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Reply on RC3

Elisa Bruni et al.

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.,
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We thank the reviewer for the positive comment. We added two figures to the manuscript (see Fig_5.pdf and Fig_C2.pdf in supplement) , to better show the validation of model results. Also, we added some explanations and comments to better contextualize model projections.

Major concerns

I assume these papers follow a rather prescriptive formulation that involves model calibration, validation, and implementation / implication sections.

The model calibration is presented (Table 3, Fig 4), where the authors calibrate the litter quality, Q10 and reference temperature for control plots at the 14 sites. The findings raise some concerns, described below, but the discussion generally handles this well.

I would assume that model validation would involve projecting trends in soil C stocks from the modified C input experiments. I appreciate that the observations of trends from these experimental treatments are highly variable (line 155 and Table 1). But there is seemingly no effort to validate the ability of the model to capture the correct magnitude of soil C change (relative to controls) in the additional carbon experiments that are listed. Revisions to the manuscript should more clearly illustrate how well the calibrated CENTURY model captures observed soil C changes across these experimental plots in response to experimental manipulations.

We added Figure 5 to show Century predictions of additional C inputs to reach the 4p1000 target, together with additional C inputs experiments and their relative SOC stock variation. We show that Century actually overestimates the effect of additional C inputs on SOC stocks. However, treatments effect on SOC are highly variable.

We add the following sentence to subsection 3.1:

"We tested the capability of Century to reproduce SOC stocks increase in the additional C input treatments (Fig. 5). Figure 5 shows the correlation between additional C inputs and SOC stock increase in the C input treatments ($R^2 = 0.23$). In the same graph, we can appreciate additional C inputs simulated by Century

to reach the 4p1000 target being $0.66 \pm 0.23 \text{ MgC ha}^{-1}$ per year (mean \pm standard deviation from the mean). This shows that Century is generally overestimating the effect of additional C inputs on SOC stocks increase. However, the effect of additional C inputs on observed SOC stock increase varies largely across different treatments.”

We suggest that this overestimation might be due to the hypothesis of equilibrium and we add a discussion to subsection 4.1:

“The capability of Century to simulate SOC stocks in the simulations of additional C treatments might be a major shortcoming of modeling results. In fact, although SOC stocks were found to be increasing on average in the additional C treatments (0.25% per year with 1.52 MgC ha^{-1} yearly additional C inputs), this increase rate is lower than the 0.4% increase of SOC stocks predicted by Century with lower amounts of virtual C inputs (0.66 MgC ha^{-1} per year). This is pointed out in Fig. 5, where we can see that predicted additional C inputs to reach the 4‰ are lower than the correlation line between additional C inputs and SOC stocks increase in field treatments. The overestimation of the C input effect on SOC stocks in Century might be related to the assumption that SOC stocks are in equilibrium with C inputs at the onset of the experiment and on the high sensitivity of the model to C inputs.”

[Figure 5: Correlation between additional carbon inputs (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.]

Model implications are well documented and described.

I don't completely understand the details of the model calibration and optimization that were taken, but it's surprising to me that the metabolic:structural partitioning varies to such a great extent among experimental sites (Table 3). Notably, the aboveground M:S calibration largely flips between the upper and lower limit of the range allowed by the model (85:15 or 15:85). Does this seem realistic? I would assume there are general estimates for lignin:N ratios of different plant parts for different crops that could be used to help constrain this parameter?

In Century, the standard function to calculate the metabolic part of the litter (LF_m) is the following:

$$LF_m = \max(0.85 - 0.018 * L:N)$$

which ranges between 0.15 and 0.85 and where L:N is the lignin to nitrogen fraction of the crop. The structural fraction (LF_f) is then calculated as:

$$LF_f = 1 - LF_m$$

We researched L:N values in the literature and we found that:

- For winter wheat L:N = 42 (Adair et al., 2008) -> = 0.15
- For pea L:N = 1.149 (Ghimire et al., 2017) -> 0.83
- For winter rapeseed L:N = 0.625 (Ghimire et al., 2017) -> 0.84
- For oats L:N = 1.12 (Ghimire et al., 2017) -> 0.83

These values are in line with those found by Century's optimization of the metabolic:structural ratio. We were not able to find L:N values for all crops, nor

for different parts of the plant. For these reasons, we considered that model parameters' optimization was the best option for our simulations. Limits of this approach are discussed in subsection 4.1.

Similarly, why is there such a large range in the Q10 values for each site (range 2-5!). I understand this was done to help calibrate the model, but I wonder if the propensity for the calibration to settle on the extreme values for parameter ranges for litter quality and temperature sensitivity points to either: (a) structural deficiencies in the model or (b) equifinality issues from trying to fit a complicated model with sparse data. This is briefly discussed at the beginning of the discussion, but I wonder if it's a finding that should be noted in the abstract & conclusion as well?

Many authors found that Q10 values were highly variable across different pedo-climatic conditions (Craine et al., 2010; Lefèvre et al., 2014; Meyer et al., 2018; Wang et al., 2010). We decided to bind the model with the Q10 range found by Wang et al. (2010) in temperate regions. We agree that the optimization tends to settle the parameters on extreme values. It is difficult to conclude whether it is a structural limitation of the model or an equifinality issue. We rather suppose it is a combination of both.

We added figure C2 to appendix C, to show that the optimization does not affect model outputs for current values of temperature in our modeling exercise. In regard to this, we do not find it necessary to discuss optimization issues in the abstract or conclusion. However, we add some sentences in the discussion to clear out this point.

In subsection 4.1, we added:

"Figure C2 shows that the optimization of temperature related parameters did not affect significantly the required C inputs estimation, for the current temperature scenario. This means that, although parameters optimization improved the simulation of SOC stocks in the control plots, the final results are not affected by it."

[Figure C2: Effect of the optimization of the Q_{10} and reference temperature (T_{ref}) parameters on the additional carbon inputs to reach the 4p1000 predicted by Century (mean standard deviation).]

To this point, the last sentence of section 4.1 is alarming, and suggest that CENTURY projects 2x the amount of SOC accumulation with 0.5x of the additional inputs, compared to observations. This raises two issues:

Lack of validation for the additional carbon experiments.

If agricultural management practices or negative emissions strategies are supposed to be informed by studies this like (as implied in the introduction) it seems like the model projections may be overly optimistic by a factor of four! This raises some serious concerns that are somewhat glossed over by reading the abstract (and conclusion).

As explained above, we added two figures to better show the capability of Century to reproduce SOC stocks in the additional C input treatments and discussed these issues in the results and discussion.

We also changed the last sentences of the abstract to stress out this point:

"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.”

Minor and technical concerns:

Line 48, maybe this sentence can be clarified to address some of the concerns above?

We added the following sentence after L.48:

“This means that the C inputs required to reach the 4p1000 target might actually be much higher.”

Line 77: I don't know what it means to 'promote a virtuous C cycle' and suggest this phrase be removed

We removed 'and eventually promote a virtuous C cycle'

Section 2.2 I understand that CENTURY also has functions where soil pH determines turnover times and soil texture (specifically sand content) modifies the partitioning of C fluxes between pools (and to CO₂). It doesn't look these were used here, which is fine, but it should be clarified in the text.

In this study, we used the original version of the Century model (Parton et al., 1987). To our understanding, Century in its original version does not have any function including soil pH for the C module (Foereid et al., 2007). Soil texture does affect the decomposition rates, but only as a function of clay, which we took into account (see Appendix A). However, a function of sand was not present in this version (see Fig. 1 in Parton et al. (1987)).

Figure 4 is fine, but I guess I expected to see some kind of predicted vs. observed soil C stock plot as part of the model calibration?

We added a figure of the fit between predicted and observed SOC stocks in the control plots to Figure 4.c (Fig_4.pdf in supplement)

And added the following sentence to the results:

“The correlation coefficient between modelled and observed SOC stocks in the control plots was 0.96 (Fig. 4.c).”

[Figure 4: a) Decomposed mean squared deviation (MgC ha⁻¹)² in control plots for all sites. LC = Lack of Correlation, NU = Non-Unity slope and SB = Squared Bias. b) Normalized root squared deviation (%) in control plots for all sites c) Fit of predicted versus observed SOC stocks (MgC ha⁻¹) in control plots for all sites (R² = 0.96).]

The word 'virtual' is used heavily throughout the text and especially in the results and discussion. We know that the simulated results are from a model. As such, I wonder the use of 'virtual' is potentially redundant (I e.g. "virtual simulations"; "virtual C inputs") and can be removed?

We removed the term “virtual” throughout the text

Line 564 should there be a +/- symbol here?

There is a typo, it should be 4558×10^3 MgDM. We converted this value to TgDM, i.e. 0.4558 TgDM to make it consistent with other values in the text.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-489/bg-2020-489-AC3-supplement.zip>