

Yu et al. present findings from detailed compositional measurements of Arctic aerosol in Svalbard during August 2012. The comprehensive combination of analytical techniques employed is incredibly useful for detailed aerosol characterization and source apportionment, in addition to the use of air mass trajectory analyses. While there is obvious importance of conducting detailed physiochemical characterizations of Arctic aerosol in terms of their radiative impacts and subsequent indirect effects on frozen surfaces, there are several issues with the manuscript by Yu et al. that would need to be addressed prior to publication, as discussed in more detail below.

### **General comments:**

Generally, the introduction could use some restructuring. The climate impacts paragraph could be expanded upon to include more details on specific aerosol types and how they affect the radiative budget and cloud microphysics, providing the motivation for why detailed characterization of aerosols in the Arctic is important – i.e., different aerosols have different effects. This is especially so since the last sentence of the introduction explicitly states that results are discussed in the context of aerosol-radiation and aerosol-cloud interactions. Thus, background material on these effects is needed. The information on lines 92-100 should be located sooner in the introduction as it provides a nice general statement of aerosol studies in the Arctic. There is no background on previous relevant studies conducted at the study location, even though there is a long-term monitoring station with aerosol measurements at Ny-Ålesund (<https://www.esrl.noaa.gov/psd/iasoa/stations/nyalesund>). It is not the same exact location as the Chinese site, but close enough to at least use those routine, publically-available measurements to provide some broader spatial and temporal context. Then, the discussion on the background on sources of BC, OM, BrC, and sulfate and the various mixing mechanisms can reside.

Laskina et al. (2015) states, “Typically, SSA particles are deposited wet and, if possible, samples used for single-particle analysis should be stored at or near conditions at which they were collected in order to avoid dehydration.” I assume the RH was much higher during collection, so can the authors comment on what might change between collection and storage under the different RH conditions? Also, Laskina et al. is focused on sea spray aerosol, but the authors conclude that continental sources were a major contributor (lines 377-379), so what about loss of semivolatile organics or sulfate during low RH storage? This possible caveats should be discussed in more detail than they currently are.

“Sulfate” particles is used intermittently throughout, perhaps “sulfate-containing” is more appropriate.

### **Specific comments:**

Title: Because this work is only conducted at one location during one month, extending it to “summer Arctic” seems like a stretch as aerosol populations can vary significantly from terrestrial to the high Arctic and can also change from early to late summer. Perhaps the authors should consider changing the title to something like, “Organic coatings on sulfate and soot particles during late summer in the Svalbard Archipelago”.

Lines 33-34: State the motivation for the particular focus on these particle types, so that this focus is justified.

Lines 34-35: State percentage of OM coated NSS-sulfate here to provide some quantification to “commonly coated”.

Lines 52-53: Regional pollutants and local natural aerosol production can also affect sea ice albedo, especially in the summer when midlatitude transport is not as frequent relative to the winter/spring Arctic Haze season.

Lines 57-63: This part is very BC focused and should be included in the second paragraph.

Lines 144-146: There are no details provided on where these meteorological data came from or what instrumentation was used to measure the mentioned parameters. Although, the data are not presented anywhere so perhaps this information is not relevant to include at all.

Line 193: Define the size of “fine” and quantify “abundant”.

Lines 200-201: “good consistent property” is vague.

Section 2.4: Separate SEM and AFM into separate sections – not sure why these specifically are combined.

Line 275: Why only 500 m?

Lines 276-279: This belongs in the results section.

Lines 290-292: Why were different days evaluated for HYSPLIT versus FLEXPART (3 versus 10 day, respectively)?

Lines 304-306: Provide a citation for this statement.

Lines 310-312: 39% for the s-rich + soot + OM out of all NSS-particles? How was NSS-sulfate and OM determined?

Lines 332-333: How was this documented? This statement is a bit vague.

Figure 1: The dates are very small and it is difficult to tell if there is any sort of time evolution in the air mass sources throughout August. Perhaps the authors could either color the lines by date or create multiple panels (e.g., 1 per week of trajectories).

Figure 3: I assume this is from TEM/EDX? Why are soot and OM not shown? Also, can the authors show a time series in addition to the diameter relative fraction? That might provide some insight into temporal changes due to air mass origin.

Table S1: There is a substantial amount of referencing to Table S1. While I am glad to see the authors have included sample and particle numbers for each analytical technique, jumping back and forth between the manuscript and SI is tedious. I suggest the table be moved to the main manuscript.

### **Technical corrections:**

There are many typos and grammatical issues that need to be fixed throughout the manuscript.

Line 32: “mixing state properties”