deal with the remarkable vulnerability of ice condenser containments to SBOs.

In his transmittal of NUREG/CR-6427 to NRR, Ashok Thadani, NRC's research director, concluded that the study had, in effect, closed the DCH issue for ice condensers but had also brought to light the high vulnerability of these plants to containment failure (primarily from hydrogen combustion) in SBO sequences. He suggested at the time that this vulnerability be addressed through the ongoing efforts to risk-inform 10 CFR 50.44. I will admit to some skepticism about his suggestion at the time, since the vulnerability in question had been well known for at least fifteen years. But NRC's research effort on Generic Safety Issue 189 (GI-189), recently made public by Farouk Eltawila's May 13 memo to the ACRS, has impressed me and my skepticism has abated somewhat.

It has been less than a year since the NRC's Executive Director for Operations announced to the Commission in SECY-01-0162 the establishment of GI-189 to deal with the vulnerability of ice condensers and BWR Mark IIIs to SBOs, and less than six months since the Commission established a high priority on resolving it. The RES staff has accomplished a great deal within these compressed timeframes and should be congratulated on their efforts (and those of the tireless contractors at ISL, BNL, and SNL). Their preliminary work has set the stage for an expeditious resolution of GI-189 and, hopefully, a timely implementation of the needed containment improvements to ice condenser PWRs.

I would very much have liked to attend the ACRS meeting at which GI-189 is to be discussed (June 6-8, 2002) and present my views on the ice condenser-related issues, but previous family commitments during that time frame make that impossible. In this letter, I will summarize some of my viewpoints. David Lochbaum of the Union of Concerned Scientists has offered to present some highlights of my assessment at the meeting, for which I am greatly appreciative, though I hasten to take responsibility for any errors in my review (political or otherwise).

My most important comment is that, while the contractor reports provide a basis for supporting a variety of the backfit options that were evaluated, Mr. Eltawila's letter appears to recommend only the low-cost portable electric generator to power only the igniters. I have long believed that a fix to ice condenser vulnerability must include, in addition, power to the air return fans. Nothing in the NRC's recent analyses has changed that belief. I will provide a detailed explanation of my views below, but I would first like to put that discussion in context by stepping back and looking at some overarching issues related to containment improvements.

Why are containment improvements so problematic for the NRC?

It has become apparent to me over the course of my many years of support to NRC that there is an asymmetry between what can be accomplished in the way of improved power plant safety in the 'front end' versus in the 'back end.' There has been no shortage of conflict between the regulator and the industry over improvements to the primary system and associated safety systems, but over time there have been many important safety upgrades as well. Compared to before TMI, we have better pump seals, better system reliability, better steam generators, better water chemistry, and the list can go on and on. By contrast, little has been done to improve containment performance, at least in the past decade or so.

Certainly there have been many changes related to the containment, and some have been based on risk analysis. But almost all such changes are some form or another of regulatory relief. Certainly there have also been innumerable cases of straight regulatory relief for the Nuclear Steam Supply System and its associated support systems, but in addition there have been many 'front end' changes that cost money but improve safety. And those changes continue. Not so for the containment system, in my belief. With very few (and minor) exceptions, risk-based changes to the containment and its operations have been strictly for relief, not improvement. It has become a one way street.

I believe the reason for this difference is simple. The plant owner has much more of a shared interest with the regulator in avoiding conditions that lead to severe accidents.

Doing so not only protects the public, but it also preserves the power plant as a source of revenue to the owner. As with TMI-2, a severe accident almost assuredly ends the life of the reactor as a productive capital asset regardless of whether the containment fails or not. This simple fact gives the licensee a double incentive to work with the regulator to reduce the core damage frequency. There is no such double incentive to reduce the conditional containment failure probability.

I think that at one time the NRC implicitly recognized this difference when it introduced the Containment Performance Improvement program in the 1980s. However, the virulence of industry's reaction to proposed improvements to BWR Mark I containments brought that program to an untimely end around 1990, and since then there has been no significant action, as far as I know, related to improvements in containment performance. That is, until Mr. Eltawila's May 13 memo.

I do not say this to put the industry in a bad light. I say it because it is important for NRC to take account of this asymmetry in its approach to containment improvements. It is inevitable, given the differences in industry's incentives, for it to be more difficult to gain industry consensus on containment improvements than on improvements to the front end. This doesn't mean the NRC should be satisfied with less progress. Defense in depth is important, and the meaning therefore is that NRC should work harder to accomplish such improvements, expect more resistance from licensees, and bring a firmer resolve to the deliberations.

Having now exercised my private citizen right to express my views, soapbox style, I will turn to the RES recommendations on GI-189.

The low-cost backfit option recommended by RES is inadequate and possibly counterproductive.

The Eltawila memo provides in its three attachments a preliminary technical basis for evaluating potential backfits to ice condenser containments (as well as Mark III containments, but I will have no comments on the Mark III issue). Attachment 1 is a brief cost study by Information Systems Laboratories, Inc. (ISL) of four basic options for adding equipment to provide backup power to containment safety systems during SBO conditions. Attachment 2 is an assessment by Brookhaven National Laboratory (BNL) of the averted costs (or benefits) that might accrue from implementation of a containment fix for the SBO vulnerability, which evaluates the dollar value of the benefits for a matrix of cases involving different plants and different analysis assumptions. Finally, Attachment 3 is a very preliminary report from Sandia National Laboratories (SNL) on MELCOR code calculations of the effectiveness of some of the backfit options.

Certainly one thing that is needed is a more integrated presentation of these disparate results, and I would hope that RES plans to prepare such a report in the future. However, given the time pressure for addressing GI-189 I can understand the decision to make available the research results in this fragmented form at this time.

By studying the BNL and ISL reports together, one can see that a number of the backfit options for ice condensers easily pass a cost benefit test for a range of analysis assumptions. For example, in Table 2-12 of Attachment 1 we see that the option involving pre-staged emergency backup power for igniters and air return fans has a total cost of $313,300 for a single unit of a dual unit ice condenser plant. Table 6 of Attachment 2 gives lifetime benefits ranging from $404,000 to $6,730,000, depending on analysis assumptions. That it is cost beneficial to fix the ice condenser containments is not surprising to me, since the inadequacy of these plants in SBO conditions is notorious, and the risk significance of SBOs is high (at least for some plants). What is surprising is that Eltawila's memo, which is the only place any integration of the three reports occurs, seems to recommend only the low-cost option labeled "1b," which involves an off-the-shelf portable generator to provide backup power to the igniters only. Several other options that also have favorable cost/benefit numbers are not mentioned in the recommendations. Knowing that there will be resistance from industry to any of these proposals, I find it hard to understand why RES would choose to endorse only the 'el cheapo' option.

More important, I believe the low-cost option is inadequate to deal with ice condenser vulnerability to SBOs. In this, I agree with Duke Power's decision in their SAMA submittals on McGuire and Catawba to evaluate backup power to igniters only in conjunction with backup power to the air return fans. At a public meeting between the Nuclear Energy Institute and the NRC in September 2000,* industry representatives criticized NRC's assertion that powering igniters only would be sufficient—they said you need to power *both* igniters *and* fans to control the hydrogen burn threat in ice condensers, and they were right.

Far too little importance has been attributed in the RES analysis to the possibility of detonations in the ice chest. What I am worried about is the possibility that in the absence of forced mixing via air return fans, a typical SBO scenario would lead to the following conditions: very high hydrogen concentrations (say, over 20%) throughout much of the ice chest; quite low concentrations (under 5%, say) in the upper plenum and containment dome because there has been little leakage through the upper deck doors until this point in time, and steam inerted conditions in the lower compartment. Then, when the concentration in the upper plenum finally becomes combustible, one or more burns occur there, resulting in upper deck doors opening and closing, perhaps in succession many times, which brings out a plume of much more combustible gas into the upper plenum from the ice chest. This plume would then ignite and carry the flame back to the ice chest, where the deflagration would transition to detonation because of the wide variety of channeling and reflecting surfaces there. A global detonation over most of the ice chest is something I don't even want to think about. Containment failure could occur either through missile generation or dynamic overloading of the containment structure.

* "SUMMARY OF SEPTEMBER 28, 2000, PUBLIC METING WITH NUCLEAR ENERGY INSTITUTE (NEI) AND OTHER INTERESTED STAKEHOLDERS REGARDING RISK-INFORMED CHANGES TO 10 CFR 50.44", Memo from Alan S. Kuritzky, NRC/PRAB to Mark A. Cunningham, chief NRC/PRAB dated February 28, 2001.

I am not the first person to worry about this scenario, of course, but my point is that I don't think today's calculational tools can help NRC develop the confidence it needs to order a mandatory igniters-only backfit. I am glad NRC has commissioned Sandia to carry out a broad matrix of code calculations on hydrogen distribution, because they shed light on the overall issue, but there are some aspects of the problem that I believe defy accurate computational analysis. This means that even an expanded program of calculation will still leave substantial residual uncertainty about the potential for highly destructive detonations. The aspects that are problematic are the following:

1. Control volume codes like MELCOR and CONTAIN are not suitable for predictive modeling of natural convection in open regions that are larger than the characteristic dimensions of the circulation patterns. The governing equations ignore the convection of momentum, for example. Such codes are notoriously incapable of reliably modeling stable stratification, because they artificially diffuse mass in the absence of any driving force (an effect that some people call numerical diffusion). Results are highly sensitive to nodalization, far more so than with true Navier-Stokes solvers. For these reasons I have always been privately skeptical of NRC's use of control volume codes to address hot leg failure due to natural circulation in SBO-induced core meltdowns. It is true that skilled analysts can devise nodalizations that reproduce flow patterns resembling those seen in experiments, but I don't think that leads to *predictive* capabilities for shapes and scales far different from the tests.
2. The unique phenomena occurring in ice chests make such calculations even more uncertain. Hot air/hydrogen/steam mixtures will be affected dramatically and in numerous ways by the ice: first through condensation of steam, which creates a bulk flow towards the ice surface called Stefan flow; second through cooling, which affects buoyancy-related flow; and third through changes to the mean molecular weight of the gas mixture due to condensation of steam, which is intermediate in molecular weight between the lightest of the gases, hydrogen, and the heavier gases that are the principal components of air (nitrogen and oxygen). These various effects will either reinforce each other or oppose each other, depending on conditions. And the processes are occurring over a complex spatial distribution of ice surfaces, not just a simple boundary.
3. One might argue that these effects will serve to increase mixing compared to the corresponding hydrodynamic problem in the absence of ice, but it is also possible that under some conditions the effects might be to stabilize stratification, inhibit the formation of large convective loops, and in general reduce vertical mixing. Similarly, the pressure pulses originating from releases from the primary system are dampened by the effects of the ice chest to the extent that the upper deck doors do not open as often or as far as would be expected in the absence of ice. The result is increased isolation of the upper plenum (where the igniters are) from the ice chest (where most of the hydrogen is if the lower containment is steam inerted).
4. The problem is further complicated by the fact that the ice/gas boundaries are, over time, responsive to the gas flow, resulting in highly uncertain spatial

> configurations of unmelted ice. The Finns have reported considerable unevenness of melting in their ice condenser test facilities when mixing processes are weak. The industry has long discussed 'channeling' and 'melt-through' as issues related to the design basis accident—one of the important roles of the air return fans is to insure relatively uniform melting.

These difficulties combine to create very large uncertainties in code predictions of hydrogen concentrations, with or without power to the igniters. With backup power to the air return fans, the NRC has a wonderful opportunity to reduce the uncertainty, and at a cost much cheaper than building a code that can accurately calculate the problem!

When I argue that there is residual uncertainty, I am not saying that there might be a 1% or 10% residual failure probability; I am saying that this fix might be ineffective 50% or 90% of the time. There is little way of knowing what its effectiveness will be *unless you ensure mixing.*

The low-cost option endorsed by RES might even make the accident worse, by causing a detonation-induced containment failure to occur many hours earlier than it might have in the absence of forced ignition. I would guess that the effect of this possibility on the risk picture would be modest, but I don't know. I suspect RES would not feel very comfortable asking one of its contractors to evaluate this downside of a mandated backfit—it could very possibly be used as an excuse to oppose the change. As with medicine, the first rule should be 'do no harm.'

The arguments in favor of providing power to the air return fans are compelling, I believe. The cost benefit numbers look good if reasonable assumptions are made about averted costs. The fans are already there, and they play the role of allowing the ice and the igniters to successfully accomplish their functions. Moreover the potential that the accident is exacerbated by the 'fix' is substantially eliminated.

I strongly encourage the ACRS to endorse the overall approach RES has initiated, but to insist on the additional assurance provided by backup power to the air return fans in ice condenser containments.

<u>The claim in the Eltawila memo that analyses in NUREG/CR-6427 are bounding is false.</u>

Page 3 of the Eltawila includes the statement "Note that in Attachment 2, the ice condenser averted cost estimates used relevant information from NUREG/CR-6427, and it appears to provide upper bound estimates as compared to plant-specific best estimates." This statement is untrue and misleading.

NUREG/CR-6427 was part of a long program (initiated around 1992) intended to resolve the DCH issue at U.S. nuclear power plants. Throughout that program the approach was best estimate and plant specific. Having been intensely involved in the ice condenser

DCH project for several years, and as a co-author of NUREG/CR-6427, I can say that there was a mix of assumptions used for the analyses that ranged from conservative to optimistic, and the end results can't be characterized as occupying any particular point in the spectrum.

For example, the plant-specific containment fragility curves (which dramatically affect the bottom line, of course) were taken from industry IPEs, which have never been audited by the NRC and which are almost certainly optimistic. (I say this because NRC's test program on containment failure has amply demonstrated the sensitivity of code results to nodalization details concerning penetrations, weldments, and other important locations for stress concentrations. It would not be expected that the industry's analysts would intentionally make assumptions that would exaggerate the potential for failure of their containments.)

In addition, certain assumptions about initial conditions in the core were also probably optimistic. Other assumptions, such as our treatment of steam spike, were probably pessimistic (or conservative), having been performed under severe time pressure. While I certainly wish that there had been time and funding to do more thorough analyses for the project, it is simply untrue to characterize the results as bounding or even conservative, regardless of how unpopular some of the results are with industry. The overall picture that emerged from NUREG/CR-6427 about ice condensers' vulnerability to SBO was qualitatively no different from the results of many earlier studies. It is because of that fact that GI-189 was established.

In the September 2000 meeting mentioned earlier, industry representatives complained that NUREG/CR-6427 was only a 'scoping study' and should not therefore be used as the sole basis for deciding on containment backfits. NRC responded that their recommendations were also based on other studies. Both sides were right. But it is inappropriate now for NRC to rewrite history by implying that the NUREG/CR-6427 was bounding in nature.

Final Observation.

While there is a clear need for additional study to support resolution of GI-189 (such as whether backfits that are not qualified for external event-induced SBOs would succeed anyway), I hope that the NRC proceeds into the implementation phase in a timely way. The RES staff has made a good start in establishing the technical basis for resolving this important issue. But I would like them to set aside their pre-conceived notions of what is the right answer and let the scientific facts speak for themselves. In this regard, I reiterate the following Observation from the 1998 ACRS review of the NRC Research program: "The Office of Nuclear Regulatory Research (RES) routinely relies on "assumed" solutions to address technical issues."[†]

[†]Advisory Committee on Reactor Safeguards, USNRC, "Review and Evaluation of the Nuclear Regulatory Commission Safety Research Program", NUREG-1635 vol. 1, p. 30 (June 1998).

There is an old saying, "if it's not broke, don't fix it." I'd like to propose a new version to the NRC: "if you're going to fix it, then *fix it!*"

I would be glad to discuss any questions the Committee or others at NRC might have in regard to these comments.

Sincerely,

Kenneth D. Bergeron