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Interactive comment on "Global risk of radioactive fallout after nuclear reactor accidents" *by* J. Lelieveld et al.

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Introduction

The paper proposed by Lelieveld et al. (2011) and the large number of papers appearing after the recent Fukushima nuclear disaster (for a very gross estimate of global nuclear risks see also Butler, 2011) are a proof of the relevance of the topic and the increasing interest and concern that nuclear energy creates both to society and to the research community. However, although such an important topic requires attention, and work on it should be encouraged, this should be done rigorously and in an interdisciplinary team so that all the relevant aspects – nuclear engineering, dispersion of the radioactive releases and consequence assessment – are fully covered. This is not the case in the paper under review (Lelieveld et al., 2011) which appears to have been done in a rush and making too gross simplifications and assumptions which affect their estimate of the global nuclear risk. Such a simplified approach would only make sense when the work is the first of its kind and there are no better estimates available. However, this is not the case here: over the past decade a number of nuclear risk estimates have been published, based on better assessed data and using more sophisticated approaches (see Andreev et al., 1998; Andreev et al., 2000; Arnold et al., 2011; Baklanov and Mahura, 2001; Baklanov and Mahura, 2004; Flexrisk, 2010-2012; Hofer et al., 2000; Mahura and Baklanov, 2002, Seibert et al., 2010). The only new aspect in the present study is the global view, rather than a focus on one continent (mostly Europe), as in the previous studies. However, this global view does not provide a true risk estimate, basically, only averaged deposition values of two key radionuclide are calculated. This is not surprising, as an approach with an appropriate degree of sophistication increases the necessary research effort and computer time in a non-linear manner. Even covering one continent stretches capacities to a limit, as we observe in our own work. Nevertheless, this does not justify the simplifications and assumptions made. The fact that there is a significant global risk can easily be inferred from any of the previous studies and the numbers produced in the present study do not signify much, as is explained below.

Major problems of the approach of Lelieveld et al.

One of the main points under dispute is the statement that the *"duration of the emission is not decisive for these calculations"*. It would be nice if it were like this, however, due to the nonlinear nature of the relationship between release and risk it is not. Deposition patterns from a continuous, long-term release correspond to wind and precipitation climatologies, whereas the deposition pattern from a release at a given time depends on the weather situation at that time. As many meteorological conditions are sampled in the climatological approach, the resulting deposition patterns will be rather smooth and cover wide areas with relatively low concentration or deposition values. In contrast, a single event will lead to the contamination of defined downwind areas and, for deposition, mainly those where precipitation occurred. Deposition events from point source releases with short duration (hours to a few days at most) are extremely episodic. This means that there will be relatively small but, at least in many cases, heavily contaminated areas. If exceedance of a threshold is adopted as a damage parameter in the risk analysis (which makes sense), this damage parameter is a nonlinear function of contamination. Thus, it makes a big difference whether the damage for each event is calculated and then the frequency distribution is analysed, or whether a mean contamination is calculated and then an attempt is made to infer the damage from this mean. We have studied the degree of nonlinearity explicitly (Seibert et al., 2004). For the areas where the actual concentration / deposition is close to the threshold value applied, the nonlinearity is strong. Especially in a global study, for each NPP there will be regions where this condition is met. It is therefore well possible that, e.g., the population of a major city is contaminated beyond any acceptable limit due to the release of one power plant over just one hour with unfavourable meteorological conditions, while the mean contamination (as determined by a continuous release as in this paper) is below the threshold and signifies no risk for this city. This is a basic flaw that cannot be redressed by more explicit wording of the paper. Unfortunately this simplification makes it impossible to interpret the results of the study in terms of risk.

Another unnecessary simplification leading to erroneous results is the application of the Chernobyl source term to all nuclear power plants. Typically, reactor accidents are characterised by the fractions of the inventory released to the environment, and for so-called large releases this fraction for caesium and iodine is typically on the order of 1 to 10%. Applying the Chernobyl fractions to all reactors would be a much better first approximation than applying the Chernobyl source term in Bq, though even this would be a debatable approach. As it is, the emitted Bq quantities assumed by the authors might even exceed the total reactor inventory for small nuclear plants. More advanced studies take individual source terms into account (e.g. flexRISK).

The statement that *"there is little information besides Chernobyl about the release . . . of radionuclides from a catastrophic accident"* is not correct. The investigation of severe accidents, including estimates of activities released into the environment based on numerical modelling of the accident progression in the plant, is a whole field of science and engineering and calculation of possible releases is a requirement for nuclear power plants. Admittedly, results of such analyses are often classified, but publicly accessible severe accident source terms do exist (Landman, 2007; NUREG, 1997; SSK, 2003; USNRC, 2008). All the routine accident modelling systems used by governmental catastrophy management units, such as RODOS (Realtime Online Decision Support System for nuclear emergency management, see http://www.rodos.fzk.de/rodos.html) are based on such source terms.

One wonders how it is possible to estimate fractions deposited within a 50 km circle from the source with a Eulerian model at 1.1 degree grid resolution. Results from the point-source release will also be severely influenced by numerical diffusion, leading to considerable underestimation of areas with heavy contamination.

The authors do not specify the details of the Chernobyl release assumed for their initial test calculation, i.e. its temporal and vertical distribution. If they have released the activity in the lowest 60 m as they did for the main calculations, that would be far from reality and make a substantial difference for contamination levels.

Deriving an empirical probability of a rare event, such as a large radioactivity release from a nuclear power plant, from two or four instances (Tschernobyl and Fukushima) cannot give statistically reliable estimates of large release frequencies, although we may use this experience to question the large release frequencies from probabilistic safety analyses, as the authors in a way do. Assigning the same probability for a large release to all reactors is a problematic (over)simplification.

Some minor remarks

- The authors have obviously simulated one year, and tracked a continuous release from each reactor starting at the beginning of the simulation. This is not a consistent approach as some material released towards the end of the simulation period will still remain airborne when the simulation was terminated so that not all release periods will have the same weight in the result.
- Hot particles were observed everywhere not only in 30 km zone around Chernobyl; they were even registred in the USA
- Not all iodine is present as I_2 gas.
- The authors do not distinguish between core damage or melt on one hand and large release to the environment on the other hand (for example, the Three Mile Island accident had core melt but not a large release except for noble gases).
- The Chernobyl disaster was not triggered by a typical core melt accident but rather by a strong power excursion, leading to a strong explosion destroying the reactor core.
- Nuclear reactors were operational long before 1954, even though they were military plutonium-producing plants and not for electric power production.

Remarks on references to other publications

The paper attributes inverse modelling of Fukushima releases to Priyadarshi et al. (2011), who focus on the specific issue of S-35 and rely on a simple, somewhat questionable method. The publication by Stohl et al. (2011) who derived release estimates for two major nuclides (Xe-133 and Cs-137) with a state-of-the-art inverse modelling methodology was also quoted, but not in the context of inverse modelling.

In their manuscript, the authors do not mention other studies with similar intention. In their answer to one of the reviewers, they admit then to have knowledge of ongoing and previous projects addressing nuclear risk at different levels and with different methodologies (such as Riskmap and flexRISK), whereas they have not cited or otherwise mentioned them, nor have they contacted the flexRISK project team.

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