We would like to thank both the reviewers for their positive comments and helpful suggestions. We plan to make a number of changes to the manuscript based on their recommendations, as outlined below.

Referee #1

J.D. Whitehead et al. present an interesting reconciliation study for particle water uptake issue based on a large number of datasets from different locations. Such work helps us to better understand measurements of sub-saturated and supersaturated aerosol water uptake and their discrepancies. This manuscript represents a substantial contribution to scientific questions and is within the scope of ACP. I recommend this paper for publication in ACP, but not in this version. The following comments and suggestions should be addressed.

(1) It is good that the paper is written concisely. But as a full-research paper, more information should be added in the text. I would like to see more and better documentation of data and results before jumping into the conclusions, especially, the aerosol size distributions, chemical compositions and mixing state. In this paper, Nccn is derived from aerosol size distribution and D50. D50 is determined mainly by aerosol mixing state and chemical compositions. Nccn highly depends on the shape of aerosol size distribution. We can see in the supplementary material that there is very large variation for Nccn among different datasets. I do suggest the authors should present a comparative compilation of the properties of aerosol size distributions from all the locations in this revised manuscript. Also, it would be very interesting to consider the dataset from such heavily polluted region for these reconciliation studies of particle water uptake.

The referee is correct that more results would benefit the paper. As requested, we will add a figure to the manuscript depicting the mean size distributions (plus their variability) in each campaign, and discuss the impact on NCCN and reconciliation.

(2) Regarding the kappa calculation, differences in hygroscopicity with HTDMA and CCNc can arise through solution non-idealities, the presence of slightly soluble or surface active compounds, or non-spherical particle shape. The magnitude of this difference and its dependence on particle size is consistent with the presence of surface active organic compounds. In other words, the role of some organic compunds in particle water uptake processes is different for the conditions of sub-saturation and supersaturation. I suggest the authors add more discussions on this in the revised manuscript.

The reviewer correctly draws attention to a number of issues that should be included in the discussion. The solution non-ideality does not remain constant as RH increases towards 100%, yet is assumed to in the kappa parameterisation, so it might be expected that this could give rise to discrepancies. The instrumentation used in these studies is not capable of investigating this effect sufficiently (see Good et al 2010), but recent developments (Suda et al, 2013) could make this possible. Similarly, slightly soluble compounds behave differently at sub and supersaturated conditions (Dusek et al 2011). In all the experiments, the aerosol is sampled through the same dryer for the HTDMA and CCNc, so shape factor should be the same for the dry size selection, however subsequent water uptake may differ below and above saturation. While it is beyond the scope of this study to fully investigate all these factors, they certainly merit further discussion, as the reviewer points out, and we will expand on these issues in the revised manuscript.

Referee #2

This is a nice short study describing reconciliation between measurements at subsaturated and supersaturated conditions over a wide range of locations, and gives quite good feeling of problems in the research area. The study highlights better agreement at larger super-saturations and provides some explanation for that, although, to my opinion, not sufficient. Despite some lack of discussion, this paper is nicely written and brings new insights into this quite complicated area; therefore, I would favour a publication in ACP after revisions listed below.

General comments: As mentioned above, my major problem is the lack of discussion as if the space limitation was an issue here, which is not the case, therefore, additional discussions should be included where appropriate:

P3, L258-263: The explanation of poorer reconciliation due to lower counting statistics at lower super-saturations is not very convincing, this should introduce a scatter, but not bias. To my opinion, a bias in estimation of the particular supersaturation is more likely to be the reason, which would be more pronounced at very low supersaturation (SS). Furthermore, the temperature stabilization in CCNC could be problematic at these SS. I understand that particular SS's were calibrated for each campaign instead of using standard DMT SS (it should be mentioned in the method section in addition to the references to previous papers), which should reduce the bias, but I don't see any other reasonable explanation. More discussion on the actual cause should be included.

We agree with the reviewer that our explanation for the poorer reconciliation at lower supersaturations does not explain the bias that is seen (just the larger errors). The reviewer is correct to note that we use particular SS (and we will expand on this in the methods section as requested), but this should eliminate the bias. We will expand on this discussion, but do not have a full explanation for the bias observed.

In addition, the reduction in the difference between the two HTDMA's (1 and 2) with increasing SS should be explained and discussed, could these two issues be related?

The difference was not statistically significant (see next point), however both HTDMA reconciliations show the same bias at low SS (which was significant) as in a number of the field measurements (discussed in the previous point). This is indeed worth mentioning, and we will add this observation to the discussion.

Figure 3. The difference between the two HTDMA instruments is significant and merits an explanation in the manuscript, different residence times? Design issues, something else?

As mentioned, the difference between these two instruments was not found to be statistically significant, and the figure shows both to be within each other's standard deviation. The purpose of this intercomparison exercise was to test the relative behaviour of the two instruments in a reconciliation study as a function of supersaturation, in order to rule out any bias within the datasets as a whole. As such, we feel this figure belongs in the supplementary information and will move it there for the revised manuscript. Fuller intercomparisons between different HTDMA instruments have been performed in the past (e.g. Duplissy et al, 2009). We will mention these, but it is not in the scope of this study to do such an intercomparison.

P4, L310 this gives an impression that all studies cover the whole period, however, quite short periods of time are covered by each campaign, ranging from few weeks to ~2 months, and this should be specified. Which also suggest that conclusion in L279-285 and 310-313 should be toned down… Yes, the range of locations is impressive, however, the short term of observations could limit the type of aerosol measured during the campaign. This is one of the most important conclusions in the paper, so requires more detailed discussion and comments. It should be illustrated by specific examples. Give details and references how the campaigns covered in this study are representative of the location.

We thank the reviewer for pointing this out and will clarify this in the revised text. We will add a paragraph or two to the text to detail how the datasets are distinct from each other with regard to the aerosol population. We will do this with the aid of a qualitative discussion of measurements with the HTDMAs, plus the mean size distributions for each dataset, which we are inserting in response to a comment from the other reviewer. For example, the measurements at Mace Head (marine) showed a strong sea salt signal, whereas the London measurements (urban) showed an externally mixed aerosol with hydrophobic and hygroscopic modes. The point is not that the data were necessarily representative of the specific locations, just that they were sufficiently distinct from each other so as to examine whether this has any bearing on the reconciliation. We accept that this has not been conveyed sufficiently and will clarify.

Space is not limited here; therefore, more elaborate discussion on extreme cases (eg. London winter and Chilbolton at lowest SS) should be included and reasons for a bad reconciliation explained.

We will add some brief discussions on any datasets, such as Chilbolton, that seem to stand out in the reconciliation studies.

Specific comments: L156-158: Which calibrations? Why disagreed? More details needed. Figure 1. Why this figure includes locations that were not discussed in this manuscript? Figure 2. Explain shaded areas in the caption. D319 cruise is the Discovery cruise? Be consistent.

Clarifications on all points will be added to the revised manuscript, and we will remove the locations from figure 1 that weren't included in the study.

References:

Duplissy et al. (2009), Intercomparison study of six HTDMAs: results and recommendations, Atmospheric Measurement Techniques, 2, 363–378

Dusek et al. (2011), Water uptake by biomass burning aerosol at sub- and supersaturated conditions: closure studies and implications for the role of organics, Atmospheric Chemistry and Physics, 11, 9519–9532

Good et al. (2010), Consistency between parameterisations of aerosol hygroscopicity and CCN activity during the RHaMBLe discovery cruise, Atmospheric Chemistry and Physics, 10, 3189–3203

Suda and Petters (2013), Accurate Determination of Aerosol Activity Coefficients at Relative Humidities up to 99% Using the Hygroscopicity Tandem Differential Mobility Analyzer Technique, Aerosol Science and Technology, 47, 9, 991-1000, 10.1080/02786826.2013.807906