



EGUsphere, referee comment RC3
<https://doi.org/10.5194/egusphere-2022-437-RC3>, 2022
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Comment on egusphere-2022-437

Anonymous Referee #3

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-RC3>, 2022

This work presents a model study in a headwaters catchment in the Upper Colorado Watershed. In general the work is interesting and well-written but the presentation is somewhat confusing. There are some major points I think the authors need to address before the work's suitability for publication can be assessed. They are detailed below.

General Comments:

-The terminology of so-called IMP's used is confusing along with the reference to these as coupled models, which I would argue they are not. It's very confusing to discuss this work in the framework of different models as opposed to just forcing used to drive the hydrologic model. The language around the different products used is very confusing and makes much of the discussion hard to follow. Some of the meteorological forcing datasets appear to be used to drive the models directly, but in the introduction it appears that only WRF simulations are used to drive models. A completely re-write of this entire section is needed to make this clear. What did the authors do with the hydrologic outputs from the WRF simulations? Why are the Noah and Noah-MP models used interchangeably but the results are not compared to ParFlow-CLM? Except for groundwater which isn't discussed very much in the manuscript, all the same results should be in the WRF simulations. Why didn't the authors just run the WRF-ParFlow model or even mention its existence? They talk about everything in a coupled sense but the models are in no way formally coupled (unless something is happening that is not discussed in the manuscript); the output files from WRF simulations are saved and somehow reformatted (this is not clear) and used to drive ParFlow-CLM. They could drive any hydrologic model and it wouldn't be considered a coupled platform, likewise the standard forcing products the authors might choose to drive the simulations off the shelf are also generated with atmospheric models, yet I would never think of this as an IMP. I suggest the authors are much more transparent about this aspect and remove the terminology from a revision. They should also provide some clear language about what is actually being done here, is this a comparison between forcing generated with WRF v other approaches? Why didn't the authors just run forced by PRISM?

-Coupled v uncoupled processes and feedbacks. There has been a lot of work to understand the role of feedbacks between two-way coupled hydrologic models and atmospheric models. Examples include WRF-Hydro-WRF (e.g. Arnault 2016), COSMO-CLM-Parflow (e.g. Keune 2016, 2019), WRF-ParFlow (e.g. Maxwell 2012, Forrester 2020), feedbacks over complex terrain (Ban 2014), and other more conceptual approaches (e.g. Miguez-Macho 2007). This is not an exhaustive list, but demonstrates that much work has been done to study these feedbacks, Some of these studies are in complex terrain and even suggest that the approach used by the authors may not be valid at high resolution without lateral flow. These studies all systematically compare different types of model physics (e.g. free drainage, the standalone atmospheric model, fully coupled system) and use varying metrics to diagnose coupling strength and changes in the atmosphere. I suggest the authors read these prior studies carefully and develop a new section that summarizes (rather than ignores the existence of) this body of work and uses this to put the current study in context. This will help frame the current work and help it look much less like a patchwork of runs that are loosely tied together. This will also help clarify my point above, to help the reader follow what is being done and what runs are conducted in the current work.

-Variability in point processes compared to integrated or averaged measures. I mention this as a specific instance below, but it is also a general point, there are instances where the authors present differences locally (at a point) that do not persist synoptically. Do the different forcing products or microphysics (I think this is the point the authors make) make some difference locally for e.g. precip, radiation, but does some averaged quantity remain unaffected. It appears this is the case for much of the analysis. That is, topographic shading makes a difference locally in LH flux but the domain averaged LH flux remains unchanged between cases. The authors draw one conclusion (local differences) without acknowledging the other (same net energy flux over the domain).

-Atmospheric uncertainty. There has been much work on differences in model physics in a model such as WRF that allows different physical parameterizations to be "swapped out" easily in simulations by changing the namelist. This is an important aspect of uncertainty, but it is almost always put in the context of one of the major forms of uncertainty in the atmosphere, propagation of initial conditions. One should always determine that such a physics change is robust using (e.g.) time-shifted uncertainty in an ensemble type approach (e.g. Walser 2004). Often upon inclusion of uncertainty in the initial model state (in the atmosphere) the differences in physical parameterization no longer dominate.

-Can the authors compare meteorological forcings at the site? A heavily-instrumented catchment (abstract line 16-) should have observations of meteorological variables and snow outside of the SNOTEL (which I don't think are used for comparison and should contain precipitation and temperature), even precipitation and temperature at gage locations would be very instrumental. It appears that the authors treat the PRISM product like observations, which is an unfortunate and hopefully accidental. The PRISM product is a model, even if statistical, that takes into account observations in a region. One would assume that then PRISM is ingesting precip from the SNOTEL sites in the domain but this isn't stated (are there even any observations that PRISM is using and is it thus totally unconstrained?).

-The authors should compare to ET observations in Ryken et al 2022 to results of current work (both WRF and ParFlow-CLM). Additionally, it appears that the Ryken et al 2022 paper has meteorological observations of precip, temperature and radiation that might be useful to partly address my comment above.

Specific comments

line 65: is PF-CLM being cited using Maxwell et al 2015 (cited on line 617)? That paper references a simulation over large scale that as I read it is forced externally and does not use or describe the CLM model.

Line 240+ This section describes the PF-CLM model in general but I could not find specifics for the model domain used in this study? What is the resolution or model configuration for the PF-CLM domain? How deep is the subsurface? What is the lateral resolution? How was this matched to the forcing datasets or the WRF outputs? Was there a balance of water and fluxes between the grids? How were model parameters determined? Are there references to prior work on this model? Calibration? If not the authors might include a description of these aspects in the current manuscript and as supplemental material.

Lines 275-281. The UCD datasets appear to have the most precip but the ear

Figure 3 caption (~line 305): a, b, c are used to identify plots in the figure but are not used in the caption. Also, it does not appear that 3c is described in the caption.

Figures 5, 6 and associated discussion. An interesting point that might be made here is that while local spatial differences are apparent in Figure 6, the domain averages (even for SWE) are the same between shaded and non-shaded formulations. This suggests that while it may be striking visually to include shading, the upscaled water balance for the catchment isn't sensitive.

lines 383- I'm not sure I agree with these conclusions. While the cumulative variability in outflow resulting from the different forcing products creates different cumulative outflows, Figure 7a indicates that there is no difference in timing across all the forcing datasets. My suspicion is that the differences in outflow are due to total water quantity (Figure 5a suggests this as well) and are simply a precip bias artifact in the different WRF runs.

line 443: "Here, we ... coupling WRF and ParFlow" rephrase, this sentence isn't correct, the models were not coupled

line 476+: This text appears to acknowledge the lack of coupling in the current work (as an aside, what is "one-way coupled" this is not actually coupled at all, as it appears the results from WRF were simply used as forcing for the PF-CLM model. This isn't bad, but as mentioned above should be discussed up front. The arguments here regarding computational expense as an excuse for not running coupled simulations are incorrect, prior studies have shown with the e.g. WRF-PF model that ParFlow is approximately 1% of the total computational time compared to WRF which is 99% of the computational time. Thus if the authors ran WRF for this domain, the additional expense to run with WRF-PF is a negligible increase in cost. Also the authors might want to correctly identify that the Forrester et al study (line 483) was run with WRF-PF and the authors might want to read and cite Forrester 2020 which discusses limitations of running high resolution, uncoupled WRF simulations in mountain terrain (the CO headwaters was studied where the lack of lateral flow caused changes in the surface energy budget and height of the boundary layer.

line 498: Is the watershed highly instrumented now or will it be? This seems at odds with statement in the abstract (line 16)?

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