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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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Review of 'The Arctic response to remote and local forcing of black carbon' by M. Sand and co-authors. In this study the authors essentially repeat a small part of the climate model experiments by Shindell and Faluvegi (2009), particularly aiming to understand the role of radiative forcing from local and mid-latitude atmospheric black carbon (BC) aerosol on Arctic climate change. The findings by M. Sand and co-authors are well in line with the original study.

A repetition of a study with a different model does have a certain incremental scientific value. However, with the jumps to causilaty, but also the lack of focus, self-critique and foremost perspective, I am hesitant to recommend the paper for publication in its present state. That said, I think these issues do not impinge on the very most central conclusion of the study, and that they can be rectified within a reasonable time-span

with substantial revisions to the text. In doing so, the authors could benefit from making their contribution shorter and more focussed on the main conclusion.

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Major comments:

The applied model is biased relative to observations of Arctic BC. That does not mean that the study is in vain, but I expect an open discussion of what that might mean for the conclusions of the study. On the contrary, it might appear that the text and construction of figure 2 with different scales rather are attempts of masking these model biases. On page 18387 line 19-20, for example, it is stated that the model exhibits a maximum BC column burden in May. Looking at figure 2b, however, there is little evidence for a consistent annual cycle in the model. I would suggest plotting the two panels on the same scales, and discuss up front that the model is unable to reproduce the observed Arctic haze - for some reason - while this may for instance have little bearing on the results because there is not much sunlight to be absorbed by BC in winter.

In a number of places it is postulated that the ice-albedo feedback is driving or determining the nature of the response to BC. The authors perform no proper feedback analysis to support this conclusion, they merely base that on a spatial correlation between ice-growth or loss and the temperature response. This might as well be due for instance to clouds responding to sea ice, or temperature feedback associated with inversions over ice. A proper feedback analysis would involve estimating the radiation flux impact of changes to the state. Given that this is not central to the study, I would recommend abstaining from or weakening many of these statements.

It is concluded that shortwave absorption by BC in the free atmosphere increases the Arctic inversion strength which suppresses the turbulent sensible heat flux towards the surface. It is difficult to support this based on the communitys lacking physical understanding of turbulence in supercritical flows, and further models behave very differently, with some predicting increasing fluxes and others decreasing. There is little evidence

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presented here to support that this mechanism is at play in this particular model. On the contrary, in Figure 12 and the associated text (18395,24) it is actually shown that the opposite is the case.

Following up on this thread, I would strongly recommend to change sign-convention such that downward fluxes are positive both at TOA and surface. This would seem the most intuitive choice, as the purpose of the study is to understand the surface response, and so a positive flux would be warming. This would greatly simplify the text.

The effects of BC on snow and ice not studied here, yet the topic features prominently in abstract and conclusions. The way it is written it is sometimes easy to get the impression that this effect was included in the study. I would suggest rewriting these parts. Would it be possible to compare estimates from other studies of the surface BC impact, with the atmospheric impact?

Northward heat transport is calculated as a residual, which requires model energy conservation and stationarity. Hence, this is only meaningful when done over multiple years, which is violated in Figure 13 where monthly energy budget terms are presented. The figure and associated text, however, seems to add very little information to the overall conclusions and so it would appear unproblematic to leave this aspect out. It may also be worthwhile mentioning in the methods section whether or not NorESM applies the 'energy-fixer' which I believe is used in CCSM4.

Arctic amplification of climate change is found in a wide range of models, and appears to be an intrinsic response to many different forcings, including that from greenhouse gases. There is little scientific doubt that the bulk of the observed Arctic amplification is due to warming associated with anthropogenic greenhouse gases. This perpective seems lost in the beginning of the introduction. It is possible that BC enhance Arctic warming, partly because of the same mechanisms that create amplification from greenhouse gases, partly because BC induced forcing is greater in the Arctic than

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elsewhere. Given that the presented experiments were done by scaling the BC up by a factor 10 only lead to a warming of at most 1 K in the Arctic, what is a realistic estimate of the anthropogenic BC forcing? And how much of the observed Arctic warming could that potentially explain? I suspect this is very little relative to that from greenhouse gas forcing (AR4 states about 0.2 Wm-2 global mean forcing for BC plus 0.1 Wm-2 from BC on snow and ice, and about 4 Wm-2 for all long-lived greenhouse gases), in which case I would recommend letting that be reflected in the abstract and conclusions. In particular, for implications on mitigation strategies (last two paragraphs) it is important to estimate whether reduced anthropogenic BC emmisions is likely to be even detectable.

Minor comments:

There is a widespread use of value-laden words, such as strong or large. I would recommend using a more balanced language, and apply subjective value only when really appropriate.

Check the definition and need for abbreviations. For example, DMS, SS and OM appear not to be defined, and PBL is only used once. Extensive use of abbreviations makes the text less readable.

18380,8 I think the authors mean concentration, not forcing.

18380,12 at the surface turbulent fluxes, as well as radiative fluxes are analysed, and at the lateral boundaries no radiation is assumed to pass.

18382,29 'Expanding on the previous study,'

18383,2 'including an analysis'

18383,11 'including impacts on sea-ice, cloud cover...' (you are not making a feedback analysis).

18383,21 ', includes aerosol and cloud...'

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18383,23 ‘which is based on an earlier aerosol module’

18384,2 ‘... fields as is done in’

18384,3 How can aerosols be interacting with the ‘dynamics’ of the model? I here assume you mean the dynamical core.

18384,26 Consider deleting ‘open’

18385,15 I find it difficult to understand this sentence.

19386,7 Delete ‘storage’. Equation 3 neglects the latent heat of melting snow.

18388,7 The Arctic is not really a ‘band’, more a ‘cap’.

18388,15+18 Consider reducing the significant digits.

18389,20 delete ‘in accordance... experiment’, because this cannot be judged from a zonally averaged plot.

18389,26 here it might be appropriate to reference for instance Hoskins (Tellus, 1991).

18390,8 ‘is likely due’

18390,12 replace ‘skewed’ with for instance ‘shifted’

18390,22 ‘Arctic Ocean’

18390,24 the use of ‘strong’ seems inappropriate

18390,26 ‘is an area with particularly large climate variability’

18391,15 This statement is an unsupported postulate. One could imagine a number of pathways in which BC might cause clouds to change.

18391,20-23 This statement again seems unsupported.

18391,25-27 The statement does not connect well with the surrounding text.

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18392,11 This is not shown, see major comments.

18392,16 The same.

18393,10 The same.

18394,18 Delete ‘and feedbacks’.

18395,17 Again a jump to causality, see major comments.

18396,6 Delete ‘and the vertical motions’.

18396,11 I believe ‘response’ reads better in singularis.

18396,13 ‘... regionality is likely linked to sea ice loss.’

18396,20 I would avoid talking about maximum entropy production here as the application of the principle to the atmosphere might be considered controversial. There are enough studies linking the temperature gradient to energy transport.

18397,3 ‘northern hemisphere Hadley cell expansion’

18397,4-6 This is not shown in the study.

The print quality of the figures is poor, and much of the text too small to be readable.

Figure 2, units are given per volume, but in the text it is written that it is column burden.

Figure 5, here I would like to see some more latitudes, if not all.

Figure 6, panels appear to be swapped.

Figure 10, why is this figure with a different colour-scale?

Figure 10+11, one might get away with showing just one of these.

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