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Interactive comment on "Modelling Nitrification Inhibitor Effects on N_2O Emissions after Fall and Spring-Applied Slurry by Reducing Nitrifier NH_4^+ Oxidation Rate" *by* Robert F. Grant et al.

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Anonymous Referee #1 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 materials) and focus more on the new model improvement/new contents.

I have reworded sec. 2.1 to clarify the relationship between the description of earlier model components in sec. 2.2 to 2.8, and nitrification inhibition in sec. 2.9. However because readers' understanding sec. 2.9 requires their understanding of sec. 2.2 to 2.8, I am reluctant to abbreviate them, as I frequently refer to specific steps in these sections to explain model behavior in the Discussion. I have removed Sec. 2.10 and 2.11, and all later references to them, to shorten the manuscript.

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.

I have added Sec. 6.5 to the Discussion in which I raise ongoing issues about modelling N2O emissions and NI effects on them. I have confined this section to these issues as they are the focus of this paper, rather than mineral N and crop yields.

Specific comments: Lines 96 to 99: This sentence is not clear for me. Please rewrite.

Done

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.

I have not seen any experimental results in the literature in which different amounts of NI were evaluated from which such an impact could be parameterized. I have added a note to this effect in Sec. 6.4.

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Discussion paper

Line 235: "ItI" in the right part should be "I(t-1)I"?

Good point. Done

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.

That could be, because the mineral N would have to diffuse upwards from the injection zone.

Line 324: "as soon as possible" may be not proper here.

Reworded in Sec. 3.2

Line 346: Could you clarify the source of the parameters in the Table 2? All from field records?

Ksat was derived from a pedotransfer function, as now noted in Table 2. All others are from field records.

Line 354: Could you provide the input parameters of the simulated crop?

That would require a lot more model explanation that has already been provided in earlier papers, and would be inconsistent with the first point about shortening the model description raised above. Also crop growth is not the key focus of this paper, but rather NI.

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?

These values refer to soil mixing coefficients during tillage, not depth which was that of application (14 cm) as stated in Sec. 4.2.

Lines 421 to 422: This sentence is hard to follow. Could you rewrite into two short sentences?

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Discussion paper

This sentence is already less than 2 lines.

Line 469: RI should be NI? Please check other places of the manuscript.

Reworded in Sec. 5.4.1 to show that RI refers to rate constants for declining NI activity used for DMPP and nitrapyrin.

Line 473: Deleting "and measured" as these are modeled values.

This line refers to Table 5 that shows both modelled and measured 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.

That would be interesting, but the point most relevant to this paper is whether NI affected modelled and measured barley yields. In common with most NI studies, yields were not much affected. I mentioned that yields were reduced by lodging in the 2016 field experiment.

Line 567: How about N2O reduction to N2 during this period? Was the rate of this process low or high?

This process is modelled in ecosys, but no experimental data are available from this study to corroborate modelled values.

Line 606: Should be "Lin et al., 2018".

Corrected

Line 632: grammar error.

I split this sentence in Sec. 6.2.1 to simplify the grammar.

Line 693: more intensive tillage could accelerate O2 transfer from atmosphere to soils. Does the model consider this?

Yes, but I thought this was getting too detailed for a discussion of NI effects.

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Line 736: May be the offset need to considered not only in Tier 3 methodology but also other methodologies.

Perhaps, but measurements of this offset are limited.

Figure 3a, b: O_2 were zero for about 10 days. Did the model simulate N_2O reduction to N_2 during this period?

Actually O_2 was not zero, but very near zero. N_2O reduction can be modelled, depending on demand for e- acceptors unmet by NO_3^-, NO_2^- and N_2O. N_2 emissions from N_2O reduction are modelled in ecosys, but were not included in this paper.

Figures 4 to 7: Did you compare daily simulations against daily observations? It looks that the auto-chamber observations were sub-daily; if so, how many observations per day? It would be useful to clarity these points.

Observed fluxes were plotted at the same 3-h frequency as that at which they were measured as now stated in Sec. 3.2.

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