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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "An AeroCom assessment of black carbon in Arctic snow and sea ice" *by* C. Jiao et al.

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We thank Dr. Stohl for his helpful comments and suggestions. Several changes have been made to the paper to address these comments. The comments are shown below with our response follows.

1. This paper is a useful compilation of snow BC concentrations as derived from BC deposition fields simulated by AEROCOM models and two models of BC behavior in land-based and sea-based snow pack. It is well written and serves the purpose of getting a quick overview of model results with respect to BC snow concentrations. On the other hand, I have not learned much from reading the paper with respect to process level understanding. The fact that the same snow pack models are used for converting

BC deposition values from all AEROCOM models into BC snow concentrations is a limitation, which likely leads to strong underestimation of the true uncertainties inherent to the modeling of BC snow concentrations. Even though, modeled snow concentrations are very different between the various models. However, the paper fails to identify the critical processes which need to be considered and/or improved in order to facilitate more accurate BC-in-snow concentration simulations (e.g., why are some models better than others?). On balance, however, considering the useful data compilation, I recommend that the paper can be published in ACP after minor revisions, considering my further comments.

We agree with Dr. Stohl that attributing differences in simulated BC deposition fluxes to specific physical parameterizations would be useful. Precise attribution, however, requires carefully designed model perturbation experiments, such as those conducted by Lee et al. (2013, http://www.atmos-chem-phys.net/13/8879/2013/acp-13-8879-2013.html), that are beyond the scope of this study. Instead, the aim of our study was to assess model performance and diversity using existing data from the AeroCom archive, and apply the spread in simulated BC concentrations in snow to help constrain the radiative effect of BC deposition in the Arctic. We have added text summarizing this point in section 4.3, paragraph 4.

2. The models seem to systematically underestimate the BC concentrations in snow in the Eastern Arctic (Russia) but overestimate BC concentrations in the Western Arctic (Alaska, Canada, Greenland). Does this indicate a shortcoming of the emission data underlying the emissions? Stohl et al. (2013) have recently suggested that gas flaring in Russia is an important source of BC in the Arctic, which would probably help to explain the underestimates seen in Russia.

We agree that the omission of flaring sources in Russia could lead to underestimation of BC-in-snow concentrations in this region. We have updated the text in Section 4.1 to incorporate this suggestion. This source was not included in the AeroCom emission inventories, but it will be interesting to evaluate its importance in future studies.

3. Pg 26225, line 25: Is it realistic to assign all BC wet deposition to the hydrophilic species? Wouldn't some scavenging (e.g., below-cloud scavenging) also occur for hydrophobic BC?

It is true that there could be wet scavenging for hydrophobic BC. Yet the amount is much smaller compared to the hydrophilic species. We examined a simulation performed with CAM5 and the Modal Aerosol Model 7 (MAM7), described by Flanner et al. (2012, ACP, doi:10.5194/acp-12-4699-2012). The Arctic annual mean wet deposition rate of hydrophobic BC (primary carbon mode) is 3 orders of magnitudes smaller than of hydrophilic BC (accumulation mode) in this simulation. Thus we believe that assigning all wet-deposited BC to the hydrophilic species is a reasonable approximation, at least in the Arctic.

4. Pg 26226: Is there no transfer between hydrophobic and hydrophilic BC, once BC is deposited in the snow pack? I would assume that BC also ages in the snowpack, especially in summer when the snowpack is strongly illuminated.

This is a good point. It is possible that a fraction of hydrophobic BC in the snow could become coated and hydrophilic after deposition. However we are unaware of relevant observations to use in modeling this process. Moreover, we speculate that the fraction of BC in the snowpack susceptible to aging is small because most of the simulated BC particles deposited to snow surfaces are internally mixed within snow grain (Flanner et al., 2012, ACP), meaning they are not exposed to vapor-phase constituents in the interstitial space.

5. Pg 26223, lines 20-24: The statement: "the emission inventory...has very weak seasonal variation..., contributing to the lack of seasonality in BC deposition" is wrong. BC emissions from most sources are likely highest in winter and this would further flatten the seasonal cycle.

The statement itself is not wrong, as the emission inventory used in this model (IMPACT) does in fact have weak seasonality (shown in the attached Fig. 1), and thus

does contribute to the lack of seasonality in BC deposition. However, we remove this sentence since it may mislead readers to think that the emission inventory used by IMPACT model does not have seasonality for all emission sectors. In fact the IMPACT model has seasonal variation for BC emitted from biofuel and biomass burning (Guangxing Lin and Yuxing Yun, personal communication). The more flat seasonality actually comes from the combined seasonal variation from those two sectors. Still, the emission inventories used by many AeroCom models do not have any seasonality in fossil and biofuel emissions, though they do have seasonality in biomass burning sources, which peak (within the Arctic) in July and August (attached Fig. 1). The reviewer's point is well taken, though, and we agree that the lack of seasonality in the fossil and biofuel emission inventories applied by most AeroCom models could bias their simulation of deposition. We have added a sentence about this to section 4.3.

6. The scatter plots (e.g., Fig. 1-3) need improvement.

We used more types of symbols in Figure 1 as well as Figure 11, and reduced the number of colors to five in order to make them more distinguishable. For Figure 2 and Figure 3, we enlarged the symbols. However, as the x-axis is observed BC concentrations and y-axis is simulated values, we cannot offset or add spacing between those markers. In those two figures, we already use eight different symbols to indicate eight different regions so the room for improvement is limited. The main purpose of Figures 2 and 3 is to give readers a general view of how the simulated values look compared to observations. Readers interested in more precise values are referred to Figure 11.

7. Fig. 4: Models should be named, as in Fig. 1-3, instead of just numbering them.

We agree, and have updated the figure.

8. Pg 26224, line 3: reports -> report or reported Pg 26224, line 8: organic carbons -> organic carbon

Thanks, we have corrected these.

9. Pg 26226, lines 4-5: this sentence is somehow incomplete. “ratio of BC concentration”: Ratio to what?

The full sentence is: "The BC meltwater scavenging coefficient is the ratio of BC concentration in the meltwater flux leaving a snow layer to the bulk concentration in that snow layer (Flanner et al., 2007).” Perhaps the sentence did not display completely in the reviewer’s draft.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26217, 2013.

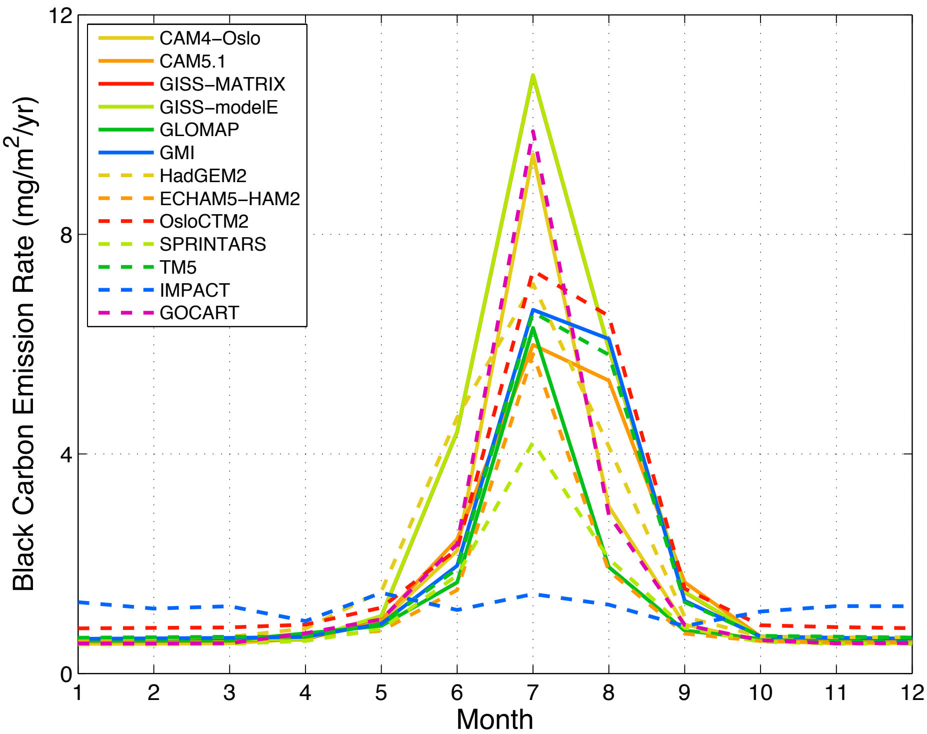

Fig. 1. Monthly Arctic mean BC emission rates for AeroCom Phase II models