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Interactive comment on “Horizontal distributions of aerosol constituents and their mixing states in Antarctica during the JASE traverse” by K. Hara et al.

Anonymous Referee #2

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This paper describes the chemical composition and size distribution of an Antarctic aerosol measured during a relatively long horizontal over the summer continental Antarctica. The measurements appear to be made carefully and the results are clearly of scientific interest. Interpretation of the measurement needs, however, several improvements before I can recommend acceptance for publication.

Major issues

Page 11403: The authors discuss the Junge-slope without i) explaining how this quantity should be interpreted in practice, ii) or performing a proper interpretation of the

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observed slopes. Very few people are familiar with Junge-slopes or their meaning, so this whole discussion is worth little without modifying the text to better consider the two issues mentioned above.

Section 3.5.2 contains a few errors that need to be corrected. First, particle growth rates rarely exceed 10 nm/hour outside urban areas, being rather closer to 1 nm/hour in remote areas. As a result, it is practically impossible that particles grow into the coarse mode by condensation and coagulation in the continental atmosphere (lines 10-12 on page 11411). For the same reason, it seems very unlikely that new particle formation could explain a larger fraction of even fine (>200 nm) particles (lines 22-23 on page 11410). A much more plausible reason in both cases is that these particles were originally primary particles (e.g. sea salt, dust or other) which had accumulated enough sulfate by condensation or cloud-processing to be classified as sulfate particles. Third, it is incorrect to state that sulfate particles were dominant in coarse mode (line 13 on page 11410), as in many cases sea salt particles accounted for more than half of the particles.

The authors need to be more careful when discussing the reaction of nitrogen compounds with sea salt during their measurements. While it is highly likely that nitrate explains most of the Cl loss not accounted for by sulfate in these measurements, there are no data to support this statement. The authors should more explicitly bring up that Cl loss due to HNO₃ or other nitrogen reactions is a plausible explanation rather than a fact (pages 11415-16, abstract). Especially, when one reads the abstract only, one easily gets the impression that also nitrogen compounds were measured in sampled particles.

Minor issues

Page 11400, line 13: The cause-effect relationship does not sound correct here. It seems more plausible that wind causes turbulence than the other way around.

Page 11404: The authors discuss possible errors in estimating aerosol mass concen-

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tration from OPC measurements. Two other issues should be mentioned here. First, the association of water also dilutes salts, so the assumed density of the particles is overestimated at high relative humidities. Second, how accurately the diameter obtained from OPC measurement correspond to that needed to calculate particle mass (particle mass equivalent diameter)?

Page 11412, line 18: Hara et al (2014) is not in the reference list.

Page 11413, lines 5 and 7: I do not understand the notation M % (mean, N %).

While the text is, in general, quite well written, there are a few grammatical problems that should be fixed (e.g. use of articles).

In some places, a space is missing between a quantity and unit, or between different parts of the unit (e.g. page 11402)

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11393, 2014.

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