Response to Philippe C. Baveye

The vast majority of the review is a polemic about soil carbon sequestration and the 4 per 1000 concept. That discussion was held earlier and readers can look at that. Here are the relevant publications:

- Minasny, B., Arrouays, D., McBratney, A.B., Angers, D.A., Chambers, A., Chaplot, V., Chen, Z.S., Cheng, K., Das, B.S., Field, D.J. and Gimona, A., 2018. Rejoinder to Comments on Minasny et al., 2017 Soil carbon 4 per mille Geoderma 292, 59–86. Geoderma, 309, pp.124-129.
- Rumpel, C., Amiraslani, F., Chenu, C., Cardenas, M.G., Kaonga, M., Koutika, L.S., Ladha, J., Madari, B., Shirato, Y., Smith, P. and Soudi, B., 2020. The 4p1000 initiative: Opportunities, limitations and challenges for implementing soil organic carbon sequestration as a sustainable development strategy. Ambio, 49(1), pp.350-360.
- Rumpel, C., Amiraslani, F., Chenu, C., Cardenas, M.G., Kaonga, M., Koutika, L.S., Ladha, J., Madari, B., Shirato, Y., Smith, P. and Soudi, B., 2020. Response to "The "4p1000" initiative: A new name should be adopted" by Baveye and White (2019). Ambio, 49(1), pp.363-364.

In addition to that, a conference on the 4 per 1000 topic was held in June 2019 (https://symposium.inrae.fr/4p1000/) where scientist were given the opportunity to present their cases (in favour or against). The reviewer had presented his case and a paper that summarises their consensus was published:

- Amelung, W., Bossio, D., de Vries, W., Kögel-Knabner, I., Lehmann, J., Amundson, R., ... & Chabbi, A. (2020). Towards a global-scale soil climate mitigation strategy. Nature communications, 11(1), 1-10.

With respect to the current paper, our replies to the comments are as follows.

Comments on the scope of this paper and its novelty

Here we present a global modelling study based on a large database of soil observations (83,416 samples). We use the quantile regression framework to determine different levels of soil organic carbon (SOC) taking into account soil, climate, topography, and land use variability. Given those levels, we estimate the additional SOC storage potential in global croplands, which is clearly stated as our aim in P1L17. The combinations of dataset, scale and methodology makes this a unique study. The first study that looks at carbon potential based on global empirical data.

You mention that other publications, such as the case study by Riggers et al. (2021) in Germany, based on a simulation, reached similar conclusions to this study. Somehow you see that as something negative. The opposite view is that it is nice to see that the conclusions hold at different scales, using different datasets and methods.

Comments on the use of the term "additional carbon storage"

How to call what we calculated was the most challenging part. We are not referring to the maximum amount of SOC a soil can hold (associated with mineralogy). In addition to that, we are not specifically talking about a net CO_2 removal from the atmosphere which is closer to the definition of carbon sequestration. We use the term additional as in "in addition to the current stocks". We will expand the last paragraph of the introduction to clarify this.

That said, we are open to suggestions if the title is not clear enough. Perhaps "~~Additional~~ soil organic carbon storage potential in global croplands".

Specific comments

"I find this title potentially very misleading. Some readers may derive the impression from it that, relative to what many researchers have described in recent years as the (limited) potential of soils to sequester carbon, Padarian et al. have somehow found"additional" storage potential. That is not the case, since the conclusion of their text really only confirms the many previous assessments that have been published and that all point to the very marginal contribution soil carbon storage or sequestration might make to climate change mitigation."

As mentioned above, we will add a clarification to what we mean by "additional" and we are also open to modify the current title.

"Line 1 of abstract: 'Soil organic carbon sequestration (SOCseq) is considered the most attractive carbon capture technology to partially mitigate climate change.' The very first sentence of the article is seriously misleading as well. . . "

Changed to "Soil organic carbon sequestration (SOCseq) is considered **an** attractive carbon capture technology. . . "

"Page 2, lines 17-18. In support of their assumption that the 0-30 cm depth of the soil is where most of the SOC storage occurs, the authors cite 2 classic but relatively old references on a topic about which a lot has been written during the last 2 decades, in particular by researchers who have recommended that measurements of soil carbon storage should routinely extend deeper than just the top 30 cm. It seems to me highly desirable that the authors justify their assumption in light of this more recent literature, and not just older articles."

We wrote: "We focused our analysis on the top 30 cm of soil since it **accounts for a large proportion** of the SOC stored in soils.". That is very different to "the 0-30cm depth range is where most of the SOC storage occurs". Proportionally speaking (in terms of depth units) SOC density is higher in the 0-30 cm range since SOC content usually decreases in depth. We will add "carbon density" to be extra explicit about this.

We also gave 2 other reasons to focus on the 0-30cm range (it is considered the depth that can be effectively managed to capture carbon, and presents faster turnover times). We agree that measuring SOC at greater depths is important but perhaps more relevant in other systems such as grassland, which are not included here, as mentioned in P10L8.

Another data related reason is that many soil surveys only collect topsoil samples. Since we want to maximise the number of observations for modelling, only using observations that measured carbon to 100+cm was not an option.

"Figure 1. I was taught by statistics professors never to calculate linear regression lines, especially when data points are severely scattered as is the case in Figure 1, without considering the uncertainty that is associated with the regression coefficients. I do not believe there is any reason to envisage quantile regressions differently. Also, the case is not made particularly well in the text as to why the authors chose to consider the median rather than the mean. Finally, the last sentence of the figure caption requires more information in order to be understandable. How reasonable is it to adopt the hypothesis that"the difference is mainly due to management practices"? Could other factors than soil, climate, topography, and land use account for the difference, and to what extent?"

Figure one is just an explanatory diagram, not derived from our data.

We use the median to more accurately describe the central tendency since the data is positively skewed. We will update the text accordingly.

Regarding the hypothesis that "the difference is mainly due to management practices" (after taking into account soil, climate, topography, and land use variation) we think is quite reasonable. Of course, the whole process is quite complex and a model will always be underspecified, particularly at the global scale where data availability is a limiting factor. We will add some references and update the text to reflect that.

Page 4, line 21. Again, why not consider the mean? That is not clear at all. Does the choice of the median make a difference in terms of the final conclusions?

We will update the text to explain why we used the median. The final estimates will vary but the final trend and conclusions will be the same (a relatively small amount of extra SOC stored vs the high levels of emissions).

Page 4, line 30. Most readers are probably not going to be familiar with Shapley values, which have not been used much in soil science, so a more detailed introduction to them and to their advantages is indicated here.

We will add more details.

Page 5, lines 5-6. There is a huge amount of material missing here. The authors mention that they ran all kinds of simulations with 9 general circulation models, "for the moderately pessimistic SSP3-7.0 scenario, which considers a world that does not enact climate policies". Why that particular shared socio-economic pathway scenario, among all the possible ones??? How does that choice affect the conclusions reached at the end of the article? Without a whole more information in that respect, it is hard to evaluate the simulations that have been carried out.

We used the mean prediction of those nine General Circulation Models (i.e. a single “simulation”). We will rewrite the section to make that clear.

Regarding why the SSP3-7.0 scenario, we think it is very illustrative to show what happen if we do not do something (hence the description of a “world that does not enact climate policies”). Of course, it would be very interesting to test all possible combinations of General Circulation Models and socio-economic pathways but that is out of the scope of this paper. We will expand the section to give more details on why we picked that scenario and add a more description of it.

The conclusions would probably co-vary with the amount of change. For the moderately pessimistic scenario, we estimated that the potential would decrease by 18%. Worse scenarios will show a larger decrease and vise versa. Of course, a complete study running all the possible combinations would be necessary to know for sure, which, as we already mentioned, it is out of the scope of this paper.

Page 5, lines 21-22, “Soil clay content has been recognized as a key factor in SOC stabilization”. Again the authors are basing their statement on relatively old references. In recent years, various researchers have shown that in practice it is not the clay content per se that matters, but the ratio SOC/clay content (e.g., Johannes et al., 2017; Prout et al., 2020)

That SOC/clay ratio does depend on clay content. We will add those reference and update that sentence.

Page 9, lines 16 and 17, “which corresponds to only 3.5% of the C emissions used to estimate the 4 %o rate”. Either I do not understand what the authors mean by that statement, or I do not understand how they could just mention it in passing without emphasizing how this statement challenges everything that has been claimed about the 4 per 1000 idea...

4 per 1000 uses an annual emission of 8.9 Pg C. Given our estimates of potential stocks and a 4‰yr^{-1} accumulation rate, that means that we could only offset 3.5% of the total C emissions (8.9 Pg yr^{-1}). We will update that sentence to clarify and emphasise how small 3.5% is.

Page 9, lines 21-23, “Our estimates are in line...”. In support of the statement in this sentence, the authors cite relatively old references again, but fail to point out that Franzluebers et al. (2012), whom they cite 4 lines earlier, reach a different conclusion. These authors observe that 10 years after conversion of an arable cropping system into perennial grassland — admittedly one of the fastest agricultural practices to sequester carbon in soil — the rate of C accumulation down to a depth of 20 cm drops by half, and after 20 years, it is only 0.2 Mg ha-1 y-1, i.e., a quarter of its initial value of 0.8 Mg ha-1 y-1 (see Fig. 2 in Baveye et al., 2018). After 50 years, the rate is virtually zero, and a new soil equilibrium is reached. So, at least some people have found timeframes that are much shorter than those found by Padarian et al. This point needs to be discussed.

In this study we are not considering grasslands but we agree that some people have found much shorter timeframes. In fact, the map under that paragraph (Fig. 5) shows a large variation in the timeframes, with a lower boundary even shorter than 50 years. We start that paragraph saying “In addition to using a fixed accumulation rate, the results presented in Fig. 5 assume a linear accumulation. Of course, soils behave differently with SOC accumulation diminishing approximately exponentially in time”. Right after the sentence you mention, we add “Regardless of the accumulation rate, the total additional carbon storage potential of the topsoil in croplands is limited.”.

We think that is in line with your specific comment. We will add a mention the lower limit (in addition of the upper limit of “over a century”) to make that clearer.

Page 10, line 32, “our practicable potentials account for only 32% of the historical carbon debt due to agriculture”. Again, this statement needs to be emphasized more than it currently is.

In the conclusions we write “Regarding the historical impact of agriculture, our results suggest that the current management practices close to our 75^{th} percentile can only recoup 32% of our estimated 92 Pg C historical debt. Even considering the best current management practices (equivalent to our 90^{th} percentile), we would not be able to fully recoup that debt, only offsetting 72% of them. Hence, agriculture has an intrinsic environmental cost that needs to be taken into account for territorial planning.”.

We will expand that paragraph to emphasise it further.

Page 10, line 9, “From that year onwards, the accumulation rate could not be maintained due to sink saturation”. There is something missing in the narrative between the previous page and this

one. On page 9, the authors are referring to accumulation timeframes of over a century, and now mention sink saturation occurring in 2050.

That comes from the review by Fuss et al. (2018), not our results. We will rewrite that sentence to make it clearer.

Page 11, line 7, "in the next 20 years". Why only 20 years?

That comes from the dataset used (WorldClim) which has averages over 20-year periods (2021-2040, 241-2060, 2061-2080, 2081-2100). We only used 2021-2040 since a) it is illustrative enough and b) uncertainty is lower (the uncertainty increases as we predict further into the future). We will expand Section 2.4 to clarify that.

Page 11, section 3.4. This section contains a lot of hand waving to try to justify asking for more funding to carry out research on soil organic matter, but I doubt that the arguments presented would convince very many decision-makers.

As you mention in page 4 of your review: "As some of us have pointed out recently, that does not mean that research on soil organic matter dynamics, and in particular on its effect on the resilience of the architecture of soils under fast changing environmental conditions, is not needed."

Research in organic matter is still needed. We point out some research topics that are directly related to this paper (that would improve the presented model) but there are many others, including the ones related to your research.

Page 12, lines 10-11, "The total amount of additional carbon that global croplands can store is relatively small in the context of global carbon emissions". It took 12 pages to get to the point where the authors concede that their conclusion is similar to what other people have said consistently since 2015, and, actually, Arrouays et al. (2002) already wrote in 2002. Hence the question I raised earlier of whether this article really needs to be published, since it contributes very little, if anything at all to the debate. As I wrote earlier, this manuscript would be useful if it were revised in a way that it carry the message that it is time to stop the nonsense, and to agree once and for all that the sequestration or even the storage of carbon in soils is nowhere near large enough to be more than a very marginal contribution to the mitigation of climate change. If the authors stated that clearly, this article might be useful to close a parenthesis that should never have been opened, and to encourage policy-makers to focus back on societal changes that can have a real effect on climate change, such as a switch to renewal forms of energy, or a move to an economy that involves less long- distance transport of goods than is the case at the moment.

We mention that in the abstract (page 1) but we will add that same sentence to make it more explicit. We present that in page 12 because it is a conclusion derived from the results of the analysis performed in this study.

Regarding the novelty of this paper, refer to or general comment on page 1 of this response.