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Interactive Comment

Interactive comment on "Estimating soil organic carbon stocks of Swiss forest soils by robust external-drift kriging" *by* M. Nussbaum et al.

Anonymous Referee #3

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The present paper propose an estimation of Soil Organic Carbon (SOC) stocks for Swiss Forest Soils (map and mean estimates), obtained using a robust variation of external drift kriging, and more specifically block kriging for deriving means. This block kriging approach is particularly rare in the field of SOC stocks distribution modelling at the regional or national scale. The topic is of the utmost importance since, as clearly stated in the introduction, such estimates are needed in Greenhouse Gas emissions reporting and can be used as baselines for future work. It seems suited to the GMDD journal since it falls within the geosciences field and present a statistical model of one component of the earth system. This paper surely needs to be published so that the content of this excellent work can be presented to a large audience, and the methods can be conveniently reused.

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Interactive Discussion

Discussion Paper

The methods and assumptions are valid and clearly presented (see however the discussion about the independent data used for the validation) and the results sufficient to support the interpretations and conclusions.

The description is sufficiently complete and precise to allow the application of the proposed methods on other datasets. Additionally, some of the robust methods presented here are published through a R package "georob" available on the CRAN. This i) demonstrates the maturity of the code and ii) makes it possible for the interested reader to have a look at the source code. The package includes the validation metrics used to assess the model quality and also maybe of help in order to understand some of the new (to the pedometricians) metrics such as the CRPS. I had a very positive impression while reading this paper, from the abstract to the conclusion as it is well structured and overall clear. As far as I could tell, the language was precise and accurate. All figures and tables or equations of the paper are needed for clarity purposes. Just one comment: since the need for taking into account the spatial autocorrelation of residuals is an important point of the paper, having variograms displayed would be useful to the reader and more directly demonstrative than nugget/total sill ratios (p.7095 l.19). The number and quality of references was appropriate. Only Minasny et al. (2013) is missing. This reference provides the most recent comprehensive review on SOC mapping. The amount and quality of supplementary material is appropriate, and as mentioned above, is completed by the R package georob. However, some questions need to be addressed before the paper can be published.

One general concern was about the brief description of validation procedures provided in the introduction, which I found misleading, and the concept of independent validation introduced there and later reused in the text. There are several ways to test the precision of SOC estimates. From what I understand l.1-8 p.7081, the authors distinguish between: validation on independent data and cross-validation which is said to be better than goodness of fit R^2. I agree with this distinction if independent data refers to this: data, which has been sampled, independently from the sampling campaign producing the dataset on which models are fitted. Ideally this validation dataset is sampled using a probabilistic scheme in order to yield unbiased estimates of errors. If the independent validation set is simply obtained using data-splitting, as here (with some constraints, related to the number of individuals in soil map units and vegetation types), I would disagree with the distinction made between cross-validation and validation on independent data, l.1-8 p.7081: as explained by Brus et al. (2011), "The difference between this" (n-fold cross-validation) "and data-splitting is that in cross-validation the splitting is repeated, which makes it more efficient than data-splitting". In other words, Meersmans et al. 2012b and Martin et al. 2011 performed validation on independent data too, sensu the validation scheme of the present paper; they should not be opposed, at least on the basis of the independence of the data used for validation, to Kumar et al. 2012 and Wiesmeier et al. 2011, who used data-splitting and who are cited l.3 p.7081 to exemplify validation against independent data. Thus i) I think that it would be adequate to avoid mentioning validation against independent data and merely quote consistently throughout the paper that validation was done by data-splitting *or*, l.1-8 p.7081 to clearly state that cross-validation can be, as simple data-splitting, viewed as validation on independent data, although validation on "truly" independent data (purposively sampled or during another sampling campaign, see above) may be preferable. ii) Since it is known that cross-validation is superior to simple data-splitting, why did the authors use data-splitting instead of cross-validation (although it was used for model building)? Following these considerations about validation, in my opinion, the authors should comment more about the validation of their estimates against the data from the Swiss soil monitoring networks (l.1-2 p.7097). This dataset is a "true" independent validation data set (as defined above), and as such is rare and valuable.

The use of robust estimators of bias and RMSE is fairly new in the field of SOC mapping. The added value of this, and of the even newer CRPS index, should be discussed in the discussion section: SOC distributions are often log-normally distributed, and these robust indicators seem suitable in that case.

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As mentioned here (p.7095 l.18), there was limited spatial structure in the residuals. This is often the case for statistical models of SOC distributions, assuming that relevant covariates are included in the models. Could the authors give the reader more insight about the error that is done when using aspatial models, such as linear regression models, without a geostatistical component? As studies dealing with regional or national mapping, for IPCC reporting for instance, are becoming more and more common, one would benefit on guidance whether simpler linear models are enough or if the spatial modelling of the residuals is needed. The sentence p.7098l.6-8 comes without quantification and do not really demonstrate the importance of taking into account autocorrelation.

Lastly, could the authors comment on the advantages of the LASSO procedure compared to other more common procedures (for instance stepwise regression and the AIC)?

Specific comments:

p.7080 l.23 : Can the authors clarify the problem arising when using a "common model" in case residual autocorrelation is weak? Additionally, what are the solutions for ML models where residuals autocorrelation often cannot be analytical derived?

p.7080 l.26 : The personal communication of Martin et al. 2013 did not include an estimate of the standard error of estimated mean stocks. Instead, it included geostatistical modelling of residuals autocorrelation and the validation (using cross-validation) of the resulting models.

p.7081 l.28 : Please briefly explain why the used procedures are robust.

p.7082 l.19 : Insert "being" before "digitized".

p.7082 l.25 : Maybe could you mention the fact that modelling SOC distribution in forest soils is known as being more difficult compared to non-forest soils, because of a higher spatial variability. This difficulty also explains why we need to address this question

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separately.

p.7083 l.11 : Does not the 29% value given here contradict the 45.5% given the page before (Hotz et al. 2005)?

p.7085 l.5 : “as in many other studies” . Please provide references.

p.7099 l.18 : Jalabert et al. 2009 provided models with RMSPE equal to 0.168g.cm-3, which is better than PTF used in the present paper. This could be mentioned as a way to minimize the additional variation added to the data when estimating soil densities.

p.7087 l.5-15 : if I understand right, some covariates, attached to the geotechnical units, are estimated using the full dataset (i.e. before data splitting). Thus, indirectly, data having been observed on profiles later left apart for validation is used for fitting the models and for validation at these left apart points? Is it not possible then, that the model performance, assessed on these left-apart profiles is in turn over-estimated?

p.7094 l.6-9 : The CRPS (and the PIT) measure is fairly new to pedometricians and looks very interesting. Could the authors give more information about its properties, if needed giving examples? Furthermore, I could not understand the sentence l.7-9.

p.7095 l.20-25 and section 4.3: looking at CPRS results, it seems that the robust treatment has virtually no impact on models quality (CPRS=0.239 and 0.238 with or without the robust treatment for the 0-30cm layer ; 0.253 and 0.252 for the 0-1m layer), despite the fact that SOC stocks dataset did include outliers. Could please the authors comment on that? Does it mean that the CPRS index not sensitive enough to changes in model performance?

p.7096 l.9: please remove (-non) before robust or rephrase the sentence.

p.7099 l.2-9 : (robust)R^2 alone is not enough to assess the prediction performance of models, and that is why the authors here adequately use a great number of different measures. The same applies to comparisons among SOC stock mapping studies, and consequently it would be particularly interesting to compare the performance of the present models to those of the literature using MPE and RMSE too and not only R2 as it is done here.

Appendix : missing hat θ index for Z and β in eq. 3

Minasny, B., McBratney, A.B., Malone, B.P., Wheeler, I., 2013. Digital mapping of soil carbon. Advances in Agronomy 118, 1-47.

Brus, D.J., Kempen, B. and Heuvelink, G.B.M. (2011), Sampling for validation of digital soil maps. European Journal of Soil Science, 62: 394–407. doi: 10.1111/j.1365-2389.2011.01364.x

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Interactive comment on Geosci. Model Dev. Discuss., 6, 7077, 2013.