Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-490-AC2, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Estimating the soil N_2O emission intensity of croplands in northwest Europe" *by* Vasileios Myrgiotis et al.

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Received and published: 28 February 2019

The authors would like to thank the second anonymous reviewer for his comments and recommendations. Below we respond to each of his/her comment (P : page - L : line).

On the structure of the article :
(1) We believe that we should keep the current structure of the article; instead of first presenting uncertainty in the results section . This is because our current introduction (P3 last paragraph and points 1-3 in P3:4) lets the reader know that in our results and discussion we will, first, address N2O EFs, then, NO3 and N uptake and finally regional-scale uncertainty.
(2) Regarding some sentences in our results sub-sections that read as being part of the

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introduction. For the first sentence in 2.4.2; the sentence was added in response to a comment by the editor and there is nowhere else in the document that this information can be added without affecting the text's flow. For the first sentence in 2.3; we followed the reviewer's recommendation and removed the sentence in the revised document.
(3) Regarding parts of the discussion that the reviewer found in the results section, we believe that these are very brief discussions to complement the presentation of results. Results are, then, discussed in detail in the discussion section.

On the errors in the references:
Indeed, there were a number of errors in the reference list which we corrected in the revised document.

On the specific issues raised (see middle of C2 and onwards, in discussion paper):
> Abstract : The ability to quantify the N2O emission intensity of croplands and crop management is one of the things that can help us reduce the N footprint. We added "among other things" to the abstract. However, there is not enough space, in the abstract, to discuss how the quantification can be used to reduce the N footprint. We believe this is discussed extensively in our discussion and conclusions.
> P3 : Removed "especially"
> P3 - L14 : Removed reference to "Haas et al, 2012"
> P4 - L5 : The model, overall, represents the state-of-the-art in agro-ecosystem biogeochemistry modelling. We believe that there is no reason do discuss specific model versions because it is difficult and text-demanding to discuss/present the differences/advances between different versions.
> P4 - L7 : Yes, the simulated yield is the amount of grain removed from the field so that it can be compared with Scotland's agricultural census statistics.
> P4 - L11 : This relates to NH3 volatilisation; an issue that was raised by the editor and reviewer 1. We revised the text in which we refer to this issue (last paragraph in P3). Nitrogen losses via NH3 volatilisation are, no doubt, important but dealing

with this N-based outflow was beyond the scope of our study. We also believe that well-managed, rainfed, NW European croplands that are treated only with ammonium nitrate are the fields least prone to NH3-volatilisation (Pan, B. et al. 2016. Ammonia volatilization from synthetic fertilizers and its mitigation strategies: A global synthesis, Agriculture, Ecosystems and Environment)
> P4 - L12 : Winter oilseed rape abbreviated here (WOSR) as suggested.
> P5 - L2 : Information added (i.e. daily).
> P5 - L11 : We added text referring to the fact that parameter ranges and values used came from a previous study where the model was calibrated (Myrgiotis et al 2018b).
> P5 - L16 : Information added (1km-2)
> P10 - L10 : Fig 4 now shows EFs according to soil texture.
> P10 - L15:16 : We added text and citations (P18 - L5:8) discussing/referring to this issue in the revised document.
> P12 - L5:6 : The transformation/translocation of N via leaching, gasification and uptake by crops are processes that act against each other because if a mole of N present in the soil is not leached it might be gasified, emitted to the atmosphere or absorbed by the crop. The order in which these processes can happen in a cubic millimeter of soil is not fixed since they are competing processes and depend on numerous factors, which act at minuscule scale. In the modelled system however, this competition is non existent. Whichever process (gasification, leaching, crop uptake) is modelled/simulated first has an “advantage” over the remaining processes. What we mean by using the sentence in question is that the model estimates N uptake and leachate formation before gas formation. We refer to this in the revised document (P13 - L5:6).
> P16 - L8 : We do not refer to management factors
> P17 - L5:18 : The tendency of the model to underestimate crop N uptake by WOSR is supported mainly by the fact that WOSR is not a cereal but (in essence) it is simulated as being one, in Landscape-DNDC. WOSR expects, on average, half of its total N need to be covered right after the 1st fertiliser application which reflects its

"rapid" (or faster-than-a-cereal) post-winter N-demand pattern. On the other hand, a winter cereal expects roughly 20% of its total N needs to be covered right after the 1st fertiliser application; and the remaining 80% to be covered after the one or two subsequent fertiliser applications (see appendix table). Therefore, it is possible that the simulated WOSR (because it grows more like a cereal) cannot take up all the N available in the soil (after the 1st fertiliser application). WOSR will eventually take up enough N, later on, to reach a yield level similar to that observed in the statistics. On this same issue, we argue that WOSR is a N-hungry crop that does not produce as much yield-biomass as cereals do. We believe that the official fertiliser use guide suggests the application of "rather" ample quantities of N to fields under WOSR. We assume that these recommendations are based on field experiments.
> P18 - L5 : typo corrected
> Table 1 : units corrected
> Figure 4 : typo corrected

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-490/bg-2018-490-AC2-supplement.pdf

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