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Interactive comment on "Alteration of nitrous oxide emissions from floodplain soils by aggregate size, litter accumulation and plant soil interactions" *by* Martin Ley et al.

Martin Ley et al.

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We thank the reviewer for her/his insightful comments and questions.

This work studied the effects of drying-wetting, soil aggregate size, litter addition and plant on N2O flux from floodplain soils. The authors used model soils and mesocosm experiments to conduct the research. As far as I can say, there are still much more space can be improved for this manuscript.

In general, it is interesting to know how soil N2O flux are controlled by different environmental factors. However, there are already many studies conducted in no matter

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Discussion paper

drying-wetting and soil aggregation, or litter addition and vegetation effects. What the knowledge gaps do you want to fill? It should be clarified in the introduction part.

Reply: We concur with the reviewer that the specific objectives of this study were not sufficiently well stated. While the effects of microhabitats related to soil aggregates, the detritusphere and plant-soil interactions in the rhizosphere on N2O emissions from soils have been studied individually, little is known about their relative effects and interactions. In a mesocosm study, we investigated this aspect for the hot moments of N2O emissions from floodplain soils during the drying phase after flooding. In particular, aggregate size effects have not been investigated in this context (as stated on lines 80ff). A particular novel aspect of the study is the minimization of the potentially confounding factor "soil structure" by mixing a given aggregate size fraction with inert material replacing the removed smaller or larger fraction. As stated on line 71ff, previous studies employing isolated aggregate size fractions have provided partially inconsistent results. The innovative aspects of objectives,will be further clarified in the introduction of the revised manuscript, with emphasis on the relevance of the research, and addressing also the potential with regards to filling knowledge gaps.

Here are technical questions:

1. Line 14-15, it is not accurate to write the buried organic matter and rhizosphere processes. Actually, the experiments were about litter addition and plant vegetation. It still takes several steps from litter to organic matter. And also, you didn't took the rhizosphere samples.

R1: We agree that the term "buried organic matter" is too unspecific. Although, sensu stricto, litter is "organic matter" as well, it indeed might be confused with further decomposed and transformed "soil organic matter". We therefore have replaced "buried organic matter" with "buried litter". We checked the entire manuscript and this is the only place where we used this rather unspecific term. We also agree that at this point "rhizosphere processes" should be replaced by "plant-soil interactions", even though in the later discussion mainly rhizosphere processes per se are invoked to explain the observed plant effects.

2. L148, for soil pH measurement, normally it is 10 g soil was mixed with 25 mL solution. The authors used 20 mL of solution,any references? The solution can be water or CaCl2, as far as I know, for alkaline soil, it it better to use water. In this study, the soil pH were ~ 8, any reasons to choose CaCl2?

R2: There are several soil-to-solution ratios recommended in the literature, among them also 1:2.5 (Blume et al. 2010. Scheffer/Schachtschabel – Lehrbuch der Bodenkunde, 16th ed., p. 151), or 1:1 (Thomas G.W. 1996. ÂńSoil pH and Soil acidityÂż In: Sparks et al. (eds.) Methods of soil analysis – 3. Chemical methods. SSSA Book Series 5, pp. 475ff.). A soil-to-solution ratio of 1:2 for mineral soil samples – as has been used in our laboratories since more than 30 years – is also recommended by one of the newest method handbooks: Hendershot et al. (2008) "Soil reaction and exchangeable acidity" In: Carter, M.R. (ed.) Soil sampling and methods of analysis. 2nd ed., Can. Soc. Soil Sci., chapter 16. Furthermore, this handbook, citing several individual studies, recommends CaCl2 as suspending solution with several advantages over water, in particular also for agricultural soils whose pH is often comparatively high. There is no mentioning in this, or any of the other cited references, of a disadvantage in using CaCl2 for carbonate containing soils. More generally, soils are heavily buffered systems and the measured pH should be virtually independent of such small variation in ionic strength.

3. Have the authors ever considered the emission/uptake of N2O by the aboveground of plant? There are already many studies in this field, such as: Smart D R, Bloom A J. Wheat leaves emit nitrous oxide during nitrate assimilation[J]. Proceedings of the National Academy of Sciences, 2001, 98(14): 7875-7878. In this study, the authors measured N2O flux from the mesocosm have both soil and plant. This flux cannot be called soil flux, but may be soil/plant flux?

R3: In the introduction (line 96ff) we considered potential bypassing of the soil matrix by N2O fluxes via plant-internal aeration channels (aerenchyma). This phenomenon is well documented for Poaceae such as the Genus Oryza or Phalaris arundinacea. However, for willows (Salix sp.) such a process has, to our knowledge, not been documented yet. Although, considering that also adventitious roots of Salix species contain aerenchyma, we cannot exclude this process to occur in our case, our results do not indicate an enhanced N2O emission via the plant, since we observed the lowest flux rates as well as lowest total integrated emissions in the mesocosms with plants. Therefore we conclude that in our experiment, such a process, if present, was of minor importance in terms of modulating net N2O fluxes to the atmosphere. However, we agree that the possibility that part of the N2O fluxes from the planted soils occurred via plant-internal channels should be mentioned in discussion section 4.3. We also agree that emission fluxes should be termed "soil/plant flux" or "ecosystem flux" instead of "soil flux". Although nowhere in the manuscript we have used the term "soil flux", we agree that we need to clarify at respective prominent places in the manuscript that in the case of the treatments with willow emissions/fluxes relate to the whole soil/plant system and not to the soil alone.

4. L274, the author can show the data in support information.

R4: we will upload a file containing the supplementary information and adjust the text accordingly.

5. L313-315, the authors didn't check the statistics difference of soil chemical/physical properties between different treatments. Therefore, the hypothesis is not really correct before statistics analysis were done.

R5: The comparison of the initial physicochemical properties by t-tests with Welch's correction showed statistically significant differences for the C:N ratio and pH. However, C:N ratios of 12 and 16 can be considered ecologically similar in terms of soil organic matter degradability, in particular since both Corg and total N do not differ that much.

The higher pH in the macroaggregated model soil is probably due to a higher carbonate content, which also is not expected to strongly affect biogeochemical processes of the N cycle. These remarks will be added in the revised manuscript, and a new column will be added in table 1 with the results of the statistical analyses.

6. L346-347, Actually WFPS-SA value were not decreased to pre-flood even until the end of experiments (Fig. 2 a and b). The explanation might be low diffusion rate of N2O in SA treatments caused reduction of N2O to N2?

R6: Considering the high WFPS in the SAU treatment, the referee's remark represents a valid explanation for the observed low fluxes under the given circumstances. However, the relatively high redox potentials, which we invoke here, argue against sufficient anoxia for complete reduction of N2O to N2. Nevertheless, we will include this aspect in the discussion in section 4.1. of the revised manuscript.

7. L409, delete one dot

8. L457, delete DOI

9. Table 2, it would be better to explain the meanings of LAU, SAU....in the table caption.

10. L638-639, no dotted line in Fig. 3?

11. Fig. 2, it would be better to put WFPS in the right Y axis. And put WFPS-LA, WFPS-SA....in the figure legend.

12. Fig. 3e, the data are not completely shown.

13. Fig. 4, would be better to have the same unit (μM) for nitrate and nitrite/ammonium

R7-13: the authors consent with all these remarks and will make changes to the revised manuscript accordingly.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-281/bg-2018-281-AC1-

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Discussion paper

supplement.pdf

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