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Comment on acp-2021-1091

Anonymous Referee #2

Referee comment on "Contrasting source contributions of Arctic black carbon to atmospheric concentrations, deposition flux, and atmospheric and snow radiative effects" by Hitoshi Matsui et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1091-RC2, 2022

The authors present a thorough investigation into the source contributions of different Northern Hemisphere regions to black carbon in the Arctic, using the CAM-ATRAS aerosol microphysical module. The key novelty of the paper lies in the separate evaluation of BC surface concentrations, deposition, column loading, and atmospheric and bc-on-snow radiative forcing. The analysis is for the most part well documented and presented, and should be of broad interest to the BC-Arctic and aerosol modelling communities.

I mostly have questions and comments relating to the clarity of the presentation, and recommend publication in ACP after fairly minor revisions.

Major points:

1) My one major concern with the entire paper and analysis is the reliance on three years only, to represent a climatology. There is significant interannual variability in BC emissions, transport, loading, precipitation etc., which is not touched on in the analysis but which is crucial for understanding the observed conditions in the Arctic - and for a realistic model representation. I would urge the authors to either document whether the three years they have used really can be said to represent a climatology (e.g. using extended simulations, or, if this is not practical, longer time series from other models that are already available through AeroCom, CMIP6 or similar), or - preferably - to add discussion of the interannual variability in their results throughout. This would be a major addition, of course, but it would also markedly strengthen the conclusions and community relevance of the paper.

2) In the description of the simulations, I could not find the model setup. I assume you are running with nudged simulations for the years 2009-2011? (If not, the RF calculations presensented later would not be correct, so I hope this is the case.) I recommend

documenting this is some more detail.

3) The global mean lifetime of BC in the baseline model is given as 5.6 days. This is at the upper end of recent estimates (see e.g. Lund et al. 2018 (https://www.nature.com/articles/s41612-018-0040-x), and could be expected to affect the transport of Asian BC into the Arctic. (Or rather, the processes that lead to this lifetime indicate that ageing and wet removal are slow enough to allow for transport into the Arctic.) However, the modelled lifetime, and therefore the type of results shown in this study, are very sensitive to how these processes are parameterized. There are currently no sensitivity studies of this in the manuscript. Would it be worth the effort to check how sensitive the results are to a realistic change in wet removal/ageing? If this dramatically changes the source region composition, then that is of course of high interest to the community as it will indicate a major source of model diversity in Arctic BC RF.

Minor points:

Figure 5: This is not a major point of the paper, but it seems to me that the model has essentially no interannual variability in BC on /in snow. There is a geographical variation, but for each location the model points all lie on a virtually straight line while the observations range over 1-2 orders of magnitude. This is perhaps worth mentioning? See also my first point above.

Figure 6: The purple regions are not easy to interpret. Is this the lowest color in the scale? (It seems so, but I had to zoom in on the colorbar on a large screen to see it.)

Line 285: "largest contributions to Arctic BC" -> this should be just "BC" I think. The figure shows the dominating source regions for the entire NH, not just the Arctic.

Line 312: AeroCom models -> AeroCom Phase II models (the RF range will differ for the various AeroCom phases)