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Biogeosciences Discussions

Interactive comment on "Carbon input control over soil organic matter dynamics in a temperate grassland exposed to elevated CO_2 and warming" *by* Y. Carrillo et al.

Y. Carrillo et al.

dcarril1@uwyo.edu

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Response to Anonymous Referee no. 2

Comment # 1: "The present study does not show enough novel information"

We consider our study to contain novel, valuable and unique information. In particular,

1. There are very few studies that address the combined ecosystem level effects of elevated CO2 and atmospheric warming on individual SOM pools and their dynamics. Further, studies that are able to look at the combined effects of warming and CO2 often assess C dynamics based on soil respiration and not on the direct assessment

of individual pool sizes and their turnover rates.

2. Our findings indicate interactive effects of experimental warming and elevated CO2 on the proportion of soil C in labile form and its decomposability, despite no apparent effect in the total C pool, demonstrating the importance of experiments that combine climate change factors, and the need to examine individual pools to assess the effects of global change factors on SOM.

3. Enhanced decomposition of soil organic matter due to greater availability of labile C, called the priming effect, has been documented multiple times under incubation conditions following the addition of C substrates in the absence of plants (Fontaine et al., 2007; Blagodatskaya and Kuzyakov, 2008). In our study, we found a positive relationship between the degradability of the resistant SOM pool and dissolved organic C as well as field-assessed plant biomass. These relationships suggest that the greater concentration of labile C substrates present in soil enhanced the susceptibility to decomposition of the resistant SOM. Our study is unique in that the CO2 and temperature treatments in the field experiment led to a gradient in C availability, which was then related to decomposability in the laboratory experiment.

4. Most field studies that have suggested priming of OM decomposition are based on measurements of CO2 flux or assessments of total SOM content. Thanks to the use of long term incubations under standard conditions, we were able to examine the response of the resistant pool in particular, as opposed to the bulk SOM pool. Changes in the dynamics of long-lived, resistant SOM pools have the highest potential to impact long term C storage.

Comment #2: "There were too many hypotheses in the introduction."

We appreciate this suggestion regarding the hypotheses. We have restructured our manuscript to simplify our goals. Our revised manuscript now is structured around two central questions:

1. How do elevated CO_2, warming and their combination affect SOM pools and their susceptibility to decomposition?

2. Are the responses of SOM pools and their susceptibility to decomposition associated with plant biomass and dissolved organic C (DOC)?

Comment #3: "One of the hypotheses is that total mineralizable C and the labile C pool size would not change with warming due to decreased soil moisture offsetting direct warming effects on decomposition, while the combination of elevated CO_2 and warming would increase the microbially available C assuming soil water was sufficient for plant activity. However, I think the introduction of importance and mechanism of the water regulation on SOM pools is not enough in the current version."

Our revised manuscript is structured around two central questions (see response to Comment # 2). The role of soil moisture in mediating responses to CO_2 and warming are addressed in the introduction and further in the discussion when pertinent to explaining our observations.

Comment #4: "The major flaw of this study is that the results of long-term laboratory incubations were based on short term (1-2) year field treatment. The changes in soil C pools are relatively long-term processes under climate change, so the pre-experimental conditions of SOM is likely to be more important than treatments themselves in influencing the results in this study.

We agree that responses of SOM pools are sensitive to the duration of exposure to treatments, and that early responses might differ from later ones. However, early responses (first 1-2 years) to experimental manipulations can be representative of longer term responses. For example, Melillo et al. (2002) observed that the initial stimulatory effect of warming on soil respiration remained for the first six years after the beginning of warming. In their case, that early response, the cessation of it and its potential cause was critical to explain the longer term behavior of soil respiration and SOM pools. In fact, obtaining information on SOM processes during the first few years of experimen-

tal manipulation is highly critical to modeling efforts because it helps constrain model assumptions about longer-term dynamics (Parton et al., 2007; Allison et al., 2010).

We also agree that changes in bulk soil C pools are usually detected only after long exposures. However, our earlier work using similar laboratory incubations in a different experiment demonstrated measurable changes in labile C after one year of elevated CO_2 (Pendall and King, 2007). In the present study, we assessed labile and resistant pools separately and demonstrated early interactive effects of warming and elevated CO_2 in labile pool sizes and decomposition rates, despite no detectable effect on the total soil C pool. These results confirm the sensitivity of the incubation approach to detecting rapid impacts on C pools with short turnover times. Additional ongoing experiments are following the longer-term responses of SOM pool dynamics in the PHACE experiment.

We concur that pre-experimental conditions could interfere with detecting actual treatment effects. To account for pre-experimental soil C conditions and potential impacts on treatment effects, we standardized the obtained mineralizable C, pool sizes and decomposition rates by dividing them by the concentration of C (g C/g soil) present in soil in each plot and at each depth.

Comment #5: "The period of growing season, e.g., which months, should be defined in the Methods section."

The growing season begins in early April and ends in late October. The time of peak aboveground biomass is, approximately, in mid to late July. Our revised manuscript includes this information in the Methods.

Comment #6: "The authors showed that total precipitation in 2007 was about 60% of that in 2008 at the time of soil collection. As water is usually a predominant factor in controlling plant growth in semi-arid ecosystems, it seems that the NPP could be greater than that in 2007. Thus it could be incorrect to use data from both 2007 and 2008 together for the regression analyses in Fig. 2 and Fig. 3. It would be better if data from different growing seasons are labeled in the figures."

Figures 1 and 2 present those regressions with data labeled for different growing seasons. We did not observe that the differences in biomass between years were large or that they were strongly driving the relationships between root and aboveground biomass or between biomass and DOC. If there had been larger differences in biomass between years, we would still expect that greater aboveground biomass would be associated with a greater fine root biomass and that greater biomass would translate into higher concentrations of DOC. Our revised manuscript will include the modified figures.

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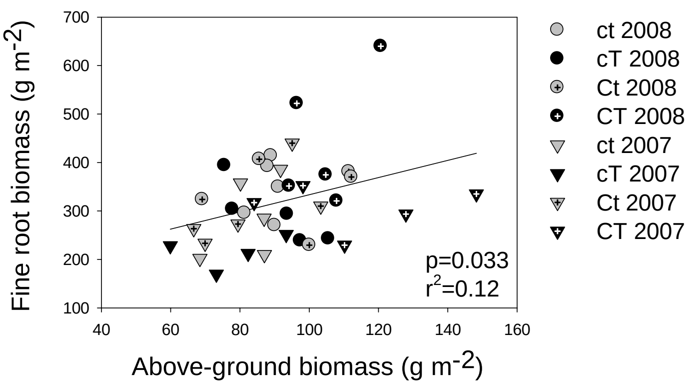

Fig. 1. Linear relationship between above-ground plant biomass and fine root biomass (0-15 cm depth) at the time of peak biomass in 2007 and 2008

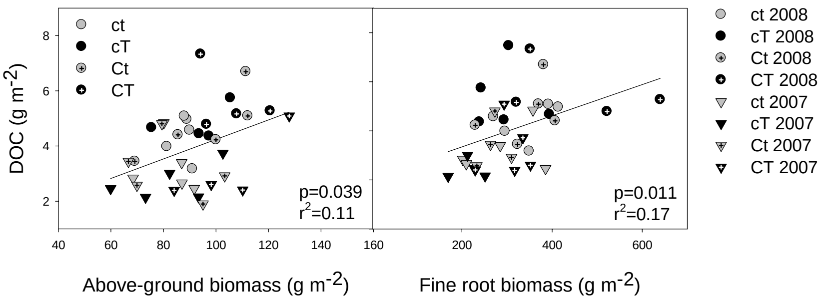

Fig. 2. Linear relationship between dissolved organic C in soil (0-15 cm; DOC) and above-ground plant biomass and fine root biomass (0-15 cm) at the time of peak biomass in 2007 and 2008.