Atmos. Chem. Phys. Discuss., 10, C4746–C4751, 2010
www.atmos-chem-phys-discuss.net/10/C4746/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "13-month climatology of the aerosol hygroscopicity at the free tropospheric site Jungfraujoch (3580 m a.s.l.)" *by* L. Kammermann et al.

Anonymous Referee #1

Received and published: 2 July 2010

General comments:

The paper presents long-term hygrosopicity measurements from a mountain site in Switzerland. The data is of importance as they are the first measurements of their kind to be conducted over a whole year at a site in the FT. The measurements and data analysis were additionally made in a more standardized way than previously possible. The article is well written and the results are presented in a concise manner. I recommend it for publication in ACP, however my main questions concern the interpretatition of the results as outlined in the specific comments below. These issues should be addressed before ACP.

Specific comments:

All the data presented in figures 2, 3, 4 and 6 are averaged over a month or more. But it is nowhere discussed how large the variation inside one month was. The annual mean of 265nm particles was 1.46, the monthly mean range is around 1.41-1.54, but the reader is left without any picture of the daily variability. For CCN activation and climate impact (which the authors also discuss), it is important to know if variation is e.g. +-0.05 or +-0.3. Please add some discussion on this.

An underlying theme in the paper is that the authors present data over a year, show that there are differences, and finally conclude that they are within natural variations and no real trends can be found. It is not clear to me that this is the case. Additionally, if a difference in GF of 0.1 between two months is reasoned to be in the natural variability, the variability could also go in the other direction and the trend would be very strong. More comments on this issue can be found under the specific comments.

Appendix B could be moved to online supplementary material instead of an appendix, as some of the same data is plotted in figure 5, and the data is only briefly mentioned in the article. This would make the resulting paper shorter and possibly more concise.

Page 13574, line 11: Add "the hygroscopicity parameter kappa" or similar to at least give some information what you are talking about also to readers not previously familiar with kappa.

13575, 23: A one month measurement can give a fairly good representation on diurnal cycles, at least for that period. The 13 months of measurement only cover one year (seasonal cycle), so using the authors definitions, even this measurement is not long enough to "provide representative information" on seasonal cycles as only one was measured.

13576, 23-25: Some short discussion is needed on the effect of sampling at 25C when ambient air can be 30+C lower.

13578, 8-9: The sample flow should not affect the size, only the spread. Please explain.

13580, 17-21: So how predominantly horizontal is the advective weather class if the mountains cause much vertical movement? Will this not impact on your analysis and conclusions?

13582, 2: This is a very broad range of GFs. I would like to see more discussion of these very different particle types.

13582, 2-3: "Small particles efficiently acquire hygroscopic material through condensation". Compared to what? Large particles acquire material even more efficiently. Do you mean in relation to their original mass?

13582, 3-6: "Nearby emissions of small non-hygroscopic particles are too low to show up as a distinct non-hygroscopic mode at D0=35 and 50 nm." But there are emissions like this nearby? There is also two modes in both the 35 and 50 nm total data, although this is not mentioned. The discussion on the smallest sizes should be thought through and presented better than just saying that differences are minor and due to random variations. The difference between autumn and summer/winter for 50 nm particles is so pronounced that if it is only waved of as minor, then the whole point of this study becomes questionable.

13582, 26: The fact that you see no pronounced seasonal trend of the mean GF (as mentioned also in the abstract) is perhaps true, but as discussed some lines earlier, the shape of the GF-PDF does change, and this can be very important for e.g. CCN activation. I would expect more SOA to be present in particles in the summer, and more non-hygroscopic particles from increased heating or traffic sources in winter. Your data seems to indicate this (Fig. 2), but still your main conclusion is that all seasonal variations are likely only random. Related slightly to the previous, later you state that kappa=0.24 is a good approximation for any model, but even the 75th percentile is already ca 0.35 for 265 nm particles. Can you still imply that kappa=0.24 for all particles is a good approximation for "any" model? Not for a model predicting CCN, at least?

13583, 6 and 13: Monthy mean values go down to 0.17, but fall within 0.3+-0.1???

13583, 3: As a motivation for assuming that most differences between months are not real seasonal variations, but just natural variation, is that May 2008 and 2009 do not agree. Have the authors checked for reasons of the difference between these months? If, let's say, the difference is due to May 2008 being colder than May 2009, then the seasonality may still be a reason for the difference between summer and winter months. Similarly, can the high GF in September be explained by other meteorological parameters? This data should be checked, and whether or not something correlates, it should be mentioned in the text. This should be an easy task since the data is surely measured at the station.

13584, 1-7: Would it have been better to define a limit in kappa space instead of GF space? In this way the Kelvin effect would not affect the limit for the smaller particles.

13586, 21-22: You should be careful to talk about indirect climate effects based on measurements at one site. Especially when you are assessing the importance of particles formed more than 1000km away from your site.

13587, 6: Again, you should be very specific in the presentation when talking about climate effects of Saharan dust measured at one site in Switzerland. But I do agree on the previous two points, that you have the instrumentation and methods to assess the impact on indirect and direct aerosol effects. Only keep in mind also the geography.

13587, 19: Are you talking about new particle formation in the FT?

13587, 22: Your CA weather class is defined as subsidence, and CC as lifting. It is unclear to me why subsidence leads to mixing from below, as this should be the complete opposite.

13588, 5: Point out that the information is in an Appendix, unless you move it to suppl. mat.

13589, 4: I have not agreed with the authors on how big differences are needed to

be statistically relevant, but writing that the total and FT data "are equal" is definately wrong.

13589, 20-25: If I understand the methodology correctly, you choose 88 (based on Fig. 5) days out of more than 200, and use this data to define that the site is representative over a regional/continental scale. And the days that are left out are the ones that by definition are most likely to not be representative. If so, then this should be pointed out very clearly also in the conclusions and abstract.

13590, 3-4: Again, the data are not equal. As you also state later on rows 10 and 13. And I think the lower GF of 50 and 110 nm particles from south an east is large enough to mention.

13592, 23-25: Have you not earlier stated that for example SDE:s are hardly noticed by the HTDMA, but have a large impact on scattering? Additionally, one needs to assume that kappa works perfectly (which it does not, as has been shown in other studies) to extrapolate data at 90% RH to all ambient conditions. I feel that this last sentence promises too much unless you can shortly show it in the results.

Technical comments:

13575, 5: Remove "to that"

13575, 20: Move "and references therein" after the reference

13576, 18: To be clear, mention that 550nm refers to wavelength and not particle size.

13577, 20: gauges

13578, 20: Remove parentheses around reference

13578, 25-27: The fraction is not obtained by integration, only the number.

13580, 7: And CET=local time?

13580, 17: are->were

13581, 13-16: "Most parts of the components were stable...most parts worked reliably...". Please remove one.

13581, 19-20: All these numbers are not important. It should be enough to say that 10100-10600 spectra for each dry size were measured.

13584, 11: Should be 3c and 3d?

13586, 5: This was investigated previously using...

Fig 2 legend: What does "all seasons, oly strong SDE" mean?

Fig 2 y labels: State the units.

Fig 3. Half the figure is left empty of data to show different legends/text boxes. This makes the data hard to read. The legend can be made 2x3 instead of 6x1, and the text boxes can be at least decreased in size.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 13573, 2010.