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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 #3

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General comments:

The paper presents the evaluation of the deposition fluxes of a selection of species by comparison with a dataset of measured deposition fluxes over European forests. The importance of the overall goal of the study, to evaluate these kind of models as extensively as possible is obvious but what really misses in the document is a proper identification of the major shortcomings in this kind of modelling exercises. What is learned from the evaluation; what should be the priority of future research and model development to really improve the predictive capacity of the EMEP model? At the end of the analysis it is clear that this evaluation relies heavily on the representation of the precipitation in the model but that also the quality of the measured precipitation, and consequently deposition, is questionable. Is the applied ICP dataset then actually a proper dataset to conduct this evaluation? Are there no alternatives? At the end the perception is that the model is doing a reasonable job in simulating the observed deposition patterns but there is no clear indication how to proceed with these findings. It seems that the highest priority of further research on the deposition in the EMEP model is the representation of precipitation in the model, e.g., including the sub-grid scale variability, but this is not addressed at all. Actually, the introduction of the model should include a more detailed description of the representation of the hydrological cycle (or at least the precipitation) in the model since this is obviously a key component of the analysis.

The paper contains in general too many acronyms, references to programs/projects which for sure don't help making the document easy to read. In the specific comments found below I indicate which sentences should be reformulated to make this statements easier to interpret for those readers that are not so introduced in the European air quality community.

Specific comments:

As already indicated in the first quick-review but possibly not communicated to the authors: There are many acronyms used throughout the document which are not all known to the reader. They are explained throughout the text but for example the abstract contains already from the start a selection of acronyms which should be written out explicitly or replaced by a short description, e.g., "a completely independent dataset of deposition measurements over European forests"

Introduction; in the abstract the relevance of the EMEP assessments to the UNECE and EU is explained in rather straightforward way; emission control strategies, whereas in the introduction in a long sentence only the UNECE and EU program names are fully explained somehow hiding the actual relevance.

Pp 935, line 19: And what is measured on the other 100 sites? Obviously not wet deposition but what other parameters are available for model evaluation: dry deposition fluxes and concentrations?

When the two aims are being mentioned I feel a more explicit motivation should be given why there is a focus on the evaluation for forest sites. Are there any scientific reasons to focus on the forest sites? Why are non-forested areas not included in this evaluation; are there indications that the model has problem reproducing especially those fluxes over the forests?

Pp 936: line 1: What is level II monitoring? In other words, what is the the difference with level I, there should some more general explanation since this is just probably information available to the community working with these measurements.

Line 4/5: This sentence is an example of too much terminology containing too many acronyms which only confuses interpretation of what is actually meant/stated. This sentence should be rephrased.

Line 10: I guess that they have to acknowledge the cooperation with the other projects but would suggest moving this to the acknowledgements.

Line 15: It is not only physical and chemical process, but especially also dynamical processes with turbulent exchanges especially controlling the deposition of particles but also of the reactive and soluble gases or are the authors in this context more specifically addressing the actual removal at the surface?

Line 29: The statement about the difficulties related to the NH3 emission inventory is interesting. After having read the section results you wonder to what extent this is also a possible cause of the differences between the simulated and observed N fluxes in this study. This actually would be one of the points of discussion of the priorities of future research: to what extent are the discrepancies explained by differences in meteorology versus possible misrepresentations of the main precursor emissions such as SO2 and NH3.

Pp 938, line 4: What is meant with canopy exchanges; it would be useful to shortly elaborate what canopy interactions (emissions, dry deposition, chemistry etc.) occur and how they can affect wet and dry deposition. And how large is the uncertainty in modelling the canopy exchanges: is it anyhow possible to quantify this but if so if it would be interesting to compare this uncertainty with the differences between modelled and observed throughfall. This also brings me to my next point: It is stated in the beginning that the focus is on deposition (so wet and dry) over forest sites but the actual comparison for N species is done for open field sites comparing the EMEP wet deposition with measured bulk deposition. This raises some questions: what is the expected difference between the actual forest and field bulk deposition? How much does dry deposition contribute to the measured bulk deposition of N?

Pp 943: line 27/28: the last statement about the poor correlation to be likely associated with ICP precipitation sounds odd: Is it suggested that the measured precipitation data are not good? Or are there other measured precipitation data also used to evaluate the EMEP model and which seem to be of better quality giving a better correlation between the model and observations?

Pp 944: line 14; the unexpected overestimate of the sulphur deposition by EMEP is indicating a too large simulated sulphur concentrations in the atmosphere, or not? Then the question arises what causes this too large sulphur content; too little deposition can be excluded which leaves the SO2 concentrations possibly due to too large SO2 emissions. It would be nice to see a more detailed analysis of the possible causes of these discrepancies.

Pp 945, line 25: These last findings are really interesting an are really calling upon a more detailed discussion on the possible explanations of this larger modelled SO4– dry deposition compared to the ICP forest observations: turbulence, sedimentation of course all largely dependent on the actual and applied model aerosol size (distribution) of sulphate aerosol.

Pp 946: line 1:3; The statement in this line expresses in a very straightforward way

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what was explained in a less direct way in section 2. I indicated there that the role of canopy processes for interpretation of N deposition should be discussed in a little bit more detail but including there this sentence clarifies most of the role of canopy interactions for interpretation of throughfall data.

Line 7-8: I feel that this interpretation of possible data-problems must be better justified. Reading over again the explanation of the PVI, where it is stated that a high PVI is pointing on a potential problem with the data collection, it still seems that one should perceive the actual measurements as the reference where the model precipitation contains so many uncertainties with respect to the temporal and spatial variability. Is there any other potential evaluation dataset available, e.g., regional scale weather forecast model output?

Pp 947: Line 7-8: How can one expect a comparison of simulated and measured precipitation concentrations to more optimally reflect the performance of the model when there are large differences between the observed and simulated rainfall? If the model significantly underestimates the rainfall, I expect the simulated concentrations for a similar atmospheric burden to be larger compared to the observations and vice versa.

Actually the statement at the end of 4.4 summarizes it all; This evaluation depends so much on a realistic evaluation of the precipitation data that the highest priority of such analysis is a more extensive evaluation of the model as well as observed precipitation using alternative reference datasets, see my comment above.

Pp 948: From the last statements of section 4.5 I would conclude that this evaluation is strongly limited by the quality of the precipitation data and where you can question if the variability being calculated by the model is within the uncertainty range of various different datasets being available for its evaluation. If there are clear indications that the ICP network precipitation measurements are prone to possible sampling errors can you then really use those data for a comparison of the model at the site scale. Would it for example to be more valid to apply area average measured fluxes?

Interactive comment on Biogeosciences Discussions, 2, 933, 2005.