

Interactive comment on “Snow albedo reduction in seasonal snow due to anthropogenic dust and carbonaceous aerosols across northern China” by Xin Wang et al.

Anonymous Referee #2

Received and published: 10 November 2016

Reviewer Comments: Wang et al

Overall Comments:

Due to the high amounts of industrialization in China, there is a need for studies of impurities in seasonal snow. This study is on a Chinese snow survey conducted in 2014. However, it is not clear how much of the data presented in this study has already been published. The authors are recommended to clarify, in much more detail, what data has been previously published, and what are novel results from this study and included in this manuscript. For example, it is not clear whether this study presents new snow chemistry data, or if this data has been published in another manuscript, such as Wang 2015. It is also unclear why the 2010 Chinese Snow survey data is included

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in the study. Averaging results from the 2010 and 2014 surveys seems inappropriate, especially because the snow conditions/depths appear to vary widely since so many sites did not even have enough snow to resample in 2014. The paper does not appear to be ready for publication in this submission, even with the following revisions. Major restructuring is needed.

The novelty of the paper appears to begin in the model comparisons with the albedo surface observations made in 2014. The reader suggests the authors significantly revise the paper to focus solely on the 2014 observations (unless the reader is confused by the text and the 2010 data has not yet been published?).

Additionally, according to the methods used, the reader asks the authors to report the BC as equivalent black carbon (eBC) rather than black carbon. If there is a reason otherwise to report as BC, please explain.

It appears (though clarification is needed from the authors) that a new model, SAMDS, similar to the SNICAR model, is applied here. However, only a small portion of this very long paper is devoted to this section. A short discussion/conclusion at the end needs further expansion. Then for the authors to focus on the observed vs. modeled albedo, as well as provide more information and justification for applying the SAMDS model more widely. The SAMDS model appears to be the main highlight of the paper. The title and paper could be restructured around the SAMDS model.

The scientific methods used could be explained in a more logical way throughout the methods section of the paper. Also, in the site description, an explanation why the site numbers begin at 90 in 2014 should be given. Also, a map including GPS coordinates and the sampling sites would aid the reader. Additionally, it is stated that the same volume of snow was not collected at all sites. How much volume was filtered at each site? This leads the reader to believe that if the same top volume of snow was not collected at each site, the integrated mass concentration of BC in each snow sample would be different?

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In conclusion, much of the intro and re-presenting of previously published data from the 2010 and 2014 Chinese snow surveys could be removed. And/or better support/clarification for the reason for including it could be included.

Suggestions for Revisions:

The abstract is not currently sufficient. The first sentence states that 92 samples were collected in 2014, however, the 92 points appear to be presented as fewer averages later. The abstract should state how many average values are presented in the study and if the 92 samples were collected across X sites, X snowpits, and at X resolution within the snowpit. Additionally, only surface sample average concentrations are presented in the abstract, how many samples were collected at the surface? And how many in snow pits, and integrated pit samples?

First of all, at the end of the introduction, previous data published from the 2010 and 2014 Chinese snow survey must be presented clearly. Then, a paragraph outlining what is specifically novel about this study must then be stated. Throughout the manuscript, when previously published data is presented, a reference must be cited for where that data has been published.

Additionally, is this paper the first presentation of the SAMDS model? This should be more clearly stated in the abstract, as this appears to be the main point/novelty of the paper. In general, the paper could be edited to more clearly explain what new data is presented in the study. There are pervasive run-on sentences; the manuscript should be edited to make the language more concise.

The discussion should be expanded to support a more widespread use of the SAMDS model. Also, stating how the model will be disseminated, would aid readers in applying it to their own research. Is there a plan to do so?

Overall, the paper needs major restructuring and revision to portray a logical flow of information. The main weakness is that it is very unclear what is new data and what

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has previously been published from the 2010 and 2014 surveys and this paper should focus only on what is new.

Specific Line Comments:

Comment 1: Jaffee 1999 cited on Page 3 Line 4 is not included in the bibliography.

Comment 2: Page 4 Line 3: Writing edit: ‘, with India at’ => ‘ and ‘ 76.1% in India’

Comment 3: Page 4 Line 13: ‘Among its main light-absorbing impurities’ This sentence introduction does not make sense. Could just remove the intro and start with ‘1ng g-1 of BC’ . . .

Comment 4: Page 4 Line 14: ‘on the albedo’ => ‘on the albedo of snow and ice’.

Comment 5: Page 4 Line 17: ‘owing to’ doesn’t read well. . . please alternate words here, as well as multiple other locations in the manuscript. Other suggestions could be, ‘due to’, etc.

Comment 6: Page 4 Line 20, first word. Include citation(s) for the previous sentence.

Comment 7: Page 4 Line 21: In the Bond 2013 paper, sub-title 0.2.3 Synthesis of black carbon climate forcing terms, number 2: “The best estimate of industrial-era climate forcing of black carbon through all forcing mechanisms is +1.1 W m⁻² with 90% uncertainty bounds of +0.17 to +2.1 W m⁻².” Rather than 90 % confidence interval as listed in this manuscript.

Comment 8: Page 5 Line 9: ‘in situ’ => ‘in situ’ and elsewhere in the manuscript.

Comment 9: Page 5 Line 20: ‘involve’ use different word such as ‘present’ or ‘include’

Comment 10: Page 5 Line 21: Begin new sentence with ‘Previously, . . .we analyzed’ if some data from the Chinese snow survey in 2014 was already published and include citation. The reader is confused if some of this data has already been published, or if this data is all unpublished and included in this study. If all the data is unpublished, could

start the sentence with, 'Here' instead of 'previously', such as ' Here we analyze...' instead of 'analyzed' to convey to the reader this is new data being presented.

Comment 11: Page 5 Line 21: Here you could explain how this study is different than what has already been published from the Chinese snow survey in 2014.

Comment 12: Page 6 Line 11: I think it is a bit misleading to say 'we explore climatic effects of ILAPs' as far as the reader can see, albedo reduction is explored, but climate models are not employed. Please rephrase or clarify.

Comment 13: Page 6 Line 13, 'Therefore' => 'Here'. Or clarify if something else is meant.

Comment 14: Page 6 Line 14: Create two sentences. 'campaign. Snow albedo is also...' Also, when was the snow campaign? 2014? Please include year for clarity.

Comment 15: Page 6 Line 15: Is this the first presentation of SAMDS? If so rephrase sentence to explain 'SNICAR and 'the new' SAMDS...'

Comment 16: Page 6 Line 19: Include sub-heading for 'Site Introduction'. Also, include the time range the samples were collected in this paragraph, was each site visited once over the month?

Comment 17: Page 7: In the site introduction, please explain why only sites 90 – 102 were visited. Why do the numbers begin at 90? Where there 90 sites in 2010 where there was not snow in 2014?

Comment 18: Page 7: This is troubling to the reader. If different volumes of sample were collected based on how dirty a site was, does this make the measurements comparable? When the sample is filtered, does the volume not matter? How much volume was filtered? The total sample? Please explain.

Comment 19: Page 7 Line 13: Wang 2015 => It appears some of the 2014 Chinese snow survey data has been previously published. A more in depth explanation in the

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introduction should explain how this study builds on previously published results of the same snow study, and what data is new. Also, please state what chemistry was analyzed in Wang 2015, just stating it was 'similar' to other studies by Hegg et al., is not sufficient. Also, it is not clear with the next sentence, were the major ions described in Wang 2015? This paragraph needs to be re-written to clearly state what has been published and what is new data being presented here. Or if results from Wang 2015 are being applied here in a new analysis.

Comment 20: Page 8 Line 11: The first sentence should be rewritten as its not clear to the reader how iron would originate from mineral dust in seasonal snow. Please explain in the text why this assumption is made.

Comment 21 Page 9 Line 5: Why is it stated that the Microtops II Sun Photometer was used instead of the CE318? Please explain the significance for stating this and the difference between the two. What does the Microtops II do that the CE318 does not?

Comment 22 Page 9 Line 14: include 'in this study'... during the 2014 Chinese survey. (If that is what the authors mean, otherwise please clarify).

Comment 23 Page 9 Line 24: Spectroradiometers have been used in a number of other studies than listed here. Please include 'e.g.' and then the citations.

Comment 24 Page 10 Line 5: How was the spectroradiometer held level? What was the field of view? How did you minimize shadows from the leveling device? What were the solar zenith angles? This data should all be included in the text and solar zenith angles should be presented in a table.

Comment 25 Page 11 Line 3: Is this the first presentation of the SAMDS model in publication? If so this needs to be highlighted more in the abstract, title, and especially the text here. Otherwise please include a citation for the SAMDS model.

Comment 26: In general it seems this paper could be more focused around the SAMDS model as this appears to be the novelty of the paper.

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Comment 27: Page 14 Line 13: This sentence is confusing and needs to be revised. For example: Delete 'For' and begin sentence with 'Most of the snow samples were collected in the afternoon corresponding to the Aqua-MODIS (13:30LT) overpass time. Then the next sentence starting with 'The averaged. . .'

Comment 28: Page 15 Lines 5 – 10. These sentences need to be restructured and concise.

Comment 29: Page 15 Line 13: Remove 'processes'.

Comment 30: Page 15 Line 21: Please explain 'left and right' samples. Do the authors mean sample duplicates? This sentence needs restructuring.

Comment 31 Page 15 Lines 21 - 25: Major run-on sentence, which needs revision.

Comment 32: Page 15 Lines 21 – 25: If the snowpack was so thin and patchy – won't blowing soil/dirt be an issue for the measurements? Please explain how this is accounted for.

Comment 33 Page 16: What type of snow was present? Fresh snow or old snow? Please explain using the international snow classification. Also, how soon were samples collected after snow falls? Also, it would be assumed that since the study was conducted in January, during Chinese winter, some sites could have fresh snow, where others could be 'older' snow?

Comment 34 Page 16 Lines 14 - 19: Run-on sentence again. Please separate into two sentences. 'The results from the 2010 – 2014 Chinese snow surveys are shown in. . .)

Table 1: Needs to be reorganized by dates collected (i.e. 2010 and 2014). Also include the number of samples incorporated in the average values. Sample depth is confusing. . . so the samples were collected over 5 cm and integrated into one measurement? What about the surface samples where less volume was collected at sites that were particularly dusty/polluted, i.e. Page 7 Line 4. Separate surface samples and

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snow pit data. Also, it appears

Table 2: Include snowpack depth for each site, to see how snow pack depth varied from 2010-2014 as that may have influenced measured BC content. Also, given the method used, should $BC = eBC$? And was this 2010 data published previously? If so, include a citation. If the 2010 data has been published before, then it appears that there are only 12 new data points in this study from 2014. Why are sites 41 – 46 listed with sample dates but no avg BC value?

Table 3: Wasn't this data publishes in Wang 2015, from sub section 2.2 Chemical Speciation Page 7 Line 13? If not, or if so clarify either way, provide references, and explain why the data is being presented again and what is new.

Figure 4: Site numbers should be included with the map and the 2014 data should only be presented since the 2010 data is already published? Atleast a distinction should be made between the 2010 and 2014 data. They should not be averaged together as there are many factors that would influence the differing values. Figure 5: These data do not appear to be normally distributed as they are heavily weighted on the low end, and just a couple larger concentrations. Also provide the p-value to show the significance of the fit.

Figure 6: Again, like Figure 5, these data do not appear to be normally distributed as they are heavily weighted on the low end, and just a couple larger concentrations. Also provide the p-value to show the significance of the fit. Why is there one red dot at 0.25 AL and 1.5 K+?

Figure 7: This is a nice figure. The land cover legend should be mentioned in the figure legend.

Figure 8: As in Figure 7, the land cover legend should be mentioned in the figure legend.

Figure 9: Why is only the visible range presented? Please clarify in Figure legend.

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Comment 34 Page 16 Lines 14 - 19: The reader doesn't understand this sentence. Are these averaged values from the surface samples?

Comment 34 Page 16 Line 19: Include standard deviations with the ranges. How many samples were in this range?

Comment 35: Page 17 Line 5: Include a reference to the table where this data is displayed.

Comment 36: Page 17 Line 11: Where is this regression value from? Reference figure.

Comment 37: Page 18 Line 2: Please explain how photochemical reactions are related to biomass burning contributions of OC?

Comment 37: Page 18 Line 3: p-value? And please include for the other figures.

Comment 38 Page 19: The reader finds this paragraph confusing. Are the percentages from Figure 7? If the samples were collected in January 2014, why are there seasonal/time references? i.e. Line 11 "Sulfate peaks were found in summer... this should be clearly tied to the Zhang 2013 reference. If the next sentence, line 12 – 17 (Which is a very long run-on sentence) also refers to Zhang 2013 then this needs to be more clearly stated. The combination of presenting results from this study and Zhang 2013 needs to be reorganized more clearly. For example a paragraph explaining Zhang 2013 could come first, and then compare this study to those. Also Zhang 2013 should probably be more clearly explained in the introduction. Again, it also appears that Wang 2015 already published snow chemistry data from these results? This needs to be more clearly referenced in the paper.

Comment 39 Page 20 Line 1: Include citation for iron originating from industrial emissions.

Comment 39 Page 20 Line 3: Is this sentence about 'this' study, or Doherty 2014?

Comment 39 Page 20 Line 6: 'Here, light absorption...'

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Comment 40: Page 21 Line 4- 6: This has already been stated and should be in the methods.

Comment 41 Page 21: The reader is not sure what the authors mean by ‘Higher degrees of snow albedo...’ do they mean more reduction or a high albedo? ‘There was a larger reduction in snow albedo for...’

Figure 10: Include labels for A, B, C. Same goes for all other figures.

Comment 42 Page 22: Why is BC now referred to as the ‘ BC mixing ratios’. Please clarify where this change in terminology was introduced.

Comment 43: Overall, ILAPs is not used consistently in the paper as light absorbing aerosols is written out many times. The reader suggests to standardize this.

Comment 44 Page 23 Lines 23 – 35: Please explain why, “...this study shows that the spectral albedo of snow reduction caused OC levels to increase?” Shouldn’t the reduction in spectral albedo be a result of the OC levels?

Comment 45: The paper needs a lot of reorganization. For a 25 page paper, one page of Discussion does not seem adequate. Rename Subtitle 4, “Conclusions”.

Comment 46 Page 24 Line 4: Include the 13 sites also in the abstract. This description from lines 3 – 7 should be included in the site description and does not need to be repeated in the conclusion. Comment 47: Page 24 Line 22: Please state the range in latitudes covered in this study in the site description. Did the 13 sites cover a large latitudinal gradient?

Comment 48 Page 25 Line 13: The last sentence of the entire manuscript seems like an odd conclusion, ‘to include OC as an input parameter in the SNICAR model’. Why not argue for the use of the SAMDS model? Does it not include OC as an input parameter? A better conclusion would be to compare the SAMDS and SNICAR models. Is the reason the SNICAR model is suggested to incorporate OC because it is widely used? Did the authors try incorporating OC as an input parameter in the off-line SNICAR

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code? The discussion and conclusion of this manuscript needs revision.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-667, 2016.

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