*Interactive comment on* “A missing source of aerosols in Antarctica – beyond long-range transport, phytoplankton, and photochemistry” *by Michael R. Giordano et al.*

Anonymous Referee #1

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Giordano et al. present a novel and comprehensive set of aerosol property measurements along coastal Antarctica during two different transition seasons, one from 2014 and one from 2015. Sulfate was found to be a major contributor to the aerosol population, from both anthropogenic and biogenic sources, depending on the time of year. New particle formation was also concluded to provide a source of aerosol, yet the composition of these newly-formed particles are unknown due to the size limitations of the instrumentation that was deployed. Although the paper is overall well written and presents observations in a relatively understudied region, there are several issues with the interpretation and presentation of the data that need to be resolved prior to publication in ACPD.
General comments:

In several sections, “strong relationships” or “strong function of” are mentioned between two parameters, but it is not necessarily clear in the figures. For instance, on the top of page 6, the authors discuss a relationship between aerosol number and wind. What are the correlations between these two? When looking at both years in Figure 1, it is not immediately clear that this relationship exists until one examines the figure closely, when looking at the 2-minute data. Providing some sort of correlation coefficient would literally strengthen these statements.

Presenting more than simply the sulfur species would make the case stronger that sulfate is the major contributor to the AMS aerosol population. What was the percentage of sulfate relative to total AMS particle mass? Specifically, on page 6, line 10, what percentage of the particles measured by the AMS were combustion-derived OA? On page 7, lines 3-4, showing the size distribution of the other AMS types would support the authors’ statement here. Without showing the other species, this leaves one to wonder if other aerosol types were relatively high at any point in time (i.e., no graphical evidence provided) in addition to how much of the aerosol were actually sulfate. For context, it would be helpful to provide data on the total aerosol population, perhaps as a time series and size distribution of the relative aerosol types for each season, even if it would be placed in the supporting information.

Several conclusions of the general seasonality of Antarctic aerosol are built upon the observations here, which only span a month or two during two consecutive years. How do the authors know if what they observed was typical or anomalous? For instance, the bottom of page 8 presents broader conclusions based on the intensive measurements presented. These statements would be more convincing if the same month or transition season was measured at least twice, for instance, if both time periods were measured in 2014 and 2015, which obviously cannot be done at this point. Although the observations are very intriguing, the authors should take care in how they interpret the results and try to steer away from making such bold conclusions of what the typical
behavior of the aerosol would be this time a year. This could be alleviated by either referring to the observations from the 2014 or 2015 sampling of the transition seasons (versus the transition season in general) or providing more background on previous measurements that would corroborate their observations.

It is great that the authors provided such a detailed explanation on the possible sources of uncertainty or limitations in the measurements that could lead to what was observed (i.e., section 3.2), however, this lengthy discussion draws away from the focus on the uniqueness of the observations. Instead, the authors could condense this section to a paragraph or two (and put some or all of the “A” figures in the supporting information), and focus more on bolstering what was observed, particularly the chemistry measurements. Present each of the three explanations separately and more directly, but focus more on the observation itself than what could be wrong with it. As is, when the three possible explanations in the beginning of the section are posed, I thought to myself, they have the data to prove this. Then, the data would be discussed much later. The section is presented more as a thought process to understand the results than a results and discussion section. Also, this section initially is focused on phase 2, but during the explanations, all time periods are discussed. Overall, the section could use some restructuring and condensing, which would provide clarity as well.

Perhaps the biggest issue in this manuscript revolves around the new particle formation discussion: The authors provide contradicting evidence that new particle formation is a large contributor to the aerosol number. This is concluded in the abstract, and several locations throughout the manuscript (e.g., page 11, lines 31-32), yet on page 11, lines 16-17, the authors directly state no new particle formation events were captured during 2ODIAC. Please be clear throughout on if new particle formation was a major source. It is difficult to discern any “banana plots” in Figure A3, so where did the conclusion that new particle formation is the major source of aerosol during this time period originate from? Perhaps zooming in on some of those growth events towards the end of 2014 would elucidate if these were indeed new particle formation events or simply emission
of small, primary particles.

More explanation and background is warranted in the PMF section. Are these typical AMS particle classifications that have been previously used or are universal? What are some previous studies that have classified AMS particle types like these? More supporting evidence is needed regarding the classifications for what the particle types were. Labeling the peak fragments in Figure 6 would help as well.

The authors briefly mention the collection of filters for offline analyses in the methods. If the analyses, whatever they might have been, were conducted, those results could provide significant supporting evidence to the conclusions drawn based on the AMS and number concentration measurements that are discussed. Of course, this is also limited by the filter pore size, which was not mentioned. If chemical analytical techniques were applied to the filters, that information could fill in quite a few gaps throughout the manuscript and would potentially provide explanation for much of section 3.2.

Specific comments:

Page 5, line 4: Although there are a couple references provided, a few sentences on the specifications and operating principles for the SP-AMS is needed. Especially considering the authors discuss the instrument limitations later on in the manuscript. On page 6, line 31, a PToF size is discussed but it us unknown up to this point that the SP-AMS contains a ToF mass spectrometer. Defining this in the methods would alleviate any confusion.

Page 5, line 5: What offline analyses?

Page 5, lines 29-30: Is this typical and/or expected in this region?

Page 5, line 32: The caption for the figure says 1-hour, not 15-minute.

Page 7, line 16: Use the acronym for CCN when they are first discussed in the introduction and simply use the acronym here (it was spelled out twice in the introduction).
Page 7, lines 20-22: This statement is highly speculative based on the data provided. Considering the limitations of the AMS (refractive aerosol, the size range), this conclusion is not fully supported by the available observations, especially since the measurements were not conducted during all seasons (Sep – Nov). A statement of this level would require a longer time period of measurements covering a wider range of aerosol types and sizes.

Page 10, line 30: Wind speed is all that is presented here, not all local meteorology. Simply stating wind speed would suffice.

Figures: I get why the authors are showing 2015 before 2014 in the figures, to enable the data to be presented in a seasonal versus chronological order. Perhaps labeling them as “Austral spring (2014)” and Austral summer (2015)” would make more sense if keeping the data in this order.

Figure 1: What are the time resolutions for wind direction/speed and AMS?

Figure 3: Why is the UHSAS so noisy? Why is only 2014 shown?

Figure 6: I see this is the combined time series from the different years, yet could cause some confusion since these data were not obtained from the same year. Be sure to label the year that corresponds to each data time period on this figure, similar to the previous figures. Also, labeling the peak fragments on the mass spectra would be helpful.

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