Review of: "Spatial Analysis of Soil Fertility Using Geostatistical Techniques And Artificial Neural Networks"

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Potential competing interests: No potential competing interests to declare.

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I reviewed this manuscript intitled “Spatial analysis of soil fertility using geostatistical techniques and artificial neural networks” and found it well written and well organized. It deals with a subject of major importance in relation to the management of soil resources and the assessment of their degree of fertility via new technologies of data acquisition, processing, and analysis. The importance of this work comes from the positive impact of such knowledge on making correct decisions, which are the key to the success of valorization programs for lands suitable for agriculture. However, the reader of this work may feel a fairly thorough and rich statistical analysis, but still may ask such legitimate questions. See the comments below:

Comment1. The direct scientific objective of the research should, perhaps, be clarified: is it an application of the geostatistical method based on the FNKH model and an assessment of its usefulness? Or is it a methodological optimization, where the authors try to make an improvement to the method in use? The two situations are completely different!

Comment2. The “extended” address of the “Agronomy” test site is repeated several times. Isn't it convenient to eliminate repetition? Comment3.

The choice of the site locality is not discussed. Does it have a specification or particularity? the same remark is made for the surface chosen for the test site.

Comment4. The use of different models for the semivariogram is not clearly justified. It would be interesting to argue the choice of each model. It is, perhaps, useful to link the choice with the sources of the elements encountered in the soil. The distribution of the elements constituting the soil must be discussed in relation to the alteration processes of the mother-rock and to other possible sources.

Comment5. Regarding the major fertility elements (N, P and K), the nitrogen element is not included among the elements measured. It is desirable to discuss this point.

Comment6. For the accuracy of the method, the model adopted, resulting from the geospatial analysis, is validated - according to the authors - using discriminant functions in particular (canonical discriminant analysis). In fact, the problem with this type of validation using such functions is that the “datasets” generated are closely linked to the basic inputs.

Whereas it is more robust to validate the model through a reliable and independent database. The discussion section can be enriched by mentioning this point.

Comment7. About the results. Soil fertility classes are essentially distinguished by particle size, phosphorus content mainly, and Ca and Mg secondarily. The OM content is mentioned in classes 4 and 5, while the soil acidity shows a reduced interval of variability, compared to what may exist if the test site is larger. Here it is difficult to determine the relative degree of fertility across classes. Example: Can class 1 soil be considered more fertile than class 5, or the opposite? or maybe class 4 is the most fertile. The notion of fertility risks becoming vague and having more than one meaning!

Comment8. About the applicability of the approach elsewhere, and especially for larger scenes. The condition achieved in this case study, i mean having a reduced scene but a relatively dense population of sampling points, makes the use of the “ordinary kriging” interpolation method possible and acceptable. But the method poses a problem for large sites with distant observation points.

Finally I hope I was able to bring up a few points to improve the final version that I hope will appear soon.