Anonymous Referee #1

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We are very grateful for the referee's critical comments. The followings are our point-by-point responses to the comments. Our responses start with "R:".

General comments

This paper presents a method for estimating the radiative forcing due to light-absorbing particles (LAPs) in snow (RFLS) using several data sources, which include MODIS albedos, snow grain size derived from MODIS data, snow depth from the ERA-Interim reanalysis, surface downwelling solar radiation from CERES, and finally, in situ measurements of BC in snow (used for computing correction factors for the algorithm). The proposed approach allows the estimation of RFLS in larger areas than would be possible with in situ measurements alone. It thus provides an additional data source complementing estimates from in situ data and climate models. As noted in the introduction, there are previous studies that utilized MODIS to retrieve the radiative forcing of LAPs in snow, but this might be the first one to consider the spatial variability in RFLS between different regions. The approach is further employed to analyze the factors underlying the spatial variation of RFLS, finding that the variations in LAP content, snow depth and geographical factors (e.g., latitude) are more important than those in snow grain size (Fig. 7). Furthermore the retrieved values of RFLS are compared with results from a few climate models (Figs. 8 and 9) and with previous studies (Fig. 10).

A practical limitation of the proposed approach is that it can only be applied in regions with no/very short vegetation. Also, judging by the correction factors needed to eliminate systematic differences to RFLS derived from in situ data, it appears that the approach works fairly well in heavily polluted regions, but for regions with relatively clean snow, the uncertainties are very large (Fig. S2b). So if one interpretes "hemispherescale" values (p. 6, line 17) as "hemisphere-mean" values, they cannot yet be obtained with this approach.

There is certainly enough new material in this work to be published in ACP. The paper is reasonable well written especially as regards the description of the approach, but I think there are disturbingly many numerical values in the text towards the end, and possibly some apples-to-oranges comparisons.

R: Thank you very much for the positive comments, which will encourage us to do more in-depth research in the future. Moreover, the referee's comments are quite significant that can help us to improve the paper quality substantially. We have addressed all of the comments carefully according to the suggestions. Especially, we have extended the study period from January-February to December-May, so that the snow cover area over the Arctic can be retrieved. We have replaced the clear-sky radiative forcing with all-sky radiative forcing, which makes more sense to the research community. We have recalculated the broadband snow albedo with wavelengths of 300-2500 nm. We have revised misleading descriptions and reduce some numerical values throughout the manuscript according to the suggestions. All of the detailed responses can be seen as follow.

1. It is not justified to "sell" the values averaged over all ISCAs as the Northern Hemisphere (NH) mean values (e.g., p.2, lines 11-13, and p. 34, lines 6-7) since they really represent only a small part of the NH land area. The approach samples only areas with (nearly) full snow cover and no/very short vegetation, which naturally results in a high bias in the computed "NH average" RF. The assumption of clear-sky conditions further increases the RF values, while the analysis of only January and February data decreases the RF in the Arctic, but perhaps increases it at midlatitudes, compared to annual-mean values. In general, you should avoid listing numerical values without explaining what they really mean, especially in the abstract.

R: The referee's opinions are very valuable. We have replaced "Northern Hemisphere (NH) averaged radiative forcing" with "radiative forcing averaged over mapped snow-covered area in Northern Hemisphere" and revised the similar issues throughout the manuscript. Moreover, we have recalculated the all-sky radiative forcing to replace the clear-sky radiative forcing and extended the study period of only January and February to December to May. In addition, we have revised the abstract and main text carefully to avoid the values without certain explanation.

2. Specifically, the abstract should state that these are clear-sky values, that the albedo reduction refers to wavelengths 300-1300 nm, and the RF values refer to areas with full snow cover and little/no vegetation above snow.

R: We have recalculated the broadband snow albedo with wavelengths of 300-2500 nm under all-sky condition. We have stated the "estimated radiative forcing" as "…radiative forcing except for midlatitude mountains in December-May for the period 2003–2018…over mapped snow-covered area in Northern Hemisphere " in the abstract and throughout the manuscript.

3. p. 6, lines 14-16, Section 2.4, and Section 4.6: Taking only two CMIP6 models, and calling them "CMIP6" or "CMIP6 ensemble mean" is misleading, especially as the two models (CESM2 and CESM2-WACCM) are very closely related and produce nearly identical results (Fig. S4). It would be advantageous to use data from more CMIP6 models, if data from more models has now become available. If not, just take CESM2 and call it CESM2! In addition, instead of a "Global climate model", you should use the specific model name for Flanner et al. (2009), that is CAM3.1. Incidentally, it is a predecessor of the atmospheric and land components of CESM2.

R: Thanks very much for the explanations and suggestions. Actually, we have limited knowledge of climate models and the referee's comments help us improve the understanding about CESM2 and CAM3.1. We have removed the comparison about CAM3.1 because it is the predecessor of the atmospheric and land module of CESM2 as the referee mentioned. We have carefully revised the improper description throughout the manuscript.

4. p. 7, line 6 and elsewhere: Why do you only use data for January and February? The reason for this should be stated explicitly. Perhaps because the midlatitude snow cover is most extensive then? However, this choice screens out almost all of the Arctic, due to the low sun angles, so that the “Arctic” RFLS values is this work in practice only represent southern Greenland. Also, considering spring months would increase the Arctic RFLS values substantially

R: The referee’s comments are quite significant. We have updated the data from January-February to December-May, so that the study period can include winter and spring, and snow-covered areas over the Arctic have been mapped.

5. p. 9. A brief description of the in situ BC measurements employed to correct the RFLS values should be included in Section 2 (at least, regions and references), perhaps between current Sections 2.2 and 2.3. Do these measurements represent BC or LAPs in general?

R: We have added more details about in-situ measurements in Sect. 2.3. These measurements are equivalent BC, which can represent the all light absorption by LAPs. We have added a detailed explanation for “equivalent BC” in p. 9, lines 17-21.

6. p. 9, lines 7–10: What was the reason for converting SWE to snow depth? To my knowledge, this has no effect on the results (in the end, SNICAR cares of SWE only).

R: Actually, SNICAR cares of SWE only. However, the offline SNICAR requires both snow depth and snow density as input, so that we converted SWE to snow depth with an assumed snow density. Anyhow, as your say, this has no effect on the results.

7. p. 9, Section 2.3. It should be stated how/why these emission data were used. I get the impression that they were used just as background information (not in estimating the RFLS).

R: Indeed, BC emission and deposition data were used just as background information. So that we have moved Figure 4a and 4b to the supplements (Figure S2a, b).

8. p. 11, lines 11-16: You describe how SBDART has several options for defining the atmospheric properties. It would be more important to tell what was assumed in the present calculations (also regarding aerosols).

R: We have added the description about the options for defining the atmospheric properties in SBDART. Details can be seen in p. 11, lines 3-7:

"In our study, the subarctic and midlatitude winter standard atmospheric condition is performed as well as the tropospheric and stratospheric background aerosols are archived in SBDART (Tanre, D. et al., 1990). According to Dang et al. (2017), the cloud optical depth in high-latitude and mid-latitude was assumed as 11 and 20 under cloudy-sky condition, respectively."

9. p. 12, line 11: You could add snow grain shape to this list.

R: We note that SNICAR only assumes a spherical snow grain. We have added the description about snow grain shape as "…and spherical grain shape.'' in p. 11, line 20.

10. p. 12, lines 20-21: "previous studies have tended to assume a semi-infinite snowpack". This is a good point, and I think it would be worth showing how much this influences the results. Consider adding a figure which shows the ratio of RFLS computed using the actual (ERA-Interim) snow depth vs. RFLS computed using semi-infinite snow.

R: As the referee's suggestion, we have added Figure 7 to show the ratio of RFLS computed using the actual (ERA-Interim) snow depth vs. RFLS computed using semi-infinite snow and taken a discussion about the influence of snow depth on radiative forcing retrieval in Sect. 4.4.

11. p. 13, line 20: add "...for clear-sky conditions" at the end of the sentence.

R: We have replaced clear-sky radiative forcing with all-sky radiative forcing throughout the manuscript.

12. p. 16, Eq. (7): Please state explicitly that the impact of LAPs on snow albedo computed in this work refers to the spectral range 300-1300 nm only. There is a chance of misinterpretation here, as usually people think of broadband albedo integrated over the entire downwelling solar spectrum at the surface. (An alternative would be to calculate "real" broadband albedo changes, integrated over 0.3–4 µm or at least 0.3–2.5 µm). This choice should not matter for RFLS, however.

R: Thanks for the referee's suggestion. We have recalculated the broadband albedo with wavelengths of 300-2500 nm.

13. In Eq. (2), diffuse and direct spectral solar radiation are added as such ($E_{dif}(\lambda; \varphi)+E_{dir}(\lambda; \varphi)$), suggesting that they both are defined wrt. a horizontal surface, but in Eq. (7) (and Eqs. (10) and (11)) the direct radiation is weighted by the cosine of local solar zenith angle ($E_{dir;\lambda} \cos \beta + E_{dif;\lambda}$), which implies that the direct radiation is defined wrt. a surface perpendicular to Sun's direction. This seems inconsistent.

R: We have revised this inconsistency throughout the manuscript.

14. p. 17, line 7: "we assumed that the properties for snow and LAPs remain invariable throughout the day". In fact, if you keep the snow physical properties and LAP concentration constant, the impact of LAPs on snow albedo decreases with increasing solar zenith angle, so the use of $\Delta\alpha_{MODIS,corrected}^{LAPs}$ evaluated at noon probably overestimates the daily-average impact of LAPs somewhat. (I would guess, perhaps of the order of 10%, but this is something that you could check with SNICAR.)

R: We have corrected the overestimates by further simulating the daily-average snow albedo by changing the solar zenith angle from sunrise to sunset using SNICAR model and SBDART model. Revisions are added in p. 16, lines 11-16 and as follow:

"Following Miller et al. (2016), we assumed that the properties for snow and LAPs remain invariable throughout the day. Based on calculated $\alpha_{snow,\lambda}^{mdl}$ and $\alpha_{snow,\lambda}^{MODIS}$ at noon, the diurnal variation of pure and polluted snow albedo can be simulated by SNICAR from sunrise to sunset. Then, daily-average snow albedo reduction ($\Delta\alpha_{MODIS,daily}^{LAPs}$) can be derived by integrating the diurnal snow albedo reduction, which is weighted by simultaneous solar irradiance from SBDART."

15. In Eq. (11), is cos β the daytime mean value?

R: We have new algorithm. cos β is calculated based on the certain latitude and solar zenith (solar azimuth) from sunrise to sunset.

16. p. 22, lines 16-17: You should remind the reader that this result refers specifically to the months of January and February. In spring and early summer, much of the Arctic is still snow-covered and solar radiation is much more abundant, so RFLS is substantially larger than in January-February.

R: We have updated the data from January-February to December-May, so that the study period can include winter and spring, and snow-covered areas over the Arctic have been mapped.

17. p. 22, line 20. "In situ observations of snow albedo reduction" actually refer to the albedo reduction calculated using in-situ observed LAPs. Here, it should be noted what was the measure of LAPs used in the in situ observations? Was it BC (excluding dust) or equivalent BC (implicitly also including dust). I guess in-situ obervations usually yield the latter?

R: We have revised "in situ observations of snow albedo reduction" as "Albedo reduction calculated using in-situ observed LAPs ($\Delta\alpha_{in-situ,daily}^{LAPs}$)…" in p. 22, line 15. Also, we have added a statement that the measure of LAPs was equivalent BC in p. 9, lines 17-21.

18. p. 22, line 11. These corrections deserve a bit more discussion. The value $c_{polluted}$ = 1:1 suggests that the approach works rather well for heavily polluted snow. However, the value c_{clean} = 5:6 for "relatively pure" snow, along with the scatter of points in Fig.

S2, suggests that the method becomes quite inaccurate then. Can you comment on the possible reasons for that? Perhaps the limiting factor is simply the accuracy of albedo calculations and observations, and a possible systematic bias between the two? For example, for 100 ng/g of BC (which many would already consider not so clean snow!) the albedo reduction is only ~0.02. So, if in Eq. (7) $\alpha_{snow;\lambda}$ mdl is biased high and/or $\alpha_{snow;\lambda}$ MODIS is biased low, this would result in c > 1, the more so the cleaner the snow.

R: We have added a discussion about the uncertainty of the snow albedo reduction retrieval, which is negative correlated to snow pollution condition, to demonstrate the low correction value for heavily polluted snow but high correction value for relatively pure snow. We also discussed the influence of in-situ observation on the correction factor as suggested. Details can be seen in p. 27, lines 18-21 and p. 28, lines 1-18.

19. p. 23–26: I think the large number of numerical values in the text is disrupting to the reader. Some concrete suggestions would be: 1) for p. 23, lines 14-20 provide the MAE and RMS statistics in the figure panels in Fig. 5, 2) in Section 4.4., put the numerical values in a table. If you prefer to keep them in the text, you could at least skip the instantaneous RF values.

R: We have simplified the number of numerical values in the text for avoiding to disrupt the reader throughout the manuscript and put the MAE and RMSE statistics in Table S1 in supplements as suggestions. We have put the general statistics of snow albedo reduction and radiative forcing in Sect. 4.4 in Table 1 and we prefer to keep the values in different regions in the text in detail. Finally, we removed the discussion of instantaneous RF values in the text.

20. p. 28, line 10 – p. 29, line 7: As noted above, the model used by Flanner et al. (2009) should be called “CAM3.1” rather than “GCM”. More importantly, you discuss

springtime RF for Flanner et al. Did you compute springtime values for the MODIS retrievals too? This should be made clear in the text. Comparing January–February values with springtime (March–May?) values would be meaningless.

R: We have removed the comparison about CAM3.1 because it is the predecessor of the atmospheric and land module of CESM2 as the referee mentioned.

21. p. 31-32 and Fig. 10. The comparison with previous radiative forcing estimates is interesting, but one should be careful not to compare apples with oranges let alone watermelons – or at least be explicit about when this is being done. In other words, I think you should provide more information about the previous studies considered here. The RF differences could arise from the consideration of different regions, different seasons, clear-sky vs. all-sky forcing etc., so these details should be mentioned. This information would probably best fit in a table.

R: As the referee's suggestions, we have added a table (Table 2) about the detailed information of the previous studies and revised the discussion about the possible sources of the RF differences. Details can be seen in Sect. 5.

22. p. 31, line 21 – p. 32, line 1: "Miller et al. (2016) reported a daily RFLS of < 4

W m−2". Figure 10b (2nd panel) shows much larger values.

R: We have replaced Figure 10 with Table 2. We have rechecked the reference and revised as follow:

"…Miller et al. (2016) reported a daily RFLS of ~35-86 (37-100) W m^{-2} based on in-situ measurements (remote sensing) in the San Juan Mountains in May 2010."

23. p. 32, line 7: Should this be Qian et al. (2014) or Qian et al. (2009) (cf. Fig 10c, second panel).

R: Thank you for pointing out the mistake, it should be "Qian et al. (2009)". We have rechecked all data in section 5.

24. p. 32, lines 16-17: It is stated that Wang et al. (2014a) reported a northern hemisphere RFLS value of 0.45 W m^{-2}. However, so far I can tell, that paper is concerned with the direct radiative forcing due to BC in air (not snow). Furthermore, Fig. 10c refers to Wang et al. (2004), which is not present in the reference list.

R: Thank you for pointing out the mistake. "Wang et al. (2004)" should be "Wang et al. (2014a)". In addition, Wang et al. (2014a) only reported the RF due to BC in air actually and has nothing to do with snow. We have removed it and rechecked all data in section 5.

25. p. 32, lines 15–21. I think your explanation is in principle correct, although at least the values of Bond et al. (2013) and Hansen and Nazarenko (2004) are annual-mean values, not January-February. But the fundamental point here is that your approach cannot provide northern-hemisphere (NH) mean values, which the cited studies attempt to provide, uncertainties notwithstanding. It can only provide values for ISCAs that are snow-covered and without much vegetation. For true NH mean values, you should also include forested regions and regions without snow, and even oceans and sea ice, and also consider the impact of clouds. It is obvious that your reported NH values are larger than the actual NH mean forcing.

R: We have revised the description of our RF as "radiative forcing averaged over mapped snow-covered area in Northern Hemisphere" and added a table about the detailed information of the RF from previous studies to demonstrate the difference.

26. p. 34, lines 5–6. Referring to the previous comment, I would much prefer the formulation "for the Northern Hemisphere ISCAs as a whole ...".

R: According to your suggestion, we have revised "For the Northern Hemisphere as a whole …" as "For the Northern Hemisphere ISCAs as a whole ...".

27. p. 35, lines 11–13. Climate models cannot incorporate remote sensing retrievals directly. They could however be used for model validation and to guide model development.

R: This sentence has been revised as "We propose that climate models validated by these refined remote sensing retrievals should be able to capture the RFLS more accurately, thereby providing more reliable estimates of the future impacts of global climate change."

28. p. 62, caption of Fig. 7. It should be indicated whether the lower panel refers to instantaneous or daily radiative forcing.

R: We have revised Figure 7. The attribution refers to daily RF.

29. p. 63, Fig. 8: A couple of things to be checked: 1) Are the Flanner et al. results allsky or clear-sky values; 2) do the $RF_{MODIS,daily}$ values represent January-February (as in the rest of the paper) or spring? It would not be meaningful to compare Jan-Feb vs. March-May.

R: We have removed the comparison about CAM3.1 because it is the predecessor of the atmospheric and land module of CESM2 as the referee pointed out.

30. Fig. S5: It is inconsistent to compare springtime radiative forcing in (a) with radiative forcing based on CMIP6 (i.e., CESM2) soot content in snow in January-February in (b).

R: We have revised Figure S5. We have removed the comparison about CAM3.1 because it is the predecessor of the atmospheric and land module of CESM2 as the referee pointed out. When comparing with CESM2, the MODIS retrievals are the averages of December-May.

1. p. 4, lines 2-3: This sentence is cumbersome. Suggestion: “As a result, persistent uncertainties remain in regional and global-scale RFLS estimates based on field measurements.”

R: We have revised this sentence as suggestion.

2. p. 4, line 7: add “explaining” before “approximately one quarter of observed global warming”.

R: Added as suggestion.

3. p. 6, line 18: replace “valuable parameters” with “valuable information”. (The reason is explained in the specific comment #27).

R: Revised as suggestion.

4. p. 7, line 18: replace “generated by” with “derived from”.3

R: Revised as suggestion.

5. p. 8, line 4: replace “solar radiation” with “solar radiances”.

R: Revised as suggestion.

6. p. 11, lines 11-12: “standard aerosol types”?

R: Thank you pointed out the grammatically wrong sentence. We have Revised.

7. p. 12, line 4: “indicent radiation, surface spectral distribution”. Do you mean “incident radiation at the surface and its spectral distribution”?

R: Revised as suggestion.

8. p. 14, line 5: I think this should be “both of which are required to exceed 0.6”.

R: Revised as suggestion.

9. p. 18, lines 4-6: I think this sentence should be moved after Eq. (15): “The spatial variability in snow albedo due to ILAPs can be expressed as Eq:(15) where R_{eff}, SD and G indicate spatial-mean values of R_{eff}, SD, and G, with G requiring spatially constant values for the solar zenith angle, surface topography, and solar radiation parameters”.

R: Revised as suggestion.

10. p. 22, line 5: “respectably” should be “respectively”.

R: Revised.

11. p. 35, line 3: It is not clear what “synthetically” means here.

R: We want to express “relatively comprehensive and systematically”. If the referee still consider “synthetically” in unreadable we will revise it in next version.

12. Figure 1 (and also Fig. S1) would be easier to read if the values given on the colour bars would match with the values used to draw the curves. Now it is difficult to say which LAP content, snow depth etc. each curve exactly represents. Also, in the caption of Fig. 1, “angel” should be “angle”.

R: Revised as suggestion.

13. In Fig. 3, “savannas” should probably be “tundra”?

R: Revised and Figure 3b has been replotted.

14. In Fig. 6 and also Fig. S5, the interpretation of the box-plots should be explained.

R: The interpretation of the box-plots had been added in this version.

15. In Fig. S3b, “confindence level” should be “confidence level”.

R: Revised.

References

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