



Interactive comment on “Comparison of modelled and monitored deposition fluxes of sulphur and nitrogen to ICP-forest sites in Europe” by O. Westling et al.

O. Westling et al.

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1. The paper reports a comparison of a model (EMEP MSC-W) calculating deposition of sulphur and nitrogen compounds with measurements from an independent data is made for both plots within the forests a comparison could only be made with data outside of the forests. In general, the model and observations correlated well, but some sites showed large discrepancies. These are mainly ascribed to differences in model estimates and and measurements of precipitation
2. The work is clearly relevant for Biogeosciences and especially for the work on modelling nitrogen exchange in forest ecosystems. The paper is written in a straightforward way and the results presented clearly. The figures showing the geographical distribution of modelled and measured deposition gives a very clear overview of the results.

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3. Since the comparison for the N compounds are only made with stations outside of the forests, I miss a discussion of the possibility to model the true N deposition to the forest floor (i.e. throughfall + stemflow). It is mentioned that models of canopy exchange of N-compounds do exist, but that the uncertainties are very large. However, has any attempt been made to incorporate such models?

Reply:

No attempt has been made to model the total N deposition (from the measurements of throughfall) to the forest floor for comparison with the model calculation. This comparison was not attempted because of the significant uncertainties which would be involved in this, as noted in the text of section 2 (and citing Erisman et al., 2005). We have attempted to clarify the text concerning this in the paper.

4. p. 939, l. 23: It becomes clear that the two years 1997 and 2000 were chosen for the comparison. Please state this earlier in the section and explain why especially these two years were chosen.

Reply:

The two years were chosen because of their different precipitation patterns. We have added text to explain this early in section 2.

5. p. 941, l. 16: What are the different coniferous (2) and deciduous (2) classes used in the model? The results presented in the paper only shows one class of coniferous and deciduous forests, respectively.

Reply:

The EMEP forest classes are now explicitly written out, as temperate/boreal coniferous, Mediterranean coniferous forests, temperate/boreal deciduous, and Mediterranean broadleaf. Within ICP, the dominant tree species for the conifer-

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ous forest plots is Norway Spruce (*Picea abies*), and the dominant tree species for deciduous forest plots is Beech (*Fagus Sylvatica*).

6. p. 943, l. 27: It is suggested that "some of the poor correlation between modelled and measured precipitation is likely associated with ICP precipitation [measurement] methods". The evidence for this is, however, not given in the paper. Since the amount of precipitation is extremely important for the determination of (wet) deposition, I suggest that the procedures of EMEP and their differences and possible failures are outlined in the paper.

Reply:

We agree that this is a crucial topic, and have added more text and analysis on this subject throughout the paper. One region where the discrepancies were quite large is Sweden, and here we have been able to quote previous Swedish studies which have looked in detail at the ICP data. In particular, these studies have shown that the precipitation amounts collected in Swedish ICP sites in wintertime can be significantly overestimated, and this can help to explain the discrepancies seen in this study.

We have also noted though that most of the precipitation data has a rather small range (700-1300 mm), whereas deposition data shows much greater variation. This means that we can obtain good correlations for deposition parameters despite relatively poor correlations for rainfall amounts.

7. p. 944, l. 22: Systematic differences are found for N deposition throughout quite large areas (southern Norway and Sweden). From the maps this difference is a substantial underestimation of the deposition by the model. Whether this is because the measurements show higher concentrations or higher precipitation in these areas is not clear from the paper. It would be valuable to expand the discussion on these discrepancies.

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Reply:

As noted above, we have been able to reconcile many of the differences seen in southern Sweden in terms of problems with wintertime precipitation samples, especially for S. This is now discussed extensively in the text.

8. p. 947, l. 2: If the differences between modelled and measured deposition of N compounds are due to differences in precipitation amounts, why is this then not also a similar problem for SO₄-?

Reply:

We have re-analysed the data in some detail, and have now written the conclusions in a more balanced way. For sulphur we have shown that differences in precipitation amounts can account for a large part of the discrepancies found for the open-field deposition amounts. For nitrogen compounds, precipitation differences cannot account for all the differences between ICP and EMEP.

9. p. 939, l. 18: Word missing, change to "It should be noted ...".

Reply:

Corrected.

10. p. 941, l. 3-4: Change: "...input data used by the all model versions..." to "...input data used by all the model versions..."

Reply:

Corrected.

11. p. 948, l. 20-21: Language suggestion: Change: "In general similarities between ... were remarkably good..." to "In general correlations between ... were remarkably high..."

Reply:

Done.

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