Specific Comments from Chris Whipple on "Wildfire in the Chernobyl Exclusion Zone: A Worst Case Scenario."

1. The report links four models. Which of the models do you feel competent to comment on? (hopefully, as many as possible)

I am comfortable commenting on all four of the models.

2. Are the analyses in the report adequate? Do they lead to results and conclusions that you feel comfortable endorsing? (Please add any reservations you have here or in #3, #4, or #5.)

Source model

Regarding the source term sub-model, the assumption that the radionuclide content of combustible materials is assumed to be equal to that of the average concentration in the top 30 cm of soil, adjusted for isotope-specific concentration factors, is reasonable. Two issues that might be addressed are, first, whether data exist to permit different concentration factors for different biota, and second, whether a thinner soil mixing layer would have produced a higher source term.

The Resuspension section (page 9) is listed under "Transport model." My reading suggests that the first paragraph of this section fits better under source term, as source terms are usually measured or estimated as release rates.

Transport model

The term "resuspension" was not entirely clear to me. I took it to mean that radionuclides deposited following the reactor accident would be returned to the air by a large fire. But among risk analysts in the U.S., resuspension typically refers to a dust inhalation pathway, where contaminants that have deposited to soil are lifted by wind from the ground surface. As I understand this analysis, a dust pathway based on mobilization of deposited materials was not included.

In addition, following an intense large fire, the CEZ surface soils would likely be more easily mobilized through either wind or surface water runoff than would be the case absent a fire. This was not mentioned.

The use of a Gaussian plume model is appropriate for the comparatively simple screening level of the analysis, and the assumption of a ground-level release point is reasonable. It would be more realistic to include a buoyancy term, given that the release is assumed to occur in a fire.

The treatment of the releases as a point source is reasonable for estimating distant exposures, but is a non-conservative assumption with respect to residents near the CEZ. Such individuals may be only a few km from a major source; the analysis assumes that everyone is at least 25 km from a point source. Software exists that can calculate air dispersion from an area source. A sensitivity analysis to see how much the point source assumption matters for close-in residents would be helpful. For an area source sensitivity case, it would be a good idea to include a buoyancy term since the plume may pass over the heads of the nearest residents.

The transport analysis is very simple – no depletion of the plume through deposition, and no variability in deposition velocity with particle size, which is also a surrogate for distance from the release point. And finally, there is no distinct consideration of wet deposition versus dry deposition. In analyses I am familiar with for radionuclide emissions from U.S. coal-fired power plants, the maximum exposures are always driven by the calculated wet deposition. The likelihood that a heavy rain could create a localized hotspot was not considered.

Exposure model

I think equation 9 is missing a term that accounts for the short duration of the fire, a term equivalent to the O_f in equation 11.

An incidental soil ingestion pathway was not included; a sensitivity analysis can indicate whether this is important for the radionuclides considered. My guess is that it will be a much more important pathway than immersion.

As noted above, dust inhalation was not analyzed.

Cancer incidence and mortality model

My only comment concerns footnote 11. The BEIR VII report was mainly based on the Japanese atomic bombing survivors, so differences between the U.S. and Ukrainian populations may not be relevant. However, the BEIR VII report recommends a relative risk model (in contrast to an absolute risk model) for most cancers. It was not clear how the risk calculations were done. The discussion of whether the excess cancers would be observable (page 22, bottom paragraph) suggests that the proportion of deaths in the Ukraine due to cancer (11%-13%) is much lower than in the U.S.

3. What are the cautions with the report? And, what are its strengths?
4. Do you feel additional or different analyses that should be done before the report is released?
5. For future analyses, can you suggest additional or different analyses that would enhance the certainty of the report, even though the report is certain enough to be released as is—or with minor change/cautions/etc.
6. Please make any other comments and/or general suggestions here.

All of these were discussed under item 2 above.