Response to the comments

We would like to thank all the reviews for their contributions, as we feel the quality of the manuscript has substantially improved due to their comments. We have addressed all the comments, and we have added some extra changes, that include:

- Pressure altitude has been changed to GPS altitude, which is more precise.
- Colours in the SEM graphs changed to be more intuitive and distinguishable. Bins of SEM analysis simplified, numbers of surface area have been updated (the changes are very minor).
- The back trajectories have been run as ensembles and moved to the Supplementary Information.

RC2

General comments:

1. Better to give more detailed information about the flight track and sampling strategy. For example, Appendix A shows the sampling time varies from ~10 to 30 mins. During the sampling, does the flight sample cloud or not? The flight height varies from 100 to 700 m, how much percent of the time is within the boundary layer of each sample?

Extra information about the samples has been added in the table (above or below boundary layer) and some extra description of the sampling strategy (all sampling was done outside cloud) has been added to the first paragraph of section 2. Additionally, flight tracks have been added to fig. 1.

2. The March in the Arctic is very often influenced by Arctic haze (not only long-range transport of dust but also anthropogenic pollution or biomass burning). Do you see any indication of anthropogenic pollution? For example, sample C089_3 shows a high carbonaceous fraction, what is the source of carbonaceous? Why only reported the chemical composition of four samples?

In our SEM analysis, carbonaceous particles are the ones that do not contain any element beyond the ones present in the polycarbonate filters (carbon and oxygen). Because of that and the fact that most carbonaceous combustion particles are at the limit of detection of the technique (and many of them are missed by it, as explained in Sanchez-Marroquin et al, 2019), we cannot say much about combustion particles. Additionally, our INP analysis technique is also not very sensitive to carbonaceous particles as these ones are not very effective at nucleating ice (Vergara-Temprado et al., 2018, Adams et al., 2020). Hence, we are not in the position of saying much about this type of particles.

Only four samples were analysed using SEM-EDS for two main reasons. One reason is that the aerosol concentrations were very low; hence, samples with low sampling volumes did not have enough particles for the analysis. The second reason is that the main author lost access to the SEM-EDS facilities when the COVID-19 pandemic started. Therefore, only a few samples could be analysed before the project finished. The nature of the analysis is that each sample takes about 1 day of SEM time and post-analysis and caries a substantial cost, hence it is not something that can just be squeezed in. The criteria to choose samples was getting a sample with a relatively large sampling volume from each flight.

Minor comments:

1. Lines 67-79: It is better to include the recently published paper from Creamean et al. (2022), which discussed the seasonal variation of INP in the central Arctic.

We have redrafted this whole paragraph to better take into account the literature data, including the contrast between the Creamean 2022 and the Porter 2022 conclusions.

2. Line 98: Add “open leads” after snow. Open leads are most likely omnipresent in the central Arctic. And later when you discuss the aerosol sources in Lines 201 -209, the sea salt aerosols are also likely from open leads.

We added “However, local sources of marine aerosol particles may still occur due to open leads {May, 2016 #873;Kirpes, 2019 #758;Chen, 2022 #869}.” At the end of Sec. 2.

3. Line 137: Change “approach to (Vali, 2019)” to “approach to Vali, (2019)”.

Thanks for spotting.

4. Lines 190-192: Do the aircraft measurements also have cloud-related measurements, like liquid water content or cloud droplet number concentration?

The focus of these flights was not on aerosol-cloud interactions, hence cloud measurements are very limited and we therefore do not report them here. As mentioned in response to referee 1, we took advantage of these flights in an opportunistic manner. In 2022 we used the same technique in flights designed to measure aerosol and INP relevant for specific clouds.