Peng et al. overview aerosol hygroscopicity data in China in terms of hygroscopic parameter and CCN activity. This review paper seems to simply list the results reported by earlier work without much interpretation based on atmospheric chemistry. For example, the hygroscopic parameters varied with particle size, chemical composition (primary vs secondary species), seasonal effect, and so on. This paper lacks the discussions on how the variable hygroscopic parameters are related to many factors mentioned above. I recommend the authors to provide more underlying mechanisms about the hygroscopicity characteristic in China and otherwise the paper would be just collection of the results. This current manuscript is not ready for publication and requires major revisions.

The introduction needs improvements. The authors describe their motivation to report the current study with fairly rational information. However, it still lacks why one needs to review aerosol hygroscopicity in China now under what circumstances. Elaborating such points would put this study in better context.

The title would be misleading. It seems to me that this study focuses on aerosol hygroscopicity in China, but not its measurements.

I am aware of the recent review paper on aerosol hygroscopicity (Tang et al., 2019). It is not convincing at all if the authors do not provide any elaboration on how the current study is distinguishable by Tang et al.

Line 97: I do not agree that the single particle studies are too limited for the overall aerosol hygroscopicity. This type of lab studies has proven powerful to establish aerosol thermodynamic models that can provide useful information on the overall aerosol hygroscopicity. It is understood that this manuscript focuses on field measurements of ambient aerosol hygroscopicity, but it needs to rephrase the argument.

Line 200: Can you explain why is there no obvious difference in aerosol hygroscopicity between summer and winter?

In Figure 1, the hygroscopic parameter was always highest in summer and lowest in winter. Photochemical processes and secondary products play a role in this trend. Can you also explain the seasonal difference in the hygroscopic parameter in terms of chemical composition difference? For instance, what is the major inorganic and organic species between two seasons and O/C ratios?

In Figure 2, it is related to the questions above. The hygroscopic parameter increased with particle size, which was attributed to enhanced contribution of secondary species. Can you provide more information on how chemical composition of PM changes with particle size increase?

Lines 319-322: How did the size-resolved mass fractions of secondary inorganic species lead to slight decrease in the hygroscopic parameter in winter as particle size increases?

Line 856: Why there was no size dependence of the hygroscopic parameters of activated particles despite the large values?

Lines 952-955: The logic is not clear. The authors mentioned that If measurement sites were affected by primary emissions, CCN activities could be reduced. However, if so, the averaged hygroscopic parameters can be reduced too. I imagine that CCN activities would be essentially attributable to the hygroscopic parameters. How can the contribution of soot and organics reduce CCN activities while the hygroscopic parameters remain high?

Lines 979-980: What are potential reasons for the consistence and discrepancies?

Minor points:
Line 217: A typo of “exntensive”.
Line 544: One possible reason for what?
Line 619: A typo of “he”.
Line 760: You mean “exceptions”?
Line 823: Please add “respectively”. Were the hygroscopic parameters for organics assumed to be 0?