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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

Anonymous Referee #3

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The paper aims at presenting a methodology for the assessment of the risk deriving from nuclear power plants in northern Europe.

While a complete methodology is described ranging from probabilistic atmospheric studies to socio-geophysical factors and economical impact, the paper deals, in its main part with the pseudo-climatological study of trajectory distribution. After the Introduction and Section 2 where the full methodology is presented, the paper deals only with the analysis of isentropic trajectories and the presentation of the techniques to determine parameters for the estimate of: airflow and fast transport; maximum impact zone and maximum reaching distance; typical transport times and precipitation factor.

My first criticism to the paper relates to the initial scope stated in the abstract: "The main purpose of this study is to develop a methodology for multidisciplinary nuclear risk and vulnerability assessment and to test this methodology through estimation of a nuclear risk to population in the Northern European countries in case of a severe accident at the nuclear risk site. For the assessment of the probabilistic risk and vulnerability, a combination of socio-geophysical factors and probabilities are considered". I have to apologies with the authors if I have missed something but in spite of the statement presented at the beginning of the abstract I cannot find in the paper anything that actually shows what the authors claim. Indeed the authors list in Section 2 what the methodology adopted is, but in the proceeding of the paper I cannot find any result relating to the application of the full methodology. If this is not the scope of the paper then the authorŠs intentions are overstated and the scope of the paper is different. I see a discrepancy between the initial objectives, the author intentions and the main core of the paper, which only relates to atmospheric transport. Furthermore the methodology presented in Section 2.1, not applied in the rest of the paper, seems to be something established and working since each bullet of the last part of the section is fully referenced. What is then the point of presenting in this paper something that has already been published extensively? Since the references are rather recent and the various bullets are interconnected, it seems to me as if the methodology is already in place and working as well as published.

For what concerns the core of the paper, (from section 2.3 onward) I have also some problems. Recently the authors have published in ACP a paper entitled: ŞMethodology for prediction and estimation of consequences of possible atmospheric releases of hazardous matter: "Kursk" submarine studyŤ with J. H. Sørensen as co-author. In this paper the parameters used to study the trajectory distributions and which are the main aspect of Part I, are the same presented in here as original developments. If this is indeed the case, I see no point in presenting again and with such level of explanation what was already published this year by these authors in the very same journal to which this paper is submitted. I also find very surprising that, in spite of the care of the authors for referencing (though most of the references are internal reports and

ACPD

3, S1876–S1878, 2003

Interactive Comment

technical notes), no citation is made to the abovementioned paper (not even is Part II). What is the difference between the indicators presented in Baklanov et al. (2003) and those presented here? Why no reference is made to that paper which, though relating to the specific case of the Kursk submarine, shows a more complete application of the methodology presented in here? Another reference in the paper shows striking similarities with the present paper content namely: "Atmospheric transport patterns and possible consequences for the European North after a nuclear accident" by Baklanov et al. (Journal of Environmental Radioactivity, 2002a) where a similar methodology is applied to the Kola Nuclear reactors which is also treated in Part II.

According to me the material presented in Part I is not sufficiently original for publication in the journal.

As for Part II, I have also serious problems in finding something original that is sufficient for publication in a peer-reviewed journal. Many of the topics treated in Part I, are presented again in Part II thus making the adoption of a two-parts paper useless.

I personally think that the authors should aim at rewriting the two papers into one, and at paying more attention on stressing the original and scientifically relevant aspects.

I shall avoid to comment in detail the papers since I consider the two objections presented above vital for their continuation in the review process.

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