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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.

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Response to anonymous reviewer # 1

Comment 1

We appreciate the opportunity to address Reviewer 1's concerns about the application of controlled laboratory incubations to assess soil organic matter (SOM) decomposition. This topic has generated considerable debate in the literature, including several highly cited papers in Biogeosciences (Conen et al., 2006; Reichstein et al., 2005). We contend that laboratory incubation is a valuable approach which is sensitive to changes in SOM decomposability that cannot be detected using other methods. The present

study demonstrates (for the first time in the literature) interactive effects of experimental warming and elevated carbon dioxide concentrations on the proportion of soil C in labile form and its decomposability, despite no apparent effect in the total C pool. We also discovered an interesting and novel relationship between resistant SOM decomposition rate and biomass and dissolved organic carbon, indicated the importance of substrate availability of labile C in resistant C turnover. This priming effect is likely to play a strong role in regulating climate change effects on resistant SOM decomposition rates and is a unique and important finding of this work.

We agree that actual field decomposition rates will be dependent on field temperature and moisture conditions. However, our work aimed at determining whether the exposure to the elevated carbon dioxide and warming treatments had generated changes in the intrinsic decomposability of the labile and resistant pools. That is precisely an advantage of incubations at standard conditions: they allow to assess whether experimental treatments have modified the characteristics of C pools that affect their susceptibility to degradation, while removing the environmental constraints and confounding effects at play in a field situation (Davidson and Janssens, 2006). The temperature and moisture levels chosen for our study are near optimal, and also relevant to natural field conditions.

We strongly disagree with the idea that laboratory incubations cannot be used to estimate kinetic parameters. Most ecosystem and global scale biogeochemistry models use decomposition constants derived from laboratory incubations, such as CENTURY (Parton et al., 1987; Parton et al., 1993) and ROTHAMSTED-C (Jenkinson et al., 1991). In addition, many previous studies have used laboratory incubation approaches to evaluate responses in the kinetic parameters of organic matter pools to experimental field treatments (e.g. Collins et al., 2000; Dijkstra and Cheng, 2007; Fissore et al., 2008; Pendall and King, 2007; Taneva and Gonzalez-Meler, 2008; Dijkstra et al., 2006) as well as to compare processes rates under standard conditions as affected by previous exposure to experimental treatments (e.g. Carney et al., 2007; Langley

et al., 2009). Attempts to assess kinetic decomposition parameters from intact soils in the field are unfortunately confounded by contributions from root respiration, unless mechanical or isotopic partitioning techniques are used (Hanson et al., 2000), and by moisture limitations (Davidson and Jannsens, 2006). Laboratory incubations therefore complement field studies.

Comment 2

We are happy to clarify the potential contribution of carbonates to the evolved CO2 in the laboratory incubations. Carbonate contents in our field site are highly stratified and the concentrations in top soil are very low. Average carbonate content by weight in our soils is 0.0045 % at 0-5 cm (S.D. = 0.062 as in most cases carbonates were not detectable) and 0.2056 % (S.D.= 0.61) at 5-15 cm (Sherrod et al., 2002). Further, the isotopic composition of CO2-C released from these soil incubations ranges from -18 to -25‰ consistent with SOM values in the different treatments (CO2 and SOM δ13C data not included in this manuscript but available on request). Thus, we are confident that C from carbonates did not cause a bias in our results. All of our soils are routinely processed for carbonate removal before C and delta 13C analyses for consistency, because carbonate content below 15 cm is higher.

Comment 3

Our study is unique because it utilizes soil exposed to the combined effects of elevated carbon dioxide and ecosystem warming in an intact native grassland. We agree that the early responses measured in the first few years are likely to contrast longer-term responses (Melillo et al., 2002). Our study is valuable because it documents early responses in sensitive (labile) pools, supporting the hypothesis that labile C may be more susceptible to initial impacts of experimental climate change than resistant C. Part of the reason why responses of soil C to climate change treatments are difficult to detect in the short term is that most studies do not assess individual C pools. One of the contributions of our study results from the fact that individual pools were assessed.

Additional ongoing experiments are following the longer-term responses of SOM pool dynamics in the PHACE experiment.

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