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Model Development

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Discussions

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Interactive comment on "The effect of satellite derived leaf area index and roughness length information on modelled reactive nitrogen deposition in north-western Europe" *by* Shelley C. van der Graaf et al.

Anonymous Referee #1

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The authors present a modeling study in which z0 and LAI were replaced in a version of the LOTOS-EUROS model for Nitrogen deposition. They find that their changes to z0 and LAI have the potential to change local N deposition by up to 15% and find an integrated change in N deposition of ∼3-4%.

General/ major comments

1. In my opinion the paper reads more like a classical research article than a model development article. For a model development article, I would expect (i) a clear de-

scription of the changes (which is delivered), (ii) how these manifest in the underlying model equations (not clear as specific equations for R_a etc are missing), (iii) a thorough model evaluation/ validation against observations (which I don't think is extensive enough –> see further comments) and (iv) finally a brief application/ example of how these changes result in consequences in the environment (this is done extensively here). I am wondering whether this paper would be better suited in a different journal such as ACP, where the reviewers would focus more on the underlying science. At the same time more complete validation is needed.

2. Validation: I am a micrometerologist with experience in BVOC exchange and am thus not very familiar with the availability of N-data for validation purposes, however I feel that at present model validation is not sufficient. There is no attempt in the manuscript beyond Table 6 to evaluate whether values for z0 are reasonable. I note that values in this paper tend to be on the high side of other studies for forests and on the low side for grasslands. On that note, I also find it surprising that major urban centers are not seen at all in the z0 map (unlike coniferous forest (such as the black forest in SE Germany), which seem to have a very high z0's). z0 values could easily be determined from Fluxnet or other networks. z0 is linearly scaled with canopy height for forests which not very sophisticated. For croplands NDVI is also used for scaling, and I am wondering to what extent another vegetation index such as EVI, would be more appropriate as NDVI has problems in dense canopies. Similarly, LAI values should be sanity checked (see next point). Regarding observed N deposition and observed N observations: There is little evidence that the model presented here is an improvement. The full LAI and z0 model appears to have a worse RMSD and higher negative intercept, while R^2 values do not appear to change. I think that based on this evidence I would be hesitant to deploy the model on a larger scale. Regarding the improved N-species atmospheric concentrations: There are several potential reasons for mismatch between observed and modeled atmospheric N-species. First, the model underestimates N deposition (which may very well be the case) or second N-inventories underestimates emissions. For example U.S. methane emission inventories from oil and gas

are almost certainly 50% too low. Therefore, I think that the improved match between observed and modeled atmospheric N-species is not sufficient for model validation. In my opinion model validation should be as close as possible to the processes and state variables of the model as possible (e.g. deposition, resistance values, z0, and LAI). Also, I would expect a sensitivity study that shows for example deposition velocity V_d as a function of u, u*, LAI, z_0 or so. Without knowing the exact equations for the individual resistances (which should be in the paper) I find it hard to evaluate whether changing z0 and LAI independently of u* is sensible. Instead of section 4.5 just before the discussion I would expect a validation section at the beginning of the results.

3. Uncertainties: At no point in the paper is there any quantification of the uncertainties in modeled fluxes, measurements, or inventories. Given that the modified model in the mean produces a few percent of changes in N-deposition and LAI estimates are probably uncertain to at least 10% (similar arguments can be made for canopy heights) if not higher, I am wondering whether there is any significant difference in the newly modeled deposition.

4. LAI from MODIS and z0. There appears to be an unreasonably large seasonal cycle in MODIS derived coniferous LAI which for example then greatly affects results (e.g. Figure 7). Tian et al 2004 (10.1029/2003JD003777) have documented issues with Modis LAI during snow-covered conditions (and particularly affecting conifers) and care should be taken that this does not affect the very low LAI's presented here.

5. Application: I am not the best person to evaluate the application sections, but I feel that for a model development paper, this should be shortened (and potentially moved to an application paper), with this manuscript focusing mostly on model validation. I am providing limited comments on the text here, but feel that there may be too much conjecture based on the limited model validation that has been done here.

6. The manuscript is missing links to the code as needed for GMD.

Specific comments:

Interactive comment

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Discussion paper

L53: N-emission inventories are mentioned here as uncertain, but are used for validation. I suggest to add more literature here that gives a quantitative estimate of the inventory uncertainty.

L60: "Most data assimilation and inversion methods rely on the assumption that sink terms in the model hold a negligible uncertainty." > This is probably true. However it is possible to create inversion systems that optimize more than one flux at the same time.

L87: "Under neutral conditions, the resulting logarithmic wind profile is defined as:" > Better: Under neutral conditions the resulting wind profile can be approximated using a logarithmic profile (or similar)

Sec 2.1.2: I would expect a more complete set of equations here for the resistances. Also the paper shows that z_0 also affects wet deposition, but wet deposition is not described at all in the methods.

L192: "The pixels with percentages higher than 85% were isolated for each CORINE land cover class. We used the remaining pixels to compute z_0 values for each CORINE land cover class. " > I don't understand what is done here.

Figure 1 and L218: "The equations are all within a reasonable range from one another for NDVI values lower than $\sim$0.8" > M90 and BS93 appear unreasonably high for NDVI >0.8. Please check whether these parameterizations remain valid for NDVI>0.8 or whether they should be removed before averaging.

L249: "The largest positive differences occur in forested areas, meaning that the default z_0 values are lower than the updated z_0 values. The largest negative deviations occur in urban areas and areas with "grass". " > z_0 values for forests appear to be really high especially in southern Germany (>3 m). My guess would be that the simple scaling with canopy height fails here.

L264 and Figure 7: Should be "seasonal" not "yearly" variation. Also note the issue with coniferous LAI, which is clearly not estimated correctly in MODIS.

Figure 8: Not sure whether this figure is needed, these values could just be given in the text or a table. Also in my opinion model validation as mentioned above is needed here, before moving on to the case study.

Figure 4 & Figure 7: Why do these show only 4 out of 5 land use classes and the omitted class is different between figures?

Figure 13 & 15: It is hard to see how well these agree. In my opinion this is not enough for validation of the model, especially since Table 5 shows little to no effect on the aggregate deposition statistics and a worsening of concentration statistics. A more direct comparison by land use etc should be done here.

L344: "Of the two, the newly implemented MODIS LAI values seem to have a larger effect on both the NH3 and NO2 concentrations, leading to a slightly depreciated RMSE and slope for both NH3 and NO" > Please see my previous comment. I feel that issues with Modis LAI may be to blame here. Also I find it problematic that the only noticeable change in the error statistics is a worsening of performance, but statistics should be calculated for individual land use classes, given that grass/croplands and forests show dramatically different behavior.

Figure 16: Deposition is clearly sensitive to LAI and much less so to z0. However LAI from Modis is also very uncertain...

L370: "These differences can in part be explained by the occurrence of relatively tall forest canopy ($\sim$30 meters) in the dataset, especially in forest in southern Germany" > note that you are comparing your mean to the other studies and your estimated z0s for south German forests are clearly outside the range of the cited literature.

L384: "Here, however, we merely focus on updating the z0 values per land use, and we consider the effect of this beyond the scope of this paper" > I find this statement problematic because the changes of z0 may or may not be sensible on their own and there is no clear validation for them in this work.

L432: "This work has shown that changes in two of the main deposition parameters (LAI, z0) can already lead to distinct changes (∼30%) in the modelled deposition fields. " > The question remains though, whether these changes are justified

Technical: - Tables should have labels above the table. - brackets around citations are off when using (e.g.(). - Tables and figures should not be mixed L219 > Figure 2 > Fig 1? L239 > Into

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