Atmos. Chem. Phys. Discuss., 13, C1094–C1097, 2013
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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Chemical composition and hygroscopic properties of aerosol particles over the Aegean Sea" *by* S. Bezantakos et al.

**Anonymous Referee #1**

Received and published: 8 April 2013

In short: Paper concentrating on the AMS (plane) and HTDMA (ground) measurements over a short campaign around the Aegean sea. Well written paper, main comments on the representability of the data sets and some of the assumptions in the used hygrosopicity model. Minor corrections.

The page and line numbers refer to the print version of the document (if that matters).

** HTDMA measurement explanations in on pages 5813-14.

I find the correction for 30% RH a little superfluous, but of course this can be done, if the authors find a need. It could be interesting to know does it actually matter much to correct from 30 to 0 % RH. Perhaps this was tested as well?

A more interesting is the choice of TDMAfit routine, which I find is a little dated. For



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standardization purposes in contrast to other studies of HTDMA datasets, I recommend the authors to consider using the HTDMA inversion methods from M. Gysel et al., 2009, which the authors should have ready access via shared authors (or just asking Dr. Gysel for access to http://aerosolsoftware.web.psi.ch/).

** 2.1.1. Airborne measurements

In general the flights were clearly in the boundary layer, but I would like to know if the 2300m flight sets were well above it (most likely)? Partly this could be got from the measurements themselves, but more independent measure could be to use (freely available) re-analysis data sets.

** size distributions and hygrosopicity (pgs. 5817-5818)

I am a little concerned that no view of the size distributions are shown. This is especially important as the location of the Hoppel gap between Aitken and accumulation mode could affect the growth factor detected. This is very much connected to the Figure 4, as the text explains that the GF measurements were done by setting the dry aerosol diameter to match the dominant mode of the distribution. From the data, it seems that most of the time this was the accumulation mode? This is very relevant, as making any kind of conclusions of the hygroscopic parameters of the Aegean sea, it might be that the selection of the dominant mode for analysis (only) will not give a very fair picture of the changes in hygroscopicity of the aerosol on other sizes. Please comment on this, especially considering that the accumulation mode particles (>100nm or so) will most likely have gone through at least a few cycles in the clouds in contrast to Aitken mode particles.

Perhaps one could also think that giving an "average GF" on line 18 of page 5818 is misleading. Average of what? Do you think the sampling is fair in the sense of the particle populations? ..and I would change the abstract to have a comment that the "..particles in the dominant mode were internally mixed..."



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Regarding size distributions and kappas in general: The paper would benefit on some more comparison to the long term measurements on the area (from Lemnos site or from the literature). This is because right now it is very hard for the reader to see if the situations observed during the campaign are very common, special for autumn conditions or very specific case. Any relevance on the other papers and studies would make this much easier for the reader.

The section 3.1.3. mentioned that during the comparison periods there was no difference in the particle composition on the size measured. Perhaps it could be good to remind the reader which sizes the AMS on board can measure, and more importantly was such homogeneous composition also the case over the whole aircraft campaign, at all altitudes. The reported kappas could be size dependent, and I could not find any information on the size distribution changes over the flight campaign route, even tough I understand that AMS is not really a good SD measuring instrument. The kappas themselves are interesting of course, but removing the size information can lead to somewhat biased idea which particles had which kind of hygroscopicity,

*** Table 1 and pg. 5815 Are the values in table 1 from the Duplissy et al or Petters and Kredenweis? In this case, it should be also mentioned in the table caption, not only in text. The text on pg 5815 ln 23-27 are ambiguous as they are reported now. Are you referencing table 1 of this paper or table 1 of the reference?

*** figures

Figure 2: please keep the captions self-explanatory: add information on which dates this plot is relevant for.

*** closure

Use of k_org of 0 for organics is quite interesting for (presumably) aged organic aerosol. Do you think that this a reasonable suggestion for aged organics? McFiggans et al, 2006, suggest something in the order of 1.07–1.14 for SOA. On your side, a recent



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paper by Prisle et al, 2011, seems to use similar assumptions. Some discussion could be nice.

Were there any specific measurements of refractory aerosol on the plane or on the site? This is because the closure was done with the AMS measurements, and large (although unlikely) fraction of EC on the samples might tip the modelled kappas somewhat.

Refs:

M. Gysel, G.B. McFiggans, H. Coe, Inversion of tandem differential mobility analyser (TDMA) measurements, Journal of Aerosol Science, Volume 40, Issue 2, February 2009, Pages 134-151, ISSN 0021-8502, 10.1016/j.jaerosci.2008.07.013. (http://www.sciencedirect.com/science/article/pii/S0021850208001778)

McFiggans et al., Atmos. Chem. Phys., 6, 2593–2649, 2006 www.atmos-chem-phys.net/6/2593/2006/

Prisle et al., Atmos. Chem. Phys., 11, 4073–4083, 2011 www.atmos-chem-phys.net/11/4073/2011/ doi:10.5194/acp-11-4073-2011

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 5805, 2013.