

Interactive comment on “Quantifying soil and critical zone variability in a forested catchment through digital soil mapping” by M. Holleran et al.

Anonymous Referee #2

Received and published: 20 June 2014

General Comments and recommendation: The paper aims to quantify soil variability at local scale. As already mentioned by the authors, “Developing robust data-driven methods that provide accurate, reliable, and high-resolution characterization of soil properties is a major challenge to earth scientists and is needed for better understanding and quantification of critical zone process and function such as soil erosion, hydrologic cycling, and carbon cycling”. That is the exercise led by the authors at the local scale in a forested environment. Because most of previous studies using similar approaches were performed at larger/meso scales, it sounds very good to narrow (spatially and methodologically) and compare.

The paper is within the scope of the Journal. It is well written and understandable. It's an ambitious implementation of a combination of methodological options consistent

C17

with the state of the art and thorough field investigations/laboratory analysis/data analysis that should be seen as a significant contribution to knowledge improvement. Clear issues were drawn from the results to address the objectives so the key-findings stated in the abstract/conclusion and through the manuscript are justified. In this point of view one may strongly support this study for publication. Nevertheless, clarifications should be provided concerning a very few aspects.

Page 2: lines 7 – 9. Maybe it's better to delete “with a mean elevation of 2400 m a.s.l., mean annual temperature of 10 C and mean annual precipitation of 1185 cm yr⁻¹”.

Page 2: line 25. “Scale of” instead of “scaleof”.

Page 4: lines 16 – 19. I would recommend reformulating that sentence, since it gives the impression that the authors are already reporting on their own results. The jump from the preceding paragraph to that sentence strengthens this impression.

Page 6 line 25 to page 8 line 23. Why not summarizing all the environmental covariates/derivation sources/tools used as a table to better help the readership?

Page 16: lines 1 – 25. Please avoid mixing equations and text.

Page 38: Table 2. Please provide short explanations for “F ratio” and “P value” in the caption.

Page 39 & 40: Table 4 & 5. a. Please provide a full list of the environmental covariates with their PC dependence values. b. It would be great to have the same list of environmental covariates in both table 4 and table 5. c. Why not providing a symmetric Pearson correlation matrix with the same list of variables in the rows and columns. d. Please provide in the same table (in the empty space) the significance of the correlations e. Could the authors explain why significant correlations (values in bold) range from so low values (e.g. 0.09) to so high values (e.g. 0.93) and why many other values in between are not significant

Specific comments:

C18

It's quite good that the authors kept low (<5) the variance inflation factors associated to each independent variable in the established regression models (reference to the table 6) to ensure nonlinearity status, but prior to that the Pearson correlation analysis revealed a significant correlation between Mod_Depth and Wet_Ind for instance which were the most frequent independent variables appeared in the regression models. This correlation is also mentioned in page 20 line 25 – 28: "Environmental covariates exhibited relatively few significant correlations with each other, with the strongest correlation of $r = 0.73$ between modeled soil depth and wetness index". So what about the variable inflation factor between Mod_Depth and Wet_Ind in term of significance of this correlation? My understanding is that high inflation between Mod_Depth and Wet_Ind may indicate that only one of them should be selected and involved into the regression analysis.

Interactive comment on SOIL Discuss., 1, 1, 2014.