

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-39

Anonymous Referee #1

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Referee comment on "Variability in black carbon mass concentration in surface snow at Svalbard" by Michele Bertò et al., Atmos. Chem. Phys. Discuss.,  
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While this paper was submitted as a new paper, it is a revision of a paper previously submitted. I am reviewing it as I would a revised paper since I also reviewed the original version.

The paper is significantly improved from the earlier version. It still suffers from some of the difficulties of the original, but I will recommend publication with revision.

As noted in my review of the original paper, the difficulty is that the sampling and analysis wasn't really designed to answer the questions being posed by this analysis. (Most specifically the snow sampling was done to a fixed depth in both experiments, rather than being designed to isolate, e.g., the influence of newly fallen snow, melt layers, hoar frost, etc.) As noted by the other reviewer of the original paper, correlative analysis is also not really the right tool to be using here for processes-level insight (e.g. see comment 1 below), and that is still the approach taken. There isn't anything that can be done at this point about the sampling, and the more focused analysis presented in this revised paper is an improvement. The data collected a useful contribution to the quantification of snow BC concentrations in the Arctic so with correction of the issues below I recommend publication.

1) There is still some difficulty with the explanations for the selected variables. See my comments below about SZA as a driving variable for snow rBC concentrations. The reasoning behind expecting surface snow temperature to correlate with daily variations in surface snow rBC concentrations is unclear. Once melting commences, any time the temperature goes above freezing surface rBC will continue to increase. Based on physical understanding of process driving surface snow concentration changes you don't expect there to be a correlation between air temperature and surface snow concentration because the process of surface snow BC concentrations increasing with melt is cumulative; when temperatures go back down, there's no physical reason why rBC would then decrease. Therefore applying correlation at the hourly or daily timescale doesn't make sense.

2) The one major issue with the paper that still needs to be addressed is that it argues there is a diurnal cycle in rBC observed during the 3-days experiment (lines 420-421 and 465, referencing Figure 3), then hypothesizes that the formation and evaporation of hoar frost is driving this cycle. However:

i.) Most problematic of all is that I don't see the purported diurnal cycle in rBC that is referenced as existing in Figure 3. As noted in my review of the original paper, the dark blue line in the bottom panel simply looks like smoothed random variations, superimposed on an overall decreasing trend. If the authors are going to argue there was a diurnal cycle and then explain why this diurnal cycle exists, they first need to show that there actually IS a diurnal cycle with some sort of statistical analysis. Simply asserting it's there is not sufficient.

ii) No observations are presented to support that hoar frost formed as hypothesized. Was hoar frost observed during the 3-days experiment measurements? Were the weather conditions (e.g. clear skies and low winds) conducive to hoar frost formation?

Smaller comments:

3) Line 149: Solar zenith angle is cited here as a likely primary driver of snow BC concentrations. It's really solar insolation that is relevant, and then really only as it drives temperature and therefore snow metamorphism changes (e.g. melting and hoar frost formation). Indeed, "Solar radiation" (W/m<sup>2</sup>) is what's shown in Figures 2 and 3, along with temperature. This text should be edited to reflect this.

4) lines 253-255 then 256-258: I don't understand this explanation given that RH is an observational variable of interest since "high RH might favour the deposition of BC suspended by the formation of water droplets through the cloud condensation nuclei." It's then stated that precipitation amount is monitored, so that would be a more direct measure of wet deposition (if that's indeed the type of deposition monitoring RH was supposed to reveal).

5) Lines 301-302 vs lines 320-322: The same result is expressed two different ways in two different places. Why? Lines 301-302 express this as "variability", then notes it declined through the campaign; lines 320-322 simply expresses it as a trend. The partitioning of information into Sections 3.1.1 (atmospheric eBC) and Section 3.1.2 (atmospheric conditions) is a bit odd.

6) "coarse mode particles" (used in text) and "dust particles" (Figures 2 and 3) are used interchangeably. What's really shown in the figure is coarse mode particles. In much literature "dust" has a fairly specific meaning (mineral dust). Here, the coarse mode particle composition isn't determined so it would be best to stick with terminology that

reflects what is actually measured. In the text you could then note that the coarse mode particles are likely a mix of soil, mineral dust and (as noted) possibly coal dust.

7) Lines 308-311: The number concentration of coarse mode particles in snow, as noted, is lower during the first half of the 85-days campaign, then increases during the second half of the campaign (Figure 2). On line 355 an 'average concentration' of 4914+/-4109 per ml is given but it's not clear whether this is across the full duration of the campaign (in which case it's not a very meaningful number, given the clear difference between the first and second half of the campaign) or it's only over the second half of the campaign (in which case the time period used for this statistic should be given).

8) lines 337 and 341: BC should be rBC

9) lines 373-375: "Dry deposition is the main depositional process for the coarse mode particles. Recently it has been suggested to have a significant contribution to the BC surface content (up to 50-60%; Liu et al., 2011; Jacobi et al., 2019)."

This needs to be explained or written differently: Atmospheric BC is not a coarse-mode particle so dry deposition of coarse-mode particles as a significant source of BC to surface snow is a confusing statement.

10) Lines 379-382: "Our data support the hypothesis related to local sources' activation in enhancing the dry deposition impacts in an old mining town as Ny-Alesund. Especially during poor snow cover conditions, as during the snow-melting season, dust particles as residuals of carbon extraction mining activities are available for wind lift\suspension." It's argued that this is a significant source of BC to snow. Yet on lines 536-537 it's stated that: "We believe our results to be representative at least of the Arctic coastal areas, characterized by similar processes and seasonality." Svalbard is fairly unusual for the Arctic in the degree to which past coal mining is likely influencing the addition of rBC to the snowpack from local surface sources (lines 379-358), so how can you assert that you think the results here are generalizable to the Arctic coastal areas overall (lines 536-537)?

11) lines 393-395: The impact of surface snow melt on surface snow concentrations of particulates is presented too much as a hypothesized process; in fact there is significant support for this in the literatures. In addition to the modeling that simulates this and in addition to Aamaas et al 2011 there are at least three other studies showing this in observational data:

Xu, B., T. Yao, X. Liu, and N. Wang, Elemental and organic carbon measurements with a two-step heating gas chromatography system in snow samples from the Tibetan Plateau, *Ann. Glaciol.*, 43, 257–262, doi: 10.3189/172756406781812122, 2006.

Doherty, S. J., T. C. Grenfell, S. Forsström, D. L. Hegg, S. G. Warren and R. Brandt, Observed vertical redistribution of black carbon and other light-absorbing particles in melting snow, *J. Geophys. Res.*, 118(11), 5553-5569, doi:10.1002/jgrd.50235, 2013.

Doherty, S. J., D. A. Hegg, P. K. Quinn, J. E. Johnson, J. P. Schwarz, C. Dang and S. G. Warren, Causes of variability in light absorption by particles in snow at sites in Idaho and Utah, *J. Geophys. Res. - Atmos.*, 121, doi:10.1002/2015JD024375, 2016.

12) lines 424-425: It's noted that the concentration of rBC in snow is 6x higher in the 3-days experiment than in the 85-days experiment. Earlier it's pointed out that the concentrations during the 85-days experiment were consistent with those found in previous studies. Any thoughts on why the very large increase in snow rBC?

13) line 427: Bond et al. 2013 didn't give snow rBC sized so this isn't an appropriate citation. The work of Schwartz et al. could be cited instead.

14) line 435: "All the measured snow impurities time series show two common features...: Which variables are you referring to here? The description that follows matches that for rBC but not for e.g. dust/coarse mode particle concentration. What do you mean by "all the measured snow impurities time series"?

15) line 520: Remaining "unaffected" is different from "not being a primary driver of variations in surface snow rBC over XX timescales". What you've shown supports the latter statement but not the former, and only sort of, since the observational period did not include any highly elevated atmospheric eBC periods.