

Replies to the Editor's comments:

The reviewer is still not satisfied that some of their comments have been adequately addressed. I have reviewed these carefully and this is my judgment:

The reviewer is not satisfied that their original point #3 has been addressed, however having read both sets of comments and the authors' response, it is not clear to me what their outstanding problem is beyond finding the results unintuitive. Each of their bullet points was addressed individually in the rebuttal and explanations are given in the revised text, which seem adequate to me. Given the reviewer was not specific about any outstanding concerns, I do not see that further modifications are necessarily required, but I would still like to extend the opportunity to the authors to acknowledge the unintuitive nature of some of the results in the text, as seemingly strange results could foster misunderstandings and undue scepticism among future readers. However this is not a requirement for publication and I leave this to the authors' discretion.

We thank the editor for their careful consideration of the manuscript. Following their suggestion we now include an acknowledgement of some of the potentially unintuitive results.

This BC enrichment in snow at a few places, however, has no influence on the fact that overall the COVID-19 measures reduced the BC-in snow concentration and thus increased the visible snow albedo (see Fig. 4g-i)." L242-244

The reviewer also mentions their original point #4, which the authors respond to and offer [an](#) additional explanation for, but this inevitably means that any atmospheric effects would need unpicking from the snow melt effects for the conclusions of the paper to stand. On line 241 of the revised version, the authors state "...but this does not affect the overall conclusion as to the albedo effect." but do not qualify this statement with anything. I would ask that the authors expand on this statement and explicitly include their reasoning behind it. I'm hoping this is a trivial modification, but this is of high importance for the strength of the thesis.

Reply: To clarify what we mean here, we have now rewritten the sentence

"..but this does not affect the overall conclusion as to the albedo effect"

as

"This BC enrichment in snow at a few places, however, has no influence on the fact that overall the COVID-19 measures reduced the BC-in snow concentration and thus increased the visible snow albedo (see Fig. 4g-i)." L242-244.

The final comment in the second review is that the original comment #2 is not adequately addressed in that the model treatment referred to only addresses BC. If this is the case, please can this be pointed out accordingly, as requested by the reviewer? If the reviewer is mistaken, please can the text be modified to avoid future confusion?

Reply:

Here, we wish to clarify that our model simulations include the airborne sulfate, OC, BC, and dust aerosol, together with the BC-in-snow aerosol (while OC-in-snow and dust-in-snow are not included). We believe this is mentioned clearly in the manuscript as below:

The reduction in anthropogenic air pollution leads widely to a reduction in BC concentration in the snow of approximately 25 - 350 $\mu\text{g kg}^{-1}$ (by 12 – 35 %) during spring 2020 (Fig. 3a-c) that reduce the snow darkening effect by embedded aerosol impurities. (L141-143).

Therefore, the reduction of anthropogenic sulfate, OC, and BC burden, combined with lower atmospheric loadings of PM_{2.5} and PM₁₀, as well as BC in snow resulted in decreased heating of the snowpack and tropospheric column. (L157-160).

Replies to the reviewer's comments

We thank the reviewer for careful reading of the manuscript. We have included the further suggestions given by the reviewer in the revised version. These changes are indicated in blue color at the line numbers indicated in the replies.

While the authors have attempted to address all my previous comments, key results in Figures 3-5 related to my comments #3 and #4 still look unclear and even mysterious to me, which makes me hesitant to recommend the manuscript for publication. However, I choose to believe that the authors have done a careful analysis, and some of the climate model results are just not very intuitive. One remaining concern is the response to my comment #2. I was pointed to S1.2 for the model treatment of dust and absorbing OC (or brown carbon) in snow, but as it indicates, the snow darkening effect is only treated for BC, not for dust and brown carbon, which should be stated explicitly in the manuscript.

Reply:

Here, we wish to clarify that our model simulations include the airborne sulfate, OC, BC, and dust aerosol, together with the BC-in-snow aerosol (while OC-in-snow and dust-in-snow are not included). We believe this is mentioned clearly in the manuscript as below:

The reduction in anthropogenic air pollution leads widely to a reduction in BC concentration in the snow of approximately 25 - 350 $\mu\text{g kg}^{-1}$ (by 12 – 35 %) during spring 2020 (Fig. 3a-c) that reduce the snow darkening effect by embedded aerosol impurities. (L141-143).

Therefore, the reduction of anthropogenic sulfate, OC, and BC burden, combined with lower atmospheric loadings of PM_{2.5} and PM₁₀, as well as BC in snow resulted in decreased heating of the snowpack and tropospheric column. (L157-160).