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Interactive comment on "Relating particle hygroscopicity and CCN activity to chemical composition during the HCCT-2010 field campaign" *by* Z. J. Wu et al.

Anonymous Referee #2

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In this paper the authors attempt closure between measured composition and hygroscopicity from both a HTDMA and CCN instrument from a mountain range in central Germany. The general scope of the paper is within the remit of ACP. However, given the authors attempt to discuss reasons for observed discrepancies, there are some recent developments that have not been discussed in any detail and need including in the manuscript. This is especially important since the paper attempts to prescribe a basic link between measured O:C ratio and representative kappa values for the organic fraction, which similarly is not put into wider context.

With this in mind, the following general points should be considered before publication

is considered.

The abstract line: 'This difference might be explained by the surface tension effects, solution non-ideality, and the partial solubility of constituents or non-dissolved particle matter. However, due to these effects being included in HTDMA-derived calculations, we could not distinguish the specific roles of these effects in creating this gap' is a little confusing. This first suggests the authors believe that using k from the HTDMA is sufficient to acount for these additional properties in explaining the discrepnacy between HTDMA and CCN derived k values. On the other hand, the authors recognise in the manuscriot that care must be taken when assuming extrapolation of this value to the point of activation is valid. Alternatively it suggests an analysis of the range of values representative of each process has been explored, which is not the case. I presume the text is referring to the implicitly coupled interaction of all effects combined at 90%RH? Ignoring the surface tension effect at 90%, which is generally valid, it would still be useful to assess what values could be prescribed to the reamining effects to achieve closure and then put this in a wider context of expected values.

Page 7658, line 15. 'Because the uncertainty in the estimation of Korg decreases with increasing organic fraction (Duplissy et al 2011)....only data with NH4NO3 volume fraction below 10% are considered'. On the contrary, this needs to be put in context of more recent discussions regarding the potential effect of semi-volatile partitioning which the authors already touch upon through a consideration of NH4NO3. The authors refer to empirical studies that show a discrepancy between HTDMA and CCN kappa. Recently Topping and McFiggans (2012), in referring to these studies, discussed the potential effect of organic semi-volatile partitioning on hygroscopicity closure. If you use only data that had evidence of organic fractions larger than 50%, what are the potential impacts of this process? Furthermore, would this then impact on attempting to correlate O:C ratio with k?

Topping, D. O. and McFiggans, G.: Tight coupling of particle size, number and composition in atmospheric cloud droplet activation, Atmos. Chem. Phys., 12, 3253-3260,

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Section 4.4 This needs to include the more recent discussion of semi-volatile loss with regards to closure discrepancies between k-htdma and k-ccn. When the authors discuss non-ideality effects, presumably this relates to the notion that deviations from ideality impact the concentrtion of water, and not the organic solutes? Can the authors predict what level of deviation would need to account for the measured discrepancy by running a simple Kohler model? How much more 'soluble' organic mass would be needed to corroborate the measured k-CCN values?

In figure 7, this study shows a significant difference in the trend of O:C ratio versus kappa as compared with the Jimenez and Duplissy results. For heavily aged aerosol, the differences could be significant could they not? Also, which K do the authors recommend in fitting this function if there is discrepancy between the HTDMA and CCN value? Please adjust the figure caption accordingly. How does this fit in which recent studies suggesting there is no correlation between O:C ration and hygroscopicity (Alfarra et al 2013)? Please include a wider consideration.

Alfarra, M. R., Hamilton, J. F., Wyche, K. P., Good, N., Ward, M. W., Carr, T., Barley, M. H., Monks, P. S., Jenkin, M. E., Lewis, A. C., and McFiggans, G. B.: The effect of photochemical ageing and initial precursor concentration on the composition and hygroscopic properties of β-caryophyllene secondary organic aerosol, Atmos. Chem. Phys., 12, 6417-6436, doi:10.5194/acp-12-6417-2012, 2012.

Minor comments:

Page 7653, line 4. Which individual species?

Page7654, line 9: fraction of what? Mass?

Page 7652:, line 15. As well (as the concentration of) ?

Page 7656, line 17. (r^2=0.?)

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