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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.

M. Bahn (Editor)

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The reviewers have raised a number of serious concerns, and some make a case that your study might be better suited for a more specialized journal. I am sorry to inform you that I largely agree with them, and therefore do not encourage you to submit a revised manuscript to Biogeosciences.

Your in situ experiment combining elevated CO2 and warming in a fully factorial design certainly provides an excellent basis for obtaining novel insights into a range of processes. The measurements appear to be of high quality, and your manuscript is well written and documents a good command of the literature. However, there are

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some major problems to your study and the manuscript (cf. also comments by referees #1-3), including the following issues: 1) A mismatch in the duration of the experimental treatments in relation to incubation time (Ref #1 and 2). As an extreme example, your first samples, taken after a warming treatment lasting only 3 months, were incubated for a period of more than 3 months. Generally, longer treatment times will likely be needed for assessing effects of elevated CO2 and warming on soil organic matter dynamics (Ref #1). 2) Possible confounded effects between incubation temperatures and warming treatments. The warming treatment of 1.5-3°C is assessed on the background of incubation temperatures of either 15 or 25°C at different times of the year, which makes it difficult to disentangle possible effects of previous C inputs from those related to potentially different temperature sensitivities of the decomposition of different SOC pools (cf. also comment by Ref #1). Also, it needs to be demonstrated that the method for estimating pool sizes and, especially, decay rates is temperature independent (cf. also comment by Ref #1). 3)Generally, the application of different incubation temperatures and times in make a direct comparison of your results across the three sampling times at best difficult. Though you state yourself that you do not intend to make such comparisons, you actually do suggest them the way you present your results (Figs. 3-6). 4)'Priming' effects cannot be inferred as there were no plant parts present during your incubation (Ref #3). Furthermore, by plotting resistant C pool decomposition as a function of plant biomass (Fig. 6) you suggest the possibility of such a relationship (and infer it for the 2007 data set) though, again, during the incubation no plant biomass was present.

While I understand that these comments may be disappointing, I do encourage you to reply to all the comments that have been made. I furthermore hope that the feedbacks will be helpful for improving the manuscript, which you may like to submit to a more specialized journal, and for designing future studies based on your manipulation experiment.

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