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Biogeosciences Discussions

Interactive comment on "Mechanisms of soil carbon storage in experimental grasslands" *by* S. Steinbeiss et al.

Anonymous Referee #3

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General comments

This study aims at quantify how the amount of aboveground plant input control the dynamics of soil organic carbon in the soil profile. The authors used the C3C4 natural labelling technique to track potential priming effect due to fresh litter input and to investigate the translocation of the old or recent mobilized carbon to deep horizons.

This issue is really interesting, the paper is clearly written, but the calculations that support the results are not sufficiently described or quite critical. In consequence, at this stage of the reviewing process, I remain quite distant with the authors' discussion and conclusions, even if the approach is very interesting. I would be very interested in knowing about their findings again, once that the calculations would have been pre-

cisely explicated and clarified.

Specific comments

1. My first comment is that the experimental device used by the authors is not really appropriate to answer their scientific question and makes the interpretation of their results complex. Indeed, the combination of a land use change with a litter experiment is quite delicate.

The authors indicate in the introduction that the change from arable field to grassland increase carbon stocks and distribution in the soil profile. They could have used their C3C4 experiment with the same above ground treatment (=removal of the above ground biomass - see l8 in section 2.1) to solve the question: 'which carbon (old or recent) explain the changes?'; But, they added a more complex question, about the role of above ground biomass and introduced another treatment with double litter input. Their aim is to study priming effect by comparison of the two C4 treatments and they mainly focus on this. The logical scheme they adopted is interesting: is it the old or new C that is decomposed, what is the fate of this decomposed C? Complete mineralization or transfer as DOC to deeper horizons?

2. My second comment is that a 2 year C3C4 experiment is very short to observe any significant change in the isotopic composition of the SOC. The data presented in this paper date back to 2004. Are there any new data available on this experiment to make the findings of the authors more robust?

3. At last, at this stage of the reviewing process, I am very sceptical regarding the findings and conclusions of this study. This is due to the way the authors computed the proportion of C4-carbon in the SOC and in the DOC.

(a) First of all, it is not very easy to find in the text the data that were used in Eq. 1 and 2. A table summarizing all the delta13C values of plant material (various organs, different plots) and SOM in 2002 and 2004 will be very helpful - and the opportunity to

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transform the fig 3 into a table might also been considered here -.

(b) Then, according to me, in Eq 1, the denominator should take into account the isotopic composition of the real plant material that is or was transferred to soil. - The delta13C C3-plant must ideally be the value measured on the previous crop input (only root, root and stem+leaves?). If this value is not available, this is a very curious choice to use the aboveground value of whereas the authors say that the aboveground biomass is removed: the only grassland input is belowground, and we do not know its isotopic composition. - In the C4 treatment without litter, the delta 13C C4-plant must be the isotopic composition of root (-14,1 permil) and in the C4 treatment with double input, it must be the average value of below (-15,1 permil) and above ground biomass (-13,2 permil), taking into account the relative contribution of the 2 plant compartments. The authors say they use only root data, but which one? and why this choice?

(c) Also in Eq 1, it would have been more correct to have a second reference C3 soil with double litter input so as to compute F in the double litter treatment. Indeed, different dynamics can lead to different fractionation during microbial decomposition of SOM.

(d) Because of the above reasons the calculation of the proportion of C4 carbon in SOC currently leads to uncertainties for the reader. The problem is that these uncertainlties are in the same range of order than what the values presented on fig 4 (about F more or less 5 percent, depending on the delta values used). The authors must explain clearly their calculations and give all the data necessary to perform them in a separate table.

(e) I am also confused about the way they computed their Eq 2. The determination of the proportion of C4-carbon in the DOC is quite sensitive because the parent material of the DOC is not known. It could be recent or old carbon. The authors decided to consider C3 source of DOC as old SOM and not fresh C3 plant, what is probably the best choice. They used the delta13CSOM2002 value (of which plot?). Then, if they computed the proportion of new C using a mixing equation, why did they choose

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delta13CSOM2004 as the value at the numerator?

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