Interactive comment on "Additional soil organic carbon storage potential in global croplands" by J. Padarian et al.

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The manuscript "Additional soil organic carbon storage potential in global croplands", submitted by Padarian et al. for publication in *Soils,* gives me from the onset two very conflicting impressions. The first is that it is a sad reflection of the dismal state of our discipline that the authors attempt to publish this manuscript in 2021 when the calculations it presents, had they been carried out many years ago as they should, would have helped avoid lots of time wasted in futile discussions about a chimera. The second feeling I have is that, after substantial revisions, this text could provide a welcome closure of sorts to a particularly dismaying episode in the history of soil science, arguably rivaling in embarrassment for our discipline with a time in the late 60s when blind opportunism led some soil scientists to be dramatically on the wrong side of history (in the "polywater" episode). I will try to explain first where these opposite feelings originate, before I provide specific suggestions to revise the manuscript in order to emphasize the message that in my opinion it should contain.

During the first decade of this century, the scholarly literature on soil organic matter dealt even-handedly with a healthy diversity of topics. Many researchers were concerned about the fact that, as a result of climate change, rising temperatures in different parts of the globe could hasten the mineralization of soil organic matter, release large amounts of CO_2 in the atmosphere, and thereby speed up climate change even more, to the point where it would get entirely out of control (Powlson, 2005; Baveye, 2007). The effect of rising temperatures in that context remained poorly understood, and the general feeling was that it was important to elucidate it urgently, in part to determine if it was possible practically to slow down the decrease in soil organic matter content, which, as the monitoring results of Bellamy et al. (2005) revealed, had been going on for decades. At about the same time, research on the "priming effect" indicated that any addition of fresh organic matter to soils to compensate for its faster mineralization would have to be carried out extremely carefully, because in the process one ran the risk of mineralizing "stable" or "recalcitrant" organic matter that had been residing in soils for extremely long periods of time (e.g., Fontaine et al., 2007). Progress was also achieved concomitantly on the nature and fine-scale heterogeneity of soil organic matter, using advanced spectroscopic techniques (e.g., Schumacher et al., 2005; Kinyangi et al., 2006), and on accounting for the effect of the microscale heterogeneity of the soil pore space on the dynamics of SOM mineralization by microorganisms (e.g., Kuka et al., 2007). In many ways, this period can be characterized as one where researchers had a clear perception of the utter complexity of the system they were dealing with, realized that all of its multiple facets needed to be investigated carefully, were not afraid to engage in a "science as subtle as its objects of study" (Dorit, 2011), and were making significant progress.

Then hype hit the proverbial fan. Several authors tried in their writing, and in meetings with policy makers in Brussels and Washington, to promote the sequestration of carbon in soils as a win-win scenario to mitigate climate change (e.g., Lal, 2004, 2010). At about the same time, others started promoting biochar, with vastly overblown, still unfulfilled, promises that the addition of large amounts of pyrolysed organic matter to soils could be a "win-win-win" solution to the climate crisis. The reception of these silver bullets by decision-makers was lukewarm, at best, but that did not deter some from trying to push their agenda further behind the scene. The result of these efforts was that, at the COP 21 meeting in Paris, the then minister of agriculture of France announced that by increasing the carbon content of soils by 0.4% or 4 per 1000 per year, one could compensate for the yearly anthropic release of CO_2 into the atmosphere. Even though several reports published earlier by French scientists (e.g., Arrouays et al., 2002; Chenu et al., 2014) had calculated that the amount of carbon that could be sequestered overall in soils was much lower than that put forth in the 4 per 1000 proposal, the latter was nevertheless adopted by COP 21 participants and was integrated in the final reports of the conference. Criticisms of the proposal were very quick to emerge from various quarters, e.g., by White (2016) who concluded his analysis of the 4 per 1000 initiative by stating unequivocally that "sequestering carbon in agricultural soils will not provide a major offset for greenhouse gas emissions". Essentially the same opinion was widely circulated in private conversations or e-mails, for example in an e-mail I received from a prominent soil researcher, who wrote about soil carbon sequestration that he was not willing to "promise that it will deliver a complete offset of fossil C emissions, which is plainly absurd".

At that stage, given the serious doubts expressed by various people about the 4 per 1000 initiative, it would seem that the reasonable thing to do would have been to pause, proceed to further estimations (like those now described by Padarian et al. in their manuscript) of the actual carbon sequestration potential of soils, and reflect carefully about the exact potential of this approach to mitigate climate change, before any more efforts were made to convince politicians that this was the way to go. A well-known historical precedent should have guided soil researchers on how to deal properly with controversial claims made by politicians supposedly on the basis of scientific or technological ideas. In 1983, U.S. president Ronald Reagan announced the concept of the Strategic Defense Initiative (SDI), quickly nicknamed derisively the "Star Wars Program". Spearheaded by several physicists, in particular Edward Teller (the "father of the hydrogen bomb"), the initiative was nevertheless greeted by intense skepticism and even mocked as ludicrous by numerous other physicists, from the moment it was launched. After the initiative failed to deliver preliminary proofs of concept, the U.S. government asked the American Physical Society (APS) to conduct an in-depth analysis of the feasibility of the initiative and of the extent to which it was advisable to devote significant amounts of money to it. Even though physics research stood to gain a lot financially from SDI, which made some physicists reluctant to "throw away the baby with the bath water", the APS report (Bloembergen et al., 1987) nevertheless, very candidly, concluded that the technologies being considered were decades away from being ready for use, and that at least another decade of research was required to know whether what had been proposed in the SDI was even possible. Thereafter, in short order, the budget of the program was repeatedly cut, its focus severely scaled-down, and eventually the initiative was cancelled. By then, one could argue that SDI had been a political success, in that it probably helped end the cold war. However, had the APS not effectively managed to terminate it, SDI would undoubtedly have turned out to be an embarrassment of major proportions for the physics community in the U.S., because of its blatant inability to deliver on unwarranted promises made, and because of suspicions of manipulation and fraud that inevitably would have arisen (Broad, 1992).

Regrettably, this sound precedent was not emulated in our discipline. In April 2017, Minasny et al. published an article that to casual readers (as politicians would likely be), may seem like an endorsement of the 4 per 1000 initiative by a large group of scientists worldwide. Indeed, in the abstract, the authors state that "reported soil C sequestration rates globally show that under best management practices, 4 per mille or even higher sequestration rates can be accomplished". Anyone not reading the article further (and in particular not reaching a contradictory statement a few lines down in the abstract) would likely conclude that the 4 per 1000 idea was what Lal liked to refer to as a "low-hanging fruit" that is easy to reach. The undeniable risk associated with such a perspective is that it would give ammunitions to those who want to slow down the transition to renewable forms of energy, away from the greenhouse-gas-producing consumption of fossil fuels.

If the goal of our work is to publish articles that get heavily cited, then the article by Minasny et al. (2017) has been a resounding success, since according to Google Scholar it has been cited more than 900 times so far, since its publication. However, from a more elevated perspective on science, the article was an unmitigated disaster. Contrary to the rich, multifaceted approach to soil organic matter that had prevailed a decade earlier, the various contributors to the article of Minasny et al. (2017) adopted an extremely narrow view on the topic. The effects of rising temperatures or priming on the carbon content of soils were entirely ignored, as was the necessarily linked fate of nitrogen or dynamics of microorganisms. A significant part of the earlier literature on all these issues, even though the knowledge it contained was eminently relevant, was completely bypassed. Several groups of researchers promptly wrote letters to the editor to criticize the conclusions of the Minasny et al. article (van Groeningen et al., 2017; Amundson and Biardeau, 2018; Baveye et al., 2018; Poulton et al., 2018; VandenBygaart et al., 2018; White et al., 2018). Like the APS report did a few decades ago for the SDI, the very sound criticisms raised by these various authors should have put an end to the propaganda campaign surrounding the "4 per 1000 initiative" and in particular its portrayal to decision-makers as a low-hanging fruit. But it did not, sadly, and hundreds of articles are now being published every year where researchers are trying to demonstrate that, locally under very specific conditions, the 4 per 1000 goal can be reached. Some articles go as far in their biased attempts to justify the initiative as to ignore the severe disequilibrium caused by rising temperatures or enhanced erosion in their modeling of future trends in soil carbon contents (e.g., Chenu et al., 2019)! Efforts such as these are not useful and do not warrant publication, since the fact that, under very special conditions, soil carbon content can be drastically increased was never in doubt. The Dogons in Mali demonstrated that conclusively centuries ago… What has been in doubt from the very beginning was the ability of soils to offer a solution to climate change at a large enough scale and over a long enough timeframe that it should be considered seriously by policy makers.

Amidst the mass of articles that have unfortunately been devoted to the 4 per 1000 initiative since 2016, some have carried out the kind of calculations that should have logically been carried out before the propaganda-style promotion of soil carbon sequestration began. A good example in this respect is afforded by the very comprehensive analysis carried out recently by Riggers et al. (2021) in the context of German croplands, to determine the extent to which changing climate in decades ahead could affect soil organic carbon stocks. These authors considered 3 different climate change scenarios between 2014 and 2099, as well as a scenario assuming no future climate change. They used 5 distinct methods to estimate organic carbon inputs based on crop yields and crop-specific parameters, and adopted a multi-model ensemble consisting of five different SOC models to predict the level of organic carbon input required to reach specific SOC stocks in soils at the end of the 21st century. Their simulation results suggest, among other things, that organic carbon input to the soil in 2099 needs to be between 51 and 93% higher than what it is today just to maintain SOC stock levels at their current value. Riggers et al. (2021) eventually conclude that "under climate change increasing SOC stocks is considerabl[y] challenging since projected SOC losses have to be compensated first before SOC built up is possible. This would require unrealistically high OC input increases with drastic changes in agricultural management." In other words, their conclusions are not very different than what White (2016) had written.

Given the recent publication of this excellent article by Riggers et al. (2021), which incidentally Padarian et al. do not cite, and of other recent work that reaches the same conclusion, it seems legitimate to ask whether the manuscript by Padarian et al. comes up with anything sufficiently new that its publication is warranted. In terms of its contents, the answer to that question is negative, in my view. Enough authors have confirmed by now Arrouays et al.'s (2002) or White's (2016) assessment that soil carbon sequestration can only be a very marginal solution to climate change. In that context, Padarian et al.'s calculations add nothing. The only purpose that one more publication on the topic would achieve is to increase the number of citations of those who wrote on it earlier. That is not a valid achievement from my perspective. Nevertheless, their paper could still have some value if it were revised in such a way as to come out as an acknowledgement by the authors that the Minasny et al. (2017) article was a blunder of epic proportions, which oriented much of the research on soil organic matter in the last 5 years in a direction it should never have taken, leads nowhere, and should now be forgotten. That, hopefully, would put an end to the nonsense started in 2015 and would restore some of the credibility that our discipline has lost in the process. As some of us (Baveye et al., 2020; Vogel et al., 2021) 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. It is in fact direly and urgently needed for other reasons than those advocated in the 4 per 1000 chimera, and that point has to be made to policy makers, without repeating the mistakes made in the last few years, i.e., without making promises that we are not sure we can keep, or even worse, that we already know we cannot keep. Soils, and the structure and fate of soil organic matter in particular, are very complex, and we cannot promise we can come up easily with straightforward answers to the daunting questions society is asking us. But we can promise that we shall try to answer them as best we can, in direct continuation of the very nice research that was carried out a decade ago.

Specific comments:

Page 1, title: 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.

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. Certainly there are a few articles presenting SOCseq as an ideal technology to capture carbon, but there is no consensus on the matter at all. There is a huge literature reviewing in detail modern carbon capture technologies, in which authors do not mention soils at all (see, e.g., Rubin et al., 2012; Wilberforce et al., 2021). Clearly, when people outside the soil science community think about carbon capture to mitigate climate change, they overwhelmingly do not think about soils among the top 10 best candidate technologies.

Page 1, lines 24-25. Again, it is not really clear what the authors are referring to with this “additional” SOC storage potential. What is it “additional” to?

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.

Page 3. 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?

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?

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.

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.

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).

Page 8, line 10, "are in located". I have a hard time understanding how, with two native English speakers as co-authors, the manuscript was submitted with obvious typos, misspellings, and syntax problems. Did they not read the manuscript, or do they simply expect someone to do the editing for them?

Page 9, lines 16 and 17, "which corresponds to only 3.5% of the C emissions used to estimate the 4 ‰ 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…

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.

Page 9, line 23, "total estimates". It is clear what the authors refer to with this expression.

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.

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.

Page 10, line 18, "imped". English!

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

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.

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.

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