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Biogeosciences Discussions

Interactive comment on "Uncertainties in model predictions of nitrogen fluxes from agro-ecosystems in Europe" *by* J. Kros et al.

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We thank the reviewer for the appraisal and suggestions, which have helped us to improve the manuscript. We have carefully tried to address the issues raised and to revise the paper accordingly. In the revision we have been able to incorporate nearly all the suggestions of the referees as explained in our responses to each reviewer. Below the essence of the questions and suggestions of the reviewer (RE) are given along with our author (AU) replies

RE: p. 6059, line 10ff. It is not clear to me why the authors define (i) initial values, (ii) model parameters, and (iii) environmental constants and variables all as 'model inputs'? What is the aim of lumping these together? And what about management data – they are clearly a model input? The definition that 'inputs' are all information needed

to run a model that is not incorporated in the model itself sounds very subjective – it is easy to incorporate or out-corporate model parameters/environmental constants into or out of the model. It is likely/possible that those parameters were not incorporated which you considered important in the model uncertainty analysis – thus here the text becomes redundant. AU: The reason for using the term 'model inputs' as all information needed to run a model is purely pragmatic (although used more often), but we make clear what the inputs constitute and as such there should be no confusion. Management data are definitely also part of the model input. Furthermore, we considered all parameters and constants both incorporate and out-corporate to select the relevant uncertainty sources to be subjected to the uncertainty analyses. We clarified this in the text.

RE: p. 6059 . "(i) model inputs affecting N inputs to the system, i.e. N fixation, N deposition, N manure input and N fertilizer and (ii) model inputs affecting N fluxes in and from the ecosystems." This is rather vague. As you present the table with all parameters considered, you might already here use the grouping of data considered in the analysis adopted in Table 5. AU: Agree, we will adapt this accordingly.

RE: p. 6059, line 16. "Uncertainty in crop rotation sequence". If I am not wrong, this point is not taken up in the results/discussions section. If this had been included in the analysis, it would require much more explanation on how crop rotation sequences were taken into account and what the results were. Otherwise there is no need to mention it here. AU: This sentence was included by accident. We did not include the uncertainty in crop rotation sequence in this study. We changed this accordingly.

RE: p. 6060, line 13. I understand the problem that you like to introduce the variables included in the analysis at this point and refer to Table 5. However, Table 5 relies on Tables 1-4 so the reader who wants to study Table 5 at this point is left alone figuring out the content of Tables 1-4 and their relationship to Table 5. Also, Table 5 contains a lot of information that is explained only later in the manuscript. I strongly recommend that you add a simplified Table at this point or postpone referring to Table 5 aver having

explained Tables 1-4 before. Furthermore, you mention here 51 parameters but on p. 6065, line 5, 56 parameters. I counted 51 as well. AU: We agree that it is better to postpone the reference to Table 5. We deleted the sentence “The selected model inputs (51 in total) and their statistical properties and spatial levels are given in Table 5”, and refer to table 5 at the end of chapter 3. We corrected the number of included parameters.

RE: p. 6062, lines 20ff. I agree that uncertainty could be defined as independent from the size of the NCU. However, why should there be perfect correlation between NCU? High spatial variability of N2O fluxes at very small scales is one of the biggest problems in determining robust N2O estimates. So, there is no reason why the error made in one NCU should have a (close) relationship with the error made in the next one. This is a strong assumption and needs to be further discussed. Furthermore, even though animal numbers and N-excretion are derived from national data, I guess that they are somehow distributed to the spatial units thus giving opportunity to be uncertain even at smaller units? AU: We only assumed perfect spatial correlation between NCU’s within the same NUTS region for part of the inputs. Hence we included spatial variability between NCU’s as a source of uncertainty for some inputs However, we agree that some of the assumptions made may be criticized, but these were needed since information about spatial variability of model inputs is very limited available at present. Our study is the first to address spatial correlation between input errors in model predictions of nitrogen fluxes at European scale and we are fully aware that we had to make quite strong assumptions in various cases, but we also believe that it is an important step forward compared to existing studies that completely ignore spatial correlation. Future research should aim to relax these assumptions but this requires more information about spatial variability of model inputs than presently available.

p. 6065, lines 1ff. Please add references to corroborate your approach. AU: We cannot add references here as we are the first to apply this approach. The used uncertainty contribution approach is however pretty straight forward and we discussed the pros

and cons of this procedure (was already there).

Description of INTEGRATOR model

RE: The INTEGRATOR model is described in quite some detail, but nevertheless not sufficiently to understand how Nr fluxes have been calculated. It is not always clear where the actual (quantitative) implementation is described. With regards to data, various data sets are mentioned between 6055, line 25 and 6056, line 5 without mentioning any data source/reference. This is taken up in 6056, line 25 and 6057, lines 8-16. I wonder why these sections cannot be merged towards the beginning of the chapter. AU: We rearrange the description of INTEGRATOR model as suggested by the reviewer and added additional references with regards to data sources.

RE: p 6055 - line 5ff. “INTEGRATOR uses (i) relatively simple and transparent model calculations based on the use and adaptation of available simple model approaches, (ii) empirical relationships between model outputs and environmental variables and (iii) high-resolution spatially explicit input data.” This list of three points does not seem to be independent? For instance, the model calculations mentioned under point (i), are they different from the empirical relationships mentioned under (ii)? Do you mean that these relationships have been derived ’using and adapting available model approaches’? If so, you might want to make this clearer... AU: We changed the first point into: “(i) relatively simple and transparent model calculations using and adapting available model approaches” and skipped the second “(ii) empirical relationships between model outputs and environmental variables”.

RE: p. 6057 – line 6. IPCC does not differentiate between land use for deposited N. you should point out that this is not IPCC consistent. AU: We add: “Note that the IPCC is using the same emission factor for deposited NH3 for agricultural and non-agricultural”

RE: p 6057, line 16-22. Much of the results presented in the manuscript rely on the spatial correlation across the scales considered. How realistic is the spatial variability represented at the level of the NCU (NitroEurope Calculation Units – not Computa-

tional: : :)? How are values for input parameters assigned to the individual NCUs? These questions are likely to be crucial for the results, but the methods are neither described nor discussed. In my understanding, it is essential that this issue is addressed in the manuscript!! AU: We have used a methodology that allows to include different degrees of spatial correlation in uncertain model inputs. The parameterization is difficult because much of the required information is lacking. We acknowledge that in our work and therefore include a robustness study to analyse the sensitivity of results for choices made. As argued before, future research should address the improved parameterization of uncertainty distributions and their spatial correlation, but this requires that the appropriate data are available. We underlined this in our discussion through adding a paragraph on this aspect.

RE: Results p. 6065, line 10 “Results at EU27 level show relatively large uncertainties” – relative to what? AU: We change this into "Results at EU27 level show larger uncertainties for . . .

RE: p. 6065, line 17. see Table 4 – do you mean Table 5? It would be good to mention a few parameters here that explain these results.” AU: This should be Table 5 indeed. We took over the suggestion of mentioning some parameters to make this more clear.

RE: p. 6066, lines 26ff. “Large uncertainties in N leaching to groundwater are generally related to countries with a relatively large area of sandy soils, for which the uncertainty is larger compared to clay and peat soils (not shown)” - why? AU: Large area of sandy soils generally coincide with high N inputs and high leaching. we clarified that in the text.

RE: p. 6067, lines 6ff. “Results confirm that uncertainties and spatial variation in model outputs are partly cancelled out due to spatial aggregation.” – The concept of this is obvious from statistical theory. The quantification of it is interesting. But: how much of this result is actually determined by the study setup, e.g. the choices on the spatial correlations? Avoid such statements in the results section and rather take it

up more thoroughly in the discussion section. AU: We agree with the reviewer that this statement belongs to the discussion (as also suggested by Rev#2) and have thus moved this text to the Discussion section. We agree that the degree of uncertainty reduction due to spatial aggregation depends on the degree of spatial correlation (the greater the spatial correlation, the less cancelling out). We have made this clear in the new text.

Discussion

RE: p. 6069 – plausibility of the uncertainty quantification. This section is very well written, however could be improved with an even more balanced discussion.

RE: 1) Most of the studies taken up in the discussion are from the Netherlands, with the exception of Del Grosso et al. (2010) and Schulze et al. (2009), but other modelling studies are available (for N2O, on which also most of the discussion focuses), e.g. Stehfest & Bouwman (2006); Ogle et al. (2010); Winiwarter and Muik (2010); Brown et al. (2001); Leip et al. (2011); Berdanier & Conant (2012); : : : many of them explicitly addressing issues of scale; thus even though the approaches how to tackle this issue is different from all these papers and the manuscript by Kros et al. gives a substantial input to the discussion, the authors should enlarge this discussion.

For European countries/Europe (examples) Brown, L., Syed, B., Jarvis, S.C., Sneath, R.W., Phillips, V.R., Goulding, K.W.T., Li, C.,2002. Development and application of a mechanistic model to estimate emission of nitrous oxide from UK agriculture. Atmospheric Environment. 36, 917-928. Leip, A., Busto, M., Winiwarter, W., 2011. Developing spatially stratified N(2)O emission factors for Europe. Environmental pollution. 159, 3223-32. doi:10.1016/j.envpol.2010.11.024. Winiwarter, W., Muik, B., 2010. Statistical dependence in input data of national greenhouse gas inventories: effects on the overall inventory uncertainty. Climatic Change. 103, 19-36. doi:10.1007/s10584-010-9921-7. Berdanier, A.B., Conant, R.T., 2012. Regionally differentiated estimates of cropland N2O emissions reduce uncertainty in global calculations. Global Change

Biology. 18, 928-935. doi:10.1111/j.1365-2486.2011.02554.x. Ogle, S.M., Breidt, F.J., Easter, M., Williams, S., Killian, K., Paustian, K., 2010. Scale and uncertainty in modeled soil organic carbon stock changes for US croplands using a process-based model. Global Change Biology. 16, 810-822. doi:10.1111/j.1365-2486.2009.01951.x. Stehfest, E., Bouwman, A.F., 2006. N2O and NO emission from agricultural fields and soils under natural vegetationâËŸA′r: summarizing available measurement data and modeling of global annual emissions. Nutrient Cycling in Agroecosystems. 207 -228. doi:10.1007/s10705-006-9000-7.

AU: Thanks for these valuable additions. We have extended the discussion, while incorporating relevant references.

RE: 2) The assessment is very relevant with respect to national GHG inventories. Yet, there is no discussion on the uncertainty estimates of the IPCC guidelines, or recommendations to it (e.g. what does your final concluding remark mean for national GHG inventories, based on IPCC?). It would be important if this is addressed in the manuscript. Again, there is plenty of literature discussing the uncertainty of N2O emission estimates in GHG inventories, see eg. Leip (2010) for references.

Leip, A., 2010. Quantitative quality assessment of the greenhouse gas inventory for agriculture in Europe. Climatic Change. 103, 245-261. doi:10.1007/s10584-010-9915-5. AU: We agree with the relevance related to the IPCC guidelines, and extended the discussion on this topic.

RE: 3) The authors are quite quick to judge that it is "is likely that" De Vries et al. (2003) Schulze et al. (2009) "overestimated the uncertainty in the N2O emission." The limitations of the present study are also mentioned elsewhere, i.e. that only 'model inputs' are considered in the evaluation, that uncertainties in climate, land cover, soil type and drainage status were not included, that uncertainties and spatial correlation coefficients are 'guestimates' rather than estimates (even though this latter is partly taken up in the robustness analysis). It would be important to group these statements

in a section on 'limitations of the study' before the 'plausibilty' discussion to enable a more balanced evaluation. AU: We agree that it is relevant to start the discussion with 'limitations of the study'. We will do this but finalize that the approach is by far much better than studies that implicitly either assume perfect spatial correlation, which leads to an overestimation of uncertainty or complete correlation (not often done), which leads to an underestimation of uncertainty. This will then put the statement in the plausibility section in perspective. In this context we like to leave it in as we do not fully agree that we "are quite quick to judge that it is 'is likely that' [. . .] overestimated the uncertainty in the N2O emission." We make quite clear that their assumption of perfect spatial correlation leads to an overestimation of uncertainty. In this study we tried to incorporate most of the known uncertainties and their correlations. In order to compensate for the 'guestimates' we included the robustness analysis. Based on all these aspects we came with the conclusion "is likely that". However, we agree that by describing the limitations (and advantages) at the beginning, we make it more clear..

Editing comments

RE: p. 6058, de Vries et al., 2011d – I find a, b, c, but not d! AU: We repaired this

RE: p 6058, line 27 – what is your definition of the term 'plot'? AU: With plot we refer to a location within the spatial unit. We clarified this in the text.

RE: Table 5. I suggest to group the data such as animal numbers (dairy cattle, other cattle, poultry, pigs and poultry, other animals). They are all the same and re-grouping would make the table much shorter and readable. AU: We condensed table 5 as suggested by the reviewer

RE: Table 5. For some data such as national fertiliser N inputs, correlation at NCU is not perfect but just inapplicable, which should be indicated. AU: We indicate this in the table 5, with a footnote

Table 7. Suggest to swap columns Mean and CV (smaller gap than between CV and

Mean) AU: Thanks for this suggestion, we swapped the columns accordingly.

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