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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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The authors use a global data set on maize and wheat yields together with soil and other environmental variables to derive statistical relationships between SOC and yield. The overall value of the study is appreciated. The interpretation of the data and observed relationships is, however, going too far because direct evidence for the postulated effects, as it could be derived from long-term experiments at different SOC levels, cannot be derived and many other influencing factors were ignored.

Title and abstract. In both, SOM is described as the key variable but the study relies on SOC data. This should be reflected in the title and the abstract. This already touches a more fundamental problem – the study does not provide mechanistic insight as to why higher SOC results in higher yields. More SOC is often obtained using more organic inputs, i.e., more macro- and micro-nutrients bound to SOM. A second issue here, related to the first one is that, correctly, a higher SOC concentration might reduce the amount of N needed as fertilizer to get the same yield, but it is not discussed how much more N must be fertilized to reach the higher SOC level.

Response: We appreciate the reviewer's comments and believe we can fully address them in a revision. The first major concern is that we do not provide mechanistic insight as to why SOM (or SOC) would increase yield. We believe that the mechanisms between SOM/SOC and crop yield have been well established, but poorly quantified. For instance, we would expect SOC to be associated with greater cation exchange capacity for the exchange of micronutrients and greater water holding capacity. SOC, because it is the majority constituent of SOM, is also highly correlated with macro-elements contained in SOM. The contribution of our project is not to tease apart the relative importance of the separate mechanisms by which SOM/SOC operates, though we do believe this would be a very important, but challenging, project. Instead, our aim is to establish relationships at broad scales between SOC and yield to provide better quantification of this relationship for policy initiatives such as the recently launched Global Soil Health Challenge. This has been identified as a critical knowledge gap among producers, policy makers, and researchers alike (Adhikari and Hartemink, 2016; Chabbi et al., 2017; Hatfield et al., 2017). For instance, the U.S. National Research Council stated in their 2010 report on sustainable agriculture that "measures of [SOM] are a cornerstone of most sustainability and soil quality assessments...However, the numerical level that would be considered good, or what change in [SOM] levels constitutes a significant functional change, has not been established (NRC, 2010)." Our paper is an attempt to answer that call, and we will make this clear in a revision.

Secondly, the reviewer raises concerns related to the challenge of using observational differences across broad spatial scales to test SOC-yield relationships. We note here and will make clear in our revision that the differences in soil carbon observed in our data set are from experimental plots capturing long-term differences in SOC within a given site. Specifically, our data capture differences within SOC in experimental plots largely driven by management interventions related to inputs (e.g. compost, fertilizer, manure, crop residues) and tillage (e.g. no-till versus till). We capture these site-specific differences in management with site-level random intercept terms.

Regarding our analysis of potential fertilizer reductions: we recognize that a combination of both organic and inorganic nutrients will be necessary to help build SOM and improve crop yields (lines 179-180). We will highlight this further in a revised manuscript and stress that building SOM and cutting back on N fertilizer will require achieving an agricultural N balance where SOM-N mineralization accounts for the reductions in mineral fertilizer. This will depend on the amount and C:N ratios of inputs used in specific agricultural systems. We also note and will provide relevant citations that this may be prove especially challenging in smallholder systems where there is often lack of access to and insufficient quality of organic inputs (Giller et al., 2009; Palm et al., 2001).

L. 96 and methods. It is not clear why authors only used aridity and latitude as variables related to climate. Yields are strongly related to rainfall and temperature, which are easily available variables.

We chose to use aridity since it is a variable that is expressed as a function of precipitation, temperature, and potential evapo-transpiration. We will include this information and relevant citation in a revised manuscript (Trabucco 2009). We did initially include rainfall and temperature variables in our statistical model, but since they were highly correlated, we chose to leave them out and include aridity since it is derived from temperature and precipitation data. The use of aridity has been used in other large-scale yield studies (Pittelkow et al., 2014).

L. 121. More recent literature suggests that higher yield is not coming along with higher plant residue inputs (e.g., Hirte et al. 2018 Agriculture Ecosystems Environment 265).

This is a good point, and one we highlight in the Introduction. Specifically, previous work has found positive, negative, and no relationship between soil carbon and yields. Our work is not designed to resolve which of these patterns is correct because we believe that those site-specific relationships capture local realities. Rather, we are trying to capture global, average relationships that can help quantify the relationship between SOC and yield for broad-scale policy targets.

L. 141. Authors argue that two thirds of maize and wheat cultivation takes place on soils with less than 2 % SOC. What is, for comparison, the average % SOC of croplands worldwide? Are these two staple crops planted on particularly C-poor soils?

As our study focuses on two of the most important staple crops that are planted globally, we chose to focus on the SOC contents for maize and wheat. We can include the average SOC concentration on croplands worldwide in our supplemental file.

L. 160. Are the authors aware of any long-term field experiment where an increase from 0.5 to 2 % SOC has been observed? This seems unlikely to me. Even a doubling (previous sentence) is ambitious. The following argumentation, that higher SOM soils may supply enough plant available nutrients to sustain crop yields with drastically cutting back N fertilizer input overlooks that these are typical situations of SOM decline, as observed in many long-term experiments, where plant productivity can be maintained at low nutrient input rates only because of SOM decline and the associated release of organically bound nutrients.

This is a good point, and we recognize that building SOC from 0.5 to 2.0% represents a very large increase. Such an increase would require a significant amount of inputs that may not be feasible due to inherent and logistical difficulties related to soil properties, climate, and farmer access to inputs (lines 216-228). We will further stress the challenges associated with increasing SOC, while also highlighting experimental results recently published that show a range of annual increases in SOC for temperate agricultural soils (Poulton et al., 2018). The annual increases reported in this study range from 0.3 to 18% and are a result of a number of different inputs ranging from farmyard

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manure to mineral fertilization, some of which the authors of this study acknowledge may not be practical for farmers.

L. 193 ff. The first para in section 2.3. belongs largely to the method section and is partially a repetition of that.

We will revise this text in a revision so as to avoid repetition with our Methods section.

L. 215. It is not clear where the yield gap comes from – how was it calculated, was it taken from the literature? Clarification needed.

We will provide more context and the relevant citation for our yield gap analysis. Specifically, we are using a global data set (Mueller et al., 2012) that provides a global assessment of the difference between observed yields and attainable yields.

L. 302. Authors refer to Söderström et al. 2014. I looked up that reference where I could not find a database as key repository but rather a research approach. Should be clarified.

Thank you for pointing this out. Söderström et al. is another manuscript from this database effort, but we will cite the original paper in a revised version of our manuscript (Haddaway et al., 2015).

L. 352. I suggest to use three classes: rainfed, irrigated, unknown.

When extracting data, in cases where authors did not specify how crops were watered, we scored them as rainfed. We will revisit the papers and revise our data set to reflect this uncertainty.

L. 353. Filling data gaps for soil pH and texture for experimental sites by a global database may introduce large errors and, potentially, biased estimates, given that these soil properties vary much over short distances. I suggest to either exclude those variables as explanatory ones or to ask authors of the studies to provide those data for their sites. Alternatively, these parameters can be categorized and used as categorical variables.

We note that many of the studies were published prior to recent initiatives to deposit data products for published papers, making the kind of analysis we did additionally challenging. As such, we acknowledge that using values from a global database is not ideal and do acknowledge this as a limitation with our manuscript (lines 240-242). We did contact all authors for meta-data and raw data from their published studies, however, we only received data from three of the authors. As part of our original data exploration, we calculated the correlation coefficient for pH (r = 0.83) and soil texture (r = 0.61) between SoilGrids data and measured data from experimental studies in our data set. We also ran our regression model without texture and pH, and the coefficients on our model terms were essentially unchanged. We chose to retain these terms, however, because we believe that they do have established biological mechanisms as to their influence on yield. Furthermore, the range of both pH and percent clay data observed in SoilGrids reflects the range of data observed in our data set. Therefore, we believe that the relationships between variables are transferable between data sets even if the two data sets predict different values for the same place.

Table 2. I suggest to add a percentage increase in production from an increase in SOC to the table to make the global yield average and the increase in production comparable to each other.

This is a good suggestion and one we will include in a revised manuscript.

Figure 2. Not clear why the figure relates to maize and yield in line 1114 whereas the caption in line 1115 refers to maize only.

Thank you for catching this. We will revise the figure title to say, “Relationship between SOC and yield of maize for published studies.”

Figure 5. The figure is interesting but results would better be presented as percentage increase in yield, and not as percentage closure of yield gap. The yield gap itself is

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prone to large uncertainty, both in extent and possible reasons, and these uncertainties are not explicitly included.

When making our figures, we did create a map that featured percentage yield increase, however, it was difficult to visualize gains when presented at the broad global scale. We believe the yield gap map provides a clearer illustration of the areas that stand to gain the most in terms of identifying impacts of SOC on yield.

Figure 4. The provided interpretation of this results ignores the fact that building up additional SOC requires additional N.

This is a good point, and as we mention above, we will provide more discussion related to the challenges of building SOM/SOC, and that in may require the addition of inorganic N or organic N amendments.

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