

Interactive comment on “The Arctic response to remote and local forcing of black carbon” by M. Sand et al.

Anonymous Referee #2

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The manuscript presents a modeling study of the temperature and energy budget responses to BC forcing at different latitude bands. The topic is highly relevant for ACP and has potential implications for BC mitigation schemes. The model tools used are, in principle, of good quality and the results are presented clearly. However, the authors discuss the uncertainties related to their study hardly at all and some parts of the results section are quite speculative and qualitative (see major comments below).

Despite this, the manuscript can be accepted for publication in ACP if the authors address the following weaknesses in their study.

Major comments:

- 1) The relevance of the simulated results depends on how well the model reproduces

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the true atmospheric BC fields. A comparison to 3 Arctic ground-level stations is shown in Figure 2 but it is also important that the model can replicate the measured fields in the mid-latitudes as well as vertical profiles to a reasonable accuracy. Therefore, the authors should include a more detailed validation of the model performance against BC measurements (midlatitude: e.g. EMEP and IMPROVE; profiles e.g. ARCTAS and ARCPAC). Based on this extended comparison, the authors should discuss in the manuscript text how the potential model deficiencies affect their results and conclusions.

- 2) The authors assume huge increases in present-day BC concentration ($10 \times$) to get a clear signal, but do not discuss whether the results are scalable down to actual concentrations. Given the possible nonlinearities in the system, can you conclude with confidence e.g. that BC forcing outside the Arctic is more important in the actual atmosphere? Does the simulation set-up allow for speculation of how changes in the BC emissions could affect the Arctic in the future?

- 3) Many claims in sections 4 and 5 seem quite speculative as they lack solid numbers to back them up.

Specific comments:

- 4) p. 18386, l. 1: What is the size range of the nucleation mode in the model? In many models nucleation mode is < 20 nm in which case emission of primary particles from combustion would be predominantly in the Aitken and accumulation modes.

- 5) Section 3.2.: For readers who are non-modellers, you could explain explicitly why you need separate online and offline simulations. Have you checked that the offline and online aerosol fields are comparable and that you can use the two simulation set-ups side-by-side? The model forcing peaks in the Arctic in May, which is also one of the months when the modeled BC concentrations match poorly with the observations. The implications of this should be discussed.

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6) Table 1 is redundant as the same information can be (and has been) presented very easily in the text.

7) p. 18388, l. 9: what are the three simulations? BCx1, BCx10 midlatitudes, BCx10 Arctic? What is the zero-BC simulation mentioned on line 26?

8) The two panels in Figure 2 are quite impossible to compare as the scales are so different. It is evident that the model significantly underestimates the observations from November to May but in many ways the light season (April-September) is more interesting. Therefore, the authors should show the observations and model results using the same scale at least for this season.

9) Panels in Figure 6 seem to be in wrong order.

10) P. 18391: Can you quantify the importance of other factors in comparison to the reduction in poleward heat flux? Currently this section reads quite speculative. Again, discuss how the fact that the model does fails to reproduce observations (optical thickness of clouds) affects your results.

11) Table 12: The chosen sign convention makes the figure a bit confusing. Consider changing to a more intuitive convention. I would also like to see numbers from this figure either in the text or in a separate table.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18379, 2012.