SOIL Discuss.,
https://doi.org/10.5194/soil-2019-64-RC2, 2020

Interactive comment on "^{15}N gas-flux method to determine N_2 emission and N_2O pathways: a comparison of different tracer addition approaches" *by* Dominika Lewicka-Szczebak and Reinhard Well

Anonymous Referee #2

Received and published: 30 January 2020

General comments:

This is a short communication on a comparison study of the effect of two different 15N tracer application techniques, i.e. mixing of tracer with soil and injection of tracer into the soil, on N2O and N2 fluxes. They used either undisturbed soil cores or disturbed, sieved soil, recompacted back to the original bulk density after homogenization. The authors measured N2O and N2 evolution from the soil after 15N tracer (nitrate) application on six different days over a period of eight days. They found generally no significant differences in N2 flux between intact soil cores and homogenized soil, with

Printer-friendly version

Discussion paper

strong dominance of N2 over N2O fluxes. The larger variability of N gas fluxes found in intact soil cores was attributed to the natural heterogeneity of soil. The paper is very short, which is not a minus in itself, as it is on an interesting and relevant topic. The idea to compare 15N label injection to intact or homogenized soil with prior mixing of the label with homogenized soil is original. Nevertheless, the paper appears to be at a premature stage, as only one soil type was studied at one water level (75% WFPS), and as the 15N label was applied at a relatively high dose (more than 100% of the natural soil nitrate pool, as indicated by the initial 15N content of the nitrate pool immediately after addition of the label), which might have strongly biased the obtained results. Therefore, I suggest that the authors conduct additional experiments with different soils, at different water levels, and with lower doses of 15N label, and evaluate the results on this broader basis of results

Specific comments:

Title: The title suggests that N2O pathways have been characterized in the study, implying that also N2O production pathways, e.g. either from nitrification or from denitrification have been elucidated, which was not really the case.

Abstract: It does not become clear from the Abstract, whether this is a (mini-)review or whether only own results were compared. Furthermore, the Abstract does not provide any information about the experimental setup. In L16-19 it should be indicated for which soil the results were obtained.

Introduction: The introduction is very short. Despite the statement in L27-28 that the 15N tracer application technique “implies a significant impact for the soil due to additional fertilization and soil disturbance depending on the way of tracer addition”, and the fact that exactly this technique was applied in the present study, no further elaboration of this topic follows. Thus, some further information from the literature should be added here.

Materials and Methods: L41: no rationale has been provided why the soil was sieved

at 4 mm, and not e.g. at 2 mm, as commonly done.

L61: The ratio 30R should be 30N2/28N2, not 30N2/29N2

L116: Not clear which differences in what were observed here.

L 136: “modulus of differences”: Isn’t the modulus the rest of a division?

L137: “Here it clear. . .”: Unclear at this point, what is clear why.

L 138-139: “. . .much better than for comparisons with aNO3 (Table 3). This shows that both gases originate mostly from the same soil pool.”: But the pool they originate from is the nitrate pool, isn’t it? Shouldn’t all three parameter be then comparable with each other?

L146: “. . .than the aNO3 value measured for total soil.”: The logic of this part of the sentence is not clear.

L160-162: Check wording, this sentence is hard to understand.

L173-175: I would have expected the opposite logic here, i.e. that oxic conditions lead to greater disagreement due PRESENCE of nitrification and hence MORE dilution of the 15N-nitrate pool by native (soil-derived) N-sources.

L191-193: I think also here the logic is wrong. As it stands, the dominance of N2 fluxes is due to the calculation method applied.

Figures general: I would not recommend the use of spline functions to connect the data points, but the use of straight lines instead.

Fig. 1: Caption and figure panels do not fit together. Caption 1B says “fraction of 15N-pool derived N2O”, but Fig. 1B shows fp_N2, but the values are in ppm, which does not make sense (should be dimensionless between 0 and 1). Caption 1C says “N2 concentration”, but Fig. 1C shows fp_N2+N2O, and again the values are in ppm, but should be dimensionless between 0 and 1.

Technical corrections: Can be found in the annotated pdf submitted with this review.

Please also note the supplement to this comment:
https://www.soil-discuss.net/soil-2019-64/soil-2019-64-RC2-supplement.pdf

Interactive comment on SOIL Discuss., https://doi.org/10.5194/soil-2019-64, 2019.

Printer-friendly version

Discussion paper