Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-50-RC1, 2020

Interactive comment on "Satellite-based radiative forcing by light-absorbing particles in snow across the Northern Hemisphere" *by* Jiecan Cui et al.

Anonymous Referee #1

Received and published: 13 May 2020

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 intro-

duction, 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 "hemisphere-scale" 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.

Specific comments

1. It is not justified to "sell" the values averaged over all ISCAs as the Nothern 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 vegation, 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.

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.

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 precedessor of the atmospheric and land components of CESM2.

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.

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), per-

haps between current Sections 2.2 and 2.3. Do these measurements represent BC or LAPs in general?

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).

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).

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).

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

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.

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

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.

13. In Eq. (2), diffuse and direct spectral solar radiation are added as such ($E_{\text{dif}}(\lambda;\phi)+E_{\text{dir}}(\lambda;\phi)$), 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 ($E_{\text{dir},\lambda}\cos\beta + E_{\text{dif},\lambda}$), which implies that the direct radiation is defined wrt. a surface perpendicular to Sun's direction. This seems inconsistent.

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 $\Delta\alpha^{\text{LAPs}}_{\text{MODIS,corrected}}$ 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.)

15. In Eq. (11), is $\cos\beta$ the daytime mean value?

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.

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?

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) $\alpha^{mdl}_{snow,\lambda}$ is biased high and/or $\alpha^{MODIS}_{snow,\lambda}$ is biased low, this would result in $c > 1$, the more so the cleaner the snow.

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.

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.

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.

22. p. 31, line 21 – p. 32, line 1: "Miller et al. (2016) reported a daily RFLS of < 4 $\mathrm{W\,m^{-2}}$". Figure 10b (2nd panel) shows much larger values.

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

24. p. 32, lines 16-17: It is stated that Wang et al. (2014a) reported a northern hemisphere RFLS value of 0.45 $\mathrm{W\,m^{-2}}$. 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.

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 wihout 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.

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 ...".

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.

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

29. p. 63, Fig. 8: A couple of things to be checked: 1) Are the Flanner et al. results all-sky or clear-sky values; 2) do the $RF_{\mathrm{MODIS,daily}}$ 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.

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).

Technical and language corrections

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."

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

3. p. 6, line 18: replace "valuable parameters" with "valuable information". (The reason

is explained in the specific comment #27).

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

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

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

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

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

9. p. 18, lines 4-6: I think this sentence should be moved after Eq. (15): “The spatial variability in snow albedo due to I_{LAPs} can be expressed as

$$Eq.(15)$$

where $\overline{R_{\mathrm{eff}}}$, $\overline{SD}$ and $\overline{G}$ indicate spatial-mean values of R_{eff}, SD, and G, with $\overline{G}$ requiring spatially constant values for the solar zenith angle, surface topography, and solar radiation parameters”.

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

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

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”.

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

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-50, 2020.