Review of "Impact of mixing state and hygroscopicity on CCN activity of biomass burning aerosol in Amazonia"

Sanchez Gacita et al.

The paper presents results from a cloud parcel model used to simulate the activation of aerosol distributions representative of aerosol from biomass burning in Amazonia. The study presents the sensitivity of the activation of three aerosol populations (representative of moderate to high levels of biomass burning) to uncertainties in assumptions of internal / external mixing, hygroscopicity, kinetic limitations and updraft velocity.

Overall I feel that the paper does not make clear why such a study is required. Much of the sensitivities discussed are already known and have been discussed previously in the literature. Also the link to between this work and previous measurement campaigns that have taken place in this region is a little ambiguous. The wider implications of the work for medium to large scale modelling studies is also not clear.

The paper does present some interesting findings; for example the importance of considering kinetic limitations for particles with moderate / high hygroscopicity. That the treatment of an externally mixed distribution as internally mixed leads to biases in the number of aerosol activated is not surprising, but is clearly demonstrated and may be of interest. Overall the paper is fairly clearly written and plots are clear and useful. The paper would benefit from a proof reading from a native English speaker to correct some minor errors.

Main Comments:

1) I find the description of the aerosol distributions considered quite confusing, in particular the explanation of the difference between Ext1 and Ext2 is hard to follow, this is key and should be made clearer.

   As I understand in Ext1 and Ext2 the authors consider two distributions, one more and one less hydrophilic. Both these distributions comprise an Aitken and an accumulation mode, with fixed size and width. Within a distribution the composition (and therefore hygroscopicity is constant). The authors then alter the overall hygroscopicity of the entire aerosol population by considering scenarios in which the total aerosol number (per mode, per case study) is constant but the relative population of the two distributions is altered.

   This is my understanding, but I think the explanation needs to be made clearer.

2) The ramifications of this work for large scale models are unclear. How do the Ext1 and Ext2 scenarios relate to aerosol schemes used in models? The externally mixed setup used in the paper with (i) a more-hydrophilic and (ii) a less-hydrophilic distribution is the same as that used by many global models (e.g. M7, GLOMAP, EMAC). The scientific impact of the paper can be increased by discussing the findings in relation to the treatment of aerosol in large scale models. Is the treatment in existing models sufficient? Similarly, many of the activation schemes suitable for global models treat kinetic limitations, so are already considering the effects found to be important. If this is the case, then why are the findings of this work important for the modelling community?

3) Is it really the case that biomass burning aerosol from the Amazon has a lower Kappa than other wildfire burning? It seems almost identical to the values from Thailand (Hsiao et al, 2016).

4) No consideration is made of coarse mode aerosol particles. Although small in number, the presence of a few large coarse mode particles could potentially affect the kinetics of the droplet activation, with consequence for the number of activated drops (e.g. Nenes et al, 2001). Would the presence of coarse mode aerosol affect the sensitivities presented? Would consideration of these particles increase the sensitivity to relative humidity?

5) Considering the relative simplicity of the model it's surprising the authors didn't consider more of the parameter space. The authors do not consider sensitivity to particle diameter (which is prognosed in most large-scale models) or mode width (which is not). Would consideration of these affect the conclusions? Why was the choice made to limit these effects?

6) The assumption that composition is independent of size seems a limitation, I understand that this is an idealised study, but from the supplement the Kappa of the accumulation mode is around 20 to 30% larger than that of the Aitken mode. Considering that the sensitivity of N_d to Kappa is largest at this very low hygroscopicity regime it is possible that this effect could be important.

Minor Comments:

1. Table 3. Typo in the temperature: 93K.

2. Pg 1, Line 12. Sentence starting "When the hygroscopicity" is confusing, especially phrase "was supposed to be instead"

3. Pg 5, Line 20: Is there a reference for the cloud parcel model?

4. Pg 5, Line 5: References for models assuming equilibrium.

5. Pg 6, Line 11: Moderately not moderated

6. Pg 9, Line3: Condense not condensate

7. Pg 11, Final sentence: This is confusing! I think you need to be clear in term of recommendations for models whether you are considering freshly emitted aerosol, or mixing with pre-existing aerosol. There seems to be no evidence for your conclusion, and the discussion is suddenly extended from freshly emitted aerosol to mixing with continental aerosol from other sources.

8. Pg 15. The word "situations" isn't quite right. Conditions maybe?

9. Pg 15, line 28: hygroscopicity particles.

10. Pg 2, Line 31: Pringle et al didn't assume an average hygroscopicity parameter over a single geographical region. The model used is typical of other global models and has two distributions (hydrophillic and hydrophobic) and an individual value of kappa is prognosed for each of the 7 aerosol modes.

Nenes, Athanasios, et al. "Kinetic limitations on cloud droplet formation and impact on cloud albedo." *Tellus B* 53.2 (2001): 133-149. Nenes, Athanasios, et al. "Kinetic limitations on cloud droplet formation and impact on cloud albedo." *Tellus B* 53.2 (2001): 133-149.