Dall'Osto et al. present an interesting 3-year analysis of multi-site aerosol size distributions in the Atlantic Arctic and attempt to assess the sources based on spectral characteristics and linkages to collocated compositional information. This work is an extension of Freud et al. (2017), who presented a Pan-Arctic evaluation of aerosol size distributions but without chemistry linkages. Although the results are useful and warrant their place in the literature, there are several issues that need to be addressed prior to publication, as discussed in more detail below.

General issues:

The authors allude to the fact that that the reason the 3 sites were chosen was because they surround the Fram Strait. There is nothing wrong with focusing on a specific region within the Arctic; however, the introduction describes Arctic processes in general and does not provide sufficient rationale for why this region in particular, other than it is warming the greatest during the last three decades. This reasoning is important but is only briefly mentioned. If the focus is sites surrounding the Fram, then more background and motivation is needed to clearly provide the link for why only these 3 sites are used and only for 3 years, as compared to the many sites presented by Freud et al. On P6, l14, the authors even state, "However, to capture all scales of the variability of Arctic aerosols…." But that is not the goal of this work based on the information earlier in this paragraph. Along these lines, I get that the GRU site is the new addition from what Freud et al. presented, but other than that and a surface level utilization of the chemistry to infer the aerosol sources, I question what is new and novel about this work. Freud et al. presents all years possible at 5 sites and does a similar cluster analysis. Surely, the information is here, but the emphasis on how this study is a progression or even improvement from Freud et al. is not evident. The authors should take care to highlight the new findings and what is different from previous work. Also, in general, it would help if the introduction was rewritten to be more focused on the goals and motivation, with the appropriate background.

Some of the categories need further explanation. First, it is not clear what the bursting category would originate from—any indication on the source? Why do they not grow to larger sizes? Is it possible this is local primary or secondary pollution particles, say, from generators? Second, the definitions of Arctic haze and aged accumulation mode are solely based on a difference of 70 nm in the mode, but even with the chemical information presented, how can these sources be told apart? Where exactly does "aged accumulation mode" originate from? This distinction is not clear. Along these lines, a couple sentences on the background of Arctic haze is warranted in the introduction. Third, how can there be a coarse category when the coarse mode (> 1um) was not measured? Is this extrapolated from the tail of the spectra? Fourth, it is not clear what the nascent categories are—nascent as in sea spray? In general, more explanation and descriptions of the assumptions made are needed for the categories.

Provide uncertainties or standard deviations for any % values listed in the text and in a number of the figures (i.e., Figs 4, 5, 6, 7, 8, 9).

It seems as if the chemistry is a crucial supporting aspect of the classifications. Perhaps these should be discussed first in the results and discussion, in tandem with the shapes of the spectra to justify the categories.

I get lost in which sites had which chemical components measured. A table containing this information would be useful and would elucidate why most of the composition figures only have 1 site. In parallel, how can the limited information at each of the sites, with regard to the supporting chemical and CCN data, be used to glean information about the other sites? The sources/categories

were not always the same as seen in Fig 4, so how can we expect to extrapolate the chemistry and CCN to all 3 sites?

There is a bit of redundancy throughout. For example, the brief synopsis of Freud et al. is given twice with the same information in the introduction. Additionally, acronyms are defined but then the words are spelled out afterwards several times throughout. The manuscript could be streamlined by removing such redundancies.

Specific comments:

Abstract: The importance of the ability to predict aerosol number concentration is not clearly stated. Please provide the broader scope.

P4, l15: Barrow has a long history of chemistry measurements there that support this generalized statement.

P4, l21: "continental sources", i.e., Arctic haze? Natural continental sources (e.g., mineral dust)? This needs to be made clearer.

P4, l24: What is "these frequent nucleation events" referring too? Which events?

P5, l3 and 4: Provide appropriate references here.

P6, l3-4: The fact that the Fram is the only deep-water connection between the oceans and the Arctic does not seem relevant.

P7, l22 (and herein): The coordinates are all provided differently and are odd. Take care to fix throughout.

P7, l24: By local sources, I assume the authors mean local anthropogenic sources. Surely, there are local emissions from natural processes.

P8, l20: The information about the hut name is unnecessary, since the coordinates are provided.

P9, section 2.2.1: How does the overlapping sizes of the distributions compare?

Section 2.2: Some of the stations are missing the dates, time resolutions, or missing data. More details on the data are most certainly needed. Are there consistent specs between the measurements at the 3 sites?

Section 2.3: Do these days overlap with the size distributions? I would assume so, but it is not clear. More details on the dates and where data were acquired are needed.

P11, l5: Was this monthly average calculated from the daily averages or the highest time resolution of the data? Please be clear on how the averages were calculated, here and elsewhere.

P15, l12-13: Or due to the large numbers of preexisting particles transported from midlatitudes.

P29, l8: Methane sulphonic acid. Methane sulphonate is molecularly different.

Figs 1 and 2: These are redundant. Figure 2 provides the information needed.

Fig 3: Redundant panels. Try combining or don't show each spectrum twice.

Figs 4, 5, and 6: I assume these are data from all 3 years? Provide more details in the captions when appropriate.

Fig 5: Color for accumulation is not consistent between the graphs and legend.