Atmos. Chem. Phys. Discuss., 15, C4948–C4951, 2015
www.atmos-chem-phys-discuss.net/15/C4948/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical profiling of aerosol hygroscopic properties in the planetary boundary layer during the PEGASOS campaigns" *by* B. Rosati et al.

Anonymous Referee #3

Received and published: 15 July 2015

Review of Vertical profiling of aerosol hygroscopic properties in the planetary boundary layer during the PEGASOS campaigns by B. Rosati et al.

This manuscript describes interesting an unique results from measurements of aerosol hygroscopicity, supplemented by particle composition measurements of non-refractory and black carbon components. The measurements were made from an airship, which allowed careful evaluation of particle properties with respect to the dynamically changing planetary boundary layer. The data are really unique and of broad interest. There are no other measurements that enable both the spatial and temporal variation of aerosol hygroscopic properties in a growing PBL to be explored at these resolu-

tions. The manuscript is unnecessarily difficult to follow, however, and needs revision for brevity and clarity. In particular, it needs to focus on specific elements of the data–the variation in hygroscopicity with evolving particle composition as a function of PBL development–and ignore speculative side-topics such as dust contributions. The manuscript now reads like "these are all the interesting things that we saw", rather than "this is an interesting and important phenomenon that we found in two locations and quantified". These data are really fascinating, and much very good analysis has been done. But the manuscript is unnecessarily disorganized, and needs focus and restructuring to be more concise and precise, and to focus on a few key conclusions rather than trying to explain every detail of the data. Some suggestions, which are not comprehensive, follow.

1. The manuscript is peppered with imprecise language. For example, on p. 9454, line 15, the OPC technique allows for "mostly unambiguous" attribution of particle diameter to scattering cross-section. Does this mean the method is "somewhat ambiguous"? Can this ambiguity be quantified?

2. What are the uncertainties in the hygroscopicity method? Can a numeric value be placed on them? For example what are the uncertainties associated with interpolating diameters from the look-up table? In situations involving Mie theory, it is not possible to propagate uncertainties from first principles. In these cases, it is appropriate to use a Monte Carlo simulation with a range of input values to determine how the various uncertainties in these parameters propagate through to the final value. I suggest this method be applied, summarized in the main text, and detailed in the supporting material. Use calculated uncertainties for every number in every table and on every graph. Without uncertainties data are meaningless.

3. The acronym "WHOPS" is unfortunate. In the U.S., this is a pejorative term for the descendants of Italian immigrants. While it might be too late to change the acronym from an earlier publication, the authors should be aware of this an minimize its use in the U.S.

ACPD
15, C4948–C4951, 2015

4. More language imprecision (p. 9457 line 10) : the AMS "roughly measures particles smaller than 1 um". Use the actual transmission efficiency of the inlet, and say, "the AMS detects particles with diameters < 0.7 um vacuum aerodynamic diameter" or whatever the number is.

5. P. 9458, line 20, "pairing", not "paring".

6. P. 9462, lines 9-10, "less spread" relative to what? What "discrepancy" is being discussed? It's very hard to follow the logic of this paragraph.

7. In section 4, the results are described with extreme detail. Can one example be shown from each measurement area (the Netherlands and the Po Valley), and then the remaining data compiled into different types of plots? I would find vertical profiles of the parameters, with different lines showing the evolution of the profiles, to be more informative than the time plots for which I have to estimate altitude from the altitude plot, then go to the parameter of interest and try to get a value. Since the evolution of the particle hygroscopicity with vertical growth of the PBL is of most interest, it would make sense to use altitude as the independent parameter for the plots.

8. p. 9465, line 4. Mineral dust is brought in as an explanation for large, non-hygroscopic particles. This needs more support. Are there ground or lidar measurements showing a dust contribution? In the next paragraph, biological material is discussed. This is all speculation. These paragraphs could be condensed to read, "the non-hygroscopic fraction of 500 nm particles may be attributable to dust, plant materials, or other components commonly found extending into the accumulation mode from the coarse mode." The digression to speculative discussion detracts from the main points of the paper.

9. p. 9465, lines 24-25. Here we learn that growth factors < 1.5 for particles with diameters <300 nm are not detected "reliably". Why is this not detailed in the instrument description, and why are "unreliable" data being shown and discussed?

ACPD
15, C4948–C4951, 2015

10. p. 9469, lines 25-28. More speculation without conclusion.

11. Section 4.2. This section does not contribute much to the story of the evolution of particle hygroscopicity with the growing PBL. The conditions are quite different from the Po Valley, and no firm conclusions are drawn from the measurements. It might be wise to exclude these observations from the manuscript and focus on the more comprehensive and interesting Po Valley analysis, from a region with much larger air quality problems and radiative forcing implications.

12. There are many typographic errors and a revised manuscript would benefit from thorough editing by a native English speaker.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9445, 2015.