#Reviewer1 Response

Comments:

Authors have done a good job incorporating reviewers' comments and improve the clarity of the manuscript. As it has been raised by Reviewer 2, the manuscript still needs substantial editing to improve the clarity of the text particularly the discussion section.

Reviewer 1 comment regarding the qualitative assessment still holds. While authors have revised their figures, it would be more useful to discuss quantitative differences in the text.

Reply:

Dear reviewer, thank you for appreciating our efforts to revise the manuscript. We again believe your comments were beneficial to improve the quality of the manuscript, and we incorporated them as much as possible. We make an effort for a rigorous review and editing of the manuscript to make the text follow-up smooth.

This version includes the revision of figures, inclusion of performance indicators in methods, inclusion of quantitative differences in the results section and revision of the discussion. We hope this version of the manuscript will be easy to read and understand by the readers.

Comments:

Although runoff observations are not available for the simulation period, can authors compare differences between the simulated runoff relative to the fully distributed forcing case to indicate runoff sensitivity at a monthly time scale?

Reply:

Dear reviewer, thank you so much for this comment; it improved the understanding of the runoff figures. We incorporated your comment in Figures 5 and 6 and discussed them in the corresponding result paragraphs.

Comments:

As indicated in Line 325, the difference in water budget components between the distributed and uniform simulations is less than 2%. These results indicate that there is no need for distributed forcing simulations in the catchment. Can authors further explain why using 2D distributed forcing is needed for their catchment besides for precipitation? Given their approach in distributing snow a priori, this does not seem to be a true test of model sensitivity.

Reply:

Dear reviewer, the 2% difference in water budget was for the 1D-AM simulation; this simulation is forced with the average of the distributed forcing. It explains why this simulation is close to the distributed simulation (2D-AD). The actual non-distributed simulation was our 1D-PM simulation, which showed a volume difference of -7%. We have included these details in the revised manuscript.

Apart from this, we showed the spatial calibration of the model in the study. It means, along with runoff simulation, we were also looking for the snow melt duration and the representation of evapotranspiration. The non-distributed simulation showed a short snowmelt period (Figure 5) compared to distributed simulations (Figure 6). Similarly, the evapotranspiration in the non-distributed simulation (1D-PM) was much higher as of distributed forcing and observation (Figure 7). It concludes on two points 1) The distributed forcing is much better than non-distributed forcing in simulating spatial snow variability and evapotranspiration. 2) We need the precipitation distribution with shortwave radiation distribution to better account for the available water resource along the year. The revised manuscript includes these points in the abstract, result and discussion section.

One last remark between the distributed and non-distributed simulations could be that we kept the same precipitation in all simulations. We considered this study as an upscaling exercise from high resolution modelling to large-scale hydrological and earth system modeling (ESM) to characterize the potential impact of sub-grid processes in ESM. As ESM usually have the same precipitation amount on a single pixel; hence, we did not change the precipitation the spatial average amount in our simulations. But, as we mentioned in section 4, the amount of measured precipitation at the meteorological station (1531.96 mm) was different from what we got after distribution (1443.72 mm). If we force the simulation with non-distributed forcing (measured at the meteorological station), the difference between the distributed and non-distributed simulation will be too high.

Comments:

Abstract – What are the units of MBE values that are reported for different variables? Line 17 – Why two values are reported for the MBE (MBE = 0.59, -0.4)?

Reply:

These MBE values were for the 2D-SD and 2D-WD simulations compared with the Sentinel-2 image. However, we also feel these values are confusing in the sentence, so we finally removed these quantities from the abstract.

The units for MBE values are in percentage. For example the a value 0.06 means that the 6 % of the total pixels are mismatched between model and Sentinel-2 satellite image. We have included this in the corresponding methods section.

Comments:

Line 20 – Regression slope is not clear. Do you mean the slope between observation and simulation?

Reply:

Yes, we mean the slope of the regression between observation and simulation. We have revised the sentence and included the description in the method section.

Comments:

Line 82 – ParFlow.CLM has been widely used across a number of catchments. Why this first objective is important in your investigation?

Reply:

Dear reviewer, the 3D catchment modeling in complex mountain terrain using ParFlow-CLM is not much documented, especially in a high spatio-temporal resolution. This model setup is unique to represent the small-scale heterogeneity in a mountain critical zone.

We could highlight some references where the author have used ParFlow-CLM to estimate the water budget with respect to snow processes. However these studies are mostly use the column simulation (Pix-PM in our case) or hillslope simulation (https://doi.org/10.1016/j.advwatres.2019.103473). Also, the upscaling using ParFlow-CLM underestimated the budget because these processes do not account for the slope/aspect based meteorological distribution(https://doi.org/10.1088/1748-9326/aba77f).

Comments:

Line 170 – Why a nearest neighbor algorithm is used?

Reply:

Dear reviewer, thank you so much for highlighting this mistake. We have revised the sentence as:

"LiDAR Digital Surface Model (DSM) of 2 m resolution was available for the catchment and upscaled to 10 m resolution using the minimum of each cell." Nearest neighbour was part of the algorithm in the Lidar processing.

Comments:

Section 3.2.1. It seems rain distribution due to topography is not considered given the size of the catchment but it is distributed due to differences in wind speed. Is that correct? Based on Line 265, it seems only snow is distributed. If that is the case, the simulation names should be modified to snow.

Reply:

Dear reviewer, we have mentioned in section 3.2.1 that considering the low altitudinal range (1993 m to 2204 m), the rain gradient between upper and lower altitudes was not considered. Looking at the contour plot in figure 1, we can see that more than 70 % of the catchment is within the 100 m altitude. So, we did not consider distributing the precipitations according to elevation nor temperature in this study. However, our code takes the precipitation as input, then using the temperature threshold of 2.5 °C, it separates the rain and snow. Finally, we multiply the snow precipitation with the snow map coefficient. We believe that the snow map from the Lidar survey contains also the elevation effect. A small paragraph as been added in the discussion section to mention that such drivers (elevation and temperature) should be consider for larger mountainous catchment with larger elevation range.

As we calculated everything with respect to precipitation input; hence, we kept the name of the simulation from precipitation distribution (PD). Changing it to snow distribution (SD) will match the acronym with the shortwave radiation distribution (SD). As we mentioned in detail in our method section, we would like to keep the acronym precipitation distribution (PD).

Comments:

As CLM uses a temperature threshold to distinguish between rain and snow, did you use the same threshold to separate rainy days from snow?

Reply:

Yes, as mentioned above, the rain and snow are separated with a temperature threshold of 2.5 °C. This value is consistent with the temperature threshold used by CLM.

Comments:

Line 235 – What is the source of Manning's n values?

Reply:

The manning coefficient we took from the literature (manning coefficient table) for the mountainous catchment. River pixels were assigned a constant manning value of 0.05 s $m^{-1/3}$, and the rest of the catchment was assigned a manning value of 0.03 s $m^{-1/3}$. We have mentioned this in the methods.

Comments:

Line 265 – Since this is a sensitivity experiment, would it be useful to do the forcing distribution in a way to preserve overall mean? As it is indicated on page 14, the differences in mean values of SW is causing the difference in runoff values.

Reply:

Dear reviewer, we feel that your point is actually dealt with in our series of simulations. Indeed, this is the reason for us to have the 1D-AM simulation, in which the uniform forcing (1D) for each variable (precipitation, radiation, wind) is actually the spatial average of the distributed forcing of the same variable. In this sense, 1D-AM is the spatially uniform equivalent of 2D-AM. And our results show that uniform forcing is very different from distributed forcing, event keeping the average the same. Then, the other 2D simulations are here to understand which distribution most influences the fine scale modelling output.

Arguably, when comparing 2D-AD simulation with 2D-PD or 2D-WD, incoming radiation is not preserved, and this is a major cause for the differences observed. But the major impact of radiation distribution is in the delay of the snowmelt, which even in the distributed run happens as a tail. This tail is a sure sign of spatial heterogeneity in the melt process, which can be seen in figure 10: melt occurs more in the lower part (unshaded) of the watershed compared to the upper-part, facing North-East. Reducing the amount of energy, but applying it uniformly would not cause this kind of pattern. So we think that although our simulation plan might be sub-optimal as pointed out here, the conclusions of the study still hold: small scale topography exerts a strong control of snow melt timing through radiation – topography interactions, and this timing in turn influences strongly the behavior of surface – subsurface coupling and the timing of the runoff.

Comments:

Figure 7 – As three different simulations are compared with observations, what slope and R2 values represent on these figures?

Reply:

Dear reviewer, we have shown the R2 values in the subplot for the simulation mentioned in the corresponding figure caption. However, in the revised figure, we have corrected the legend.

Comments:

Section 5.3. It would be useful to compute percent difference in monthly water budget relative to 2D-AD.

Reply:

Yes, we have done this in Figures 5 and 6; it even improved the visible differences. We have discussed these differences in the corresponding text as well.

Dear reviewer, in addition to these changes we would like to mentioned a few other things related to your comments,

1. We kept some of the introduction to the paragraph in results section. We believe this will help the reader to grasp the insight of the paragraphs.

2. We kept the word ‘snow patch’ in quite a few places as we really think this is most appropriate word to describe the late spring conditions. Patchy is exactly the meaning we want to convey here. spatially heterogeneous does not cover that meaning, as spatially heterogeneous can describe a situation where the snow depth changes with space and yet there is snow everywhere. We feel that patchy does convey better the fact that the snow cover becomes discontinuous, with discontinuities that do organize in a non-random yet not regular manner: patches of naked ground appear first on barely visible ridges, then spread to only leave patches of snow in barely visible troughs. Patchy is the best adjective we can come up with to express this notion of patches. Moreover, “snow patches” can be found in the snow science litterature, to convey this exact notion (e.g. Mott et al, 2018; Schlögl et al 2018; Kochanski et al, 2019)

Kochanski, K., Anderson, R. S. et Tucker, G. E.: The evolution of snow bedforms in the Colorado Front Range and the processes that shape them, The Cryosphere, 13(4), 1267-1281, doi:10.5194/tc-13-1267-2019, 2019.

Mott, R., Vionnet, V. et Grünewald, T.: The Seasonal Snow Cover Dynamics: Review on Wind-Driven Coupling Processes, Frontiers in Earth Science, 6, doi:10.3389/feart.2018.00197, 2018.

Schlögl, S., Lehning, M. et Mott, R.: How Are Turbulent Sensible Heat Fluxes and Snow Melt Rates Affected by a Changing Snow Cover Fraction?, Frontiers in Earth Science, 6, doi:10.3389/feart.2018.00154, 2018.

3. We would really like to keep the section 4 as a separate section from methods. Section 4 contains results of the spatialisation algorithms used to create the forcings used in the simulations, so that this section is a kind of hybrid section between results from this spatialisation methods and further methods (simulation setup). It thus seems better to keep this paragraph as its own section.

4. In addition we have run each simulations for two more years with same forcing to avoid any kind of perturbation in surface fluxes because of subsurface storage. We have mentioned it in methods sections with appropriate reference.

5. We have shortened the conclusion and presented the main conclusion in bullets.

#Reviewer2 Response

Comments:

This is the revised version of the manuscript "Impact of distributed meteorological forcing on snow cover and simulated hydrological fluxes over a mid-elevation alpine micro-scale catchment" by Gupta et al. I read through the response to reviewers' comments and the revised manuscript and found the paper improved. However, I still have a few further comments (please see the attached annotated pdf and):

Reply:

Dear reviewer, thank you for appreciating our efforts to revise the manuscript. We again believe your comments were beneficial to improve the quality of the manuscript, and we incorporated them as much as possible. We made an effort for a rigorous review of the manuscript to make the text smoother. In addition all the comments from the annotated pdf were addressed.

This version includes the revision of figures, inclusion of performance indicators in methods, inclusion of quantitative differences in the results section and revision of the discussion. We hope this version of the manuscript will be easy to read and understand by the readers.

Comments:

-the model is run/evaluated only for 1 single year. If there are more years available it would be good to see if the conclusions remain the same. I understand this may require too much efforts from the authors at this stage, but at least the Discussion section could elaborate on this,

Reply:

Dear reviewer, what we presented here is valid for every hydrological year. In our companion paper we are presenting the simulation for a different year and the same results hold. We have added this statement in the discussion section as: “The approach of meteorological distribution and cross validation with observation and Sentinel-2 images is also valid for subsequent years in the catchment. This will be presented in the companion paper to be published.”

Comments:

-there are still several unclear sentences and formulations, informal sentences, typos, etc., which would be good to revise,

Reply:

Dear reviewer, we have done a rigorous review of manuscript on this point. We make sure the clarity and flow of all sentences throughout the manuscript. Any inappropriate sentences or words have been completely revised.

Comments:

-for the methods section: please include description of model performance metrics; and please avoid using the same letters for different variables,

Reply:

Dear reviewer, we have added a separate section in the methods to elaborate the performance metrics. We have also found repetition of a letter for two different variables which we have revised.

Comments:
-it would be better to use terms consistently, e.g. approach or run or forcing or simulations but not to mix if these refer to the same,

Reply:

Dear reviewer, thank you so much for the comments. We have tried to ensure consistency in the vocabulary across the revised manuscript.

Comments:
-for the Results section: table 4: it is very good that these performance metrics were added, but in this case, there should be a section in the Results which describes these,

Reply:

We have added a separate paragraph in the end of result section to explain the table. We have summarised the whole study with reference from table 4.

Comments:
-for the Discussion section: please avoid the repetition of methods and results, and please add further references on existing studies: how do these results compare to the results of other studies? -for several smaller and larger technical corrections, please see the attached pdf.

Reply:

Dear reviewer, we have omitted the repetition of methods and results in the discussion section. We have thoroughly revised the discussion section and added the appropriate references wherever needed.

Dear reviewer, in addition to these changes we would like to mentioned a few other things related to your comments,

1. We kept some of the introduction to the paragraph in results section. We believe this will help the reader to grasp the insight of the paragraphs.

2. We kept the word ‘snow patch’ in quite a few places as we really think this is most appropriate word to describe the late spring conditions. Patchy is exactly the meaning we want to convey here. spatially heterogeneous does not cover that meaning, as spatially heterogeneous can describe a situation where the snow depth changes with space and yet there is snow everywhere. We feel that patchy does convey better the fact that the snow cover becomes discontinuous, with discontinuities that do organize in a non-random yet not regular manner: patches of naked ground appear first on barely visible ridges, then spread to only leave patches of snow in barely visible troughs. Patchy is the best adjective we can come up with to express this notion of patches. Moreover, “snow patches”

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5. We have shortened the conclusion and presented the main conclusion in bullets.