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 > 120N_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 = VII20N_90%_1.4 > VI20N_80%_1.4 =
   I20N_73%_1.4 = VI20N_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.