

Biogeosciences Discuss., author comment AC1  
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## Reply on RC1

Elisa Bruni et al.

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Author comment on "Additional carbon inputs to reach a 4 per 1000 objective in Europe: feasibility and projected impacts of climate change based on Century simulations of long-term arable experiments" by Elisa Bruni et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-489-AC1>, 2021

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**We thank the reviewer for the positive comments. We addressed the issues raised by modifying some parts of the manuscript. Please find the details below.**

Detailed comments

In the next paragraph, I developed these points and made some comments that will help the authors to improve the manuscript.

L.33

Overall, I think that the abstract should be improved. It seems to me that it did not reflect the good quality of the manuscript. The authors should clearly highlight the objectives of the study.

**We rephrased some sentences in the abstract, to better clarify the objectives of the study.**

- **33-36 were rephrased as follows:**

**"In this study, we assessed the amount of organic C inputs that are necessary to reach a target of SOC stocks increase by 4‰ per year on average, for 30 years, in 14 long-term agricultural sites in Europe. We used the Century model to simulate SOC stocks and assessed the required level of additional C inputs to reach the 4 per 1000 target in these sites. Then, we analyzed how this would change under future scenarios of temperature increase."**

- **46-51 were rephrased as follows:**

**"In the experimental sites, we found that SOC stocks in treatments with additional C inputs were increasing by 0.25% on average. This means that the C inputs required to reach the 4 per 1000 target might actually be much higher. Furthermore, we estimated that annual C inputs will have to increase even more due to climate warming, that is 54% more and 120% more, for a 1°C and 5°C warming, respectively. We showed that modeled C inputs required to reach the target depended linearly on the initial SOC stocks, raising concern on the**

**feasibility of the 4 per 1000 target in soils with a higher potential contribution on C sequestration, that is soils with high SOC stocks. Our work highlights the challenge of increasing SOC stocks at large scale and in a future with warmer climate.”**

L.35-36

After reading the title and the first sentences of the abstract, it is not clear if the required level of carbon inputs is assessed only for the long-term agricultural experiments or for other soils? After reading the entire manuscript, we understand that the model simulation concerns only the long-term experiments. But it should be better underlined in the abstract.

**We rephrased two sentences in the abstract, to better specify that the objective of our modeling exercise is to estimate the carbon inputs in the 14 long-term experiments, as follows:**

**(L.33-36)**

**“In this study, we assessed the amount of organic C inputs that are necessary to reach a target of SOC stocks increase by 4‰ per year on average, for 30 years, in 14 long-term agricultural sites in Europe. We used the Century model to simulate SOC stocks and assessed the required level of C inputs increase to reach the 4 per 1000 target in these sites.”**

**In the introduction, we also rephrased the part stating the objectives of our work, to take into account this comment:**

**“Our work was set up in this context with the objectives to: 1) estimate the amount of C inputs needed to increase SOC stocks by 4‰ per year; 2) investigate if this amount is attainable with currently implemented soil practices (i.e. organic amendments and different crop rotations) and 3) study how the required C inputs are going to evolve in a future driven by climate change. We used the biogeochemistry SOC model Century, which is one of the most widely used and validated models (Smith et al., 1997) to simulate SOC stocks in 14 different agricultural LTEs around Europe. We set the target of SOC stocks increase to 4‰ per year for 30 years, relative to the initial stocks in the reference treatments. With an inverse modeling approach, we estimated the amount of additional C inputs required to reach a 4p1000 target at these sites. Finally, we evaluated the dependency of the required additional C inputs to different scenarios of increased temperature.”**

L.39

The objective of determining the impact of temperature on the simulated additional carbon inputs to reach the 4 per 1000 objective should be highlighted in the objectives.

**We moved L.39 (“Then, we analyzed how this would change under future scenarios of temperature increase.”) to L.37, when stating the objectives of the model.**

L.42-43

In this sentence, I think that it is not clear to understand if the additional C inputs are simulated from the current soil C stocks (which implies that there were C inputs in the previous years) or from the initial soil C stocks at the beginning of long-term experiments.

**We added to the sentence that the additional carbon inputs increase is calculated with respect to the initial carbon inputs in the control treatment:**

**“We found that, on average among the selected experimental sites, annual C inputs will have to increase by  $43.15 \pm 5.05$  %, which is  $0.66 \pm 0.23$  MgC ha<sup>-1</sup> per year (mean  $\pm$  standard error), with respect to the initial C inputs in the control treatment.”**

L.51

I would suggest to add a few sentences to conclude on the feasibility of reaching the 4 per 1000 objective. At the end of this abstract, the reader can wonder what are the conclusions and perspectives of this study.

**To express our concern on the feasibility of the 4 per 1000 highlighted by our work, we added the following sentence to L.50: “raising concern on the feasibility of the 4 per 1000 target in soils with a higher potential contribution on C sequestration, that is soils with high SOC stocks.”**

**We also added the following sentence to the end of the abstract: “Our work highlights the challenge of increasing SOC stocks at large scale and in a future with warmer climate.”**

L.87

See also Powlson et al. (2011) for the definition of soil C sequestration which implies a net removal of CO<sub>2</sub> from the atmosphere.

Powlson, D.S., Whitmore, A.P., and Goulding, K.W.T. 2011. Soil carbon sequestration to mitigate climate change: a critical re-examination to identify the true and the false. European Journal of Soil Science 62:42-55.

**We added the cited reference.**

L.106

The authors should precise the reason of the initialization of the models. Is it because the initial SOC stocks are not available?

**We rephrased L.106-107 to explain that model initialization is required either for lack of data or to allocate carbon in the different model compartments, which cannot be measured, as follows:**

**“This means that the initial status of SOC has to be set, either for lack of data on total initial stocks, or to determine the allocation of C among model’s compartments that cannot be measured. This is commonly accomplished by assuming that SOC is at equilibrium at the beginning of the experiment (Luo et al., 2017; Xia et al., 2012).”**

L.119

One sentence to explain the choice of this model?

**We added a sentence saying that Century is one of the most widely used and validated models and cited the following paper to support our statement:**

Smith, P., J.U. Smith, D.S. Powlson, W.B. McGill, J.R.M. Arah, O.G. Chertov, K. Coleman, et al. "A Comparison of the Performance of Nine Soil Organic Matter Models Using Datasets from Seven Long-Term Experiments." *Geoderma* 81, no. 1–2 (December 1997): 153–225.  
[https://doi.org/10.1016/S0016-7061\(97\)00087-6](https://doi.org/10.1016/S0016-7061(97)00087-6).

Moreover, we added the following sentence to the beginning of subsection 2.2.1:

**"For this study, we selected the Century model, which has proved to be well suited to simulate accurately the soil dynamics in a range of pedoclimatic areas and cropping systems (Bortolon et al., 2011; Cong et al., 2014; Parton et al., 1993), and because we had the full command of the model for fine tuning of parameters."**

L.122

I think that it is important to remind that the simulations concern the long-term experiments and not other soils.

**We added: "at these sites" and rephrased two sentences to take into account other comments:**

**"We set the target of SOC stocks increase to 4‰ per year for 30 years, relative to the initial stocks in the reference treatments. With an inverse modeling approach, we estimated the amount of additional C inputs required to reach a 4p1000 target in these sites."**

L.127

There is one control plot for each long-term experiment but there are 14 control plots in total. So the sentence should be rephrased.

**We rephrased the sentence as follows:**

**"We compiled data from 14 LTEs in arable cropping systems across Europe (Fig. 1), where a total of 46 treatments with increased C inputs to the soil were performed and one control plot in each experiment was implemented."**

L.153-157

We need to have more details:

1/In Table 1, we have the carbon inputs for the crop rotations (so we can assess additional plant carbon inputs in comparison to the reference ). Are these additional plant carbon inputs included in the column of additional carbon inputs? If yes, that means that additional carbon inputs include both plant and manure inputs, right?

**Additional carbon inputs in the 6<sup>th</sup> column of the table represent carbon inputs from the organic treatments only. Additional carbon inputs from different yields compared to the reference are included in column 4 ("Carbon inputs from crop rotations"). We reformulated the 6<sup>th</sup> column's title and the table description to make it clearer, as follows: "Additional carbon inputs from organic treatment"**

**The only site where additional C inputs from different yields is equal to the additional organic treatment is Foggia. We clarified this by adding the following note: "\*\*\* In Foggia, additional carbon inputs from organic treatments were calculated for each rotation as the difference between C inputs in the rotation**

**and the reference wheat-only rotation.”**

2/ In the text, how do the authors appreciate if the 4 per 1000 objective is attainable? Do they assess the difference of SOC annual variation between treatments and reference?

**We consider that the 4 per 1000 objective is attained when the treatment reaches a 4‰ increase of SOC stocks, relative to the initial SOC stocks in the reference treatment. This is specified in L.327-331. However, we rephrase the following sentence in L.130-131 to clear it out earlier in the text:**

**“We set the target of SOC stocks increase to 4‰ per year for 30 years, relative to the initial stocks in the reference treatments.”**

3/ Finally, I wonder if this section should not be part of the Results section? L.173-176

In Table 2, the presented initial SOC stocks seem to be measured (from the title). So I wonder why the initialization of the model was done by simulating initial SOC stocks. Why not using measured initial SOC stocks?

**Initial SOC stocks were measured. However, we needed to initialize the Century model to allocate SOC stocks at time 0 in the different model compartments. A relaxation approach to initialize the model such as in Dimassi et al. (2017) could not be used, due to the requirement of an equilibrium state to perform the optimization algorithm to calculate C inputs to reach a 4 per 1000 target.**

**For this reason, we consider that Table 2 is part of the material used, since it is only a description of the sites.**

L.79-181

I just wonder why details of sampling are described for this site but not for the other sites...

**We described Broadbalk separately because it did not have any replicates, like Foggia and Champ Noel. But unlike these sites, Broadbalk soil was sampled with a protocol allowing a better estimation of SOC concentration. We rephrase this sentence to make it clearer:**

**“SOC stocks were measured in 3 – 4 replicates, apart from Foggia and Champ Noël 3 experiments, where no replicates were available, and Broadbalk. In this experiment, SOC was measured in each plot using a semi-cylindrical auger where 10-20 cores were taken from across the plot and bulked together (more details can be found on the e-RA website<sup>1</sup>).**

L.194

As I said earlier, I think that the authors should add a few sentences somewhere to explain the choice of this model. Why this model instead of another one (ROTH C, DNDC...)?

**This was added in the introduction (“We used the biogeochemistry SOC model Century, which is one of the most widely used and validated models (Smith et al., 1997”).**

**Also, at the beginning of subsection 2.2.1, we added:**

**“For this study, we selected the Century model, which has proved to be well suited to simulate accurately the soil C dynamics in a range of pedoclimatic areas and cropping systems (Bortolon et al., 2011; Cong et al., 2014; Parton et al., 1993), and because we had the full command of the model for fine tuning of parameters. ”**

L.235

The following points need to be clarified:

1/Do initial sizes correspond to initial SOC stocks at the beginning of the experiment or when SOC were measured after a certain number of years where we supposed an equilibrium?

**Initial sizes correspond to initial SOC stocks at the beginning of the experiment. We reformulate L.235 as follows to make it clear: “The initialization of the model consists in specifying the sizes of the SOC pools at the beginning of the experiment.”**

2/If initial sizes of SOC refer to initial SOC stocks at the beginning of the long-term experiments, why not using measured initial SOC stocks?

**As explained earlier, although total SOC stocks are known, we needed to initialize the Century model to allocate SOC stocks at time 0 in the different model compartments. Since the Century compartments are conceptual their fraction can't be derived from observations.**

L.252

I am not sure that this paragraph is at the proper place. I explain myself: the authors introduced the Century model in the previous paragraph and they go on with the Century model calibration in the following paragraph. In this paragraph, if I understand well, the C inputs are estimated by using allocation coefficients for each of the treatments of the long- term experiments, there are no direct use of the Century model. That's why I suggest to move this section.

**We moved this subsection to subsection 2.1.3, before the description of the Century model.**

L.366

As the calibration was partly done by using data from control, is it not normal to expect a good fit of modelled values to control SOC values, no? Why not checking the fit of calibrated model to the SOC values of the other treatments of the long-term experiments?

**Before choosing Q10 and reference temperature as the best parameters' calibration option, we tested different other parameters (i.e. the five pools decomposition parameters, Q10 only, reference temperature only etc.). In these calibration tests, model fit to observed SOC was not improved. This is why we decided to show how the parameters' optimization worked in the control plots. However, since a validation of SOC increase in the rest of the treatments is missing, we add Fig. 5 to paragraph 3.1 (see Fig\_5.pdf in supplement). This figure shows how the virtual C inputs simulated by Century to reach the 4‰ target reproduce the correlation between additional C inputs and SOC stocks increase in the C inputs treatments.**

**As shown in Fig. 5, although the correlation of C inputs with SOC stocks increase**

is not clear-cut ( $R^2=0.23$ ), with a large variability of the effect of C inputs on SOC stocks increase, we state that Century is generally overestimating the effect of additional C inputs on SOC stocks increase. We discuss this result stating that the hypothesis of equilibrium at the beginning of the simulation might be one of the major sources of this error.

**[Figure 5: Correlation between additional carbon inputs ( $\text{MgC ha}^{-1}$  per year) and annual SOC stock increase (%) in the carbon inputs treatments and mean standard deviation of the additional carbon inputs to reach the 0.4% target in Century.]**

L.410-412

But did the authors test the correlation between the optimal input increase and  $Q_{10}$  or decomposition rates?

**We tested it, but there was no significant correlation between optimal input increase and  $Q_{10}$  (see Fig\_RC1.pdf in supplement)**

**[Fig.RC1: Correlation between total C inputs to reach the 4p1000 target ( $\text{MgC ha}^{-1}$  per year) and optimized  $Q_{10}$  parameter values]**

L.480-481

Where is this result (0.25% increase per year) presented? Also, I suggest the authors better introduce the additional C treatments (which are actually the real treatments in the long-term experiments) and the virtual treatments.

**The result is presented at L.438, now slightly reformulated ("In the experimental treatments were applied  $1.52 \text{ MgC ha}^{-1}$  per year on average and SOC stocks were found to be increasing by 0.25% per year relative to initial stocks.")**

L.494

Just one comment: by composting the organic amendments that will be spread on soil surface, there will be some C emissions during the composting process so it will be necessary to make a full assessment of C cycle with and without composting to be sure that the composted C input result in net C sequestration.

**In this study we focused only on the soil C inputs, but we agree that a full assessment of the C cycle would be needed in a more exhaustive analysis (cf. following comment for line 573).**

L.496

Another comment: in the case of animal manure, if farmers produce more manure, it implies more animals and larger C emissions through animals. Consequently, even if more manure is returning to soil, it will not result in net C sequestration.

**We add these two comments to the end of subsection 4.2.1, with the following sentences:**

**"Moreover, producing additional animal manure implies larger GHG emissions through animal digestion and manure decomposition. Consequently, even if more manure is returned to the soil, it will not necessarily result in climate change mitigation."**

L.573-575

Are possible emissions through the different managements taken account?

**No, they are not. So we add a sentence to L. 573 to make it clear:**

**“However, a full C cycle assessment should be considered to make sure that GHG emissions associated to such treatments do not exceed additional C storage (Guenet et al., 2020).”**

L.589-591

I am not sure to understand that point. Do we really need that all soils increase their SOC stocks by 4 per 1000? Some soils could be increased by more than 4 per 1000 and if this counterbalances for other soils which cannot be increased by 4 per 1000, overall the objective should be attainable, no?

**We rephrase these sentences to make our point clearer:**

**“A counterpoint is also that the largest contribution of C sequestration will come from soils with medium or high SOC stocks (i.e. higher than 50 MgC ha<sup>-1</sup>, such as grasslands and forests). In these soils, the required additional C inputs will have to be higher according to Century, raising concern on a compensation of CO<sub>2</sub> emissions through improved SOC stocks at a global scale”**

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-489/bg-2020-489-AC1-supplement.zip>