

## Response to Anonymous Referee #2

We appreciate Referee #2's critical and detailed assessment of our manuscript and we are grateful for his/her constructive comments which helped us to greatly refine our paper. Here we provide a point-to-point response to all the issues raised by the referee. We hope our replies and revisions will satisfy all the requests.

### General Comments:

#### Comment 1:

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, where 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.

(1) 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.

(2) 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 CO<sub>2</sub>). In consequence, the conclusion that most soil carbon during EPE is lost due to DIC (in particular SIC), might be not true. On the contrary, most of the DIC originates from dissolution of CO<sub>2</sub>, which originates from SOC mineralization and not SIC leaching. This should be discussed much more in detail.

#### Response:

(1) We have added one paragraph to discuss the fate of DIC in Section 3.4 as follows:

*“An uncertainty related to the importance of leaching processes in the overall carbon budget along the “soil-river-ocean” continuum lies in the ultimate downstream fate of the leached carbon. If part of this carbon is retained in the surrounding soils or carried along from the river to the ocean in the form of DIC without outgassing into the air, it will not constitute a source of atmospheric CO<sub>2</sub> on a relatively short term (over years or decades). However, soil columns used in our study has a depth (60 cm) typical of or even deeper than the average soil depth in the alpine grasslands of Qinghai-Tibetan Plateau (Wang et al., 2001). Hence, we assume that carbon leached in our experiments will have minimum retention in the soil. Furthermore, compared to DOC and DIC in the soil solution, the leached carbon is more likely to be subject to more intensified mineralization and outgassing during the land-ocean transfer, given more intensified mixing processes, oxygen exposure and photo-oxidation of terrestrial carbon upon releasing into the river (Hedges et al., 1997; Battin et al., 2009). Hence, we postulate that carbon leached from soils is more vulnerable to decomposition and/or release compared to that retained in the soil. That being said, it will be necessary to confirm our results and hypothesis using field-based leaching experiments to better understand the ultimate fate of leached soil carbon: whether it will be retained in the deeper soil or show a higher degradability upon leaving the soil matrix. Such information will be complementary to our study and further elucidate the importance of leaching processes in terms of ecosystem carbon budget.”*

(2) The reviewer raised an excellent point and we agree that the source of DIC needs to be discussed in

order to determine the contribution of SIC dissolution to soil carbon loss. We have therefore calculated the flux of biogenic and lithogenic DIC from soils and added discussions in Sections 3.3 and 3.4 as follows:

*“... Based on the isotopic mass balance of Eq. (3) and (4), lithogenic carbonate (with a  $\delta^{13}\text{C}$  value of 0‰) contributed 51.2% to the leached DIC while biogenic DIC produced by SOC degradation contributed 48.4% (Fig. 5). The  $\delta^{13}\text{C}$  value of leached DIC decreased to  $-12.3\text{‰}$  and  $-13.5\text{‰}$  during the second and third EPEs, corresponding to a contribution of 77.0% and 84.4% by biogenic sources in the total DIC, respectively (Fig. 5). These results confirm our previous hypothesis that SOC decomposition contributed significantly to soil DIC fluxes. Combined with the total flux rate, we calculated that both lithogenic and biogenic DIC fluxes were  $\sim 2.1 \text{ g C m}^{-2}$  in the first EPE. Subsequently, lithogenic DIC flux decreased to  $\sim 1.3 \text{ g C m}^{-2}$  while biogenic DIC flux increased to  $7.6 \text{ g C m}^{-2}$  in the third EPE. This demonstrates that the increased DIC flux with repeated EPEs was mainly derived from increased contribution of SOC mineralization. Interestingly, increasing DIC fluxes with repeated EPEs were not observed in the KQ and GC soils (Fig. 4) despite their higher SOC contents and  $\text{CO}_2$  release rates (Fig. S5). ...”*

*“Regardless of its source, the EPE-induced leaching loss of inorganic carbon was  $31.5$  and  $10.6 \mu\text{g DIC g}^{-1}$  soil from the alkaline XLHT and GC soils, respectively, approximately three and five times higher than the corresponding DOC leaching loss ( $5.9$  and  $3.9 \mu\text{g DOC g}^{-1}$  soil, respectively). However, the KQ soil had a relatively lower EPE-induced DIC loss ( $4.4 \mu\text{g DIC g}^{-1}$  soil) than the DOC leaching loss ( $11.6 \mu\text{g DOC g}^{-1}$  soil) mainly due to its lower initial SIC content and relatively neutral soil pH value. Hence, DIC was the main form of soil carbon loss in alkaline soils during EPEs regardless of its source. When the source of the leached DIC is taken into account, dissolution of  $\text{CO}_2$  produced by SOC mineralization (biogenic DIC) constituted more than half of the leached DIC (at least from the XLHT soils; Fig. 5), whose contribution increased with re-occurring EPEs (Fig. 5). This implies that SOC mineralization measured by  $\text{CO}_2$  fluxes was under-estimated by approximately 8 times in the XLHT soils during the three EPEs (Fig. 5). In addition, DIC loss exclusively resulting from SIC dissolution or weathering was also a significant fraction of soil carbon loss, equivalent to 219% SOC loss in the form of  $\text{CO}_2$  during EPEs (Fig. 5). These results collectively corroborate that inorganic carbon loss is the main form of soil carbon loss in alkaline soils during EPEs.”*

*“... In view of DIC sources, biogenic DIC loss derived from SOC mineralization contributed to more than half of the total leached DIC fluxes and accounted for 184% of the NEP at XLHT. ... These results also imply that SOC mineralization measured by  $\text{CO}_2$  fluxes might be drastically under-estimated in alkaline grassland soils during EPEs. ...”*

## **Comment 2:**

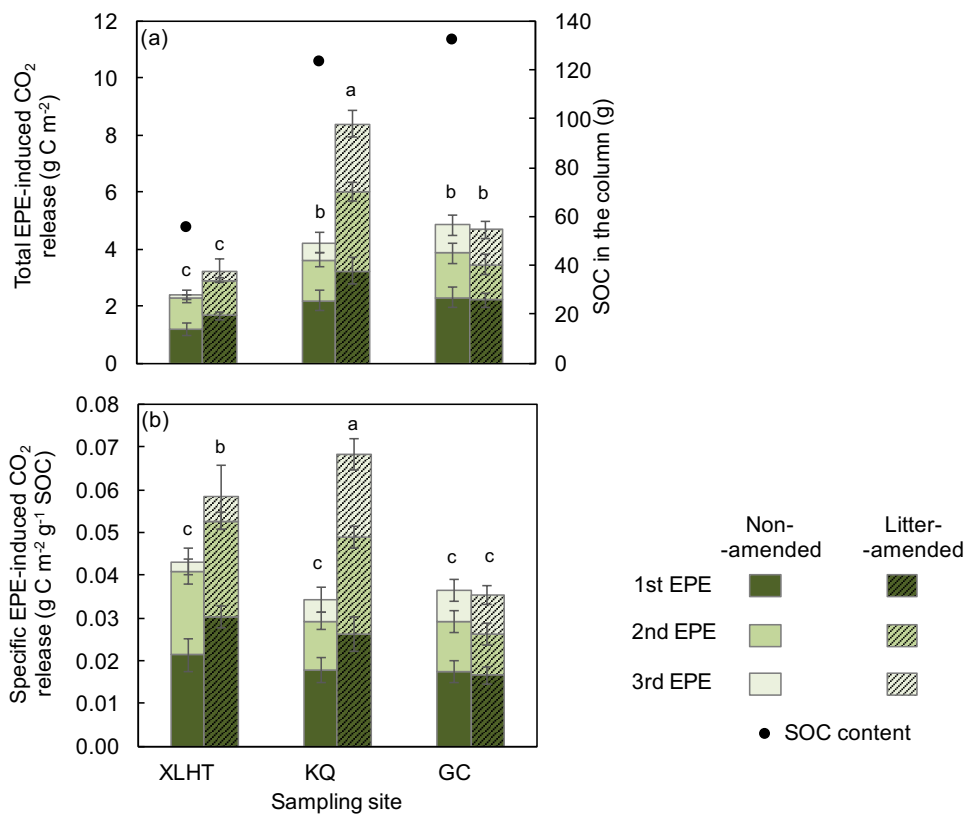
The experimental setup seems appropriate for the objectives presented in the manuscript, however the presented results need some reconsideration and recalculation, especially respiration data should be presented as specific respiration to account for different SOC contents in the investigated soils (for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

## **Response:**

Following the reviewer's suggestion, we calculate the specific respiration data normalization to SOC and added in Sections 3.2 and Figure 3b as follows:

“... The specific soil respiration rates normalized to SOC were 2.2, 2.6, and 7.9  $\mu\text{g C g}^{-1} \text{SOC h}^{-1}$  in the GC, KQ, and XLHT soils, respectively. This indicated that SOC in the XLHT soils was easier to degrade despite its low content.”

Figure:



“Figure 3: Total (a) and specific (b) extreme precipitation event (EPE)-induced CO<sub>2</sub> release in the litter-amended and non-amended grassland soils during three EPEs. Mean values are shown with standard deviation ( $n = 3$ ). Lower-case letters (a, b, c) indicate significantly different levels among the litter-amended and non-amended soils determined by Duncan’s multiple range test (one-way ANOVA,  $p < 0.05$ ).”

### Section 3.2:

“Using data shown in Fig. S3-4, we calculated that total EPE-induced CO<sub>2</sub> release during three EPEs was higher in the KQ and GC soils than in the XLHT soil ( $p < 0.05$ ; Fig. 3a) with a lower SOC content and a lower SOC:N ratio (Table 1). However, the specific EPE-induced CO<sub>2</sub> release normalized to SOC content showed no significant difference in the non-amended soils among three sites (Fig. 3b), indicating that a similar proportion of SOC (~4%) was subject to EPE-induced CO<sub>2</sub> release in the alpine and temperate grassland soils. The total EPE-induced CO<sub>2</sub> release (including CO<sub>2</sub> from both litter and SOC mineralization) was significantly higher in the litter-amended KQ soils than the non-amended ones, similar to the specific EPE-induced CO<sub>2</sub> in the KQ and XLHT soils. The specific EPE-induced CO<sub>2</sub> was significantly different for the litter-amended soils among sites ( $p < 0.05$ ), showing a pattern of KQ > XLHT > GC. This pattern was consistent with the mean sand content in the order of KQ (46.9%) > XLHT (39.1%) > GC (27.2%). In addition, the higher total and specific EPE-induced CO<sub>2</sub> release in the litter-amended KQ soils may be related to its relatively lower soil pH (~7.7), which facilitates the release rather than the dissolution of respired CO<sub>2</sub> in soil solution. We therefore conclude that the KQ soil, with a coarser texture and a lower pH (Table 1), may have provided less sorptive protection for the labile DOC components after EPEs (Kell et al., 1994; Nelson et al., 1994) and allowed less dissolution of the respired CO<sub>2</sub>, and hence showed a more responsive respiration to the precipitation events. These results suggest that SOC contents and SOC:N ratios are important factors influencing the total EPE-induced CO<sub>2</sub> release, while the availability of labile

*organic carbon, soil texture and pH are key factors affecting the specific EPE-induced CO<sub>2</sub> release in these grassland soils.”*

**Specific Comments:**

**Comment 3:**

Page 3, line 25: The dimension of the soil pits seems quite 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.

**Response:**

Thank you for pointing out this inaccurate description. We have confirmed that the pit was 25 cm × 25 cm × 70 cm. This is corrected in the text.

**Comment 4:**

Page 5, line 1: How was the water added to the soil columns? Did you have 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 CO<sub>2</sub> measurements? Were they closed or flushed with constant air flow? Please provide more information about the experimental setup for the reader.

**Response:**

More information about the experimental setup is added at Section 2.2 as follows:

*“...approximately 1 L of rainwater (rainfall of ~127 mm), comparable to 30% of the MAP of the investigated sites, was added to the surface of each soil column over 3–4 h at rates of one drop per second using syringes and allowed to leach through the column to be collected with a clean beaker within 12–14 h. ... To monitor soil respiration every 1–2 days following each EPE, soil columns were first aerated for 1 h using CO<sub>2</sub>-depleted air that had been passed through saturated sodium hydroxide (NaOH) solutions (twice; Fig. 1) and then incubated for 4 h with lids closed. CO<sub>2</sub> gas in the column headspace was collected by gas-tight syringes for the subsequent measurement. After collection of CO<sub>2</sub> gas, the lids were open to allow the exchange with the ambient air. Soil respiration was monitored for 30 days after the first EPE and observed to stabilize approximately on the 20<sup>th</sup> day (Fig. S3). Hence, the first, second, and third EPEs were conducted on the 1st, 31st, and 51st day of incubation, and the CO<sub>2</sub> measurement was conducted for approximately 30, 20, and 20 days after the first, second, and third EPEs, respectively.”*

**Comment 5:**

Page 7, line 8: 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 CO<sub>2</sub>-C g<sup>-1</sup> SOC h<sup>-1</sup>). This would allow an easier comparison of the different soils with different SOC content.

**Response:**

The specific respiration rate has been added in Section 3.2 as follows:

*“...The specific soil respiration rates normalized to SOC were 2.2, 2.6, and 7.9 μg C g<sup>-1</sup> SOC h<sup>-1</sup> in the GC, KQ, and XLHT soils, respectively. This indicated that SOC in the XLHT soils was easier to degrade despite its low content. “*

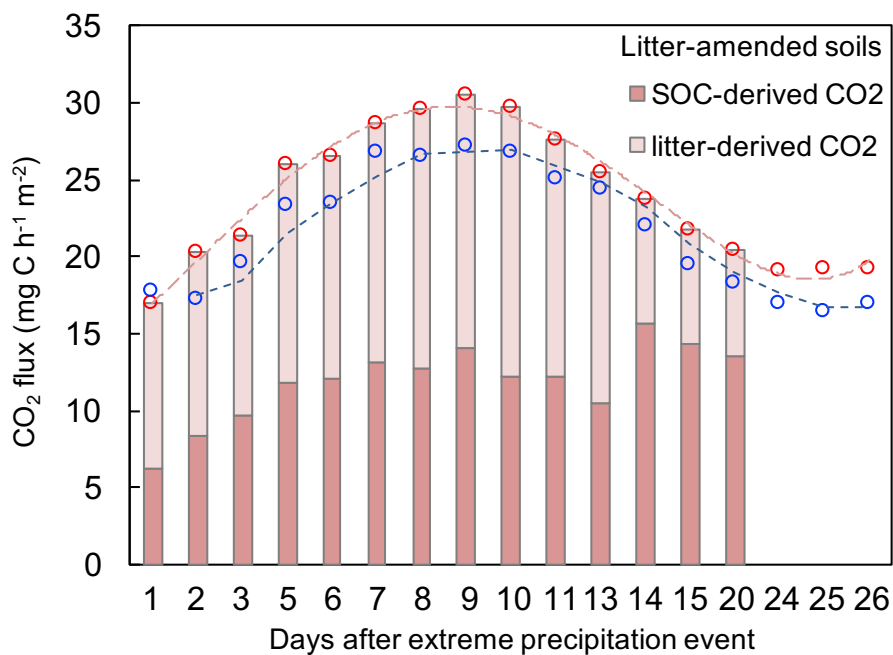
### Comment 6:

Page 7 line 13: 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  $\delta^{13}\text{CO}_2$ , they should be able to separate litter mineralization from SOC mineralization.

### Response:

Good point! We take the GC soils as an example to quantify the relative contribution of litter and SOC mineralization to the respired  $\text{CO}_2$  in the following parts and added one inside-figure in Fig. S3:

Figure:



Inside figure of Fig. S3: Variations of litter-derived and SOC-derived  $\text{CO}_2$  release with time in the litter-amended GC soils during the first EPE.

### Section 3.2

“Total respired  $\text{CO}_2$  was higher in the litter-amended than non-amended soils before and after EPEs (Fig. S3), likely caused by the degradation of labile components in the fresh litter and/or increased degradation of SOC primed by litter additions (Fröberg et al., 2005; Ahmad et al., 2013). To further differentiate the contribution of litter (C4) and SOC (C3) to the respired  $\text{CO}_2$ , we examined the  $\delta^{13}\text{C}$  values of  $\text{CO}_2$  evolved from the GC soils after the first EPE. ... Using the two-endmember mixing model of Eq. (1) and (2), we calculated that litter contributed ~64% of the respired  $\text{CO}_2$  in the litter-amended GC soils. However, along with the consumption of labile carbon in litter, the  $\delta^{13}\text{C}$  signature of  $\text{CO}_2$  decreased from  $-18.7\text{‰}$  on Day 1 to  $-21.8\text{‰}$  on Day 25 after EPE in the litter-amended soils (Fig. 2). Accordingly, the proportion of litter-derived  $\text{CO}_2$  decreased from 64% to 20%. The litter-derived  $\text{CO}_2$  flux in litter-amended GC soils was estimated to range from  $7.0$  to  $17.5 \text{ mg C m}^{-2} \text{ h}^{-1}$ , while the SOC-derived  $\text{CO}_2$  flux increased from  $6.2$  to  $15.7 \text{ mg C m}^{-2} \text{ h}^{-1}$  after the first EPE (Fig. S3). Compared with the SOC-derived  $\text{CO}_2$  flux in non-amended GC soils (ranging from  $17.2$  to  $27.1 \text{ mg C m}^{-2} \text{ h}^{-1}$ ), litter addition had a negative priming effect on the degradation of native SOC while increasing total respiration through labile litter degradation.”

### Comment 7:

Page 7, line 17: (1) The used mixing model should be mentioned in the method section. (2) Further with the given isotopic values I cannot understand how the authors calculated contribution of litter mineralization to total respiration. Using a mixing model of:  $1 - ((c_{\text{mix}} - c_{\text{litter}}) / (c_{\text{control}} - c_{\text{litter}}))$ , where  $c_{\text{mix}}$  is the isotopic value of CO<sub>2</sub> from the litter amended sample (-18,7‰,  $c_{\text{litter}}$  the isotopic value of the added litter (-16,2‰ and  $c_{\text{control}}$  the isotopic value of CO<sub>2</sub> 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  $\delta^{13}\text{CO}_2$  values did you used for the control (non- amended) samples. Did you measured  $\delta^{13}\text{CO}_2$  for the control only at the beginning or at the same resolution as  $\delta^{13}\text{CO}_2$  for the litter-amended samples? (3) Further, are there any isotopic measurements of the other two sites. If so, why they are not shown here?

### Response:

(1) The description on the mixing model has been added in Section 2.4 as follows:

*“The relative contribution of litter- and SOC-derived CO<sub>2</sub> to total respired CO<sub>2</sub> in the litter-amended soils was estimated using the following mass balance model:*

$$f_{\text{litter-derived}} + f_{\text{SOC-derived}} = 1 \quad (1)$$

$$f_{\text{litter-derived}} \times \delta^{13}\text{C}_{\text{litter-derived}} + f_{\text{SOC-derived}} \times \delta^{13}\text{C}_{\text{SOC-derived}} = \delta^{13}\text{C}_{\text{respired-CO}_2} \quad (2)$$

*where  $f_{\text{litter-derived}}$  and  $f_{\text{SOC-derived}}$  are the proportion of litter- and SOC-derived CO<sub>2</sub> in the total respired CO<sub>2</sub>;  $\delta^{13}\text{C}_{\text{litter-derived}}$  is the  $\delta^{13}\text{C}$  value of litter-derived CO<sub>2</sub>, equivalent to -16.25‰;  $\delta^{13}\text{C}_{\text{SOC-derived}}$  is the  $\delta^{13}\text{C}$  value of SOC-derived CO<sub>2</sub>, which assumes the same value as that in the non-amended soils at the beginning of incubation (-23.1‰) according to Cerling et al. (1991);  $\delta^{13}\text{C}_{\text{respired-CO}_2}$  is the measured  $\delta^{13}\text{C}$  of respired CO<sub>2</sub>.”*

(2) Regarding the calculation results on the proportion of litter-derived CO<sub>2</sub>, I must admit that I had made a very stupid mistake. I previously wrongly calculated the value of ( $c_{\text{control}} - c_{\text{litter}}$ ) as -8.95‰, which actually was -6.85‰ (i.e., (-23.1‰) - (-16.25‰)), and this mistake resulted in the incorrect proportions (72% to 39%). I have revised these values and re-checked all the data in this manuscript. The revised parts are as follows:

*“Using the two-endmember mixing model of Eq. (1) and (2), we calculated that litter contributed ~64% of the respired CO<sub>2</sub> in the litter-amended GC soils. However, along with the consumption of labile carbon in litter, the  $\delta^{13}\text{C}$  signature of CO<sub>2</sub> decreased from -18.7‰ on Day 1 to -21.8‰ on Day 25 after EPE in the litter-amended soils (Fig. 2). Accordingly, the proportion of litter-derived CO<sub>2</sub> decreased from 64% to 20%.”*

(3) Due to budget constraints and logistic reasons, we measured the isotopes of respired CO<sub>2</sub> only in the GC soil during the first extreme precipitation event. The other two soils were not measured. As the present study mainly aimed to compare the relative importance of respiration and leaching in EPE-induced soil carbon loss from grassland soils, the isotopic analysis was conducted for only one soil to provide some information regarding the source(s) of the leached and respired carbon.

### Comment 8:

Page 7 line21.: (1) 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. (2) “EPE-induced CO<sub>2</sub> 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?

**Response:**

(1) Calculation on EPE-induced CO<sub>2</sub> release has been moved to the method Section 2.4 as follows:

*“EPE-induced CO<sub>2</sub> release via respiration was assessed following two steps. First, cumulative respiration during the first 20 days after each EPE (until respiration rate stabilized) was calculated. Second, difference between the measured cumulative respiration and that estimated using the stabilized basal respiration rate after each EPE was calculated as the EPE-induced CO<sub>2</sub> release.”*

(2) Results on the specific EPE-induced CO<sub>2</sub> release normalized to the SOC content has been added as Fig. 3b, and discussed in Section 3.2 (please see our response to Comment 2).

**Comment 9:**

Page 7, line 25: “Litter amendment significantly increased the EPE-induced CO<sub>2</sub> 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 CO<sub>2</sub> release which is the sum of CO<sub>2</sub> from litter mineralization and SOC mineralization from the mineral soil. Therefore, it is only possible to say that EPE increased total CO<sub>2</sub> release. However, as mentioned above, I would recommend to use the isotopic data of CO<sub>2</sub> 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 CO<sub>2</sub> 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 CO<sub>2</sub> 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 CO<sub>2</sub> 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.

**Response:**

We agree with the first part of the comment and have quantified the litter- and SOC-derived CO<sub>2</sub> as details below. As mentioned in our replies to previous comments, the isotopes of respired CO<sub>2</sub> were only measured in the GC soils during the first extreme precipitation event due to logistic reasons. Nonetheless, we have added detailed discussion on the total EPE-induced CO<sub>2</sub> release as well as its influencing factors in the following parts:

Section 3.2

*“Total respired CO<sub>2</sub> was higher in the litter-amended than non-amended soils before and after EPEs (Fig. S3), likely caused by the degradation of labile components in the fresh litter and/or increased degradation of SOC primed by litter additions (Fröberg et al., 2005; Ahmad et al., 2013). To further differentiate the contribution of litter (C<sub>4</sub>) and SOC (C<sub>3</sub>) to the respired CO<sub>2</sub>, we examined the  $\delta^{13}\text{C}$  values of CO<sub>2</sub> evolved*

*from the GC soils after the first EPE. ... Using the two-endmember mixing model of Eq. (1) and (2), we calculated that litter contributed ~64% of the respired CO<sub>2</sub> in the litter-amended GC soils. However, along with the consumption of labile carbon in litter, the δ<sup>13</sup>C signature of CO<sub>2</sub> decreased from -18.7‰ on Day 1 to -21.8‰ on Day 25 after EPE in the litter-amended soils (Fig. 2). Accordingly, the proportion of litter-derived CO<sub>2</sub> decreased from 64% to 20%. The litter-derived CO<sub>2</sub> flux in litter-amended GC soils was estimated to range from 7.0 to 17.5 mg C m<sup>-2</sup> h<sup>-1</sup>, while the SOC-derived CO<sub>2</sub> flux increased from 6.2 to 15.7 mg C m<sup>-2</sup> h<sup>-1</sup> after the first EPE (Fig. S3). Compared with the SOC-derived CO<sub>2</sub> flux in non-amended GC soils (ranging from 17.2 to 27.1 mg C m<sup>-2</sup> h<sup>-1</sup>), litter addition had a negative priming effect on the degradation of native SOC while increasing total respiration through labile litter degradation.”*

*“Using data shown in Fig. S3-4, we calculated that total EPE-induced CO<sub>2</sub> release during three EPEs was higher in the KQ and GC soils than in the XLHT soil ( $p < 0.05$ ; Fig. 3a) with a lower SOC content and a lower SOC:N ratio (Table 1). However, the specific EPE-induced CO<sub>2</sub> release normalized to SOC content showed no significant difference in the non-amended soils among three sites (Fig. 3b), indicating that a similar proportion of SOC (~4%) was subject to EPE-induced CO<sub>2</sub> release in the alpine and temperate grassland soils. The total EPE-induced CO<sub>2</sub> release (including CO<sub>2</sub> from both litter and SOC mineralization) was significantly higher in the litter-amended KQ soils than the non-amended ones, similar to the specific EPE-induced CO<sub>2</sub> in the KQ and XLHT soils. The specific EPE-induced CO<sub>2</sub> was significantly different for the litter-amended soils among sites ( $p < 0.05$ ), showing a pattern of KQ > XLHT > GC. This pattern was consistent with the mean sand content in the order of KQ (46.9%) > XLHT (39.1%) > GC (27.2%). In addition, the higher total and specific EPE-induced CO<sub>2</sub> release in the litter-amended KQ soils may be related to its relatively lower soil pH (~7.7), which facilitates the release rather than the dissolution of respired CO<sub>2</sub> in soil solution. We therefore conclude that the KQ soil, with a coarser texture and a lower pH (Table 1), may have provided less sorptive protection for the labile DOC components after EPEs (Kell et al., 1994; Nelson et al., 1994) and allowed less dissolution of the respired CO<sub>2</sub>, and hence showed a more responsive respiration to the precipitation events. These results suggest that SOC contents and SOC:N ratios are important factors influencing the total EPE-induced CO<sub>2</sub> release, while the availability of labile organic carbon, soil texture and pH are key factors affecting the specific EPE-induced CO<sub>2</sub> release in these grassland soils.”*

#### **Comment 10:**

Page 8 line 8: The authors mentioned that CO<sub>2</sub> 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 CO<sub>2</sub> is dissolved in the GC and XLHT soils due to the higher pH Therefore CO<sub>2</sub> 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.

#### **Response:**

The relative effect of litter amendment on DIC fluxes is now discussed in this paragraph. Because the added litter-OC was only 0.7 g per column calculated using added litter biomass of 1.59 g and litter-OC content of 43%, and the maximum DIC content derived from dissolution of CO<sub>2</sub> produced by litter degradation was 0.8 g assuming that all the litter-OC was mineralized and that all the produced CO<sub>2</sub> was dissolved in soil solution. Compared with the SIC content (38.15 g C per column) in XLHT soils, litter-mineralization-derived DIC was far smaller, but the dissolved CO<sub>2</sub> derived from litter-OC mineralization could be more



important in the KQ soils having only 0.7 g SIC per column. To clarify this, we have revised this paragraph as follows:

*“... Due to the high SIC content in the XLHT soils (38.15 g per column) and the low litter-OC amendment (0.7 g per column), there was no significant difference of DIC fluxes between the non-amended and litter-amended XLHT soils (Fig. 4a). However, for the KQ soil having a relatively low SIC content which was similar to the added litter-OC (0.7 g per column; Table 1), the influence of litter addition on the DIC flux was quite obvious. Therefore, although the contribution of dissolved CO<sub>2</sub> to DIC fluxes should be more important in high-pH soils, the relative effect of litter amendment on DIC fluxes under EPEs seemed more significant in soils with a low SIC content.”*

**Comment 11:**

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

**Response:**

Due to budget constraints and logistic reasons, we only measured the isotopes of leached DIC in the XLHT soils and took it as an example to trace the source of the leached DIC. This is now mentioned in the Methods.

**Comment 12:**

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?

**Response:**

Given the above comments and discussions, it may be inaccurate to treat the degradation rate as the same as the CO<sub>2</sub> release rate. To clarify, we have revised this sentence as follows:

*“Interestingly, increasing DIC fluxes with repeated EPEs were not observed in the KQ and GC soils (Fig. 4) despite their higher SOC contents and CO<sub>2</sub> release rates (Fig. S5).”*

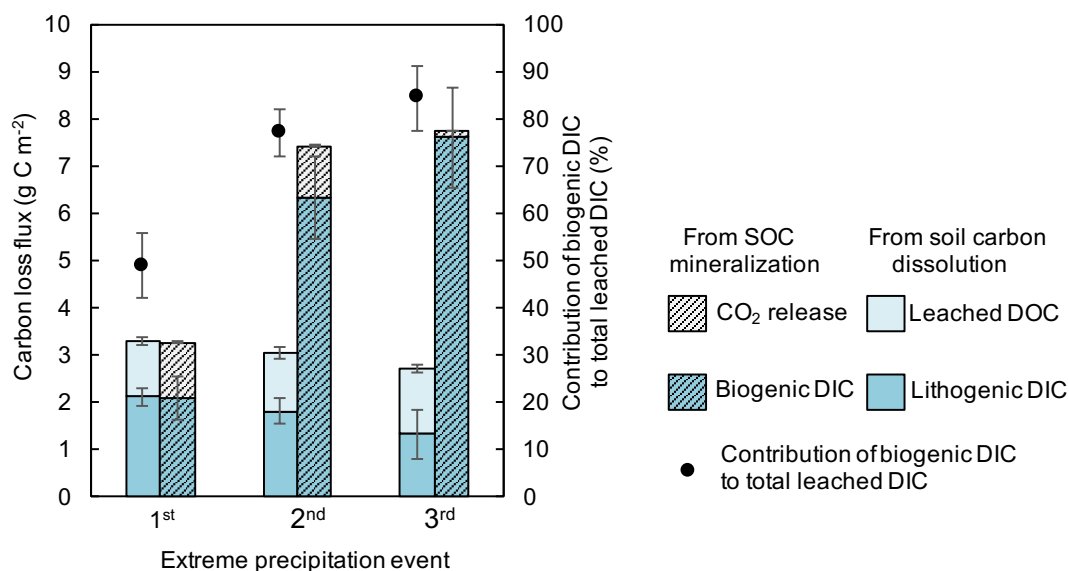
**Comment 13:**

Page 8, line 31: 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 CO<sub>2</sub> from SOC and litter mineralization is a major source for DIC. Therefore, the high value of 32.5 mg C g<sup>-1</sup> 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 alkaline soils might be questioned, at least for the KQ soils.

**Response:**

Good point! It is inaccurate to say that SIC loss is the main form of soil carbon loss in neutral to alkaline soils. We have now used DIC flux rates in the units of mg C g<sup>-1</sup> soil to compare with SOC loss. In addition, regardless of the DIC sources, DIC (not SIC) was a major form of carbon loss from these grassland soils. In view of its sources, biogenic DIC should not be neglected. Therefore, we have revised Figure 5 and added one paragraph in Section 3.3 to discuss the role of biogenic DIC in leached DIC.

Figure :



“Figure 5: The flux of carbon loss from soil organic carbon (SOC) mineralization including CO<sub>2</sub> release and biogenic dissolved inorganic carbon (DIC), and that from soil carbon dissolution including leached dissolved organic carbon (DOC) and lithogenic DIC in the XLHT soils. Mean values are shown with standard error (n = 3).”

### Section 3.3

“Regardless of its source, the EPE-induced leaching loss of inorganic carbon was 31.5 and 10.6 μg DIC g<sup>-1</sup> soil from the alkaline XLHT and GC soils, respectively, approximately three and five times higher than the corresponding DOC leaching loss (5.9 and 3.9 μg DOC g<sup>-1</sup> soil, respectively). However, the KQ soil had a relatively lower EPE-induced DIC loss (4.4 μg DIC g<sup>-1</sup> soil) than the DOC leaching loss (11.6 μg DOC g<sup>-1</sup> soil) mainly due to its lower initial SIC content and relatively neutral soil pH value. Hence, DIC was the main form of soil carbon loss in alkaline soils during EPEs regardless of its source. When the source of the leached DIC is taken into account, dissolution of CO<sub>2</sub> produced by SOC mineralization (biogenic DIC) constituted more than half of the leached DIC (at least from the XLHT soils; Fig. 5), whose contribution increased with re-occurring EPEs (Fig. 5). This implies that SOC mineralization measured by CO<sub>2</sub> fluxes was under-estimated by approximately 8 times in the XLHT soils during the three EPEs (Fig. 5). In addition, DIC loss exclusively resulting from SIC dissolution or weathering was also a significant fraction of soil carbon loss, equivalent to 219% SOC loss in the form of CO<sub>2</sub> during EPEs (Fig. 5). These results collectively corroborate that inorganic carbon loss is the main form of soil carbon loss in alkaline soils during EPEs.”

### Comment 14:

Page 9, line 33: “It is also worth mentioning that soil carbon leaching fluxes in this study (10.1–25.3 g C m<sup>-2</sup> yr<sup>-1</sup>) far exceed annual SOC loss through warming-enhanced respiration at these sites (0.2–0.6 g C m<sup>-2</sup> yr<sup>-1</sup>) 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 CO<sub>2</sub> needs first mineralization of SOC by microorganism.

### Response:

After considering both Reviewer #1 and #2's comments, we have removed this part to make this manuscript more rigorous and accurate.

**Comment 15:**

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

**Response:**

Revised. Thank you!