This paper documents a modeling study of CCN in the Amazon with WRF-Chem, evaluated with ACRIDICON-CHUVA observations. The authors find biomass burning aerosols influence the Amazon clouds, but also suggest a saturation of the effect in very polluted conditions.

The authors have gone some way towards addressing the most important of my previous comments. The review responses were reasonably comprehensive and well organized. Both the introduction and conclusion of the paper are improved. However, the paper text still needs some important changes before it is suitable for publication.

Major comments

Abstract: The authors change "underestimation" of CDNC to overestimation, but Figure 2 hasn't changed, and still shows that the model underestimates CDNC. I am confused as to why this was changed and why the abstract says the slope is two when it is 0.334.

The last sentence of the abstract still needs changing, in line with the modified conclusions (but see below).

I did not find the promised supplement.

In my previous review, I said:
"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."

I don't feel this comment has been adequately addressed. The authors claim to have separated Twomey effects from microphysical effects, but they only do this in the discussion, not in the abstract or the conclusions. The authors still say "the additional CCN emitted from local fires did not cause a notable change in modelled cloud microphysical properties" in their abstract, but the additional CCN clearly leads to increased CDNC – which is an obvious and observed change in microphysical properties. Again in the conclusions, the authors say "Our model results are in disagreement with observations of microphysical effects at much higher aerosol loading from previous campaigns", and this statement is not at all justified. The simulations clearly do show microphysical effects, but they may not be the same effects as the microphysical effects observed.

In fact, in polluted conditions, if CDNC increases when aerosol concentrations increase, while r-eff does not increase, that means LWC must increase, because of the relation r-eff ~(LWC/N)^0.333. Increasing LWC with increasing CDNC is an aerosol-cloud microphysical interaction, in fact one quite commonly observed in models (e.g. McCoy et al, ACP 2018), and not a saturation of anything. Because it probably is not the same aerosol-cloud interaction as seen in observations, the structure of the paper may not need changing. **However, I really do think the authors should make a much more fundamental change to their conclusions than they did in response to my previous review**. The saturation of the Twomey effect in polluted conditions due to the saturation of effective radius is obvious because of the re~(LWC/N)^0.33 relation, and adds nothing new to our understanding of atmospheric science. On the other hand, other findings in the paper, for example, the testing of the Freud parameterization, the finding that CDNC is underestimated, are legitimate new findings that are worth publishing. The conclusion should be rewritten to emphasise these instead, and the abstract changed to match.

Minor comments

The Reid et al, 1999 paper is highly relevant to this study and should not be brought up for the first time in the conclusion. The authors should discuss the main findings of the paper in the introduction, and put their results more fully in the context of Reid's work in the discussion.

L37 "nucleii"->"nuclei"
L371 "measured my"->"measured by"