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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.

P. Barré et al.

barre@geologie.ens.fr

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We thank Sander Bruun for his valuable comments to improve our manuscript. Sander Bruun's comments are between " " and our answers are in plain text.

"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. 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 then 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 stabile 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."

We tried to extract most information from the data, with as little a priori information as possible, a part from using first order decays or linear decay. It appeared that different models provided similar best fits but that a general comparative model (mono-exponential + constant or short-lived exponential + mono-exponential + constant for Rothamsted) can be used. The constant indeed forms the basis of our definition of the stable pool but we specified in the introduction: "In soil C models such compounds are represented by a C pool with a high turnover time (several centuries) or even by an inert C pool (Falloon and Smith, 2000). However, in many respects, this stable soil car-

bon remains terra incognita" (P4890L12-15) and in the results "truly inert or so slowly decomposing that it appears inert at the time scale of the experiment" (P4902L3). Consequently, we do not exclude that our so-called "stable" pool has in fact a c. 1000-year turnover time but that we are unable to pick this decay with "only" 80 years of experiment duration. We would likely have observed an exponential decay for the "stable" pool with a 1000-year experiment. However, it would not necessary change our conclusions. Indeed, in 920 years at Versailles we may have a bi-exponential model with the same intermediate pool as in the 80-year experiment and a "stable" pool with the same concentration as in the 80-year experiment but with a 1000-year turnover time instead of an infinite turnover time as it is at the moment. Indeed, we could have used 3 pool models with fixed turnover time but the choice of the turnover time values would have been arbitrary. Due to the duration of the experiment it is likely that we would have had a big uncertainties on the concentration of each pool if we would have used three pools with turnover times of 10, 100 and 1000 years. The stable pool concentration inferred by the plateau would have likely been in the 95% confidence interval of the concentration of the pool with a 1000-year turnover. However, we think that it would be highly interesting to model this dataset with the existing soil models to see how far we are from the stable pools of the different models. We plan to perform this in the future. The architecture and the optimized turnover time of the intermediate pool are close to these of Century or Orchidee so we expect to have reached the Orchidee "passive" pool at Versailles. We would also be very interested to see what a model based on the continuum concept can learn us on this dataset and compare its outputs with first order kinetics models.

"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."

Bayesian statistics relies on the Bayes theorem of conditional probabilities. Bayesian statistics are widely used in data assimilation communities (i.e. in geophysic for instance) where uncertain quantities (model parameters in our case) are optimized using uncertain results of an experiment (measurements) and a model linking the two spaces. In our case, this process allows to include a priori information on the parameters that are sought for. Such formalism is general and may include the more classical parameter estimation process. We can indeed set very large errors on the parameters so that the optimization is reduced to a classical least square minimization. We will improve the description of the method in a revised version of the manuscript to describe the Bayesian term of Eq. 1.

"Specific comments 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."

OK, we have to think about a new title such as "Quantifying and isolating stable OC through long-term bare fallow experiments".

"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 to you need a reference to prove what Heimann and Reichstein contend?"

OK.

"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."

Long term bare fallow will for sure always contain some labile C (the mineralisation of the stable C, though very slow, should sustain some microorganisms which contribute to a labile pool). However this pool should be very small and thus we expect that total C can equate with stable C. At the moment, the "stable" pool represents between 73 and 100% of soil C at Versailles.

“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.”

OK.

“p. 4897 l. 24. What to you mean “we converged”.”

The algorithm converged not us!

“p. 4898 l. 22. What do you mean “wetter”.”

We mean that it has a higher Precipitation over Evaporation ratio.

“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.”

OK.

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

We will find another writing.

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

OK.

“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”

The data at Kursk do not exclude that we have only a plateau. The decrease rate of SOC through time strongly depends on the last measurement point, which leads to large error estimates on the model parameters and consequently on the stable SOC. The upper boundary of the estimate corresponds to the highest observation plus the error on the measurements. In this case we thus obtain a relatively large value (large

errors) that prevents for any strong conclusion about the stable C pool concentration. However we checked that with smaller measurement errors we would obtain much smaller upper boundaries.

“Table 2 and 3. I am surprised that in Table 2 for the mono-exponential model + constant 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 95n% confidence intervals not including 0 in table 3?”

The models have slightly changed: on Table 3, the values correspond to a model in which the plateau values were forced to be positive using a lognormal transformation. (see P4898L6-10 and P4901L26-27)

“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 parameter are free. In the letter Bruun and Luxh{no}i (2006) we discuss this problem.”

Thanks for this interesting reference. Ok, the 3 pool model was not falsified.

“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.”

We agree but still prefer to maintain the figure.

“p. 4904 l. 16. Change “that” to “than””

OK.

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

Century and Roth-C models have a structure close to the one we used: most of soil C is in a pool with intermediate turnover times (∼several decades) and in a "stable" (∼1000 years) pool in these two models. We agree that it would be interesting to have real Century and Roth-C (and other models) simulations on this dataset.

"p. 4904 l. 24. Relationships"

OK.

"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?"

OK. We can strengthen this part. We think that the main information regarding the stable C pool concerns its size: it is large and it represents an important fraction of the total C of soils under vegetation.

Interactive comment on Biogeosciences Discuss., 7, 4887, 2010.