SOIL
Discussions

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Interactive comment on "SoilGrids 2.0: producing quality-assessed soil information for the globe" by Luis M. de Sousa et al.

Luis M. de Sousa et al.

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Thank you very much for your useful comments. Below you will find detailed answers.

> I commend De Sousa et al. (2020) for their progress on the SoilGrids soil mapping project. The authors must be lauded for communicating the performance of their machine learning-based mapping of soil variables in various depths. In my opinion, the authors should address a couple of concerns I have with their approach before the manuscript is published in SOIL.
>
> 1. De Sousa et al. (2020) have predicted SOC content, bulk density, and coarse fraction volume. These are the ingredients to predict SOC stocks

C1

> (kg m-2) which is arguably the metric most people are interested in. Is there a reason why SOC stocks are not presented in this manuscript? Is the modelling efficiency too low to calculate SOC stocks with good conscience (median modelling efficiencies of 0.37 for SOC and 0.22 for coarse fraction volume)? It should be communicated with the reader if the machine learning model is good enough to calculate SOC stocks.

We are in the process of preparing a manuscript that will discuss and compare global estimates of SOC stocks. That study will derive SOC stocks from the point observations themselves, not from the various data layers. As such this topic is beyond the scope of the present paper.

> 2. In the ISRIC FAQs there is a section on SOC stocks: https://www.isric.org/explore/soilgrids/faq-soilgrids#How_where_SOC_ stock_maps_generated. In my opinion, this should not be in grey "literature" but be discussed in this paper. What are the SOC stocks globally in SoilGrids version 2017 and SoilGrids 2.0? Why do they differ? A discussion of both approaches would be helpful.

We find this a somewhat surprising comment as you refer to the FAQ section on our website. As indicated earlier, we are preparing a separate manuscript on SOC stocks, discussing differences in methodological approaches. This paper would have been too large to include all the modelling steps and decisions necessary for the production of global soil carbon stocks. Therefore we decided to focus only on primary soil properties as defined by the GlobalSoilMap specifications.

> 3. In the paper, it is stated that "'Litter layers' on top of minerals soils were excluded from further modelling". This leads to a severe underestimation of

C2

SOC stocks. Apart from changing conventions in soil science, why should 'litter layers' be excluded? Peat layers, however, are included? In my opinion, organic layers should be included in the training of the machine learning model. Alternatively, the authors could provide estimates on an extra litter layer.

Thanks for the useful suggestion. This is an element we are considering in the SOC manuscript, currently under preparation.

4. In the FAQs, it is also written: The organic layers on top of mineral soils were removed from the calculations and models. The total global carbon stocks obtained with version 2 (599 Pg of carbon for 0 to 30cm) are more in line with other global estimates (see for example: Jackson et al, 2017, Table 2 and Scharlemann et al, 2014). Is the inclusion of the organic layer the reason for very high SOC stocks in SoilGrids 2017?

Again, you are referring to the FAQ pages on our website. The present paper is not a comparison of the SG2017 and SG2020 predictions. Possibly, we may consider this intriguing aspect in the upcoming SOC manuscript, mentioned above.

5. The cross-validation procedure between SoilGrids 2.0 and SoilGrids 2017 has changed. If I understand it correctly, in SoilGrids 2017 10-fold-cross-validation on individual datapoints was used while in SoilGrids 2020 10-fold-cross-validation on individual profiles has been used. Is this correct? In my opinion, the difference between the previous and current approaches should be discussed.

The reviewer is right that we modified the k-fold cross-validation procedure by ensuring that all observations belonging to the same profile were in the same fold. In addition,

we did not randomly assign profiles to folds but applied a strategy that enforces that the folds are spatially balanced. We modified the text in the manuscript to include additional details:

Profile locations were stratified into a quasi-regular partition of the Earth's surface. An Icosahedral Snyder Equal-Area Grid (ISEAG) of aperture 3 and resolution 6 was created for this purpose with the dggridR package for the R language (Barnes et al., 2016). This grid consists of 7 292 cells (mostly hexagons) of an average area close to 70 000 km2. Each observation was assigned to the ISEAG cell (stratum) in which the corresponding soil profile is located.

The observations assigned to a spatial stratum were distributed evenly among the ten validation folds using the caret package for the R language. In this way each spatial stratum contributed a similar number of observations to each of the validation folds. All observations were kept in the same fold for both model calibration and evaluation.

6. Ploton et al. (2020) have recently shown the importance of proper spatial cross validation. In their case, they assessed the effect of spatial autocorrelation by two approaches. On the one hand, they constructed spatial folds on clustered observations; on the other hand, they used a spatial blocking approach. From the method section, it is unclear if a spatial cross-validation was performed. "Balanced spatial distribution within each validation fold" – does this mean that every fold has just the same spatial coverage but were geographically close profiles possibly mixed for training and crossvalidation? I would recommend a schematic drawing on how the cross-validation folds were constructed to make it clear to the readers of SOIL, especially those who are not familiar with digital soil mapping

As explained in our reply to your previous comment we explained the spatially balanced cross-validation approach in more detail in the revision (but we prefer to not include a schematic drawing). Our approach does not belong to the class of 'spatial cross-validation' techniques as used in Ploton et al. (2020). The reason that we did not use such technique is that we think these methods have no theoretical underpinning and produce biased (i.e., overpessimistic) validation results. In fact, one of the authors has a 'Matters Arising' paper under review that addresses the misconceptions of the Ploton et al. (2020) paper.

> 7. In Figures 4 and 5, the spatial coverage of some training data is shown. It seems like for many variables global digital soil mapping is still "Predicting into unknown space" which is part of the title of a paper by Meyer and Pebesma (2020). That paper deals with mapping the area of applicability for machine learning-based digital mapping projects. There is an R package associated with it https://github.com/HannaMeyer/AOA_CaseStudy. It would be great if an area of applicability maps could be provided for every soil variable presented.

Thank you very much for raising this important point. We are aware of the paper from Meyer and Pebesma (2020). We realise its implications for DSM, especially when spanning across large areas with varying densities of input observations. We are exploring this approach for future improvements of the procedure. We added a mention to these issues into the discussion of the paper.

> 8. Observation depth was included as a covariate in the machine learning model. How much of the explained variance is explained by soil depth for the different variables? Additionally, a discussion if the 3D mapping approach may lead to biased estimates due to the stepped nature of tree-

> based method would be helpful. Here is a relevant paper that should probably be discussed here: Predicting soil properties in 3D by Ma et al. (2021).

We are aware of the limitations of using depth as covariate (2.5 DSM) described in the recent studies mentioned. We think using depth as covariate is a practical approach in global modelling. We absolutely agree that further research is needed to assess the implications especially when using global legacy datasets with varying densities. See also our reply to a comment by RC2. We have modified the text to discuss this point:

> In this study, the vertical dimension of soil variability was only taken into account by using the depth of the observation as a covariate (so-called "2.5 DSM"). Recent publications (Ma et al., 2021; Nauman and Duniway, 2019) indicate that such approach can be too simplistic or lead to problems with consistency over the predicted depth sequence. This may be true for local datasets where the short-range spatial variability is of a similar magnitude as the vertical variability. Further research is necessary to assess the effects of using depth as a covariate on global datasets and models. Alternatives such as 3D smoothers (Poggio and Gimona, 2017) or geostatistical models exploiting 3D spatial auto-correlation are worth exploring in further studies.

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