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Interactive comment on "Planning spatial sampling of the soil from an uncertain reconnaissance variogram" *by* R. Murray Lark et al.

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General comments

This paper presents a sound approach to planning geostatics-based sampling campaigns based on a small initial set of observations. It shows clearly that conventional methods are expected to seriously underestimate the uncertainty in a proposed second-stage grid sampling. The use of Bayesian methods is appropriate and well-

presented. The paper is a nice mix of rigorous mathematics, well-explained, and the implications for practicioners, as exemplified by the final section of the Conclusions. The expositon is exceptionally clear. Readers need a basis in model-based geostatistics, for which the relevant references are given.

There is a large difference between the required grid spacings as computed by the conventional method (1.4 samples per ha) assumed correct variogram) and the presented method (5.12 samples per ha at 0.8 probability, and even 3.13 at 0.7 probabiity, i.e., still more than double the sampling effort). This is not surprising given the small number of observations, and is well-discussed, but could be highlighted more. The message is that anyone using variogram models estimated by conventional methods from small samples to plan a sampling campaign is seriously underestimating the uncertainty and will likely end up with unacceptable prediction variances. Perhaps this message could be emphasized.

Specific comments

The restriction to regular grid sampling is not explained. Perhaps it could be mentioned that this applies when there is no parts of the target area for which different levels of kriging prediction variance are required.

The presented method assumes first-order stationarity of the target. P3L26 "The assumption of a stationary mean seemed plausible..." is not well-supported. Was this from Table 1, which just shows that the mean and median are similar over the whole dataset? Or from examination of Fig. 1 which does not show any obvious trend? Or from fitting an exploratory trend surface and discovering a poor fit compared to fitting a spatial mean?

Perhaps the authors can briefly mention adaptations that would be needed in the non-stationary case. Would a mixed model first be applied to remove the trend and then the presented method be used on the residual variogram? Or would the parameters of the mixed model also be incorporated into the Bayesian analysis?

P3L9 the use of FAO-Unesco 1974 soil classification is obsolete; I presume this was used because the mentioned exploratory soil map uses it. The named classes have not changed much if at all in the successor World Reference Base (WRB), so this is not a problem for understanding this paper.

P5L17 "These latter two priors were judged to be acceptable..." good, but how were they selected initially? Why the prior range parameter of 1000 m and total variance of 10? Presumably based on expert knowledge of the maximum possible range and total variance under the circumstances, so that larger values need not be considered. But, looking at Fig. 1(b), it's clear that the effective range can not be more than about 200 m (which we see confirmed later in Fig. 3(a)). So why such a large prior upper limit? Similarly, for the total variance parameter \sigmaˆ2, Table 1 shows a standard deviation of 0.6 mg kgˆ{-1}; this squared is only 0.36 mg kgˆ{-2} and indeed Figure 3 (a) shows almost no density past 2 mg kgˆ{-2}. Knowing the statistics in Table 1, why was such a large prior upper limit for the total variance selected? Does this not slow down the MCMC algorithm?

The choice of k=2 from the profile likelihood leads to a higher nugget proportion than the almost-identically likely value 1.5, and even 1 is not much less likely. This in turn leads to higher estimates of kriging prediction variances.

Technical corrections

L3 "a decision on sampling intensity which is robust" the 'which' could refer to either the decision or the intensity; suggest rephrasing "a robust decision on..." Or, if it's the sampling density which is robust, in what sense is that?

Lark et al. (2017) in the text is likely Lark, R.M., Hamilton, E.M., Kaninga, B., Maseka, K.K., Mutondo, M., Sakala, G.M. and Watts M. J.: Nested sampling and spatial analysis for reconnaissance investigations of soil: an example from agricultural land near mine tailings in Zambia, European Journal of Soil Science, 68, In press. This has no year in the reference list.

No year is given for Vrugt J. A.: Markov chain Monte Carlo simulation using the DREAM software package: theory, concepts and MATLAB implementation, Environmental Modelling & Software, 75, 273–316. The text reference implies 2016.

DOI for all entries in the reference list would be useful.

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