

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

### **Anonymous Referee #1**

Received and published: 1 May 2010

This study is a timely and good (if not even necessary and awaited) contribution to the characterization of the global atmospheric aerosol. It gives particle hygroscopicity in terms of the hygroscopicity parameter  $\kappa$  derived from the ECHAM/MESSEy Atmospheric Chemistry model and describes its global distribution, and also (but less detailed) variations with heights and time, and a comparison to respective values calculated for pre-industrial times. It is well written and comprehensive. I enjoyed reading it!

There are, however, a few minor (mostly technical) details that need to be addressed (see specific comments). Once that is done, I recommend publication.

Specific comments:

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page 6305, line16: a “to” is missing: “it is sufficient to avoid . . .”

page 6305, line18: I’d think it’s “fresh” instead of “freshly”

page 6306, line 17: replace “manuscript” by “work” or “study”

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.

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

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.

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. (by the way: there is a “be” missing in the end of line 4, between “may” and “less”)

page 6314, line 9-10: As said above: I am not convinced that it is only N. America and

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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.

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))

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.

References: Each reference is followed by a number ranging in the 6000. Just make sure these numbers will be gone in the end. Also: you cite several ACP-discussion-papers. 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.

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.

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.

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. (by the way: the literature list here ends with “. . .” –

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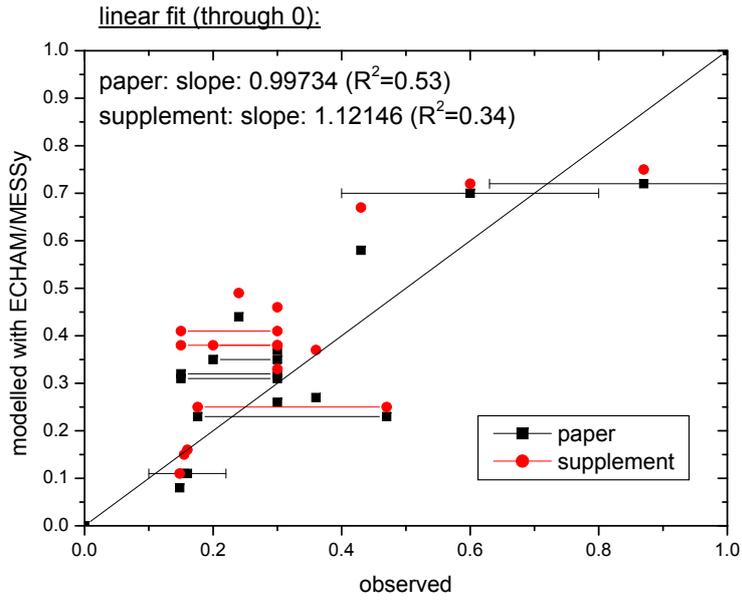
delete this).

Literature: Wex, H., G. McFiggans, S. Henning, and F. Stratmann (2010), The influence of the external mixing state of atmospheric aerosol on derived CCN concentrations, *Geophys. Res. Lett.*, in print, 10.1029/2010GL043337.

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

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**Fig. 1.** Comparison of observed and modelled kappa values from your manuscript and from the supplement