COMMENT:

Assessing the impact of meteorological forcing on simulated water and energy budgets particularly in mountainous catchments is important. While authors have addressed an important topic, the study falls short on performing a comprehensive quantitative model evaluation compared to observations. Furthermore, broader implication of the results have not been discussed.

REPLY:

Dear reviewer we are very thankful to you for reviewing the manuscript and highlighting the importance of our study. Your valuable comments will certainly improve the quality of our manuscript. As you mentioned, we tried to explain the importance of meteorological distribution methodology throughout our manuscript using a sensitivity study of meteorological distribution on water budget, and we tried to incorporate all possible experiment related to our hypothesis.

However, concerning statistical estimations that have not been included in the study, we will add them in the revised manuscript, the different statistical metrics (slope, correlation, RMSE and MSE) for albedo (table below present values to be written in the new manuscript, **Review Table 1**), evapotranspiration (**Revised Figure 7** below) and snow comparison with the observed data (snow/no-snow surface ratio, **Revised Figure 10** below) is included with our response. Unfortunately, we don't have streamflow data for the selected period but we believe that this is not necessary for the objectives and conclusions of this study. We documented the soil parameters according to field experiment and geophysical survey data which is explained in the text. Their values and distribution do not give us large ranges to tune the model. A companion paper to be submitted soon will discuss more the sensitivity with respect to subsurface configuration and calibration against updated observations including some discharge data.

COMMENT:

Authors performed a limited sensitivity experiment at a small catchment scale to assess the impact of distributed meteorological forcing on simulated water and energy budgets. Although authors performed a number of scenarios, results mostly analyzed qualitatively without providing further insights.

Authors have mostly used qualitative assessment to compare simulation results. Further quantitative assessment of catchment average and spatially distributed data are needed to understand the impact of spatial heterogeneity of meteorological forcings.

REPLY:

Dear reviewer, we agreed that our manuscript lack from quantifications. In our revised manuscript we will include statistical differences among the different simulations to better quantify the intrinsic and relative changes in hydrological budgets. Slope, correlation coefficients and RMSE have been added for evapotranspiration on each plot of **Revised Fig. 7.** Similarly the statistics for albedo and sentinel images has been also included in the attached figure/tables (**Review Table 1, Revised Figure 10).** These all statistics will be further added in the final draft of the manuscript.

COMMENT:

Model evaluation against observed evapotranspiration showed that all model scenarios overestimate ET. Therefore, no further improvement has been achieved by using distributed forcing.

REPLY:

Dear reviewer, we thank you for highlighting this issue. First it has to be noted that the simulated evapotranspiration in the 1D run (uniform meteorological forcing) overestimates ET observations (**Figure 7**). The main reason is that for these simulations we don't take into account solar angle for the available radiation for melting. However, it is obvious on **figure 7** that the representation of evapotranspiration is much better when we distribute the shortwave radiation (2D-AD and 2D-SD simulations) which take into account solar angle and catchment slope. At the contrary, this is not the case when only precipitation or only wind is distributed. This is one of the major highlight of our manuscript. We added some statistical estimates to better quantify the improvement in evapotranspiration among different simulations.

Another reason to overestimate the evapotranspiration could come from our footprint area. We have one meteorological observation at flat surface and the catchment is very undulating. Though we presented a wind direction mask (**Figure 1**) to consider large heterogeneity (moisture, vegetation) in the ET observation footprint and better compare evapotranspiration series. It is not as good as in representing the actual footprint area and this can lead to differences between observation and simulations. However, this is not the purpose in this study to have the best comparison for ET which would require a complete footprint area calculation over complex terrain. This is a challenge by itself and these remarks will be added in a separate discussion part.

COMMENT:

The thickness of the last subsurface layer is 110 m. At this resolution, groundwater system is simulated as a single reservoir. I wonder why authors needed to use ParFlow.CLM for such a simple parameterization of the subsurface. I agree with authors that simulating lateral flow processes in these steep catchments are important. However, they did not show sensitivity of lateral flow simulations in their simulation scenarios.

REPLY:

Dear reviewer we thank you for this very relevant comment. We started setting up our model with the shallow subsurface (20m) which led to an unsaturated domain everywhere except at the very bottom of the catchment close to the outlet. This means that we were badly simulating transmissivity and potential underground flows associated with the unsolved saturated zone (from top bottom and from side to stream). Hence, to better account the saturation transition from deep subsurface to shallow subsurface from a better solved pressure field, we have increased the depth of the domain. With this configuration we simulate explicitly the saturated zone on more than half of the catchment (**Review Figure 1**). It has to be noted that Parflow suppose hydrostatic profiles within a single mesh, which means that water table depth can be interpolated from solved pressure/suction values in the lower meshes. In our case it could be easily seen as the transition from deep subsurface saturation to shallow subsurface saturation along the mid of the watershed. Finally, the model water velocity outputs are used for running the particle transport model EcoSLIM and calculating the residence time with this configuration is better adopted to account both surface and deep flow path in the catchment.

Last important remark is that the domain has a no-flow boundary condition on the sides and at the bottom of the domain. ET and streamflow outlet are the only way to get the water out. In other words, this means that we are not simulating larger scale flow path (water that enters from the sides of the domain or that gets out through the bottom of the domain).

Also as you said, we do have lateral flows simulation for our catchment for different layers. However the particle tracking simulations we did, showed very low contribution from deep underground flows to streamflow or to ET. Then, in this manuscript we put more emphasis on surface processes impacted by meteorological distribution forcing to catch the spatial snow variability and associated hydrological fluxes. Subsurface sensitivity and its impact on lateral flows will be discussed in detail in our companion manuscript. These remarks will be added in a separate discussion part.

COMMENT:

Given the small size of the catchment, it is difficult to use existing gridded meteorological products to assess the impact of distributed forcing on simulated results. However, given the size of the model, authors could expand the extent of their sensitivity analysis and perform additional scenarios.

REPLY:

Dear reviewer as you said we agree that the size of catchment does not allow to use the gridded meteorological products for sensitivity analysis. ParFLOW-CLM is a critical zone physically based model which allow us to be very close to hydrological processes. This requires reliable data for forcing, ground, vegetation and hydrology to keep consistency in the model framework to simulate all water path with the same accuracy. We then chose to work only with local observations from which we built distributed forcing based on published algorithm and evaluate the model. From this approach we can clearly see the importance of snow and incoming radiation distributions.

Adding extra sources of data would have brought confusion to the message we wanted to carry with this study. Indeed, using reanalysis data could have led to better simulation of ET or streamflow scores but might be for wrong reasons. It can be good for the elevation at which our catchment lies but may not be as good for few hundred meters apart for different slope orientation or different micro-topography. It has also to be noted that these reanalysis products are much coarse in mountainous regions.

However, note that in our discussion we refer to Y. Fan et al. 2019 who have highlighted that slope/aspect has to be accounted for Earth System Models especially if their resolution is decreasing. We believe our study contribute to that identified issue.

SPECIFIC COMMENTS:

COMMENT:

State in the abstract that the impact of precipitation, wind and shortwave radiation were explored.

REPLY:

Thank you so much for pronouncing this statement. We will add it to our revised version.

COMMENT:

Line 75 – This is the common land model not the Community Land Model

REPLY:

Sorry for this mistake. We will correct it.

COMMENT:

Line 225 – How did you assess equilibrium state?

REPLY:

The equilibrium state in the model is reached through the 10 years long spinup. The equilibrium is assessed by subtracting the subsurface storage of previous year (Y) from the advancing year (Y+1).

$$\Delta\, Subsurface\ storage = Subsurface\ storage\,(Y+1) - Subsurface\ storage\,(Y)$$

The mean difference for the first year was 50 mm which became -2.2 mm at the end of 10^{th} year reaching the equilibrium state in the model.

COMMENT:

Line 255 – Please change “subsurface stock” to “subsurface storage”.

REPLY:

Thank you for highlighting this, we will change it at every instance.

COMMENT:

Line 230 – Differences among various simulation scenarios are not entirely clear. Please clarify.

REPLY:

Thank you so much for mentioning this issue. We will add our attached table (**Review Table 2**) as a separate table in the revised manuscript. This table includes what is distributed and what is not distributed among different simulation.

<u>COMMENT:</u>

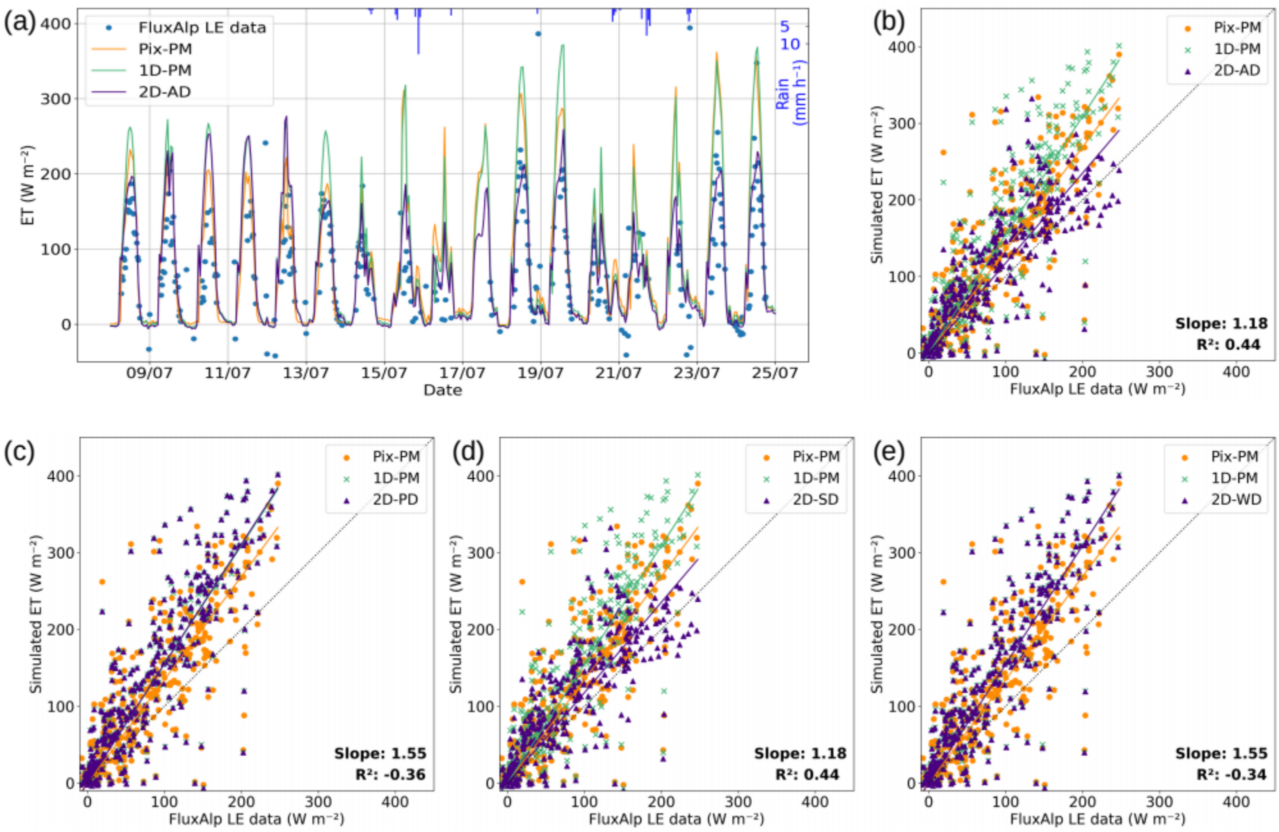
Do you have any runoff observations in this watershed?

<u>REPLY:</u>

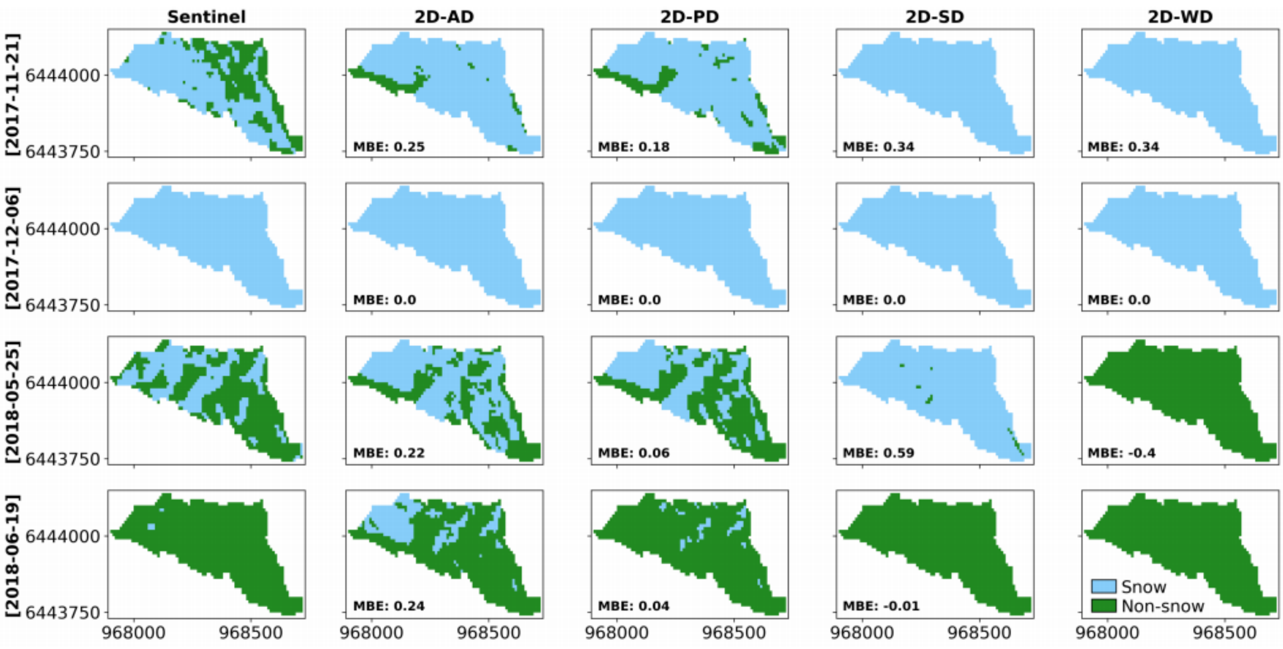
Dear reviewer we do not have any runoff for the year 2017-2018, the year selected for this study. The runoff measurement in the catchment started from spring 2020. As of now we do have a calibrated model for ground parameters. However, as already said above, soil model parameters (Vangenuchten) has been documented from observations we have in our catchment: pedological survey, permeability survey at surface, the underground investigation (accessibility in a tunnel below the catchment) and electromagnetic/GPR survey. This do give us much window to tune our model, it may not be perfect but not far from reality.

Review Table 1: Statistical metrics for observed and simulated parameter among different simulations.

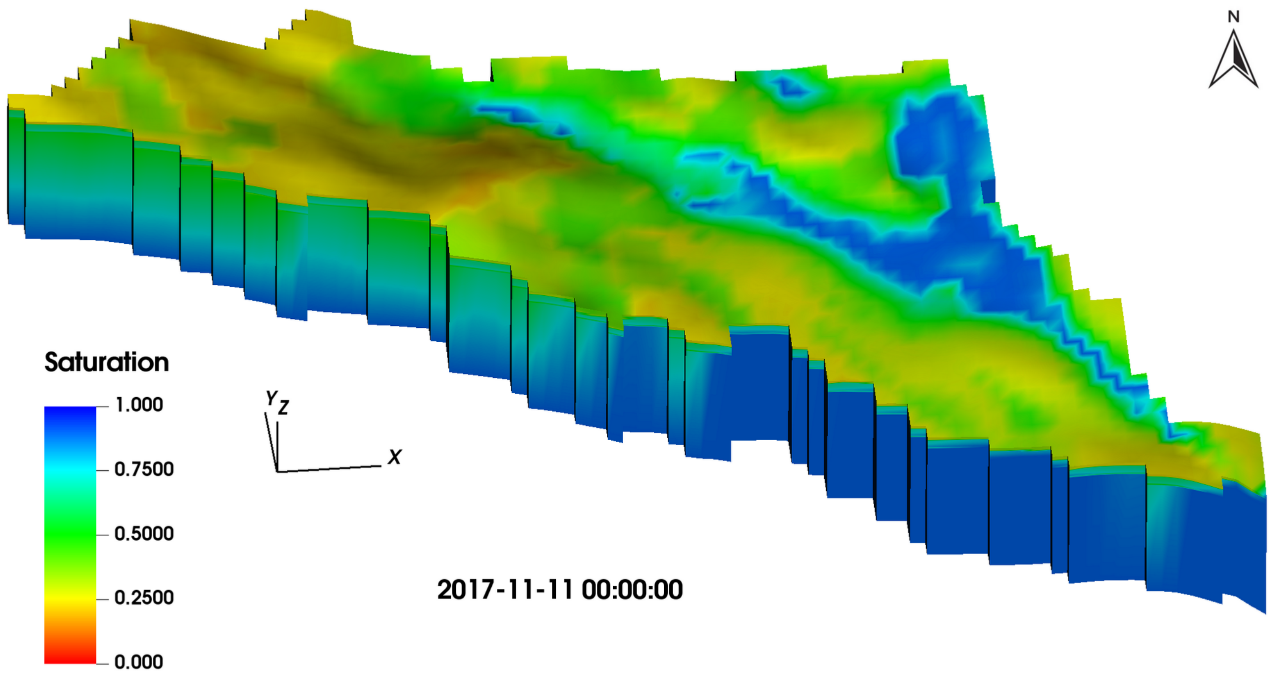
Variable	**Metrics**		**2D-AD**	**2D-PD**	**2D-SD**	**2D-WD**
Evapotranspiration	Slope		1.18	1.55	1.18	1.55
	R-Square		0.44	-0.36	0.44	-0.34
	RMSE		50.77	79.14	50.90	78.41
Albedo	R-Square		0.85	0.88	0.77	0.85
	RMSE		0.12	0.10	0.14	0.12
Snow cover (Sentinel2)	MBE	21 Nov, 2017	0.25	0.18	0.34	0.34
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.22	0.06	0.59	-0.40
		19 June, 2018	0.24	0.04	-0.01	-0.01
	RMSE	21 Nov, 2017	0.63	0.65	0.58	0.58
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.74	0.75	0.78	0.63
		19 June, 2018	0.50	0.23	0.07	0.07



Revise Figure 7: (a) Evapotranspiration simulation masked with wind direction mask for 17 days in summer for all distributed run (2D-AD). Scatter plot in the same month for (b) all distributed run (2D-AD), (c) only precipitation distributed run (2D-PD), (d) only shortwave radiation distributed run (2D-SD) and (e) only wind distributed run (2D-WD).



Revise Figure 10: Snow map for different simulations compared with the Sentinel-2 images for 4 cloud free images: snow pixels (light skyblue) and non-snow pixel (green).



Review Figure 1: Saturation after spinning up model for 10 years. The saturated zone in upper half of the catchment is much deep compared to lower half of the catchment.

Review Table 2: Distributed and non-distributed approach adopted for different simulation.

	Precipitation	**Shortwave radiation**	**Wind speed**
Pix-PM	Distributed mean	Non-distributed	Non-distributed
1D-PM	Distributed mean	Non-distributed	Non-distributed
1D-AM	Distributed mean	Distributed mean	Distributed mean
2D-AD	Distributed	Distributed	Distributed
2D-PD	Distributed	Non-distributed	Non-distributed
2D-SD	Distributed mean	Distributed	Non-distributed
2D-WD	Distributed mean	Non-distributed	Distributed