

## ***Interactive comment on “Modelling transport and deposition of caesium and iodine from the Chernobyl accident using the DREAM model” by J. Brandt et al.***

**R. Draxler (Referee)**

Roland.Draxler@noaa.gov

Received and published: 26 June 2002

### General Comments

The authors applied the DREAM model to test several dry and wet deposition parameterizations against deposition measurements made after the Chernobyl accident. The key findings were that wet deposition dominated the total deposition, being about 10 times greater than dry deposition (no surprise), and that the "simple" dry deposition method and wet deposition method based on RH rather than model predicted precipitation rates provided the best fit with the measurements (a bit of a surprise). The results could be of great interest if they were more fully developed. Some further analysis by the authors would make this paper an excellent contribution.

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## Specific Comments

1) The Abstract does not contain a summary of the major results of the paper. It needs to be rewritten. The text on page 847 (15-25) makes an excellent starting point.

2) The dry deposition results are masked by the much greater values of wet deposition. Is it possible to sort the measurement data into those samples where it rained and those where it did not – hence creating a subset of the data that are only affected by dry deposition. Are there a sufficient number of "dry-only" and "wet-only" samples for analysis?

3) Wet deposition is one of the key elements of this paper. It is not at all evident that the deposition results tell us anything more than the precipitation fields from MM5 were not as good as the RH field! The authors only devote one sentence (Page 843 line 15) to note that they used MM5 to generate the meteorological fields and a reference to indicated better results with MM5 than ECMWF during ETEX. These meteorological models can be difficult to configure and can generate very different precipitation results depending upon the value of just a few parameters. There should be some discussion about how the MM5 configuration might influence the precipitation prediction. More information is needed on the use of ECMWF data for MM5 initial and boundary conditions. For instance, was MM5 initialized once at the beginning of the simulation and then only the BC were updated? Or were initial conditions applied every six hours? Was nudging applied? How were the parameters configured that controlled the precipitation forecast?

4) Although many readers (including this one) appreciate that all the model details are contained in one paper, the paper is too long for the scope of the results. Most aspects of the DREAM model have already been published. Differences between the way the model was used in this study and previous versions were not clearly explained. Sections 2, 3, and 4 could be substantially reduced if only the modifications to the model were explained. Most of the deposition equations appear to be right out of the

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literature (nothing wrong with that) and need not be repeated.

5) The author's results would have a lot more general applicability if they performed the same deposition tests with the original ECMWF data and precipitation fields and observed precipitation. Although the author's mention some of this in the conclusions (Page 849 Lines 22-25) as potential for future investigations, this paper could be substantially enhanced if we knew a just a little more about the measured precipitation compared with the MM5 precipitation. Some simple statistics such as how the MM5 total over the domain compared with the measured totals would help with the data interpretation.

#### Technical Corrections

Page 826 (15) - change "worldwide" to "world's"

Page 826 (22) - delete "therefore"

Page 827 (6-7) - delete "After the Chernobyl accident"

Page 827 (19-20) - delete "as input to the models"

Page 828 (1) - delete "developed"

Page 828 (7) -delete "the treatment of"

Page 829 (4) - Don't Eulerian models also have the same advection errors as Lagrangian models?

Page 829 (15) - What is the depth of the lowest layer?

Page 834 (Eq. 5) - Suggests that mass is removed from the entire vertical extent of the puff when the lowest part of the puff is within the lowest layer. Would this not over-estimate dry deposition?

Page 837 (Eq 11) - The Hanna and Maryon references seem too recent to credit them for this equation. It looks a lot like (the inverse) of Panofsky's 1963 paper (QJRM, pp

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85-94) on the diabatic correction to the neutral momentum profiles. Same with Voldner and Eq. 12.

Page 843 (2-3) - delete "Especially" and "of areas"

Page 843 (4) - delete "therefore"

Page 843 (8) - replace "all the" with "our" and delete "that are included here,"

Page 843 (12) - delete "used in this study"

Page 843 (19-20) - "shows the situation at two day intervals,"

Page 844 (7) - The usual convention is to reference illustrations in sequence.

Page 845 (2) - Regardless of the statistical test results, very few readers will believe that the deposition results shown in Fig 3 have a "significant correlation coefficient."

Page 850 (References) - About half the references are internal reports, commission publications, or conferences. Many of these are almost impossible to find in non-European libraries.

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Interactive comment on Atmos. Chem. Phys. Discuss., 2, 825, 2002.

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