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To: Dr. M. Silverberg, USNRC
From: Dr. D.W. Cooper

Re: RADIONUCLIDE RELEASE UNDER SPECIFIC ACCIDENT CONDITIONS
(Volume 1, a PWR Analysis), Draft 14 January 1983

This will summarize my comments after spending about a day and a half reviewing the document in question.

The work is generally very well done, representing the state of the art in most instances, and my comments should not be construed as criticism of the calibre of the personnel preparing the report nor of the general quality of the report. I will not list all the areas deserving commendation, limiting my remarks to those areas that could be strengthened or extended. To repeat, I think the work represents a high level of scientific achievement.

In the paragraphs that follow, I list suggestions for improvement of the report and possible extensions.

In those cases where the aerosols would travel at high rates of speed through piping (such as through the primary system) before being emitted to containment, one could model turbulent deposition to the walls (distinguished from turbulent agglomeration). The preparers must be aware of this work (and may have incorporated it, although I did not see it indicated explicitly).[1]

In all cases where substantial volume fractions of the atmosphere containing the particles are being condensed on walls, etc., diffusiophoresis should occur, taking roughly the volume fraction of the gas and associated particles to the walls. See the recent scrubber literature.[2]

The high radiation fields should produce high concentrations of ions of both signs. In a bipolar atmosphere where the species have equal mobility, one expects particles to come to Boltzmann equilibrium with a mean charge of zero. However, the ionic species typically do not have equal mobilities, leading to a net charging of the aerosol. The differences in charges will lead to enhanced coagulation, and the net charge will lead to electrostatic scattering. The charges will also lead to some collection due to "image" forces between the particles and surfaces.[3]

Convection currents within primary containment should be sufficient to produce a well-mixed situation even with very slow flow, so that the diffusion of vapors from hot to cool surfaces should occur appreciably.

It is stated that condensation was only considered to occur for particles larger than 0.6 μm. Typically, condensation occurs readily for particles 0.01 μm and larger. Furthermore, for particles that contain soluble species, growth occurs even when the atmosphere is not saturated, and once they become droplets, they resist evaporation and solidification.

Equation 7.2 is unconventional, especially the first term, accounting for inertial collection. This term is ill-behaved above Stk of 0.5. Further, at Stk of 0.1 it gives a collection efficiency for a single droplet of 0.23, which is higher than that found by Langmuir and Blodgett (about 0.08) and that obtained from the widely-used expression $Stk^2/(Stk+0.35)^2$ (0.05). Stk is about 1 for a 100-μm-diameter droplet and a particle of 10-μm aerodynamic diameter, so this is not just an academic quibble.

There are situations where concentrations of 10^3 g/m^3 are predicted to exist in the core regions for thousands of seconds (see p. 7-33). Even if "core" should be "containment," this seems incorrect. By my calculations, even the containment would have substantial reductions (factor of ten or more) of the mass concentration in this time frame, due to coagulation and settling.[4]

The release fraction predictions differ sometimes as much as a factor of 100 between the NAUA and CORRAL-2 codes. This deserves investigation. Further, the results should eventually be compared with NUREG-0715, "Rebaselining of the Reactor Safety Study Results."

The interaction of the melt and the concrete warrants further work, too: 1μm seems small for the sparged particles, especially since the particle diameter from such atomization processes is often proportional to the square root of the surface tension, which I think is quite high for metals, higher than the sodium chloride aqueous solution on which some of the estimation was based.

The empirical formula for condensation aerosol production (B-9) may be correct, but I would prefer to see some further analysis, allowing it to be expressed in appropriate dimensionless form, with empirical coefficients as correction factors.

Finally, there are figures that show concentration versus particle size and seem to be frequency distributions. If they are, then they are changes in concentration per change in particle size or *per* logarithmic particle size interval, and this should be made clear. Better would be presentation as cumulative size distributions, although this is less clear for the cases where the distributions are not unimodal.

I appreciated the opportunity to serve on the review group and will make every effort to attend the next meetings, and I will try to spend some more time on the volume in hand.

REFERENCES

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