Referee Comments #2

The integrated process model (IPM) which resolves the processes extending from the atmosphere through the bedrock is a hot topic in recent years. Using the IPM, researchers try to investigate the interactions between atmosphere and underground hydrological processes (e.g., lateral flows, groundwater dynamics), which used to be neglected by traditional meteorological modeling works. The ParFlow-CLM model is also a famous tool that couples the one-dimensional and sophisticated land surface model (CLM) with the three-dimensional groundwater model (ParFlow). Xu et al. Tested the sensitive of some hydrometeorological variables, which were simulated by the WRF model coupled with an integrated hydrologic model, to the choices of physical parameterizations, meteorological forcings that provide lateral boundary conditions, and terrain shading options. The author found that, physical parameterizations contributes to the largest spatial temporal variance in simulating the temperature, precipitation and other related hydrological variables. Although the topic is important and the introduction is well written, I still think the innovation is not strong and the manuscript needs major revision. My major concerns are below:

1. The author emphasize the necessity and importance of IPM in the introduction and also take the IPM as one of the innovation of this research. However, the simulation work is based on one-way coupling (use the WRF simulated meteorological forcings to drive ParFlow-CLM). Whether this one-way coupling can be called as IPM is confused, as there is no feedback between meteorology and the underground hydrology.

Response: We thank the reviewer for her/his comments noting the sophisticated tools used in this study, as well as the importance and high quality of the manuscript writing. The concern about the use of the phrase “integrated process modeling” is potentially one of semantics in this case, although we’re happy to revise to avoid any potential confusion for the reviewer and/or other readers. There is a small body of literature that does use “integrated process model” terminology (e.g., Zhang et al, 2016 doi:https://doi.org/10.5194/hess-20-529-2016; Davison et al, 2017 doi:https://doi.org/10.1002/2017MS001052) that demonstrates the utility of coupling process models built to explore discipline-specific processes as a mechanism to advance interdisciplinary research. There is also a literature comparing and contrasting one-way coupling vs two-way coupling for mountainous hydrology: e.g., Camera et al, 2020 doi:https://doi.org/10.5194/nhess-20-2791-2020 and Rudisill et al, 2022 doi:https://doi.org/10.1002/hyp.14578 where the latter paper finds that in snow-dominated watersheds such as the ERW, which found that the representation of uncertainties in the representation orographic precipitation is the single largest driver of hydrological uncertainty while the inclusion or exclusion of two-way coupling has little effect on atmosphere-through-bedrock state evolution. Nonetheless, to avoid confusion we would like to clarify that we are referring to a one-way coupled IPM (or WRF-Parflow-CLM) in the revised manuscript.

2. Another issue is that the finding that "physical parameterization is much more important than lateral or initial conditions"has been revealed by numerous works in meteorological discipline. For example, Solman and Pessacg (2012) found that the largest spread among WRF ensemble simulation members is caused by different combinations of physical parameterizations. Pohl et al (2011) tested the uncertainties of WRF simulation caused by physical parameterizations, lateral forcings, domain geometry. And they also suggested that physical parameterizations have the largest influence on precipitation. So, from the perspective of meteorology, the current finding is not surprising. The author should review the previous works and rethink the added value of the current work.

Response: We thank the reviewer for raising this issue and will add the cited references in the revision where appropriate, especially given the concurrence of our findings with those references. However, our findings are not redundant with the published literature: those references either evaluated large-scale meteorological processes or did not focus on high-altitude complex terrain regions, which are central to our study. Additionally, most previous studies do not show how the range of reasonable IPM configurations (based on configurations that have been presented in the published literature) affects discharge, ET and subsurface hydrology. We would argue that these aspects to our work represent an additional set of novel contributions that will be of interest to the readers of HESS.

With this set of one-way atmosphere-through-bedrock process modeling results, we can and have uncovered how choices in atmospheric process model configurations impact the surface and subsurface hydrology. Specifically, we evaluate and quantify the sensitivity of discharge, ET, and subsurface hydrology to IPM configurations, and we also address how 3D topographic radiation schemes affect both spatial distribution and spatial average hydroclimate simulations. More importantly, this study aims to guide the plan of field observational activities in the future by (1) uncovering how uncertainties in the representation of atmospheric processes impact surface and subsurface process modeling and (2) providing direction for those field observations with the greatest potential to constrain atmospheric processes. We will revise the paper to highlight the added value of the contributions of our work to the existing, relevant literature.

3. Since the ERW is a heavily-instrumented catchment with a growing atmosphere-through-bedrock observation network (emphasized in abstract) and the "The goal of this work is to provide the mountain hydrology research community with a properly-configured IPM that can inform ongoing and future field campaigns and their process-modeling needs in the UCRB.", why don't you use the in-situ observations to evaluation the T2m and precipitation.

Response: We agree with the reviewer and look forward to adding the comparison against the precipitation, two-meter air temperature and ET in-situ observations in the paper. We agree adding those comparisons against in-situ observations will help quantify the model performance in terms of surface air temperature and precipitation and will emphasize the value of observational networks supporting model evaluation. The SAIL-based observations will be used in a future study to compare with IPM skill once the SAIL campaign is completed (2021-2023).

4. Moreover, I am really confused about the use of Parflow-CLM here. Is it used to only provide streamflow and groundwater storage? The simulated snow and ET a in re provided by CLM-Parflow or the default land surface model in WRF? Actually, the Parflow-CLM is often used to investigate the potential influence of three-dimensional groundwater on the responses of terrestrial hydrological processes to meteorological forcings (e.g., numerous high impact works performed by Maxwell and Condon). However, here, I did not see what will be different if we used the traditional one-dimensional land surface model to investigate the same issue. I suggested the author to compare the difference when using the results from default WRF land surface simulation and that from Parflow-CLM (such as ET, total water in the soil column). This may help enhance the innovation of current work.

Response: Yes, the primary reason for using ParFlow-CLM is to allow for the quantification of streamflow and groundwater storage. As the reviewer is probably aware, standalone WRF does not simulate these processes, although branches of the code (WRF-Hydro) do provide some insight into at least streamflow, although with a simplified and prescribed stream network. Groundwater in WRF-Hydro is highly simplified (shallow soil layers and a bucket model) while ParFlow simulates the full continuum of variably saturated flow in three dimensions.

We are interested here in developing, testing, and analyzing simulations with realistic representations of atmospheric processes and to explore how they interface with representations of surface and subsurface processes that are as realistic as possible. For this study, we would like to avoid complicating the analysis with additional model structural errors where we can, so that in the future, we can ultimately relax towards the more simplified representations of surface/subsurface processes in models such as WRF-Hydro.

The innovation of this work is to better understand how these choices in atmospheric parameterizations, forcing, and other configurations/options impact the greater hydrologic cycle. We respectfully disagree that the one-dimensional land surface model alone cannot yield this information, which is why we performed the additional modeling steps with ParFlow-CLM.

5. The experimental design needs more detailed information. I suggest the author to provide more introduction to the experimental design. For example, why do you only use the CFSR2 and ERA5 in the UCD and NCAR simulation? Why does the no3DRad_inner radiation scheme is only used in BSU_CFSR2 and BSU_ERA5?

Response: We designed the experiments with the intent to evaluate different subgrid-scale physical scheme configurations, atmospheric boundary forcings, and topographic specific subgrid-scale parameterizations. However, we recognized that this study would be computationally constrained given our prioritization of the use of sub-km horizontal resolution IPM simulations, hence why the model configuration matrix was not completely sampled. We first chose to run a series of experiments with the three most prominent WRF subgrid-scale physical scheme configurations in the literature. We learned that the BSU configuration is the optimal physical scheme relative to the others chosen. As such, and due to computational restraints, we then chose this configuration to interrogate the topographic specific subgrid-scale parameterizations (e.g., noD3Rad_inner). We hope to fill some of the gaps in the simulation matrix in future work, particularly when we compare these simulations with the SAIL observational campaign (once completed in 2023). We plan on including more descriptive language on the experimental design in the revised manuscript.

6. I suggest to show the topograpnhy of the inner domain in Figure 1 which will be helpful to better understand the influence of 3D-radiative scheme. Currently, I am confused why the valley gets more radiation after considering the topographic shading and slope effect.

Response: To address this comment, we can upate Figure 1 and present the topography of the inner domain in more detail and providing more descriptive text for Figure 1. This will help show how and why the east side of the valley receives more radiation because it's west facing, and the western side of the valley gets less radiation in the early morning due to solar geometry. Similar conclusions have been observed in Arthur et al. 2018.

7. Moreover, the author should proofread the manuscript. For example:

- The Figure S-4 in L 299 should be Figure S1?
- "Figure S-3 and Figure S-4"should be "Figure S-3"?
- There is no description or analysis on the Figure 8c-8d.
- I also noticed some grids are masked out in Fig S-5 and Fig S-6, but no interpretation is given.

Response: We thank the reviewer for catching these typos and will revise all of them accordingly in our revision.

Reference:

Solman and Pessacg. (2012). Evaluating uncertainties in regional climate simulations over South America at the seasonal scale.
Pohl et al. (2011). Testing WRF capability in simulating the atmospheric water cycle over Equatorial East Africa