Reply to comments by Referee #1:

We thank referee #1 for providing very useful comments and to help increase the quality of the study. Here are replies to all the comments.

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.

We have now plotted the two panels on the same scale. The reason why we originally chose to have different scales was not an attempt to mask out the model bias, but to show that there is a seasonal cycle in the surface concentrations in the model as well, even though the concentrations are underestimated compared to the observed BC concentrations from the 3 Arctic stations. However, we agree that having the two panels on the same scale seem more appropriate. Even though the model shows a maximum in the BC column burden in May, does not necessarily mean that there must be a maximum in the surface concentrations in May as well. We have extended section 3 with more observations and a discussion on how the model is underestimating BC at the surface (but might overestimate BC in the Arctic free troposphere). We have also included a discussion on how this model bias may influence our results in the discussion chapter.

The motivation for the study is partly driven by the attention BC aerosols and Arctic climate change have received lately. To this end it would of course be an advantage if our model (and most other GCMs) was able to better represent the observed BC concentrations. However, there are also more generic issues related to climate response to absorbing aerosols in the Arctic due the frequent very stable stratification of the Arctic troposphere. To take one step back and analyze this we have made a number of quite unrealistic assumptions of our study, i.e. scaling concentrations and perturbing one latitude band at the time. Due to the quite low RF from BC in the atmosphere we would have had to scale the concentrations substantially anyway to get a robust signal. Now (with the BC underestimation) we chose a scaling factor of 10, while if the model concentrations had been in better agreement with observations the scaling might have been about a factor 3. The magnitude of the absolute perturbation would in both cases have been about the same.

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.

We agree that many of our statements regarding feedbacks were too strong, and we have modified the text to account for this. In the study presented here we have not performed an atmospheric only simulation and it is therefore not possible to identify to what extent the responses are pure feedbacks in the system. The Arctic is a region with potentially strong feedbacks through snow/ice-albedo relation. This feedback mechanism is certainly operative in the model. However, we can not rule out the possibility that in the regions where surface cooling occur and sea ice extent increase this can be caused by reduction in net radiation to the surface through direct forcing and fast responses followed be increased sea ice extent and then reduced ΔT. In the cause-effect chain described above, the increase in sea ice will through the feedback loop further decrease ΔT, but it may not have been initialized by a change in ΔT.

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 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.

We agree that the results don't show that an increase in the temperature change per height leads to a weakening in the turbulent heat flux, and that the text in the first draft was not very well written on this point. In the model the vertical heat flux under stable conditions is determined by the product of the diffusion coefficient and the local vertical temperature gradient (i.e. the non-local term is mainly important under unstable conditions). In the ARC case the BC aloft lead to larger (or less negative) vertical temperature gradient, which in itself will enhance the downward sensible heat flux. At the surface itself the reduction in net downward short-wave radiation will also decrease the skin temperature and thus enhance the surface sensible heat flux. However, there are physical reasons to believe that the turbulent diffusion coefficient should decrease as the atmosphere becomes more stable and weaken the flux. However, the parameterization of the diffusion coefficients are complicated and also affected by other factors (e.g. wind speed) that change by various degrees between our simulations. Unfortunately we have not sampled the diffusion coefficients during the simulations, so it is not possible to be really definite about the change in the diffusion coefficients. We have changed the discussion in the paper to be more qualitatively based on the discussion above.

What remains a robust finding is that the annual average increase in sensible heat flux to the surface is of the order 0.6 Wm-2 (fig. 12), which is much smaller than the change in the radiative balance at TOA (4.2 Wm-2). The net flux change at the surface (radiation, sensible and latent heat) is close to zero for the Arctic region (new fig. 11). Thus the results show that *the vertical mixing of heat is to weak to enable the Arctic energy surplus at the TOA to penetrate to the surface and being taken up there (mainly by the ocean).*

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.

We have now changed the sign-convention such that downward flux is positive at the surface.

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?

We have now rewritten the parts in the abstract and the conclusion, so there should be no confusion that it is only atmospheric BC (and not BC deposited on snow and ice) that is treated in this study. We know no studies that directly compare the surface BC impact with the atmospheric impact, but we can compare with estimates from other studies of the two effects separately. Flanner (2007) calculated a global annual mean RF from BC in snow of 0.054 W/m^2 (0.049 W/m^2) for a strong (weak/normal) boreal fire year. The temperature response to these RF's was estimated to 0.15 K (0.10 K). The temperature response is enhanced in the Arctic (0.4 K-1.9 K for the year with weak boreal fires), and is comparable in size with our results for the MID experiment (see figure 7 in Flanner 2007). Hansen et al. (2005 and 2007) estimated a global adjusted BC/snow RF of 0.05 W/m^2 with a 0.065 K global warming. Jacobson (2004) predicts a warming from fossil fuel and biofuel BC deposited on snow to be 0.06 K. Chung and Seinfeld (2005) estimate the climate impacts from anthropogenic BC in the atmosphere. The global averaged surface air temperature response is estimated to 0.37 K, if the BC is assumed internally mixed. The climate sensitivity of BC direct RF is estimated to 0.6 K/Wm^2.

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.

We agree and have removed figure 13 and the associated text. NorESM is to a large extent based on CCSM4, so the energy-fixer that is used in CCSM4 is also applied in NorESM. Since there are many parameterizations in the physics code, we don't think it is necessary to mention this specifically. The energy-fixer is described in the model documentation for CCSM4/CESM1: http://www.cesm.ucar.edu/models/cesm1.0/cam/

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 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.

We have rewritten the start of the introduction: 'Strong local feedbacks (snow/ice-albedo, clouds) enhance the warming by long-lived greenhouse gases and other forcings. In addition increased poleward heat transport and absorbing aerosols (black carbon) may have contributed to the amplification (IPCC 2007).'

Shindell and Feluvegi 2009 concludes that 'decreasing concentrations of sulphate aerosols and increasing concentrations of black carbon have substantially contributed to rapid Arctic warming during the past three decades. '

Our study does not try to answer the question of how much of the observed warming in the Arctic that can be attributed to BC aerosols. This study focuses on processes related to absorbing aerosols in the atmosphere and we try to understand the difference in the Arctic response between the two regimes (Arctic BC forcing and mid latitude BC forcing). We scale up BC background concentrations in the Arctic by 10, and averaged globally this gives a forcing of 0.4 W/m^2. In the mid latitude the forcing from 10xBC is larger globally (1.5 W/m^2) due to a larger area. It should be mentioned that the NorESM model has a relatively low climate sensibility (Iversen 2012). In a model comparison study NorESM1-M is amongst the least sensitive of the 14 models used (Andrews et al. 2012).

Please note that 4 W/m^{2}is not the forcing that IPCC AR4 states for all long-lived greenhouse gases, but is due to a doubling of CO_2 relative to preindustrial conditions. The RF due to all long-lived greenhouse gases is reported to 2.63 W/m^2. It is perhaps more relevant to compare BC with 1.6 W/m^2, which is the combined RF to all forcing agents, including sulphate and BC. This number may be too high as the AR4 did not include the second indirect effect. Because internally mixed BC was not included in the previous report, new studies show that 0.2 W/m^2 may be too low (Samset et al. 2012, Myhre et al. 2012, Chung et al. 2012).

By reducing BC emissions the concentrations of BC would decrease, but a significant climate signal to the reductions in the Arctic might hardly be detectable, due to the large natural variability and other factors controlling the response. Mitigation strategies concern many small sources that each may not be detectable, and this is why it is a great advantage to use climate models to study their climate effect.

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.

This has been corrected throughout the manuscript.

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.

This has been corrected.

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

This has been corrected.

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.

This has been added.

18382,29 'Expanding on the previous study,'

This has been added.

18383,2 'including an analysis'

This has been added.

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

This has been corrected.

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

This has been corrected.

18383,23 'which is based on an earlier aerosol module'

This has been corrected.

18384,2 '... fields as is done in'

This has been corrected.

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

By ‘dynamics’ we mean the meteorology/climatology of the model and not the dynamical core. We have replaced ‘dynamics’ with ‘meteorology’.

18384,26 Consider deleting ‘open’

This has been corrected.

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

We have perturbed BC in the radiation code to avoid affecting the transport and microphysical/chemical properties of aerosols (except aerosol optics and CCN activation). However, due to online simulations the BC burden will in principal be different between the reference run and the experiments because the meteorology is different. We have checked that the difference in the burden is small.

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

This has been corrected. We have now included the latent heat consumed during snow melt.

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

Since we have two latitude bands 28N-60N and 60N-90N we use the word band, since this is a common phrase, even thought the Arctic can be thought of as a cap.

18388,15+18 Consider reducing the significant digits.

The significant digits have now been reduced.

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

This has been corrected.

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

The reference has been added.

18390,8 ‘is likely due’

This has been added.

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

This has been replaced.

18390,22 ‘Arctic Ocean’

This has been corrected.

18390,24 the use of ‘strong’ seems inappropriate

This has been removed.

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

This has been corrected.

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

We agree that there are many factors in which BC can affect clouds. We have rewritten this chapter entirely.

18391,20-23 This statement again seems unsupported.

We have removed the last part of the sentence.

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

We have rewritten this chapter entirely, so the statement now connects better with the text.

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

We have rewritten this chapter entirely; see the reply to the second major comment.

18392,16 The same.

We have rewritten this chapter entirely, see the reply to the second major comment.

18393,10 The same.

We have rewritten this chapter entirely, see the reply to the second major comment.

18394,18 Delete ‘and feedbacks’.

This has been corrected.

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

We have rewritten this chapter entirely, see the reply to the second major comment.

18396,6 Delete ‘and the vertical motions’.

This has been corrected.

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

This has been corrected.

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

This has been added.

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.

This has been corrected.

18397,3 'northern hemisphere Hadley cell expansion'

This has been corrected.

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

We have rewritten the text: 'In our study we also find a poleward shift of the jet stream for the MID experiment with local warming at mid latitudes (not shown).'

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

The figures have now been printed with a larger resolution and with larger text.

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

We are not sure where in the text the referee refers to in this comment.

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

We have now plotted the temperature change for all latitudes.

Figure 6, panels appear to be swapped.

Yes, and we have now corrected this.

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

The figure has been reprinted with the same colour-scale as the others.

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

We agree and we have removed figure 11.

References:

Andrews, T., Gregory, J. M., Webb, M. J., and Taylor, K. E.: Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere–ocean climate models, Geophys. Res. Lett., 39,L09712, doi:10.1029/2012gl051607, 2012.

Chung, S. H. and Seinfeld, J. H.: Climate response of direct radiative forcing of anthropogenic black carbon, J. Geophys. Res., 110, D11102, 2005.

Chung, C. E., V. Ramanathan, and D. Decremer.: Observationally constrained estimates of carbonaceous aerosol radiative forcing, Proc. Natl. Acad. Sci. U. S. A, 109, doi:

10.1073/pnas.1203707109, 2012.

Flanner, M., Zender, C., Randerson, J. and Rasch, P.: Present-day climate forcing and response from black carbon in snow, J. Geophys. Res., 112, D11202, 2007.

Hansen, J., M. Sato, R. Ruedy, L. Nazarenko, A. Lacis, G. A. Schmidt, G. Russell, I. Aleinov, M. Bauer, S. Bauer, N. Bell, B. Cairns, V. Canuto, M. Chandler, Y. Cheng, A. Del Genio, G. Faluvegi, E. Fleming, A. Friend, T. Hall, C. Jackman, M. Kelley, N. Kiang, D. Koch, J. Lean, J. Lerner, K. Lo, S. Menon, R. Miller, P. Minnis, T. Novakov, V. Oinas, J. Perlwitz, J. Perlwitz, D. Rind, A. Romanou, D. Shindell, P. Stone, S. Sun, N. Tausnev, D. Thresher, B. Wielicki, T. Wong, M. Yao, and S. Zhang.: Efficacy of climate forcings, J. Geophys. Res., 110, D18104, doi: 10.1029/2005JD005776, 2005.

Iversen, T., Bentsen, M., Bethke, I., Debernard, J. B., Kirkevåg, A., Seland, Ø., Drange, H., Kristjánsson, J. E., Medhaug, I. and Sand, M.: The Norwegian Earth System Model, NorESM1-M–Part 2: Climate response and scenario projections, Geosci. Model Dev. Discuss, 5, 2933-2998, 2012.

Jacobson, M. Z.: Climate response of fossil fuel and biofuel soot, accounting for soot's feedback to snow and sea ice albedo and emissivity, J. Geophys. Res., 109, doi:10.1029/2004JD004945, 2004.

Myhre, G., Samset, B. H., Schulz, M., Balkanski, Y., Bauer, S., Berntsen, T. K., Bian, H., Bellouin, N., Chin, M., Diehl, T., Easter, R. C., Feichter, J., Ghan, S. J., Hauglustaine, D., Iversen, T., Kinne, S., Kirkevåg, A., Lamarque, J.-F., Lin, G., Liu, X., Luo, G., Ma, X., Penner, J. E., Rasch, P. J., Seland, Ø., Skeie, R. B., Stier, P., Takemura, T., Tsigaridis, K., Wang, Z., Xu, L., Yu, H., Yu, F., Yoon, J.-H., Zhang, K., Zhang, H., and Zhou, C.: Radiative forcing of the direct aerosol effect from AeroCom Phase II simulations, Atmos. Chem. Phys. Discuss., 12, 22355–22413, doi:10.5194/acpd-12-22355-2012, 2012.
Samset, B. H., Myhre, G., Schulz, M., Balkanski, Y., Bauer, S., Berntsen, T. K., Bian, H., Bellouin, N., Diehl, T., Easter, R. C., Ghan, S. J., Iversen, T., Kinne, S., Kirkevåg, A., Lamarque, J. F., Lin, G., Liu, X., Penner, J., Seland, Ø., Skeie, R. B., Stier, P., Takemura, T., Tsigaridis, K. and Zhang, K.: Black carbon vertical profiles strongly affect its radiative forcing uncertainty, Atmos. Chem. Phys. Discuss. 12, 28929-28953, 2012.

Samset, B. H., Myhre, G., Schulz, M., Balkanski, Y., Bauer, S., Berntsen, T. K., Bian, H., Bellouin, N., Diehl, T., Easter, R. C., Ghan, S. J., Iversen, T., Kinne, S., Kirkevåg, A., Lamarque, J. F., Lin, G., Liu, X., Penner, J., Seland, Ø., Skeie, R. B., Stier, P., Takemura, T., Tsigaridis, K. and Zhang, K.: Black carbon vertical profiles strongly affect its radiative forcing uncertainty, Atmos. Chem. Phys. Discuss. 12, 28929-28953, 2012

Shindell, D. and Faluvegi, G.: Climate response to regional radiative forcing during the twentieth century, Nat. Geosci., 2, 294-300, 2009.