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Interactive comment on “N_2O isotope approaches for source partitioning of N_2O production and estimation of N_2O reduction – validation with ^{15}N gas-flux method in laboratory and field studies” *by* Dominika Lewicka-Szczebak et al.

Anonymous Referee #1

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Review Lewicka-Szczebak et al. “N2O isotope approaches for source partitioning of N2O production and estimation of N2O reduction- validation with 15N gas-flux method in laboratory and field studies”

The study by Lewicka-Szczebak et al. compares different approaches for N2O source partitioning based on isotopomer measurements and also compares the so-called mapping approaches with experimental data. The paper is well written. The authors have so far coined the tool box for interpretation of N2O isotopomer data. With this study, they advance this branch of research by presenting a method that allows to

incorporate SP, 18O and 15N values into one sound approach. The paper is definitely an appreciated and valuable contribution to the scientific community and should be made accessible through a journal with wide audience like BG. I have only minor comments. One more relevant is summarized here (and specified in the detailed section), the others can be found in the detailed section below.
In the results section, the authors present an approach of correcting endmember isotope values obtained from the literature for the specific conditions of the given experiments or sites. As far as I understand the correction, rN2O values that are calculated based on 18O/SP maps are invariant towards this correction. The authors don’t get back to the value of this correction compared to showing DELTAdelta maps or using DELTAdelta values and the reported isotope endmembers in the literature.

See some more detailed comments below. Title good Abstract good Introduction L29: please change to “important for”
L32: Determination instead of Partition?
L37: partition sounds odd to me at this place. I suggest referring to it as “determination”. You don’t do the partition. You want to determine it?
L45: pathways instead of pathway
L70: I am not sure if I understand correctly. The observed isotope effect for N2O reduction is quite stable and doesn’t depend on r N2O. Do you mean “change in N2O isotopic composition due to N2O reduction depends largely on y n2o”?
L77-78: I don’t understand “N2O mixing ratio between two N2O production pathways and rN2O”. Section 2.1: has this experiment been conducted in 2015/2016. Please confirm if this is the procedure that has been applied in the past and state clearly that this paper reports on past campaigns and current incubations of the same soil.

Materials and Methods
L192 cf: On the one hand, water was added to obtain a certain water content in the

soil. On the other hand, columns were continuously flushed with He/O2/N2. These gases are dry (i.e., very low dew points), so that they will take up significant amounts of water from the soil, considering a flush time of 5 days. As a consequence, the soil columns will dry out under this treatment. How did the authors account for this drying effect when referring to the WFPS values? Are the WFPS values an average or the initially values that was aimed at?
L210: I suggest mentioning that fluxes were calculated based on the dynamic chamber principle. Correction for the inlet concentration is omitted since the gas mixture used for flushing is N2O-free. Do I get this right?
L310: Please give a rationale for expecting higher fungal denitrification than Ni
L319: I sugges explaining what the cases are: Case 1 assumes reduction of N2O produced through bacterial denitrification and subsequent mixing, whereas case 2 assumes mixing and subsequent reduction of the mixed pool by denitrifiers. . . . if I am getting you right.
L320: please explain c, and explain what happens if the condition is met.

Results
L421-425: Actually, I can't follow this short statement entirely. I understand that the authors aim at getting rid of the implicit assumption of DELTAdelta maps that the dominant source of N2O is denitrification. I thank the authors for this advancement. Some unclarities remain however:
1. For a 18O/SP map, the endmember values for bD, nD and fD are changed by the same amount, -6.4 per mil of water. Correcting for 18O of water makes sense, if nitrate exchanges O with water completely. However, when looking in the literature cited, it seems like the endmember values in the literature have been corrected for 18O already. For this reason, and I want to discuss this with the authors, I suggest correcting for the difference in d18O water between the presented study and the body of literature. Please clarify.
2. Again, for a 18O/SP map, this correction is equivalent with a correction of the

measured N2O 18O signature with water 18O, as correcting all endmembers for the same value is mathematically the same as correcting the measured values.
3. For 15N/SP maps, this is actually a further development, since this allows correcting both Ni and bD, fD and nD endmembers, in contrast to only correcting measured values with one assumed precursor composition.
I am asking the authors to comment on this and make this clear in the text if they agree. Please also explain the sign of your correction. It interferes with the definition of the apparent isotope effect in your reference 4 of the supplementary material (Sutka 2006).
L434: Please define minimum reduction line. I guess it is the dashed red line, but it hasn't been defined in the figure caption.
L461: there is a large discrepancy of field and laboratory NH4 delta values. This observation should also be taken up in the discussion again.
L495: In my opinion, the pie charts are i) not well described and ii) consume more space than necessary. If the authors stick to the pie charts, I ask them to indicate that the share of N2 produced during N2O reduction is given as N2 in percent. Hatched superimposed patterns show the source processes for N2O that has been reduced to N2. All in all, my suggestion is showing 2 bar plots (case 1 and case 2) with categories bD (bdN2O, bdN2), nD, fD, Ni, N2.
Figure 5: figure 5 gives everything but an overview of the results. I suggest showing only one case for Laboratory or field and instead have the numbers intelligible, and annotate scales. Please only show significant correlations, and don't correlate font size with correlation strength.
L569: why is $p<0.1$ considered as significant? I suggest rephrasing the sentence, proved sounds odd in terms of statistical inference. The p agreement between reference method and SP/O Map ($p<0.1$??) as well as Case 2 of 3DI model ($p<0.05$) was statistically significant.

Discussion

L718-720: could the authors please explain how the uncertainty was actually transferred in the model. In line 331-332, the authors state that "…uncertainties of the source's data ifs fed into the model through the variance in the calculation of unnormalized likelihood". However, I don't understand then the following sentence, that the prior distribution was assumed uninformative. Does this mean that for each endmember isotopic composition, the whole range of values was allowed? Please clarify how this actually works.

Conclusion
L863-864: I think this needs to be rephrased. The authors haven't presented a triple isotope plot, but rather present a likelihood function that allows for including three isotopic quantities and 4 associated isotopic endmembers.

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