

## ***Interactive comment on “Spatial variability of some soil properties in west coastal area of India having oil palm (*Elaeis guineensis* Jacq.) plantations” by Sanjib K. Behera et al.***

**Anonymous Referee #1**

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The manuscript report about soil mapping in the state of Goa, samples distance was on average 5-7 km. Surface and subsurface soil was mapped for Ca<sup>2+</sup>, pH, OC, EC, and other variables. The study report interesting information which will be of sure interest for decision makers, and practioners. However a series of objections prevent me from suggesting its publication in Solid Earth. 1)The main critical point is the lack of an hypotheses to be tested, which I believe is central to every article. The lack of an hypotheses to be tested result in a discussion/result section that is not very effective in pointing the scientific advances introduced by this study. 2)The mapping could be greatly improved by using co-kriging approach with other environmental variables. 3)No uncertainty in the prediction is reported in the maps (while this is one of the main

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advantages of using kriging. 4)Some map report a wider scale of values that are not met in the actual predictions (for example Bray-P in surface soil). 5)Information on how the predicted variables were grouped in homogenous areas are lacking. 6)I suggest reporting the variograms and not only the variogram model parameter.

The title is a bit vague. I suggest the authors change it to match the purpose of the study (ideally in a way that just by reading the reader understand the main finding of the paper).

INTRODUCTION: No hypothese is reported here.

Line 32-44: I believe that this part is too generic, I suggest the authors remove it. I am inclined to believe that the readers of solid earth are convinced of the importance of soils. Line 52-54: I find also this part a bit generic: for example, not all geostatistical tools are aimed to predict unknown locations. The authors may consider for example the techniques to analyse point patterns and clusters, or Kriging simulation techniques. Also on line 53 consider change "reducing" to "reduces". Line 56-59: I do not understand exactly the point of the authors reporting that Li et al., 2011, Behera and Shukla,2014 and Behera and Shukla, 2015 found different spatial patterns in soils. I believe that that all the readers would agree that soils exhibit spatial variability, and that the patterns differ from location to location (probably also depending on the investigation scale). Last but not least the authors pool together studies from very different regions. Line 65: high compared to which other plants? I suggest that the authors specify that. Line 65-67: I absolutely do not doubt the word of the authors about the yields of oil palm. However I find this way of reporting information is a bit anecdotal. I suggest the authors report findings from other studies regarding oil palms (ideally from meta-analysis). Also judging from the title ("Natural <sup>13</sup>C distribution in oil palm (*Elaeis guineensis* Jacq.) and consequences for allocation pattern") the only reference reported by the authors is not primarily on oil palm yield. Line 67-73: I am not sure of how these information about oil palm production may contribute to frame the hypothesis tested by the authors (which I believe is the ultimate goal of introductions).

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Line 74: This part sounds a bit ideological to me, as if the authors are replying to an ideal speaker who is against the use of fertilizer. Moreover I believe that this is out of the scope of the study. Line 82-84: Given the emphasis that this study report on geographical variability, reporting that "Mg deficiency and B deficiency affect oil palm production in oil palm plantations of India " seems a very broad statement. I suggest the authors narrow their focus to the Goa State. Line 90: However it seems to me that the authors did not use this approach here, but only studied the soil, without matching it to the leaves nutrient content . Line 92-93: "the recommendations in general ... are generic". This sounds a bit generic :)

MATERIALS AND METHODS: Line 126-128: How were the points randomized? Did the authors take any precaution to exclude bias (unintentional) in point selection? GEOSTATISTICAL ANALYSIS: Line 149: I am not convinced of the possibility of using Pearson correlation coefficient to assess the significance of correlation. In fact I think that the Pearson correlation coefficient is a value that varies between -1 and 1 that indicates the strength of a correlation and its direction. However there is no probability associated with it. How did the authors define the variogram binning intervals? Line 154: How was the trend of the data checked and removed? Which order of polynomial function did they use? How did they decide on the significance of the different factors? Line 165-166: This was reported also before. Line 151-154: Why did the authors transform the data? As reported also by ESRI webpage ([http://webhelp.esri.com/arcgisdesktop/9.3/index.cfm?TopicName=Understanding\\_transform](http://webhelp.esri.com/arcgisdesktop/9.3/index.cfm?TopicName=Understanding_transform)) "Kriging as a predictor does not require that your data have a normal distribution." Looking at the maps no mapping on the uncertainty of the predicted values is reported (for which the distribution is a necessary assumption). Line 169: I suggest the author report briefly on the goodness-of-fit criterion adopted, since I, and presumably other readers as well, had no access to the text from Agterberg from the 1984. Line 170-173: Do I understand correctly that the authors are saying that a point estimate from a map with a very high G can be more close to the real value than the measured one? I think that 1) this opens interesting (philosophical) questions on whether the

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measured values are the reality or just our closest guess to it. 2) I believe that the authors show here an excess of confidence in geostatistical tools. RESULTS: Line 189-190: The correspondance between predicted pH and rainfall and parent material could be easily checked. Even better a co-kriging approach may help to improve the prediction. Line 258: How homogenous was each area? What was the uncertainty on each predicted value of the different areas? Line 252-254: I suggest that the authors consider co-kriging using temperature as explanatory variable. Line 266-267: This is very generic. Line 269: please report a reference for the presence of sandy loam soils in the north-western and for the influence of texture on EC.

CONCLUSION: Line 303: The correlations should consider also the spatial structure of the data (maybe autoregressive model?).

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