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Interactive comment on "Accumulation of physically protected organic carbon promoted biological activity in macro-aggregates of rice soils under long term rice cultivation" *by* Yalong Liu et al.

Anonymous Referee #1

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The manuscript by Liu et al., deals with the link between organic C distribution among different aggregate-size fractions and microbial activity in paddy soils under long term cultivation. Our knowledge on C stabilization in soil subjected to alternating redox conditions, and the role of physical protection (as well as other mechanisms of OM stabilization) in C accumulation in these soils is rather limited. This manuscript can therefore represent an important and novel contribution to our understanding of these processes in paddy soils. The concepts proposed are very interesting, the scientific approach adequate, and the data set impressive. However, the manuscript lacks focus, is rather long and often repetitive, and requires a significant work on the English

language. Moreover the interpretation and discussion of data is often unfounded. This does not do justice to an otherwise valid contribution. In my opinion, the manuscript should be reconsidered for publication in BG after major revisions.

Specific comments

1) The introduction is rather general, long and tends to be repetitive. The authors should rewrite this part providing a more focused outlook on the interaction between C stabilization, aggregate stability and biological activity in rice paddies. They should also provide one or more hypotheses which the manuscript lacks. It is not clear from the introduction alone, why the authors choose a 700 y chronosequence to test their hypothesis. This time is longer than the expected residence time of physically protected organic C that is the subject of the manuscript.

2) The authors utilized a fractionation procedure that provides a number of aggregate-size fractions. The functional distinction between these fractions, and consequently the interpretation of all the results obtained, strongly depends on the sonication energy applied. The authors suggest that they adopted a "low energy sonication procedure" with applying 170 J/g for all soils irrespective of the pedogenetic processes that characterise their formation (known to have a direct bearing on aggregate stability). It is essential that the authors justify the fractionation procedure, provide further details on how they determined this energy input, and what the size-fractions represent.

3) Linked to the previous comment, the interpretation of the results and the discussion is somewhat confusing. The fractionation procedure does not allow to separate physically-protected organic matter from organic matter stabilized by interaction with mineral surfaces. I would assume (but the authors should confirm in the manuscript) that with the low energy sonication applied, macro-aggregates have been broken releasing micro-aggregates, mineral particles and inter-aggregate particulate OM. This would mean that all fractions except the clay-sized fraction, could have different amounts of OM stabilized by different mechanisms. This has to be taken into

account during the discussion.

4) FTIR spectroscopy analysis: I am not aware how the authors obtained a quantitative distribution of OM functional group constituents (Table 3) from specific peak bands in the IR spectrum, considering that each functional group vibration has a different molar absorptivity. In fact, Table 3 suggests that there is more phenolic than aromatic C and this is not possible since all phenols are also aromatic. I suggest using the ratio between specific peaks within each spectrum to obtain comparative results on OM composition.

5) The objective of carrying out an incubation of soils with maize biomass is not totally clear to me and must be justified. I do not understand how results from an incubation under oxic conditions may contribute to understanding the role of physical protection in paddy soils. It seems that most of the maize-OM added was mineralized over the incubation period. The authors do not provide information on how maize application influenced the distribution of aggregate-size fractions. Moreover, they associate the C gain in the sand sized fractions to physical protection in macro-aggregates (L661-665), however stating that this is the predominant mechanisms of OM stabilization in these soils is incorrect considering (1) the fractionation procedure does not distinguish between free particulate OM and that occluded within macro-aggregates, and (2) the relatively short incubation period does not allow to take into consideration other stabilization mechanisms with longer turnover times.

6) Soil respiration: This approach involved measuring the emission of CO_2 over a 37 d anaerobic incubation period. However, I would expect CH_4 and dissolved CO_2 to contribute to the total anaerobic OM mineralization. These were not taken into account.

7) The discussion requires rewriting considering all the previous comments. Moreover, it need to be more concise and focused. The authors often cite other works to support their interpretations that are based on soil processes in upland soils where oxic conditions predominate. In my opinion this is not always correct especially when referring

to microbial biomass composition and activity, gene abundance and their influence on soil processes.

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