

Interactive comment on “Tropospheric aerosol hygroscopicity measurements in China” by Chao Peng et al.

Anonymous Referee #3

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The review by Peng et al. is an ambitious study in trying to summarize aerosol hygroscopicity measurements in China. The authors efforts are commendable and will certainly guide the future research efforts (at least in China and beyond). The review is well written and easy to follow, so should be acceptable for publication after addressing the comments.

One major comment is arising from the author's efforts to make a fair summary of all the measurements, but without connecting observations with processes and/or sources. As such, reading the large portions of text becomes boring, because it only mentions facts (easily found in individual papers by concerned readers) without linking or extending scientific knowledge. The review is not only meant to provide a summary of observations (that would be rather a report, not scientific study), but most importantly

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to critically analyse available knowledge and subsequently to identify scientific knowledge gaps. CCN part of the review is written much better, but HTDMA part is lacking interpretation on every page or even more often. Few good and bad examples were noted, but the authors should read their text carefully to recognise the rest.

The second major comment relates to uncertainty analysis and even more importantly taking into account that uncertainty when interpreting the results of various studies. “Smaller” or “larger” is irrelevant on absolute scale, it is only important when the differences are outside the uncertainty range of GF or kappa. When the differences are within the uncertainty range it should be stated accordingly. Therefore, it advised to carefully use the words “different”, “similar” which carry very little scientific significance.

The abstract is currently a very formal structural summary when instead it should be a scientific one, highlighting identified knowledge gaps (perhaps, limiting to the most important ones). It should give a flavour what was uncovered by the review and engage the reader.

Minor comments

Line 164. typo in 0.25, same in next instance.

Figure 1 contains no error bars.

Line 345. It is important for the review paper to give an in depth explanation of the observed phenomenon, not just acknowledge that differences were observed. Diurnal patterns must come from either dynamics of BL, photochemistry or sources, or interplay of the three.

Line 413. Same comment about summarizing observations without linking to processes and sources. Observed bimodality typically means different sources like traffic and secondary aerosol formation.

Line 417. It should be specifically reworded: "It was found that secondary inorganic aerosol species increased hygroscopic growth of accumulation mode while organics

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were decreasing hygroscopicity of the Aitken mode".

Line 429. ...suggesting that ISORPIA-II was not capable to reproduce ALWC at low RH.

Line 437. Same comment on observations versus processes.

Line 488. Again missing comment as to what bimodality and increasing kappa means.

Line 588. Good example of trying to explain the observations and link to composition and sources, not just documenting them.

Line 596. Good example

Line 673. ...or internal/external mixtures of organic and inorganic compounds.

Line 697. Can the reason be discerned? Well mixed aged aerosol removing differences of various sources of origin?

Line 702. organic matter, not materials

Line 726. if evident state the number of higher RH. Was it evident at 90%?

Line 758. ...almost all of the...

Line 766. Not even in summary there is interpretation what multimodal hygroscopicity means in terms of processes and sources.

Line 775. How different? Opposite?

Line 781. However, Meier et al. (2009) found that primary particles smaller than about 50nm in diameter exhibited decreasing hygroscopicity. If I interpreted correctly.

Line 789. The results should be interpreted in terms of processes and sources.

Line 796. That was already stated numerous times, no need to repeat. The paragraph should start with underlying reasons.

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Line 815. If kappa is considered a robust method, it does not matter at which RH GF was measured at, because lower RH would result in lower GF and kappa should be the same. If it was not the same, then that should be highlighted by proper comparison and stated clearly, because that is very important. Not all species exhibit hysteresis and even fewer when internally mixed.

Line 824. NaCl has the highest deliquescence of 75% among the relevant atmospheric species, so the statement should state that no kappa(HTDMA) should be derived below 75-80%. The following Figure is manifesting that, but needs error bars added to data points.

Figure 5. Uncertainty of the calculated kappa is clearly above 10% based on very basic considerations. If one considers size uncertainty of two independent DMA at 10% each and RH measurement which is inherently drifting during HTDMA operation, one would get ~17% total uncertainty. Therefore, no one can objectively claim kappa differences of ~10%, because those will be within the overlapping error bars.

Line 877. was lower, not became lower. There is more to it. Calculated (chemical) kappa is relying on compound specific kappa values, which have uncertainty and without even mentioning rather arbitrary kappa of organic matter.

Line 883. ...while the increase in aerosol hygroscopicity was much smaller due to the change in chemical composition.

Line 897. Was that outside uncertainty range?

Line 1023. ...and both consistencies.... and discrepancies were reported

Line 1033. ...research directions can be proposed.

Line 1042. ...in eastern regions

Line 1046. ...hygroscopicity in the cleaner troposphere. "Pristine" can only possibly apply to remote oceanic regions or Antarctica. Not even Arctic is pristine.

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Line 1069. ...can be easily activated at the lowest supersaturation due to their size.

Line 1074. It should be stated that κ (HTDMA) derivation should be limited to RH above 75-80% due to reasons discussed.

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