

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2020-489-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2020-489

Anonymous Referee #1

Referee 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-RC1>, 2021

General comments

Overall, the manuscript entitled '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' will be probably of great interest for the readers of Biogeosciences Journal. Indeed, it provides interesting results on additional carbon inputs that would be necessary to reach the 4 per 1000 objective in long-term arable experiments from Europe. Also, it also highlights the impact of temperature increase on these additional carbon inputs. Overall the manuscript is well written, the paragraphs are well organized and the ideas are well supported by relevant references. It was also appreciated that most of the limits of the approach have been underlined. That's why I think that this manuscript should be considered for publication but I have noticed some points that need to be addressed before publication.

Detailed comments

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

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.

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.

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.

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.

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.

L.87

See also Powlson et al. (2011) for the definition of soil C sequestration which implies a net removal of CO₂ 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.

L.106

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

L.119

One sentence to explain the choice of this model?

L.122

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

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.

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?

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?

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?

L.79-181

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

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...)?

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?

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?

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.

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?

L.410-412

But did the authors test the correlation between the optimal input increase and Q10 or decomposition rates?

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.

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.

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.

L.573-575

Are possible emissions through the different managements taken account?

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?