Black font is reviewer's comments and blue font is a reply.

Review 2:

This manuscript addresses an important question, namely how deposition of light absorbing particles (LAPs) on snow may alter hydrologic fluxes and streamflow across High Mountain Asia, yet the current framing and evidentiary support do not meet the standard expected for publication in Hydrology and Earth System Sciences. The central claims are derived from an offline CLM5 SNICAR sensitivity experiment in which LAP deposition is set to zero to define a clean snow counterfactual while meteorological forcing is held fixed. This design isolates a single land surface pathway through snow darkening but, by construction, excludes atmospheric aerosol radiative effects, aerosol cloud precipitation interactions, and land atmosphere feedbacks that are integral to the real world hydrologic influence of aerosols. Moreover, the manuscript provides limited quantitative evaluation of the simulated snowpack and streamflow, and it does not present basin integrated effect sizes with interannual variability or water balance closure diagnostics needed to establish the robustness and practical significance of the reported shifts, for example earlier snow disappearance versus order one week hydrograph centroid changes. Given these scope limitations, the unrealistic nature of the zero deposition baseline, and the absence of uncertainty and sensitivity analyses despite known forcing and deposition biases in high elevation regions, the conclusions currently read as qualitatively expected and insufficiently constrained for HESS without substantial reframing, additional diagnostics, and stronger evaluation.

We appreciate critics and detailed suggestions from reviewer 2. Reading through the comments, we consider adding new section – discussions to 1) model uncertainties including forcing, and model parameters, and 2) describe the limitations of our study due to the modeling scope and how comprehensive hydrological impact due to the aerosol (dust and BC) could be investigated using the fully coupled ESM. We hope these discussions can address many major comments.

Major comments

1. The attribution is not defensible given the experimental design. The simulations are offline and meteorology is identical in CLM-LAP and CLM-clean, so the study isolates only the land surface snow darkening pathway. Any manuscript level statements that suggest aerosols broadly alter HMA hydrology are not supported because atmospheric aerosol radiative forcing, aerosol cloud precipitation coupling, and land atmosphere feedbacks are excluded by construction. The scope must be narrowed to deposition on snow effects, or the modeling framework must be expanded.

We will state that the modeling scope is to examine the impacts of aerosol deposition in snowpack on hydrology, not including aerosol impacts on cloud microphysics, radiative transfer in atmosphere. We did not discuss the impacts of dust and BC in the atmosphere on precipitation, which can be as large as 10% enhancement compared to the simulations without dust aerosols in the Himalaya mountains (Adhikari and Mejia, 2022). Our forcing is based on WRF-chem including dust aerosol transport, which is the model setup closer to the reality (than WRF simulation without dust aerosols). However, the same forcing was used to force CLM-clean. We consider including this discussion in a new section (i.e., Discussion section).

2. The clean snow counterfactual is physically unrealistic and not policy relevant. Setting all LAP deposition fluxes to zero represents an extreme bound rather than a plausible baseline for HMA. A scientifically meaningful counterfactual should be framed as a perturbation around observed present

day conditions, for example scaling deposition fluxes by factors f in {0.25, 0.5, 0.75} or applying source sector reductions. Without this, the effect sizes cannot be interpreted as actionable or comparable to mitigation scenarios.

We agree that setting all LAP fluxes to zero is unrealistic especially when trying to discuss policy implications. Therefore, we will remove the statement of implication of pollution reduction on hydrologic consequence based on our results. We aim to quantify the radiative impact from total LAP instead of comparing with reduced anthropogenic/natural LAP emissions, which have been used for the past studies (e.g., Usha et al., 2022). Another example of zeroed out flux experiment is Adhikari and Mejia, (2022), who did two WRF-chem experiments with zero dust aerosol and dust aerosol introduced to look at dust impacts on precipitation in a Himalayan basin.

3. Basin scale effect sizes are not quantified in a way that supports the conclusions. The paper relies heavily on maps and qualitative language such as slightly reduced or greater sensitivity. HESS readers will expect basin integrated numbers with interannual variability. At minimum provide, for each basin and for headwaters versus full basin, the following diagnostics computed annually and seasonally: $\Delta Q = Q_{LAP} - Q_{clean}$ in mm per year and percent, $\Delta ET = ET_{LAP} - ET_{clean}$ in mm per year and percent, and ΔS as change in storage including at least soil water plus groundwater plus snow water equivalent carryover. Verify closure of the incremental water balance $\Delta P_{liquid} + \Delta M = \Delta Q + \Delta ET + \Delta S$, where P_{liquid} is rainfall and M is snow plus ice meltwater production. Without this accounting the interpretation of runoff decreases being driven by earlier depletion versus enhanced ET is not mathematically demonstrated.

Thank you for this comment. We acknowledge that water balance analysis is one of the first analyses to be done in a hydrologic simulation. In fact, we did look at monthly water balance for both clean and LAP cases for the 4 river basins (for high elevation zone like 3-4km, 4-5km), which may highlight water-balance difference between clean and LAP simulation cases (See Figure 3-1). We verified water balance closure for the four river basins (See Figure 3-1). These or similar figures can be included in the supplementary.

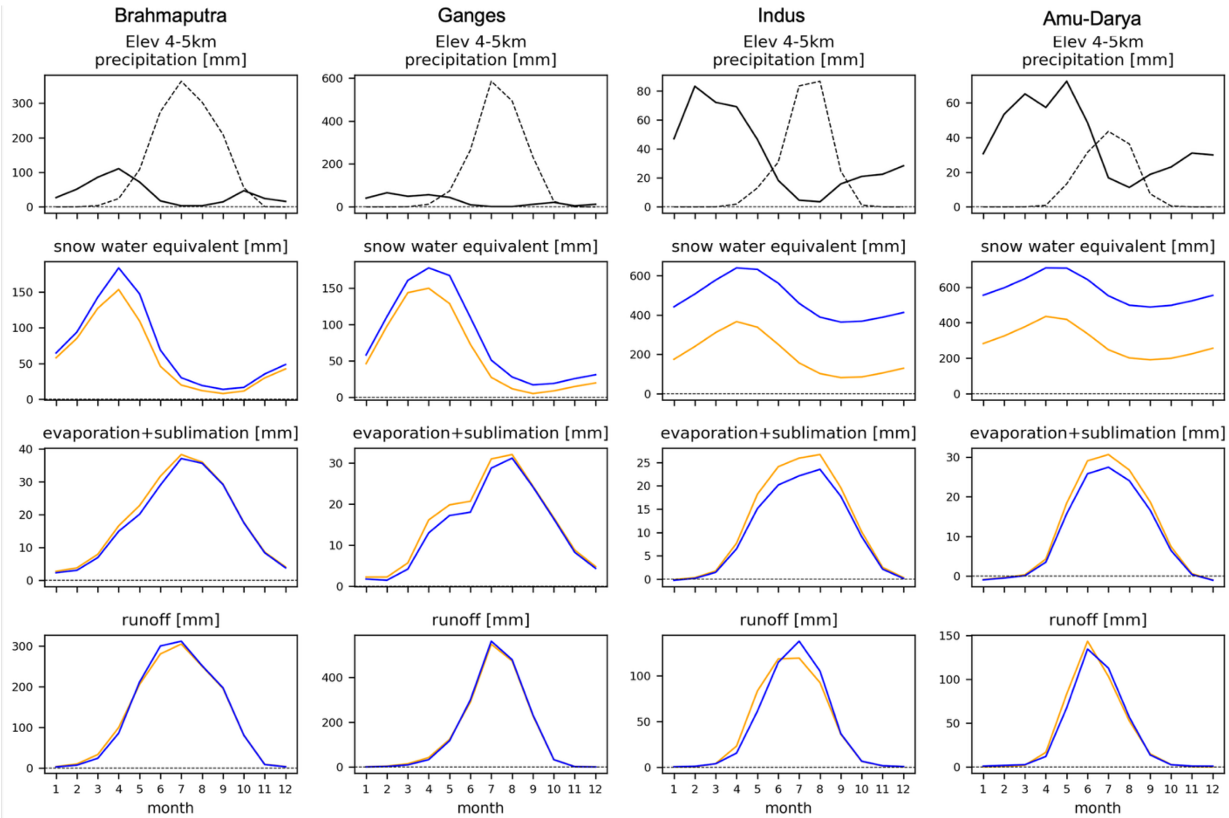


Figure 3-1: 2004-2018 monthly mean precipitation [mm], SWE [mm], ET [mm] and runoff [mm] from top to bottom panels over 4-5km bands in Brahmaputra, Ganges, Indus, and Amu-Darya basins from left to right. Both CLM-LAPs (orange) and CLM-clean (blue) are shown.

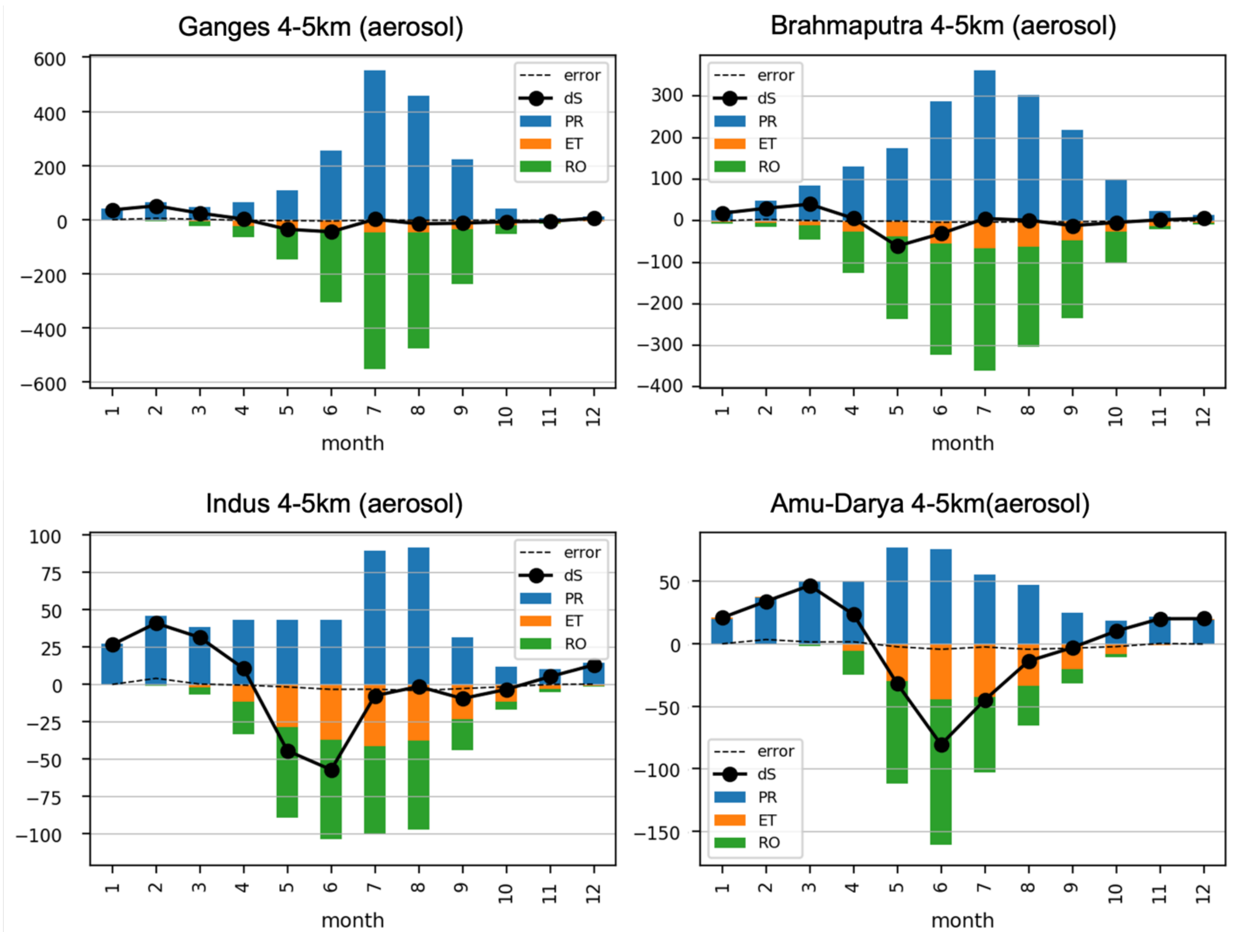


Figure 3-2: 2004-2018 Monthly water balance closure for four basins (only 4-5km elevation zone). CLM-LAP is used for these figures. Error = dS –(PR-ET-RO) were dS is monthly change in hydrologic states (canopy storage+SWE+Soil moisture), PR is precipitation, ET is evapotranspiration and RO is total runoff.

4. The manuscript does not show robustness of timing metrics. The text claims snow disappearance advances by two weeks to over a month, yet centroid shifts are at most within a week and peak timing shifts are within a few days. This can occur due to routing integration and rainfall dilution, but the manuscript does not quantify it. Provide a reach wise relationship between local snow disappearance shift and hydrograph centroid shift, for example regression or correlation across reaches, and show how this relationship varies with snow fraction, elevation hypsometry, and rainfall contribution. Otherwise the timing narrative appears internally inconsistent.

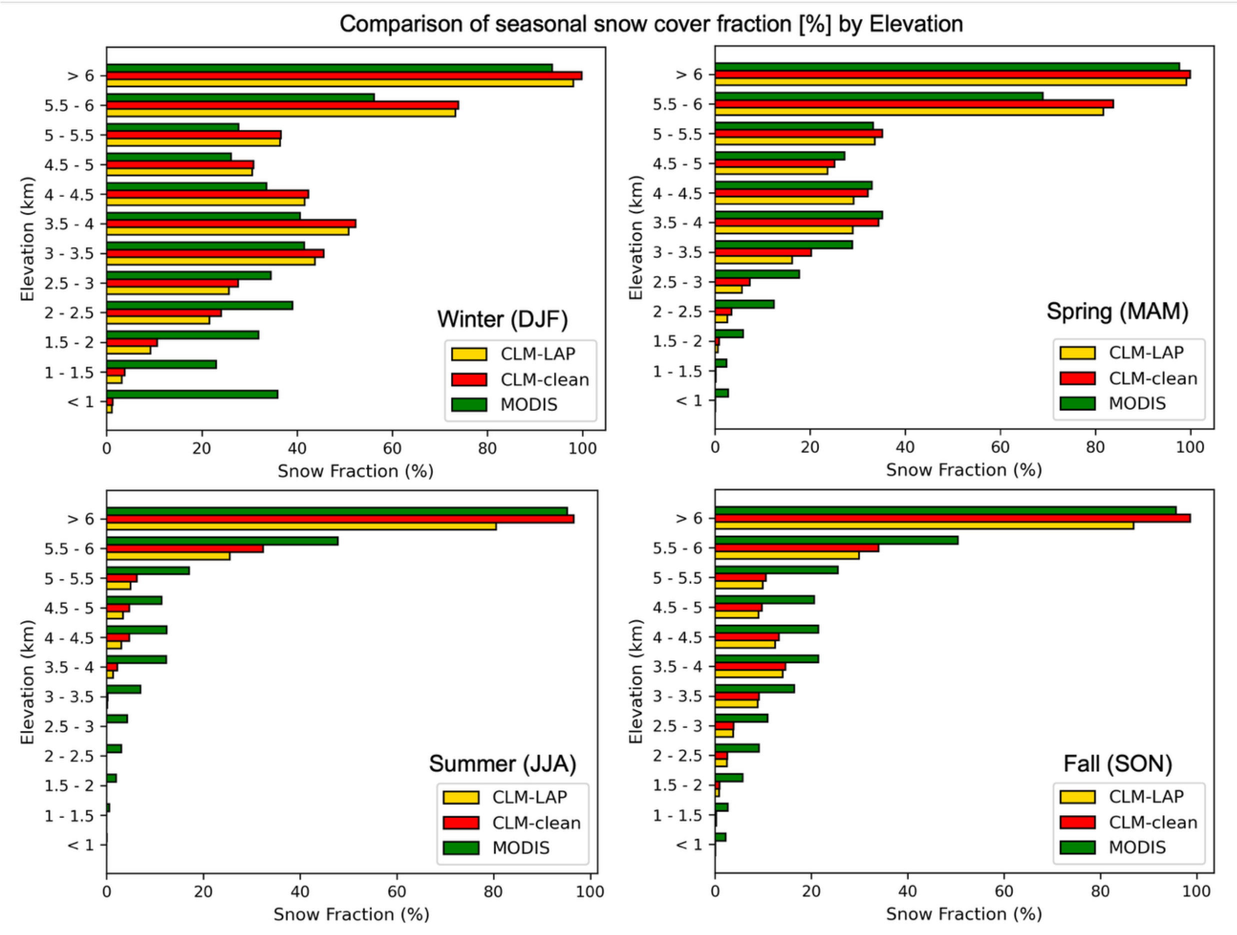
We will perform additional analysis (i.e., correlation analysis between streamflow timing change and snowmelt timing change) to show what key snowpack metric change is related to this streamflow timing change in the new figure.

5. Streamflow evaluation is insufficient for a study making streamflow conclusions. A brief supplement mention does not meet HESS expectations. At minimum show outlet and selected upstream gauge comparisons of the mean annual cycle and interannual variability, with standard skill metrics such as NSE, KGE, bias, and timing error in centroid day. If gauges are unavailable for some basins, this limitation must be stated explicitly and the discussion must be scaled back accordingly.

We did not access to streamflow observations beyond monthly flow downstream of Ganges and Brahmaputra due to limited data availability for the simulation period. In addition, these observed streamflow data is affected by human intervention, in particular, data downstream of Ganges. Since streamflow simulations do not count for irrigation, dams in river (i.e., streamflow simulation is considered as natural flow), overestimation is expected and it is less meaningful to present streamflow evaluation beyond the supplementary. We will clearly state this limitation when we conclude the hydrologic sensitivity to snowpack darkening. We will add %bias (R) is +30% (0.86) for Brahmaputra, and +42% (0.72) for Ganges at monthly scale in main text. Ganges River is highly irrigated affecting observed flow, while Brahmaputra River is less affected by irrigation, which is consistent with our error metrics. The production of reliable naturalized flow is out of scope for our study in the subcontinent of India, but this type of work just began recently (e.g., Chuphal and Mishra, 2023), and much more work will be needed for this region. We may consider adding time series of the comparison as well in supplementary.

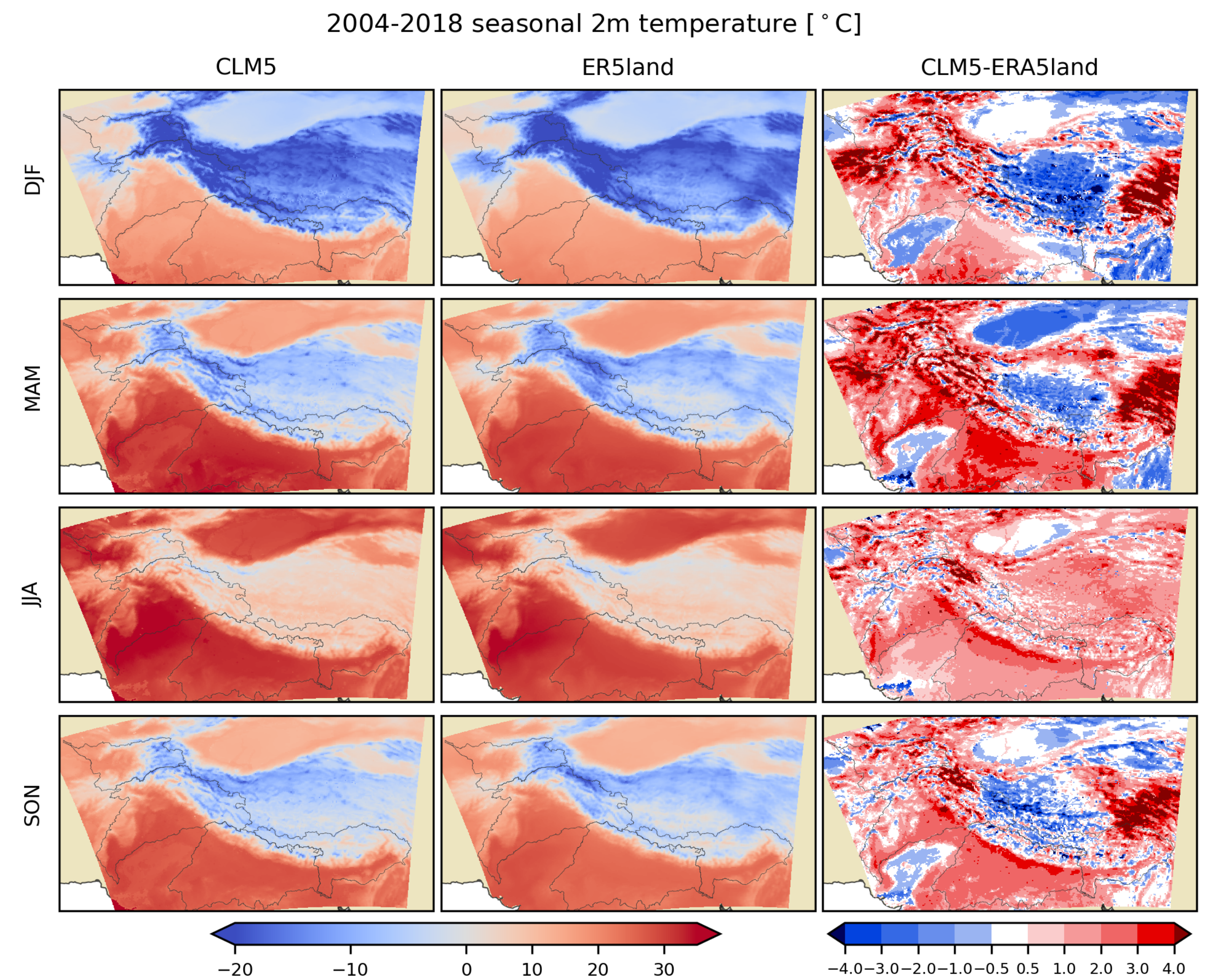
6. Snow and energy balance evaluation is missing where it matters most. Because the signal depends on snow persistence and albedo feedback, the manuscript should evaluate simulated snow cover duration and snow water equivalent using independent datasets such as MODIS snow cover and available SWE or snow depth proxies. At minimum, evaluate snow disappearance timing at elevation bands. Without this, the reported two week to one month changes could reflect forcing cold bias or model snow parameterization rather than LAP physics.

We would like to clarify that since both used the same meteorological forcing for CLEAN and LAP runs, which have the same bias, differences in snowpack between the two simulations are directly from aerosol radiative forcing impacts. However, how bias in meteorological forcing affects this sensitivity is not evaluated and will be beyond our modeling experiment scope. We should clarify this. That being said, we will provide a comparison of fractional snow cover (fSCA) between MODIS fSCA and CLM fSCA (both CLM-clean and CLM-LAP) – see below. We also consider providing a comparison of monthly SWE with ERA2land. This will give the readers some insight into uncertainty in snow simulations from the CLM simulations. We will plan to add this analysis in supplementary. Overall, elevational trend of CLM fSCA follows that of MODIS for each season, especially above 3000 m above sea level, but MODIS suggests greater snow cover area in lower elevations for all the season. The Difference in fSCA between the two CLM simulations are smaller that difference between CLM and MODIS, e.g., for the spring, the largest difference was 12% at 5.5-6.0 km band; For the summer, CLM-LAP had a snow fraction of about 20% lower than that of MODIS at the 5.5 to 6 km elevation bands. On the other hand, CLM-LAP and CLM-clean.



7. The forcing dataset has known high elevation biases that directly confound the LAP signal, and uncertainty is not propagated. The manuscript reports strong cold bias at high elevation and underestimation of surface black carbon in mountains. These biases affect both snow duration and LAP loading and therefore the melt response. Provide at least one uncertainty analysis that brackets the response, such as temperature bias correction sensitivity, precipitation partition sensitivity, or deposition scaling to represent underestimation. A single deterministic pair of simulations is not sufficient to claim basin specific sensitivity differences.

Thank you for this comment. We considered temperature bias correction of WRF-chem based on ERA5-land. However, the comparison between MATCHA with ERA5-land for 2m-temperature – see figure below - (also rainfall, snowfall from MATCHA, runoff and ET from CLM5 output were also compared) for each season. Overall, ERA5-land exhibit colder than MATCHA (~ > 4 degree) in the high elevations in the western part of HiMA, where Roychoudhury et al., (2025b) shows MATCHA temperature shows the strongest cold bias relative to the point observations in the domain. This means that ERA5-land would show even greater cold bias, if ERA5-land is compared to the same point observations. Therefore, we decided not to perform sensitivity test for meteorological forcing (in addition, this would become beyond the scope of our modeling experiment).



Instead, we will add discussion as follows. Depending on whether the cold bias happened under 0 °C or above 0 °C or cross the freezing point (i.e., it is above 0 °C from observation, while in the model, it is below), the impact would be different. For below freezing, cold bias would only have minor impact on precipitation phase and snow process, but for around or above freezing, the cold bias would increase snowfall and snow accumulation, delay melting, and thus dampen LAP concentration and impact but make it last longer (due to longer snowpack existence). Roychoudhury et al., (2025b) shows the lower elevations in upper Indus regions exhibit 2-m temperature discrepancy between MACHA and observation can be across the freezing point. The modeling experiments using perturbed temperature (and also precipitation and dust/BC deposition) can reveal their relative effects on snowmelt and streamflow, and this experiment is possible for a selected smaller HiMA basin.

8. The routing configuration is too simplified for inference about timing and downstream attenuation. A spatially constant Manning coefficient n = 0.05 over all reaches is a strong assumption. Even if the differential signal between experiments is less sensitive than absolute flow, this must be demonstrated. Provide a sensitivity test for n in a plausible range and report whether ΔQ, centroid shift, and timing of maximum increase and decrease are stable.

Thank you for comment on the river model parameters. The concern of spatially constant manning n is raised by reviewer 1. Diffusive wave routing used for this work needs two geometric parameters:

channel width which is a function of upstream areas of each reach in the model (See Mizukami et al., 2016, Section 3.2.1), and manning n. Manning n is quite uncertain. There is a guidance for manning n value, based on channel materials and riverbed conditions, but these are not practical for determining the manning n value for each river reach for large river basins. Based on *Open-Channel Hydraulics textbook* (Chow, 1959), mean of manning n for natural streams is 0.508 (See table below). We recognize that some large domain river modeling studies used spatially distributed manning n (e.g., Getirana et al., 2012), while others use spatially constant (Yamazaki et al., 2011). Even in the published work distributing manning n, the distribution methods were not scrutinized. Furthermore, with the same river model parameters for both LAPs snow and clean snow simulation cases, the manning n distribution will have minimal impacts on streamflow sensitivity to aerosol deposition in snowpack (both have the same uncertainty in the streamflow simulations). We will revise the sentences to incorporate this argument briefly.

Table: Manning's n for Channel (Natural streams)

Riverbed conditions	Normal values
clean, straight, full stage, no rifts or deep pools	0.030
same as above, but more stones and weeds	0.025
clean, winding, some pools and shoals	0.040
same as above, but some weeds and stones	0.045
same as above, lower stages, more ineffective slopes and sections	0.048
same as above with more stones	0.050
sluggish reaches, weedy, deep pools	0.070
very weedy reaches, deep pools, or floodways with heavy stand of timber and underbrush	0.100
Mean	0.051

Adopted from Chow 1959

In addition, we will be able to provide how different manning n values (possibly three different values – 0.02 0.05 (which was used in the paper), and 0.1) affect the sensitivity while we will revise the paper.

9. Glacier processes are not adequately represented for HMA hydrology and may bias seasonal conclusions. CLM5 uses a prescribed glacier land unit and does not simulate evolving glacier geometry or mass balance. In basins such as Indus and Amu Darya, glacier melt can materially influence summer and fall flows. The manuscript should quantify the fraction of runoff attributed to glacier melt in each basin, discuss limitations explicitly, and avoid attributing late season streamflow differences solely to LAP driven snow processes without separating glacier contributions.

We will provide our estimate of glacier melt runoff (and how many percent of the total runoff) and discuss our results are based on snowmelt change due to LAPs. One situation where mountain glaciers affect our result significantly is where if any complete glacier depletion during 2003-2018. Since depleted glacier ceases runoff supply.

10. The ET response is described but not diagnosed mechanistically. The manuscript attributes persistent ET increases after earlier snow disappearance to darker snow free surfaces and soil evaporation, but this is not demonstrated. Decompose ET into soil evaporation, canopy evaporation, and transpiration, and show whether increased ET is energy limited or water limited by presenting

changes in available energy and soil moisture. This is essential because the sign and persistence of ET anomalies strongly affect the annual runoff conclusion.

We can show these decomposed ET variables, and how snow-free surfaces increase soil evaporation. We plan to add the discussion in Supplementary. However, we are confused by the comment on energy-limited vs moisture limited and how this is related to this given the same available moisture and energy inputs. One key difference between both simulations is how much energy is absorbed into snow, and how this difference in energy absorption is translated to snowmelt runoff (then ET and runoff).

11. Snow cover type classification and thresholds are arbitrary and not tested. The perennial versus seasonal categorization depends on SWE greater than 5 mm and 60 day persistence at 12 km resolution. Report sensitivity to alternative thresholds and demonstrate that the headline conclusions, including the reported 40 percent reduction in perennial snow area, are not an artefact of threshold choice or grid scale smoothing.

Sixty-consecutive day threshold for snow cover type is based on the past studies (Petersky and Harpold, 2018; Sturm et al., 1995). We cited Petersky and Harpold already and we will add Sturm's work as well. Rather than sensitivity of snow cover classification (which was primary for "masking" of the subsequent spatial analysis such as Figure 2 g, h), we think it would be good to check how these thresholds affect difference in seasonal snow cover appearance day and disappearance day between CLM-clean and CLM-LAP. We consider presenting this supplementary but discuss these uncertainties in the main text.

12. The novelty is presented as a list of technical upgrades but the manuscript does not demonstrate that these upgrades lead to new hydrologic insight. To justify publication in HESS, the paper needs either a deeper process level contribution, for example elevation band attribution of ΔQ and ΔET tied to LAP loading and radiation changes, or clear improvements relative to prior studies with quantified differences. Otherwise the outcome reduces to an expected earlier melt earlier runoff narrative.

We will highlight that new insight from our study (on process understanding processes from runoff production to streamflow) is the impacts of LAPs in snowpack not only on hydrologic fluxes at snow cover area, but also on streamflow in downstream areas of the mountain. The past studies did not include how changes in snowmelt pattern affect river flows. This will be highlighted in the introduction. To conduct this work, we incorporated the latest version of the model components. These updates need to be described just for transparency of the models, but not for novelty of the experiment. We will simply describe our model setup. We will remove the word "novelty" and comparison with the model used by Usha et al., (2020, 2022) in Line 141-149 in section 2.3 (This gave the readers the impression that our model setup was upgraded from their models, which we did not intend to do).

13. The management and policy implications are overstated relative to the experiment. Because the study does not simulate realistic emission reductions and does not include atmospheric feedbacks, statements about air pollution control changing water resources should be limited to a qualitative sensitivity framing. If policy relevance is retained, provide scenario based deposition perturbations and translate impacts into operational metrics such as changes in seasonal low flow quantiles, timing of center of mass, or irrigation season deficits.

We agree with the reviewer that we should not discuss implications on air pollution management given our scope of model experiments. We will still mention the current state of air pollution in the regions (as one of the motivations for this study) in introduction section, but in conclusion, we will stay in focus on hydrologic implications.

Minor comments

1. Title wording is awkward and overly broad. Consider replacing “Hydrologic implications of aerosol deposition on snow” with “Hydrologic implications of light absorbing particle deposition on snow” or “Streamflow impacts of snow darkening by light absorbing particles” to match what is actually simulated.

Following the suggestion, we consider changing the title to “Hydrologic implications of light absorbing particle deposition on snow in High Mountain Asia river Basins”

2. Define all acronyms at first use and use them consistently. LAPs, BC, BrC, SWE, ET, HMA, MATCHA, SNICAR, CLM, PFT. Avoid switching between aerosol, LAP, and BC when the mechanism discussed is specifically snow darkening by LAPs.

We will correct all the acronyms throughout the manuscripts and use consistent acronyms (e.g., LAP instead of BC).

3. Use consistent terminology for fluxes. “Snowmelt runoff” is confusing. Use snowmelt for meltwater production and runoff for surface plus subsurface outflow. If you mean the contribution of snowmelt to runoff, state “runoff attributable to snowmelt” and define how it is diagnosed.

We agree that, in general, “snowmelt” is ambiguous. We will distinguish the following terminology: snowmelt refers to meltwater from snow that may refreeze within the snowpack or runs off from the snowpack. Snowmelt runoff: meltwater that leaves the snowpack that contributes to runoff, as opposed to rainfall contribution. Snowmelt runoff leads to reduction of snowpack mass (i.e., SWE), while snowmelt may not. We consider adding this distinction in introduction section.

4. The phrase “rain plus snowmelt” should be defined once with an explicit equation and then replaced with a shorter term, for example liquid water input or meltwater plus rainfall input. If glacier melt is included in runoff forcing to mizuRoute, clarify whether it is included in this input term.

This comment is related to the previous comment. The term “rain plus snowmelt runoff” is liquid water that contributes to water input into the land surface. We use this instead of precipitation to distinguish water input immediately released to land-surface. For water balance in CLM5 and what is imported to the river model is rather complex, and Lawerence et al., 2020 section 2.7 describe water balance. We will summarize CLM hydrology and we explain that total runoff into input of the river model is surface runoff + subsurface runoff + glacier melt.

5. The spin up description is confusing and should be rewritten for clarity. State clearly the cycling period used for equilibrium and the transient period used for analysis, with dates and number of cycles.

We will revise the description of the CLM spin-up as follows. “For both simulation cases, initial soil and snow states are obtained by running each simulation case by recycling 5 times for 2003-2007 periods.”

6. In Section 2.1, clarify whether perennial snow includes glacier land unit or only snow on the vegetation land unit. At 12 km, this distinction matters for interpretation.

Perennial snow cover identified in CLM simulation is simulated only in the vegetation land unit. Glaciers are non-vegetated unit and glacier melt is computed separately. To clarify again, LAP radiative forcing is effective for only snowpack. LAP effect on streamflow is based on change in snowpack though rainfall and glacier melt contribute to the streamflow.

7. Units and symbols need tightening throughout. Replace "C-degree" with "°C". Use mm per year or mm yr−1 consistently, and m3 s−1 for discharge.

We will correct the unit as suggested.

8. Several sentences are long and would benefit from being split to reduce ambiguity, particularly in the Abstract and Conclusions where multiple causal claims are chained.

We will edit the text throughout the manuscript to make the sentences concise. We will avoid using multiple causal clauses in a single sentence.

9. The novelty paragraph in Section 2.3 reads like a checklist. Consider condensing and focusing on what materially changes hydrologic inference, not just model versioning.

This was raised by reviewer 1. We will highlight that new insight from our study is the impacts not only hydrologic fluxes at the snow cover area, but also on streamflow in downstream areas of the mountain. The past studies did not include how changes in snowmelt pattern affect river flows. To conduct this work, we incorporate the latest version of model components. However, these updates are important and would like to clarify what is the state-of-art in each model component.

10. Figure 1 caption should explicitly define what "1000 m elevation bands" means and how they were computed. Also confirm whether the elevation shown is the CLM 12 km elevation or a higher resolution DEM aggregated.

The figure caption will be revised as follow: "Four river basins in High Mountain Asia. Elevation is color-coded in 1000-meter intervals (top panel)". Elevation data uses higher resolution DEM (Global Bathymetry and Topography at 15 Arc Sec: SRTM15+ V2.5.5) for just visualization, not CLM 12km resolution.

11. Figure 2 caption has a potential sign confusion. Ensure sign convention and colorbar labeling are unambiguous. Also specify whether day of year is water year or calendar year.

For panels g and h, this is simply the difference of first and last snow dates in days, and day of year is not involved. We will revise the figure caption as follows (though we may re-structure the figures and caption will be further revised accordingly):

"Figure 2: Mean snow cover characteristics (2004-2018) for CLM-LAP and CLM-clean simulations. Panels show the first day of snow cover (a, d), last day of snow cover (b, e), and snow cover type (c, f). Differences between simulations (CLM-LAP minus CLM-clean) are shown in panels (g) and (h), where positive values indicate a delay in snow cover appearance and disappearance for the CLM-LAP simulation. Snow categories include no snow (<5 mm always), ephemeral, seasonal (>5 mm for > 60 days), and perennial (>5 mm always)."

12. Figure 3 needs clearer phrasing and labeling. Add basin mean annotations or an inset table of basin averages. Clarify whether seasonal panels are mm per month averaged over the season or total seasonal sum divided by months.

We put variable names above the top panels and the units of variables in the texts to the left, but this layout may not be sufficiently clear. We will explicitly state "annual mean <variable_name> [mm/yr]" for the top panels, and spring (MAM) mean <variable_name> [mm/mon] for the second-row panels and summer (JJA) mean <variable_name> [mm/mon]. We will duplicate the unit next to the colorbar. We will rewrite the figure header to "2004-2018 mean water flux difference (LAP snow minus Clean snow)". In terms of basin mean value, we are considering adding bar chart showing mean difference over 3 cases 1) seasonal snow area for both clean and LAPs, 2) perennial snow area for both clean and LAPs, and 3) Perennial snow for clean and seasonal snow for LAPs. Note that the case where perennial snow for LAPs and seasonal snow for clean does not exist (we will mention this in the caption).

13. Figure 4 needs axis labels and units for soil moisture, ET, and runoff, and should clarify whether soil moisture is volumetric water content, equivalent depth, or column integrated storage.

We will spell out y axis labels and soil moisture is the column integrated storage in a unit of depth.

14. Section 3.3 centroid metric should be defined mathematically. Provide the equation $t_c = (\Sigma\, t\, Q(t)) / (\Sigma\, Q(t))$ and specify whether Q(t) is daily mean discharge and whether t spans calendar year or water year.

Yes, this is how the centroid of annual hydrograph was computed. We will add the equation in the text.

15. Fix typographic inconsistencies and hyphenation. "LAPdeposited" should be "LAP-deposited" consistently. Ensure consistent usage for snow-free, high-elevation, basin-scale, and data assimilation.

We will use consistent use of terminology and hyphens throughout the text.

16. The Conclusions contain several forward looking statements about air pollution management. These should be softened and clearly labeled as implications of a sensitivity experiment rather than predictions.

This is related to comment 13. We will revise the conclusion by removing practical implications regarding air pollution and focus on hydrologic sensitivity results.

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