

Biogeosciences Discuss., author comment AC2  
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## Reply on RC1

Balázs Grosz et al.

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Author comment on "Evaluation of denitrification and decomposition from three biogeochemical models using laboratory measurements of N<sub>2</sub>, N<sub>2</sub>O and CO<sub>2</sub>" by Balázs Grosz et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-77-AC2>, 2021

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We would like to thank Reviewer 1 for the time that they took to provide really thorough, useful comments. Their efforts towards improving our manuscript were much appreciated. We have made significant modifications reflecting those comments, which are outlined below.

Reviewer 1:

Manuscript "Evaluation of denitrification from three biogeochemical models using laboratory measurements of N<sub>2</sub>, N<sub>2</sub>O and CO<sub>2</sub>" by Grosz et al.

In the present study, gas fluxes (N<sub>2</sub>O+N<sub>2</sub>, CO<sub>2</sub>) modelled by three biogeochemical models (Coup, DeNi, DNDC) were compared to measured ones. For this, data from two laboratory experiments were used in which two different soils (one sand, one silt-loam) were incubated under various conditions (different temperatures, water contents, N addition, ryegrass incorporation, etc.). The models were run without calibration.

The topic is within the scope of the journal.

Due to enormous difficulties in quantifying N<sub>2</sub> fluxes from soils against its high natural atmospheric background, N<sub>2</sub> losses are the largest uncertainty of the N cycle at present. Therefore, there are very few studies that provide reliable information on N<sub>2</sub> losses and (N<sub>2</sub>+N<sub>2</sub>O)/N<sub>2</sub>O emission ratios, although such information is urgently needed for model validation. The manuscript, therefore, deals with an interesting and important topic, since it includes measured N<sub>2</sub>+N<sub>2</sub>O data and a comparison of three widely used biogeochemical models.

The manuscript is a quite long and partly difficult to read. This is amongst other reasons due to the fact that there are several inconsistencies in the manuscript (see general comment on "Inconsistencies" below) and also due to the large number of tables and figures (in total 14 figures and 17 tables in the text and the supplementary material, see general comment on "Tables and Figures" below). Some parts are redundant (see specific comments below). All sections including the title have to be revised to different degrees, especially the description of the results and parts of the Discussion. In my opinion, the

manuscript cannot be published in Biogeosciences in the present form, revision is needed.

#### General comments

Language: Some proof-reading is needed, since there are some linguistic errors.

**A1: Thank you for drawing our attention to the remaining linguistic errors. The article was checked by two native English speakers but of course we will read it again carefully.**

Abbreviations are not consistently used throughout the text (especially carbon/C). The abbreviation "SOC" is not defined (cf. l. 258, first appearance), "DOC" must be defined in l. 161 (not in l. 185).

**A2: We will define SOC and DOC as requested, and check that other abbreviations are used consistently.**

Treatment numbers (silt-loam soil): In the Results and Discussion sections (including Table 7) treatment numbers are provided with subscripted information including water-filled pore space, bulk density and N addition. These superscripts should be left out, since they worsen the readability of the text. All important information about the treatments can be found in Table 2.

**A3: We will remove the subscripts from the text and the tables as suggested by the reviewer. The text will be modified to improve readability while still making the various influences on fluxes clear. We will also include citations to Table 2 to remind readers where to find additional information.**

#### Inconsistencies:

(i) Test criteria: According to l. 171/172, "comparing the magnitude of measured and modeled fluxes was not a criterion" for model evaluation. However, in l. 235/236 it is written, that this comparison "was considered a secondary criterion." This should be clarified.

**A4: Yes, the magnitude of measured and modelled fluxes was, in fact, considered, although it was not our primary focus. We will clarify the text accordingly.**

(ii) Tables 1 and 3: For the silt-loam soil, the sum of NO<sub>3</sub>-N and NH<sub>4</sub>-N in Table 1 corresponds to the respective sum in Table 3 (14 mg N kg<sup>-1</sup>). This is not the case for the sand soil (19.2 vs 16 mg N kg<sup>-1</sup>). Do the numbers in Table 3 refer to pre-incubated soil

material? If so, it is strange that the N concentrations of the silt-loam do not differ. This should also be clarified.

**A5: This is a good point - Table 1 shows values measured from field-fresh soil, while Table 3 shows values prior to pre-incubation, after the soil had been stored for some time. As mineral N is not a basic soil property but instead subject to change, we have removed the values from Table 1 and just keep those from Table 3, which more accurately reflect conditions at the beginning of the incubations.**

(iii) Figures 3 and 7: In Figure 3d the measured CO<sub>2</sub> fluxes of the four individual control cores of the sand soil are shown. Around day 10, a maximum respiration rate of about 0.4 g C m<sup>-2</sup> d<sup>-1</sup> is depicted for three of the cores, the fourth one had a lower respiration rate. However, in Figure 7 an average CO<sub>2</sub> emission rate of almost 0.8 g C m<sup>-2</sup> d<sup>-1</sup> is shown. The peak around day 5 (Figure 7) seems not to be correct either. This is an important point (comparison of measured and modelled data) and should be carefully checked.

**A6: Thank you for noticing this. An outdated version of Figure 7 was uploaded. The correct dataset was shown in Figure 3d, and we have now replaced Figure 7 with the up-to-date version.**

(iv) Discussion: The summary of the results does partly not agree with the data shown in the respective figures and tables or is misleadingly formulated (l. 578-582; see detailed comment below).

**A7: Please, see response below.**

Tables and figures: As outlined above, the huge number of tables and figures makes it sometimes difficult to follow the text. I strongly recommend to reduce the number of figures and tables and only keep those which are really necessary for the aims of the study (not all data generated must be shown).

**A8: We will, as requested remove some tables and figures. See more details below.**

(i) It is obvious that not all tables provided as supplementary material are necessary, since there is no reference to some of these tables.. All tables to which is not referred to in the text must be omitted (Tables S.3, S.4, S.5).

**A9: Although we agree that we need to reduce the number of the figures and tables, some of the data we included are for use in further modelling studies and therefore, also tables and data that were not cited may be worth including. However, it is a good point that we need to make the readers aware that the information is available, so we will include a line in the results that summarizes what additional information is included in the supplementary material and then cite those figures and tables that are otherwise not mentioned.**

**Specific changes we will make:**

**We will merge Table 2 and Table 3.**

**We will merge Table 5 and Table 6.**

**We will delete Figure 5, Figure S.1, Figure S.3 and Figure S.7**

**Figure 2 and 3 will be moved to the supplementary material.**

(ii) The Tables 2 and 3 could be merged, since the only additional information in Table 3 is "Calculated 15N enrichment". The initial mineral N concentrations are already shown in Table 1 and "Added N" in Table 2.

**A10: We will remove Table 3 and the information about N<sub>min</sub> and the "Calculated 15N enrichment" will be added to Table 2 or the description of the experiments.**

(iii) Since one focus of the manuscript is on the comparison of measured and modelled data, the measured data need not to be presented in that detail as in the present manuscript. Therefore, I recommend omitting Figures 2 and 3, because all information needed for the comparison of measured and modelled data are included in Figure 7 (average N<sub>2</sub>O+N<sub>2</sub> and CO<sub>2</sub> emission rates).

**A11: We would like to keep these figures because Figures 6 and 7 just present the combined N<sub>2</sub>+N<sub>2</sub>O fluxes but not the individual fluxes. The individual fluxes provide additional useful information on their own that isn't visible when they are combined. However, we agree with this criticism, in so far as we need to reduce the number of figures in the article. Therefore, we would move Figure 2 and 3 to supplementary section (from which several other figures will be removed).**

(iv) Figures 4, 6, and 7: The sub-figures (d) to (f) should be (a) to (c), since respiration rates are described first in the text, i.e. before the N<sub>2</sub>+N<sub>2</sub>O data. The order of the figures/tables should correspond to the order in which they are cited in the text.

**A12: We followed the suggestions and reordered the tables/figures and replotted the figures.**

(v) There is no reasonable order of the figures and tables in the supplementary material, each table/figure should be on a separate page;

**A13: We reordered the figures and tables and we show each figure and table on separate pages.**

(vi) there are too many unnecessary references to figures or tables in the text (see specific comments below)

**A14: See specific responses below.**

(vii) If results for all three models are shown, they should always be presented in the same order, i.e. Coup, DeNi and DNDC. In some figures (Figures 4, 5, 6, S.7) and in Table S.5, the order is changed.

**A15: This is an excellent point. We changed the order of the results in the figures and the tables as suggested.**

Specific comments

Title: The title must be revised. The title focuses on denitrification, but CO<sub>2</sub> which is not an end-product of denitrification is also mentioned; "decomposition" should be included. "Denitrification" alone is too unspecific. It should be clearly mentioned what was evaluated (e.g. denitrification products, temporal variations).

**A16: We will revise the title of the article as requested.**

## **Evaluation of temporal variation of denitrification products and decomposition from three biogeochemical models using laboratory measurements of N<sub>2</sub>, N<sub>2</sub>O and CO<sub>2</sub>**

Abstract:

The structure could be improved. The motivation of the study does not really become clear. The aim/objective provided is too vague or appears too late in the Abstract (l. 29/30).

**A17: Both of the reviewers commented on the abstract, so we will take some time to revise it. The aims/goals must be clear and obvious, and we will revise the structure to make that the case.**

l. 14/15: What is exactly the research gap that should be highlighted here? (Bad agreement between measured and modelled data, missing processes, .....?)

**A18: Inappropriate input data, simplified process descriptions and imprecise model parameters result in poor prediction of N<sub>2</sub> and N<sub>2</sub>O fluxes, while the scarce availability of reliably measured N<sub>2</sub> and N<sub>2</sub>O soil flux data make it difficult to properly validate denitrification models. We suggest that the accuracy of models could be improved using appropriate, recent datasets, and we use two new datasets to test the model outputs from 3 models and identify the most urgently-needed areas for improvement.**

l. 16: "to test the denitrification sub-modules of existing biogeochemical models" is too unspecific. It has to be mentioned with regard to what the sub-modules were tested. The information provided in l. 22 should be included.

**A19: We will modify the text as suggested: "The denitrification and decomposition sub-modules of three common biogeochemical models (Coup, DNDC and DeNi) were tested using the data. We use measured data from two laboratory incubations to test the model response on NO<sub>3</sub><sup>-</sup>, soil water content and temperature manipulations on denitrification sub-modules of existing biogeochemical models. No systematic calibration of the model parameters was conducted since our intention was to evaluate the general model structure or 'default' model runs."**

l. 20/21 "Three common..." and first part of l. 21/22 "No systematic...": This information should be included in l. 16.

**A20: Yes, see the previous answer.**

l. 28/29: uncertainties: More specific information is needed here, for instance, one or two examples or a short explanation.

**A21: We will modify the text as follows: Differences between the measured and modelled values can be traced back to model structure and/or parameter uncertainty (i.e. modelled microbial growth affected the timing of N<sub>2</sub> and N<sub>2</sub>O fluxes, while modelled hydrology affected whether anaerobic conditions were present for complete denitrification).**

l. 29: This information must be provided earlier.

**A22: Yes, as shown above, we will specify the aim of the paper in Line 14/15.**

Introduction:

Some additional background information is needed (see comment on l. 68-72).

l. 37: "nitrogen" must be replaced by "N"

**A23: We will change the text as suggested.**

l. 42: The colon should be left out.

**A24: We will modify the text as suggested.**

l. 44 and l. 47: The information in brackets should be provided as subclauses ("which is a function...", "i.e. high background...")

**A25: We will follow this suggestion.**

l. 55: "input data may result" instead of "input data result"

**A26: We will modify the text as suggested.**

l. 57/58: The references cited here are the descriptions of the models used in the present study. Do they really demonstrate that "measurements of both N<sub>2</sub>O and N<sub>2</sub> fluxes... are necessary to develop and test algorithms"? Del Grosso et al. (2000) is missing in the reference list.

**A27: Thank-you for pointing out the missing reference.**

**We agree that these references don't necessarily demonstrate that both N<sub>2</sub>O and N<sub>2</sub> fluxes are necessary to develop and test algorithms. To be more precise, we will use "...both N<sub>2</sub>O and N<sub>2</sub> fluxes are necessary to develop and/or test algorithms"**

**We can read the following text in the article of Parton et al., 1996:**

**"The denitrification sub-model was tested by comparing the simulated N<sub>2</sub> and N<sub>2</sub>O as fluxes from denitrification with the observed gas fluxes from the Weier et al.[1993] paper. This is a verification of the model, since the data were used to develop the model."**

**In the article of Del Grosso et al., 2000, the measured N<sub>2</sub>+N<sub>2</sub>O fluxes were compared with the modelled values (Figure 8.)**

**You are right, that the reference "Li et al., 1992" is not an appropriate citation to demonstrate why the N<sub>2</sub> and N<sub>2</sub>O flux data were necessary for testing a denitrification model. We modified the citation to "Leffelaar and Wessel, 1988"**

l. 61: Delete the colon before "Coup"

**A28: Deleted as suggested.**

l. 59: Delete "these"

**A29: Deleted as suggested.**

l. 65: Delete "(Coup, DNDC, DeNi)", it is redundant

**A30: Deleted as suggested.**

l. 68-72: This passage needs to be revised. It must be clearly shown that the models are not able to properly predict denitrification processes and the dynamics/fluxes of the end-products (research gap must be comprehensibly identified). Appropriate references must be cited. At the moment, it is only mentioned that the models were used with "success". The fact that the use of the acetylene inhibition technique may lead to incorrect results does not prove that the denitrification sub-modules of biogeochemical models provide incorrect predictions.

**A31: We will reformulate this sentence as follows:**

**"However, to our knowledge evaluation of the denitrification sub-modules of these models was limited due to the lack of proper N<sub>2</sub> datasets. The development and/or testing of the NGAS and DailyDayCent models (Parton et al., 1996 and Del Grosso et al., 2000) used measured denitrification data based on the acetylene inhibition technique (Weier et al., 1993). But this method is no longer considered suitable for quantifying soil denitrification under field conditions (Bollmann and Conrad, 1997; Nadeem et al., 2013; Sgouridis et al., 2016). Therefore, it is questionable whether past evaluations of N<sub>2</sub> flux modeling were valid."**

l. 76-86: The first sentence (l- 76/77) should be left out. Instead, "specifically" in l. 83 should be deleted and the aims presented in l. 83-86 should be moved to the beginning of the paragraph (laboratory incubations are considered in the aims).

**A32: We will follow the suggestions of the reviewer.**

Materials and methods:

Some important information is missing.

l. 95: The soil classification system used (World Reference Base for Soil Resources) should be added and "organic matter" should be replaced by "organic carbon"

**A33: We will add info that the soil classification system used was World Reference Base for Soil Resources and change to organic carbon as requested.**

l. 97 and l. 110: More information is needed about how the soil samples were obtained. Using an auger, steel rings, a spade?

**A34: We will add the information that spades and shovels were used to collect the soil.**

l. 97 and 111: Why were the soil samples sieved to 10 mm and not 2 mm? Soil chemical analysis are usually conducted on the fine-earth fraction (< 2 mm). No additional sieving for soil chemical analysis is mentioned.

**A35: We will include the following information: 10 mm sieving using a high capacity rotary sieve was part of the procedure to prepare the approximately 2**

**tons of soil for this extensive joint research project, which provided the base soil for several groups. The use of 2 mm sieving prior to mineral N analysis was omitted to allow unbiased quantification of these N species.**

Table 1: The number of decimal places should be consistent within the table, C/N ratios should be provided without decimal place. The unit of bulk density should be written as "g cm<sup>-3</sup>"; "CaCl<sub>2</sub>" should be placed immediately after "pH" and not in the same line as the units of the other soil properties.

**A36: We will modify Table 1 as suggested**

l. 111: Were the NO<sub>3</sub> and NH<sub>4</sub> concentrations also determined using air-dried soil material? Nitrate and NH<sub>4</sub> should be extracted as soon as possible after sampling, air-drying of the soil samples is not recommended. This may lead to erroneous results.

**A37: The original mineral N extraction was done with field-fresh material, but of course, as mentioned above, mineral N is subject to change, so we have removed these values from Table 1 and just keep those from Table 3, which more accurately reflect conditions at the beginning of the incubations.**

l. 117: How many replicates per treatment were used?

**A38: We had three replicates per treatment. We will add the missing information to the description.**

l. 118: Delete "then" before "added"

**A39: We will modify the text as suggested**

l. 120 reference to Fig. S.3: The order of the figures and tables in the supplementary material should be changed (Fig. S.3 should be Figure S.1, etc.)

**A40: We will follow the suggestion of the reviewer and change the order of the figures in the supplementary material.**

l. 121: How was "water content kept constant"?

**A41: To clarify the method we will reformulate the description: "During the incubation, only temperature was changed (Fig. S.3), while the initial settings of water content were not changed and loss of soil water by evaporation was minimized because the mesocosms were kept closed."**

l. 123-126 and l. 161/162: All information needed to understand and also interpret the results of the present study needs to be provided (even if they are already published), since some readers may not have the possibility to access the relevant article(s). Therefore, for all technical devices used, information about the model, the manufacturer and the location of the manufacturer's headquarter should be added. The basic principle of the methods to determine NO<sub>3</sub>, NH<sub>4</sub>, etc. used should also be included; pH: Which solvent was used?

**A42: We will add all missing information and method descriptions to the text. CaCl<sub>2</sub> solvent was used for the pH measurements.**

l. 125: "at the beginning": Were the analyses conducted using pre-incubated soil material or the "original" soil material?



**A43: To clarify the method we will reformulate the description: "Soil samples were collected after pre-incubation immediately before packing of the mesocosm as well as at the end of the incubation."**

Table 2: The number of decimal places should be consistent within the table (bulk density, WFPS). The units of bulk density and water content are missing. All units must be written with squared brackets. The abbreviation "WFPS" has to be defined. What does the asterisks in the last columns mean?

**A44: We will modify Table 2 as suggested and define the abbreviation previously in the text. The asterisks will be deleted.**

Table 3: Tables 2 and 3 should be combined (see general comment on "Tables and Figures" above) and Table 3 left out.

**A45: We agree and we will merge Table 2 and Table 3 as suggested**

l. 140: "C-to-N" should be replaced by "C/N" (cf. Table 1) and "nitrogen" by "N"

**A46: We will follow the suggestion of the reviewer.**

l. 149: At which days/time steps were gas samples collected manually?

**A47: The samples were collected every third day. We will add this information to the description.**

Table 4: All units must be written with squared brackets.

**A48: We will change the format as suggested.**

l. 174-176: This is discussion material.

**A49: The sentence will be moved to the discussion.**

l. 189: I guess,  $\text{NH}_4$  would be correct (instead of  $\text{NH}_3$ ).

**A50: Yes, it should have been  $\text{NH}_4^+$ . We will change it accordingly.**

l. 207: Delete the colon

**A51: We will delete the colon.**

l. 215-217: This information would better fit in the Introduction.

**A52: Good point, we will move this information to the introduction.**

l. 219: Table S.6 should be Table S.1, etc.

**A53: We will modify the order of the Tables in the supplementary material as suggested**

l. 259 "treatments": There is only one treatment (ryegrass addition) and one control. "Treatments" should be replaced by "soil cores".

**A54: We replaced the text as suggested: "For the sand soil cores with application of ryegrass, the C and N of ryegrass were exclusively added to the labile pool."**

I. 264: Which model parameters and settings were modified and how? More specific information must be provided.

**A55: We provided more information in Table S.7 in the supplementary material. We will modify the text of the article accordingly.**

I. 272-279: A clearer separation between silt-loam and sand soil is needed here.

**A56: We agree and will reformulate:**

**“for the silt-loam soil we ran the model calculated with one soil layer because water content was assumed homogenous. For the sand soil, however, five, 2 cm thick soil layers with differing water content were simulated because significant differences in water content were evident/expected.**

I. 301: How was normality checked?

**A57: With Shapiro-Wilk test (it was significant), and with a histogram and Q-Q plot**

Results:

See general comment above on “Tables and figures”. There are several incorrect references to tables and figures.

I. 306/306: Delete reference to the Figures S. 3 and S. 4, since they do not show any results. Moreover, Fig. S. 4 depicts information about the WFPS of the sand soil, not of the silt-loam soil.

**A58: We will delete the references as suggested and modify the description of Fig.S.4**

I. 309/310: The reference must be to Figure 1a (not S.1)

**A59: We agree and will change the reference suggested**

I. 311-313: The sentences should be combined (treatments III to V which were characterized by.....showed highest N<sub>2</sub>+N<sub>2</sub>O fluxes)

**A60: We will reformulate and update the description based on the suggestion of Reviewer II:**

**“Cumulative N<sub>2</sub>+N<sub>2</sub>O fluxes decreased in the order V ≥ III ≥ IV = VII > I = VI = II showing treatments III to V which were characterized by elevated bulk density or N level exhibited higher fluxes than the other treatments”**

Table 5: The meaning of the letters needs to be explained.

**A61: We will extend the text with the explanation of the letters.**

Figure 1: How many replicates were measured? Standard deviations or errors should be included, if possible.

**A62: We measured 3 replicates but the figure with error bars would not be**

**interpretable anymore. We will add the standard deviation of cumulative fluxes in Table 5.**

l. 339: Wrong and/or unnecessary references.

**A63: We modified the text: "when both irrigation (Fig. S.4) and temperature (Fig. S.3) manipulation events occurred."**

l. 340 "fluctuations in the CO<sub>2</sub> fluxes": Is this information really needed here?

**A64: We agree that this information is unnecessary, and we will delete this.**

l. 344: The reference to Table S.2 should be replaced by information about relevant dates/time intervals.

**A65: We would like to keep this format (days of the experiments), because the time intervals can be deduced from the table and the figures were also plotted with the number of days and not with the dates or time intervals.**

l. 344/345 "Initially,...": This information is redundant and therefore not needed here.

**A66: To clarify temporal dynamics, we will reformulate this sentence**

**"N<sub>2</sub>+N<sub>2</sub>O fluxes were initially high in both treatments (Figs. 2a and 3a) but decreased rapidly following the drainage period during the first 12 days of incubation (see Table S1and Fig. S4)."**

l. 34/348: Is this information really needed here? What does "(09/02 and 14/02)" mean? The description do not agree with Figure 2a (N<sub>2</sub>+N<sub>2</sub>O: core 1 and 2 and limited core 3).

**A67: We agree with you, that we need to reformulate this sentence. Previously, we used the date form and not the days and these dates still remained in the text. We will change them. We will also revise the text "N<sub>2</sub>+N<sub>2</sub>O: core 1 and 2 and limited core 3" to agree with the Figure 2a.**

l. 375: "d-f" should be added after "Fig. 4"

**A68: We will add the text as suggested**

l. 376-379: Where is this shown? Table 8?

**A69: These data were shown in Table 8. We revised the text:**

**"On average, DeNi calculated ~4 times higher N<sub>2</sub>+N<sub>2</sub>O fluxes than measured. In contrast to this, N<sub>2</sub>+N<sub>2</sub>O fluxes obtained from Coup were about 4 times lower than the measured values, despite the fact that the N<sub>2</sub>O estimation of Coup was quite close to the measured values (Table 8.). In DNDC, it is notable that N<sub>2</sub> fluxes were always zero and it therefore underestimated N<sub>2</sub>+N<sub>2</sub>O fluxes even more (~30 times) than Coup (Table 8.)."**

l. 380: "and DNDC" should be added after "Coup". The reference to Figure S.6 should be moved to the end of the sentence.

**A70: We will modify the text as suggested**

l. 391: "smaller" must be corrected to "higher"

**A71: Agreed, we will change the text “smaller” to higher**

l. 396-406: This paragraph needs to be revised. It is absolutely inconsistent. According to l. 396, it deals with “cumulative N<sub>2</sub>+N<sub>2</sub>O fluxes”. For Coup and DeNi, cumulative fluxes are described. However, for DNDC, there is a reference to Table 8 in which average fluxes are shown and l. 404-406 deal with Table 7 instead of describing the results shown in Figure 5 (cumulative fluxes). The explanation for the numbers in Table 7 is in the next paragraph (l. 407-413).

**A72: We agree with the reviewer that this paragraph should be better structured. We will add a description of the DNDC (based on Fig. 5) and update the results based on the suggestions of Reviewer II. We will move sentences 404-406 to the next paragraph.**

l. 431-434: See general comment on “Inconsistencies” above. The measured data shown may be partly incorrect.

**A73: Yes, as we commented above, the uploaded version of Figure 7 here was an old version, which we will replace with the up-to-date version.**

l. 443: “are” should be corrected to “were”

**A74: We will correct the text as suggested.**

Discussion:

References to figures and tables should be kept to an absolute minimum in the Discussion section (too many references are, for example, in l. 499, l. 501-503, l. 504-526, l. 545, l. 575-582). An informative summary of the results that are discussed in the following is sufficient, since the results were already described in detail in the Results section. Section 4.1.2 should be shortened. Not every change in gas fluxes must be explained in great detail. A more general description of the assumed processes and possible causes would be more useful.

**A75: We note that this is a long article, and it is easier for readers to not have to search for results that are referenced in the text. However, we agree that several of these can be removed and that Section 4.1.2 should be shortened to highlight key changes.**

l. 494/495: This section is confusing. According to the Materials and methods section, the whole soil material was pre-incubated (not only the control soil).

**A76: Yes, all soil material was preincubated. We have changed this sentence to make it clearer: “In contrast, the control soils not only had no ryegrass amendment but were also pre-incubated (further decreasing the amount of labile carbon present by the time the incubation started).”**

l. 500-503: Shorter; the information provided here can be combined.

**A77: We will combine these.**

l. 505: delete “without organic...”; information not needed here (already known)

**A78: We will delete “without organic...”**

l. 505, l. 578: Delete "treatment" (the control is not a treatment)

**A79: We agree, we will delete "treatment"**

l. 523/524: Delete the references to the Tables 6 and 1

**A80: We agree, we will remove the references to Tables 6 and 1**

l. 546/547: delete "because....fluxes. However," (redundant)

**A81: We will delete the "because....fluxes. However," part of the text.**

l. 574: Change to "the results of DNDC did not reflect" (leave Coup out)

**A82: We will follow the suggestion of the Reviewer and leave out Coup.**

l. 578-582: The description of the results seems to be incorrect. In l. 578 it is mentioned that "40 % higher CO fluxes" were determined in the control of the sand soil compared to the ryegrass treatment. However, in the Figures 6f and 7f, the modelled and measured curves look quite similar and – according to Table 9 – the cumulative CO<sub>2</sub> fluxes were around 10 % lower in the control.

**A83: It is already correct described in the text but this value was incorrectly type in the table. The C1\_4 DNDC\_CO2 value (157.1) in Table 9 is incorrect, and should be 89.72. We will correct the text and figure.**

According to l. 581/582 "similar soil respiration and N<sub>2</sub>+N<sub>2</sub>O fluxes" were calculated by DeNi for the control and the ryegrass treatment of the sand soil. The description must be more specific. The course of the modelled fluxes look largely similar, but the modelled amounts of N<sub>2</sub>+N<sub>2</sub>O emitted which are addressed in the preceding text are not similar. With respect to the cumulative N<sub>2</sub>+N<sub>2</sub>O fluxes, DeNi calculated 32 % lower fluxes for the control (Table 9). In this respect, DeNi was comparable to DNDC (33 % lower cumulative N<sub>2</sub>+N<sub>2</sub>O fluxes for the control), not to Coup (10 % lower cumulative N<sub>2</sub>+N<sub>2</sub>O fluxes for the control).

**A84: We agree that the description could be more specific. We will extend the text as follows: "To illustrate the extent of agreement of the models, the time course of fluxes, the relative difference between treatments, as well as the absolute difference in cumulative fluxes need to be considered. Within each of the three models, the temporal patterns of fluxes are similar when comparing the ryegrass treatment to the control (Fig. 6 and 7). Although the absolute values of cumulative fluxes were quite varied between models, there were some striking similarities in relative treatment differences. For cumulative soil respiration, COUP and DeNi show almost the same treatment effect (around 10% higher fluxes in ryegrass vs control), while DNDC was almost 40% lower in the ryegrass treatment (Table 9). Measured values were, of course, 245% higher, showing that none of them were able to reproduce actual decomposition rates. For cumulative N fluxes, both DeNi and DNDC calculated a similar treatment effect (50% higher fluxes with the ryegrass amendment), while COUP calculated a lower effect (12% higher N fluxes with ryegrass). Similar to CO<sub>2</sub> fluxes, measured N fluxes had a significantly higher treatment effect (668% higher N fluxes with ryegrass), again highlighting that all of the models were too conservative with their estimates."**

l. 601-604: This paragraph should either be included in sub-section 4.2.4 or the sub-sections 4.2.3 and 4.2.4 should be merged.

**A85: We will merge to sections 4.2.3 and 4.2.4 to "4.2.3 Denitrifiers and anaerobic volume fraction"**

I. 625-627: A reference should be added

**A86: We will add this reference: Smith et al., 2003 Exchange of greenhouse gases between soil and atmosphere: interactions of soil physical factors and biological processes European Journal of Soil Science, December 2003, 54, 779–791 doi: 10.1046/j.1365-2389.2003.00567.x**

I. 644: Delete "(as was done in this study)", since no data obtained using the Helium gas flow soil core method were used.

**A87: We modify the text as suggested**

I. 648: "These models" should be replaced by "The models tested in the present study"

**A88: We modify the text as suggested: "The models tested in the present study were developed decades ago, and new technical solutions appear constantly."**

Conclusions:

I. 657/658: The focus of the present manuscript is not only on denitrification end-products, but also on CO<sub>2</sub>. It should, therefore, be mentioned here, too.

**A89: We followed the suggestion of the reviewer and modified the text: "The goal of this study was to assess the ability of the denitrification and decomposition sub-modules in three biogeochemical models to predict the N<sub>2</sub> and N<sub>2</sub>O and CO<sub>2</sub> fluxes of incubated soils in response to different initial soil conditions and changing environmental factors."**

I. 657: "these...." should be replaced by "the biogeochemical models Coup, DeNi and DNDC"

**A90: We modify the text as suggested**

References:

Del Grosso et al. (2000) is missing in the reference list.

Other references were not checked.

**A91: We will compare the cited references in the text and reference list**

Supplementary Material

Figure S.4: The red line (mean values) and the blue areas (SD or SE?) must be explained.

**A92: This is a good point. The red line is the mean value and the blue areas are the SD. We will modify the description of the figure.**

