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Atmospheric Chemistry and Physics Discussions

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

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We thank Seibert and colleagues for the extensive comments and suggestions, though we do not agree with some of them which appear to be based on an incorrect interpretation of our research goals and methods.

Comment: The "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

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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."

Reply: We agree that this is an important topic and also that interdisciplinary teams can make significant progress. We commend Seibert and colleagues for assembling such an interdisciplinary team and for the flexRISK website, which provides much useful information. We also agree that we do not cover every aspect (we do not claim this). Unfortunately, the comments and references cited by Seibert et al. do not provide material that could help advance our particular study. Below we assess all the references indicated by Seibert (those not already cited in our article; note that many are not available or not directly relevant). Our work has not been done in a rush, rather it is done from a different perspective then typical for the nuclear power plant risk research community, using approaches that have been established for examining the comparative characteristics of outflow from numerous point sources worldwide; we claim it is the first of its kind; our assumptions are transparent and can be adjusted if more information eventually becomes available. Of the list of references by Seibert et al., none provides an actual risk estimate based on better data. Some of the work cited by Seibert et al. applies the Lagrangian trajectory model FLEXPART of Stohl et al. (1998). This model uses a different methodology, but it is definitely not "more sophisticated" than our Eulerian model. Lagrangian models have the advantage of reduced numerical diffusion in the large-scale advective transport; however, their representations of important sub-gridscale transport processes such as vertical turbulent diffusion, deep cumulus convection, gravitational sedimentation, and precipitation scavenging are normally inferior to the complex representations in Eulerian models, and in many cases (including the cited version of FLEXPART) they completely neglect some processes like deep cumulus convective transport. Given the critical importance of these processes in the present study, we argue that our application of a Eulerian model is novel, and is an important complementary approach (though not a replacement) to the more standard Lagrangian model analyses.

Comment: 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.

Reply: The global view is indeed new and also the use of empirical information about the occurrence of INES 7 accidents, which allows a risk estimate. We use the contamination threshold of 137Cs recommended by IAEA (2005, 2006), previously used in regional analyses of the Chernobyl accident. We do not understand what is meant by the remark “appropriate degree of sophistication”, since the references provided by Seibert et al. do not provide greater sophistication, only a higher implicit resolution in the Lagrangian models. The remark that “there is a significant global risk can easily be inferred from any of the previous studies” is not substantiated by any of the comments or references provided, as far as we could find. Actually, none of the references presents a risk assessment. Some online (test) calculations (unfortunately without basis in the peer-reviewed literature) are presented through the flexRISK website about the deposition of radioactivity in Europe (available since the website was updated on 10 January 2012, i.e. one week before Seibert et al. posted their comment), although source functions are not available. The latter is no surprise as the only reliable information available about INES 7 accidents is from Chernobyl, and to some degree also from Fukushima. If Seibert et al. would be able to provide quantitative statements that could be taken up in our Table 1, then this would really add to this discussion.

Comment: 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,

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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.

Reply: Seibert et al. confuse the deposition calculation after a single event with our risk assessment. Nevertheless, we tend to agree that our remark “The duration of the emission is not decisive for these calculations” contains implicit knowledge familiar to global modelers, and thus may have contributed to this confusion, and we will change this in the revised manuscript. For our risk assessment we need to account for all possible meteorological conditions that can occur throughout the year. Clearly during a particular event of limited duration the results can be quite different from the annual average. We will explain this more clearly in the revised manuscript and add a paragraph to also show monthly risk patterns for comparison (adding to section 4). We will remove the above sentence and also include the following: “To assess the effect of individual accidents, one would need to simulate the actual emission profile and the actual meteorological conditions, as we have done for the test case of Chernobyl to confirm the model’s representativeness and viability for this study (also see http://flexrisk.boku.ac.at/)”. We do not understand the discussion about “non-linearity” and how it relates to our work. It is unfortunate that the argument is supported by the unavailable (classified) reference of Seibert et al. (2004). We do not make any simplifications in this respect as we simulate a wide range of possible weather conditions. Obviously, during an individual event of limited duration a region directly downwind, in particular when affected by the rainout of radioactive particles, will receive a relatively high dose of radioactivity. We pursue a statistical representation of the atmospheric transport and deposition pathways over the different seasons, in contrast to the deterministic approach suggested by Seibert et al. Therefore our method is not “flawed” but different.

Comment: 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).

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Reply: We agree that this is a simplification. In the revised manuscript we consider the fractional release of caesium and iodine, as suggested (scaling the emissions according to reactor capacity). The differences between the two simulations is not large, although the new, fractional release calculations suggest that more people are affected in W-Europe and less in S-Asia. We will change this in the revised manuscript. Again we emphasize that statements such as flexRISK being "more advanced" are inappropriate, in particular since flexRISK has not published any useful information about the radioactivity releases after major accidents.

Comment: 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.

Reply: We carefully re-checked the references and the RODOS website (which presents a project description) but found no information about the sources of radioactivity after accidents.

Comment: 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.

Reply: We have assumed that the reactors are central in the model grid cells, and that

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the 1.1° resolution is representative for an area with a diameter of 100 km. This is reasonable because the mean meteorological conditions in such an area are expected to generally be similar. Our results are not sensitive to this assumption because most radioactivity (>90%) is transported beyond this distance. Actually, the main topic of our manuscript is to show the long-distance effects of major accidents and consequently the large-scale risks. For example, when one would argue that in a higher resolution model the highly-concentrated deposition of radioactivity near the source is better represented, it would not make any difference to our results as we apply the threshold of 40 kBq/m2 (hence even a multiple exceedance of this threshold would not change the result of the area being "contaminated").

Comment: 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.

Reply: The source profile of 137Cs from Chernobyl, including the emission height, has been adopted from Brandt et al. (2002), which is based on several publications. We mention this in the revised manuscript.

Comment: 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.

Reply: Our method leads to a frequency of severe accidents of once every 10-20 years (based on 440 reactors worldwide), which is not unrealistic considering past experience (and the increased number of reactors at present compared to the historical average). Can Seibert et al. provide a better method to derive "statistically reliable

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estimates of large release frequencies”? We maintain that our method provides more realistic estimates than previous work, although the difference is only a factor of two (we estimate 1 in 5,000 per year, and previous work estimated 1 in 10,000 per year).

Minor remarks

Comment: 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.

Reply: We simulated 1.5 years in total, starting in 2004. The model setup is designed such that first the airborne tracer mass is in “steady state” (which takes about 3-4 months). Thus the first 6 months are used for spin-up and the last 12 for the analysis. Note that at the beginning of our analysis there is already tracer mass present in the atmosphere, which is consequently deposited within the analysis period and is about the same as the tracer mass that is still airborne at the end of the analysis period. We will include this in the revised manuscript.

Comment: Hot particles were observed everywhere not only in 30 km zone around Chernobyl; they were even registred in the USA.

Reply: We agree. However, we specifically state that we limit our study to caesium and iodine, which are not released as hot particles. It is expected that Chernobyl is not representative for non-graphite reactors. Nevertheless, it is correct that we therefore underestimate the total deposition. We will mention this more clearly in the discussion.

Comment: Not all iodine is present as I2 gas.

Reply: We agree. However, this does not have implications as the half-life of other iodine containing gases is the same, and the atmospheric dispersion will be very similar.

Comment: 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).

Reply: We only address INES 7 accidents. Three Mile Island was an INES 5 accident. From our table 1 you will find that 137Cs emissions from Three Mile Island accident are not known, at least not publicly.

Comment: 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.

Reply: Good point. Thanks. We have corrected this throughout the manuscript.

Comment: Nuclear reactors were operational long before 1954, even though they were military plutonium-producing plants and not for electric power production.

Reply: Our study focuses on civilian nuclear power plants as information about military reactors is close to absent. We will note this in the manuscript.

Remarks on references

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 estimatesfor two major nuclides (Xe-133 and Cs-137) with a state-of-the-art inverse modeling 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. Intheir answer to one of the reviewers, they admit then to have knowledge of ongoing andprevious 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.

Reply: We do not comment the other references, which focus on Fukushima. We were not previously aware of the flexRISK and Riskmap activities. In the revised manuscript

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we refer to the flexRISK website.

New references

Seibert et al. present a list of references, indicating that much work has been done in the field that should have been accounted for in our study. A central part of their criticism is that we have ignored previous work. However, the references given appear to be largely unavailable or not relevant. Below we cite from these publications and evaluate their usefulness in view of the present work.

Andreev et al. (1998) Risks due to beyond design base accidents of nuclear power plants in Europe – the methodology of Riskmap. J. Hazardous Materials, 61, 257-262.

This article aims to present a modeling tool to perform risk assessments based on individual NPP accidents. “In view of the uncertainties, emissions are assumed to be released between 50 and 200 m above ground with equal distribution in this interval.” This deviates from our assumption that the emissions take place up to an altitude of 60 m; however, no justification for the 50-200 m is given. This study provides examples of accidental releases, using the Lagrangian trajectory model FLEXPART. The release of radioactivity is tracked for a maximum of 10 days. The total period for simulations encompasses the year 1995 (though no results are presented). The paper actually provides one example of the geographical distribution of the deposition resulting from a release at NPP Cattenom (in relative units).

Andreev et al. (2000) Riskmap. Erstellung einer Karte des Nuklearen Risikos für Europa. CD-ROM, im Auftrag des österreichischen Bundesministeriums für Umwelt, Jugend und Familie, Wien,. Online unter http://www.umweltbundesamt.at/fileadmin/site/umweltthemen/kernenergie/Riskmap/English/Main.htm.

This is the Riskmap project website. It is not clear if this website contains the same information as the CD-ROM. It is quite interesting that the Riskmap website recommends using the “frequency of exceedance of the contamination threshold for Cs-137” as a “risk indicator”. This is exactly what we are doing and which is criticized by Seibert et al.; however, Riskmap applies 185 kBqm-2 and we 40 kBqm-2 (following the IAEA recommendation). The website presents two images, one for the “IAEA scenario” and one for the “RT scenario”, which are quite different (see figure 1), and a ranking of countries initiating risks in Europe, but there is no information how these results were obtained.

Arnold et al. (2011) flexRISK: Lagrangian particle dispersion modelling for the assessment of nuclear risks in Europe. In: C. Lin, D. Brunner, C. Gerbig (Eds.), C14356. Presentations and Posters - AGU Chapman Conference on Advances in Lagrangian Modeling of the Atmosphere, Grindelwald, Switzerland, OCT 9 - 14, 2011. http://www.empa.ch/plugin/template/empa/*/113644.

Power point presentation

Arnold et al. (2012) flexRISK - Flexible Tools for Assessment of Nuclear Risk in Europe. In: Steyn, Douw G., TriniCastelli, Silvia (Eds.), Air Pollution Modeling and its Application XXI, Springer, Dordrecht, ISBN 978-94-007-1358-1, 737-740, 2012.

Publication is not available

Butler (2011) Reactors, residents and risk, Published online 21 April 2011. Nature, doi:10.1038/472400a, http://www.nature.com/news/2011/110421/full/472400a.

This is a news item about Fukushima in Nature online

Baklanov and Mahura (2001) Atmospheric Transport Pathways, Vulnerability and Possible Accidental Consequences from Nuclear Risk Studies: Methodology for Probabilistic Atmospheric Studies. Danish Meteorological Institute Scientific Report, 01-09, ISBN: 87-7478-450-1, 43 p.

The summary indicates the following. “The risks for radioactive contamination and radiological consequences for any studied area are connected with sources in this and adjacent area.” The results are summarized as follows: “We applied for probabilistic

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atmospheric studies two research tools: (i) isentropic trajectory modelling to calculate forward trajectories originated at NPPs (for a multiyear period), and (ii) statistical analysis tools (exploratory, cluster and probability field analyses) to explore the structure of calculated trajectory data sets seasonally and monthly. The results of this study are applicable for the further GIS analysis." This report focuses on northern Europe and the Arctic, presenting suggestions for a methodology and a project plan. It presents examples of air mass trajectories from the Kola NPP, selected air flow probability fields, a typical transport time field at 1 and 2 days for the Leningrad NPP, and indicators of the maximum reaching distances of two NPPs.

Baklanov and Mahura (2004) Assessment of possible airborne impact from risk sites: methodology for probabilistic atmospheric studies, Atmos. Chem. Phys., 4, 485-495.

"The main purpose of this study is to develop a methodology for a 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 sites." Conclusion: "The evaluation of atmospheric transport is given from the probabilistic point of view. In bounds of the probabilistic atmospheric studies several research tools were recommended to apply: (i) long-term trajectory modelling, (ii) a set of statistical methods to analyze trajectory modelling results, and (iii) constructing and mapping probabilistic fields of different NRS possible impact indicators due to atmospheric transport." This paper provides useful recommendations, though does not perform a risk assessment, in particular as it does not address the source function. The above conclusion is in line with our own approach, and in principle supports alternate perspectives and approaches such as ours. We will include this reference in the revised manuscript.

flexRISK (2010-2012) Flexible tools for assessment of nuclear risk in Europe. Online http://flexrisk.boku.ac.at.

Project website that presents a link to calculate the deposition distribution of radioactivity from hypothetical accidents in Europe (updated 10 January 2012). For the source terms it is indicated that “All data concerning nuclear installations are based on publicly available information: rated power, inventory, accident sequences, frequency of occurrence of accidents, releases for selected accidents etc., which are usually provided by the operators. In case of missing information the needed data was supplemented by data of similar installations.” We checked the website and all references, but source terms are not given.

Hofer et al. (2000) Risks Due to Severe Accidents of Nuclear Power Plants in Europe – the Methodology of Riskmap. In: ESEE (Ed.): Transitions towards a sustainable Europe. Ecology - Economy - Policy.3rd Biennial Conference of the European Society for Ecological Economics, 4-6 May 2000, Vienna.Online http://www.wu-wien.ac.at/project/esee2000/PapersPDF/C316.pdf

Conference proceedings by Andreev et al. (2000), actually not by Hofer et al. (2000). The abstract mentions “The present study evaluates the geographical distribution of risks due to severe accidents of commercial nuclear power plants in Europe. The indicator for risk defined in RISKMAP is based on the deposition of the long-lived radionuclide Cs-137. RISKMAP is based on simulations assuming repeated severe accidents with large releases of radionuclides in every nuclear power plant in Europe which are dispersed and deposited over Europe according to historical weather conditions.” This sounds promising, however, the paper presents the following: “The present study has developed a methodology to map the risk due to severe accidents of nuclear power plants (NPPs) in Europe.” For the source term it is indicated “The Cs-137 source terms and release parameters chosen were taken from different source term studies, such as Sdouz et al. (1993).” We checked this reference, which appears to be not accessible, and the source terms are not given. “Transport and diffusion in the atmosphere are calculated with the Lagrangian particle model FLEXPART“. This model is not superior to ours. Main result: “Based on the geographical distribution of risk due to NPP accidents further evaluations and possible ways of presentation of the results could be found. Figures 1 and 2 show examples how the results can be presented.“ Hence a risk assessment is not presented.

Landman (2007) Source Term Treatment for Nuclear Accidents in RODOS PV6.Final Draft, Version 2.0, RODOS(RA2)-TN(04)-04.

Publication is not available

Mahura and Baklanov (2002) A Probabilistic Analysis of Atmospheric Transport Patterns from Nuclear Risk Sites in Euro-Arctic Region, Danish Meteorological Institute. Scientific Report, 02-15, ISBN: 87-7478-469-2, 87 p.

This report presents a “methodology for evaluation of the possible atmospheric transport of radioactivity from the nuclear risk sites (NRSs), ...and combines atmospheric transport modeling and statistical analyses to evaluate possible impact of an accidental release from main NRSs located in the Euro-Arctic region. The main purpose of this study is a probabilistic analysis of atmospheric transport patterns from selected NRSs for the GIS-based studies of region’s vulnerability and risk assessment of the NRS impact.” The results include the “annual boundaries of the maximum possible impact zone indicators after 24 hours of atmospheric transport for the selected NRSs in the Euro-Arctic region”, and an example of 137Cs wet deposition fields for the unit discrete hypothetical release at the Kola NPP during a) 15-16 Mar 2002, 00 UTC, and b) 1 Feb – 1 Mar 2002, 00 UTC. The source terms of 137Cs are not given.

NUREG (1997) Code Manual for MACCS2: Volume 1, User’s Guide, NUREG/CR-6613.

This is not relevant

Seibert et al (2004) Entwicklung von Entscheidungskriterien betreffend die Beteiligung an UVP-Verfahren entsprechend der Espoo-Konvention. Bericht an das Bundesministerium für Land- und Fortswirtschaft, Umwelt und Wasserwirtschaft. Unpublished (classfied).

Publication is not available

Seibert et al. (2010) Flexible Tools for Assessment of Nuclear Risk in Europe – Report 2010. Vienna Scientific cluster Annual Reports. http://www.vsc.ac.at/fileadmin/user_upload/vsc/reports/BOKU-p70079-Arnold-Report.pdf.

Project report presenting a brief description of the trajectory model FLEXPART. Main result: “The production runs have finished and we have started the evaluation of the results. Figure 3 shows an example of the deposition output of one single run (source in Mochovce, Slovakia).”

SSK (2003) Leitfaden für den Fachberater Strahlenschutz der Katastrophenschutzleitung bei kerntechnischen Notfällen. Berichte der Strahlenschutzkommission (SSK) des Bundesministeriums für Umwelt, Naturschutz und Reaktorsicherheit, Heft 37, Urban & Fischer München, ISBN 3-437-22178-7.

Publication is not available

USNRC (2008) NEDO-33201, Revision 4, "ESBWR Certification Probabilistic Risk Assessment," Section 10: Consequence Analysis.

Publication is not available

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31207, 2011.

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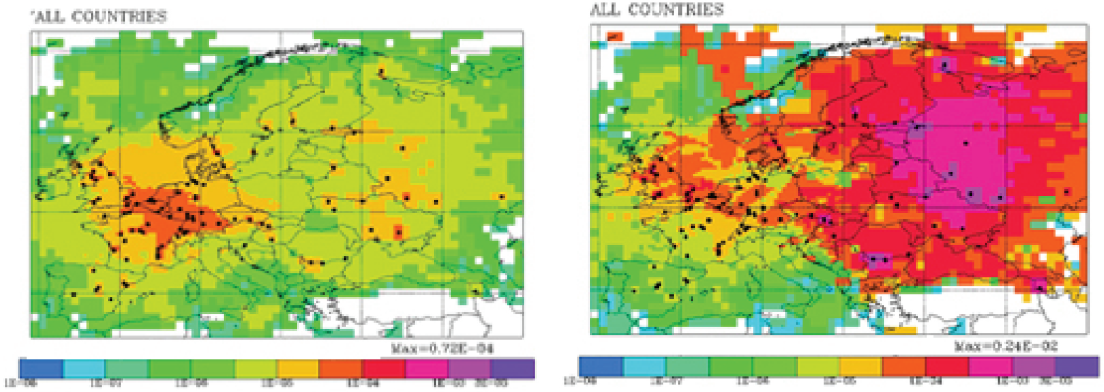

Nuclear risk maps from the Riskmap website; left with the IAEA scenario and right with the RT scenario.

Fig. 1.

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