Review of Manuscript BG-2013-372

Yuan et al report in their study about a “priming effect” of Rice Straw (RS) application to rice soils. By the addition of 13C labeled RS the authors identify not only increased CH4 production in general, but they also identify that RC amendment increases CH4 production also from other carbon sources in the soils, such as soil organic matter (SOM) and rice root organic carbon (ROC).

The manuscript is generally well written, short and precise. It is well structured and easy to read, except of some (important) definitions that may be clarified as outlined below. The scope is clearly within the focus of Biogeosciences. Nevertheless, as indicated by reviewer #1 I see some issues that need to be addressed by the authors. In contrast to reviewer #1, I think that an open discussion of the limitations of the approach would still make this paper an important and interesting contribution to the scientific literature on this issue.

In agreement with Reviewer #1 I see certain limitations in the way to calculate the contribution of CH4 production from ROC. As already indicated by reviewer #1, it is assumed that the contribution of CH4 produced from SOM is the same in both planted and unplanted rice microcosms. However, I would not completely deny this assumption. I think the authors should discuss and mention this limitation openly to enable the reader to decide whether he or she may follow this assumption or not. By doing a sensitivity analysis here, one may get further help in judging whether this assumption may lead to a total reversal of the findings in case of uncertainty here, or whether the presented results are robust despite this limitation. Using the two different labels in the rice straw is an elegant way to separate carbon sources here, but the uncertainty of the assumption of equal $d^{13}C_{CH4\text{-}SOM}$ for planted and unplanted microcosms should be clearly mentioned. Being faced with the formulae and referring to a previously published paper may confuse the reader here.

The concern of reviewer #1 about artifacts in determining the CH4 production from ROC is also correct from my point of view. However, the authors may consider focusing even more on the comparison of the treatments with and without RS. If both treatments (+ and – RS) are treated similarly, the effect of how and if the rice plant was removed is the same for both treatments. Thus it would be valid to derive the difference here and derive a “priming effect”, although as stated by reviewer #1 the exact determination of CH4 from ROC remains uncertain.

It may be worth considering showing first the results from the slurry experiments (which are easier to support the “priming effect”) introduce the assumptions made for the microcosms.

I would, however, suggest reconsidering using the term “priming effect” in general. From my point of view, it is more that the total carbon flow is similar but directed more towards CH4 production, away from CO2 production (Fig 4 b and d). So if I understand that correct there is not more carbon degraded from the SOM pool (this would be a priming effect), but the mineralization of SOM is directed more towards CH4. If I understand this wrong, the authors should clarify a bit more here. So from my point of view this is a change in the SOM carbon mineralization pathway due to RS addition, rather than a “priming effect”, which would be, in a strict sense, an increase of total SOM mineralization due to RS addition.

I do not see so much of a problem in the comparably high ratio of CO2 to CH4 production in the anaerobic incubations, as such high ratios have frequently been observed in many studies (which has led to the hypothesis that there must be other than inorganic electron acceptors present). There are only few studies, mostly from laboratory incubations with substrate additions where a clear 1:1 ratio of CO_2 to CH_4 is achieved. Nevertheless this point should be openly addressed. It would of course be helpful is electron acceptors would be known after the 40 days of preincubation. This would then also enable to clarify whether the increased percentage of SOM conversion into CH_4 is driven by "earlier and more reduced" conditions in the amended soils while in the unamended soils electron acceptors may have remained.

I think that after clarifying the mentioned points and openly addressing the limitations the paper merits publication