Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-264-RC2, 2017

Interactive comment on "Comparing soil carbon loss through respiration and leaching under extreme precipitation events in arid and semi-arid grasslands" *by* Ting Liu et al.

Anonymous Referee #2

Received and published: 12 October 2017

General comments The manuscript presents a study that attempt to evaluate the effect of extreme precipitation events on soil carbon losses in arid and semi-arid grasslands. The objective was to distinguish between C losses due to respiration and leaching. Additionally, leaching losses were separated into DIC and DOC losses. Therefore, a soil column experiment was conducted, were respiration and leaching losses were measured after an artificial precipitation events. Soil inorganic carbon losses due to leaching was higher than due to an enhanced respiration. As already mentioned by the first referee, the relevance of C losses depends on the fate of DIC and this should be more pronounced in the discussion. In addition, soil carbon losses due to DIC leaching has to be more discussed in detail, especially the fact that about 50% (or even

more with the already recalculated values) of the DIC originates from SOC degradation (dissolved CO2). In consequence, the conclusion that most soil carbon during EPE is lost due to DIC (in particual SIC), might be not true. On the contrary, most of the DIC originates from dissolution of CO2, which originates from SOC mineralization and not SIC leaching. This should be discussed much more in detail.

The experimental setup seems appropriate for the objectives presented in the manuscript, however the presented results need some reconsideration and recalculation, especially respiration data should presented as specific respiration to account for different SOC contents in the investigated soils (for more detail see specific comments).

Specific comments

Page 3,line 25: The dimension of the soil pits seems quiet small 10 cm x 10 cm. Even by using a shovel I would expect that you need a bigger area to go down to 70 cm.

Page 5, line 1ff: How was the water added to the soil columns? Did you had needles in the top lid of the soil column? Did you used a constant rate, like 0.5 mm per minute? How much time was in between the EPE events? How long did you wait until you start a new EPE? How where the soil columns treated in between the CO2 measurements? Were they closed or flushed with constant air flow? Please provide more information about the experimental setup for the reader.

Page 7, line 8ff: Additionally, to the respiration rate I'd suggest to calculate a specific respiration rate, which is the respiration rate divided by the amount of SOC (mg CO2-C g-1 SOC h-1). This would allow an easier comparison of the different soils with different SOC content.

Page 7 line 13f: The authors argue that litter addition increase respiration due to mineralization of labile litter compounds and priming effects. However, here were no values presented which would underline this statement. Since the authors measured δ13CO2, they should be able to separate litter mineralization from SOC mineralization.

Page 7, line 17ff: The used mixing model should be mentioned in the method section. Further with the given isotopic values I can not understand how the authors calculated contribution of litter mineralization to total respiration. Using a mixing model of: 1 – ((c_mix-c_litter)/(c_control-c_litter)), where c_mix is the isotopic value of CO2 from the litter amended sample (-18,7‰, c_litter the isotopic value of the added litter (-16,2‰ and c_control the isotopic value of CO2 from the non-amended sample (-23,1‰, the contribution of litter mineralization to total respiration was around 64% at day 1 and only 19% at day 25. Which values for δ13CO2 values did you used for the control (non-amended) samples. Did you measured δ13CO2 for the control only at the beginning or at the same resolution as δ13CO2 for the litter-amended samples? Further, are there any isotopic measurements of the other two sites. If so, why they are not shown here?

Page 7 line21ff.: Despite the fact that the calculation described here might be simple, it should be part of the method section and not of the result/discussion section. "EPE-induced CO2 release was higher in the KQ and GC soils than in the XLHT soil ($p < 0.05$; Fig. 3) that had a lower SOC content and a lower SOC:N ratio (Table 1)", as mentioned above, I suggest to calculate a specific respiration normalized to the absolute amount of SOC in the soil column. The specific respiration will provide more information about the stability and the loss of C from the different sites. A rough calculation based on figure 3 revealed that respiration of the 3 sites in the non-amended treatment might not differ. However, this has to be checked with the measurement values. It is also not clear to which EPE is shown in Fig 3, is it the first, second or third one?

Page 7, line 25ff: "Litter amendment significantly increased the EPE-induced CO2 release from the KQ soil ($p < 0.05$) but did not have any effect on the XLHT and GC soils." - I disagree with this statement, because figure 3 only show total CO2 release which is the sum of CO2 from litter mineralization and SOC mineralization from the mineral soil. Therefore, it is only possible to say that EPE increased total CO2 release. However, as mentioned above, I would recommend to use the isotopic data of CO2 to separate

respiration into the two sources litter and soil. Which would allow a comparison of SOC mineralization between non-amended and amended soils.

Further the authors explained the higher CO2 release in the KQ soil with a coarser soil texture and less sorptive protection of DOC, however I can't follow this explanation since Table 1 show a lower sand content in the KQ than in the XLHT soils. "These results suggest that soil texture, SOC content and quality are important factors influencing EPE-enhanced soil respiration."This is a quite strong statement, because there is no information given about the SOC quality in the different soils used in this study, also the soil texture of the used soils is more or less similar and the presented results does not justify such a strong statement. In fact, the higher CO2 release in the KQ can be explained by several other factors, such as the higher pH of the GC and XLHT soils. The higher the pH the more CO2 is dissolved, which can be seen in Fig 4 in the higher DIC contents of these sites. Also negative priming effects may occur in the GC and XLHT sites which lead to a reduced SOC mineralization, because microorganisms utilize the added litter first. This should be discussed in more detail.

Page 8 line 8ff: The authors mentioned that CO2 from litter decomposition contributed to DIC, however in the following lines the argue that this effect is more pronounced in the KQ soils than in the XLHT and GC, due to the higher SIC content of the later once. This is true speaking in relative amounts, but considering absolute amounts I would expect that much more CO2 is dissolved in the GC and XLHT soils due to the higher pH Therefore CO2 might also be a source of DIC in these soils with high SIC content. Which is also shown in line 23-24 on the same page.

Page 8, line 19f.: Why was this measurement only done for the XLHT soils? Or why the results from the other sites not shown?

Page 8, line 27f.: "Interestingly, increasing DIC fluxes were not observed in the KQ and GC soils (Fig. 4), although they had higher SOC content and degradation (i.e., respiration) rates (Fig. S5)" - What do you mean with higher degradation?

Page 8, line 31ff.: Here the authors calculated a specific leaching loss normalized to SIC content and compared it to DOC losses. However, in line 9 same page, it is mentioned that especially for the soils with low SIC content, the dissolution CO2 from SOC and litter mineralization is a major source for DIC. Therefore, the high value of 32.5 mg C g-1 SIC has to attributed to SOC mineralization and not to SIC loss. In consequence, the drawn conclusion that SIC loss is the main form of soil carbon loss in neutral to alkaine soils might be questioned, at least for the KQ soils.

Page 9, line 33: "It is also worth mentioning that soil carbon leaching fluxes in this study (10.1−25.3 g C m −2 yr −1) far exceed annual SOC loss through warming-enhanced respiration at these sites (0.2−0.6 g C m −2 yr −1) given an assumed temperature sensitivity of 2 in climate..." - I think the authors can not make such a comparison, since most of the DIC in this study originates from SOC degradation, therefore DIC losses has to be attributed to respiration losses and not DIC losses. Meaning that dissolution of CO2 needs first mineralization of SOC by microorganism.

Technical corrections Fig. S4 and S5 are swapped, the caption of Fig. S4 belongs to Fig. S5 and vice versa.

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