I am providing a follow-up review to Kollner et al. “Particulate trimethylamine in the summertime Canadian high Arctic lower troposphere.” I reviewed the responses to all three reviewers and believe the manuscript has been significantly improved. Notably, the attribution of TMA to biomass burning, the consideration of SPMS organic nitrogen markers, and the classification of a greater fraction of analyzed particles were particularly important revisions that were implemented. While the manuscript could be more concise, it is comprehensive, which is also important. My remaining suggestions are provided below.

Major comments:

Section 3.2: It is a significant improvement that the “K/S-containing” particle type, corresponding to an additional 29% of the particle number analyzed by ALABAMA, is now included in the manuscript. While the authors say in the responses that they intend to focus on the TMA-containing particles, this overview of all particles characterized provides context for this analysis and is currently presented in paper. Therefore, it is important to properly present an overview of these particle types, even if in-depth analysis of these particles is not conducted or presented. Please add a note about why 28% of the particles still remain unclassified, as this is still a significant fraction. The addition of the “others” mean mass spectrum as Fig S7 is quite useful; however, it is very similar to the “K/S-containing” particle type, leaving me wondering why the majority of these particles were not included with those particles. If there are differences in the intensities of the peaks, could these be due to shot-to-shot variability of the desorption/ionization laser interacting with the particles? These particles seem to have the same components in the same general mass spectral pattern, with the exception of perhaps less intense negative ion mass spectra (perhaps due to water content variations between particles). With this consideration, could a greater fraction of these “others” particles be re-classified?

Section 3.2: It would also be useful here, or in sub-section(s), to briefly comment on the particle types observed during this study compared to the previous Arctic summertime single-particle mass spectrometry studies by Sierau et al (2014, ACP) and Gunsch et al (2017, ACP). This would provide context for the variability between Arctic studies.

Discussion associated with Figs 6, 10, 11, 12, 14, & 15: Since many of the vertical, size and time bins contain fewer than 100 analyzed particles each, it would be useful to calculate and report errors, according to binomial statistics, on the discussed number fractions.

Minor suggestions/comments:

General: Since few readers have experience with both the Aerodyne AMS and single-particle mass spectrometers and this paper now shows data from both, it is very important throughout the manuscript to indicate whether fractions correspond to number or mass fractions to reduce potential confusion (also since most of the atmospheric chemistry community is used to thinking about mass concentrations).

Table 1: The clarifications to this table are very helpful. Thank you!

P12 L15: Note that magnesium and calcium are regular components of seawater; biologically-active water is not required, as implied here.

Fig. 8: For the reader not familiar with the differences between the HR-ToF-AMS and ALABAMA, I suggest pointing out in the caption that the number fractions and total numbers correspond to ALABAMA data as this could get misunderstood/lost by the non-expert reader. Perhaps also change the figure labeling with this clarification in mind.

P22 L17-20: The authors seem to contradict themselves by saying that “this is the first study demonstrating the incorporation of amines in Arctic aerosol from inner-Arctic sources” and following this by citing the work of Leaitch et al (2017) who present “measurements confirm[ing] the presence of particulate amines and its marine-biogenic source at another Arctic site”.