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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 #2

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This manuscript presents estimates of the annual dry deposition fluxes of reactive nitrogen. The fluxes are calculated using the inferential technique, which combines measured concentrations and modelled deposition velocities. The concentration data are obtained from an extensive network of 55 sites across Europe. Four different dry deposition models are compared. The results of this study constitute an important contribution to the understanding of nitrogen balances and ecosystem loads. They also demonstrate that there are still large uncertainties involved in the quantification of ni-

trogen deposition. The paper is scientifically sound, clearly written and suitable for the scope of ACP. I recommend publishing it after the authors have properly addressed the following comments.

Major comments

(1) I would like to see a more coherent assessment of the main differences between the models. This kind of model comparison is stated as a key objective of the study (end of Introduction). The present version mostly points out differences in the results rather than explains their origin in terms of model formulations. Based on the current understanding of controlling processes, the plausibility of different parameterisations could be evaluated, rather than inherently assuming that all models are 'equal' (directly comparable) and thus the variation between the results could somehow be considered as an estimate of model uncertainty. In many respects, however, the models cannot be considered comparable. For example, in CDRY the parameterisation of the cuticular resistance of SO2 is directly adopted for NH3 without any justification from the measurement data (based on the scaling approach by Wesely who also did not have any NH3 flux data). This must be considered an ad hoc approach as compared to the actual parameterisations derived from an analysis of experimental data.

(2) The present discussion of the model differences seems somewhat illogical in that on one hand the authors argue that the models have been developed based on diverse data, resulting in large variation in results (p.29296, 3-5; p.29300, 23-27; p.29316, 25-26), but on the other they explain that there are very few datasets for model development (p. 29316, 19-21). As the datasets indeed are few, one would assume that the same data have been used for the development of different models and thus the predictions would tend to converge rather than diverge.

(3) The paper would benefit from a more complete and unified description of the deposition models. This could be included in the Supplementary material. Just listing the different resistance terms with notation varying between the models (p.29300, 11)

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and referring to variables not defined in the present paper (e.g. p.29301, 5) is not very helpful to the reader.

(4) As the focus of the study is on the annual deposition fluxes, long-term datasets have only been included in the model validation (Section 3.3). However, it is questionable if these datasets can actually be considered long-term measurements. According to Erisman et al. (1996), the data coverage of the NH3 flux data at NL-Spe is as low as 17%. At other sites the coverage may be better, but still is far from perfect. This means that, in order to obtain annual estimates, the measurement data must be complemented by inferential modelling, i.e. the technique used in the present study, but the available data indicate significant biases in the model predictions (Erisman et al., 1996). For a more realistic view of the data availability, the implications of data coverage and gap-filling should be discussed.

(5) The authors mention in the end of Section 3.4 that Nr flux measurements have been made at some sites of the NitroEurope network, but apparently these data have not been available for the present study. It might be useful to explain this in the introduction where NitroEurope is discussed, as the scarcity of validation data is a key limitation and the readers aware of this project may expect to see these new flux data to be used for model evaluation.

(6) In several places the chemical transport models (CTMs) are referred to, as the same parameterisations are used both in inferential flux calculations and CTMs. In particular, the model description (Section 2.1) starts with a discussion of CTMs. However, this discussion does not add anything significant and in some points may just appear confusing (and logically should be extended to equally relevant topics such as the treatment of the sub-grid scale). For example, it is obvious that modelled concentration data and grid-square-averaged meteorological data are used for CTMs, while inferential modelling is preferably based on directly measured input (Section 2.1). It is also obvious that 'surface' can be defined in many ways (Section A4), but this does not mean that the same flux-gradient profiles could not be applied within numerical weather

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prediction models (or subsequent off-line calculations) for physically consistent definitions. There is also some incoherence in the related terminology (parameterisation, model, module, routine, SVAT) throughout the paper.

(7) The implications of the sensitivity tests should be expressed in a more explicit way. The sensitivity to the definition of the nominal surface height is striking, but this also depends on the reference height of the data used for the parameterisations.

Specific comments

p.29296, 24-.: These references do not indicate that CBED and IDEM are "used within chemical transport models".

Section 2.1.1: It would be more logical to start from Eq. (1).

p.29298, 17; p.29307, 14: The figures should be numbered consecutively in accordance with their appearance in the text.

Eq. (3): This function is not used in all models. The light response term is different in EMEP-03, while CBED does not include the f_e and f_w terms.

p.29299, 20, 25: If G_s =1/R_s is the bulk stomatal conductance (with LAI saturation), then R_s is not expressed on a unit leaf area basis.

p.29300, 15: In Eq. (3), R_s is defined for trace gases, not water vapour. The measurements of stomatal conductance are for water vapour, not trace gases.

p.29307, 11-13: It is difficult to understand why the model default LAI values would be better estimates than the direct measurements.

p.29314, 16: If G_s denotes the stomatal conductance of water vapour, then the notation should be changed, since R_s = 1/G_s includes the diffusivity scaling. Otherwise, the trace gas considered must be specified.

p.29321, 24-29; 9.29522, 16: It is not clear whether the authors consider the throughfall

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data reliable for the estimation of dry deposition fluxes. It is explained that lots of such data would be available that "may provide estimates of dry deposition, though uncertainties are large", but these data are not used. However, throughfall data from BE-Bra are used for evaluating the model performance at this site.

p.29322, 18: What is meant by 'total' here?

p.29322, 19: I cannot find any data for 1994.

Table 1: Why the 'measurement type' is not specified? It makes little sense to compare one-sided and total LAI values.

Fig 1c: It is not possible to seee the differences between the models for SN, G and C.

Fig. 4: This figure is unnecessary. It is not discussed in the text and the same data are presented in a table.

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