Replies to the reviewer-I

In "Air pollution reductions caused by the COVID-19 lockdown open up a way to preserve the Himalayan snow cover" a chemistry-climate model is used to show that reductions in emissions during the COVID-19 lockdowns led to decreased melt over most of High Mountain Asia. The simulations are supported by remote sensing and in situ aerosol measurements. This work is well motivated, the science is sound, and the results are convincing. I recommend publication subject to minor revisions.

Reply: We thank the reviewer for appreciating our efforts and for their suggestions. We have now incorporated the suggestions in the revised manuscript (in blue colour at the line numbers indicated in the replies).

The main issues are:

1. The authors need to discuss the limitations of the spatial resolution of their grid size in the ECHAM6-HAMMOZ model (1.875x1.875 deg). This is coarse. For example, Sarangi et al. (2020) use a 12 x 12 km (~ 0.10 degree) grid in WRF-Chem-SNICAR simulations to model LAPs over much of the same region. At 1.875 deg, many of the Himalayan sub ranges are smaller than a pixel, thus the topographic influences, which are always substantial in the mountains, are not accounted for. One usual effect is that snowfall and snow on the ground are underestimated (e.g., Liu et al., 2022). This coarse grid size can impact the anamolies found here as the changes in snow mass are small, at most +16 mm, and the bias in the likely underestimated snow mass may change between the control and COVID simulations because the topography is not fully accounted for.

   Reply (1) : We agree this is an important point and we thank the reviewer for the suggestion. We have now added a discussion on the limitations of the spatial resolution as below at L371-381.

   “A limitation of our simulation is the relatively coarse spatial resolution in the ECHAM6-HAMMOZ model (1.875°x1.875°). Other studies used a finer spatially resolved regional model; for example Sarangi et al. (2020) use a 12 x 12 km (~ 0.10°) grid in the regional WRF-Chem-SNICAR model over the same region. In our model grid of 1.875°, many of the Himalayan sub ranges are smaller than a pixel, and, hence, the topographic influences, which are substantial in the mountains are limited. One effect may be that snowfall and snow on the ground are underestimated (e.g., Liu et al., 2022). The coarse grid size can impact the anomalies found here as the changes in snow mass are small, at most +16 mm, and the bias in the likely underestimated snow mass may change between the control and COVID simulations. Biases are, however, the same in the control and COVID simulations and, thus, their effects will be diluted when we compute the anomalies.”

2. The authors find that "pollution changes in COVID-19 lockdown period and associated changes in meteorology has increased snowfall" (l 235-236). I do not understand the mechanism for increased snowfall from emissions reductions. Please elaborate.

Reply (2): It is now elaborated as “Our model simulations show that air pollution reductions in the COVID-19 lockdown period and associated changes in radiative forcing caused changes in the tropospheric circulation and thermodynamics (see Fadnavis et al., 2020 for a detailed analysis). These changes in meteorology have increased snowfall by 2-5 mm day^{-1} (3-20 %) over the Western Himalayas and Tibetan Plateau region (Fig. 5 j-l).” (L235-240).

3. There are many careless typographical and grammatical errors. I've noted many, but not all of these in an annotated PDF. I suggest an English language editing service. The rest of my comments are minor and are in the attached PDF.

   Reply(3): Thank you for the suggestions given in the PDF file. All the suggested changes are now incorporated in the revised manuscript. The changes are marked in blue color.

Works cited:

Liu, Y., Fang, Y., Li, D., and Margulis, S. A.: How Well do Global Snow Products Characterize Snow Storage in High Mountain Asia?, Geophysical Research Letters, 49, e2022GL100082, https://doi.org/10.1029/2022GL100082, 2022.

Sarangi, C., Qian, Y., Rittger, K., Ruby Leung, L., Chand, D., Bormann, K. J., and Painter, T. H.: Dust dominates high-altitude snow darkening and melt over high-mountain Asia, Nature Climate Change, 10, 1045-1051, 10.1038/s41558-020-00909-3, 2020.

Replies to Reviewer-II

A chemistry-climate model is used in this study to assess the impact of COVID lockdown and emission reduction on the Himalayan snowpack in March-May 2020. It is found that the lockdown led to a reduction of black carbon (BC) in snow (2-14%) and thus a large reduction in the snow melting (10-40%) and surface runoff (5-55%), which leaves an impression that the presence of BC in the snow is the single key factor causing snowmelt and runoff over the Himalayan snowpack. While results in the current are quite interesting, some of the analyses and conclusions are questionable. Below please see my specific comments and some major concerns.

Reply: In our model setup we have reduced aerosols and gases as per Google and Apple mobility data (see L356). We have further elaborated on the above lines as

Chemistry-climate model simulations, supported by satellite and ground measurements, show that lower gases and aerosol pollution during lockdown led to changes in meteorology, and a reduction in black carbon in snow (2-14%) and thus in snow melting (10-40%). (L18-21)

We have also already mentioned that the emission changes in the model, affected the meteorology, that has impacted the Himalaya snow (see at L235-240), "Our model simulations show that air pollution reductions in the COVID-19 lockdown period and associated changes in radiative forcing caused changes in the tropospheric circulation and thermodynamics (see Fadnavis et al., 2020 for a detailed analysis). These changes in meteorology have increased snowfall by 2-5 mm day^{-1} (3-20 %) over the Western Himalayas and Tibetan Plateau region (Fig. 5 j-l)."

Also in section 3 "Summary and conclusion" we have already stated that "Our model simulations indicate that the associated reduction in anthropogenic aerosols and greenhouse gases in spring 2020 have benefited the HKH snow reservoirs. It caused an enhancement in the snow cover fraction by 6 - 12 % and snow mass by 2 - 20 %, corresponding to a decrease in snow melting by 10 - 40% and surface water runoff by 0.2 - 3 mm day^{-1}. As a consequence, the water content of the reservoir increased considerably by 4 to 59 %." (L279-284)

Therefore, we do not think that our manuscript gives the impression that "BC in the snow is the single key factor causing snowmelt and runoff over the Himalayan snowpack."

1. This study focuses on the impact of COVID lockdown and emissions reduction on Himalayan snowpack. I don't understand how the discussions of Himalayan glaciers and comparison to the melting rate due to climate warming since the Little Ice Age are relevant unless the authors think the BC influence on snowpack is also applicable to glaciers.

   Reply (1): We thank the reviewer for this point as we agree it was confusing. We have now removed that line from the revised manuscript.

2. The discrepancy in March AOD between MODIS and model simulation (Figure 2) is attributed to dust. Is the dust impact on HKH snow impurity considered in the model simulations as well? How about organic carbon (e.g., Brown et al., 2022)? Please clarify.

   Reply(2): We have considered impurities other than dust in our experimental set-up. In our model simulations, we have incorporated changes in BC, OC, and sulfate aerosol emissions. Thus, we have considered changes in organic carbon in our model (see section S1.2).

   In the model, we have also considered the impact of dust as well. Dust is parameterized as per soil type and wind speed. The same dust parametrization was employed in the CTL and COVID simulations. (L370 in section S1.2). In the COVID simulation, the reduction in air pollution affected the winds and therefore the wind-blown dust deposition over the Himalayas and Tibetan Plateau region. In our simulations this leads to a reduction in dust burden over these regions, as shown in Fig. S1c (details in section S3).

3. Model results (Figures 3 and 4): This part is particularly confusing, which leaves me many questions:

   (3a) Why is there a strong increase of BC concentration in the snow at some places (Fig. 3a-c)?

   Reply (3a): Thank you for the comment. There are many factors at play that may have led to an increase in BC concentration in snow in some locations. For instance, more efficient dry and wet deposition of BC can be noted in the Eastern Himalayas in May, which may have resulted from shifts in the large-scale circulation (Fig. S3 in supplement). Reduced snowfall would also lead to an enhanced concentration, as aging of snow on the ground, along with accumulation of BC following partial snow melt (BC is removed with lower efficiency than snow water) would result in higher BC concentration in surface snow compared to following fresh snowfall.

In the case of Afghanistan in March, given the low snow cover fraction in this area during this time of year, along with the weakly positive change in snowfall (Fig. 5j in manuscript) and negative change in runoff (Fig. 4d in manuscript), we suspect the increase in BC concentration may be due to an increase in the number of days with snow on the ground compared to snow-free days. Complete snow melt leads to a depletion of all accumulated BC in the surface snow, thus clearing the slate for lower concentrations in fresh snowfall. Given minimal fresh snowfall, less frequent occurrence of complete snow melt would therefore lead to higher BC concentrations. Additionally, averaging over fewer snow-free days in a month, which by definition contain zero BC in snow, would result in higher monthly mean BC concentrations.

Overall, however, these are edge cases in mountainous regions with sensitive dynamics during the spring season when snow cover is in flux. The general conclusion remains that lockdown measures result in less reduction of snow albedo due to BC. We have added a corresponding explanation in the manuscript at L144-L150.

○ (3b)How is the SW radiative forcing at the surface (Fig. 3d-f) calculated? If it is not explicitly induced by BC absorption in the land surface snow model, I don’t understand why it can be attributed to BC entirely. Dust in snow, surface temperature, snow cover, snow grain size, cloud cover, and snowfall/precipitation can all be different between the two experiments.

Reply: (3b) Thanks for pointing out this confusion. The above sentence is corrected as “Our simulations reveal that the decrease in BC-in snow concentration and the overall reduction in atmospheric pollution, as well as associated radiative effects have decreased the shortwave radiative forcing at the surface by 0.2 – 2 W m^{-2} in March – May 2020 (Fig. 3 d-f), leading to a decrease in tropospheric heating by solar radiation of 0.001 to 0.015 K day^{-1} (Fig. 3 g-i).” (L150-154)

○ (3c) Why is the change in tropospheric heating rate attributed to SW radiative forcing at the surface? I would think the atmospheric light absorbers and downwelling SW fluxes are more important. Otherwise, the quantities in the top, middle and bottom panels would be highly correlated.

Reply (3c): We agree. To clarify this point we have rewritten the above sentence as “Our simulations reveal that the decrease in BC-in snow concentration and the overall reduction in atmospheric pollution, as well as associated radiative effects have decreased the shortwave radiative forcing at the surface by 0.2 – 2 W m^{-2} in March – May 2020 (Fig. 3 d-f), leading to a decrease in tropospheric heating by solar radiation of 0.001 to 0.015 K day^{-1} (Fig. 3 g-i).” (L150-154)

○ (3d) 4(g-i) are described as surface albedo, but the units (W/m2) indicate radiative fluxes. What is the difference between Fig. 3(d-f) and Fig. 4(g-i)? Their spatial patterns are so different.

Reply(3d): Thank you for this point, we have now corrected the units. Fig 4 (g-i) shows mean visible albedo that has no unit while Fig 3 (d-f) shows short wave radiative forcing at the surface (W/m2).

- (3e) Section 2.2: How are the relative changes calculated? They seem to be too large. It could be just because of putting the smaller numbers in the denominator. If the numbers are derived from small areas, a significance test needs to be performed among the model ensemble members.
- Reply(3e): As mentioned in the methodology section S1.2 we performed 10 member simulations for control and covid-19. The relative difference is obtained as COVID-19 minus CTL. It is mentioned in the figure caption. The difference was not divided by CTL. We have shown hatched areas indicating the 95 %-significance level in all figures in section 2.2. In Fig. S7 we show monthly variation of anomalies of dust burden for the 10 members of the ECHAM6-HAMMOZ model simulations.

4. Figure 5: The snowfall rate in COVID experiment is larger than in the control experiment, while the snow cover fraction in some areas is significantly larger or smaller, which doesn't appear to be explainable by the LAP darkening effect on melting. This needs to be clearly explained and reconciled with the attribution to BC or LAP effects.

   Reply(4): We agree this is an important point. To clarify, the discussion on snowfall is now revised as

   "Our model simulations show that air pollution reductions in the COVID-19 lockdown period and associated changes in radiative forcing caused changes in the tropospheric circulation and thermodynamics (see Fadnavis et al., 2020 for a detailed analysis). These changes in meteorology have increased snowfall by 2-5 mm day^{-1} (3-20 %) over the Western Himalayas and Tibetan Plateau region (Fig. 5 j-l)." (L235-240).

5. Line 68-73: The citation of LAP impact does not seem to be accurate. The Bair et al. (2021) study focus on the Indus River Basin and the decrease of snow melt is based on estimates from an energy balance model (not directly from remote sensing measurements).

   Reply(5): Thank you for this point. We have now corrected this as remote sensing approaches instead of remote sensing observations. (see page 3, L68) As described in Bair et al (2021), they used 2 remote sensing approaches: (i) MODIS Snow-Covered Area and Grain Size plus Dust and Radiative Forcing in Snow, interpolated and smoothed to account for clouds and off-nadir views; and (ii) Snow Property Inversion from Remote Sensing, which accounts for snow containing LAPs and also provides interpolated and smoothed results.

6. Line 145-148: Dust burden is also much reduced in the COVID simulation (Fig. S1, S2, S6). Does that indicate the wet deposition rate is systematically different between the two runs? Please add an analysis on this.

   Reply(6): Thanks you for this suggestion. We have now included a new figure showing wet and dry deposition rate of dust (Fig. S2d-i) and added the following text.

"The lower dust load is related to the interactive change in atmospheric dynamics in the model, which also leads to changes in the wet and dry deposition rates of dust (Fig S2d-i)" (L130-131).

7. A1.1: how reliable is MODIS AOD over the bright snow surface?

   Reply (7): There are indeed uncertainties associated with the MODIS AOD data over snow, as documented by Huang et al (2020). We now clearly state this at L313-314 as follows:)

   " Uncertainty in MODIS AOD data over snow are documented by Huang et al (2020)."

   Huang, G., Chen, Y., Li, Z., Liu, Q.,Wang, Y., He, Q., et al. (2020).Validation and Accuracy Analysis ofthe Collection 6.1 MODIS Aerosol Optical Depth over the Westernmost City in China Based on the Sun-Sky Radiometer Observations from SONET.Earth and Space Science,7,e2019EA001041

8. A1.2: AEROCOM-ACCMIP-II emission inventory for year 2020 is used in the simulations. The references provided here are from more than 10 years ago. I wonder why not using CMIP6 or other modern emission inventories. How different is the configuration of model experiments from the CovidMIP protocol (Jones et al., 2021)? Please provide a discussion on this.

   Reply(8): We think that AEROCOM-ACCMIP-II emission inventory is well developed and hence used in this study. However, we are open to using other emission inventories for our future studies.

   Our model configuration is as per model experiments from the CovidMIP protocol. We have mentioned it in our manuscript at L366-367.

References:

Brown, H., et al. (2022). Brown carbon fuel and emission source attributions to global snow darkening effect. *Journal of Advances in Modeling Earth Systems*, 14, e2021MS002768. https://doi.org/10.1029/2021MS002768

Jones, C. D., et al. (2021). The climate response to emissions reductions due to COVID-19: Initial results from CovidMIP. *Geophysical Research Letters*, 48, e2020GL091883. https://doi.org/10.1029/2020GL091883