Reply to comments by Referee #2:

We thank referee #2 for providing very useful comments and help increase the quality of the study. Here are replies to all the comments.

Major comments:

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

We agree that the relevance of the study improve if the BC concentrations in the model are close to the observed concentrations. However, there is also more generic process understanding of how the Arctic is affected by absorbing aerosols that can be gained from our study.

We have now included a model performance against BC measurements at mid latitudes as well (EMEP). NorESM has been validated against BC measurements in Kirkevåg et al 2012 and a predecessor of CAM4-Oslo (CCM-Oslo) in Koch et al. 2009. The model underestimates the BC concentrations at the surface, both in the Arctic and at mid latitudes. The vertical BC observations in the Arctic are still quite sparse, so it is difficult to validate modeled BC for a good range of conditions in the Arctic, but the comparison in Koch et al. (2009) show that BC is both over and underestimated in the Arctic free troposphere NorESM have a larger aerosol absorption optical depth at higher latitudes compared to most other models in the model comparison study AEROCOM (Myhre et al. 2012 and Samset et al. 2012), mainly because the BC concentrations in the NorESM is higher than in many other models (in the Arctic free troposphere) . We have expanded the discussion on how the model bias may influence our results in the manuscript (discussion chapter).

2) The authors assume huge increases in present-day BC concentration (10 x) to get a clear signal, but do not discuss whether the results are scalable down to actual concentrations. Given the possible unlinearities 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?

The conclusions are made under the assumptions of linearity, but we are aware of possible unlinearities in the system. A substantial scaling is necessary to obtain a statistically significant result, however, it should not be too large so that the underlying assumption that the response is close to linear is not valid. Hansen et al. (2005) found that the response was close to linear for scaling giving a global aerosol RF of the order of 1 Wm^{-2}. The global RF following the scaling applied here is always below 1.5 Wm^{-2} (cf. section 3.2). At least for the surface, BC is underestimated in the model. The true atmospheric BC forcing in the Arctic is *not well known, and given the uncertainties it is not obvious that the deviation from the real forcing of BC in is a factor 10. A recent study by Chung et al. (2012) shows that the direct radiative forcing from absorbing carbonaceous aerosols could be a factor 2-4 higher than previously estimated. It should also be noted that the NorESM model has a relatively low climate sensibility compared to other models (Iversen et al. 2012, Andrews et al. 2012).*

It is difficult based on the simulation set-up, to speculate on how changes in BC emissions could affect the Arctic in the future, partly as we have only perturbed the vertical profiles of BC and do not include deposition of BC on snow and ice. If BC emissions within the Arctic itself would increase in the future, the BC would be emitted directly into the planetary boundary layer and the temperature response might be quite different than our results.

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

We have rewritten many of the statements in these sections; please see comments in referee #1.

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.

The radius of emitted BC in the model is 12 nm, but it will grow by condensation of H_2SO_4 and thereby also hygroscopic growth. It will not grow larger than approx. 50 nm (but can grow further to larger size modes by coagulation).

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 setups 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.

The regional changes in the aerosol burdens in the offline and online are of comparable size (they differ 13 % in the Arctic and 6 % at mid latitudes), so the two simulations set-ups can be used side-by-side. In the Arctic the burden change is lower in the online simulation, potentially because the N-S mixing is reduced (consistent with the reduction in northward heat transport in this case). We have included an explanation on why we need separate offline and online simulations in the manuscript.

As discussed above the match for the surface concentrations are poor, but that does not necessarily mean that the match for the burden or the radiative effect is equally poor. Also, the May 2006 measurement from the Zeppelin station is an outlier, as this was a week with an extreme weather situation, causing a direct transport of agricultural fires from Eastern Europe to the Zeppelin station with record-high air pollution levels in the European Arctic (Stohl et al. 2007). Even though the model does not greatly underestimate the surface concentrations in May, it underestimates the surface concentrations in April, which is also a month with relatively high forcing. In order to calculate the Arctic BC forcing, we need to know the vertical profile of BC in the Arctic, and that is not well known due to lack of *measurements. Therefore the true BC forcing in the Arctic is uncertain. Our model underestimates the surface BC concentrations, but might overestimate the BC concentrations in the free troposphere (see first reply in comment 1)).*

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

We have now removed the table.

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?

The 3 simulations are BCx1, BCx10 midlatitudes, BCx10 Arctic. We have rewritten the text so this is better clarified. We have also removed the comparison with the zero-BC simulation (from another study) as this may be confusing and since the simulation is not part of this study.

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.

We agree and we have combined the two panels into the same figure.

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

Yes, and this have now been corrected.

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.

The change in the Arctic atmospheric energy budget (cf. new figure 11) is determined by the changes at TOA, surface and at the lateral boundary at 60°N. In the ARC experiment there is a consistent change at the TOA of 4.2 Wm-2 since the increase in the SW downward flux is not nearly compensated by the increase in outgoing LW. At the surface the large reduction in SW absorption is very closely compensated by increases in LH, SH and LW. The net energy balance at the surface has been highlighted in the revised manuscript. Thus the imbalance at TOA does not result in a compensating imbalance at the surface which could have been transported out of the region by ocean currents. Since the energy that corresponds to a increase in tropospheric temperatures of 1K over 60 years (cf. figure 6) is much smaller than the TOA imbalance would give, the excess energy must (by energy conservation arguments) be compensated by a change (reduction) in the northward heat transport. This has been highlighted in the revised section 5, discussion the energy budget.

We have rewritten parts of the section concerning the cloud response. The model overestimates the liquid water path (LWP) in low Arctic clouds. However, it is not easy to determine how this may influence our results without performing several extra simulations.

Based on simple physical reasoning one may expect that excessive water in clouds will reduce the impact of the indirect aerosol effect. It may also limit the possibility of a semi-direct burn-off effect of BC located within the cloudy layer. A burn-off of low clouds would enhance the penetration of SW radiation to the ground and cause a warming. Thus the excess LWP may have contributed to the surface cooling, although a quantification of this is not possible. A short discussion of this is included in the discussion section.

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.

We have changed the sign convention, so the downward flux is positive for the surface. We have also included more numbers from figure 12 (now figure 11) in the text.

References:

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