Reply on RC1
Anonymous Referee #2

Referee comment on "Evaluation of denitrification and decomposition from three biogeochemical models using laboratory measurements of N₂, N₂O and CO₂" by Balázs Grosz et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-77-RC2, 2021

In this paper, the authors tested the denitrification sub-models of three models, Coup, DNDC and DeNi) using the data from two laboratory incubations. The purpose of the study is understandable, but I recommend separating this paper into two papers, the incubation results and the modeling results, at least. The followings are the reasons why I recommend it:

1. There are so many descriptions for the observations and their interpretation in the manuscript, but it is difficult to imagine from the abstract. There was not described the incubation results in the abstract.

2. The results for the incubation were not described properly and the statistical results were not used properly; e.g. 3.1 Silt-loam soil
   > Cumulative N₂+N₂O fluxes decreased in the order V20N_88%_1.52 > III40N_80%_1.4 > IV20N_80%_1.46 > I20N_73%_1.4 > VII20N_90%_1.4 > VI20N_80%_1.4 > II10N_80%_1.4.

   From Table 5, the order was the following:
   V20N_88%_1.52 => III40N_80%_1.4 = IV20N_80%_1.46 = I20N_73%_1.4 > VII20N_90%_1.4 > VI20N_80%_1.4 => II10N_80%_1.4

   From the relationship, the following description contradicts the facts.

   > The highest cumulative N₂+N₂O fluxes were thus related to higher bulk density and WFPS (Table 5).
   > The treatment with lowest NO3- application (II10N_80%_1.4) showed the lowest
N2+N2O flux, while the highest bulk density resulted in higher N2+N2O flux compared to all other treatments (Table 5).

If the discussion does not conducted based on the statistical results, the authors should not indicate it.

3. Both the incubation results and modelling results were not discussed enough in detail. Discussion for the incubation seems to be a review of previous reports, and the discussion for modelling did not include appropriate citations.