Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-587-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "High–Arctic aircraft measurements characterising black carbon vertical variability in spring and summer" *by* Hannes Schulz et al.

Anonymous Referee #1

Received and published: 29 August 2018

Review of Schultz et al.

This paper presents vertical distributions of black carbon aerosol from two aircraft campaign in the high arctic during the spring and summer seasons. They look at BC loading, BC fraction of total aerosol, BC mass median diameter and BC/CO and they run back trajectories. The instrumental methods and the writing are fine though the analysis is a rather qualitative and the conclusions basic. The main finding of the paper, as I read it, was that there are seasonal differences in BC sources within and transported to the arctic in the spring and summer that drive marked differences in BC loadings between the two seasons. I don't think that's a particularly surprising finding and the

larger motivations, outlined in the introduction, of connecting these observations to deposition rate to the surface and atmospheric heating are not fully realized. Nor is it a paper that can serve to constrain sources of BC to the arctic; as such I am struggling a bit to define what exactly this paper is about or how it might be used by the community. Below are a few specific instances of where the analysis fell short of conclusive for me.

1. The authors posit (in section 3.2) that the data imply that there is increased wet removal of BC in the summer relative to the spring driving the lower concentrations and smaller size distributions. While this may be true I don't follow the logic of how they have isolated wet deposition from dry deposition within the arctic and different convective processes driving transported airmasses in different seasons. It seems to me that a wide variety of combinations of transport pathways and in-arctic processing could lead to the observed trends.

2. In Section 3.3.1 the authors say "this suggests that the rBC mass is contained in fewer, larger particles." I don't think that this is actually true and I don't know what conclusion we can draw from this statement without any point of comparison. rBC is stated to be a small fraction of total aerosol (3.8%) but relative to what? I believe that anything over 1-2% is actually a large fraction by number when considering most continental locations and many remote ones as well. The MMD observed is rather large but not dramatically larger than observed in other studies for some biomass burning emisisons, including residential burning or for inefficient fossil fuel combustion. I don't think there are a great number of published size distributions for BC aerosol produced from shipping sources, especially in open waters so they could be from that, for example.

3. I'm not sure I see the utility of the labored analysis of the BC/CO relationship. As noted by the authors, sources themselves are known to have high variability in this ratio due to different combustion efficiency and the behavior of this ratio, even for a single source, as a function of processing time, is not well characterized. I think it's probably true that if BC and CO are uncorrelated, one can assume that BC has undergone a long period of transport but trying to make sense of a non-zero number to tell you things

about unknown sources and unknown processing is hard unless you know something about either the source or the processing. In this case the authors seem to be trying to make statements about both at the same time and I'm not sure the data can really tell us much.

4. In all of the discussion of the vertical distributions of BC properties in the springtime polar dome I can't summarize what is learned. Looking at Figure 5, I would guess that the springtime arctic in levels I and II is most impacted by local sources because the MMD is large and the BC/CO ratio is relatively high. Local biomass burning sources and possibly ship emissions could have those characteristics but I don't know if the expected sources are large enough to explain the observed loadings. At the end of this section I guess the conclusion is that different sources are contributing to BC loadings at different atmospheric levels but I don't know what the community can do with that information as presented.

5. In section 3.4, I don't understand why the authors choose to only run back trajectories for times associated with higher concentration observations. It seems that it would be more conclusive if you were to compare back trajectories across a range of observed BC loadings and see which patterns emerge that are common to the high and low concentrations observations respectively.

6. In section 3.4.1 on page 19, there is a mention of the data implying "more efficient wet removal in the upper polar dome". But, again, I think it could also be that the convective processes that lifted the air parcel near the sources were different. What does this really tell us about the polar dome that is useful? Similarly, in section 3.5 the authors try to tie differences between layers to differences in local removal within the arctic but I don't know how local effects can be resolved distinct from source differences and transport effects.

7. On page 23 there is a brief discussion of how MMD might affect the radiative properties of BC in the arctic. Yet the present study doesn't actually report BC coatings or

hydrophilicity so this paragraph serves mostly as a literature review which is an odd note to end the results section. Also, just because BC contributes a small amount of aerosol by number doesn't mean it can't be an important absorber. I agree with the authors that it is probably a negligible contribution to extinction but absorption is what you're really interested in and you have very little information about possible absorption enhancements for these particles.

Smaller comments include:

P9, line 7: "southern boarder" should be "southern border"

P17, line 9: "Highest values" should be "The highest values"

P18, line 31: "The minimal latitude" should be "The minimum latitude"

P18, line 34: "Which is reaching up to" should be "Which reaches"

Towards the end of the paper the authors have several instances of the acronym MSD instead of MMD. Please make the terminology consistent.

P22, line 32: "Likeliness" should be "likelihood"

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