REALISTIC ESTIMATES OF THE CONSEQUENCES OF NUCLEAR ACCIDENTS

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It has long been normal practice in the engineering profession to use the device called feedback, or checking, or review to assure ourselves that what was done in the first place was not only correct, but that after some period of elapsed time it was still correct, in the light of any new information or experiences that had come into being in the meantime. This procedure has been applied to such details as what is the correct allowable stress to use in designing a bridge, what are the most appropriate combinations of loads to be considered simultaneously, what may turn out to be the real duty cycle as compared to the design duty cycle. This entire system has functioned very well across a broad spectrum of our endeavors. However, occasionally, due to either obsolescence of a system, lack of interest in a system, or lack of economic incentive, such things do not occur and we have a deteriorating component or system. This paper addresses itself to what is perhaps a unique case - the review of nuclear accidents. That is a particular activity which has had so much attention, so much review, so much reanalysis aimed at exploring hypothetical concerns that instead of moving toward a sounder and sounder basis, we have moved in the opposite direction and have moved further and further from reality. When we first raised the question in the spring of 1980 - have we all overestimated the consequences of a nuclear accident, we met with a great deal of skepticism. However, in the intervening year it has become very clear that almost everyone has been considering hypothetical disasters that were not only hypothetical, but more likely impossible. At the end of 1980, the Nuclear Regulatory Commission (NRC) held hearings on the subject. The U.S. Presidential Nuclear Oversite Committee chaired by Governor Babbitt of Arizona held hearings on the subject; and a special session at the joint ANS/ENS meeting in Washington contained six papers on various technical aspects of the subject. The Presidential Oversite Committee came to the conclusion that this was a matter of substance and so indicated in a letter to President Carter and the NRC requested a special staff assessment of the question. We do not believe this issue should be categorized as a new argument. It is rather a request that all people working in the field go back and look at what is the actual experience and what is a possible reaction and possible mechanism rather than assuming catastrophic conclusions which probably cannot occur.

The matters to be reassessed fall into several categories. The first category is that of fundamental chemistry. It is essential that people with basic understanding of chemistry and/or chemical engineering become involved in the actual computer analysis of various accident scenarios. Too often the computer modeler has been forced to leave out basic laws of chemistry, either because he does not understand them or they do not lend themselves to inclusion in the model that is being built. Just because a particular computer model cannot handle the details of chemistry does not mean that the chemistry does not exist or that the reactions will not occur. Some of the analyses which we have reviewed consider highly soluble materials like iodine or cesium compounds as though they were noble gases.

A second area, at the interface of chemistry and physics is the matter of aerosols. Many of the analyses assume material exists as an aerosol and have assumed it is long-term stable, aerodynamically stable, independent of physical properties or densities. In many cases the assumptions, when extracted from the interior of the calculations, call for aerosols at a density of pounds per cubic foot. Such a material may be defined as a sponge but hardly would qualify as an aerosol. In the same category, many of the analyses assume that the only effect of wetted surfaces, wetted walls, and wetted partitions is on the temperature of the gas or air and therefore on total pressure. Those computer codes and models do not recognize removal by impact or solubility of both solids and soluble gases that might be in a gas stream impacting on such wet and dripping surfaces.

The third general area of issues that we believe are not appropriate is the deletion of the effects of time in many of the analyses. That is, it is assumed, in some cases, that if a small fraction of one fuel pin reaches some threshhold temperature, the core becomes molten, it eats through the pressure vessel, falls into a pool of water, causes a steam explosion and subsequent failure of the containment building. Moreover, all of this is assumed to occur instantaneously so that volatile materials like iodine and various aerosols, etc., are treated as though they were generated at the time of the steam explosion or at the time of containment building failure, rather than as being generated at the time the fuel melts. Because the densification and/or agglomeration of aerosols is so very fast at the high densities involved, even the matter of tens of seconds means that the material that is airborne will be reduced by several orders-of-magnitude long before any subsequent reaction occurs, especially since the volatiles are liberated inside the pressure vessel. That is where the fuel melts.

Another physical phenomena, as important perhaps as the aerosol agglomeration and fallout, is the transport phenomena that must occur inside the containment building. Some of the analyses assume that the instant material leaves the primary system it reaches the dome of the containment building with no attenuation whatsoever. This is probably not an unrealistic assumption for the true noble gases. But it is a very bad assumption for everything else. The pressure vessel itself, the steam generators, the pumps, the connecting piping and all other parts of the primary system are inside shielded cubicles. Any type of release from the primary system, whether it be a pipe break, a valve leak, a pump seal failure, a gasket leaking, or any other failure occurs interior to a separate compartment, the transport of primary system activity must therefore first be from the point of fuel melting to the "break" in the primary system. From that "break" released material must go from one compartment to another, or from a compartment to the containment building dome area. The record of accidents and all major experimental work indicates that during such transport orders-of-magnitude attenuation exists for everything other than noble gases. Computer models which do not include phenomena such as this clearly are not realistic.

The primary concern is that incorrect perception of the extent of possible public hazards can lead to non-optimum or even counterproductive policy decisions. It is time to start using technical facts rather than fantasy for policy planning.