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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric salt deposition in a tropical mountain rain forest at the eastern Andean slopes of South Ecuador – Pacific or Atlantic origin?" *by* S. Makowski Giannoni et al.

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This paper is on an interesting topic that quite frankly I have not thought of before-namely the role that sea spray may have in geochemical cycles and how specifically Na and Cl may be important. In this regard, I think the paper is certainly appropriate for ACP. This said, while I generally get the general idea of what they were doing the paper does not follow traditional lines of analysis from an aerosol point of view. This is not too surprising given the authors background is geography and land surface. But, I would strongly encourage the authors to reach out some colleagues in Germany for

some help in interpreting MACC and trajectory analyses. For example, the Max Planck Institute for Chemistry, Mainz is world renowned for their aerosol work in South America and would be worth consulting. I think the authors have something quite interesting right here, but as an aerosol scientist it does not close as neatly as it should for ACP. I thus recommend major revisions with the understanding that they will get some aerosol help. I am not going to go into details, but I do have several important comments that need to be addressed.

1) I am not sure HYSPLIT at 2.5x2.5 degree resolution is a trajectory model of choice for this kind of mountainous terrain. Now this said, it would probably give you the prevailing wind direction (coming from the west or east), but as I think there is some danger of confounders (see next comment) something a bit more sophisticated is in order. Consider, often at altitude you could have easterlies, but coming up the mountains on the westerly side are upslope anabatic winds. This could be an even bigger part of the budget. See comment 3.

2) The authors already noted that biomass burning tracers appeared seasonally in their analysis, and suggest that this corroborated colinear transport of sea salt and biomass burning. Although, they note that Cl is in fact emitted. Actually, Na and Cl are both strongly emitted by biomass burning, and in the July through October time frame, copious amounts of smoke are transported directly over the study region. It has even been noted as a pathway of cloud condensation nuclei in the Central Pacific. In the winter months, smoke from northern South America is also frequently in the region. At the very least biomass burning in MACC should be included in the analysis. Perhaps seasonal maps of total mass load for sea salt and smoke are in order too?

3) The authors look at elemental masses, but really they need to look also at molar stoichiometry in this circumstance. Although the paper says that the mass ratio of Cl:Na goes down in time, they need to understand that Cl depletion is in some ways a photochemical clock. If Na:Cl is what it is for fresh sea water, you know it is local-maybe upslope winds. As it goes up, the sea salt is older (across the continent?). This

should be included in the analysis

4) The paper gets in the way of itself in its diction and grammar, and thus a good edit might be in order. Also the way some of the data is presented the data gets to be difficult to interpret. I would then do a time series plot of the factor loadings. In Figure 3 for example, the PCA analysis would be better interpreted as a table. I also had a hard time making heads or tails of the lower figures in figures 4 through 6.

Hope this helps, Jeffrey S. Reid, US Naval Research Laboratory.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27177, 2015.