



EGUsphere, referee comment RC2
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Comment on egusphere-2022-437

Anonymous Referee #2

Referee comment on "Sensitivities of subgrid-scale physics schemes, meteorological forcing, and topographic radiation in atmosphere-through-bedrock integrated process models: A case study in the Upper Colorado River Basin" by Zexuan Xu et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-437-RC2>, 2022

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:

- 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.
- 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.

- 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.
- 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 are 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.
- 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?
- I suggest to show the topography 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.
- 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.

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