**Studies of Nuclear Hazards
and Constitutional Law**

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Dr. Carl J. Paperiello,
Mr. Jim Wiggins, and
Mr. Norm Lauben, Ralph Myer,
Steve Borjack,Den Boglewebe, and
Harold Scott
Office of Nuclear Regulatory Research
U.S. Nuclear Regulatory Commission
Rockville, Md.
by telefax, 301-415-5153

Dear Gentlemen:

Subject: Postscript of Notes of Tele-conference 9 January 2006

I have sent you my letter* dated January 11th a few minutes ago today, January 12th. I tried to fax it yesterday, but it was too late to be received in your office; as the transmission was blocked. I have a few more comments to make about the results of the conference, which I offer in this present postscript-letter.

1. I have requested a copy of the NRC report, a NUREG document as I vaguely recall, titled something like "Long-Term Cooling of the Three Mile Island Unit 2 Reactor" — the report which treats of the switch to natural circulation cooling. I mentioned that the original version, if not also the final version, was given to me by Dr. Mattson in my meeting with him and Carl Berlinger on day April 26th of the accident. Also given to me for my study and critical review was a *Sandia paper* which predicted natural circulation cooling for the TMI-2 reactor — calculations, and a theoretical model for such, of natural circulation or natural convective cooling of a bed of particles, which was Sandia's model of the state of the reactor core assumed for their analysis. I wish to have a copy of that Sandia paper/report. I assume that it is contained in the NRC file on the action of switching to natural circulation cooling.

2. Returning to my question about a BWR LOCA without a prompt reactor scram, the reaction to my query offered by Ralph Myer is such as I have encountered earlier in my career, when I first raised the question — during a colloquy I gave before the nuclear engineering faculty and students at Purdue University in 1974 — upon an invitation by Professor Alexander Sesonske — author of Nuclear Reactor Engineering with Samuel Glasstone. Mr. Myer's off-hand

* I have revised that Letter, and am sending the Revised Letter with this postscript.

reaction which he offered in the discussion is that the loss of coolant would result in more voiding in the reactor core, and hence a reduction in the reactivity. I cautioned against such an assumption without a rigorous theoretical modelling and experimental confirmation; for a pipe rupture in the reactor system would result in sudden changes in the coolant pressure at each point in the reactor coolant system, including the water / steam channels and plenums inside of the reactor vessel; and, therefore, it should be necessary to account by theoretical equations all which determines the motion of water in that reactor during the LOCA.

I did not mention it in the conference, but I recall a General Electric report, issued about 1972 or so, of GE's design basis LOCA analysis which included a sequence of schematic drawings of the water distribution inside the reactor vessel and in the coolant piping at various points in times following the assumed sudden pipe rupture — water distributions as calculated by GE's LOCA model. (By drawings of the water distribution, I mean the indications of water present in each region of the system, as distinguished from steam, and a steam-water mixture.) The calculations assumed a prompt reactor scram. The drawings that I recall, showed the water distribution at the start of the LOCA, which represented the initial condition of the coolant distribution in the core — the normal state at full power; but that very soon after the start of the LOCA, the coolant channels in the core became filled with water up to the tops of the fuel assemblies/channels (according to those drawings), and that soon thereafter, the channels emptied of water. Thus, the sequence of drawings showed a momentary filling of the channels with water! In our conference I mentioned my calculation (made for my book, *The Accident Hazards of Nuclear Power Plants*) that a decrease from 43% average quality (normal full power value) to 41% would drive the reactivity to prompt critical!

I caution against dismissing my concern by mere argument, instead of a rigorous mathematical, theoretical calculation using an exacting model of the reactor and the coolant system outside of it. I think we have to be careful in making assumptions. The readings of the TMI-2 in-core thermocouples during the TMI accident should be a lesson in this respect. The Rogovin report has a section about those thermocouple readings, and asserts, but with hindsight, that because the T/C leads come up to the top of the fuel assemblies via the bottom of the reactor, through the center of the fuel rod assembly, those leads could have melted and formed new thermocouple junctions, so that those haphazard junctions were actually measuring temperatures of the solid material in which leads were embedded. But what did the NRC engineers assume during the accident?

As I said in the conference, not having during the accident any information available to me about the core thermocouple (t/c) system in the TMI-2 reactor, I assumed that the t/c's measured only the temperature of the coolant exiting the fuel assemblies at the top of the core, by assuming that the t/cs were led into the reactor from ports at the top of the reactor vessel — the closure head. (That was the case for the Shippingport PWR, as I recalled.) I think I was led, by official statements given out during the accident, to assume that the thermocouples were led down to the top of the core, by the officials calling the thermocouples, "core exit thermocouples." It is difficult to determine from the Rogovin report just what the NRC engineers, including Harold Denton, assumed as to whether the thermocouple data indicated temperature of fluid existing over the top~~s~~ of the fuel, or measuring temperatures deep down in the core as a consequence of melting of the leads. However, the testimony of Dr. Mattson, Darrell Eisenhut, and Victor Stello before the House Subcommittee on Energy and the Environment on May 9th, 1979, pages 11-16, seems to reveal the assumption that was in fact made by the NRC engineers during the accident — that the temperature readings indicated fluid temperature exiting the fuel assemblies — a temperature above the core, including notions of super-heated steam exiting the tops of the fuel. (Eisenhut testified that, "The thermocouple is raised above the fuel, too, so it is physically removed." — removed from the fuel material, was his assumption!) The post-accident reactor examinations clearly show, however, that the thermocouples were destroyed, and possibly or probably formed and reformed junctions down, and deep down, into the original core region of the reactor vessel.

Another example is the articles by Doug Akers and others of EG&G asserting a best estimate scenario as to when the core melted and 19 MT poured down onto the bottom of the reactor vessel. That best estimate scenario occurs at a little more than 4 hours into the accident — That scenario was formulated on the basis of the data of the first 16 hours of the accident. But as I mentioned in my letter and in the conference, EG&G did not analyze the TMI reactor accident data for the time <u>after</u> the first 16 hours of the accident. I think that before we could conclude when the molten material poured onto the bottom of the vessel, we would have to examine the data for beyond the first 16 hours, and extending past the point in time when the action was taken to put the reactor into natural circulation.

The unpredicted power oscillations in the LaSalle BWR in 1988 (or was it 1987 or earlier?) is another instance of a lesson learned about the danger of assuming the behavior of a reactor disturbance without a scientific calculation~~s~~.

I did not state in my letter, but I implied as such, that if the BWR LOCA without scram has never been calculated for the potential course it could take, then I would urge that it be investigated and analyzed promptly. I would like very much a copy of any report of such a work, of course.

I have sent some papers with my letter dated January 11th which might seem to be out of place or random bits of things. I give the following explanations:

1. The two graphs of Loss of Feedwater Accident in the TMI-2 system are results of a mathematical/theoretical/computer model I recently made of the TMI-2 system, just to give some indication of the seriousness of my investigations of the accident. The graphs relate to the danger of going water solid — the concern for which caused the TMI-2 operators to switch off the ECCS injection. I will be sending your office a CD ROM of much of my research, both of the nuclear hazards and the U.S. Constitution; and this CD will include a TV debate I had with the head of Davis-Besse, Admiral Joe Williams, held shortly after Chernobyl. In that debate

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we debated the danger of going water solid in the June 9th loss-of-cooling mishap at Davis-Besse.

2. Tables of results of thermal conduction calculations of a model for TMI-2 of a pour of *x* kgs of molten UO2 onto the steel bottom of the reactor vessel, to evaluate the temperature variation in the steel plate. I vary the quantity of the pour, and the timing. The model is a two-dimensional heat conduction model. On the basis of the calculations I estimate that there probably occurred a succession of pours, to build up an insulating layer of UO2, and not just one pour of 19 MT, as seems to be the assumption of the official analyses.

3. Title page of my Hinkley Point C evidence (testimony) before a British Court of Inquiry (several judges including a mechanical engineering judge, Professor Simpson of Univ. of Edinburgh, as well as a biology judge, and economics judge, and the chief judge, a legal counsellor for the Queen), the contents of my evidence, and a one-page summary of it; followed by pieces of the Transcript of Day 85 of the Inquiry, when I presented my evidence and submitted to cross examination, and examination by the Inspector and the engineering judge, Prof. Simpson. I thought that you would be interested in his questions about Probabilistic Risk Assessment.

4. The CD ROM will also include a video of the TMI accident symposium of March 25, 1999 at Penn State, in which Harold Denton spoke as well as William Traverse, and will include my commentary of those speeches, among many other documents and things.

I have an enormous analysis of the TMI accident to write and publish, besides a number of other urgent works, as I mentioned ever so briefly in the conference. Finally, I remind you about my requests for documents, which are necessary for me, in order to complete my analysis of the TMI accident, and my overall analysis of the accident hazards of nuclear power plants.

Sincerely yours,

Richard E. Webb

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