

## ***Interactive comment on “Biogeochemistry of an amazonian podzol-ferralsol soil system with white kaolin” by Y. Lucas et al.***

**Anonymous Referee #1**

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This manuscript presents the chemical composition of groundwaters measured at different sampling depths and locations in a well-documented podzol-ferralsol soil catena. Based on the results and their knowledge on the site, the authors proposed that two hydrological pathways exist in the soil catena and related groundwater compositions to soil mineral paragenesis. Eventually, they provide a geochemical scheme of the functioning of the studied podzol-ferralsol soil system.

### General comments

The study site description is well documented and the database presented is impressive. However, I did not fully get the point of the study. The authors need to explain better why they did this study and then discuss what the conclusions and implications of their work are. It is all the more crucial for the impact of this study that it is easy

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to get lost in all the presented data tables. I would have needed more explanations in the introduction on why it is interesting to know about the biogeochemistry of this catena and why the adopted approach was thought to be fruitful. For instance, is there any scientific controversy about the biogeochemical functioning of podzol-ferralsol soil systems with white kaolin? Why groundwaters measurements would be the key for significant advances? If so, why the locations of the sampling sites are appropriated? Have the authors any a priori hypotheses on how groundwater elemental concentration will change with the location of the sampling site? Concerning the conclusions and implications, the authors wrote in the end of the introduction that “Such knowledge is necessary to evaluate the possible changes in the natural organic matter (NOM) dynamics through global change, as well as to understand the relationships between soil, kaolin and landscape features, in order to preview the soil characteristics and the possibility of kaolin ore from remote sensing. (PP2236L19-24)” Unfortunately these aspects were absolutely not discussed in the manuscript. To my point of view, the authors need to rewrite the abstract, introduction, discussion and conclusion sections to present more clearly the objectives of their work and what are the important conclusions and implications of their findings.

### Specific comments

The role of the vegetation is very little discussed. It may be due to the fact that the authors focused on Al, Si and Fe. However, I would have appreciated a discussion about the role of the vegetation that may have different characteristics along the catena on DOM concentration and Si cycle. In its present form the manuscript is more a geochemistry paper rather than a biogeochemistry paper. By the way, the results on other elements than Al, Fe and Si are not discussed. In this case, I suggest the authors present the results on the other elements as supplementary materials.

The part on the characteristics of DOM is not convincing. The fitting with the PROS-ECE model is intriguing. The authors wrote “Here we chose 4 acidic sites, a sufficient number to ensure a good fitting of the experimental curves (PP2248L25)”. Is there

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any objective criterion to choose 4 acidic sites? If not what would have been the results if the fitting was done using 3 or 5 sites? It is speculated about the composition of DOM that “This DOM has thus likely a high proportion of small carboxylic acids such as formic, oxalic or citric acids whose site densities are 83.3, 83.3 and 41.7 meq gC<sup>-1</sup>, respectively (PP225225)” or “The DOM circulating in depth in the kaolinitic horizons was quite different. (PP2252L22)” Why did the authors not look at the DOM using more advanced analytical tools (chromatography, IR. . .)?

The justification about the selection of the sampling points is not convincing. “The sampling points were selected with the purpose of identifying the main biogeochemical compartments of the soil system. (PP2240L7)” What does that mean? It seems like the authors knew what the zones of interest for sampling were and decided to sample there. That is not necessarily a problem but they should present the story like that and specify why these zones were selected.

I would need that the authors discuss the spatial variability of the site. They do not have replicates. If chemical groundwaters composition can vary strongly within the same soil horizon, it would be a problem. Can the authors comment on that?

Minor comments

P2235L23 : “at least  $13.6 \pm 1.1$  PgC is stored in Amazonian podzols and could return the atmosphere if the climate changes with the onset of a dry season.” Hopefully only part of it coul return to the atmosphere!

PP2240L23 was the driest instead of drier

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