**Biogeosciences
Discussions**

# *Interactive comment on* "Long-term bare fallow experiments offer new opportunities for the quantification and the study of stable carbon in soil" *by* P. Barré et al.

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We thank the reviewer 2 for reviewing our manuscript and stimulating the discussion. The reviewer's comments are between " " and our answers are in plain text.

"This is generally a well written paper. The approach to use bare fallow plots of long-term field trials to estimate the stable pool is suitable. None-the-less, these sites have some problems, as pointed out below. These problems are not adequately discussed in the manuscript in general, nor do the authors discuss if and how they could affect their results."

We are aware that uncertainties are associated to the data of long-term experiments,

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particularly regarding long-term monitoring. We took it into account in our statistical optimization by considering an a priori large error on the measurements (0.5 to 0.75 gC kg-1, see P4897L13ff), based on data (repeated measurements at Grignon site) and statistical quantities (quality of the fits, residues distribution and chi-2 values). We will discuss more the problems related with the sites and how they affect the results but we think these problems were, at least partly, "mathematically" considered in our study. We also want to emphasize that, in spite of the "problems" inherent to every long-term field experiments, all sites included have been properly monitored during several decades and produce highly valuable and unique data.

"It seems the authors report soil management information as far as it is available. Site history and management history before turning the soils into bare fallow may affect composition and turnover of the slow pool, which for a large part originates from times when there is no information on management. How does this affect the data? This is not discussed."

This is always a problem with SOC modelling and when dealing with "stable" and "intermediate" OC pools. Part of the "stable" OC was formed centuries ago under a vegetation cover, a management and a climate that obviously cannot be known for sure. The recent past history of the sites during which the "intermediate" pool formed is known in most cases. However, from our records and investigation there are unknowns for some sites (i.e the land use of the Versailles plots between the clearing of the forest in the 17th century and the establishment of the prairie, the management of this prairie). This unknown past could affect both the concentration and the chemical nature (dependent on inputs) and interactions with the mineral matrix of the "intermediate" and the stable pool. As well as pedoclimatic factors, this history might affect the abundance and composition of both pools, however, we think that it has no impact on the study presented here since we made no hypothesis regarding the initial state of the pools. Of course it is an interesting scientific question for the future (however to address it operational methods to isolate the pools from soils with known long history will be needed).

"The authors state that atmospheric C inputs "can be considered negligible". For some long-term sites it has been found that there is a relevant input of atmospheric input as dust of industrial emissions. Such inputs cannot generally be neglected and need consideration. Then there is evidence for the presence of larger amounts of coal in the plots of Versailles, and the authors are most probably aware of this problem, but do not mention or discuss it. The presence of coal has major implications on the discussion and relevance of the results, especially as the authors consider that they have reached the stable pool in this experiment."

Apart from the Bad Lauchstädt "static fertilization" (Germany), atmospheric C inputs are generally neglected in long-term agronomical experiments. We do not have any indication that they represent a significant part of SOC in the 6 considered sites. We have also neglected C losses due to the erosion (which can compensate for some atmospheric C deposition) because the plots are in sites where should be nil. We are aware of the presence of coal in the plots of Versailles: it may be due to the initial clearing of the forest between the 17th and 19th century or to anthropogenic deposits. At this stage coal has not been quantified in these plots. Their input is antecedent to the initiation of the bare fallow, as confirmed by the observation of coal particles in archive samples from 1928. The coal in Versailles is likely part of the "stable" pool as it is for every modelling exercise on SOC stocks using pools with first order kinetic on soils containing coal. The optimization routine determined that the "stable" pool has been reached at Versailles. We agree that it would be interesting to determine properly the percentage of coal in the "stable" pool at Versailles: if coal proves to come from domestic deposits, it would allow to estimate by difference the "pedogenic" stable C (of which charcoal due to land clearing belongs to). However, as stated in the abstract (L20-21) and in the discussion, we chemical analysis of long-term bare-fallow samples represent another piece of work, which we also would like to perform in the future.

"Was there a change of depth of cultivation in any of these experiments? This is not mentioned. If there is such a change this would lead to a dilution of the ploughed A

horizon OC content and also have major implications for the results. E.g., for Ultuna the depth of cultivation and depth of sampling is missing."

There were no changes of till depth during the experiment. Sampling depth is 20 cm at Ultuna (Table1) and the tillage depth is also 20 cm (it will be added). Some calculations can be made on the potential effect of "accidental" deeper ploughing. Because the soil just below the plough layer certainly not is C-free (in Askov the soil just below the plough layer contains a C concentration that is about 50

"The authors consider bulk density changes small over time, except for Rothamsted (page 10, line 16ff). But Rothamsted is the only site with detailed bulk density measurements. This needs to be discussed diligently. It cannot be ignored. Is the turnover time affected by changes in soil bulk density over time?"

There were precise bulk density measurements at Ultuna as well, and no bulk density change was observed. We also have bulk density measurements at Versailles and the changes were small compared to Rothamsted (it was not ignored but discussed P4896 L1-15 and P4899 L7-22). At Grignon, no archive bulk density data are available, but we recently compared the bulk density of the long term bare fallow plots and that of an adjacent prairie and the results were not significantly different (R. Cardinael unpublished). We have estimated the turnover times of the "labile" and "intermediate" pools at Rothamsted using C concentration and C stocks, and we did not find significant difference. As bulk density changes was the most important at Rothamsted among the documented sites, we therefore consider that the missing information on bulk density does not affect our results significantly. Moreover, we consider that small changes in bulk density will not significantly influence the environmental conditions of the microbial decomposers, and should not thereby affect turnover times of the C pools.

"SOM pools in different turnover models have different denominations. Although these are all used in the literature, it would be good for this manuscript to clarify the terms. Fig. 3 uses both terms "intermediate" and "slow" pools. Is this the same? It would be

helpful to just use one terminology."

We tried to avoid the denomination used in the most common soil models but we use the three pools classically distinguished in such models: (i) a "labile" pool with the most rapid turnover ( year) which represents easily mineralizable organic compounds; (ii) a pool with an intermediate turnover time of a few decades; (iii) and a pool with a very long turnover time. Concerning the Fig.3, this was changed before the Ms was published in BGD.

"Page 2, lines 25ff : I do not understand this sentence. What do Heimann and Reichstein contend?"

This phrase was not convincing for Sander Bruun either, we will rephrase it.

"Page 3, line 6: This statement is not the case and very general."

Ok, we can replace "poorly understood" by still discussed.

"I also do not agree with the following statement that the stable C pool has not been isolated or characterized experimentally. A similar statement is also given on page 3, line 26 ff. A much more detailed analysis of the state of the art and the relevant literature should be given. See also the following comment on overlooked bare fallow experiments. In chapter 4.3 the authors consider that long-term bare fallow soils have been overlooked (page 16, line 11). Obviously the authors overlooked that long-term bare fallow experiments have been used previously for estimating the composition and turnover of soil organic matter components (Kiem et al., Org.Geochem.31, 655-668; Org.Geochem. 33, 1683-1697; 1699-1713; Kiem and Kogel-Knabner, SBB 35, 101-118."

We agree that there is an important body of literature on studies discussing the stabilization mechanisms of SOM (e.g. van Lützow et al., 2008, J. Plant. Nutr. Soil Sci., 171, 111-124) and that some of these studies were partially conducted using bare-fallow soils (e.g. Balabane Plante, 2004 EJSS, 55, 415–427 ; Plante et al., 2005,

Geoderma, 129, 186-199). These kind of studies were used to link qualitatively these stabilization mechanisms to model pools (e.g. Ludwig et al., 2008, J. Plant. Nutr. Soil Sci., 171, 83-90). However, to our knowledge, no chemical or physico-chemical extractions have successfully allowed isolating the "stable" pool of models and consequently it has not been clearly identified yet. In other words, "model" the "measurable" has not worked and as the "stable" pool of models has been isolated from the other SOC pools at Versailles; we can try to "measure" the "modelable". From this point of view, the two relevant studies by Kiem, which we know, are conceptually very different from ours. Nonetheless, we contend that the term "overlooked" is too strong but we clearly feel that the LTBF were underused.

"The equivalent soil mass approach was first used by Ellert and Bettany (1995) and proper reference to this paper should be given."

The concept of equivalent soil depth sampling (which is the same as equivalent soil mass) was originally introduced by David Jenkinson in Jenkinson DS (1971) The Accumulation of Organic Matter in Soil Left Uncultivated. Rothamsted Experimental Station Report for 1970, Part 2, 113-137. Lawes Agricultural Trust, Harpenden, UK. The idea has also been used by Powlson and Jenkinson (1981; J. Agric. Sci. Cambr. 97, 713-721; see page 716) and many others since. We will add this reference.

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