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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hygroscopicity and chemical composition of Antarctic sub-micrometre aerosol particles and observations of new particle formation" *by* E. Asmi et al.

Anonymous Referee #2

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This work brings new insights into atmospheric aerosol properties from one of the most remote parts of the world, where aerosol properties are almost exclusively still driven by natural processes without significant anthropogenic influence. Measurements of aerosol hygroscopic properties, volatility and chemical composition at Aboa station cover a period of 4 weeks in January 2007. The article deserves publication in ACP, but there are some issues I would like to ask authors to consider and provide additional information.

Main comments:

The authors should avoid repetitions of possible role of organic compounds. It is very

demanding to carry out analysis of organic compounds when aerosol loading is so low, there is no doubt about it. But nearly all information about aerosol organic fraction is derived from bulk PM1 OC/EC samples with coarse, several days, time resolution. Then this should be clearly stated at one place in discussion or results and avoid repeating paragraphs on possible role of organics, which is mainly speculation. The fact that organics is an important part of aerosol mass is well established. Discussion being a good example, where in general all possible roles of aerosol organics in aerosol-cloud-climate are listed, but it is all only far reaching speculation for this case study.

Whole discussion of CCN properties carries on again repetition on possible role of organics, but there are no measurements of CCN activity as well as aerosol organic matter speciation. Why is this included at all? The fact that composition is dominated by S-species makes clear point that these particles will be likely good CCN.

As far as I understood, authors did not measure aerosol concentration after first DMA, but only after second DMA (page 27308, lines: 5 – 11). I do agree that this should not be a big issue for H-TDMA measurements; however, I do disagree in case of V-TDMA measurements. Authors stated: "....the absolute concentration is insignificant....." (page 27308, lines: 10-11) and later on that aerosol at Aboa was highly volatile (Chapter 3.5). In case of volatility it is not only about modal peak after heating. If one does not know aerosol concentration after DMA 1, results might show single mode and conclusion about well internally mixed aerosol is easy to derive consequently. But there can be also significant fraction of particles, which volatilizes completely and aerosol is than externally mixed with two populations of particles. How this possibility was considered and treated?

Instead of largely repeating speculations on role of the organic material and at this point questionable volatility data, I would like to suggest addition of DMPS data and discussion of the observed aerosol size distribution, not only nucleation events. It will be useful addition, especially with respect to sizes elected for H-TDMA/V-TDMA measurements.

Minor and more detail comments:

p 27305, line 2: too many “to”

p 27305, line 20: suggestion for change to study natural aerosol processes

p 27306, line 9: suggestion for change to . . .the first aerosol hygroscopicity. . .. . .

p 27306, line 21: are there any additional checks for contamination besides the SW winds sector?

p 27307, line 13: How often did you check sheath flows and how stable there were?

p 27307, line 19: suggestion for change to . . .Particle numbers were measured with a TSI . . .. . .

p 27308, line 13: What is the altitude of Aboa station? How might lower pressure during sampling change impactor cut off for various stages?

P 27309, line 11-23: did you check the real cut off of the unit used for OC/EC sampling with DMPS for example?

2.2.2: Chemical sampling and analysis: Authors should discuss here all possible errors and uncertainties, precision and accuracy, so the reader can assess results discussed later on properly.

P 27310, line 9-10: how do you define “wet diameter”. Would not be more appropriate use the term “ambient”? It is often dry in Antarctica, so “wet diameter” might be misleading

P 27312, line 6: these data are “weather conditions” not “climatic conditions”.

P27312, line 25: does it mean that aerosol volume is PM1 volume? If so, it should be clearly stated.

P 27313, lines12 -21: how is recent marine influence defined? Did you consider also altitude in you trajectory analysis?

P 27314, line1: can you give numbers by how much higher? Is it statistically significant?

P27314, line 26: How do you define daytime? Some thresholds in solar insulation?

P 27315, lines 1-16: Do these correlation have any meaning and statistical significance with respect to very low number of samples?

P 27319-20: why there is no analysis of nucleation event on 20 Jan, which is different from next day “banana” nucleation

P 27320, line 19: particle dynamics and/or different air mass origin

p 27322, line 14: these elements are also emitted from the ocean in a form of sea spray aerosol

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27303, 2009.