

Interactive comment on “Evaluation of digital soil mapping approaches with large sets of environmental covariates” by Madlene Nussbaum et al.

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Many thanks you for your helpful feedback. We comment on your review in the subsequent text. Please further consider the our suggestions for changes of the manuscript in the supplement to this document.

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Discussion paper



Spatial distribution of prediction uncertainty

Having spatial estimates of model performance is an important aspect of DSM. Even though the authors note the computational demands as the reason for not including these, I think it is possible to create at least one map of performance. I have computed similar model performance for larger study areas than those presented here with a smaller pixel size using a PC with 32 GB of RAM.

Comment on page 24: This is an important part of the modeling routine that is not presented here. It would be helpful to include some maps of uncertainty associated with each of the methods. This would be especially important for evaluating the maps shown in Fig. 8. Is it possible to calculate uncertainty for the model averaged result?

Undoubtedly, reporting uncertainty of predictions — e. g. publishing maps of limits of 90 %-prediction intervals — is important for digital soil mapping products, and evaluating the empirical coverage of such intervals with independent validation data is crucial. We therefore presented an elaborate evaluation of model uncertainty with independent data for one response in the main part of Nussbaum et al. (2017), along with a map of the width of 90 %-prediction intervals in the Supplement to that publication.

However, in this article, we refrained from adding similar information for just one or a few responses because this would not give the full picture of the performance of the various methods. Computing and evaluating the quality of predictive distributions for all 48 responses and 6 methods would certainly have been of interest, but it was not feasible because of substantial computational load. Group lasso, geo-additive models (geoGAM), boosted regression trees (BRT) and model averaging (MA) require a model-based bootstrapping approach, detailed in Nussbaum et al. (2017). Only quantile regression forest ([Q]RF) and robust external-drift kriging (EDK) directly provide information on prediction uncertainty. Hence, complete evaluation of prediction uncertainty would have involved 96 000 model fits when we use 500 bootstrapping repetitions

per response and method. In Nussbaum et al. (2017) we used even 1000 repetitions to ensure stability of the results.

Legacy data correction

One of the most interesting findings in this paper was the successful implementation of the legacy data correction for the timing of sample collection. While this is noted in the conclusions, I think it should be more prominent throughout the paper, as this is a common issue of using legacy data and most papers do not address it.

We agree that accounting for temporal variation in legacy soil data is important. But we refrained from emphasizing this aspect in the manuscript for two reasons:

Firstly, we were not able to correct the data to our full satisfaction. We lacked a sufficient number of repeated measurements to truly account for changes in soil properties over time (this was also remarked by referee 2). Furthermore, the periods we used for grouping the observations did not consist of well-defined sampling campaigns because the campaigns partly overlapped or some of the samples were collected under different protocols (research projects, monitoring programmes). Meta-information on sampling and lab methods could not be gathered for all samples. Thus, we were not able to fully compensate methodological differences. Our chosen strategy accounts for all this variation only in a “batch-like” manner.

Secondly, accounting for temporal variation in legacy soil data is very specific to a particular dataset. It is difficult to generalize respective approaches. The strategy proposed in our manuscript could possibly be used with other data sets, however we recommend to develop more specific procedures that are better tailored to the characteristics of a given data set.

Answer to further comments in the manuscript

We address most of the specific comments of referee 1 in the supplement where we suggest changes of the manuscript. Here we respond to some comments which seemed of more general interest.

Soil density (comment on page 7)

I'm not sure why soil density should be included in the calculation of each property for a given layer. Also, is density referring to bulk density? It is not clear. It may be easier to say that property values for a given depth increment were calculated as a depth-weighted average.

According to Swiss soil classification “density” refers to the fine soil fraction ≤ 2 mm (we will clarify this in the revised manuscript, see attached supplement). Accounting only for layer thickness (soil depth) is not sufficient when converting soil data from horizons to fixed-depth layers. The mass of soil per depth increment, i. e. the density of the soil, must be considered as well. For example, to assess the acidification status of a soil profile a denser horizon contributes a larger mass of fine soil with a critical pH than a looser horizon.

Resolution of predictive maps

The modeling resolution should be more clearly stated in the methods. The caption of Figure 8 is the only place that I found the model resolution mentioned (20 m)

We agree and suggested to add this information in the methods section (Section 3, P8 L26): "To create the final maps we predicted each response at the nodes of a 20 m grid."

Soil texture — separate model for *sand* (comment on page 10)

Why not model sand independently too? As the reminder, it is subject to the compound model errors of both silt and clay.

You suggested to model *sand* content separately instead of just computing it as the remainder of the sum of *clay* and *silt* content to 100 %. We agree that it would be nice to predict *sand* content with meaningful estimates of prediction uncertainty. Nevertheless, we refrained from separately modelling *sand* content because a substantial part of the soil texture data were field estimates by soil surveyors. For field estimates, sand content is computed as the remainder of the sum of the estimated clay and silt content to 100 % (Brunner et al. 1997; Jaeggli et al. 1998). Furthermore, soil function assessment (Greiner et al. 2017) relies on clay and silt content as input. Hence, uncertainty assessment for soil functions using soil texture data does not depend on predictive distributions for *sand* content.

Interpretation of vegetation map (comment on page 20)

This is an important finding that isn't really discussed. A brief explanation of the influence of the vegetation map on the soil properties would be helpful. How much variability of vegetation types were present in this study area? deciduous or coniferous trees? Do the relationships make sense for properties like pH, ECEC, etc.?

The vegetation map (1:5 000, Schmider et al. 1993) was very detailed, and we had to merge mapping units to obtain a reasonable number of observations per unit. Interpretation of the modelled effects would therefore be challenging. But we refrained in general from interpretation of covariate effects as this would make an already quite long manuscript even longer. As an aside: Nussbaum et al. (2017) discussed briefly the role of the vegetation map as covariate for predicting topsoil effective cation exchange

capacity (ECEC). The modelled coefficients for vegetation map units were generally in accordance with pedological knowledge.

Multi-flow catchment area (comment on page 22)

I would think a multi-flow algorithm would have more smooth values. As opposed to a single flow algorithm like deterministic 8 used in ArcGIS.

A multi-flow topographic wetness index algorithm (Tarboton, 1997) results in a smoother surface (less sudden changes between neighbouring pixels) over most of the terrain than a single flow algorithm. In locations where the flow accumulates, however, upstream catchment area is larger when computed by multi- than single flow algorithms. The flexibility of the multi-flow algorithm allows the flow to actually drain to the lowest point in the terrain. Hence, the frequency distribution of multi-flow catchment areas has a fatter upper tail. This may cause extrapolation errors when predictions are computed by a parametric approach such as EDK.

Spatial coordinates in final models (comment on page 24)

Were these selected for the final models?

Spatial coordinates were included in lasso models for 20 out of 48 responses. geoGAM selected a smooth spatial surface for 5 responses only. Spatial autocorrelation modelled by EDK was generally rather weak judged by the ratio of nugget to sill, but differed for the study areas and responses (see page 13, line 31ff and Tables S10 and S11 in the Supplement to the manuscript). For RF (as well as for BRT with similar covariates importance) spatial coordinates were relevant, but covariates from other thematic groups showed clearly larger predictive skill (see Figure 6 in manuscript).

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Please also note the supplement to this comment:

<https://www.soil-discuss.net/soil-2017-14/soil-2017-14-AC1-supplement.pdf>

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2017-14>, 2017.