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Atmospheric Chemistry and Physics Discussions

Interactive comment on "

Dry deposition of reactive nitrogen to European ecosystems: a comparison of inferential models across the NitroEurope network" *by* C. R. Flechard et al.

Anonymous Referee #1

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General Comments

This is a well written and well organized paper. The model descriptions are extensive and a lot of material is covered in the paper. The approach of using the same site data for each model is informative. The paper contains a wealth of information. It is very informative to have so many sites included and to have them grouped by land cover type. While all major inorganic reactive nitrogen (Nr) species are covered, in the end the main focus of the model analysis, comparison and interpretation with observed

data and commentary on research needs is on ammonia. While this is very valuable and important in the European context, it would have been valuable to spend some effort interpreting the model results for other major Nr species. With the inclusion of many sites covering four major land cover types and addressing annual Nr deposition this paper makes a valuable contribution to the literature.

The section on reducing uncertainties narrows down to only discussing NH3 and NHx fluxes. Are there no flux data for other species besides NH3? This points out a real need on the flux measurement side. It would be helpful to see something more added about HNO3, even the need for more extensive flux studies to reduced uncertainties (more than within canopy air chemistry). Hopefully the authors are considering more interpretation, analysis-related papers to mine the information presented here.

Specific Comments

The title creates an expectation that some type of mapping exercise is to be undertaken to provide deposition estimates to ecosystems across Europe, which is not the case. A more accurate title might be: "Dry deposition of reactive nitrogen to four European ecosystem types: a comparison of inferential models across the NitroEurope network."

Page 10, line 19: This gamma value seems awfully precise. On page 17, use of this stomatal compensation point was deemed inappropriate when fertilizer was applied. Thus, this is for periods other than when fertilizer was applied, later termed "background conditions". This restriction should be noted here.

Page 13, line 8: It would be very helpful to have a map of the sites somewhere, either in the main body or the supplemental material. This will help the non-EMEP reader interpret differences in conditions due to geography.

Page 15, line 7: It should be noted that the NO2 measurements from a chemiluminescence system will be biased high due to interferences from PAN and (unknown amounts of) HNO3.

Page 15, line 31: While the NH4+ fine fraction may be higher than the SO4= fine fraction in the Netherlands (sea salt influence?) one would not expect this for more continental conditions. Are there no measurements elsewhere to be used?

Page 17, lines 12-14: A reference would be useful. One example is: D.R. Matt and T.P. Meyers, 1993. On the use of the inferential technique to estimate dry deposition of SO2, Atmospheric Environment, vol. 27A, No. 4, 493-501.

Page 17, lines 23-25: Was this approach tested by taking a complete period and dropping out some hours/days to approximate how well this handled the issue? The tone of the sentence suggests not.

Page 17, lines 29-30: Were the fertilization months excluded in the annual deposition total for all four models? The discussion suggests yes, but it would be helpful to explicitly state that here.

Page 18, lines 4-6: The point about inferential models over-estimating annual deposition where fertilizer application is involved is an important one. It would be useful to note here that this will be illustrated in Section 3.3. This point will also be valid for vegetation having non-negligible compensation points. However, this point is never mentioned again in the Reducing Uncertainties section or in the Conclusion section and it should be.

Page 20, line 4: It looks like the IDEM model predicts larger values in Autumn as well.

Page 23, line lines 27-30: Is it possible to provide the percentage of the dry Nr deposition from NO2, PAN and other NOy species at the CAPMoN sites so that we are comparing apples to apples? CDRY has a large fraction attributed to NO2.

Page 23, line 31: The 10-15% range seems only to apply to CBED, EMEP-03 and IDEM. Also, it appears that the top of the range is more like 20%.

Page 24, line 7 and lines 11-12: Per page 17 comment, were periods of fertilizer omitted here? Please clarify for the reader.

Page 24, lines 22-23: The inclusion of atmospheric N deposition in the list for emission potential (via the “or”) seems like mixing apples and oranges. The compensation point response to N deposition is not the same as the emission potential, gamma.

Page 24, line 24: Suggest an insertion for clarity: “. . . atmospheric Nr inputs in excess of fertilization, since . . .”

Page 25, line 5. Should note these fluxes are for summertime as well as daytime.

Page 26, lines 3-8: There is not very good agreement between the throughfall and the flux measurements. This section illustrates the problem with using throughfall to provide a reasonable evaluation of the dry deposition estimates.

Page 27, lines 1-14: It seems worth noting here that, given that there is likely some compensation point existing year round, then one would expect the models to predict high relative to the flux measurements, even for the background deposition. But they were mostly low. CBED is the exception. Only IDEM was high. A little more interpretation would be useful.

Page 28, line 16: by “more extensive coverage” do you mean more varieties of land-use?

Page 28, line 17: What species were measured at the intensive Nr flux measurement sites?

Page 28, lines 31-32: Good point.

Page 29, line 2: Would “inter-model differences” be better than “inter-model discrepancies”? Discrepancies suggests we know where truth is.

Page 29, line 8: The statement “broadly comparable with the ensemble average” seems to refer to the ability to capture gross differences in deposition to different land cover types. There is a large inter-model variation at all of the flux sites and at 3 of the 5 sites the models seems to be biased low (when they should be biased high). A more

nuanced conclusion would be helpful.

Page 29, line 25: It seems that organic nitrogen should be noted. What about "... uncertainties in wet deposition estimates, including the lack of WSON, add to the ..."

Technical corrections

Page 2 line 9: Insert "s" on "non-stomatal pathway are".

Page 2 line 24: Suggest replacing "and" with "due to increased" to read "concentrations due to increased emissions by..."

Page 22, line 19: Change "that" to "than" Page 22, line 23: Change "dry deposition to" to "dry deposition as"

Page 25, line 8: At the end of the line should "known" be inserted? "... contribution of PAN and other known atmospheric organic nitrates ..."

Page 26, line 7: Should the second kg N be 18 kg NHx-N /ha/yr?

Page 26, line 10 and 11: Should these be kg NH3-N /ha/yr?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29291, 2010.