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

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Response to Reviewer 2

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.

RESPONSE: Thank you for this overall positive assessment. We do indeed view our work as a step forward in addressing a complex issue with relevance – academically, for policy, and for practice – to global agricultural sustainability. Equally, we appreciate the limitations of our work. We believe that our response to your comments below will ensure that we detail these limitations openly in our manuscript so that the advance we offer can be built upon constructively to evaluate the inferences we make.

General comments:

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.

RESPONSE: Thank you for this comment. As Reviewer 2 mentions, this also came up with Reviewer 1. In our revision, we will address this more comprehensively. Specifically, as we mentioned in response to Reviewer 1, we recognize that a combination of both organic and inorganic nutrients will be necessary to both build SOM and improve crop yields. Building SOM and cutting back on N fertilizer will require that the SOM-N mineralization compensates for the reductions in mineral N fertilizer, and we will state this in our revised manuscript.

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.

RESPONSE: This is a good point, and we will provide further explanation in our discussion related to the “soil carbon dilemma” (Janzen, 2006). Namely, that to derive the benefits of SOM, it must be mineralized and used. As Janzen mentions in this paper, to both build SOM and derive nutrients from it, continuous inputs of organic matter will be needed to account for that which is lost through mineralization. We will ensure that our revised discussion highlights this expected effect of SOM (i.e. nutrient supply), but will also make sure to highlight that SOM is expected to have positive effects on productivity for other reasons (e.g. improved aeration and moisture supply). As such, a soil with rapid SOM decline could provide more nutrients but also could be limiting for other reasons. We will call for controlled experimental work to help uncouple such mechanisms.

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.

RESPONSE: We do have a large proportion of studies from China in our data set as is highlighted in Fig. 1. We state, however, in our methods that the variation observed in our data set for our model parameters reflects that observed within the global data sets we used for our extrapolations (lines 443-447). However, we agree this is an important point when making “global generalities,” and in a revised discussion we will emphasize the need for studies to inform such understanding to come more evenly from systems where wheat and maize are grown.

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.

RESPONSE: This is a good idea, and we will include such a table in our supplemental materials. Furthermore, we have uploaded our entire data set to KBS repository and provided a link to it within our manuscript for anyone to view and use. This way, readers can explore the data to see the breakdown in variables as Reviewer 2 mentions.

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) where 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 soils with higher SOC, 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.

RESPONSE: When we initially created our model, we did not include any interactions because of the sheer number of potential interactions that could be included, which would take up too many degrees of freedom. Additionally, and perhaps more importantly, 3-and-more way interactions are very hard to disentangle and their statistical significance should be interpreted with caution (Gelman and Loken, 2013). Following the philosophical and operational statistical methods we adopted ((Hobbs and Hilborn, 2006) – cited in our methods), we limited most of our exploration to only two-way interactions where we had a strong ecological rationale for expected effects. As such, we decided to include an SOM x N interaction to specifically explore potential reductions in N fertilizer with increased SOC concentrations. This was an effort to see if there is a level of SOC that can compensate for N input. We will further justify our decision for including this interaction and not others in a revised version of our manuscript. However, we do acknowledge that the interactions Reviewer 2 suggests present interesting lines of inquiry. We re-ran our regression model with additional interactions to include SOC x irrigation, SOC x clay, and SOC x aridity. These interactions did not offer any additional explanatory power (the r2 was essentially unchanged and the coefficients were small). Furthermore, the main findings between SOC, N inputs, and yield were essentially unchanged with additional interactions. As our main findings remain the same with and without these additional interactions, we are choosing to maintain our analysis as it is. We will provide as supplementary material a table that shows the lack of sensitivity of SOC and N input effects to inclusion of these additional two-way interactions, justifying the regression model results we focus on.

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.

RESPONSE: This number was meant to point out the fact that the slope of the relationship between SOC and yield levels off at 2% SOC. We will clarify this point in our revision.

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.

RESPONSE: This discussion was meant to highlight the challenges of quantifying the relationship between SOM and yield since the relationship could potentially be causative in both directions, with greater SOC leading to higher yields but also higher yields increasing SOC concentrations. This sentence was intended to demonstrate SOC as a cause – at least to some extent – of higher yields in the case of our analysis. For instance, if yield was on the x-axis as an explanatory variable for SOC on the y-axis, we would expect higher yields to keep driving higher levels of SOC (i.e. the relationship would appear more linear) since we know that soils can accumulate con-

centrations much greater than 2% (i.e. we are at a point that is well below theoretical and/or empirical soil C saturation points); however, our data do not display this pattern and higher yields do not appear to be driving higher levels of SOC. We will clarify this point in our revision.

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 any 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).

RESPONSE: We did not perform any leverage tests for our initial analysis. However, as suggested by Reviewer 2, we did evaluate Cook's distance and did not find any influential data points that would significantly change our regression relationship. Additionally, we re-ran our regression after removing the 4 data points in question, and model coefficients remained essentially the same. We will mention this in the revised methods and in the legend 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.

RESPONSE: We nested year within site as a random effect in our regression model to account for spatial and temporal correlation. We will clarify this in our revision. There were some instances of low yields at rain-fed sites within our data set. We believe these observations are important to include as they capture the local realities of the relationship between climatic variables and yield. Our data set then uses these locally observed data points to capture a global average relationship between SOC and yield, which we state in the paper will need to be built on at sub-regional scales to provide data directly relevant to farmers and land managers.

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

RESPONSE: We will make this change in a revision.

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

RESPONSE: We will make this edit in a revision.

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 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.

RESPONSE: We will make this point clear in a revision.

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?

RESPONSE: As mentioned above, we chose to include only an SOC x N interaction since we were asking a question specifically related to the interaction between SOC and N as it relates to agricultural inputs. In the revised paper, we will include the additional regression analyses to show that including these interactions did not offer any more explanatory power to our model and our main findings (i.e., coefficient estimate sizes) essentially remained the same.

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

RESPONSE: This is a good point and one we will include as a way to explain why N had a greater impact on yield at higher SOC concentrations. Other benefits of higher

SOM include better moisture retention, improved structure and aeration, etc., so there is a substantive list of benefits expected for plants at higher SOM concentrations.

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

RESPONSE: We include the basis for this calculation in our methods (lines 495-502). In the revision, we will refer specifically to those lines in this section of the discussion.

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

RESPONSE: This is a good point and one we will include in our revision.

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.

RESPONSE: We agree that there are a number of potential interactions between SOC and other variables included in our model. As mentioned above, we will now explain our decision and also include the additional regression output as a supplemental table.

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.

RESPONSE: As mentioned above, in the revised discussion we will emphasize the need for studies to inform such understanding to come more evenly from systems where wheat and maize are grown. Further, we will highlight that our Chinese-site derived observations fall within the range of those we used for our global extrapolations. Therefore, we felt confident that potential bias for establishing a global, average relationship between SOC and yield was minimal in this instance.

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.

RESPONSE: Thank you for bringing this to our attention. Reviewer 1 also noticed this error, and we will edit our caption in a revision to specify that the regression relationship only includes rain-fed maize.

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

RESPONSE: We will clarify this in a revision as well to specify that the regression relationships are plotted for rain-fed maize.

Fig 5: the numbers on top of the colored boxes are small and difficult to read, especially when printed in B&W

RESPONSE: We will edit these to increase their font size for readability in our revision.

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