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Interactive comment on "Chemical and physical properties of Amazon forest soils in relation to their genesis" *by* C. A. Quesada et al.

Anonymous Referee #1

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This is the first of three manuscripts by Quesada et al. and is the one that reports on the soil data collected by this group in their Amazon studies. Most of this paper is descriptive – implications of the results are discussed with respect to aspects of soil genesis, while the next paper in the series discusses covariation of soil and forest characteristics.

Overall, this is a very important data set that is highly deserving of publication, with only minor comments that the authors may wish to take into account in the text. The authors are to be applauded for their collection of both physical and chemical properties so as to provide a uniquely useful data set on soil variation across the Amazon basin. They also come up with semi-quantitative indices based on soil properties that summarize a number of properties indicative of the overall soil state – such as anoxia.

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The title of the paper – chemical and physical properties … .in relation to their genesis – is not fully realized, in part because the authors compare soils that differ in a number of soil forming factors (age, parent material, vegetation, etc.). Figure 15 summarizes a lot of information, but in the end with so many things varying, perhaps the title should be "variation in chemical and physical properties of soils across the Amazon basin" There is more discussion of the relations to taxonomy and soil genesis in the third paper in this series.

I would make a plea to the authors to make their original data available as some kind of an appendix (or, if it is an online data archive, to give the archive information here so that people can find the data). I am sure many modelers would be grateful. The reporting of data by soil order alone leaves the reader unable to ask questions about causes of variation within soil orders. I realize the authors' point here is to link the soil properties to the soil taxonomy as a way of linking to pedogenesis, but I feel like some information is getting lost and the most useful information is not in forms that others can use.

Given that the RADAM-Brasil survey is the only other such dataset available, it would perhaps be useful for the authors to add a short paragraph in the introduction as to how their study varies (or relates to) that earlier study. Are the data common to both collected in different ways? In what ways does this data set improve on the older one (the uniformity of methods across all sites would seem to be one advantage, also the P fractions, but are there other improvements or differences?)

Page 3937 and 3946- The authors refer to 'good' and 'bad' physical structure based on their index, but I am not sure I understand what these terms mean. I think the authors should give the formula for the calculation of the index they use (Muchena, 1979) somewhere in the text to avoid confusing the reader. Figure 13 is very hard to interpret and we are given no guidance at all – is a plus evidence of a 'good' quality? Why are there multiple marks in the columns and what does the size of the bar mean? The authors

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refer to the end members in their scoring system as a "friable, low density and well aggregated soil" – which I assume is "good" and "hard, massive, compact, dense soil", which I presume is "bad". Can these end members be pointed out on Figure 13? Also, good or bad for what? Plant growth? Soil fertility? Degree of anoxia and therefore decomposition rate? More clarity here would be useful given that the terms are very qualitative; it might be more useful to the reader to replace 'good' and 'bad' with 'high score' and 'low score' throughout the paper.

This is a very long manuscript with many figures. I think there are some places where the manuscript could be shortened, notably in the discussion section. Much of the beginning of the discussion refers to ideas that have been discussed elsewhere in the literature (e.g. the Walker and Syers type model of how forms of P will change with soil age and the progressions of cation availability with intensity of weathering (e.g. pages 3948-3950). The authors give a nice summary but it could be shortened.

The same argument goes for the discussion on page 3951. What is missing from the dataset is mineralogy – given that we do not know if there is primary apatite in the parent material (or are those data available?) or in the soils themselves, despite the discussion of physical characteristics, the discussion of 1:1 clays, etc. could probably be dropped.

Speculation on the relationship of lignin and degradability of soil and availability of nitrogen seems out of place, given that there is no information available on lignin.

If space is not a constraint, I don't really have a problem with leaving the speculative statements in and long explanations in the paper, but if it is a constraint I think it detracts a bit from the main points that can be made from the data that are available. (which are that the data show spatial trends and interesting covariances that likely relate to edaphic factors)

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