

## ***Interactive comment on “Early season N<sub>2</sub>O emissions under variable water management in rice systems: source-partitioning emissions using isotopocule signatures along a depth profile” by Elizabeth Verhoeven et al.***

**Anonymous Referee #1**

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The study by Verhoeven et al. attempts at partitioning N<sub>2</sub>O fluxes to the source process groups nitrification/fungal denitrification and (nitrifier)denitrification, under consideration of N<sub>2</sub>O reduction to N<sub>2</sub>. Information on the relative contribution of the source processes to total N<sub>2</sub>O emission is valuable for assessing options for agricultural management that aim at minimizing N losses. In addition and intertwined in the process, the authors present an approach that allows to estimate N<sub>2</sub> emission, which is one of the major unknown fluxes in the N cycle. From this perspective, this manuscript is well suited for BG, and an appreciated contribution for the scientific

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community.

The study is very detailed, and based on field data, so that I support publication of this manuscript. However, some points should be addressed:

1. One of the objectives is to “semi-quantitatively assess N<sub>2</sub>O and N<sub>2</sub> losses among rice water management treatments”. Though this objective is set at prominent position, there is hardly information in form of tables or figures. One would expect such information in view of the objectives.
2. In view of N losses, Crop yields would be very interesting as well. It would probably be wise to add such data in view of objective b
3. The core of the study clearly is the comparison of open and closed system calculations, and their plausibility. The manuscript stops short of clearly presenting and comparing the results of the associated calculations in form of a figure. Such a figure would help the reader to understand why some scenarios were excluded. In addition, the exclusion of open system dynamics could be presented in more detail
4. The supporting information is frequently used in the manuscript, which is ok, but in view of the complex calculations described in section 2.7, I suggest that an example data point is used to show the calculation procedure, and why a sum of squares of 500 was considered meaningful.
5. The authors present calculated Net isotope effects, however the authors are not clear with regard to their assumptions (open/closed system), and the calculation applied violates some basic assumptions of Rayleigh distillation (details below). Though the authors attempt to provide information why the calculated values do not agree with literature isotope effects, the approach is constructed and in my opinion does not bring the manuscript any further. I suggest considering to skip this section.
6. Nutrient concentrations are quite variable. I suggest adding nutrient concentrations and measured fluxes for an appropriate time interval prior to experiment start to show the comparability of the treatments. Please also add seeding dates and all fertilizer applications to the figures 2,3,5 and 6.

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See some more detailed comments below.

Title

ok

Abstract

P1L18: please add emissions after N2O

L24: please add and and in front of "fungal denitrification"

## Introduction

P2L9: I suggest changing from "biological" to "microbial source processes"

L25: please check the comma after while

P4L4: the "which serves to enrich" construction of the sentence sounds odd to me. What about "The reduction of N2O to N2 enriches the pool of remaining N2O that is measured in 15N and 18O and, thus changes d15N-N2O, d18O-N2O and SP.

L9 onwards: This segment on calculation approaches leaves the reader a little confused. Will there be calculations in the manuscript? Why this segment? Please add an explanatory sentence, or consider skipping this segment. It is also not necessarily true that closed system calculations lead to higher substrate enrichment. This depends very much on the amount of reacted substrate. In general, I am missing some background information: Rice is one of the dominant crops in the world, consumes a tremendous amount of water, even in water-scarce regions, and flooded rice production also contributes greatly to the global methane budget. Saving methane may be counterbalanced by N2O emissions . . .

## Materials and Methods

P5L26: why did the DS treatment receive less fertilizer than the WS treatments? At first glance, this does not make a lot of sense. Please clarify.

P6L15: do I understand correctly that the precision of the GC was +- 12ppb / 24 ppb? This would be a quite low precision, however for the fluxes it may be less severe. Chamber height controls the sensitivity of the chamber so that I suggest giving also a detection limit at, for instance, 0.6 ppm maximum headspace concentration.

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P9L11-14: I am not sure if I understand this correctly: is 15N-N2O in this case the isotopic composition in soil water, or in emitted soil air? Please clarify. I suppose, the authors use 15N-N2O in pore water. I don't agree with the authors that this calculation is valid, for the following reasons:

- 1) 15N-N2O is not necessarily formed from exactly the location of which the nitrate originates, and may have formed from no3- / nh4+ as well.
- 2) the reaction coordinate is unknown, i.e., there is no knowledge on how much of the nitrate / nh4+ has been transformed. The equation is only valid, if the n2o has formed in an infinitesimal time after consumption of the substrate.
- 3) there are other possible intermediates in these conversions, all of which obscure this calculation. This needs to be clarified in detail.

P9L19: I am not sure what "Additionally" means in this context. I would assume that for both open and closed systems, two possible scenarios were considered. To clarify this I suggest: "For both the open and closed modeling methods, two possible scenarios were considered..."

P9L25-32: This segment is unclear to me. I guess it is most straightforward to tell my understanding of it, and you clarify in the text: there are 5 publications reporting d18O-N2O for a pure culture experiment during which exclusively N2O was produced, which gives you a good estimate for d18O-N2Oden. You want to add the value measured by Lewicka 2017 to this database (reason remains unclear, I can only encourage mentioning the really careful experiments by Lewicka 2017 as reason to extend the database). However, Lewicka 2017 was corrected for 18O-H2O. Maybe I am right in this assumptions. It became more clear to me after having a look an Figure 1. If so, I suggest you mention Figure 1 in line 22-23, and add 18O-N2Oden, 18O-N2Onit, and the corresponding SP values to figure 1, with an extra tick mark at the corresponding axis, and have the label in the plot region close to the axis. The whole approach may become more clear then. I also suggest not starting with the special case of the 18O-values corrected for water 18O, but start with the general explanation and then describe the detail.

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In view of the following text, I don't understand why the orange sc2-line does not cross the sample. For my understanding, this is not correct. Please clarify.

#### Results

P13L20: from figure 3, this pattern is not obvious for  $^{15}\text{N-N}_2\text{O}$ . Please clarify.

P15L2: Nutrient concentrations are quite different for the treatments. Please add an appropriate time period prior to experiment start to show that initial nutrient concentrations were equal.

P15L28: see comments above on net isotope effects.

#### Discussion

P18L3-4: The sentence starts with while, it seems like the sentence has not been finished correctly.

P19L11: it is unclear what you mean with a stronger trajectory towards  $\text{N}_2\text{O}$  reduction.

P19L22: not clear if the denitrifying microsites are assumed to be more abundant in WS treatments? Please clarify.

P19L24: How can abiotoc  $\text{N}_2\text{O}$  formation explain the high SP values greater than 30 in WS-FLD, i.e., the scatter? As you point out, this pathway is associated with SP of 35.

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