

Interactive comment on “Estimating the soil N₂O emission intensity of croplands in northwest Europe” by Vasileios Myrgiotis et al.

Anonymous Referee #2

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The manuscript “Estimating the soil N₂O emission intensity of croplands in northwest Europe” by Vasileios Myrgiotis et al. evaluates N₂O emissions factors as well as N uptake and N leaching factors with the focus on four arable crops in Eastern Scotland with the use of the process-based model Landscape-DNDC. An uncertainty analysis of model predictions associated to soil input and model parameter values used in the simulations is presented too. From a scientific perspective, the manuscript is found to be a sound study that contributes to approach better options for climate change mitigation from regional agriculture production. The methodology, based on modelling, available regional databases (soil, crops, climate and management) and previous studies from the same group and the literature is appropriate to the aims of the study. Conclusions are consistent with the results. The manuscript is well written and results are overall

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well presented. The structure of the paper may, however, be improved. For consistency purposes with section 2 (presentation of materials and methods), I suggest that the results start with the section on “Relative uncertainty of model predictions” (currently last subsection in the results). This should be also ok from the storyline perspective. I have also found parts of discussion in the results section (last paragraphs pages 12-13, from line 14, page 14 to line 5 page 15). Also, some sentences in materials and methods are read as an introduction (first sentence in sections 2.3 and 2.4.2). The format of the references needs a general revision (format, missing titles of articles, . . .).

Specific list of issues to be addressed in a revision of the manuscript:

Page 1. Abstract. “Reducing the nitrogen (N) footprint of agriculture is a global challenge that depends on our ability to quantify the N₂O emission. . .”. But it also depends on many other things. How, more specifically, could the ability to quantify N₂O emissions contribute to reduce N footprint?

Page 3. “Especially”. I think this word does not add anything here. Suggestion “Model-based estimates of soil N₂O depend. . .”

Page 3. Line 14. It is not clear in the sentence why the reference of Haas et al., 2012 is used.

Page 4. Line 5, which specific model version is the “state-of-the-art” version?.

Page 4. Line 7. Is simulated crop yield the grain yield?

Page 4. Line 11, “ammonium nitrate was considered. . .”. NH₃ volatilization could be a potentially significant flux on the contrary of what is previously reported (page 3, lines 30-31). This should be addressed in in the text.

Page 4. Line 12. WOSR, winter oilseed rape, should be used here first, not in page 10

Page 5. Line 2. “input data on climatic. . .” Time resolution daily, weekly, monthly. . .??

Page 5. Line 11 “their values. . .” Where they come from? Are default values?

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Page 5. Line 16. Size of the grid cells?.

Page 10. Line 10. Fig. 4 does not show EFs according to soil texture.

Page 10. Lines 15-16. According to the results the total mean EFs ($\text{N}_2\text{O} + \text{NO}_3 + \text{NU}$) estimated from N fertilizer would be about 50%. Half of N from fertilizer applications is somewhere else (changes in soil N storage, volatilization (NH_3 , N_2 , nitric oxide...), not harvested parts of the crop... This should be considered when discussing the results and conclusions.

Page 12. Lines 5-6. "N loss via leaching and crop uptake act in opposition to the emission of N_2O ...". The explanation can not be derived from the results in the study.

Page 16. Line 8. According to correlations in Fig. 9, C content and water retention are more strongly correlated with N_2O emissions than fertilizer rates. If the authors refer to management factors, please specify.

Page 17. Line 17. How the tendency to underestimate crop N uptake is supported? No direct or indirect evaluation of the simulated WOSR N uptake is done in the study. On the other hand simulated yields of WOSR are very close to measurements (Fig. 4).

Page 17. Line 28 "discrepancy between modelled leaching losses and observations". It is not clear which are the observations that the authors do refer.

Page 18. Line 5. "fertiliser"

Table 1. Please add the pertinent units to the parameters reported.

Figure 4. Change "tn ha⁻¹" by "t ha⁻¹"

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