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Interactive comment

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 #2

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This paper presents simulations with WRF-chem showing that it can reproduce trends in cloud droplet number concentration over the Amazon, although with a low bias. The model is also used to evaluate a parameterization of activated cloud condensation nuclei at cloud base, which is an important and interesting quantity. Some conclusions about the inability of regional modeling studies to represent aerosol-cloud interactions at high aerosol concentrations are drawn.

The paper uses interesting observations. Some are similar to those published already by Braga et al, but the specMACS observations are new and valuable. The model is state-of-the-art and has good potential to aid our understanding of the situation studied.

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The evaluation of the Freud et al (2011) method is useful.

However, there are some significant shortcomings. Firstly, while the model evaluation in the paper is valuable, the authors need to do more to make the most of the excellent measurements available: measured and simulated in-situ aerosol concentrations should be compared, and it would also be useful to show simulated and observed liquid water content, even though in principle this is constrained by CDNC and effective radius. Secondly, and more importantly, the main conclusions of the paper are unconvincing, as I explain below. The paper will be suitable for publication in ACP if the authors are able to address my comments below.

1 Major comments

1. Can the authors explicitly compare simulated cloud-base aerosol or CCN concentrations to in-situ observations? Is it the aerosol concentration or the activation scheme/simulated updraft that explains why the model produces fewer CDNC than is observed? CPC, CCNC and UHSAS data are already published by Andreae et al (ACP 2018) so hopefully this is straightforward.

2. The introduction needs to put this study in the context of the relatively large body of literature relating specifically to aerosol-cloud interactions in the Amazon region and in deep convective clouds, which is currently hardly mentioned.

3. Maybe the authors thought this too obvious to be worth mentioning, but effective radius goes as $(q/N_d)^{1/3}$ where q is the liquid water content (see for example Morrison and Gettelman (2008)). Therefore a saturation-like behavior, or at least a strongly reduced dependence of r_e on N_d, is expected for high N_d. For example, if r_e is 10.0um when N_d is 200cm^{-3}, r_e is 6.9um at 600cm^{-3}, and 6.3um at 800cm^{-3}. So within uncertainties due to spatial fluctuations in liquid

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water content, r_e saturates at about 700cm^{-3}, while N_d is still linearly increasing. Then, as in reality N_d varies sub-linearly with activated CCN concentrations due to collision-coalesence, one would expect saturation in re as a function of CCN (or large Aitken and accumulation-mode aerosol concentrations) to happen even earlier. The authors should put the results in Section 3.4 in this context. Given that only very small changes in effective radius are expected as CCN increases, it is not clear that the saturation effect observed is unexpected. The results need to be put into this context.

4. Further to the previous comment, concerning the sentence 'The modeled r-eff profiles began to saturate around 500 cm-3 at STP below-cloud CCN, with only small differences at higher concentrations (Figure 3), meaning that the modeled aerosol-cloud interactions saturate at approximately that concentration.' While the effective radius is indeed the critical quantity that determines cloud albedo and the Twomey effect, it is cloud droplet number that determines the 'microphysical effects' of aerosols (on warm rain formation, droplet freezing rates, and droplet evaporation), and simulated CDNC apparently does not saturate (line 277). This apparent saturation of effective radius in the model is not sufficient grounds to say the model is in disagreement with observed aerosol-cloud microphysical interactions above 500/cc, as is stated in the conclusion. The statement that the validity of regional modeling studies of the Twomey effect (for which effective radius is the right variable) is in doubt also seems unfair at the moment. However, if the authors can show the saturation effect is still true when aerosol concentrations are doubled, or biomass burning emissions quadrupled, in a sensitivity study, then I think the statement could be better justified, at least for the authors' model.

5. Freud et al (2011) say effective radius is always larger than volumetric mean radius, not smaller, by an average of 8%, and one can also show $r_e > r_v$ for the gamma distributions used in the WRF microphysics schemes, so the equation at line 185 is the wrong way up.

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2 Minor comments

In the introduction, Morrison and Gettelman (2008) is specified as the microphysics parameterization, while in the model description it is Morrison et al. (2009). I don't think these are the same, although I think they are both based on Morrison et al (2005). Please specify which is used.

L178: please add references to elucidate this statement. L184: Please split up this sentence, it currently seems to be two sentences joined together.

Figure 5: The CDNC is underestimated by the model while the effective radius is overestimated, so the LWC might be simulated quite well, but it's hard to tell by eye. How does the LWC compare between model and observations?

The last part of this paper has some overlap with Braga et al, ACP 2017 (reference 'a' in the authors' notation), this is not a problem but it would be useful to discuss the overlap in the introduction and clarify that the study adds to Braga et al in that the Freud et al method is tested with a regional model.

A couple of strange sentences the authors may wish to fix: Abstract: "Our study casts doubt on the validity of regional scale modeling studies of the cloud albedo effect in convective situations for polluted situations. . ..." (perhaps "convective, polluted situations"?) "Comparisons between entire model domains and in situ measurements are inherently difficult since the exact measured clouds will never be realistically measured. . ..." ("measured. . ..simulated?")

There are a few other typographical errors, "data" are plural, "less" is used in place of "fewer" but in general the written English is in good shape.

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