

Interactive comment on “Simulating black carbon and dust and their radiative forcing in seasonal snow: a case study over North China with field campaign measurements” by C. Zhao et al.

Anonymous Referee #1

Received and published: 17 June 2014

This paper presents a model study, with a focus on model representation of snowpack, black carbon and dust in north China. The study is conducted using the WRF-Chem regional model, which has been coupled for the first time with the SNICAR model. SNICAR that was originally written for the CCSM/CESM global model to address forcing by black carbon and dust in snow. The study includes comparisons between modeled and observed snow depth, snow water equivalent, 2m air temperatures, and snow BC concentrations, as well as qualitative comparisons of atmospheric aerosol optical depth and a discussion of snow dust concentrations and how that relates qualitatively to dust AOD. Finally, radiative forcing by BC and dust in snow and in the atmosphere

C3702

are calculated. The two are found to largely cancel each other, which is an interesting finding.

I have no overall concerns with the work presented. The paper is a bit long and could definitely be shortened in places. It could use some editing for English. Some wording changes are suggested below where they are really needed for clarity, but other edits would also be advisable.

More significant comments:

The Conclusions are a bit of a catch-all: Much of what is said therein is a repeat of what was already said in the Results discussion. The Conclusions should be shortened to build on, rather than repeat, the results, and should focus on the most important points.

Figure 4 showing a comparison of 2m air temp in observations vs the model, and associated discussion on pages 13346-13347: This paper is fundamentally about forcing by BC and dust in snow. I compliment the authors for showing comparisons of snow cover/SWE and snow BC concentrations, since both are important (and previous model/obs comparisons have only focused on the latter). However, it's not clear to me what information is added by comparing the 2m air temperature. I think this comparison could be removed from the paper.

pg 13347 line 26-pg 13348 line 2. The finding that dry deposition is greater than wet deposition is quite striking to me. I believe that wet deposition dominates in almost all global models. I think this point should be highlighted, and the authors should compare this to dry vs wet rates in other models that have calculated forcing by BC/dust in snow. This can probably only be done at the global scale, but would nonetheless be interesting.

pg 13348, lines 16-18 and figure 7. I was very struck by the 3-orders-of-magnitude hourly variation in snow BC concentrations shown in the figure. The hourly variations in the model are greater than the geographic variations in the observations! I can't picture

C3703

what processes would lead to such large variations in surface snow concentrations on this time-scale. Is there a reason to think these variations are realistic? Do you know what produces these variations? Can you use vertical variations in BC concentrations observed in field studies to conclude whether this kind of variation is possibly realistic?

pg 13348, lines 23-25: "This indicates the caveats in comparing model simulated monthly mean values with observations from snow sampling ... because temporal variations of seasonal snow can be significant". First, this sentence needs editing; it currently doesn't make sense. Second, I think the authors are arguing that rather than comparing to monthly averages the comparisons should be more tightly coincident in time. I disagree. If the very large temporal variations in the model are real the chances of ever getting agreement seem very unlikely, given that would require the model to perfectly capture whatever processes are leading to the large variations at exactly the hour when the observations were made. Instead, I would argue that this means the better comparison would be of longer averages: e.g., model monthly averages vs observed snow BC concentrations averaged over a vertical interval (snow depth) considered representative of about a month's snowfall. This would then average across whatever processes are driving the very large hour-to-hour variations.

pg 13349, lines 10-15: I don't understand what point is being made here. The paragraph starts off talking about BC scavenging in snow melt, but then moves on to talk about atmospheric BC concentrations.

pgs 13349-13350: The large differences in the MODIS and MISR retrievals are interesting in and of themselves, but given the large differences between them and the lack of quantitative comparison to the model I'm not sure that this adds any useful information. The only statement made is: "Overall, the model captures well both satellite retrievals". This is very hand-waving/qualitative. Unless the authors can do better than this I think the paper should be shortened by omitting this.

Smaller comments:

C3704

Abstract, pg 13333, lines 8-9: "...are quantitatively or qualitatively consistent with observations". Which is it? This statement isn't very meaningful.

Abstract, pg 13333, lines 16-18: "This study represents a significant effort in using a regional modeling framework to simulate BC and dust and their direct radiative forcing in snowpack." There is no need to assert to the reader that the work presented is significant. Let the reader decide this for themselves.

pg 13334, lines 11-12: "For example the visible snowpack albedo can be reduced from 0.95 for pure snow to 0.1 for dirty snow with 100ug/g BC". While this is factually accurate it's a bit misleading, in that this concentration of BC in snow is really not found anywhere. If you are going to give an example, please use a more realistic concentration.

pg 13335 lines 8-9: There have been quite a few other studies of forcing by BC in snow other than the series by Flanner et al. and Qian et al. These should be cited as well.

pg 13335, line 13. Inappropriate to cite Hansen and Nazarenko, 2004 here. Their study looked at RF by BC in snow by applying broad regional changes in snow albedo, so this is actually probably the last study of forcing by BC in snow that you should cite if you are trying to argue that amounts and properties of BC in snow are extremely heterogeneous!

pg 13336 lines 27-29 & pg 13344 Section 3.1 opening paragraph: At first read, I assumed that the field observations of BC in snow were a part of this study. In fact they come from earlier work by a different group. This should be stated clearly where they are first mentioned (pg 13336) and again at the very start of Section 3.1, which should perhaps be retitled "BC in snow observational data set" or some such. The title "North China field campaign" leads the reader to believe this campaign was part of the present work.

pg 13339: A subtle change in wording is needed: "The radiative influence of aerosols

C3705

on snow albedo simulated by SNICAR has been validated with recent laboratory and field measurements". This could be read to mean that the model accurately represents both BC concentrations and albedo changes. I would reword to: "The change in albedo for a given snow BC concentration simulated by SNICAR has been validated. . ."

pg 13341, lines 25-27: Another small wording change: "In this study, SNICAR accounts for the light absorption by snowpack containing BC particles residing within snow grains. . .". This could be read to imply that it ONLY accounts for absorption by BC within snow grains, not BC externally mixed with snow. I would reword to something like: "In this study, SNICAR accounts for the fact that BC residing within snow grains absorb more sunlight than does interstitial BC."

pg 13342, lines 18-19: Can you estimate approximately the scale of the effect if you were to account for enhanced absorption by dust inside snow grains vs. interstitial based on the difference it makes for BC? I am not sure if you have the information to do this, e.g. the mass of dust internally/external mixed vs the mass of BC internally/externally mixed. This would be nice to have but is not necessary.

pg 13347, lines 20-21: "...indicating that BCS is determined by both snow coverage and BC deposition". Snow BC concentrations are, by definition, a function of both BC deposited and snow deposited (ng BC per gram SWE). It's not clear to me how the data shown tell us anything more than this.

pg. 13348, lines 14-15: To say that BCS "agrees well" with the observations for sites 1-32 is perhaps a bit optimistic. Why not be more quantitative and accurate: "agrees to within a factor of two". Then also give the mean and/or median ratio of the two across these sites.

pg. 13350, line 17: Referring to the field measurements of snow BC concentrations as "retrievals" (here and elsewhere later in the paper) is confusing, as this implies remote sensing was involved. Please change this to "BCS estimates", "observed BCS", or similar.

C3706

pg 13351, lines 19-21: "The DSTS was detected in the campaign through visual comparison of the sampling filters with a set of standard samples (Huang et al., 2011)." It's not clear what is meant by this. What information did Huang et al. provide that wasn't covered by Wang et al., which measured these samples with the ISSW?

pg 13351, lines 22-26: Again, why use the results of Huang et al., when I believe they are superseded by the Wang et al. analysis?

pg 13352: line 1 and elsewhere following: Please replace "SSA complement" with the more commonly used "co-albedo".

Figure 12: Please explain why the "enhancement" is <1 for 1.5-5.0um. Is this because the light doesn't penetrate the snow, so there is less absorption by the BC?

pg 13352, lines 27-28: Reword to: "Figure 13 shows the spatial distribution of forcing by BC and dust in the atmosphere and in snow. . ."

pg. 13354, lines 4-5: The description "reduces away from Northeast China" is vague. I think you mean, "BCS within Northeast China decreases strongly moving from the industrial region to the south, northward towards the northern boundary of the domain".

pg 13357, line 9: Again, Hansen and Nazarenko is not an appropriate citation. They did not account for accelerated snow aging with albedo reduction by BC. Cite instead Flanner et al. (2007), which was the first model study to account for this effect.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13331, 2014.

C3707