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Discussions
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Interactive comment on "The challenge of simulating the sensitivity of the Amazonian clouds microstructure to cloud condensation nuclei number concentrations" *by* Pascal Polonik et al.

Anonymous Referee #1

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This paper compares observed CCN, CDNC, and cloud droplet effective radius measured by the HALO aircraft in the Amazon with model simulations (WRF-Chem) and new remote sensing instrument deployed on HALO. This is an important topic, since measurements are needed to evaluate model predictions of cloud-aerosol interactions which are known to be highly uncertain, especially for convective clouds. The uncertainty in simulated cloud-aerosol-interactions impact predictions of aerosol indirect forcing in climate models as noted by past IPCC reports. In general, the results are presented logically and clearly although some addition description on the findings in some of the figures is needed. In addition, there are some flaws in the manuscript that need to be addressed before it is suitable for publication.

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Discussion paper

Major comments

1) The introductory material needs to be improved. The description of the relevant research on aerosol-cloud interactions is too brief. There needs to be more motivation here. For example in terms of a model, accurately simulating these interactions requires a good understanding and simulation of cloud and aerosol populations. So a quick summary of previous efforts to simulate these quantities in the Amazon would be useful as well. There have been some review articles on measuring and model cloud-aerosol-interactions that could have provided justification for the present work. In addition, the last three paragraphs seem to be more about methods than motivation for the research.

2) The authors note in several places comparisons with satellite derived droplet effective radius, but I could not find such comparisons. Either this needs to be included, or the words dropped from the manuscript. I would find it interesting to compare satellite derived values with in situ ones. I assume the satellite derived values assume vertically homogenous profiles, and it would be useful to compare HALO CCN profiles to test the validity of this assumption.

Specific Comments:

Line 8: The authors mention indirect effects here. But the paper never quantifies them as such. They do show CCN and droplet effective radii, but I would consider these simply consider these as parameters that are a metric (of many) of cloud-aerosol interactions. The indirect effect of biomass burning on clouds in a climate model sense is never discussed. So using these words in this way is misleading as to what the paper is about.

Line 11: The word “pollution” implies anthropogenic origin, but biomass burning is ambiguous in this case. Yes, the fires are probably started by humans, but is that the same as urban pollution? I see “highly polluted” used to describe high aerosol concentrations in the literature – in cases that are manmade or not.

Line 13: Here it states that simulated effective radii was too low, but later in Fig. 5 it looks to be higher than observed.

Line 20: Satellite retrievals are mentioned here, but as I noted elsewhere I did not see such as comparison. Do the authors mean specMACS which is remote sensing but on the HALO. There is some confusion here.

Line 36: The sentence should start as "Microphysical parameterizations . . ." to be more precise. The two papers cited in this sentence are not the best, since they are primarily about cloud microphysics and not cloud-aerosol interactions. I suggest including some of their more recent papers that focus more on this topic, as well as a few other authors.

Line 37: I do not think the Zhang et al. (2010) ever mentioned an improvement in terms of short-term forecasts. Instead, they demonstrated differences in the predictions associated with including such feedbacks. Either change this statement or find another paper that supports this claim.

Line 38: I would add precipitation to this list since it is an important meteorological forecast metric and its sensitivity to aerosol-cloud interactions has been examined by a number of studies.

Lines 41-43: I is not just high aerosol concentrations that provide the signals for aerosol-cloud interactions, it is more important to be in a situation with rapid changes in aerosol concentrations – from low to higher values. Aerosols can quickly "saturate" clouds so the high events listed here by themselves are not sufficient. One needs to see how a cloud responds when going between low and higher CCN.

Line 57: Table 1 probably does not need to be cited at this point. I assume that this should be cited somewhere in Section 2.2. It would also make more sense the table to be cited after Figure 1.

Lines 59-79: The description here seems to better fit the methods section. For the introductory material it would be better to state why a model is being used in conjunction with the observations during the measurement campaign.

Lines 101: In terms of activation, is secondary activation included? This process may be important in deeper convection as described in Yang et al. (2015) and Fan et al. (2018). If not, ti would be useful to describe how it could influence the results in this study.

Line 113-114: What about clouds at the restart times? It takes some time for clouds to develop. Please comment on how that assumption affects the model simulations of aerosols.

Figure 1. Please include the grid spacing for both of the grids somewhere in the plot. Include a label for Manaus. Also label the outer nest in the figure itself and not just that caption. When looking at the figure initially, I assume the entire map was the outer domain.

Line 177: the title is good, however, the section does not provide a motivation as to why remote sensing and modeled cloud data are used when in situ data is available? I presume at this point, one would want to evaluate how well the remote sensing and modeled cloud data sets are, but that motivation is missing. After reading the rest of the manuscript, I cannot find any other use of satellite derived droplet effective radii. I was expecting a satellite vs in situ observation. Why is this being mention here then?

Line 180: The phrase "providing a valuable comparison" begs the question "in comparison with what?" I must be missing some point the authors are trying to make here. With the other two methods mentioned next?

Lines 207-209: I am not sure I agree with the assertion that the nested domains have a "homogeneous environment". Convective clouds can have complex organization, i.e. it is easily possible to have shallow clouds on one side of the domain and deep convection on the other, or clear skies in one region and cloudy in another, etc. Also the aerosols, largely from biomass burning are not necessarily uniform across the nested domain. Can the authors provide some evidence regarding the homogeneous conditions over the nested domains?

Line 234: Some additional discussion of what is plotted in Figure 4 is needed. Presumably, only CCN at and below cloud base is shown. Presumably one can compute CCN everywhere and the authors just want to highlight it below cloud. But that is never really stated explicitly. Is the entire nested domain plotted? Again not clear. Also the AOD is very hard to see using the grey shading. Is there any other way to show the biomass burning plumes better? I can only really make them out for AC17. There is also no discussion of Figure 4 before jumping into Figure 5.

Lines 240-244: It is probably worth mentioning that the WRF-Chem droplet effective radii will depend on the specific microphysics scheme. One could argue that a spectral bin approach would be more realistic than a two moment approach, such as the Morrison scheme. Ideally the error bars on the modeled values is needed too – but that is impossible to quantify.

Lines 270-272: Have the authors evaluated the WRF-chem simulated size distributions with observations? Errors in the size distribution will affect estimates of CCN at various supersaturations. It is clear the simulated CCN is too low (Figure 2), but the simulated cloud droplet effective radii profiles are not that bad. There could be compensating errors in the model. Another comparison of observed vs simulated concentrations, using the AMS measurements on the HALO would provide some information about whether simulated aerosol concentrations are too low and whether the relative composition is correct (which will affect kappa). I am not saying an extensive evaluation is needed, but some additional discussion seems warranted on model performance. I appreciate the comments on resolution in the next paragraph, but as the authors stated I would expect a 3 km grid spacing to be adequate for this study.

Lines 314-318: This is a strong statement that is somewhat misleading. While I agree with the statement regarding microphysical effects at higher aerosol loading for the studies listed, I believe there are other studies that do note a saturation effect (perhaps not for just biomass burning). The last sentence can only be applied to the particular model and its configuration for this study, rather than casting doubt on all regional scale modeling. The present model may be missing processes or has poor assumptions regarding other aerosol-cloud interactions, not to mention uncertainties in emissions, that affect the results. Other models may or may not have similar issues.

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