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Interactive comment on “Sources of carbonaceous aerosols and deposited black carbon in the Arctic in winter–spring: implications for radiative forcing” by Q. Wang et al.

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The paper of Wang et al. investigates the relative contribution of black carbon from anthropogenic pollution versus biomass burning to the arctic snow pack. This is done by combining information from the ARCTAS flights on the relative contributions of anthropogenic versus biomass burning black carbon in the atmosphere with GEOS-Chem/GEOS-5 simulations, then comparing results to snow samples.

Overall this is a readable paper which makes good use of the ARCTAS flights in a modeling framework. The paper describes what they did and I for one am pretty appreciative that I could mentally follow along. It lays out the problem and cites the previous

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literature extremely well. The topic they have chosen is considered a critical climate issue, and the paper is appropriate for ACP. There are many (including myself) who are starting to be persuaded that BC on arctic snow may have been a bit overblown. For the record, the cryosphere area is a bit out of my expertise, so my comments will be limited to the broad lines of biomass burning (as they use the FLAMBE source function which I authored 10 years ago), and the generally observability of the atmosphere/snow system that they model.

Speaking as someone who has studied aerosol and fire observability issues from many angles, I can attest that the problem they are attacking is very difficult. The uncertainties in any emission source function for black carbon is probably at best a factor of 2, and in some cases an order of magnitude. The authors have previous experience with the FLMABE product, using an ARCTAS version perturbed by Edward Hyer in our shop which included a full carbon budget. The present authors scaled FLAMBE emissions by $\frac{1}{2}$ to match previously performed CO comparisons. In general however, it is our experience that FLAMBE underestimates particle emissions, likely by a factor of 2 or 3. So this correction based on CO may not be wholly appropriate. As CO is largely from smoldering combustion, and black carbon from flaming, nonlinearities in the source profile can be problematic-particularly in boreal or mid-latitude fires. That said, in their particular application the model appears to compare very well with their presented verification data. So well, in fact, it makes me a bit suspicious- not in that I think there is any impropriety. But clearly they did some tuning to source functions, which have high uncertainties of their own. I think this could be laid out a bit better in the paper.

It would have been beneficial if they could have run an ensemble of source functions to see what the underlying sensitivity of the system was. These problems are non-linear, and when operating at 2.5 degrees a lot of structure could get washed out (no pun intended). To their credit, in the paper the authors do mention this problem. Without such a sensitivity study, their results may be a bit difficult to interpret. For example,

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if they increased FLAMBE back to native, what does this do to their numbers? They mention in the paper that they intercompared with GFED (which is another good way to bound the problem), but the results are not really elucidated. While I am up on biomass burning, I suspect that there are similar issues on the anthropogenic side. Fortunately, the ARCTAS data helps with the in-atmosphere partition. But again these measurements are not in Asia.

In modeling I take tuning for granted. But here the authors are in a bit of a pickle. In the really interesting part of their finding, that is of BC deposition in Siberia, their reanalysis has no verification data—just a few data points from Sarah. This makes sensitivity work more crucial. They provide fractions of anthropogenic versus biomass burning to 2 significant figures, but the known uncertainties in sources and meteorology do not support this. If you tinker with emissions (like changing them by a factor of two in FLAMBE) and the scavenging efficiencies for BC and other aerosols (adjusting the scavenging parameters by up to an order of magnitude), you can get pretty much whatever result you want. This tuning is not necessarily wrong - just highly uncertain. Some of the ideas, e.g. only hydrophobic BC gets scavenged in cold clouds, are likely wrong. In any case, I think that their conclusion that snow BC is mostly anthropogenic (I notice that they apparently misspoke themselves in their conclusions since they say just the opposite, i.e., anthro only 43% in spring) is very uncertain and quite possibly wrong. This said, I don't have a better answer (i.e. if we knew what we were doing, this would not be science).

In concussion of my major comments, clarifying these issues in the paper is critical, and sensitivity runs would be preferable to understand what the bounds might be.

Other minor things: I am not so sure regressions are an appropriate metric in figure 6. To me it is more representative of a two state system (background versus polluted).

I found the green versus blue dotted lines a bit hard to read in Figure 7.

For figure 10, it would be best if possible, to confine the data to 2007-2009 to match

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the subsequent plots.

Kind regards, Jeffrey Reid, NRL.

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