Atmos. Chem. Phys. Discuss., 12, C3433–C3441, 2012
www.atmos-chem-phys-discuss.net/12/C3433/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic climate response to forcing from light-absorbing particles in snow and sea ice in CESM" *by* N. Goldenson et al.

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Received and published: 9 June 2012

1 Response to first review

To the first anonymous reviewer: Thank you for your comments. We agree that including measurement comparison to the terrestrial snow model data would enhance the paper, and we will include that in the revised submission. A similar plot to Figure 2 for the measurements on land is attached (see supplement for figures). Also, a revised version of Figure 2 is attached, with additional averaging done over measurement points being compared to the same model grid-box.

Compared to the data for particles in sea ice or snow on sea ice, measurements in

land snow tend to have lower concentrations than that of the climatological modeled values, by about a factor of two. Year to year variability could explain deviations of this magnitude. McConnell et al. (2007) document anomalous years with black carbon concentrations in Greenland snow that are a factor of ten above the norm even during pre-industrial time, using a Greenland ice core. However it is important to report this bias in light of our finding that particles in land snow in the model contribute more to the climate forcing.

In response to your other, numbered, comments:

1. To clarify, the number of wavelengths is not limited directly by the cost of transporting tracers in the sea ice. This will be clarified in the text. Lines 18 - 19 will be changed from

   > "...and the number of particle sizes and wavelengths considered for light-absorbing particles is fewer than in the land snow model."

   to

   > "...and the number of particle sizes is fewer. The number of wavelengths considered for light-absorbing particles is also fewer than in the land snow model."

2. Acknowledging how the wording is confusing, we will change this to read

   > "The albedo is determined by radiative transfer through the top layers of ice, and snow if it is present. There are two optically-active layers of snow on sea ice, in which light-absorbing particles can influence optics. The top two layers of the sea ice itself have in-ice optics influenced by light-absorbing particles."

3. This paragraph was meant to justify the lack of consideration of other light-absorbing impurities (e.g., algae) found at the bottom of the ice. However, due to its irrelevance to the description in which it is placed, the paragraph will be removed.

4. - 6. will be changed as suggested

7. Sentence will be restructured to emphasize the model that we are describing.

8. Good point. We will note that for albedo calculation purposes, treatment of snow on Greenland is sufficient.

9. Depths of measurements in these new versions of what was Figure 2 come from no deeper than the top 5cm of a sample. In the model, the values being compared are for the top layer. In the sea ice model this is the surface scattering layer (SSL) described in Holland et al. (2012) and is 4cm or 5cm for thick snow and sea ice respectively. The SSL is adjusted to be half the snow thickness for thin snow and 1/30th the ice thickness for thinner ice. In CLM the top snow layer is 2cm (Flanner and Zender, 2005).

10. - 11. Noted and corrected.

12. The difference in season of maximum forcing and temperature response simulated by the model does have to do with the sea ice. While, we point out that the equilibrium radiative response (ΔQ_{SW}) is strongest over sea ice, we might also have described the role of ice in insulating ocean-atmosphere heat exchange, as you suggest. The net atmospheric surface flux (net radiative and turbulent) is normally out of the ice in winter (see attached figure), and it is balanced by the conductive heat flux through the ice. Thinner ice causes greater conductive heat flux and a warmer surface. The net atmospheric surface flux is larger, which can only be understood as a response to the warming (not a cause for).

We will add additional discussion of this point to the revised submission.

13. We appreciate the careful editing, and will make the correction.

14. Part of the caption of Figure 13 would be better moved to the text, which will then read as

    > "Figure 13 shows the ice thickness difference maps, where sea ice thickness differences are a substantial fraction of those seen in Fig. 11a and b."

15. Wording will be adjusted as suggested.

2 Response to second review

To the second anonymous reviewer: Thank you also for your thoughtful review. I will address comments in order presented:

2.1 Main comments

- We debated your suggestion, but we prefer retaining the use of a control experiment with zero surface particles influencing albedo, as opposed to an 1850 control, for the following reasons.

  While an 1850 control is a standard for IPCC experiments with Greenhouse gases, it is less appropriate for our study. The year 1850 was not pre-industrial in the sense of being prior to substantial anthropogenic soot emissions, nor are such emissions cumulative over time the way carbon dioxide is in the atmosphere. McConnell (2010) shows that black carbon concentrations in snow in Greenland were similar in 1850 and 2000, peaking at some point in between close to 1910.

As we show, if Greenhouse gas concentrations are kept constant, experiments with 1850 levels of black carbon are not substantially different from the year 2000 climate state.

To do a proper study of the effects of industrialization we would want to use a transient 20th century simulation, rather than equilibrium simulations of specific years. We would also require separate tracers for natural and anthropogenic black carbon emission sources, which we did not have (prescribing deposition fluxes), making it difficult to make claims about the effects of industrialization even in the year we study. The historical problem is also complicated by the small size of the particulate impurity forcing compared to Greenhouse gas forcing over the same period. Any results regarding the relative impact are difficult to disentangle, and the Greenhouse gas forcing is the much larger.

Rather than quantify how industrialization has changed the impact of this climate forcing, we evaluate the model sensitivity. Small forcings such as this can have a large impact via feedbacks, and we can better understand those dynamics of the climate system by looking at it in isolation. It was suitable to focus on contrived scenarios comparing the presence and absence of particulate impurities in snow and sea ice because it allows us to understand model sensitivity with a large enough signal to difference distinct climate responses. It also allows us to compare to other studies that also quantified the forcing in this way (compared to zero particulate) in only part of the components of the climate system (sea ice or snow).

- Rather than crowd existing figures with too many additional sub-plots, we propose a new figure that compares the anomalies in the annual mean temperature, snow, and ice thickness fields due to particulate impurities in snow and sea ice versus due to the experiment with doubled carbon dioxide. (See the attached.) Reference to this figure will be made in Section 4.3.

- To interpret Figure 5 it is important to note that this is a plot of melt *rate*. Additionally it was produced with only monthly output, so what appears as a flat top to a curve might well be due to lack of temporal resolution. But, more substantively, it is not the case that less snow is available to melt in the summer in the case that includes particulate impurities, because the amount of snow available to melt is not constant for these two cases. In fact, as shown in Figure 6 of the discussion article, there is anomalously *more* snow in the springtime in Asia in the simulation with particulate impurities, likely due to the circulation anomaly apparent in Figure 10.

- Mineral dust deposition fields will be added to Figure 1 of the revised paper, as shown in the attached figure. It is easier to appreciate the difference in dust deposition fluxes between those two years looking at the seasonal line plot (also attached).

  The sea ice responses to 1850 particulate deposition and that of the year 2000 case are similar. While the 1850 sea ice response is slightly greater, the total ice thickness differences are equivalent within the error.

  The distinct spatial distributions or sea ice response could be due to the distinct spatial distribution of emissions and resulting changes in circulation, or the relative contribution of dust and black carbon. Because we have not separated these effects in separate model experiments we cannot say for certain, but we can note that maximum ice thickness difference in the land-snow only (year 2000) case is more spatially concurrent with the 1850 deposition (greater relative contribution of dust) ice thickness difference maximum. We did not emphasize this observation because we do not expect the form of the model output to be linear in response to the components of the forcing. As we show, the BC and dust forcings do not add up linearly in the response. Additionally, because the forcing acts at a distance, via feedbacks in the climate system, anomalous circulation changes can potentially play a large role in any spatial maps of modeled fields.

- We will consolidate discussion of brown carbon in the revised submission, and consider which other studies might bear mentioning prior to Section 2.3 (e.g. in the introduction, as you suggest).

2.2 Other Comments

- We might replace the sentence in question with the following, to clarify part of the motivation for our experiment design.

  > "Studying these processes individually is useful because they do not always have effects that add linearly. Other work on surface particulate impurities, for which aerosols might be interactively generated rather than prescribed, would capture additional feedbacks in the climate system. Thus it is useful to study the impact of the surface particulate forcing in isolation, just as we study each of these processes individually to inform our interpretation of model simulations in which all are parameterized at once."

- black carbon concentrations in snow (or sea ice)
- "...550nm mass absorption cross-section (MAC) of 7.5 $\pm$ 1.2m^2 g^{-1}, the value recommended..."
- It is reasonable to think that solubles (such as sulfate) coating BC are dissolved in droplet water in clouds, so that BC that is wet-deposited to snow is either uncoated or has less coating than it did in the atmosphere. This would mean that it has a lower mass absorption efficiency than the atmospheric aerosol BC. As discussed in Flanner et al. (2012), BC internally-mixed with an ice coating - which is absent from this and other prior studies with this model - can enhance absorption, but sulphate coatings behave in a similar way to ice-coatings, so

overall MAC may not be grossly under-represented. Unfortunately the frequency of distinct mixing states of BC in snow is not known.

- Good point. This refers to the same range of values used in Flanner et al. (2007), which comes from Bond and Bergstrom (2006). The standard deviation is that which they calculate from the set of laboratory measurements that they average to obtain their recommended MAC value.
- The reason for the use of the 1850 prescribed ocean heat flux is that we lacked a year 2000 equilibrium control run from which to derive that quantity. We did evaluate the ocean heat flux in transient twentieth century simulations to verify that for those years it is minimally different. However, it is unusable because the net global mean surface flux (and hence derived ocean heat flux) is not zero at year 2000 due to anthropogenic forcing.
- Not just anthropogenic, but most light-absorbing aerosol comes from the hemisphere that contains most of the land mass - this is also true for particles from fire or dust.
- By "alpine" we refer to mountain glaciers in general, and can change the wording to reflect that.
- Thank you for pointing us to the McConnell reference. We will add an additional data point to the figure comparing model to measurement on land (which reviewer 1 requested).
- It is true, we can change our wording to reflect the certainty that the presence of light-absorbing particles will have affected the snow-pack, even if they are then removed for the purposes of calculating snow albedos.
- Typo noted and corrected.

- It is surprising that dust explain half the sea ice deficit in the climate response only in so far as it surprised us. There is no reason that it should have been surprising, so we will remove the rhetorical flourish.
- "...overall surface albedo (decimal 0 to 1) difference between runs with and without light-absorbing particles in snow and sea ice."

References

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