

Interactive comment on “Mechanisms of soil carbon storage in experimental grasslands” by S. Steinbeiss et al.

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The paper by Steinbeiss and co-authors is interesting in the context of on-going scientific discussion on the issue of stability and sequestration of organic carbon in soils through the addition of C substrates to soils and associated priming effects (e.g. Fontaine et al. 2007; Nature 450, p. 277-281 Kuzakov & Bol 2006; Soil Biol. Biochem. 38, p. 747-758). Indeed, more and more research papers which are able to identify priming-like effects (e.g. through isotope techniques as done by Steinbeiss and co-authors) do observe them for soil pools or fluxes. Such priming like effects occur with a wide variety of C substrates and at various locations in the soil, and not always close to the place where this C is added. Clearly, as this paper suggest adding more C is not necessarily better with respect to increase soil C storage, it also identifies that gains

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at the upper part of the profile only tell part of the story. The ability to isotopically distinguish within these gains and losses of C in the profile between new, plant inputs and native older soil OM is of great help. Such information is helpful to answers more general research questions as posed by Janzen (2007; *Soil Biol. Biochem.* 38, p. 419-424) The soil carbon dilemma: Shall we hoard it or use it? and how to go about doing this either way. It would have been interesting in this paper to have had some information on the microbial biomass and its composition under the various treatments, to see whether or not the influence of the amounts of litter on changes in the soil C pools and priming effects were mediated by purely the microbial community or related to rate limited steps on the abiological processes (Kemmitt et al. 2008; *Soil Biol. Biochem.* 40, p. 61-73).

Despite its timeliness, I think that the paper suffers from some issues which must be rectified for it reach its full impact potential, I think they relate to:

a) Lack of information on the statistical methods employed to derive the significant differences observed in the paper, this important as comparisons are made between treatments with varying numbers of replicates. Also, within the temporal context results at a specific time point might be directly influenced by antecedent soil conditions, which should be corrected for. This should help the reader to feel more certain that the significant differences are statistically real. b) I think that the results of Figure 1 to 3 could be put into one Table and a new Figure should be prepared which show the pools or their changes with depth for the 3 treatments, in relation to the relative losses or gains derived from litter or native OM C. Also how much of each of two pools are primed C. This should help the reader to visualise the changes occurring in the soils with respect 3 treatment (average C3, C4 no litter and C4 double litter). A similar approach could/should be taken with Figure 5, 6 and 7 for the DOC. c) It should be noted that in absolute terms with respect to changes in soil C, one normally expect that the DOC fluxes only represent say 1 to 5% of the total C flux, so are indicative of the soil C dynamics, but cannot account for all the soil C changes which are found with

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depth between the treatments.

I hope that my suggestions are helpful to the authors and they are able to incorporate them.

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