

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

Anonymous Referee #1

Received and published: 18 June 2012

The manuscript “Uncertainties in model predictions of nitrogen fluxes from agro-ecosystems in Europe” describes a study in which the propagation of uncertainty in input parameters and coefficients used in the model INTEGRATOR to estimates of fluxes of reactive nitrogen (N₂O, NO_x, NH₃, leaching and runoff of N) is assessed. The authors study both the quantitative overall uncertainty of these N_r fluxes at three different resolutions – the spatial calculation units used, country-level, and EU27 wide – and the contribution of the grouped uncertain parameters to the final uncertainty estimates. A robustness test is included as well. Particular attention has been paid to the spatial correlation of the data, which is a major determinant of the results of the study.

It is a challenge to quantify nitrogen fluxes that are characterised generally by a high spatial variability yet are expensive to measure; and hence the number of observa-

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tions is not sufficient to allow an independent estimate of regional scale fluxes. The manuscript proves that the authors have spent a lot of thinking on how this issue can be addressed in a model study and they come up with an analysis which was very carefully planned and carried out. The manuscript is accompanied with appropriate illustrations (tables and figures). Approach and results are highly interesting and relevant and fall within the scope of Biogeosciences. I recommend to publish the manuscript after some suggestions below have been taken into account.

Scope of uncertainty analysis

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.

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.

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.

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 after 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.

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?

p. 6065, lines 1ff. Please add references to corroborate your approach.

Description of INTEGRATOR model

The INTEGRATOR model is described in quite some detail, but nevertheless not sufficiently to understand how N_r 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.

p 6055 - line 5ff. "INTEGRATOR uses (i) relatively simple and transparent model cal-

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culations 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...

p. 6057 – line 6. IPCC does not differentiate between land use for deposited N. you should point out that this is not IPCC consistent.

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 Computational...)? 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!!

Results p. 6065, line 10 “Results at EU27 level show relatively large uncertainties” – relative to what?

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.

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?

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

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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.

Discussion

p. 6069 – plausibility of the uncertainty quantification.

This section is very well written, however could be improved with an even more balanced discussion.

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.

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... or globally

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 vegetation: summarizing available measurement data and modeling of global annual emissions. *Nutrient Cycling in Agroecosystems*. 207 -228. doi:10.1007/s10705-006-9000-7.

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.

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

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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 'plausibility' discussion to enable a more balanced evaluation.

Editing comments

p. 6058, de Vries et al., 2011d – I find a, b, c, but not d!

p 6058, line 27 – what is your definition of the term 'plot'?

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.

Table 5. For some data such as national fertiliser N inputs correlation at NCU is not perfect but just inapplicable, which should be indicated.

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

Interactive comment on Biogeosciences Discuss., 9, 6051, 2012.

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