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To: Dr. Chad Oliver

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The manuscript by A. Hohl and A. Niccolai entitled “Wildfire in the Chernobyl Exclusion Zone: A Worst Case Scenario” describes results of a relatively simple simulation study on the potential effects of forest fires in Chernobyl Exclusion Zone on additional human mortality due to exposure to radionuclides. The study has found that even the worst case scenario (by intentionally over-estimating the effects at various components of the source, transport, exposure, and mortality) suggested an outcome not so catastrophic. It is comforting. The paper is relatively easy to follow, and it will be able to send a strong message to the world that, perhaps, there is no need to worry too much about the fires in CEZ. However, I feel that the paper can become even better if the authors also consider the following.

First, it is probably necessary to mention that somewhat unexpected extreme cases can occur in reality. Although, the model is conservative, and generally assigns over-estimates in the components, it deals only with the mean values after all. The model does not take into account variability of each component. So, we do not have information on the possibility of occurrence of extreme cases (e.g. mortality of unexpectedly many individuals). One way to cope with this is to modify the model so that it includes variability of various components explicitly. (Even the IPCC report on global warming for policy makers talks about likelihood and probability.) It will make the model somewhat more complex, but is worth considering. Then you could say something about the probability of having X number of additional deaths due to fires in CEZ. That will be the useful information to political leaders and policy makers.

Second, I feel that expression at the end of Discussion should probably be changed. It says “Given these background rates of cancer mortality, the additional cancers would not be distinguishable from normal occurrences.” The last two paragraphs of Discussion indicate that the number of increased cancer deaths due to forest fires will be 81 persons. In contrast, the number of deaths without fires is expected to be 396 (= 158 + 238) persons. The ratio 81/396 is considered “not distinguishable” by the authors; however, it will be 20% increase in cancer deaths, and may be considered “significant.” It is a matter of human life. So, if it is stated that the condition is “distinguishable” or “not distinguishable”, at least some statistical tests should accompany the statement. Alternatively, the judgment of its being distinguishable or not should be left to the policy makers.

Third, I feel that the intended audience of this report is not clear. If this is to be presented to scientifically oriented communities, there should be more description and discussion of the results. For example, Tables 6, 7, 8, and 9 present various results in relation to distance from the source. However, there is little description and discussion of the contents of the Tables. Also, scientists

would probably want to know variability of various factors and effects, not just means. On the other hand, if the intended audience is the general public and policy makers, list of numerous numbers as Tables 6, 7, 8, and 9 should probably be omitted. No one would look at the fine print of those numbers in detail, and it is also not easy to understand what they mean. A better way of presentation is the use of graphs that depict general patterns clearly. Important general patterns should also be explained in words in the text. Otherwise, there is no point in presenting those results.

Yours truly,

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