The authors presented a comprehensive study of the hygroscopic properties of organic aerosols at a forest site in Wakayama, Japan using a HTDMA and an AMS. The hygroscopicity parameter of fresh biogenic secondary organic aerosols was estimated and its relationship to CCN concentration was also evaluated. The dataset is rich with substantial amount of information, however, the discussion is over spread that the major conclusion becomes blurry. The manuscript is acceptable for publication in ACP after the following concerns are clearly addressed.

Major comments: In general, the definition or quantification of BSOA and ROA should be clearly clarified and highlighted with proper references in your manuscript, as most of your discussion is based on this assumption. I suggest to make an individual section introducing it rather a few lines, for instance Page 19, line 9-15. Similarly, in your TextS9, you said 'The diurnal variation data on the mass concentration of BC was scaled to represent the diurnal variation of non-BSOA-OA'. How did you prove your method is valid, any references? As I see, there is big uncertainty within the estimation of BSOA-OA concentration from this method, which you used further to calculate their CCN contribution. Please carefully clarify. Also, I see you occasionally have BC peaks, correlated with high CO concentration. You might have biomass burning organic aerosols, how did you deal with those or did you filter their contribution or should we neglect their contribution? Please discuss.

Your RH values are pretty high, which means supersaturation conditions might be possible reached in the real atmosphere. This indicates that your particles, especially large ones (larger than 300 nm) might already activate under supersaturation and then lose water again due to evaporation after RH decreases. This process will strongly affect your results, did you consider this into your discussion.

The correlation between κorg and νLOOA/(νLOOA+νMOOA) is not high, and you used this relation to derive κLOOA and κMOOA, which may introduce even higher uncertainties. I think your analysis should start from the closure between measured κ and ZSR_derived κ. You can replace κorg with κLOOA and κMOOA, and ask your computer to find the best solution for κLOOA and κMOOA and to see if these values are different from those of your current analysis. In addition, I don't understand those error bars in your Fig. 5.

I am not sure about your Section 4.4. You said 'your particles larger than 70 nm are assumed to be CCN active', which means you neglected the effect from the chemical composition. Then you started to consider the effect from chemical composition by dividing the spectrum with BSOA-contribution and contributions from other components, see Page 21, line 17-22. To me, this is a little bit in conflict with each other. Secondly, your Fig. S17 are actually based on external mixing state assumption. For your internal mixing aerosol population, particles are having quite similar chemical composition. I don't see the point that how could BSOA contributes to CCN concentration alone. The logic behind it as I see is the involving of BSOA into organic aerosols will change the hygroscopicity parameter κ, then influence the critical diameter of particles that are able to activate, for instance, not 70 nm anymore, which thus change the potential CCN concentration. If this is true, then your method to derive the contribution of BSOA to CCN concentration is not sound or at least with huge uncertainties. Please carefully clarify. Mei et al., (2013b), who you cited in your introduction, gave a proper way to calculate the CCN concentration due to an elevated κ_{org} in their section 5.2.