

Interactive comment on “Global meta-analysis of the relationship between soil organic matter and crop yields” by Emily E. Oldfield et al.

Anonymous Referee #2

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This study examines the relationship between SOM and yields of wheat and maize across a range of agroecological contexts around the globe. The authors then apply this relationship to better understand the potential of increased SOM stocks to improve yields, as well as reduce N fertilizer inputs.

The study is ambitious in scope and their approach involved a number of assumptions and simplifications, and therefore requires considerable caution in the interpretation of their findings. Despite these drawbacks, I appreciated the effort and feel that the study represents a valuable and novel contribution towards addressing a complex issue with relevance to global agricultural sustainability. While I enjoyed this paper, I have several comments/critiques for the authors.

General comments:

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The premise that increased SOM will reduce N inputs seems a bit misleading. Both the building of SOM (to 2% SOC) and its continued maintenance at this higher level will require considerable quantities of organic matter inputs both now and into the foreseeable future. So it seems unlikely that total N inputs will actually decrease, but really we are talking about a shift from inorganic to organic N sources. The authors allude to this in several places, but it could be spelled out more clearly. In reading the authors' responses to Reviewer 1, it seems that they now better recognize the need to address this.

Related to this, the study largely ignores the dynamic state of SOM. For example, soils in a state of rapid SOM decline may actually be supporting yields better than a soil at a similar level of SOM, simply because more nutrients are being mineralized as this SOM is lost.

I appreciate Fig. 1 showing origin of the datasets considered in this study, but am a little concerned about the high number of observations from China and how this might bias the findings. This should be addressed in the discussion.

Related to the above comment, it would be nice to see a table that provides a breakdown of how the sites were distributed in terms of number of sites with and without irrigation and with wheat vs. corn, as well as different ranges of pH, aridity, clay content, latitude, so that readers can better assess potential biases in the dataset on their own. This could be a new table in the main text or alternatively in the supplementary materials.

I understand the value of keeping the model relatively simple, but was surprised that several potentially important interaction terms were left out, while others (i.e., SOM x N input) were included. For example, I would expect to see a strong interaction between SOM and irrigation, such that SOM would be more important in rain-fed systems (particularly in semi-arid regions) than in irrigated agriculture, where the water related benefits of SOM would be less important. Also, I would have expected the different

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crop types (and potentially sandy vs. clay textured soils) to respond differently to varying SOM levels. Please consider including these terms or at least explain why the SOM x N interaction was included in the model and some of these other terms not.

Specific comments:

L112: the reported value of 0.25 is not very informative here in the text without providing units or some sort of additional explanation.

L116-121: the logic behind the sentence “the asymptotic relationship between SOC and yield lends support to the idea that building SOC will increase yields – at least to a certain extent – as opposed to simply being an outcome of higher yields.” Is not entirely clear. Could it not be that yields have a larger effect on building SOM at higher levels? These two sentences should be perhaps omitted or further clarified.

L133: It seems the asymptotic relationship and leveling off above 2% (in Fig 2) may be strongly influenced by relatively few observations and I wonder if the authors conducted an sort of leverage tests (e.g., Cook’s distance) to examine the potential influence of extreme observations. This is especially notable for the 4-5 sites that were at or above 2.5% SOC and with very low fertilizer addition and yields (in the bottom right corner of Fig. 2).

Also, It is not entirely clear how inter-annual variability was taken into account, especially for rain-fed sites, where a severe drought in the year of yield data collection could drastically skew results.

L154: specify that your are referring more to ‘inorganic’ of ‘synthetic N inputs’

L155: suggest replacing ‘achieving’ with ‘obtaining’, as crops to not achieve nutrients they obtain them.

L159-163: As mentioned above, the authors should acknowledge that higher SOC is not necessarily allowing for lower total N inputs, but perhaps lower synthetic N inputs, since there is likely to be relatively higher inputs of organic matter (and organic N) in

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soils with higher SOC, or at least there should be if are managed in a way that that seeks to maintain these levels of SOC.

L164-165: Again, why where interactions only examined between SOC and N input and not for other factors that are very likely to interact with SOC, such as irrigation, crop type, texture, and aridity?

L165-167: Could this also have to do with Liebig’s law of the minimum, such that higher SOM levels are really just supplying more P, K and other essential nutrients that may be co-limiting to N at higher N levels, but not at low N application levels. Please clarify

L188-191: What is this calculation and the 3.73 million tons based on? Please elaborate.

L233: yes, water retention is important, but also improved nutrient (especially N) supply from decaying SOM

L372-374: Again, based on this section and Table 1, what was rationale for including the SOM x N interaction? Also, as mentioned above why were other variables, that were likely to strongly interact with SOM (e.g., irrigation, clay, crop type), not included? This seems rather arbitrary and inclusion of these other interaction could have helped explain significant variability in yield across sites.

Fig 1: again need to the discuss the potential bias of having so many sites from one country, China, especially since fertilizer inputs in China are typically much higher than other parts of the world.

Fig 2: this is confusing, as the title suggest that the relationship includes maize and wheat, but then the next sentence says that its just for rain-fed maize. Please clarify.

Fig 4: which crops/conditions are being presented here. As for Fig 2, this needs to be better clarified.

Fig 5: the numbers on top of the colored boxes are small and difficult to read, especially

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when printed in B&W

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