

Interactive
Comment

***Interactive comment on “Carbon input control
over soil organic matter dynamics in a temperate
grassland exposed to elevated CO₂ and warming”
by Y. Carrillo et al.***

Anonymous Referee #2

Received and published: 29 March 2010

Interactive comment on “Carbon input control over soil organic matter dynamics in a temperate grassland exposed to elevated CO₂ and warming” by Y. Carrillo et al.

This paper describes results of a long-term laboratory incubation of soil in a semi-arid grassland, looking specifically at the interactive effects of short-term exposure to elevated CO₂ and warming on soil organic matter dynamics. They used a two pool decay model to estimate the size and decay rate of different soil C pools, and found positive relationships between decomposition rate of the resistant C and plant-input C. These results highlight the importance of incorporate the biological factors, such as plant dynamics, into C cycle models. However, this study does not show enough novel

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



information and can not be accepted in its current form.

There were too many hypotheses in the introduction section (Page 1579, Line 12-26). I suggest the authors to give 1-2 key hypotheses or expectations at the end of introduction section, or else it will be confusing.

One of the hypotheses is that total mineralized C and the labile C pool size would not change with warming due to decreased soil moisture offsetting direct warming effects on decomposition, while soil water was sufficient for nominal plant activity under the combination of elevated CO₂ and warming (Page 1579, Line 16-20). However, I think the introduction of importance and mechanism of the water regulation on SOM pools (Page 1578, Line 23-25) is not enough in the current version.

The major flaw of this study is that the results of long-term laboratory incubations were based on a 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.

The period of growing season, e.g., which months, should be defined in the Methods section (Page 1580, Line 16).

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 in 2008 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.

Interactive comment on Biogeosciences Discuss., 7, 1575, 2010.

BGD

7, C346–C347, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

