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Biogeosciences Discussions

Interactive comment on "Free and protected soil organic carbon dynamics respond differently to abandonment of mountain grassland" *by* S. Meyer et al.

Anonymous Referee #2

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1. General comments: This study examined the changes in C input to soil and its turnover upon abandonment of managed grassland with a focus on fast-cycling SOM fractions. Detail examination on several SOM pools that cycles relatively quickly (here isolated as wPOM, fPOM, and oPOM) with the use of radiocarbon has rarely been done and thus the results are potentially highly valuable to the scientific community on this subject. The study objectives are clear and important ones. The results seem reasonable but their findings are highly dependent on the validity of the radiocarbon modeling approach used. Unfortunately I am not the expert on this and hope other reviewers can take a detail look. Estimated decomposition rates of SOM fractions are comparable to the ones in the literature, suggesting that their approach was reasonable. The

authors, however, also reported implausible values of C input and decomposition rates for certain SOM fractions (Table 3, 4). It is difficult for me to judge if this is due to inevitable errors associated with "time by space substitution" used in this study, inherent limitation of radiocarbon approach in general, or something else. The manuscript would improve by providing (i) more detail information on site and density separation method and (ii) further discussions on the advantages and limitations of current approach. Some sentences including conclusion seem too speculative given the critical assumptions required for radiocarbon approach. Having this said, this study is unique because it tackles the challenging issue of the changes in SOM processes upon land use change, which is clearly important but difficult to quantify. Related literature was adequately covered. I think overall quality of the manuscript is good. I list specific questions and comments below.

2. Specific comments: Terminology: This manuscript is relatively well-written but I still strongly recommend to clearly define the minimum numbers of SOM quality terms in an early part of the manuscript and use them consistently. As SOM literature is expanding rapidly, it is important to minimize the confusion among all of us. Specifically, I think it becomes more clear by avoiding the terms "unprotected and protected" and stick with "labile and stable" as the authors defined in Result section.

Abstract: Line 9. I think the C input and decomposition rates are not "determined" but "estimated" using models with assumptions. Line 16, "litter quality" trend important in this study? If so, more information should be provided in Results/Discussion section. Line 26. "labile, readily decomposable" – redundant

Methods: "Time by space substitution" (p 9948) is the most critical assumption in terms of affecting result interpretation and conclusion in this study. Thus, the authors should provide more detail description of each land-use plot at each site (for example, location, landscape position, %clay) and discuss the validity of the assumption and/or take more conservative approach when interpreting results and drawing conclusion. These information may be in another paper apparently under review. But, due to its importance, it

should be included in this manuscript. At Matsch site, meadow receives manure every year. Does it accounted for in turnover calculation? I assume the mass and carbon recovery of physical fractionation procedure was reasonable (i.e. the sum of C among the fractions is close to total soil C) and such information may be in another paper submitted. However, the authors should provide at least a brief information on the recovery as it can significantly affect the result interpretation. Sonication treatment: the recovery of oPOM can seriously change with the condition of sonication treatment. Thus the information on setting of output energy used (W), it calibration, type and diameter of horn should be noted.

Discussion: A few sentences are unclear and I sometimes wondered if those are authors' speculation or suggestions based on the data they collected or in the literature. In general, please refer to figures and tables when stating with your own results. Page 9960, line 28-30, is this authors' opinion? Please clarify what "highly structured soil" means here. Soil mixing occurs by soil faunal activity as well as mechanical disturbance. It is still far from clear what factors controlling the transfer of C between unprotected and protected SOM fractions. The last paragraph in Discussion: I agree that the comparison between flux- vs. ground-based (including radiocarbon) estimates of C cycling is highly important issue. Thus I think it would be valuable to expand this section by extending discussion further.

Table 3, 4 and overall: significant digits?

Interactive comment on Biogeosciences Discuss., 8, 9943, 2011.