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

February 1, 1984

MEMORANDUM FOR: Jim McKnight, Distribution Services

FROM: Beverly J. Roberts, Technical Information Group
Advisory Committee on Reactor Safeguards

SUBJECT: LISTING OF ACRS DOCUMENTS ON THE ACCESSIONS LIST

The attached ACRS documents are being provided to you for listing on the accessions list. Please forward to Public Document Room.

Beverly Roberts

Beverly J. Roberts
Technical Information
Assistant

Attachments:
As Stated

ACRS CONSULTANTS REPORTS AND MEETING MINUTES

CT-1256A	Seale ltr 5/28/80 to Kerr, Comments on the Class 9 Subcte Mtg
CT-1301A	Schott ltr 12/5/80 to Kerr, Comments on the Class 9 Subcte Mtg
CT-1337A	Schott ltr 5/7/80 to Mark, Comments on the Sequoyah Subcte Mtg
CT-1683	Bush ltr 10/28/83 to Boehnert, Comments on the Review of Generic Technical Activity B-10 (Resolution of Mark III Issues)
CT-1691	Schrock ltr 11/23/83 to Boehnert, Comments on the ECCS Subcte Mtg
CT-1692	Tien ltr 11/28/83 to Boehnert, Comments on the ECCS Subcte Mtg
CT-1693	Gall ltr 11/16/83 to Igne, Comments on EPRI/AIF/MPC Bolting Seminar held November 2-4, 1983
CT-1694	Pomeroy ltr 12/1/83 to Okrent, Comments on the Seismographic Networks Report
CT-1695	Thompson ltr 12/1/83 to Igne, Comments on EPRI NDE Review
CT-1696	Corradini ltr 12/7/83 to Kerr, Comments on the IDCOR Phenomenology Meeting held November 29-30 and December 1, 1983
CT-1697	Shack ltr 12/9/83 to Shewmon, Comments on the SECY-83-267C: Reinspection and Repair of BWR Piping
CT-1698	Gall ltr 12/9/83 to Igne, Comments on Metal Components Subcte Mtg BWR Pipe Cracks
CT-1699	Bender ltr 12/10/83 to Shewmon, Comments on BWR IGSCC Safety Issues Concerning Primary Systems Piping
CT-1700	Dillon ltr 12/6/83, Comments Relative to BWR Water Chemistry
CT-1701	Luco ltr 12/22/83 to Boehnert, Comments on the Fluid Dynamics Subcte Mtg
CT-1702	Luco ltr 12/19/83 to Savio, Comments on NRC's Standard Review Plan Section 2.5.2, Vibratory Ground Motion
CT-1703	Maxwell ltr 12/19/83 to Okrent, Comments on Extreme External Phenomena Subcte Mtg
CT-1704	Maxwell ltr 12/20/83, Comments on a Proposed Revision to Standard Review Plan 2.5.2, Vibratory Ground Motion

CT-1705	Catton ltr 12/20/83 to Wang, Comments on IDCOR Meeting Number One- Accident Phenomena and Containment Loading
CT-1707	Thompson ltr 12/22/83 to Okrent, Comments on the Proposed Revision to the Standard Review Plan Section 2.5.2, Vibratory Ground Motion
CT-1709	Rodabaugh ltr 12/10/83 to Igne, Comments on Metal Components Subcte Mtg
CT-1710	Gall ltr 12/15/83 to Igne, Comments on the Metal Components Subcte Mtg
CT-1711	Page ltr 12/17/83 to Okrent, Comments on the Draft Revision of NRC's Standard Review Plan Section 2.5.2
ACRS-1950	Mintues of the Joint Meeting of the ACRS Subcte on Safety Philosophy, Technology, and Criteria/Class 9 Accidents February 3, 1982
ACRS-1970	Minutes of the ACRS Reliability and Probabilistic Assessment Subcte Meeting on the Zion Probabilistic Safety Study March 25-26, 1982
ACRS-1994	Minutes of the ACRS Subcte Meeting on Class 9 Accidents May 28, 1982
ACRS-2005	Minutes of the ACRS Subcte Meeting on Perry Nuclear Power Plant Units 1 and 2 June 28-29, 1982
ACRS-2059	Minutes of the ACRS Subcte Meeting on Class 9 Accidents December 21, 1982
ACRS-2144	Minutes of the ACRS Subcte Meeting on Class 9 Accidents October 12, 1983
ACRS-2149	Minutes of the ACRS Subcte Meeting on ECCS November 8-9, 1983
ACRS-2154	Minutes of the ACRS Subcte Meeting on Advanced Reactors November 16, 1983
ACRS-2163	Minutes of the ACRS Subcte Meeting on Fluid Dynamics December 8, 1983
ACRS-2168	Minutes of the ACRS Subcte Meeting on Advanced Reactors December 14, 1983

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PDR 020184

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DEPARTMENT OF NUCLEAR ENGINEERING

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ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS

May 28, 1980

Dr. William Kerr, Chairman
Subcommittee on Class 9 Accidents
Advisory Committee on Reactor Safeguards
Nuclear Regulatory Commission
1717 H Street
Washington, D.C. 20555

Dear Dr. Kerr:

The following comments are submitted for consideration by the Class 9 Subcommittee. I am aware that the ideas expressed are not original, but putting them down on paper might aid in coming to grips with the Subcommittee assignments.

This letter deals with the root question of the Class 9 Accident and the appropriate approach that might be taken to arrive at recommendations on this most complex problem. For the present discussion, it is assumed that the core melt accident is the Class 9 Accident.

I might add in this introduction that the ability to diagnose and intervene in the initial development of events that can lead to a core melt accident before threats to the health and safety of the public can occur is the most productive and effective approach that can be taken.

The discussions that follow are largely directed to the "intensive care" phase for those cases where diagnosis and intervention fail.

<u>The Class 9 Accident</u>

At this stage, the definition of a Class 9 Accident is not completely clear. Which of these is it?

1) The accident that is worse than the Design Basis Accident?

2) The accident which has consequences more severe than those defined as limiting in 10 CFR 100?

3) The core melt accident?

For the purposes of this subcommittee, it is important to know the extent to which the above answers are equivalent. Let us examine the problem further. From a physical point of view, the third answer is an appropriate starting point.

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The TMI accident forces us to consider the core melt accident as being possible; it also allows greater confidence to be placed in the defense-in-depth concept that is provided by the containment building. Indeed, if the TMI experience is extrapolated in severity of the core melt and the performance of the containment extended in a manner considered consistent with the TMI analyses, it is possible that a core melt could have occurred, but significant containment breach would not have resulted. Where does this leave the Class 9 Accident question?

In an attempt to come to grips with this question, the mechanistic approach is discussed below, and then an alternate approach is suggested. The second approach is not original; indeed, it is the more traditional approach to an impasse of this type.

The Class 9 Accident question is inherently complex. It involves technical questions that have been both difficult to frame and virtually impossible to answer uniquely since the days of the Ergen committee. Most important, what is the basic approach to be used in the approach to the Class 9 Accident question at this time? Certainly one approach would be a purely mechanistic one; i.e., asking a sequencial series of questions and formulating appropriate answers in turn which would trace (or chase) an uncooled core through its history until cooling could be restored and the situation stabilized. Unfortunately, in this approach, the answer to Question (i) implies the details of Question (i + 1) - and indeed a supressed desire to answer Question (i + 1) may influence the nature of the answer that is given to Question (i). To say it a little differently, mechanistic sequences may not be independent since they may tend to steer themselves to the consideration of pet problems of the mechanic. The perils of this approach are basic to the history of safety analyses to date. Certainly the argument between 661MJ and 1200MJ as it unfolded in the CRBR program represented a reduction of the mechanistic approach to absurdity. (If I may digress, it is hardly a surprise that if one disables the consideration of all shutdown process [thermal expansion, doppler, etc.] that can occur prior to gross fuel motion, then the mechanistic computer code will indicate progressively more and more energetic behavior as it plows through the equation set to achieve the condition of gross fuel motion.) The real lesson for the Class 9 Accident consideration is that the argument over detail that ensues from mechanistic differences of opinion may not really converge to the basic issues that exert great influence in bounding or mitigating the effects of such accidents; there is a tendency to concentrate on the question of what constitutes appropriate input for the already identified effects or phenomena. But if an effect is not in the model, it may very well be neglected.

There is an alternative which is more traditional in dealing with such questions. In essence, the approach is to identify the likely outstanding threats to the present system and deal with them each in a specific way. Thus one would examine the core melt accident in terms of the threats that are posed to the integrity of the pressure vessel, the integrity of the containment, and possible exposure to the public in turn and identify the possible means of mitigating the effects of these threats.

Thus, the likely threats to the pressure vessel integrity might be: (1) the in-vessel steam explosion with consequent generation of missiles; and (2) the melt through of a core-structure molten mass which would dump the molten mass into the containment building sump.

Threats to the containment integrity might be: (1) in-containment steam explosions resulting from the molten fuel-structure mass dumping into the containment sump; (2) hydrogen explosions due to the accumulation of hydrogen gas in the containment building due to venting of metal-water reaction products; or (3) failure of containment due to a molten core-structure mass melting through and eroding the containment building basemat.

Finally, the exposure threat to the public is highly dependent on the retention of radio-iodine in the containment. Certainly, if the water-chemistry of TMI is general, the exposure doses are much less than those typically calculated in past evaluations. There are no doubt other threats that should be added to the above list.

The threats should then be examined to "order" their relative probability. The same difficulties that make the mechanistic approach difficult also will likely frustrate any attempt to assign absolute probabilities to these threats. Even so, once the threats are "ordered," specific actions or additional design features to enhance the integrity of the pressure vessel or the containment, or to reduce exposure dose can be examined and recommendations made. This approach would, in effect, be an examination of the defense-in-depth concept in a systematic way, with the intent of responding to those core melt accident induced threats so as to ensure the integrity of containment.

If done with discipline, the suggested approach would avoid the frustrating arguments of the mechanistic procedures. The containment building concept did not evolve as the response to specific, well defined threats resulting from detailed accident event sequences. Rather it is the result of the desire to go to a defense-in-depth response to undefined threats to the health and safety of the public through the use of passive engineered safeguards. And this concept has served us well. It may well be that "as appropriate" stiffening of the containment building based on a more up-to-date perception of the threats to the health and safety of the public is in order. This, along with detection and diagnosis of event chains that could lead to core melt problems, may be the appropriate responses at this time.

Some Specific Threats and Possible Information Sources

Possible future subjects for consideration by the subcommittee are listed below:

1. The Steam Explosion - Conyers Herring, Stanford University (He has looked at steam explosions as part of a risk assessment study.)

2. Hydrogen-Explosion or Burning

3. Iodine Retention in Water Systems

4. Early Stages in a Core Melt Accident - John Bolstad, LASL and John Ireland, LASL

5. Core-Concrete Interactions - Dana Powers, Sandia Corp.

As stated at the beginning, most of the content of this letter has been discussed before. Even so, it might be of value to have it down on paper. If there are any questions that deserve further discussion, please let me know.

Very truly yours,

Robert L. Seale

Robert L. Seale
Professor and Head
Department of Nuclear Engineering

RLS/dg

CT-1301A
PDR 020184

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In reply refer to: G-7
Mail stop: 329

December 5, 1980

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ADVISORY COMMITTEE ON
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Dr. William Kerr, Chairman
Subcommittee on Class 9 Accidents
Advisory Committee on Reactor Safeguards
U.S. Nuclear Regulatory Commission
1717 H Street, N.W.
10th Floor
Washington, DC 20555

Dear Dr. Kerr:

I record here my observations on particular concerns raised for attention in the November 21, 1980 meeting of the Class 9 Accidents Subcommittee of ACRS.

Predominant concern of this meeting was the posture of NRC to define and, through its own or other Federal research programs, to answer basic technical questions which stand to impede sensible conclusion of pending rulemaking commitments for light water reactors. I observe that in matters of generation and control of potentially explosive quantities of combustible hydrogen gas, the primary definition of questions and the activities which stand to illuminate them sufficiently for rulemaking are the direct results of licensing activities under the Interim Hydrogen Control Requirements for Small Containments. NRC/Research has thus far taken what is, by comparison, a spectator role in this area. Thus the points of departure for the thoughts I develop below are particular items which arose in the open session presentation by W. R. Butler.

Owing to the wide range of containment sizes, strength, geometries, and thermal capacities already established, it is evident that a wide range of fundamentally different control measures will be employed across the regulated industry, with selections in each situation of whatever works most satisfactorily for acceptable cost. Questions # 7 and 8 of Enclosure I to SECY-80-357 on Degraded Core Rulemaking explore some of this range, and the full range of applicable control measures probably has not yet been formulated, much less narrowed down to clear preferences.

In Butler's review of the exploratory work on Mark III BWR containment for Grand Gulf, I noted particularly the consideration of a novel and, to my mind, technically attractive variant whereby, in response to LOCA onset, the containment atmosphere would be rendered inert to explosive hydrogen burning

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by massive infusion of carbon dioxide. In common with the continuously inert condition of Mark I and II, this mitigation approach does not appear to accomplish simultaneous disposal of hydrogen, which would presumably be done by a separate and slower method. I believe, though, that rapid inerting might one day be done by controlled burnout of the atmospheric oxygen, thereby avoiding net addition of noncondensable gas with attendant inflation of the static pressure. The flame control technology which enabled Senator Schmitt and his several predecessors to travel to and from the surface of the moon on schedule ought to be adaptable to meeting the requirements of such emergency hardware.

Also noteworthy was Mr. Ethrington's inquiry to Butler about NRC staff's reaction to the considerations of hydrogen generation assembled by H. Ring of Savannah River and communicated to ACRS by P. G. Shewmon's memorandum dated October 30, 1980. I agree with some of Ring's technical arguments about explosions, and disagree with others. At this point, I concur generally with Butler's oral reply, to the effect that the packet of correspondence distributed last month is not sufficiently complete or detailed for conclusive evaluation, but that resolution of the questions raised is important and is being taken seriously by NRC staff. I proceed here to sort out these questions by setting aside the emotional components of their existing documentation and separating the questions of hydrogen source and hydrogen quantity in the Three Mile Island accident.

The quantity of hydrogen which burned explosively at 1:50 p.m. on March 28, 1979 is logically bounded above with decent precision by accounting for the magnitude of the pressure excursion which was measured at a limited location and assumed to have applied globally, but may have overshot the globally equilibrated pressure. What seems important is that we not become engrossed in resolving the ambiguity imposed by the limitations of measuring equipment in place at TMI, which was recognizably inadequate to diagnose hydrogen-air fires. As tantalizing as this ambiguity may be, I recognize that NRC's regulatory posture is now (Interim Rule) and is likely to remain justified by the more surely quantified inventory of elemental hydrogen that was still in the high-pressure parts of the reactor at midday on March 28, as a gaseous bubble (150 lb. moles) and dissolved in the water of the primary system (34 lb. moles) (these inventories quoted from C. Mark's Note on Hydrogen Burn and Generation, dated April 9, 1980, and evidently traceable to the HYD Appendix to the President's Commission Report). Irrespective of whether or not this 184 lb. moles is a majority of the hydrogen accumulated on March 28, it is nevertheless very large in relation to the designed capacity to accommodate hydrogen, and it is large enough to pose an explosive combustion threat to such smaller/weaker containment structures as are used with other utility-sized, light water reactors. Moreover, it is not an upper bound to the hydrogen generable by a reactor that might be misoperated more severely than occurred at TMI.

The more important question raised by Ring is that of the main source of the elemental hydrogen which was/might again be produced by accidental misoperation of an LWR (or, in his own sphere of concern, a plutonium production reactor). For if it should become known that zirconium reacting with water was not the main source at TMI, then the inventory of Zr places no stoichiometric bound on the ultimate capacity of an overheated reactor for generating H_2, and the soundness, or even the utility, of the % Zr yardstick for reckoning hydrogen production scenarios would be lost. Ring's strongest arguments in this area seem to be (1) the failure of oxidized zirconium debris to have been identified, much less inventoried, at TMI, and (2) insufficiency of knowledge of radiolysis at the extreme temperature and attendant physicochemical conditions encountered in the TMI or other LOCA accidents. Indeed, Ring does not even acknowledge, much less accept, the conventional belief emphasized on page 2 of the HYD Appendix that accumulated H_2 in small concentration strongly inhibits further radiolytic generation of hydrogen from water.

Perhaps NRC staff, DOE, and/or contractors will proceed to assemble the existing data base on radiolysis, assess the limitations therein, and provide for necessary extension to accident conditions. Concurrently, the TMI clean-up effort needs to inventory zirconium and its compounds, in addition to fission products.

Sincerely,

Garry L. Schott

Garry L. Schott

GLS:pma
xc: G. R. Quittschreiber (address same as addressee)
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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS, U.S.N.R.C.

MAY 1 9 1981

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In reply refer to: G-7
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May 7, 1981

Dr. J. Carson Mark
Chairman, Sequoyah Reactor Subcommittee
Advisory Committee on Reactor Safeguards
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

Dear Carson:

Having received and read the packet of documents distributed on December 19, 1980 by D. E. Bessette as background information for the January 6, 1981 ACRS Subcommittee meeting on Sequoyah, and having since augmented that body of information by attending the Workshop on Impact of Hydrogen on Water Reactor Safety held in Albuquerque on January 26-28, 1981, I record here a selection of my observations and opinions that pertain particularly to hydrogen control in the ice-condenser containment of the Sequoyah reactor.

At issue is the capability of Tennessee Valley Authority's (TVA's) interim distributed ignition system (IDIS) or of an upgraded, permanent one to dispose of combustible quantities of gaseous hydrogen that might be released accidentally from the reactor into the air-filled containment vessel. Some criteria for this capability are that mechanical threats of rupturing the steel containment or of disabling equipment essential to controlling remaining threats be rendered assuredly and significantly less than would exist in the absence of this or alternative means of controlling combustible hydrogen. Inherent in safe disposal of an arbitrary ultimate quantity of hydrogen by deliberate unvented burning is a succession of ignitions (terminable ultimately by exhaustion of oxygen) whose global increments in pressure are limited by the quantity of hydrogen consumed in each stage. These ignitions may occur in rather short times, provided that the intervals between them lead to an average power of combustion which can be dissipated into the available heat sinks. By this reasoning, the active and passive equipment which serves to limit the quantity and concentration of hydrogen accumulable between successive ignitions, as before first ignition, must not be disabled or seriously impaired as a consequence of a prior ignition. Such equipment, to which I focus my subsequent attention, forms a subset of the equipment which must survive the ignition cycle to accomplish safe cooldown of the crippled reactor.

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A very conspicuous effort has been expended to establish that electrically powered glow plugs will indeed reliably ignite as lean a hydrogen-air mixture as will propagate flame, and that the plugs themselves and their electrical supplies are not threatened by any otherwise tolerable ignition event. However, large-scale movements of gas which would occur during cyclical accumulation and burning of hydrogen, and the equipment which channels and propels some of these movements, have been comparatively taken for granted in the analyses and testing of the mechanism by which distributed ignition leads to benign disposal of hydrogen.

First, convection dominates the initial mixing of hydrogen with air in combustible proportions on the scale of the entire containment vessel and its numerous compartments. Also, convection in the localities of the individual igniters is responsible for ignition through contact between accumulating combustible mixture and some part of the few dozen square inches of igniter surface sprinkled throughout the acres of inert solid surface exposed inside the containment. For definiteness and tractability, the computational modeling of the behavior of the Sequoyah or other ice-condenser containment with distributed ignition and selected source rates of hydrogen and steam has resorted to somewhat crude assumptions with respect to the effective homogeneity of distribution and consumption of hydrogen within internal compartments of the containment. True homogeneity would assuredly not be achieved under reactor accident conditions, any more than other quantitative features of the preconceived accident scenario, be it S2D or some other, would be realized. Nevertheless, the evidence is fairly convincing that the gross patterns and rates of convection that bound the accommodatable average combustion power (or its equivalent rate of steam release) are also sufficient to support the gas motions needed for ignition over a usefully wide range of rates and locations of hydrogen release, steam accompaniment, and attendant malfunctions of other elements of the very complex system of normal and emergency reactor controls. Indeed, misexecution of the deliberate ignition strategy owing to assumed degradation of the capacity for convective transport has been modeled, as partly recounted later in this report. Thus the convective capacity of the Sequoyah containment to dispose of hydrogen by distributed ignition with adequate stirring rests primarily on the function of the 80,000 cfm emergency air recirculation system. Turbulence introduced by the water sprays and buoyant convection caused by differences of composition and temperature are also significant contributors in the response expected to the total, complex spectrum of possibilities.

Large-scale gas movement would also arise from intentional, distributed ignition in the spontaneous displacement away from the flame as the burning gases expand and pressure is equalized more or less immediately throughout the connected volume of the containment. Between compartments, this movement of gas follows the same, comparatively narrow pathways as the convection discussed above that regulates the accumulation and ignition of hydrogen. Moreover, this flow occurs transiently in response to propagation of combustion in confined spaces. Thus it is necessary to consider the possible violence of its aerodynamics, for the reason that damage to equipment which propels, channels, or restricts intercompartmental flow stands to degrade the system's capacity to respond to subsequent influx of hydrogen in the intended way. Even the capacity to provide continued transport of heat energy in steam to its intended sink in the ice gallery might be degraded to an unacceptable extent. Foremost among this potentially vulnerable equipment are the recirculation fans themselves and the one-way doors at three levels associated with the ice gallery. Both these items involve moving parts and both are sufficiently large that testing of their mechanical durability at a readily affordable scale may be inconclusive.

To my mind, it is almost trivial to assure that the fan hardware will not burn up, i.e. fail through overheating, by brief engulfment in a hydrogen-air fireball. But what I find worrisome, having seen photographs of the deformed doors on the interlevel elevator shaft following the 26 psi excursion due to combustion in the Three Mile Island accident, is the prospect that locally intense aerodynamic forces of a nonsteady deflagration in the ductwork might, for instance, bend the fan impeller enough that the bearings would subsequently burn out, or distort the housing so that the impeller would jam.

The other potential bottlenecks in the required intercompartmental convection route are the sets of doors at the base, intermediate level, and top of the ice gallery. An ignition sequence producing an upper compartment transient pressure even 2 psi above that in the lower compartment, as indicated in a base case calculation recounted below, would slam these doors rather rudely, such that they might or might not reopen freely. I understand that the top set of such "doors" are actually flexible mats covering some sort of grate, so as to be easily lifted. Nevertheless, even these might be propelled into a fouled condition by a sudden reversal of pressure gradient of a few psi amplitude. To be sure, the plenums with many parallel doors make it unlikely that more than partial blockage of upward flow through the ice gallery might occur at any level following single or even repeated combustion transients. Actually, I am skeptical that burning throughout the upper compartment, and the resulting overpressure, would ever occur in as all-or-none fashion as the models have presumed. Nevertheless, I believe that appreciable pressure pulses from above do pose a difficultly controllable mechanical threat to the one-way doors associated with the ice gallery.

I proceed to survey some of the pertinent, existing evidence on these issues of causes and limitations of gross gas circulations within the Sequoyah containment, as documented for ACRS at the end of 1980 by TVA and TVA's industrial collaborators, and as augmented by other, off-line investigations, particularly those at Brookhaven and Sandia National Laboratories.

To begin, I believe that the TVA work in this area is impressively thorough and well-balanced. I elaborate here on specific material in some of the appendices to TVA's December 15, 1980 Report on the Safety Evaluation of the Interim Distributed Ignition System, Volume 2 of Sequoyah Nuclear Plant Core Degradation Program.

Appendix C, entitled, "A Description of Phenomena Which May Affect the Post Core Damage Event Distribution of Hydrogen in the Sequoyah Containment," addresses the matter of convection directly. Its tenor runs between qualitative and downright armwaving, but this is the nature of the problem, and I find the perspective of this appendix to be intuitively sound. Moreover, internal details of the containment hardware are mentioned that do not appear to have otherwise been brought out in ACRS's exposure to the IDIS.

Examples:

(a) page C-4, item B.3, line 5; flow paths bypassing the ice condenser back to the upper compartment.

(b) page C-8, first paragraph; "doghouses" rising as dead-end volumes above the main lower compartment, but (page C-15, 3rd paragraph) with small, ducted relief directly to the recirculation fans.

(c) page C-11; intermediate deck doors (of unspecified configuration) between the ice bed and the upper plenum.

(d) page C-12; air handling units in the upper plenum.

(e) page C-12; top deck blankets covering the upper plenum.

Appendix D, entitled, "Nonsymmetric Containment Loads," directly addresses development of transient pressure heads between compartments, a phenomenon for which I expressed concern to the Class 9 Accidents Subcommittee in my letter of September 16, 1980 to William Kerr. Page D-4 discusses the cases of lower compartment burning at assumed flame speeds of 30 ft/s and 10 ft/s producing heads across the ice gallery of 11 psi and 3 psi, respectively. "Westinghouse TMD pressure studies" and something called "SPA code" are cited. These pressure heads for lower compartment burns may be realistic or even conservative; they unquestionably exhibit the expected dependence on assumed flame speed, which was absent or not discernible in mid-1980 modeling with MARCH.

Appendix T, entitled "CLASIX Program Description," and Appendix U, entitled "Summary of Analyses of Ice Condenser Containment Response to Hydrogen Burn Transients," relate heretofore unadvertized, important particulars of the CLASIX code. Page T-5, second paragraph, states:

> "Based on the flow path parameters and the differential pressures, a volumetric flow rate is calculated. Then, based on the source volume conditions, the individual constituent mass flow rates and energy flow rates are determined."

Page U-25, table 8, compares situations with burning in both lower and upper compartments, with 6 ft/s and 12 ft/s effective propagation speeds. Calculted peak pressures in the upper compartment generally exceed those in the lower compartment, and in most cases those in the ice gallery as well. These possibilities are the basis for my concern (vide supra) for damage to the one-way doors above and below the ice, given my belief that the check-valve action of these doors was probably designed on the basis of buoyant forces much smaller than those developed in the full-gale winds of a deflagration transient.

Appendix H, entitled "Containment Response to Detonations," does not offer the definitive treatment of its subject. It does, however, demonstrate the thoroughness of the TVA staff in scouring the literature, in that the significant disparity in accounting for the impulsive loading from a gas detonation profile between the WASH-1400 treatment and that (by Don Rose, with coaching from several of us) buried in the Technical Staff Analysis Report on Chemistry (by R. E. English, dated October 31, 1979) to the President's Commission on the Accident at Three Mile Island is called out, even if not resolved. I do take direct issue with the final sentence of the second paragraph on page H-5, which says, "In addition, the many pipes exiting the ice bed and the open doors exiting the upper plenum would significantly contribute to a dispersal of the shock wave." Experience contradicts this intuitive contention; reflections from partial obstructions actually merge quickly with an established gas detonation and ensure its continuation, or even promote the transition of a preflame compression wave into a detonation.

In a draft report "Analysis of Hydrogen Mitigation For Degraded Core Accidents In The Sequoyah Nuclear Power Plant," (SAND80-2714 NUREG/CR-1762, draft dated December 1, 1980), Sec. 1, pages 6-71, Sandia staff have addressed deliberate ignition for hydrogen control in the ice condenser containment at Sequoyah. The avowed purpose was for comparison with two alternative means for hydrogen control.

Many aspects wherein the TVA interim system is amply capable are recognized, and vulnerability of the system in particular accidental circumstances is determined. I recognize the conspicuous elements of this vulnerability as arising from burning in the upper compartment, particularly following loss of half or all of the intended fan-forced convection. By such loss, repeated burning in the lower compartment may be prevented through depletion of oxygen and/or accumulation of excess steam, which would be stripped out in passage through the ice gallery. The strong recommendation (on page 30, second paragraph, final sentence) for removal of the four igniters situated in the upper plenum of the ice gallery is logically predicated on the assumption of failure of the large recirculation fans. I believe that if these fans fail, the system is in sufficient difficulty that the absence of these four igniters will not remedy much of the spectrum of troubles. Nevertheless, even though detonation of the amount of hydrogen which might accumulate at this location would not of itself be likely to threaten the integrity of the containment structure, the close proximity of two sets of doors and of many other obstructions within the plenum does make it seem preferable to remove igniters to a position above the top blanket.

Highlights of a separate study of deliberate ignition in large, dry containment, with ad hoc application to the Sequoyah ice-condenser containment, were presented in the Albuquerque Workshop under the title "Impact of H_2 Combustion on Degraded Core Accidents in Large PWR Containments" by W. T. Pratt and R. A. Bari of Brookhaven. Among the points derived from the large dry containment investigation were:

(a) Inerting by steam was a conspicuous possibility;

(b) Intermittent burning of small accumulations of hydrogen is indeed beneficial when successfully accomplished, but it is vulnerable to faulty execution.

The application to the ice condenser situation led to such observations as:

(a) Outcome of MARCH modeling in sensitive to assumed particulars.

(b) Inerting of the lower compartment by steam may be expected under some assumable circumstances.

(c) MARCH is useful for scoping studies, but not for designing a mitigation system.

In the wrapup session of the Albuquerque Workshop, L. W. Lau of TVA announced the Commission's approval (on January 27, 1981, I believe) of Sequoyah operation under the IDIS through January, 1982. He indicated that deliberate ignition would be retained if and only if it is shown to meet a prescribed goal in reduction of risk, including equipment survivability matters still being evaluated. Inerting by Halon injected after onset of an accident is currently the first backup option if distributed ignition should be rejected. I observe that once sufficient Halon is mixed with air throughout the containment, a matter of perhaps 20 to 30 minutes, burning of hydrogen ceases to be a possibility, irrespective of further equipment function or rate or location of hydrogen release. The technical tradeoff is that the required Halon introduces additional gas pressure almost up to the 12 psig static design pressure of the Sequoyah containment. Lau expected the necessary TVA decision to be reached in about June, 1981 for implementation early in 1982.

Sincerely,

Garry L. Schott

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