

# ***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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## **1 General comments**

The present manuscript describes the study of the variation of the carbon, nitrogen and sulphur contents in the soil and litter in uplands of the State of Amazonas, Brazil. The main goal of the authors was to answer long-standing research questions about the dynamic of these nutrients in the Amazon region by using more reliable data than previous studies. Unfortunately the present manuscript fall short in delivering the aimed target. Some of the reasons have already been provided in the comments of the Anonymous

Referee #1. In the next section I provide a few comments to address some of the key scientific issues that I identified in the present manuscript.

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Before proceeding, let me mention that I think the present manuscript has the potential to provide new and interesting knowledge about the soil. It only depends on how willing the authors are to make the necessary corrections and adjustments.

Interactive comment

## 2 Specific comments

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

Interestingly, the authors have provided a very thorough description of the soil sampling and laboratory analysis protocols, reagents and equipment. However, aside for the little information of the procedure for selecting the sampling sites, 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.

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.

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 site 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.

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.

### 3 Final considerations

I conclude that the manuscript, in its current form, is not suited for publication in SOIL. I suggest the authors to make a major revision, providing more background information on how the sampling sites were selected and using more appropriate statistical analyses. Additionally, I strongly recommend the authors to avoid making any upscaling attempt with the existing data because I think that it is insufficient and inappropriate for such purpose.

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