

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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The paper attempts to estimate the size of the stable pool of soil organic carbon based on long-term bare fallow (LTBF) experiments from difference places around Europe. The subject is relevant for the journal and interesting in several aspects of biogeosciences and in general the applied methods are appropriate and the study is novel.

I very much appreciate the following aspects of the study.

I appreciate the effort to use data from the LTBF experiments, and I agree with the authors that, they are a unique resource that can be used to look at stable C this from a new perspective.

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I also appreciate very much the application of Bayesian statistics to estimate the uncertainty of the estimates of the stable carbon. This is of uttermost importance because it is very easy to estimate a stable pool and draw some conclusions that are in fact not supported by the data. Therefore, I find the uncertainty estimates almost more interesting than the absolute values.

There are however a few things that I miss.

I miss a discussion of the importance of the choice of model and the length of the experiments for the estimates of the stable pool of SOC. The general comparative model that you use is a mono-exponential + constant (for most sites) where the constant corresponds to the stable pool. This actually forms the basis for your definition of the stable pool. The stable pool corresponds to the fraction estimated with this model and the experiments that we have. However you might also have chosen something completely different for example a 3 pool model with fixed turnover times of 10, 100 and 1000 years and then the 1000 year pool would be the stable pool. Would that have changed the conclusions? Regarding the length of the experiment I would expect that with a mono-exponential + constant model, longer experiments would have led to not only more precise estimates of the stable fraction, but also smaller values. In deed if we had a 1000 year experiment almost no C would be left and you would also estimate to stable pool which was very low. With the 3 pool model I have suggested this would be different.

I also miss a better presentation of the application of the Bayesian methods. I believe that Bayesian statistics if not common knowledge to the average reader of this paper (including myself) and therefore you need to describe that in a little more in detail. What is the purpose of it and what do you gain compared with other methods for estimating parameters and their uncertainty. What is a priori information. Eq. (1) is of no use to me unless it is explained in a little more detail.

Specific comments

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Title. I am not sure that the title is really appropriate. The point of the paper is not to show that the experiments offer new opportunities. You are actually trying to learn something about stable carbon in this paper.

p. 4890 l. 4-5. I believe that the references Davisson and Janssens (2006) and Jones et al. (2005) are used a little out of context. Why do you need a reference to prove what Heimann and Reichstein contend?

p. 4891 l. 16. Do we ever reach the stable fraction? I believe it is more a matter of how far away from it we are.

p. 4895 l. 21. You do not mean to say that at Kursk and Askov no bulk density changes were assumed. You mean that bulk densities were assumed not to change.

p. 4896 l. 20. "\$\dots\$ and a, b, c, d and e are parameters \$\dots\$"

p. 4897 l. 24. What do you mean "we converged".

p. 4898 l. 22. What do you mean "wetter".

p. 4901 l. 4-5. Yes, but remember that a pool of organic matter with a half life of the same magnitude as the experiment will only be half gone by that time.

p. 4901 l. 21. I am not sure I like the word consensual in this context

p. 4902 l. 15-16. Be more specific. What do you mean by having to wait for a while. Maybe it is better to say that you need a longer experiment.

p. 4902 l. 15-16. At this stage of what?

p. 4902 l. 15-16. I am a little puzzled about the fact that the upper boundary of the estimate of the stable pool is in fact higher than all the observations at the Kursk site. It must be possible to conclude that the stable pool is smaller than the amount of C on the final observation in the experiment.

Table 2 and 3. I am surprised that in Table 2 for the mono-exponential model + constant

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and Table 3 they have different AIC values. I am also surprised that the constants are not significantly different from 0 for some of the sites in table 2, but have a 95% confidence intervals not including 0 in table 3?

p. 4903 l. 21-23. I am not so sure that I believe that it supports the 3 pool model so much, only you are not able to falsify it with the current data. The other 3 pool models have completely different structures and you would most likely be unable to falsify them with the data if all the parameters are free. In the letter Bruun and Luxhøj (2006) we discuss this problem.

Fig. 3. I do not think that this figure is very illustrative. If there are any correlations between turnover time and the variables it would be very difficult to spot. The best thing to do would be to do a statistical analysis of the effect of temperature, humidity and sand, but of course you do not have enough data for that.

p. 4904 l. 16. Change "that" to "than"

p. 4904 l. 18-23. Again I am not so sure that the comparison with models with completely different structure is so meaningful.

p. 4904 l. 24. Relationships

p. 4907, conclusions: The only thing you seem to be able to conclude from the study is that the LTBFs are valuable. What did you learn about the size of the stable pool?

References

Bruun, S., Luxhøj, J.: Letter to the editor on "Can incubations be used to measure meaningful pools of soil organic matter?", Soil Sci. Soc. Am. J., 70, 2164, 2006.

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