

Interactive comment on “Modelling Nitrification Inhibitor Effects on N₂O Emissions after Fall and Spring-Applied Slurry by Reducing Nitrifier NH₄⁺ Oxidation Rate” by Robert F. Grant et al.

Anonymous Referee #1

Received and published: 16 October 2019

General comments: This manuscript reported modeling impacts of nitrification inhibitor (NI) application on N₂O emission. The authors incorporated new processes into the ecosys model and compared the simulations against field observations and some literature reports. In general, the work in this manuscript can contribute to the simulations of NI impacts on N₂O production and emission. However, I think some changes can further improve this manuscript. Firstly, the new contents in this manuscript are simulating NI impacts and a lot of descriptions in the section 2 (model development) are the introductions of the ecosys model, instead of the new model development. These introductions are not necessary for me since they have been well described in literatures. I suggest the authors delete unnecessary descriptions (or move them into supporting

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materials) and focus more on the new model improvement/new contents. Secondly, I have noticed some discrepancies between simulations and observations in yields, mineral nitrogen, and N₂O emission. However, some discrepancies were not fully discussed. I would like to see more discussions regarding what might be reasons for the discrepancies and how the discrepancies (and reasons) inform further improvement in simulating N₂O emission following soil thaw and NI impacts. Specific comments: Lines 96 to 99: This sentence is not clear for me. Please rewrite. Line 230: From this section, it seems that impacts of NI are not related to the application amount of NI. Is this reasonable? Does this need to be considered in further model developments. Line 235: "Itl" in the right part should be "I(t-1)"? Line 311: So the measurement depths were shallower than the depth of slurry injection? Is this a potential reason for the discrepancies between the simulated and observed mineral nitrogen. Line 324: "as soon as possible" may be not proper here. Line 346: Could you clarify the source of the parameters in the Table 2? All from field records? Line 354: Could you provide the input parameters of the simulated crop? Line 372: Did you mean disturbance of the soil profile from surface to 0.5m or 0.8m? If so, is this setting accommodate to normal tillage practices? Is 0.8m too deep? Lines 421 to 422: This sentence is hard to follow. Could you rewrite into two short sentences? Line 469: RI should be NI? Please check other places of the manuscript. Line 473: Deleting "and measured" as these are modeled values. Line 534: Could you please discuss more about the discrepancies in simulating yields in this section, such as the reasons and implications for further model improvement. Line 567: How about N₂O reduction to N₂ during this period? Was the rate of this process low or high? Line 606: Should be "Lin et al., 2018". Line 632: grammar error. Line 693: more intensive tillage could accelerate O₂ transfer from atmosphere to soils. Does the model consider this? Line 736: May be the offset need to considered not only in Tier 3 methodology but also other methodologies. Figure 3a, b: O₂ were zero for about 10 days. Did the model simulate N₂O reduction to N₂ during this period? Figures 4 to 7: Did you compare daily simulations against daily observations? It looks that the auto-chamber observations were sub-daily; if so,

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how many observations per day? It would be useful to clarify these points.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-281>, 2019.