**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "On aerosol hygroscopicity, cloud condensation nuclei (CCN) spectra and critical supersaturation measured at two remote islands of Korea between 2006 and 2009" *by* J. H. Kim et al.

Anonymous Referee #2

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The authors present hygroscopicity data from two islands outside the Korean peninsula. In addition to presenting the data, they perform CCN closure studies with various assumptions to test the sensitivity of CCN concentrations with respect to particle hygroscopicity as well as time resolution. The data from the campaigns are highly relevant for ACP publication. However, a number of clarifications and modifications are needed before the paper can be published.

General comments:

The hygroscopicity closure is far from perfect, and looking at Fig. 9, it seems as the slope of the fit is actually much steeper than what is indicated when forcing the linear fit through the origin of coordinates. Why is the GF-kappa so much more constant than the CCNC kappa? This makes me very uncertain of the quality of the data. How was the CCNC and the H-TDMA calibrated, and how often was it calibrated during the campaigns? Without this information it is hard to draw too much conclusions. On Page 19694, row 29 you state that although the two DMA-CCN measurements were based on different concepts the difference between the datasets is too large to be attributed to methodological differences. This is quite important, and I think you leave this question too fast. I would like to see an error propagation calculation, preferably based on calibration measurements, but at least on reasonable assumptions. What is the expected uncertainty in your derived kappa values? Once you have this, you can speculate more freely on possible reasons for the difference.

In chapter 4.2 you state that some of the difference in kappa can be explained by the organic fraction and that you don't see the full hygroscopcity in the H-TDMA. But if you would use kappa values from the CCNC to predict the CCN concentration, wouldn't you over predict the CCN concentration quite a lot? (even more..?) This somehow undermines the argument of organics being the reason for the discreprency in kappa between CCN and H-TDMA, I think. Can you comment on this?

Specific comments:

Page 19685, row 10: "climate change prediction". It is actually uncertainty in radiative forcing.

Page 19685, row 21: "was suggested as a tool". Suggested by whom?

Page 19685, row 26: "Provides a theoretical link..". This is too vague. Explain briefly what is included in the kappa approximation and what assumptions are made (it is basically ideal raoults law + Kelvin effect with water properties).

Page 19687, row 13: "10-300 nm". Does this cover most of the size distribution? It seems like a very small size span. You also state that the sampled air was not dried, but I assume that there were driers in the DMA sheath air?

Page 19687, row 18: "The two CCN instruments were calibrated..". How often were they calibrated, and how much did they drift between calibrations? Same question goes for the HTDMA data.

Page 19691, row 16: "Converted GF90 values were then classified into four categories:" These GF boundaries are tied to a specific dry size. In fact they are based on soluble volume fractions, so for different dry sizes you have to correct for the Kelvin effect. Did you do this? This also has to be mentioned in the text.

Page 19692, row 27: "in the range of 1.4 to 1.7". At which dry size? GFs must always be connected to a dry size.

Page 19695, row 4: "When the results from Wiedensohler et al. (2009) is...". Should be "If the results from Wiedensohler et al. (2009) are…".

Page 19695, row 15: "linear behavior". This is exponential and not linear.

Page19696, row 19: "that significant" should be "that a significant". However, I am not sure how that it is possible to explain the difference just by strange behavior of the organics. I would like to see a test calculation, based on some "extreme" but still realistic assumptions regarding the particle properties (basically it is solubility and surface tension effects that comes to mind).

Page 19697, row 9: "measured Nccn are compared" should be "measured Nccn were compared". Stick to one tense (in this case past tense).

Page 19697, row 11: "data, CCN" should be "data, the CCN".

Page 19697, row 12: "because size" should be "because the size".

Page 19697, row 12: "All aerosols were assumed to be internally mixed". How do you

define internally mixed? Describe in the methods your way of evaluating the H-TDMA data. There are a number of different ways to do this.

Page 19699, row 8: "the temporal variation of the size distribution was taken into account as in "Method 3"". Isn't the temporal variation of the size distribution taken into account in all approaches?

Page 19699, row 12: Delete "(not shown)".

Page 19699, row 18. Delete "goodness". "accuracy" is a better word.

Page 19700, row 24. "Kammermann et al. (2010) have found that" should be "Kammermann et al. (2010) also found that"

Page 19702, row 10: "size-resolving" should be "size-resolved".

Page 19702, row 10. I think successfully predicted is quite a strong statement. An error of 28% is neither great nor terrible, at least if you compare to old closure studies (see e.g. Kammerman et al. 2010).

Page 19702, row 16. You should state if it is an under-prediction or an over-prediction of the CCN concentrations. This must also be clear in table 5, as well as in all other places of the manuscript (e.g. the abstract)

Page 19702, row 20. "as global" should be "as a global".

Figure 4. Explain what the error bars represent.

Figure 5. Explain what the error bars represent.

Figure 6. This figure can be deleted. Time series are very hard to interpret, and should only be used if there is a special feature that should be highlighted, and I do not see what that is in this case.

Figure 7. The staples do not seem to add up to 1, which they should, why is this?

Figure 8. can be deleted on the same note as Figure 6.

Figure 10. Explain what the error bars represent.

Figure 11. Explain the box-plot, what limits are assigned to the boxes and what are considered outliers?

Figure 13. The x-axis' are CCN concentrations from the CCN counter I presume (not modeled values)? Please clarify this in the caption.

I would like to see a table which explains what CCN models include what assumptions. This would be very helpful as a compliment to the text when looking at the figures and trying to interpret them..

References: Kammermann, L., Gysel, M., Weingartner, E., Herich, H., Cziczo, J., Holst, T., Svenningsson, B., Arneth, A., Baltensperger, U., Subarctic atmospheric aerosol composition: 3. Measured and modeled properties of cloud condensation nuclei, Journal of Geophysical Research, vol 115, D04202, doi:10.1029/2009JD012447, 2010.

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