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Interactive comment on "Carbon, nitrogen and sulfur (CNS) status and dynamics in Amazon basin upland soils, Brazil" *by* Jörg Matschullat et al.

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Dear editor (and possibly dear referee colleague, responsible for those review comments),

We as authors are a little bit stupefied by some of the comments since they make assumptions about our work that are neither corroborated by the article itself nor by previous related publications.

Referee 1 writes: "... the correct approach would (have been) to preplan a sampling programme, based on statistical evaluation of the number of sites and samples required

to produce statistically significant results".

Our comment #01: The Amazon basin is not Central Europe or comparable in site accessibility with many other places in the world that have easy road access. So far the entire basin is represented on the Brazilian side in most publications by locations and sites near the Embrapa research sites in the states of Acre (Rio Branco), Rondonia (Porto Velho), Amazonas (Manaus) and Pará (Belém), to name the most prominent ones. In all cases, these sites are in the direct perimeter of the related capital city (maximum 30 minute drives on asphalted roads.

In order to overcome limitations that certainly do derive from those limited locations/sites, we have planned for about six months where to position locations and their sites in order to cover the Amazonas state upland (terra firme) part of the Amazon basin, acknowleding its geological, morphological, climatological and hydrological differentiation into the central (Amazon "Graben") part and the southern "shoulder" part, details of which are given in the manuscript. This selection was done using the expertise from our Embrapa team plus of the National Institute for Amazonas Studies INPA in Manaus that know Amazonas state and its soils very well. By definition, we intended to cover as large an area as possible, as far away from direct urban or municipal impact as possible, where sites represented little (if at all) disturbed rainforest. in their vicinity, we selected another site (or more) with post-forest land cover (agriculture, pastureland etc.), following more of less recent deforestation. We are rather confident that we did identify very appropriate locations to realize our study. At each sampling campaign, an Embrapa-based, highly experienced soil scientist was part of the team, confirming the choice of locations and sites. Differences (representativeness) were certainly not larger as compared to sampling e.g., only brownsoils (cambisols) of Central Europe (see FOREGS and GEMAS soil mapping projects by EUROGEOSURVEYS).

It goes without saying that an even larger number of locations and sites would be nice to have, and yet, we do not know of anyone who has undertaken with one team any related task, covering so many locations with repeated sampling and fulfilling very tight quality control.

The referee continues: "Instead the authors have chosen eleven sites in three clusters, and sampled at three places at each site to produce their results. It is hard to accept that the locations are in any way representative, and so to draw conclusions about the Amazon terra firme soils as a whole, on th ebasis of the presented result, cannot be justified."

Our comment #02: We have not chosen eleven but 13 locations with 29 sites (some locations had more than two sites) - see figure 1. There are no three clusters. One might argue for two clusters, one being in the Amazon "graben" (locations 01 to 06) and then the rest, yet really, these are no clusters - see geological, morphological and land cover maps. The data also clearly corroborate our claim that there is no geographical bias in the data. There is, however, a clear distinction between the more wet central part and the relatively drier southern part (locations 07 to 13).

The authors partly overlap with those that successfully published a highly-cited review paper on soil respiration with in-depth discussion of representativeness (Oertel et al. 2016). Apart from the focus there on soil respiration, the vast majority of the several hundred studies that the review is based on did a) also take soil samples and b) did usually take rather fewer samples and mostly no repetitions at all.

We claim that our sampling approach is very representative indeed, fully corroborated by the data, where subsequent sampling campaigns very well reproduce the previous results. This is not only recognizable in CNS data, but also in major, minor and trace elements (65 elements of the PSE, manuscript in preparation). We can provide the evidence, if you wish.

Referee 1 writes: "A second criticism is that no statistics are given. ... only give qualitative impression".

Our comment #03: There are indeed different "schools" in earth science and soil sci-

ence, some of which trust very strongly in statistics and other that focus more on field evidence and observations. It is not our place to judge between those "schools". Instead we wish to point out that Dr. Solveig Pospiech (co-author and Post-Doc with ample experience in high-level statistics) and her supervisor, Prof. Dr. Gerald van den Boogaart (highly recognized as leading specialist in geostatistics), have read all versions of the manuscript and helped with the R-scripts to perform the statistical evaluation presented in the manuscript.

Obviously, there is quite some statistics behind the figures 2 to 5 and again, very thorough quality control. Therefore, the generalized claim of referee 1 that there was no statistics given, is not true. What the referee may miss, is some tabular statistical indices such as "alphas", "r squares", etc. to describe probabilities, variances, etc. We can happily deliver that yet feel that it is not needed to fully benefit from the content of our manuscript. We do not really understand, how figures with unmistakable scales do not deliver a quantiative impression, but a "qualitative" one only.

Referee 1 ends: "Each of the above flaws is major, and they mean that the paper is unacceptable for publication. A third point that can be made is that comparing tropical soils with European soils, many of which must have been under ling-term cultivatioon, seems illogical".

Our comment #4: We do certainly not agree at all that there are such flaws. We agree that there may be a bias on our side towards field evidence and observations, while referee 1 likely prefers much stronger emphasis on explicit statistics. As mentioned above, this can be delivered, yet will not change any of the content nor of our conclusions.

The other statement is kind of strange to us, too. While not that strongly evident with this manuscript and the underlaying data, our other pedochemical data clearly show that the WSA (world soil average) data are obviously biased towards temperate climate and European and North American soils. In 2012, Patrice de Caritat (Geoscience Australia) and Clemens Reimann (Norwegian Geological Survey) published an excellent paper in EPSL, comparing European and Australian soil data and developed "Predicted Empirical Global Soil (PEGS2) reference values" (see doi 0.1016/j.epsl.2011.12.033).

Of course one can - and should - compare large-scale data sets, if the derived insight and/or message is a valuable contribution to discussion. In this particular case (topic of our manuscript), there simply exists a major bias in much of the science world towards European and North American data and their interpretation. One concrete effect is the oversimplification of tropical soils as being nutrient poor, especially in the humid tropics. Since nitrogen and sulfur are key nutrients and the organic carbon to nitrogen ratio an important indicator for soil quality, the comparison we make (and which is not even in the focus of our manuscript) is of interest to the community.

The critized "upscaling" is a truly small - and as such very clearly defined within the manuscript - attempt to show consequences of the new data, if we upscale them. The manuscript does nowhere claim that we now deliver the "truth" about Amazon basin soil CNS concentration. We upscale to one hectare, and yes, one can upscale higher and see how that relates to other assumptions already published.

We simply hope that our replies and explanations help to better understand our work, of which we are proud and certain that the data quality is of very high quality and trustworthy.

Sincerely yours, and writing for all co-authors

Jörg Matschullat

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