Response to comments about "Worldwide health effects of the Fukushima Daiichi nuclear accident," by John E. Ten Hoeve and Mark Z. Jacobson (Energy and Environmental Science, doi:10.1039/c2ee22019a, 2012)
http://www.stanford.edu/group/efmh/jacobson/fukushima.html

By M.Z. Jacobson (Jacobson@stanford.edu) July 20, 2012.

Responses to Mark Lynas
http://www.marklynas.org/2012/07/fukushima-death-tolls-junk-science/

Lynas: "Jacobson is a long-time anti-nuclear advocate."

Response: This statement is not correct. Lynas confuses advocacy with reporting scientific results. I compare many technologies against each other (e.g., gasoline versus hydrogen, diesel versus gasoline, wind versus coal, ethanol versus gasoline, nuclear versus solar, etc.) and have an obligation to inform the scientific community and public about the results. Results I have reported in papers, talks, and conferences are based on the scientific findings from the studies performed, not because I have a special affinity of one technology over another. I have no financial interest in any technology that I investigate.

Lynas: "The Ten Hoeve and Jacobson paper uses an atmospheric transport model (which is not really intended for this purpose)."

Response: The model used is not an "atmospheric transport model," which is a model that does not simulate meteorology or chemical processes. Instead, it is a coupled climate, weather, air pollution, chemistry, transport, radiation, cloud, ocean, soil, vegetation, and health effects model. It is the most detailed and complete atmospheric model worldwide

(http://www.stanford.edu/group/efmh/jacobson/GATOR/index.html)

and the most appropriate model available to study this phenomenon. It has been used in many health effects studies and evaluated against numerous datasets on local, regional, and global scales.

Lynas: "Hardly anyone I meet in the nuclear community these days still believes in the LNT."

Response: This statement cannot be correct unless Mr. Lynas does not read scientific reports or papers. For example, as recently as April 2011, the U.S. Environmental Protection Agency stated in its report justifying how to calculate radiogenic cancer risk:

> "Underlying the risk models is a large body of epidemiological and radiobiological data. In general, results from both lines of research are consistent with a linear, no-threshold dose (LNT) response model in which the risk of

> inducing a cancer in an irradiated tissue by low doses of radiation is proportional to the dose to that tissue."
> (http://www.epa.gov/rpdweb00/docs/bluebook/bbfinalversion.pdf)

The 2005 U.S. National Academy of Sciences Biological Effects of Ionizing Radiation (BEIR) Report states,

> "The committee concludes that current scientific evidence is consistent with the hypothesis that there is a linear, no threshold dose-response relationship between exposure to ionizing radiation and the development of cancer in humans." (http://www.nap.edu/catalog.php?record_id=11340)

Further, as stated by Professor Burton Richter in his July 2012 E&ES "Opinion" at http://www.stanford.edu/group/efmh/jacobson/fukushima.html, "I agree with the authors' choice. The LNT model is what UNSCEAR and the U.S. BEIR committees use...However, there is no agreement among the critics as to what the threshold should be, and, until there is, use of the LNT assumption should give an upper bound to the biological effects."

In addition, our cancer death range (15-1300) effectively accounts for not including the LNT at the low end. Mr. Lynas would prefer we report only a low estimate, and ignore the high estimate, which is not scientific. Also, Fukushima is not a case of low doses accumulated over a long time but of acute emissions, with most impacts over one month.

Finally, we used a recommended dose and dose rate effectiveness factor of two to all organ-specific risk coefficients, except for the breast, as described on the "Calculation of health effects" study page of the paper. This resulted in lower risk at lower radiation exposure than at higher exposure; thus, we did not assume a linear risk coefficient with exposure but one the provided lower risk at lower exposure.

Lynas: "But my main complaint was the one you didn't address, which was the inappropriate use of LNT and collective dose (via a risk coefficient multiplied by population)."

We never multiplied a risk coefficient by the entire population in a given location.

Our risk coefficients were age-, sex, and organ-specific, and we used the age distribution of each country worldwide and the variation of dose spatially and temporally (including variations within countries) for each radionuclide, such as shown in the video at http://www.stanford.edu/group/efmh/jacobson/fukushima.html and a DDREF of 2 for all organs except the breast. This is a standard way of calculating health effects based on information available to the scientific community at this time, and it is a similar methodology for other types of air pollutants as well.

The risk coefficients were obtained from the U.S. EPA, who themselves acknowledge the correctness of this methodology for calculating risk for cancer at low dose as they

themselves provide results from this methodology for the entire U.S. population exposed to low doses from both their model and the ICRP (2007) model in Table 3-23 of their report at http://www.epa.gov/rpdweb00/docs/bluebook/bbfinalversion.pdf

You selected a quote from ICRP ignoring how it relates to our calculation and without examining how the scientific and regulatory communities apply these risks.

Regardless, the wide range in our uncertainty of mortalities (15-1300) effectively accounts, at the low end, for the uncertainty in using the LNT across subsets of populations and organs, so we see no basis for your criticism.

Rod Adams (citing Health Physics Society): "Estimation of health risk associated with radiation doses that are of similar magnitude as those received from natural sources should be strictly qualitative and encompass a range of hypothetical health outcomes, including the possibility of no adverse health effects at such low levels." My real challenge with your work is that the range of outcomes that you published did not include the possibility of zero adverse health effects."

Response: Figure 1 of our paper at http://www.stanford.edu/group/efmh/jacobson/fukushima.html shows from data at 8 locations between Japan and the U.S. east coast that the concentrations of Cs and I were both 3-8 orders of magnitude above the background levels, so this quote does not appear to apply to these data.

Regardless, recent data have found that approximate 13,646 (35.8%) out of 38,114 Fukushima children had developed thyroid cysts as of the end of March, 2012, which compares with a rate of 0.8% of children in a 2001 Japanese control study (http://enenews.com/now-35-8-of-fukushima-children-have-thyroid-cysts-or-nodules). These data suggest that the large scale exposure was not close to natural.

Rod Adams. "In response to Lynas complaint that you inappropriately used 'collective dose' to calculate cancer deaths you wrote: "We never multiplied a risk coefficient by the entire population in a given location. "That indicates a misinterpretation of the concerns and cautions offered by radiation specialists regarding the use of collective dose."

Response: We did not use a "collective effective dose" as referred to in Section (k) of the Executive summary of ICRP Publication 103. A collective effective dose is defined as the summation, over all subsets of population of the product of effective dose multiplied by population (e.g., http://www.iaea.org/ns/tutorials/regcontrol/intro/glossaryd.htm#D47). ICRP is critical when this quantity is calculated then combined with a relative risk to obtain the health effects of the population as a whole. With collective effective dose, it is assumed (rather than determined) that everyone in a population subgroup receives the same dose. For example, measurements of an individual's average exposure to X-

radiation, as determined by a small number of tests, multiplied by the total country population, give a collective effective dose.

We did not assume the exposure was the same for each individual in a large population based on a sample of data. Instead, we modeled the concentration each person was exposed to in each model grid cell each time step. This quantity was constant for the cell and time step, as the model was not resolved down to the individual. The concentration was multiplied by the inhalation rate of a person as a function of age and combined with the relative risk for each organ and sex to obtain the mortality/morbidity risk of each individual. This variable was then multiplied by the population of the subset with the same inhalation rate and age in the grid cell. In other words, we calculated individual mortality risk and summed this over the population. We did not calculate collective effective dose then multiply it by relative risk.

In the limit of extremely high model spatial resolution, this methodology converges to simulating different concentration exposures for each person in the world. At the resolution used, it gives rise to uncertainties in the exposure of individuals in different parts of the grid cell, but an examination of gradients across adjacent grid cells suggest there is no possible way for the concentration distribution to result in zero risk worldwide due to Fukushima. The only way that outcome could be obtained is if the relative risk were assumed to be zero. However, UNSCEAR, U.S. BEIR, U.S. EPA, and ICRP, among others, all agree that relative risks are not zero at low doses.

Lynas: "In conclusion, I don't like to go in for ad hominem stuff…"

Response: If you don't like it, then you won't do it.

Lynas: "This is not the perspective of an objective energy scientist, but of an anti-nuclear campaigner pursuing an ideological agenda"

Response: As stated, our study was based on pure science and pursuit of better understanding. According to Prof. Burton Richter in his commentary at http://www.stanford.edu/group/efmh/jacobson/fukushima.html, "*It is a first rate job*".