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Comment on acp-2021-349

Anonymous Referee #1

Referee comment on "Arctic black carbon during PAMARCMiP 2018 and previous aircraft experiments in spring" by Sho Ohata et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-349-RC1, 2021

This manuscript provides a detailed analysis of observations of black carbon (BC) obtained during the PAMARCMIP research flights in the Arctic in 2018, as well as interpretations of data from previous experiments. The manuscript is well written and clear, and provides insight regarding the sources, characteristics, and variability of BC in the Arctic. The authors convincingly argue that inter-annual variability in BC abundance is largely governed by biomass burning (BB) amount and transport, while the anthropogenic component of BC is much less variable. This subject is of interest to ACP readers, and the work is of high quality. I recommend publication after minor revisions.

There are two primary areas I would like to see addressed in a revised manuscript. First, the authors compare BC column loadings between four different field programs and two climate models with nudged meteorology. The authors correctly note that model-measurement comparison is affected by location mismatches between the measurements and model-simulated transport from biomass burning events. They address this concern by comparing larger spatial averages from the model domain with the measurements (Fig. 5). However, they ignore vertical mismatches by limiting the comparison between model and measurement to the 0-5 km range reachable by the PAMARCMIP aircraft. Vertical distribution of emitted smoke is extremely difficult to simulate in models, due to considerable uncertainty in the initial injection height of the smoke as well as uncertainty in the vertical lifting during subsequent long-range transport. I would like to see the authors extend their analysis of the HIPPO, ARCTAS, and NETCARE data to the highest altitudes reached on those campaigns, as well as examine the sensitivity of the model-simulated BC column amounts to different choices in integration height (e.g., 0-5 km, 0-7 km, 0-10 km).

Second, the discussion of BC removal by precipitation could be sharpened. How well models simulate this removal is absolutely critical to their representation of BC abundance following long-range transport. The Arctic is a challenging environment to simulate removal due to the dominant role of mixed-phase clouds in this process. In the manuscript, the authors attempt to say something about transport efficiency by comparing observed dBC/dCO ratios following transport with those prescribed for the BB emissions in the models. This seems like an apples-to-oranges comparison. More important would be, how does the model dBC/dCO ratio vary along the line of the trajectories? And how does the final dBC/dCO ratio compare with that from the measurements? Further analysis along

these lines may help explain whether the model and measurement ratios differ because of errors in the emission ratios at the fire locations, or because of scavenging of BC during transport. Some more exploration of these issues would be welcome, as it's key to improving model representations of BC abundance in the Arctic.

Technical corrections:

Line 145: Please spell out "DMT" and provide the country.

Line 319: Replace "larger increases" with "larger values".

Table 6: What are the values in parentheses?

References: Please ensure that all journal names are abbreviated following Copernicus guidelines.

Figures: Please review all figures for compatibility for color-impaired readers. For example, vary line types and thicknesses and symbol types, and choose a color pallet that is more easily discernable. I know several scientists with this impairment and reading figure is often a problem for them. Thank you.