Atmos. Chem. Phys. Discuss., 14, C3277–C3279, 2014
www.atmos-chem-phys-discuss.net/14/C3277/2014/

Atmospheric Chemistry and Physics Discussions
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# *Interactive comment on* "Enhancements of the refractory submicron aerosol fraction in the Arctic polar vortex: feature or exception?" *by* R. Weigel et al.

**Anonymous Referee #3**

Received and published: 5 June 2014

Major comments;

Overall: The writing throughout the paper is very hard to follow and overly complicated. Much of this appears to be related to the authors writing style which should be simplified to promote readability. To be honest, I found myself reading and rereading sections trying to be sure that my interpretation of the authors' intent was correct. I shouldn't have to do that.

Overall: There is a tendency to write portions of the text as if the sulfate/volatile aerosol is a separate component of the aerosol from the non-volatile refractory portion. I sus-

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pect that this is just some inexact wording but it is a bit disconcerting nonetheless. For instance, the summary paragraph on Page 9867 (line 11-22) suggests that 'increasing particle mixing ratios as function of altitude inside the vortex are solely supplied by non-volatile particles' when it would be more realistic to say that they are primarily supplied by aerosol containing non-refractory material larger than 10 nm. There is ample evidence of this new particle formation in the 25 km range from the OPC and other data sets. Recently MIPAS measurements have shown that SO2 abundances have a strong gradient at this same altitude range (decreasing downward) supporting the idea that the aerosol is primarily sulfate (see papers by Hoepfner). Climate models using interact aerosol formation show a similar phenomenon. It is not inconceivable and in fact it is likely that the aerosol forms on down welling nuclei of some sort whether they are meteoritic or terrestrial in origin. This sulfate originates in the tropics where aerosol evaporates in the tropical stratospheric upwelling associated with the Brewer-Dobson circulation and which subsequently finds its way to high latitudes and then is caught up in the winter time down welling associated with the vortex. It should also be noted that the refractory portion of these aerosol will be transported along with gas phase sulfur (SO2 mostly) and likewise descend in the winter vortex where they can serve as nuclei for the reforming sulfate aerosol. These could have a quite diverse composition including sea salt, surface material, soot, and even recycled meteoritic material.

Page 9860; line 15-19; unless I just totally do not understand what the authors are doing here, this is a major error. I assume that the authors are using these size distributions as if they represent the size distribution of the refractory material, scale the number density and integrate to get a total volume and compute mass. However, all of these size distributions are for total aerosol size distributions that are primarily liquid sulfate aerosol which may/probably have solid, non-volatile inclusions within them. Any size distribution of these inclusions is likely to be largely uncorrelated to these size distributions and any refractory material masses inferred using these are almost certainly incorrect (and much too large). The authors must clarify how they are employing these size distributions and use realistic distributions or admit that they do not exist. For that matter I am not totally convinced that an aerosol would necessarily have only one nuclei since they are the end result of coagulation of many smaller particles for which some number of them could include independent inclusions. Perhaps these stick to each other and form a more complex inclusion; I don't know. (relevant to Page 9876, line 3-6). Unless, I have totally misunderstood what is being done, the numbers discussed in detail on Page 9879, line 12 and further are not correct.

Comments:

Page 9855, line 3-6; I don't see how the first half and second half of this sentence goes together. At the least, this statement cannot be made without further support.

Page 9866, line 12-14; this is an over generalization and doesn't add thing to the text. I would remove it or add appropriate caveats.

Page 9869, line 3-5; Here the authors are neglecting other sources of non-volatile particles.

Page 9876, line 18-20; A factor of 20 is a huge uncertainty and makes me wonder (beyond other strong trepidations about this analysis)

Minor comments:

Page 9852, line 5-7; I think this sentence is has too many superlatives particularly given the large uncertainties cited later in the paper.

Page 9853, line 3; ‘is’ not ‘to be’

Page 9866, line 18-23; I don't follow this section at all.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9849, 2014.