

## ***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, dear Alessandro Samuel-Rosa as second referee,

Let me reply to your, Alessandro's comments (ASR) in the sequence of your review:

ASR 2: "I agree with the Anonymous Referee #1 when s/he says that the sampled data is insufficient and inappropriate to answer the initial questions and upscale the results to the entire upland Amazon basin. There is very little indication on why and how the sampling sites were selected or how representative of the entire upland Amazon basin they are. Sampling along a single year also is insufficient to make statements

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about the seasonal dynamics of carbon, nitrogen and sulphur contents in the soil and litter."

Our comments 01: Could there be a misunderstanding inasmuch as our "upscaling" exercise is of minor relevance within the context of our work and has been overemphasized by both referees? Even reading our abstract now, I have a hard time seeing that point in the limelight.

However, to question our new data as insufficient and inappropriate for answering the initial research questions demands debate: We say: 1) What are CNS concentrations in Amazon basin upland soils - and how do these compare to other world soils? We deliver exactly that with a larger number of samples as before - not modelled but truly taken (repeatedly) and measured state-of-the-art. We ask 2) if there are differences in CNS status between forest and post-forest soil - and we deliver answers. Our question to the referees: where is the misunderstanding?

The second part of the referee's comment above relates to the methodology of site selection. We can of course go into more detail - and this has partly been addressed in our replies to referee 1. Top criterion was representativity for Amazonian state upland soils. Criterion 2 was ferralsols (oxisols, latosols). Criterion 3 was accessibility within the usual constraints. This meant site accessibility in both dry and wet season. We drove (4WD) as close as possibly with our 4WD, then had to walk and carry all gear into the forest or onto farmers land, partly hundreds of meters to > 1 kilometer. All sites with any type of farmland were locations with private owners that are under the supervision of Embrapa Amazonas. Thus, we could rely on trust of the people and infrastructural support (tractor to pull us out of the dirt, help in hacking trails of several hundred meters length into forest). As mentioned before, all locations were considered highly representative by Embrapa and INPA specialists prior to the first field campaign.

The third point needs to be countered, too. When extreme conditions define seasons of a single year, related effects are generally stronger than under normal conditions. We

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were fortunate to encounter such extremes as described in the manuscript. That allows studying climate-related effects and is common practice in many regional climate-change studies.

ASR 2: "there is no background information of upscaling methods. Of major concern is the fact that the authors completely ignored the (often) large uncertainty of upscaled results. In the same manner, as already pointed to the Anonymous Referee #1, the description of statistical methods and the presentation of results is very incipient."

Our comments 02: The manuscript clearly states that the data were upscaled to one hectare. All necessary information to recalculate that is given in the manuscript. We are more than aware of the risks involved in any type of upscaling. And we claim nowhere that this little exercise goes beyond its scope. My only assumption is that our wording invites related misunderstanding - this can of course be changed. On statistical methods, see our reply to referee 1.

ASR 2: "It is not clear why the authors attempted to compare the soil contents of carbon, nitrogen and sulphur of the Amazon with the soil of other parts of the world. For that reason, the Anonymous Referee #1 stated that "comparing tropical soils with European soils[...] seems illogical". If the authors have any strong argument in favour of doing so, then their argument should be provided in the manuscript. But note that, when making such global comparisons, then the authors should also include other tropical forest in the analysis as well."

Our comments 03: As explained in our replies to referee 1, there is a significant bias in world soil average data (and wide range perception even amongst the science world) that humid tropical soils are XXX. With our "crazy" comparison of the latest empirical CNS data for all Europe, we can show that reality is not quite that blunt. Instead, mineral soils of Europe with radically different climatological and younger geological history show - based on median values - exactly similar values with our from the Amazon basin. Coincidence? We do not think so.

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To include other tropical forests as well, as referee 2 suggests, has been attempted. And yet, any search will quickly reveal that very few high-quality data are around. The current attempt of the Max Planck Institute of Biogeochemistry to compile global subtropical and tropical data clearly illustrates this. We are in dire need for more and better data for this vast part of the world - and our manuscript contributes to that.

ASR 2: "Regarding the statistical analysis of the data, I want to stress that the authors should take into account the fact that the sampling sites were chosen in a non-probabilistic manner and are highly clustered. This means that geostatistical considerations could be necessary when analysing the data. Also, some sampling sites are very close to major cities and/or major rivers, while other sampling sites seem to be very isolated. The authors need to demonstrate that the closeness to major rivers and cities have no (or only minor) influence on the results."

Our comment 04: Exactly that approach has been taken in our statistical analysis - see reply to referee 1. Solveig Pospiech and Gerald van den Boogart took great care to test exactly that, too. Just generally speaking: Proximity to major rivers or to larger urban centres of our locations is just as balanced as it is in reality - see figure 1. As soon as a location is far enough not to get inundated in the rainy season (=varzea or igapo) the absolute distance from a river is irrelevant. It is more important, which lithology underlies the locations and what land cover there is. With urban structures, it is a similar story: As soon as there is no more direct urban impact (see e.g. deep within Reservatorio Adolfo Ducke, near Manaus), there is no noticeable urban impact in soil chemistry.

ASR 2: "Finally, given that the present study was funded by government agencies, both Brazilian and German, the authors should present a long term data management plan. For instance, this could be done through its publication in a research data repository."

Our comment 05: There is, but is this ever part of an individual manuscript? We provide all manuscript-related data to the readers as electronic supplement. And all data are

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and will be available to anyone interested with all meta-data.

Final words of ours: We understand that some of our wording will have provoked misunderstandings and that we should be more explicit on some details in methodology.

Sincerely and for all co-authors

Jörg Matschullat

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