

## ***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.***

**Anonymous Referee #2**

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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.

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

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not discussed.

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.

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.

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?

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.

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

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

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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.

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

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