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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.

O. Westling et al.

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1. 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 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.

Reply:
These are good points and we have attempted to address them in the revised version of the paper. The conclusions section has been expanded to discuss in more detail the lessons learned and priorities. Further text on the quality of the ICP (and EMEP) measurement network data has been added. A more detailed evaluation of the data has shown that some of the problems illustrated in this paper are likely due to the problems of sampling, rather than modelling, precipitation in winter conditions. Text has been added to reinforce this point.

Concerning the hydrological cycle, we do not go into this level of detail as the focus of the paper is the comparison of data, and the hydrological cycle of the underlying meteorological model involves very complex discussions. Further, we do not believe that such a discussion would help very much. The problems with precipitation are due mainly to two factors: (1) that precipitation can show very large sub-grid variations over an EMEP grid square, whereas the model has only one value for the grid element's precipitation; (2) there seem to be problems with the ICP data themselves, especially in wintertime, as noted above. These problems with precipitation simply reinforce that this is one of the most challenging areas for meteorological models and observational networks.

2. 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

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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.

Reply:
We have tried to remove acronyms as far as possible, but some are inevitable and hard to avoid even in the abstract (EMEP, MSC-W, ICP)

3. 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”

   Reply:
   As noted above, we have tried to remove acronyms as far as possible, but some seem inevitable, in particular EMEP, MSC-W, and ICP in the abstract. UNECE, RAINS and some others were removed.

4. 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.

   Reply:
   We removed the reference to UNECE and RAINS, so hopefully the main idea of supporting emissions control strategies stands out better now

5. 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?

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Reply:
The other sites report for example ozone, VOC, or heavy metals. Since only the wet deposition network is of interest for this article we have left the text unchanged.

6. 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?

   Reply:
   There are strong environmental reasons to focus on forests, and these are now given explictly in section 1. Further, forests present a particular challenge for models since the contribution of dry deposition is typically much larger than for other ecosystems (because of their large roughness and effectiveness in trapping particles).

7. 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.

   Reply:
   Text has been added to explain more about ICP monitoring and the level I and II terms.

8. 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.

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Reply:
The text has been simplified

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

   Reply:
   The text is deleted, since the acknowledgements cover this

10. 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?

    Reply:
    We had intended turbulence to be covered by 'physical' processes, but have now simply kept 'complex processes', deleting the physical and chemical words. The given references provide plenty of examples.

11. 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.

    Reply:
    We agree. We have added text on this to the conclusions

12. Pp 938, line 4: What is meant with canopy exchanges; it would be useful to shortly elaborate what canopy interactions (emissions, dry deposition, chemistry

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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?

Reply:
With 'canopy exchanges' we mean canopy uptake on the surface of the foliage. This clarification has been added to the paper. We have also added further text and references concerning this point. Further, we have added Fig. 2 to put the measurements in more context, showing also that it is not straightforward to interpret the open-field measurements and possible contributions from dry deposition, and added extra text concerning the possible contributions of dry deposition.

13. 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?

Reply:
Yes, some of the problems of this intercomparison stem from problems with the ICP precipitation data. ICP themselves have explored this and come to the conclusion that measurements have large uncertainties. We have added more text to explain these important points, especially with regard to Sweden, where the dis-

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crepancies are most obvious. For this region we have been able to cite Swedish comparisons with data from the national Meteorological Institute, and can explain many of the problems in terms of known problems of wintertime precipitation sampling. Further, new figures have been added to illustrate the differences between depositions and concentrations in precipitation, and to help aid these discussions.

14. 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.

Reply:
Actually, since both measurements and the model have uncertainties, the differences found are in most cases not so great. Mean differences of around 5% as noted for total deposition of sulphur are well within such uncertainties. However, the analysis of these differences has been improved in the revised manuscript.

As noted above, we have added Fig. 2 to put the measurements in more context. We have also added some text about the puzzling discrepancy for deciduous forests, and the need for more research for the latter. We have removed the reference to differences between the average and mature forest stands in this paragraph, as we now feel that other factors have more importance for this discussion.

Discussion of differences for individual areas, and in particular for Sweden, has been extended greatly, as noted above.

15. 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 mod-

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elled 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.

Reply:
This discussion has now been resolved more in terms of the differences in precipitation (and problems with open-field sampling, especially in winter). In the new manuscript we have not tried to interpret data differences in terms of dry deposition problems. Fig. 2 has been added to explain partly why such a discussion is difficult, and in general there is more acknowledgement in this paper of the uncertainties surrounding measurements.

And as noted above, we have added further text concerning the discrepancies in Sweden, which can be traced to a large extent to problems of wintertime precipitation sampling.

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

Reply:
This sentence has been incorporated into section 2, to improve the explanation of the role of canopy interactions for interpretation of throughfall data.

17. 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

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and spatial variability. Is there any other potential evaluation dataset available, e.g., regional scale weather forecast model output?

Reply:
Given that the evaluations of e.g. Erisman et al. (2003) and Draaijers et al. (2001) found serious problems with precipitaion measurements within the ICP network, we cannot regard these measurements as a perfect reference. We believe that both the model and the measurements have weaknesses, and these two estimates of deposition complement each other to some extent. Comparison for individual sites is very difficult because of these problems, but it is still useful and impiortant to check the overall consistency of the two deposition estimates. We have expanded the text concerning the EMEP measurements of precipitation, as this gives another dataset against which the EMEP model (and hence, indirectly the ICP deposition estimates) can be judged. We have also added text to explain why estimates of pollutant deposition can be modelled with better accuracy than estimates of precipitation itself. And as noted above, we have added further text concerning the discrepancies in Sweden, which can be traced to a large extent to problems of wintertime precipitation sampling.

18. 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.

Reply:
As the range of the deposition data was much greater than than of precipitation, the poorer results for precipitation do not have a major influence on the correlations found for deposition or concentrations. Text is added for this point.

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19. 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.

Reply:
Apart from the extra text mentioned above, we have added comments about this to the conclusions.

20. 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?

Reply:
There are clear uncertainties in this dataset which limit the comparison. However, both the EMEP model and the ICP deposition data have limitations. We believe that the EMEP measurement network has somewhat better data quality, but this data also has its problems.

The idea of looking at area-average measured fluxes is interesting, but even with the number of ICP sites used in this study, there are very few EMEP grid squares with more than one ICP site. However, we believe that the current approach of displaying scatter plots for all parameters provides a good way of looking at the network-wide agreement, without too much focus on the individual sites.

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