

Interactive comment on “Hot regions of labile and stable soil organic carbon in Germany – Spatial variability and driving factors” by Cora Vos et al.

Cora Vos et al.

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Dear Dr. Smith, Thank you for reviewing our manuscript so thoroughly and taking the time to write helpful and detailed comments to improve our paper. We are very grateful for this. Please find our answers to the comments below:

Introduction

Overall, I think that the introduction needs some restructuring and needs more “meat” to it. Many statements are vague, blanket statement and don’t provide much insight or examples (e.g. “The effects of land use and management are not the same for all soil organic matter compounds...” How? Why? Give me more details). I think that the manuscript would benefit from a closer look at the flow and organization of

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the introduction. I suggest taking a close look at each paragraph; map out the main point, make sure this main point is reflected in the topic sentence, and verify that the preceding and following paragraphs fit/flow. There are a few paragraphs that just don’t fit (seem out of place) and it detracts from the main points of the introduction (which is essentially to build up to, i.e. provide background and rationale, the objectives and hypotheses of the study). As such, please align the introduction specific to the goals and objectives of the study.

Answer: We agree with the reviewer that in some cases more details need to be given in the introduction. We also see now that a stricter alignment of the introduction with our research goals would be helpful. We will follow this advice and restructure the introduction section in the revised version of the manuscript.

I strongly encourage the authors to reframe the objectives of the study as hypotheses in lieu of the somewhat vague research questions that are currently reported in the introduction. What do the authors expect the distribution of POM vs. MOM to be across Germany (and why)? Which factors (land-use, climate, soil type, clay content, etc.) do the authors expect to be more important in driving these distributional patterns? And the final question “can regions of high vulnerability...” needs to be clarified. First, I don’t know how you define “vulnerable” and second, I am unaware how you plan on verifying that your predictive approach (i.e. machine learning)

Answer: We agree that the third objective needs to be clarified and we will introduce the term “vulnerable” before and be more explicit regarding the methodology. However, we refrain to rephrase our objectives as hypotheses as the study design is not like in traditional studies that test different treatments for which a hypothesis is formulated.

Many of the statements or research addressed here are specific to European agroecosystems and yet the authors often make broad statements about land use and management effects on SOC as fact. However, land use and management effects on SOC differ greatly depending on cropping system, location (climate, topography, parent ma-

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terial, etc.) and there is often an equal amount of work that supports different results than what you present in this paper. As such, please be more specific and make sure to constrain postulations with “in temperate cropping systems...” or something to that example. I would be satisfied with a sentence early on stating that you are limiting the state of art (or body of knowledge) to your specific system (i.e. western European cropping systems).

Answer: The reviewer is right in that some statements in the introduction mainly refer to Western Europe and we will follow her advice and state this early on in the introduction.

As mentioned earlier, many sentences are vague. Please try to be more specific and detailed when building up the background and rationale in the introduction. There is more “telling” than “showing”. Please see the attached line-by-line review.

Answer: Thank you for uploading the commented version of the manuscript. We agree that the revised version of the introduction must be more specific and detailed and will change it accordingly.

Methods

Overall, I suggest reorganizing the methods section to be more aligned with your objectives. This is especially true when it comes to the use of calibration versus all samples. Sections often jump from calibration to all and it makes it a bit confusing. There also needs to be more technical details into how soils were collected and processed (e.g. collected with a corer, composite samples, one sample per depth, homogenized, dried, etc. ?). Replication need to be explicitly stated (how many samples did you use for each classification combination – i.e. land use, or depth, etc.). Including a supplemental table that lists all the samples/sites or something may help clear this up. There are also several areas where the methods need to be more explicitly stated and many instances where citations are needed. Please see attachment for line by line comments.

Answer: We can see that the methods section can be confusing for the reader in the

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present stage and we will revise and improve it in the revised version. More details on the soil sampling and handling will be included and methods will be described in more detail.

Calibration samples versus all: The experimental design (use of calibration sites versus all sites) needs to be clearer. It was confusing with the way the methods section was organized for the reader to understand why/what/how calibration samples were used as compared to all sites. Perhaps have a separate calibration section in the methods where all of this is addressed would be clearer.

Answer: We agree that a separate calibration section is a good idea to clarify the methodology. We will restructure the methods accordingly.

A major issue I have with the methods is combining the oPOM and fPOM fractions together as a “light fraction.” As much as I hate to ask authors to redo their analyses, I think that the best way to deal with the oPOM is to either ignore it or analyze it separately.

Answer: We agree with the reviewer in the point that fPOM and oPOM are not the same. We have, however, good reasons to combine them into a light fraction for the purpose of prediction: - The oPOM fraction generally constitutes only a very small part of total SOC (Mean: 4%). Thus, it is very hard to predict this fraction separately via NIRS. We tried it as a first step but calibration results were very poor. This is why we do not treat oPOM separately from fPOM. - We do, however, not want to ignore the oPOM fraction completely for the following reason: The novelty in the prediction of C-fractions via NIRS consists of using the log-ratio transformation to ensure that the carbon content of both fractions adds up to 100% of the total carbon content of the sample. Therefore, we cannot omit the oPOM fraction since it would be unclear to which value the fPOM and MOM fraction should add up.

Results

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Please review my comments in the attachment and address them. Most importantly, I do not agree with using total SOC to explain fraction SOC. Of course, C would explain C. Total SOC is NOT a driver – it is a response variable for this study.

Answer: We will address the helpful comments in the results section in the revised version of the paper. We do, however, not agree that the SOC content is merely a response variable in our dataset.

The question needs to be answered whether the light and the stabilised fractions are regionally so variable that they require a separate analysis and cannot be predicted from the total SOC content. If total SOC content is a strong predictor for the fractions we could easily build a model to predict fractions from total SOC and do not need fractionation work. It is important to check whether and which of the fractions are closely related to total SOC, as this implies a higher relevance of this fraction for the total SOC content of the soil. For example, our results show that total SOC is much closer related to the light fraction in the black sands than in the other soils where texture is a more important driver for the distribution of the fractions. You are also missing any reference to Fig. 6 and Fig. 8 in the results! If you don't use them – don't put them in the manuscript (or put them in supplemental).

Answer: Thank you for noticing this. We will include these references in the revised version of the manuscript.

Discussion

I would almost reorganize the discussion to be more explicitly aligned with the study objectives – first discuss the how SOC is distributed among fractions at a national scale, then discuss which drivers are relevant and finally end with whether or not you can predict “vulnerable” (but please define) areas using your approach. Section 4.1 is entirely too brief, especially since it supposedly addresses your first objective. Again – don't just tell me what other results support or do not support your results, show me!

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Answer: We agree that section 4.1 should be more detailed and should show more results of other studies. We refrain, however, from restructuring the discussion as proposed by the reviewer for the following reason: In our first draft version, the discussion was structured exactly as proposed by the reviewer. There we encountered the problem, however, that there were alternating parts about black sands and “normal” soils which forced us to repeat the same information over and over. We therefore decided to structure the discussion into a “black sands” and a “normal soils” part.

You have a great discussion on the “black sands” section. I would love to see that reflected in the entire discussion section. Some of the details I was looking for in section 4.1 are included in 4.2. I think it would be good to combine section 4.1 and 4.2 (and address your first objective) and discuss black sands in the context of objective 1.

Answer: We agree that it would indeed be a good idea to combine these sections in the revised version.

In section 4.4, it would be great to discuss why/why not you think your approach worked to identify vulnerable areas. It is one of your objectives and you do not directly discuss it in the discussion. It needs to be addressed. I think concluding section 4.4 with a paragraph answering “Can regions of high vulnerability to carbon losses be identified by this predictive approach?” is warranted.

Answer: We also agree with this proposal and will enhance the discussion of our third objective accordingly.

Conclusion See notes regarding final sentence. I believe that with a few revisions (as per my and other reviewers' suggestions) this manuscript is publishable and I look forward to the revisions!

Answer: We will reformulate the last sentence to make it more specific in the revised version.

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2017-30>, 2017.

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