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Interactive Comment

Interactive comment on "Assessment of possible airborne impact from nuclear risk sites - Part I: methodology for probabilistic atmospheric studies" *by* A. A. Baklanov and A. G. Mahura

H. van Dop (Referee)

H.VANDOP@PHYS.UU.NL

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General comments

The paper adresses the important issue of risk assessment following a nuclear accident. In the first paper in a series of two, they explain the methodology of their statistical approach. The area of application is roughly on the European scale with a ˜ 2 x 2 degree resolution, which excludes near source risk evaluation.

Mainly based on large numbers of trajectories, a number of statistical measures are proposed and presented. Though the authors give an argumentation for their methodology, I miss the historical context of related studies in the same area. For example, in the ECE-EMEP (Eliassen et al.) programme, quantitative yearly estimates are made (also based on trajectory modelling) of impacts (air concentration, deposition) of a range of air pollutants for numerous sources in Europe. This activity started already in the early seventies and is still on-going. A similar study was performed at IIASA (RAINS model by Hordijk et al.). Also, more recently, an extensive case study was made after the Chernobyl accident in 1986 with the Joint Research Centre in Ispra as coordinator. This case study was followed by a tracer experiment which was used by a large group of modellers to test their models with. I admit that this was a case study, an approach which the authors did not follow, but they could at least briefly have mentioned it. (This case study was followed by a numerical exercise, also led by JRC (Galmarini et al.), where a modelling community could mutually compare dispersion data). I am making this remark, because I think that it would give the paper a perspective which is does not have now. Also the particular choice for the methodology followed in the current paper could then be compared with earlier ones and (additional) reasons given why the authors favour their approach.

The methodology yields maps which identify regions which are at risk after a radioactive release at a specified site. Some hints on how these data can be used in an integrated risk analysis (Obviously a full risk analysis would have to contain all nuclear sites on the continent)

The current paper presents the methodology, which is illustrated with one example from a (the?) Leningrad Nuclear power plant and is followed by a companion paper which present the results from 11 or so other nuclear power plants in the Euro Arctic region. I am still wondering why method and results could not be contained in one paper. This would avoid numerous unnecessary repetitions. Moreover, I do not see that the results obtained from the other ten or so locations contribute much more to the list of conclusions already made.

Specific comments

section 2.2: The authors are not very specific regarding the input data. Why the NCEP data, and why not the ECMWF (reanalysis data)? Data were interpolated (from standard isobaric levels?) to isentropic surfaces. This is always a problem close to the surface. The first 1000 m have often ˜constant potential temperature, which introduces ambiguity in defining isentropic surfaces. Levels range from 255 to 330 K (5296 10) (the atmosphere is not that warm, must be a misprint, even if potential temperature is meant) I would appreciate if the author could mention at what heights the trajectories initially started. This is crucial information, since the wind shear in the first km can be considerable.

How many years were considered? If it is less than three, there can be a considerable climatological bias.

section 2.3.2 The first approach is clear. The second approach is nothing more (or less) than the first. It only contains a normalization so that results are now in % in stead of number of passage. Am I wrong? Please explain.

There is a circular reference (5299 15) and (5300 18). I have difficulty in precisely understanding the third approach.

(5301 17-21): I do not see the isoline $>$ 90 % in figure 2. Moreover figure 2 was already explained to me at the beginning of section 2.4.1 in a slightly different context. (Please explain)

section 2.4.3 What is the advantage of a polar grid, why is it introduced? What is 'detalization'?

section 2.4.4 The section on the removal by precipitation contains ideas only. The first two approaches are to be used in conjunction with a trajectory analysis and the third approach is an evaluation with a dispersion/deposition model. The first approach is tricky since you use the same precipitation climatology for all trajectories: I could imagine that eastward trajectories are much 'wetter' then 'westward' trajectories. Anyhow, the point is that there is a correlation between the direction (and length) of trajectories and precipitation, which makes the application of a precipitation climatology in a trajectory analysis dubious.

The second approach is interesting. It assumes a relationship between increasing humidity (along a trajectory) and precipitation. This sounds reasonable (most precipitation and cloud modelling schemes in modelling are based on similar simple concepts), but I think the approach should be tested first, before applying it in a risk analysis.

I am wondering why the authors (in a trajectory analysis) do not use observed precipitation along the trajectory. (It would not be that difficult to label each trajectory with an observed precipitation history (provided that you have a data base which contains precipitation data (and cloud cover)). Remains the question how two relate these data to the wet scavenging of e.g., Cs or I.

technical suggestions

All figure captions should contain more text enabling the reader to understand what he sees without extensive consultation of the main text, e.g. give the definition of a probability isolines in the caption.

5289 26: 'in this or adjacent area' What is meant here? The Euro arctic region

5291 24: 'Saltbonis' should be 'Saltbones'

5289 17-19 English formulation?

5292 5: 'other': 'another' ?

5294 19: add a reference to Galmarini/Girardi

5295 4: 'firth' should be 'fifth' ?

5296 9: 'terms' should be 'times' ?

5297 15: refernce to Miller (1981) is missing

5305 13 'nuclear' should be 'nuclei'

The use of the English language is reasonable, but slight editing by a native English is recommended (also for part II)

1. There are a number of ?? in the text e.g. on p. 11 which should be replaced by appropiate symbols.

2. Figure captions should explain more of the details in the figures: e.g. give the definition of a probability isoline.

1. Figure 9,10,11 are too small to be readable

2. Figure captions should explain more of the details in the figures

3.

4. There is a substantial overlap with acp2003-083. Merging the two papers and removing some less relevant figures might be considered. (On the other hand, part I is very brief on the conceptual details)

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5289, 2003.

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