

Interactive comment on “Origin of elemental carbon in snow from Western Siberia and northwestern European Russia during winter–spring 2014, 2015 and 2016” by Nikolaos Evangeliou et al.

Anonymous Referee #1

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General Comments:

I have no show-stopping issues with the analysis presented or the paper. The authors present a fairly comprehensive analysis of EC from snow samples collected across northern Russia, compare them to modeled values, and do a source apportionment analysis using FLEXPART in a new mode that allows running backtrajectories that track deposited mass, rather than ambient atmospheric concentrations. They also compare their EC concentrations to those from other measurements around the Arctic, and they test modeled EC against these concentrations from other studies.

C1

The data set and analysis presented are useful and the paper should be published once the issues raised below are addressed.

Specific Comments:

Very minor editing for English would be good. (e.g. “a component of the fine particulate matter” “a component of fine particulate matter”; “further tried to futher analyze”; “TRA and DOM contributed double to snow BC sampled at low latitudes”. . .)

The sampling dates varied from early Feb to late April. When the samples were collected could influence the results in two ways that are not sufficiently discussed: 1) Biomass burning (wildfires) in northern Eurasia can become significant in March to April. The source apportionment (Fig 2) shows a very small role of wildfires, but there is some influence in some of the northern samples in 2015 and a significant role in one of the samples in 2016. It might be useful to indicate in Fig 2 (perhaps above each bar?) what date the samples were collected. 2) It seems possible there might have been some surface melting of the snow before sampling. If this is the case, surface concentrations could be elevated due to consolidation of BC at the snow surface, rather than due to increased deposition. Was there any effort made to determine whether the snow might have experienced melt at some point prior to being sampled? Either way, this should be noted.

Hegg et al. found that biomass burning constituted a significant fraction of BC in snow from their northern Russia samples, in contrast to what you found here (i.e. see Fig 2). Hegg et al. could not distinguish between wildfire emission and domestic wood-burning emissions, so one possible explanation is that a significant fraction of the DOM (domestic burning) category in this study is wood burning. This would bring the source attribution of Hegg et al. and that given here in better agreement. It would be very useful if you could state what is included in the DOM emissions category; whether or not for this region a significant fraction of the DOM emissions are from wood burning; and to compare your source apportionment results to that of Hegg et al.

C2

Pg 2, lines 42-44. “Modelled BC was in good agreement ($\delta\dot{S}\ddot{E} = 0.53 - 0.83$) with measured EC. However, a systematic region-specific model underestimation was found.” The wording here needs editing. First, R is an measure of correlation, not agreement. R could be 1.0, but if the two differ by a factor of 2 there is hardly “good agreement”. Second, an R of 0.53 means R-squared of 0.28, which is not a very high correlation coefficient. I would say they were moderately correlated, and the measured values were higher than the modeled values (by, e.g. “on average, XX%”).

Pg. 9, lines 246-250: Same comment as made above re: the text in the Abstract around “good agreement”, and confounding “correlation” and “agreement”. As discussed in the text that follows, there was often significant bias in the modeled values relative to the measured values!

Pgs. 12-13 and Figure S2 discussion of comparison of FLEXPART and Doherty et al. (2010) results: First, Figure S2 would be more useful if it showed the locations of the samples compared in a map and then the actual comparison in an x-y correlation plot. Trying to compare the two maps as given is not very useful, given the large range in concentrations. In an x-y plot, locations in different regions could be given different symbols, corresponding to the regional comparisons (e.g. Canadian Arctic, Western Siberia) as discussed in the text. Second, again, the text significantly over-states the level of agreement. In this case R is 0.24 ($R^2 < 0.06$ – i.e. the model only captures <6% of measured variability - !), and there is a 50% bias in the concentrations, on average.

Pg. 13, line 397: Again, R of 0.63 (R^2 of 0.29) is not “quite high”

Pg. 13-14: Hegg et al. (2010, ACP) presents a source attribution of the BC in Arctic Canada snow measured by Doherty et al. (2010). It would be good to incorporate these results in the discussion here. Not doing so seems like an omission.

Pg. 17, lines 521-522: “The model captured levels of BC quite effectively despite the large variation in measured concentrations.” Again, I disagree with this very optimistic statement of the results of the comparison.

C3

Smaller comments/corrections:

Pg 2, lines 45 and 47: The use of >-100% and <-100% is a bit ambiguous. “>-100%” could be read as more than a factor of 2 difference, and “<-100%” as less than a factor of 2 difference. I’d suggest rewording for better clarity.

Pg. 3 lines 73-75: “Sea ice has a much higher albedo ($\approx 0.5-0.7$) compared to the surrounding ocean (≈ 0.06), thus BC deposited on sea ice reduces the heat uptake of the ocean.” I understand what you’re trying to say here, but as written it’s not accurate: BC deposited on ice does not reduce the heat uptake of the ocean – the presence of sea ice does. BC deposited on ice lowers its albedo, increases heat uptake by sea ice, accelerates its melt, and therefore decreases surface albedo both directly and indirectly.

pg 4, lines 111-112: After discussing (correctly!) that BC/EC are operationally defined it’s stated that “In the present study, EC measurement data from three campaigns are compared to simulation results” – without stating what measurement method is used!

Pg 5, lines 133: It is well known that quartz-fiber filters can have low and highly variable capture efficiency for particles in liquid samples. Was capture efficiency tested/measured? If not, at a minimum this potential source of bias needs to be acknowledged. Hopefully, some tests were done. (As an example, Hadley et al., 2008, Env Sci Tech found that to get high filter capture efficiency they had to run the samples through 3 stacked filters~)

Pg. 6. Line 161: I would reword “driven with 3-hourly” to “3-hour resolution”

Pg. 7, lines 201-203: “Assumed aerodynamic mean diameter and logarithmic standard deviation are used by FLEXPART’s dry deposition scheme, which is based on the resistance analogy...” The assumed size for BC (0.25 microns) is reasonable. However, the deposition rate should be driven by the size of the particles *containing* the BC. It is very unlikely that the BC in the atmosphere was externally mixed with other aerosol

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components; much more likely is that multiple components were internally mixed in larger particles. This would affect dry deposition rates based on resistance.

Pg. 10, pg 287-289: Doherty et al. (2010) specifically measured BC in snow in northern Russia, including western Russia. It's odd not to note this, and to not compare your results directly with theirs from a similar region. Also it's odd to only state that concentrations were "up to 800ng/g", rather than discussing more representative results from their analysis.

Pg. 26, Figure 1 caption: It might be good to remind the reader in the caption that the ECLIPSE emissions don't include wildfire emissions.

Pg. 27, Figure 2 caption: Some rewording/re-parsing of the (very long!!!) first sentence of this caption would make it much more readable. . .

Figure 1: Right-most panel, showing spatial distribution of EC concentrations. I found the color-scale used here not very intuitive. It might be better to go from, e.g., dark blue for low values to bright red for high values.

Figures 3-5: I found the little red stars indicating sampling location difficult to find. I'd suggest making this symbol larger.

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