Köllner et al. provide a detailed summary on Arctic boundary layer and lower free tropospheric aerosol composition during the summer using multiple analytical methods, satellite data, and air mass trajectory analyses. Two main air sectors were defined during their observations including Arctic air from the north and southerly transport from lower latitudes. Sources reported include wildfire, industrial, and other combustion sources in addition to marine sources such as sea spray. While this detailed information fills a key gap in aerosol observations during the Arctic summertime, there are a few issues that should be addressed prior to publication as delineated below.

The abstract is essentially a list of findings and contains no information on how the current work ties into broader implications for Arctic climate or why these results are novel. The authors should consider adding a couple sentences to demonstrate the importance of their work.

The authors provide a very comprehensive, detailed account of their results, but how their findings fit into the bigger picture is not apparent. A synopsis of *why* assessing the aerosol composition to this level of detail is needed to provide a clear picture on the importance of such measurements. For example, why do we care about knowing the abundance of DCA, sulfate, BC, and organic aerosols, specifically? What has previous work elucidated in terms of these specific aerosols and their impacts on radiation and cloud formation? What observations exist and why are the limited? What are models lacking that require data such as those from the current work? These are key questions for Arctic aerosol-cloud-radiation studies that should be elaborated upon to motivate the purpose of this work. Surely, any Arctic aerosol scientist would know why this work is important; however, to emphasize the importance to a larger crowd (and, say, a cloud physicist looking for information on why aerosols are important for clouds), a broader context and implications should be included in the introduction and revisited in the conclusions.

While the authors do compare their findings a couple of times to Schmale et al. (2011) and reference others like Shaw et al. (2010), Kawamura et al. (2012), and Leaitch et al. (2018), the comparisons are quite limited, even to the extent where they state "This finding is consistent with previous ground-based and shipborne studies…" and only provide references but not what the findings from those studies. In order to put their results into the context of others, and demonstrate why their results fill key observational gaps, a mentioning of *how* their results compare to other analogous studies by briefly describing what those studies concluded is needed. Also, other key studies such as those by Quinn et al. (2002, 2009) and Winiger et al. (2019), to name a few, that describe aerosol composition at various sites throughout the Arctic should be included in the discussion and comparison of results. Specifically, for the air mass source assessment, a more direct link to Stohl (2006) is warranted, given that study evaluated air mass sources of the Arctic using FLEXPART in detail. Are the current results consistent or contradictory to previous work?

Quinn, P. K., Miller, T. L., Bates, T. S., Ogren, J. A., Andrews, E., and Shaw, G. E.: A 3-year record of simultaneously measured aerosol chemical and optical properties at Barrow, Alaska, Journal of Geophysical Research: Atmospheres, 107, AAC 8-1-AAC 8-15, 10.1029/2001JD001248, 2002.

Quinn, P. K., Bates, T. S., Schulz, K., and Shaw, G. E.: Decadal trends in aerosol chemical composition at Barrow, Alaska: 1976–2008, Atmos. Chem. Phys., 9, 8883–8888, https://doi.org/10.5194/acp-9-8883-2009, 2009.

Winiger, P., Barrett, T. E., Sheesley, R. J., Huang, L., Sharma, S., Barrie, L. A., Yttri, K. E., Evangeliou, N., Eckhardt, S., Stohl, A., Klimont, Z., Heyes, C., Semiletov, I. P., Dudarev, O. V., Charkin, A., Shakhova, N., Holmstrand, H., Andersson, A., and Gustafsson, Ö.: Source apportionment of

circum-Arctic atmospheric black carbon from isotopes and modeling, Science Advances, 5, eaau8052, 10.1126/sciadv.aau8052, 2019.

The results and discussion section seems a bit fragmented as the discussion goes back and forth between the north and south influences, and describing single particle composition for both is combined. Perhaps the entire section would be easier to follow if all data (composition, FLEXPART, and satellite data) were discussed in tandem but organized by source region (north versus south). Also, why are vegetation fires and anthropogenic sources a separate section? Aren't they technically a south air mass influence sources and aren't they technically long-range transported? Not clear why these are segregated from the section on the southerly sources.

Seems like Figure S1 should be in the paper. There is a description of the site locations in text, but it is not clear to those unfamiliar with the area. I would think this is important to show for a clear link between sources and transport pathways, and the type of land they cover.

It was not clear to me until later in the methods why the UHSAS size range was restricted. Please provide the size range of the ALABAMA in section 2.2.1. Also, what is the size range of the SP2 data used?

The statement on the top of page 16 indicates the aerosol data presented are from "sampling outside of clouds". But does that mean the measurements were conducted during cloud free periods or were the aerosols sampled below/above clouds during cloudy periods, or some combination of each? I assume based on the generalized north and south air mass descriptions that clouds were mostly present during the latter half of the study, but it is not clear when exactly the aerosols were sampled during cloudy versus clear conditions. This would be important to discuss, at least briefly, because aerosol composition can be quite different in the Arctic boundary layer during clear versus cloudy conditions.

What is the relative contribution of the Arctic versus southern air mass periods? It is obvious for the beginning and end, but what about the 5-day transitional period? The authors could provide the % of each air mass contribution for the entire study at the bottom of page 11.

Why is organic:sulfate only shown in select panels/figures (i.e. Figures 10, 11, 14, and 15)? The ratio is also shown as a different color in Figure 11a but should be consistent with the rest of the figures.

For Figure 15, there does not seem to be major differences between the PES sources for fires, population, and industry. The authors describe each separately, but some discussion on why they look so similar is needed.