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Atmospheric Chemistry and Physics Discussions

Interactive comment on "River Breezes for Pollutant Dispersion in GoAmazon2014/5" *by* Adan S. S. Medeiros et al.

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Response to Review #3

1 - Comment from Referee: The topic addressed by this paper is so important to the overall project that it deserves to be done more thoroughly. I always fear that, in the course of large field projects, topics are doled out for publishing among participating groups in such a way to avoid overlapping discussions, but, in so doing, incomplete research results. Fortunately, the authors have considerable information in hand, and, if they are willing to re-enter the data analysis phase a bit, they could complete the missing parts. The resulting paper will be of greater use to the community. Please make a major revision. Among the missing parts I identified are:

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1 - Author's response: The authors thank the reviewer for the acknowledgement of the importance of the manuscript.

2 - Comment from Referee: Plots and discussion of the vertical structure of the convective boundary layer (CBL), and this material must include wind direction & speed as well as potential temperature. There are three ways to address this issue with the available data: The conventional radiosondes at MAO, the aircraft soundings on takeoff and upon landing, and the model output. The authors make a conclusion that breeze circulations are not important in dispersion, on the two case study days, but they offer little context. How thick was the convective boundary layer, over land and over the river, where vertical mixing will be suppressed?

2 - Author's response: The reviewer's goal for the manuscript and the authors' goal for the manuscript appear to differ.

As authors, we already have the observational result: A result of GoAmazon2014/5, which was unexpected given extrapolation of results of previous modeling studies in the region, was the absence of an effect of the rivers in aircraft measurements of gaseous and particle tracers of pollution at 500 m before, over, and after the river (i.e., in direction of typical easterlies of the trade winds).

Therefore, the purpose of the study is to assess if current modeling capabilities and understand capture the absence of a river breeze effect on the dispersion of the Manaus pollutant outflow.

In this regard, the previous modeling studies that led to the above-mentioned extrapolation were not precisely focused on the same question as ours, they used different grid sizes, and they employed older physics and parameterizations. The intention of the present study was therefore to provide a modeling study focused on the river to test if our current understanding of physics of this environment and approach to its modeling was consistent with these aircraft observations. The conclusion of the manuscript is that the physics embedded in the model suggests at limit of 150 m in the height of

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the river effects.

The reviewer's interests for model-measurement closure appear broader than the purpose of the present manuscript, and we also appreciate the intellectual interest in these broader questions. Nevertheless, these broader interests appear outside the need of the focus and scope of conclusions of the present manuscript.

The abstract and introduction are revised to clarify the scope of the manuscript and to avoid the confusion reflected in this comment/reply between reviewer/authors for the general readership.

3 - Comment from Referee: Better exploitation of the single month of modeling than was done. The current side-long glance at these results is insufficient. I am looking particularly for average diurnal behavior near the surface, and all I get is Figure S1, showing wind speed, temperature and RH. Since an RH plot looks like the upside-down rendering of the temperature plot over the diurnal cycle, one would do better showing the specific humidity. Better still, plot the temperature and specific humidity hourly medians on the same plot, reserving the right vertical axis for specific humidity and saturation specific humidity. (It's not that difficult, though the latter has a slightly nonlinear scale, of course.) More important is some indication of the diurnal variation of wind direction, so that the reader can understand how the breeze baroclinicity affects directional wind shear.

3 - Author's response: As with comment 2, we appreciate the reviewer's broader interest, as reflected in, "I am looking particularly for," but the authors' interest for this manuscript is different. Please see reply 2. As mentioned there, the revised abstract and introduction for the readership clarifies the intention of the authors' study. We do agree with the reviewer that additional important and interesting studies and analyses of river breezes can be made, even as we judge that the provided story is sufficient to support that state-of-the-art modeling approaches support the observation of the absence of a river breeze effect on Manaus pollutant dispersion on most days at most

times as a broad statement, with exceptions at certain days and at certain times, as discussed in the manuscript.

4 - Comment from Referee: A focus on the river breeze overshadows the other effect of the rivers, mainly that the flow is channeled. (Kindly write 'channeling' rather than the word 'canalization', which, though a word in English and happily a cognate to its Portuguese counterpart, is almost never used.)

4 - Author's response: "Canal" is adjusted to "channel" throughout the manuscript.

5 - Comment from Referee: How might the dispersion of the pollution plume change as it goes over the river, where surface fluxes must diminish as compared to over the land? Make some estimates of the buoyancy flux over the river and compare with the direct measurements over the land.

5 - Author's response: The purpose of the manuscript is to respond to this question: "How might the dispersion of the pollution plume change as it goes over the river?" The conclusions state: "This study evaluated the effects of river breezes on pollutant plume dispersion or channeling in the central Amazon." The results plotted in Figure 3 show that the effective buoyancy modeled for the river "peters out" at 150 m. The G-1 aircraft flights directly over the river at 500 m (which was the lowest allowed legal height for flights at that time) show no evidence of an underlying river in the gas and particle tracers of the pollution.

6 - Comment from Referee: Abstract, line 3. Did I miss something? Where do the authors discuss how the chemical composition of the plume changes downstream?

6 - Author's response: We thank the reviewer for this observation. However, the intention of this sentence appears to be different for this observation. What we want to address is a known fact, i.e., the Manaus pollution plume changes the atmospheric composition downstream.

7 - Comment from Referee: Abstract, lines 9-13. If the authors' model result indicates

that any breeze circulations might be confined below the 150-m level, was the design of the project, with flights limited to 500 m altitude, flawed? Also, was any effort done to consider lighter-wind vs. strong-wind conditions. One might think the breeze circulations would be more apparent in the former case. Please comment.

7 - Author's response: Please see comment/reply for 2 and 3 above. The study seeks to explain the observations, i.e., the study leads with the observations. The reviewer has a different kind of manuscript and scientific endpoints in mind (see comment/reply 2 and 3).

8 - Comment from Referee: Abstract lines 25-27. The authors conclude that "..most pollution was transported at heights well above the effects of the river breezes. . ." What then does this indicate about the wisdom of locating the vast DOE ARM resources at a single site at the surface (T3), downwind? Please comment on this and elaborate about what these sentences mean. How is the pollution 'information' communicated to the surface? Answering this question brings you straight back to trying to understand the differences between the CBL over land and whatever is present over the rivers.

8 - Author's response: The results suggest that observations at the T3 location are not greatly affected because the river breeze appears to push the pollution "up and over" rather than "capture and channel". The pollution is at the surface past the river and is sampled at T3 because of the atmospheric convection. Measurements at 500 m in the G-1 when location above T3 and measurements at the surface at T3 for the same time period are in agreement with respect to many atmospheric and particle tracers of pollution. The results are not shown in this manuscript because the topic is judged as out of scope (cf. Martin et al., BAMS, 2017).

9 - Comment from Referee: Line 48. Prof. Maria A. F. Silva Dias, has been cited for some time as "Silva Dias". Please follow this pattern, so that readers looking up cited papers will not get confused.

9 - Author's response: We thank the reviewer for the contribution. The alteration was

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done in the manuscript.

10 - Comment from Referee: Line 166. On what time scale can one conclude that "carbon monoxide was mostly inert on the time scale of the simulations..."? Here are some competing scales: a) Mixing in the convective boundary layer zi/w*, where w*, the convective velocity scale that depends on the surface buoyancy flux. This might be different over the river & over the land; b) the time it takes the air to advect from Manaus to the point of measurement; and of course, c) time of day (time since the surface layer became convective, though one might argue that over the river that layer was convective all night.

10 - Author's response: "Inert" refers to the chemistry, i.e., that the reaction of CO + OH —[O2]–> CO2 + HO2 has a much longer lifetime than the transport time within the high-resolution central box of the simulation. Chemical lifetimes of CO are typically 30 to 90 days on global scale in the lower atmosphere. The revised text changes: "carbon monoxide was mostly inert" to "carbon monoxide did not have significant chemical sinks".

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