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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distribution of the effective aerosol hygroscopicity parameter for CCN activation" *by* K. J. Pringle et al.

K. J. Pringle et al.

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Response to Reviewer #1

We thank the reviewer for the useful comments and we respond to the specific comments below.

page 6305, line16: a "to" is missing: "it is sufficient to avoid : : :"

Added

page 6305, line18: I'd think it's "fresh" instead of "freshly"

Changed to: "freshly emitted sea spray"

page 6306, line 17: replace "manuscript" by "work" or "study" Done

page 6307, section "Comparison with observations": Recently, a paper was published at GRL (Wex et al., 2010, currently in press), in which kappa was derived for a large number of measured hygroscopic growth factors (taken from compilations of globally measured values). The average rural kappa given there is 0.24, comparing well with your values, while that for marine aerosol is lower than yours (0.45 for the majority of the more hygroscopic particles – if the sea-salt mode was included in the averaging, it would be 0.51). Include this paper in your comparison and comment on that difference for the marine values.

Added a paragraph: Section 3.1

"A recent study by Wex et al. (2010) re-analysed data from AMS and HTDMA measurements taken in a range of locations and presented typical Kappa values for different conditions, making the distinction between more hygroscopic and less hygroscopic modes that can be distinguished by the HTDMA. They found Kappa values of 0.3, 0.24 and 0.45 for urban, rural and marine aerosols in the more hygroscopic modes and 0.02, 0.04 and 0.08 in the less hygroscopic mode. Urban and rural values agree well with the model simulated mean continental value, but the Wex et al. (2010) data suggests a lower marine Kappa than that simulated by EMAC and that suggested by Andreae and Rosenfeld (2008). This is partly due to the fact that our marine value also includes data from the Southern Ocean where Kappa values are large (> 0.9, Figure 1) whereas the marine data analysed by Wex et al. (2010) is often in continental outflow regions where values of 0.5 to 0.7 are more common. Furthermore, it is conceivable that we underestimate the effects of the organic fraction in sea salt particles, which acts to reduce Kappa."

page 6308, line 5-6: This sentence isn't complete.

Changed to: "Considering that the two approaches used to estimate the average continental and marine values (Andreae and Rosenfeld, 2008, and ours) are completely different and independent, we find the overall agreement between the two studies quite

good."

page 6310, line 7-8: This does likely not only reflect the more mixed nature, but also the more aged one (where SOA and sulfates have kappa below that of sea salt). You say that, implicitly, by mentioning the reduced influence of sea salt, but I'd just like to read the word "aged" here somewhere.

We have replaced the word "mixed" for "aged". It now reads:

"Marine values are significantly lower above the surface layer; values > 0.9 do not occur and the global mean marine Kappa value is reduced to 0.60 (from 0.72), reflecting the more aged nature of the aerosol distribution and the reduced influence of sea salt."

page 6312, line 1-4: It is certainly true that the monthly averaged marine and continental data show no trend because different locations cancel each other out. But I don't agree with you implication. You'd need to look at many more separate stations to be able to say that. For places where you looked at two sites (e.g. central Europe and the Carribean), the shown trends agree. There was also a strong trend at the "Seattle/Vancouver"-site. So it is possible (and likely) that non-negligible seasonal trends could be found for mid-latitudes sites, which then should not be lost in averaging. So please change that sentence accordingly.

We agree that this implication is not fully substantiated. We have removed the line "implying that seasonal variability may be less important over a large scale than the site specific variability"

page 6314, line 9-10: As said above: I am not convinced that it is only N. America and the N. Atlantic where long-term-measurements are needed. To me, this also seems to hold for Europe (site 10 and 11), and with no examined sites in the mid-latitudes in Asia it likely implies that seasonal variations occur in regions with pronounced differences in the seasons, i.e. the mid-latitudes in general.

The paragraph has been altered and now reads:

"In most regions the standard deviation of Kappa due to time variability is small compared to the deviation due to variability within the region implying that, if a choice has to be made, it is better to focus analysis and measurements on the regional (rather than temporal) distribution of Kappa. N. America, however, has a more pronounced seasonal cycle than the other regions – Kappa values range from 0.4 to 0.2 with a minimum in June and July. A similar cycle is found in the N. Atlantic. In these regions the standard deviation due to variations in time (t5) is larger than that in space (x, y), for example in N. America St Devx,y = 0.05, St Devt5 = 0.07. In Europe, the standard deviations in space and time are of a similar magnitude (St Devx,y = 0.08, St Devt5 = 0.05), thus temporal variability in this region can also not be neglected. This analysis implies that longer term measurement campaigns are particularly required to characterise Kappa in N. America, N. Atlantic and Europe, although every region shows some variation due to the annual cycle, thus long term measurements would be advantageous."

page 6318, line 5-6: You need to give the units for the "Delta Sc" (i.e., % - isn't it "absolute" here, while it is "relative" just a line later (the 17% decrease))

We have added a % sign after the initial use of the word critical supersaturation (line 3, pg 6318).

We are reluctant to put a % sign after the magnitude of the absolute change in Sc (line 5) as it may be misleading.

page 6319, line 8: Who thought that? Certainly not me! And I know many others who didn't. So: Delete that half sentence, or give a citation.

Deleted

References: Each reference is followed by a number ranging in the 6000. Just make sure these numbers will be gone in the end.

I will check with the production office.

Also: you cite several ACP-discussionpapers. As these papers are still in their review process, and as some ACPD-papers never get published in ACP, I advise you to not cite these papers unless they are published at ACP prior to your publication.

All now in ACP, except:

The hygroscopicity parameter (Kappa) of ambient organic aerosol at a field site subject to biogenic and anthropogenic influences: Relationship to degree of aerosol oxidation R. Y.-W. Chang, J. G. Slowik, N. C. Shantz, A. Vlasenko, J. Liggio, S. J. Sjostedt, W. R. Leaitch, and J. P. D. Abbatt Atmos. Chem. Phys. Discuss., 9, 25323-25360, 2009 We cite this paper as we use the data collected in the field campaign (site 14). As so few measurements of Kappa are available it is worth also including this data.

Table 2: There is an inconsistency in the numbering of the sites between the manuscript and the supplement (manuscript goes from 1 to 14, supplement misses the 7 and goes to 15) – also: some few kappa values in the supplement version have 3 digits, while most others only have two. Just use two digits everywhere.

Changed

Figure 7: It is enough to show the plot up to a size of 250nm – the important coloured lines could more easily be distinguished, then.

Done

Supplement: It would be nice if you could add a plot showing the comparison between observations and your results for both, the data in the paper and the supplement (I quickly tried it, file attached, and I found it rather instructive). This could be added to the supplement. It more clearly shows, that using only the CCN-active particle fraction overestimates kappa, a result that is also related to the work done in the above mentioned paper by Wex et al., 2010. You should comment on the implications in the introduction of your supplement.

We thank the reviewer for taking the time to add this useful plot. We have added it to

supplement, along with a brief discussion:

"In Figure 3 we show the comparison between modeled Kappa and observations for the two different methods of calculating Kappa: (i) only CCN active particles are considered and (ii) CCN active and inactive particles are considered. When only the hydrophilic (CCN active) aerosol are considered, Kappa is generally overestimated and the correlation is quite poor (R2 = 0.34), this overestimation is reduced when both hydrophilic and hydrophobic modes are considered, and the correlation improves (R2 = 0.53). This comparison supports the findings of Wex et al. (2010) who re-analysed AMS and HTDMA measurements and found that neglecting the hydrophobic aerosols in calculation of for CCN calculations leads to an overestimation of CCN concentrations."

(by the way: the literature list here ends with ": : :" –delete this).

I will contact the production office and check this is removed.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6301, 2010.