

# ***Interactive comment on “Mineral nutrients in Saharan dust and their potential impact on Amazon rainforest ecology” by Joana A. Rizzolo et al.***

## **Anonymous Referee #2**

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Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-557, 2016 MS: Mineral nutrients in Saharan dust and their potential impact on Amazon rain forest ecology Authors: Rizzolo et al.

## **Abstract**

1. the problem of this manuscript that begins by the Title and the Abstract is the generalization of ideas. First I suggest that the authors be more specific in the Title, it should focus on the soluble fraction Fe(II)/Fe(III) issue, which is something important but not enough to account a full story about the Amazon rainforest ecology; 2. In the introduction, clarify at Line 124(pag 6) when the authors say “. . .Considering that iron is absorbed by plants only as soluble Fe(II)/Fe(III)”, previously the authors have stated

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that Fe(III) was also recovered by the action of rhizosphere; 3. In methods, Line 142, pag 6, provide filter porosity; 4. In Item 2.5, specify how the samples were storage and if the observations were conducted in the filed or in a particular laboratory condition? 5. The sentence in Line 229-231 “The mass concentration of particles over the Amazon Basin in the wet season is typically around  $10 \mu\text{g m}^{-3}$  in locations that are influenced by biomass burning emissions”, ... here there is confusion on the wet season and biomass burning season. Which reference is attributed to the mass concentration mentioned? 6. Part of the text in Lines 254 to 268 (pages 11-12) could be placed at Material and Methods. If possible the authors could place BCo time series superimposed to mass concentrations at Figure 1. This could give some idea on the contribution of BC to the bulk atmospheric concentrations, or if they are lagged in time; 7. In Table 2, how the elemental analysis was conducted for Cu, Zn, Na, Ca, K and Mg ?? and about the  $\text{NH}_4$  ? 8. In the title of Table 2, it is not “aerosol characterization” it is aerosol composition; it does not correspond to “during the Saharan dust event “, it is before, along and after the event; 9. In Lines 279-282 the authors say that K, Zn and Cu are of biogenic sources, probably mostly emitted during biomass burning. If the detected pulse of dust in this work is coincident with an African biomass burning event as pointed by the authors, what is the level of certainty to say that their main source is the mineral fraction? 10. In Line 322, the comparison of the present work with Andreae et al. (2015): does both work have same methods and associated errors? Results of Andreae et al. (2015) correspond to what period of the year. Specify please. 11. Text in lines 333-338 is unnecessary; 12. Dates in Figure 2 is unreadable; 13. Figure 3 should be completely edited. It is not possible to use the Hysplit output directly; 14. For Figure 3, use ensembles, not a single trajectory; a family of trajectories gives a better idea of all geographical contributions; 15. Lines 369-374; Figure 1 shows before, along and after the “dust storm”, I suggest that the authors run the Hysplit model in these 3 circumstances and then make their conclusions; 16. In Line 387 provide complete localization of the three AERONET sites: Dakar and Ilorin in Africa, and Embrapa/Manaus in Amazon; 17. In Figure 5, AOD do not distinguish dust from biomass

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burning products. From the location of higher AODs in the diagrams it seems that your source could have some contribution from biomass burning than mineral dust. Also the results presented in the Hysplit are not totally in accordance to wind flows at the charts at Figure 5. Maybe the source is a net combination of both; I strongly suggest that the authors add a map with fire spots for the period of sampling, so to make better differentiate; 18. In Figure 6, what is MC ? Please, correct the legend of time; 19. The discussion on fungi is very poor. There is none description of the species nor anything on their biogeography. The lesson of this result is the fact that a more detail aerobiological research should be conducted to be published; 20. In Line 463-465, the authors say “Smoke plumes are known to entrain fungi over long distances (Mims and Mims, 2004). Dust from Lake Chad is rich in bacteria and fungi.” Here becomes explicitly that the authors are not able to stablish a source of the particulate matter entering Amazon in the considered event: Saharan mineral dust or sub-Saharan biomass burning ?? 21. The Amazon itself is a fantastic source of bacteria and fungi, and only an endemic specie of Africa, detected in Amazon, at high level (ex. The top of the ATTO) could make a clear distinction; 22. In item 3.5 the authors says that “a small amount of atmospheric iron could affect the microbiota in the canopy, rather than have a significant effect on soil and root uptake for plants.” This is an speculation and from this work it is not possible to conclude anything; 23. In my opinion, most of item 3.5 is Introduction to the study since most of the text is compilation from the literature associated to this work. 24. The conclusion unrealistic, should be reduced to the basic findings.

GENERAL OPINNION: The positive issue in this work is the measurement of Fe(II) in dust, this is great and I encourage to be improved in the future. From Table 2, it is clear that the event provided only 4 measurements, and 2 of them are not statistically significant. Most important is that they occurred during the storm. The tentative of describing the transport mechanism and sources (desert and/or) biomass burning needs improvements. There are many text here that only explain the general sense of the problem and definitively the fungi issue should be excluded. Maybe focused in a specific work or using more data. Most important to have in mind is that between the

mineral apportionment and the ecological response (in all levels) there is a long way of processes that this work do not account alone. From the above, I recommend this work as a letter or communication not as a Research paper.

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