



## ***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.***

**Anonymous Referee #5**

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### General comments

The paper reports a comparison of a model (EMEP MSC-W) calculating deposition of sulphur and nitrogen compounds with measurements from an independent data set (ICP-Forest). The comparison for  $\text{SO}_4^{2-}$  is made for both plots within the forests and outside whereas for  $\text{NO}_3^-$  and  $\text{NH}_4^+$  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 measurements of precipitation.

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

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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.

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?

#### Specific comments

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.

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.

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.

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.

p. 947, l. 2: If the differences between modelled and measured deposition of N-

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compounds are due to differences in precipitation amounts, why is this then not also a similar problem for  $\text{SO}_4^{-2}$ ?

#### Technical comments

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

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

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

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