We thank the two anonymous referees for their valuable comments and constructive suggestions on the manuscript. Below, we explain how the comments and suggestions are addressed and make note of the revision we made in the manuscript.

Anonymous Referee #1

General comments:

- ***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 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.***

We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

Specific 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.***

The conclusion section is significantly shortened. Only the key points and discussion are included.

- ***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.

Although 2-meter air temperature is not as directly related to the calculation of snow aerosol concentrations as snow cover and SWE, it is an important metric to evaluate the general performance of model. Furthermore, the near-surface temperature could significantly affect snow accumulation and melt processes, so it is useful to show that the model does a reasonable job in simulating the near-surface temperature.

- ***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.***

Wet deposition does not always dominate the removal of aerosols even in global models. It depends on the model treatment of dry and wet depositions, and their relative importance varies for different aerosol species (e.g., dust could be very different from sulfate [Huneeus et al., 2011; Zhao et al., 2013]). At regional scales, dry deposition could dominate near the source regions where aerosol concentrations near the surface are high, and dry deposition is more efficient than wet deposition on days with limited rain or snow. We did some further analysis to compare dry and wet depositions of BC in this study as shown below. Figure r1 shows the dry and wet deposition fluxes of BC on snow. We selected two box regions, one with larger wet deposition fluxes and the other with larger dry deposition fluxes, for more quantitative comparison. Figure r2 shows the frequency distribution of dry and wet deposition rates of BC on snow in January-February, 2010, averaged over the two regions. In region one, although wet deposition of BC on snow can be as high as 50-100 $ng/m^2/s$, it is limited to precipitation days with existing snow on the surface (~30% of the time). Dry deposition of BC is also limited to times with snow on the surface (~60% of the time). It turns out that BC wet deposition on snow is larger than BC dry deposition in region one. In region two where the surface is always covered by snow during the study period, wet deposition happened about ~60% of the time, while dry deposition happened all the time. In addition, wet deposition in region two is much less than that in region one. This turns out that the average BC dry deposition on snow is larger than BC wet deposition in region two.

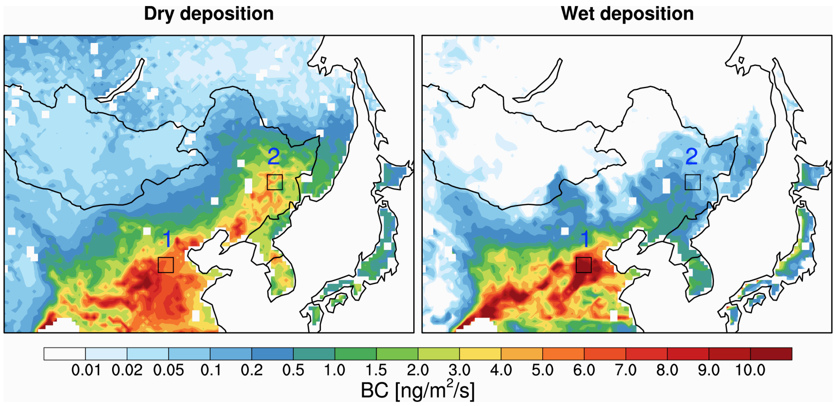

Figure r1. Dry and wet deposition rates of BC averaged over January-February, 2010. The two black boxes (region 1 and region 2) are defined for the analysis in Figure r2.

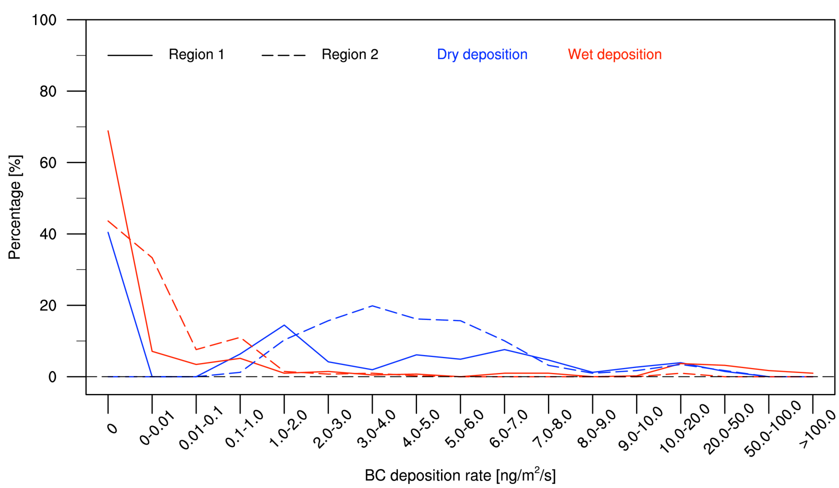

Figure r2. Frequency distribution of dry and wet deposition rates of BC on snow in January-February, 2010, averaged over the two black boxes (region 1 and region 2) that are defined in Figure r1.

Huneeus, N., M. Schulz, Y. Balkanski, J. Griesfeller, S. Kinne, J. Prospero, S. Bauer, O. Boucher, M. Chin, F. Dentener, T. Diehl, R. Easter, D. Fillmore, S. Ghan, P. Ginoux, A. Grini, L. Horowitz, D. Koch, M.C. Krol, W. Landing, X. Liu, N. Mahowald, R.L. Miller, J.-J. Morcrette, G. Myhre, J.E. Penner, J.P. Perlwitz, P. Stier, T. Takemura, *and C. Zender: Global dust model intercomparison in AeroCom phase I. Atmos. Chem. Phys., 11, 7781-7816, doi:10.5194/acp-11-7781-2011, 2011.*

Zhao, C., S. Chen, L. R. Leung, Y. Qian, J. Kok, R. Zaveri, J. Huang: Uncertainty in modeling dust mass balance and radiative forcing from size parameterization, Atmos. Chem. Phys., 13, 10733–10753, 2013c.

- ***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 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?***

First, we want to clarify that BC concentrations in the snow column are not available from this campaign. We added a description in the observation section "Although snow samples were gathered at several depths during the campaign, the BC mass concentrations in snow are mainly estimated at the top snow layer. Therefore, the simulated BC mass content in the top snow layer (which never exceeds 3 cm thickness) is compared with the observational values averaged at the top layer (2-5 cm depending on sites) snow samples."

Second, we plot the time series of BCS at site 3, where a 3-orders-of-magnitude hourly variation exists, to demonstrate the temporal variation of BCS. Figure r3 shows that, when snow starts accumulating on the ground, the BC mass in snow is much less than the snow mass (minimum BCS value). Then, BCS increases with the accumulation of snow due to both dry and wet deposition. When snow starts melting, BCS keeps increasing till the snow disappears. Through this evolution process, the ratio between BC and snow mass concentrations could be very small at the beginning and become very large at the end, both of which occur in very thin snow layer. Since the conditions in very thin snow layer were rarely sampled, the 10th and 90th percentiles of hourly BCS may better represent the BCS temporal variation. We have now revised the Figure 7 using the 10th and 90th percentiles of hourly BCS to show the hourly variation, which is much smaller than that in the original figure.

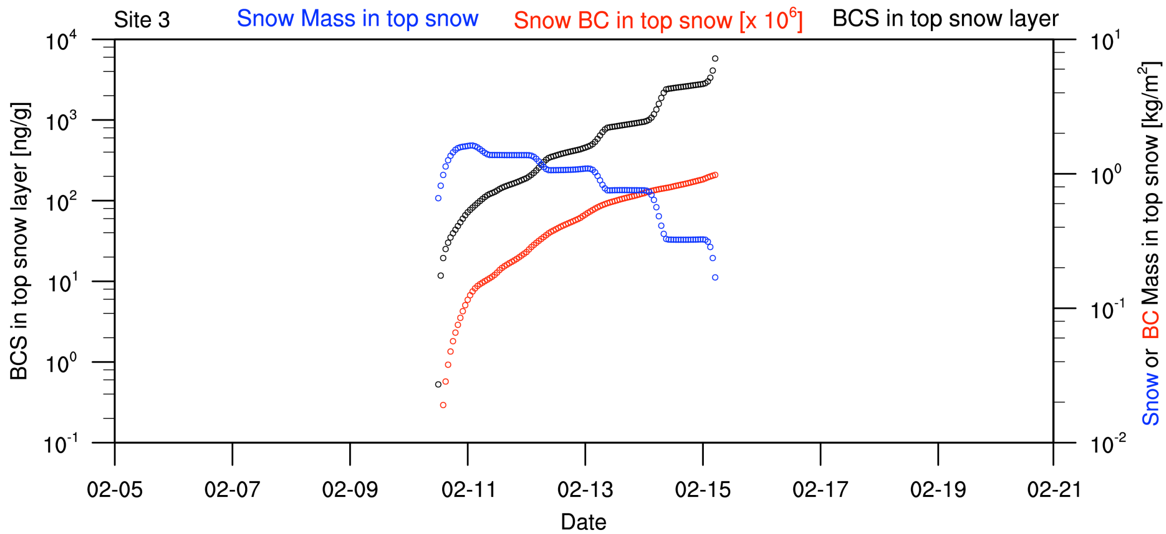

Figure r3. Hourly snow and BC mass concentrations and their ratios (i.e., BCS) in top snow layer at site 3.

- ***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.***

We agree that it is unlikely for models to perfectly capture the observation at the exact hours. Long-term average of both observed and simulated BC concentrations in the top snow layer may be a better choice. However, the snow sampling at all sites from this campaign is only made at a specific time. We actually did some sensitivity analysis by applying different temporal averages of model results. It is now clarified in the text "Due to the large temporal variability of seasonal snow and the uncertainty in simulating the exact timing of weather systems and aerosol emissions and deposition, the comparison between long-term averages of observed and simulated BCS is desired. However, snow sampling at all sites from this NCC campaign was only made at specific times relatively far apart along the road trip. In addition, more comparisons are also conducted using the average of model results sampled within a few (e.g., ± 2) hours and a few (e.g., ± 2) days around the observation sampling time at each site. The comparisons do not change significantly (figures not shown). The large temporal variability of BCS may also indicate the caveats in comparing model simulated monthly mean values with observations from seasonal snow sampling at a specific time, a common practice in global modeling studies [e.g., Huang et al., 2011; Qian et al., 2014]."

The BC concentrations in snow column are not available from this campaign. Now it is clarified in the observation section "Although snow samples were gathered at several depths during the campaign, the BC mass concentrations in snow are mainly estimated at the top snow layer. Therefore, the simulated BC mass content in the top snow layer (which never exceeds 3 cm thickness) is compared with the observational values averaged at the top layer (2-5 cm depending on sites) snow samples." In addition, even with the measurement of BC concentrations in snow column, it is still difficult to use it directly to evaluate the simulations, due to the large spatial variability of snow depth within the model grid size (i.e., 36 km in this study).

- ***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.***

Our point here is that the biases in simulating BCS could result from the biases in modeling BC lifecycle in snow and in the atmosphere. Now the text is revised as "Model biases in simulating BCS may result from the biases in simulating BC lifecycle in the snow and atmosphere. For example, the biases may be partly due to the uncertainties in treating BC scavenging by snowmelt. As discussed above, the aerosol scavenging ratios in snow are prescribed in this study. These values are highly uncertain, and there are no direct measurements over the study region to constrain these parameters. On the other hand, the biases may also result from the biases in simulating the atmospheric BC. Since observations of atmospheric BC concentrations are not publicly available in this region, satellite retrieved AOD is used to evaluate the general model performance in simulating aerosols in the atmosphere."

- ***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.***

Besides the difference in instruments and retrieval algorithms, the difference between the MODIS and MISR retrieved AOD could also be due to the different overpass time. The simulation results also show clear diurnal variation of AOD over the study region. Although satellite retrievals have their own uncertainties, we believe it is still worthwhile to compare the simulated AOD with satellite retrievals to demonstrate that aerosol simulation is not way off. In addition, the satellite retrievals also show clearly the dust band over Northwest China, which is also captured by model simulations. Therefore, we still think it is necessary to keep this comparison in the paper.

Minor Comments:

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

It is corrected as "…are consistent with observations."

- ***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.***

“significant” is deleted.

- ***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.***

The sentence is deleted.

- ***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.***

More studies of impact of aerosol in snow are cited now.

- ***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 heterogenous!***

Deleted.

- ***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.***

It is clarified now.

- ***pg 13339: A subtle change in wording is needed: “The radiative influence of***

aerosols 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: : :"

Corrected.

- ***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."***

Corrected.

- ***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.***

We agree that it is interesting and important to have an estimate of the absorption enhancement by dust inside snow grains. But it is beyond the scope of this study. We may look into this topic in future.

- ***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.***

The sentence is deleted.

- ***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.

Revised. The model generally agrees well with the observations and has negative biases (within a factor of two) in the relatively clean sites (e.g., sites 1-32) with a median model to observation ratio of 1.03.

- ***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.***

Corrected.

- ***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?***

The sentences are revised.

- ***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?***

Wang et al. [2013] is also added.

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

Corrected.

- ***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?***

At longer wavelengths ice absorption is sufficiently strong to compete with BC particles residing within the ice grains, thereby decreasing the absorption efficiency of the BC particle in snow relative to one residing in air. This is especially true for larger ice grains, as indicated in Figure 12. For example, the 1.8μm single scattering albedo of a 1000μm (effective radius) ice grain is only 0.63, compared with 0.92 for a 100μm ice grain at the same wavelength.

- ***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: : :"***

Corrected.

- ***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".***

Revised.

- ***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.***

Corrected.

- **Anonymous Referee #2**

General comments:

- ***In this paper a regional model is used to simulate BC and dust, and their radiative forcing, in snowpack in North China over the period January to February 2010, when a field campaign was conducted in the area. The regional model used is WRF-Chem, and the modelled depositions of the atmospheric aerosols are coupled with the SNICAR model for the first time in this study. The paper describes well the WRF-Chem model and the regional simulations, and how it compares to the measurement campaign of BC in snow in 2010. The spatial pattern of the observations is captured by the model. They find that the radiative warming in the snowpack is of comparable magnitude to the surface radiative cooling due to BC and dust in the atmosphere. I recommend that the paper can be published in ACP after minor revisions, considering my comments below.***

We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

Specific comments:

- ***I find the conclusion section too long. In the introduction it is written: "The findings are summarized and discussed in Sect. 5" that does not match the title of the section. Some of the text in the conclusions is more or less a repetition of text in the result section. I would recommend to shorten the conclusion section and if needed include a discussion section.***

Now we change that sentence in the introduction to "The findings are concluded in Section 5." The conclusion section is significantly shortened. Only the key points and discussion are included.

- ***Figure 6 show that dry deposition is of larger importance than wet deposition over parts of the region of interests, which is interesting. I would like a few more sentences in the model description (Page 13338, Line 4) regarding dry deposition of BC. Also, include in the model description what is not included, e.g. deposition of ice-borne aerosols which is mentioned on page 13356.***

Now we add more about dry deposition and ice-borne aerosols in the model description part "Dry deposition of aerosol mass and number is simulated following the approach of Binkowski and Shankar [1995], which includes both particle diffusion and gravitational effects." and "In this study, cloud-ice-borne aerosols are not explicitly treated in the model but the removal of aerosols by the droplet freezing process is considered."

The other review had a similar comment on this. Please refer to the response to one comment of the other reviewer for the comparison between dry and wet deposition of BC on snow.

- ***Section 2.4 Emissions: Which year is used for the China emissions from Lu et al. (2011)? For emissions in the model domain not given in Lu et al. (2011) (e.g. Russia, Mongolia, Korea), are the Zhang et al (2009) emissions used? Please specify. For the spin up period, is GFED 2009 emissions from October to December used?***

We clarified it in Section 2.4 "Anthropogenic emissions are obtained from the Asian emission inventory described by Zhang et al. [2009] at 0.5^{o}x0.5^{o} horizontal resolution for 2006 except that BC, OM, and sulfate emissions over China are from the China emission inventory for 2010 described by Lu et al. [2011] at a 0.1^{o}x0.1^{o} horizontal spatial resolution and a monthly temporal resolution for the simulation period. Biomass burning emissions for the simulation period (October 2009 - February of 2010) are obtained from the Global Fire Emissions Database, Version 3 (GFEDv3) with a monthly temporal resolution and a 0.5^{o}x0.5^{o} horizontal resolution [van der Werf et al., 2010] and vertically distributed following the injection heights suggested by Dentener et al. (2006) for the Aerosol InterComparison project (AeroCom)."

- ***Related to the emissions, the model results show large daily and diurnal variation of BCS (Page 13348, Line 17). Is there assumed any diurnal variability in the emissions?***

As we clarified in Section 2.4, emissions are at monthly temporal resolution. There is no diurnal variability of emissions assumed. Please refer to the response to one comment of the other reviewer for the explanation of the large daily and diurnal variation of BCS.

- ***The model uses chemical initial and boundary conditions from a quasi-global WRFChem simulation. I would recommend including a few sentences describing this simulation, including emissions used and deposition processes. A short discussion of weaknesses/advantages of the use of regional models could be of interest, especially regarding long range transport of BC. Is it possible to quantify how much of the deposited BC in the regional model domain comes from outside the domain?***

More description about the quasi-global WRF-Chem simulation is added "The chemical initial and boundary conditions are provided by a quasi-global WRF-Chem simulations for the same time period to include the long-range transported chemical species. The quasi-global WRF-Chem simulation is performed at 1^{o}x1^{o} horizontal resolution using a quasi-global channel configuration (using periodic boundary conditions in the zonal direction) with 360×130 grid cells (180°W-180°E, 60°S-70°N). The quasi-global simulation is configured similar to the regional simulation and also driven by the NCEP/FNL data. More details about the quasi-global WRF-Chem simulation can be found in Zhao et al. [2013c]." and "For the quasi-global WRF-Chem simulation that provides the chemical boundary for the regional simulation, anthropogenic emissions are obtained from the Reanalysis of the TROpospheric (RETRO) chemical composition inventories (http://retro.enes.org/index.shtml) except over East Asia and the United States, where anthropogenic emissions are from the Asian 2006 emission inventory [Zhang et al., 2009] and from the US National Emission Inventory (NEI) 2005 (WRF-Chem user guide from http://ruc.noaa.gov/wrf/WG11/Users_guide.pdf), respectively. Emissions of biomass burning aerosols, sea salt, and dust are treated the same as described above for the regional simulation."

It is possible to quantify the contribution of transported BC from outside the domain through a sensitivity experiment in which the transported BC from the boundary can be turned off. This is an interesting topic but beyond the scope of this study.

- ***The field campaign in North China provided BC concentration at several depths (Page 13344, Line 20). Did you try to use this information when comparing model***

results and observations?

The BC concentrations in snow column are not available from this campaign. Now it is clarified in the observation section "Although snow samples were gathered at several depths during the campaign, the BC mass concentrations in snow are mainly estimated at the top snow layer. Therefore, the simulated BC mass content in the top snow layer (which never exceeds 3 cm thickness) is compared with the observational values averaged at the top layer (2-5 cm depending on sites) snow samples."

- ***Figure 2 show the spatial distributions of snow depth and snow water equivalent from the WRF-Chem and the CMC reanalysis. Could you explain why there is no snow (white boxes) at the exact same sites in the reanalysis and the model data?***

They are all lakes, so no data available at those grid boxes.

- ***Page 13348, Line 11: Could you mention why there was no data available for sites 41-46?***

It is clarified now in the observation section "This method is not possible to state with confidence that any of the light absorption in the snow is due to BC, when the soil and snow particulates in snow samples are the same color, which is particularly likely to happen when the snow is thin and patchy. For the sites (e.g., 41-46) with those snow samples, the BC mass concentration in snow is not reported [Wang et al., 2013]."

- ***In figure 7, is it a way to indicate if there are no observations? In figure b) I will suggest to align the dot in the circle with the station number. The explanation of the WRFChem box plot was hard to follow. "minimum and maximum simulated values at 24 h of January–February". What does at 24 h mean? (midnight, daily mean)***

Now Figure 7 is revised to better align the results with the station numbers. The numbers of stations without observations are removed.

The caption is clarified as "the whisker bar of model results shows the 10th and 90th percentiles of simulated hourly values of January-February."

- ***On page 13351, it could be useful to use the site numbers when discussion the observations in the different regions.***

Site numbers are added with the discussion.

Technical corrections:

- ***Page 13334, Line 3: "the sophisticated representation of snow metamorphism processes available for climate study.." -> "the most sophisticated" ?***

Changed.

- ***Page 13334, Line 8: "In general, the model simulated spatial variability of BC and dust mass concentrations in the top snow layer (hereafter BCS and DSTS, respectively) are quantitatively or qualitatively consistent with observations." -> "that are quantitatively" (?)***

It is corrected as "…are consistent with observations."

- ***Page 13347, Line 21: "BCS is determined by both snow coverage and BC deposition". Is snow coverage the right word?***

This sentence is deleted to also address a comment from the other reviewer.