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Reply on RC2

Heleen Deroo et al.

Author comment on "Effect of organic carbon addition on paddy soil organic carbon decomposition under different irrigation regimes" by Heleen Deroo et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-53-AC2, 2021

Reviewer 2

This paper is dealing with the role of reductive desolution of Fe (oxyhydr)oxides in a six-week pot experiment with rice plants coupling with reduction of native organic carbon (OC) as an alternative soil electron acceptor. The goal of this study was to point out the role of Fe content on crop residue (Maize shoots), which can be fastly consumed by microorganisms or eventually stabilised as Fe-organic carbon complexes. The authors used two Bangladeshi soils with contrasting SOC-to-reducible-Fe (SOC:Feox) ratios were kept under a regime of alternate wetting and drying (AWD) or continuous flooding (CF). The topic is well-suited for a publication in Biogeosciences, but it may be improved, before publication. I suggest minor revision.

Response: Thank you very much for your appreciation, as well as for your interesting suggestions!

- **Lines 55: In the introduction, the choice and importance of maize shoots, which can be rapidly metabolized by micro-organisms, should be discussed more largely than in lines 55**

Response: As explained in Lines 83-84, we have specifically chosen for maize shoots because of their contrasting $\delta^{13}C$ compared to native SOC. Rice-maize cropping systems are moreover common in Bangladesh and elsewhere, so incorporation of maize shoots into a field with young rice plants forms a realistic situation. We are aware that maize shoots are easily degradable by microorganisms, and regard this as an additional advantage when studying priming effects, because any stimulating or repressive effect of maize shoot addition on SOC mineralisation should relatively rapidly occur. However, the latter was not the main reason for our choice for maize shoots, so we do not consider it relevant to mention this as such in the manuscript.

- **Line 55,85: There are much older references, such as Ponnamperuma, 1984, Better find more recent references…**

Response: Thank you, we agree with the remark that some references are old, and we propose to support those with additional citations of some more recent articles. In particular, in Line 53, we propose to add a reference to the article of Mandal et al. (2004), *and in Line 88, we would additionally cite the paper of* van Bodegom et al. (2003)*.*

- **How did the authors adjust and maintain the water content for Control and AWD conditions?**

Response: As described in a paragraph in Lines 119-125, we evaluated the water content in all pots every one or two days, and added demineralised water when necessary. For CF, we topped up the standing water each time until a mark of 2.5 cm above the soil surface. For AWD, we observed the water table by means of a perforated tube (a so-called "pani pipe"), and reflooded the pots until a mark of 2 cm above the soil surface as soon as the water table dropped more than 8 cm below the soil surface.

- **The data are looked at from a maize straw addition point of view, but the initial carbon (TC) is not really looked at, or it has not been clearly explained: what is the initial carbon between substrate C, basal soil dervied C.**

Response: We are not sure if we correctly understand this remark as intended by the reviewer. We suspect that the referee inquires about the C content of the soils. As described in Lines 111-112, the C content of the used maize shoots was 474.4 g C kg^{-1}, and Table 1 mentions that the C content of the two soils were 14.1 g C kg^{-1} (Balina) and 22.4 g C kg^{-1} (Sonatala).

- **What are the international standards used in lab? What are their $\delta^{13}C$ values? This should be given. These terms should be defined early in the M&M section, related to analytical procedure. This will help the reader for a better understanding.**

Response: Thank you, we agree that it would be more transparent to mention the $\delta^{13}C$ values of standards used for calibration of the CRDS. We propose to mention in the manuscript (Line 182) that "The CRDS was calibrated using standard gases with known $\delta^{13}C$ values of -35.95 ‰ and -26.43 ‰.".

- **Lines: 90-95: Give more details for how each soil was sampled. For example, were sterilized tools used? Was just one location sampled for each soil, or a few different locations that were then bulked?**

Response: One field was sampled for Balina, and one for Sonatala. Within each field, 15 locations were sampled (using a common, unsterilised yet clean spade) and bulked. We propose to change the sentence "The puddle layer (0 – 15 cm) was sampled at 15 locations per field in May 2014, and stored in air-dried, ground and sieved form." *(Lines 93-94) to the sentence:* "Per soil series, the puddle layer soil (0 – 15 cm) of 15 locations within one field was sampled and then bulked by means of a clean spade in May 2014, after which the soil was stored in air-dried, ground and sieved form."

- **Lines: 90-95: Moreover, why 2 different soils are taken for the experiment, with two different TC amount? It can be difficult to appreciate the difference since the initial soils are not the same for the experiment?**

Response: To improve the representativeness of the outcomes of the study, we included two soils with varying SOC:Fe_{ox} ratio, since we mainly expected that our findings would vary in function of this parameter. However, for sure the difference between both soils is not only limited to the contents of SOC and oxalate-extractable Fe, and the variation in outcomes between both soils therefore undoubtedly also reflects differences in other parameters like the quality of SOC and the soil texture and mineralogy, in spite of our efforts to select soils with otherwise relatively limited variation. Considering the observed differences in SOC decomposition upon exogenous OC addition between the two soils, it *would be very relevant to expand this experiment to other soil types, so that the results can be mechanistically linked to the soil type, and the observed dynamics could be incorporated in biogeochemical models. We already discussed the effect of soil type in the paragraph of Lines 453-470 and did not further elaborate on this.*

- **Line 90-95: It was mentioned that soils were sampled 2014. Did authors check the soil physicochemical and biological properties before starting the experiment at 2018? If so how was the variations/changes of soil properties?**

Response: The initial soil properties after soil sampling were first assessed by Akter et al. (2018) in 2014, and we repeated analysis of the SOC content (and its $\delta^{13}C$*) before starting the experiment in 2018. Over the course of those four years, the SOC content of both soils slightly decreased (i.e. Balina: 16.5 g C* kg^{-1} *□ 14.1 g C* kg^{-1} *| Sonatala: 23.6 g C* kg^{-1} *□ 22.4 g C* kg^{-1}*) in spite of being stored in dry form in a cool place. We did not repeat the other mentioned analyses before starting the experiment in 2018.*

- **Line 160-165: Why did authors used NH2OH.HCl extraction procedure to measure Fe bound C contents instead of DCB extraction procedure?**

Response: Here, we provide the same response as to the question of reviewer 1 on the use of hydroxylamine:

"We could not use the citrate bicarbonate dithionite (CBD) method because this extractant contains C (in both bicarbonate and citrate). Moreover, crystalline pedogenic Fe, which is included in CBD extracts, is not very reducible (van Bodegom et al., 2003). Poorly crystalline Fe forms the most likely source of reducible Fe and is typically quantified by means of ammonium oxalate extraction. However, since oxalate again contains C, we used hydroxylamine instead, which approximately targets the same Fe forms. We are thus convinced that hydroxylamine is a better extractant than CBD for reducible Fe in flooded soils, including for the C associated with this poorly crystalline Fe fraction. Any C that is associated with CBD-extractable crystalline Fe (if quantifiable) is also less likely to potentially contribute to enhanced dissolution of SOC, precisely because little crystalline Fe is subjected to reductive dissolution."

- **Table 1: Better to define Feox and SOC:Feox under the table.**

Response: We agree that it is clearer to define Fe_{ox} *and SOC:*Fe_{ox} *below Table 1, and propose to do that.*

References

Akter, M., Deroo, H., De Grave, E., Van Alboom, T., Kader, M. A., Pierreux, S., Begum, M. A., Boeckx, P. & Sleutel, S. (2018). Link between paddy soil mineral nitrogen release and iron and manganese reduction examined in a rice pot growth experiment. Geoderma, 326, 9-21.

Mandal, K. G., Misra, A. K., Hati, K. M., Bandyopadhyay, K. K., Ghosh, P. K. & Mohanty, M. (2004). Rice residue-management options and effects on soil properties and crop productivity. Journal of Food Agriculture and Environment, 2, 224-231.

van Bodegom, P. M., van Reeven, J. & Denier van der Gon, H. A. C. (2003). Prediction of reducible soil iron content from iron extraction data. Biogeochemistry, 64(2), 231-245.