

Referee Comments #3

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.

We thank the reviewer for his or her comments on this work being interesting and well-written. We have addressed all points the reviewer requests clarification on 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 meterological forcing datasets appear to be used to drive the models direclty, 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?

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.

We agree that the use of the word “coupled” should be used with caution, as some of these models are formally coupled within their source-code infrastructure, and others are “step-wise coupled”. To avoid any potential confusion, we have modified the manuscript to only use the

word “coupled” when there is a formal two-way and self-consistent pairing of codes (for example, ParFlow and CLM). In contrast, the associated meteorological variables used to drive ParFlow-CLM are now referred to as “forcing” only, as this is a one-way interaction between the codes/outputs. 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.

The name of the meteorological products are the forcing datasets for the WRF runs, not the forcing data for ParFlow-CLM. Precipitation, temperature and other hydroclimate simulations from WRF are used as the atmospheric drivers for the ParFlow-CLM simulations. We will revise the manuscript to explicitly clarify that the meteorological forcing datasets are used as the initial and boundary conditions for the WRF model, and the WRF model outputs are then used to force ParFlow-CLM.

The Noah and Noah-MP hydrologic output from the WRF simulations included ET, surface and subsurface runoff. We agree with the referee (and referee #2 who identified this too) and will compare the Noah and Noah-MP simulated ET against ParFlow-CLM. 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) opposed to ParFlow, which simulates the full continuum of variably saturated flow in three dimensions. Thus, while we indeed have generated multiple datasets with the different LSMs, this is not our primary objective for intercomparison and we believe showing more of these results would be distracting. We focus on the set of LSM outputs from WRF for simplicity (and to avoid confusion) and show the groundwater and streamflow outputs from 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 it's 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 authours are much more transparent about this aspect and remove the terminology from a revision. They should also provide some clear language about what is actual 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?

Response: As is hopefully now more clear based on the last answer, we use WRF outputs to force ParFlow-CLM as a mechanism to simulate the integrated hydrologic response of the watershed. We do this because WRF is not capable of simulating two key processes of interest: not just groundwater storage changes but also streamflow. We did not run the fully coupled

WRF-ParFlow model platform because these simulations are extremely computationally expensive. We agree that it would be interesting to perform these 2-way coupled simulations in the future, but believe that an important first step is to determine what the hydrologic response is without the two-way interaction (i.e. WRF-forced ParFlow-CLM as we've done here). Fully coupled 2-way simulations are by no way the "standard" in hydrologic modeling, and the amount of fidelity we've included in this study stands to dwarf that of other approaches. That being said, we acknowledge that the existence of the code could be pointed to as an interesting next step. Two-way integrated meteorological and surface/subsurface hydrology simulations in complex terrain are especially limited, and a new frontier in the field.

Furthermore, we do appreciate the sentiments of the reviewer and will ensure that we will more clearly outline various methodological approaches taken in our model simulations and analyses in our revision. For example, the WRF simulations were regredded using bilinear interpolation and used as the forcing dataset to run the ParFlow-CLM model. Therefore, the simulations are, as the reviewer points out, a one-way coupled IPM. We will ensure that we mention that WRF-Parflow is also available as a two-way coupled IPM too (and cite the relevant literature).

Finally, we agree with the reviewer and can run the ParFlow-CLM simulation forced by PRISM. We plan to regrid the 800-meter resolution PRISM daily precipitation and temperature variables and temporally interpolate them using an adjustment to the hourly distributions provided by the WRF hourly output. Other than precipitation and temperature (only variables made available from PRISM), we will use the WRF hourly output to force the ParFlow-CLM simulation.

-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.

Response: We agree with reviewer that there are multiple two-way coupled IPM configurations and will ensure that our revision better contextualizes our one-way coupled IPM configuration within the broader two-way coupled literature. We can develop a new paragraph in the introduction to demonstrate the details of those models in the paper, and

-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).

Response: We agree with the reviewer and look forward to adding more discussion of locally specific versus watershed average differences in our IPM results. For example, the importance of topographic shading on the spatial distribution of radiation flux. The variances across different forcing products and subgrid-scale physical schemes are shown in our paper through both spatial distributions and average quantities. However, the 3D topographic radiation schemes only redistribute the energy flux thus affect LH flux spatial distribution, but do not show significant difference in the watershed average quantities. In the revision, we will more clearly highlight these points.

-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.

Response: We acknowledge the reviewer's sentiments about using a traditional experimental design to isolate structural versus internal variability uncertainties in climate modeling approaches, specifically through the use of initial condition perturbation ensemble members (internal variability) and holding subgrid-scale physics parameterizations constant (structural uncertainty). We will add a few sentences in the Methods section pointing to the reviewer's important points, explicitly cite Walser and Schaer (2004) and be more explicit about how our experimental design differed from the more traditional approach. With that said, we also would like to acknowledge that although our experimental design didn't assess internal variability through the more traditionally employed initial condition perturbation approach (i.e., alter the initial conditions provided to the atmospheric model by a rounding error) we did isolate large-scale boundary conditions through the use of several atmospheric reanalyses that, in a way, also isolate slight differences in large-scale boundary conditions (e.g., integrated vapor transport).

We believe that a major finding of our work that hasn't necessarily been explored in the literature extensively over the East River watershed is that these slight differences in large-scale boundary conditions (analogous to perturbing initial conditions by rounding errors) did not markedly change water year precipitation totals unlike changes in subgrid-scale physical parameterization choice.

-Can the authors compare meterological forcings at the site? A heavily-instrumented catchment (abstract line 16-) should have observations of meterological 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 observationsm, 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 obsevations that PRISM is using and is it thus totally unconstrained?).

Response: The large-scale meteorological forcings used in the WRF simulations have coarser grid resolutions than the WRF inner domain, so comparing IMP simulation results with meteorological forcing at the sites do not add scientific significance in model evaluation.

The reviewer is right about the PRISM product that it used SNOTEL datasets and was generated using statistical methods. PRISM dataset has been widely used to evaluate climate models in the complex-terrain region, with many thoughtful uncertainty analysis (e.g., Lunquist et al., 2020). We used interpolated precipitation and temperature product PRISM to evaluate the model performance over the whole domain. In the supplementary material, we presented the comparison of precipitation and temperature against snotel stations. In the paper revision, we can compare with in-situ observation data, including precipitation and temperature measurements taken at sensors installed in the East River watershed. We did not provide these in the initial manuscript to avoid distracting from the main message of the paper, but see the relevance and could add these to the supplemental in the revision.

-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 meterological observations of precip, temperature and radation that might be useful to partly address my comment above.

Response: We agree with the reviewer and can add the comparison against the ET measurement at the eddy-covariance tower at the East River, but again do not want to distract from the main purpose of this study by making this an ET intercomparison so this can potentially be added to the appendix

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.

Response: Yes, while this is the same ParFlow, we agree a better citation can be used, the standard are: (Ashby and Falgout, 1996; Jones and Woodward, 2001; Maxwell, 2013). We will cite more appropriately in the revised version, including explicit mention of previous version of the model in the East River (Maina et al., 2022; Foster and Maxwell, 2018; Pribulick et al., 2016).

Citations:

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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.

Response: The ParFlow-CLM subsurface domain is 30-meter deep, 100-meter horizontal resolution. The WRF output are re-gridded using bilinear interpolation to match the ParFlow-CLM grid cells. The model parameters are based on a variety of geological and soil parameters, and calibrated using streamflow measurements. More details can be found in some previous

papers (e.g., Foster et al., 2019, Pribulick et al., 2016, see the answers to the previous question). We will add more detailed descriptions of the ParFlow-CLM model in the manuscript

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.

Response: We agree with the reviewer and can revise the figures accordingly.

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.

Response: We agree with the reviewer that turning on and off the 3D shaded radiation scheme do not significantly affect the domain average. The reviewer is correct that the 3D shading radiation scheme do not affect the upscaling water balance, but redistribute the spatial distribution of radiation fluxes thus SWE and energy spatial pattern. The east side of the valley gets more radiation because it's facing west, and the western side of the valley gets less radiation as it gets less radiation in the early morning. Similar conclusions have been observed in Arthur et al. 2018.

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.

Response: We agree with the reviewer that Figure 7a indicates the difference in timing in all the IPM across all forcing datasets. The difference of precipitation in WRF simulations leads to the differences in streamflow simulation in ParFlow-CLM. We can revise this sentence to clarify and avoid misunderstanding.

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

Response: We can reword this as 'one-way coupling IPM' or "WRF-ParFlow-CLM" to avoid misunderstanding.

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.

Responses: The reviewer is correct that the WRF simulation used most of the computational and throughput time. However, we are unaware of any study which shows only a 1% CPU demand for ParFlow-WRF opposed to standalone WRF. Personal communication with the primary ParFlow developer confirmed this is not a strict rule of thumb, and that there are various factors which determine the additional CPU time to include ParFlow in a WRF run. This is especially true in complex terrain where there are sharp wetting fronts and higher demands on the Richards' equation solver. Thus, it's difficult to know the exact additional demands of the fully coupled code without performing the simulations.

We agree to update the statement as "one-way coupling IPM" in the revision. We also concur with the reviewer that computational expense is not the major excuse for not running coupled simulations, moreso that we wished to establish a baseline set of simulations without fully coupled feedbacks before considering the 2-way interacting model.

We agree with the reviewer to cite Forrester 2020 paper, which discusses how the subsurface groundwater flow in ParFlow-CLM (lateral groundwater flow and subsurface lower boundary conditions) affects the atmospheric model. This impacts were particularly enhanced during summertime, while we run the simulation for a whole water year. We agree to highlight the findings from previous fully-coupled WRF-PF model, and extend the discussion of one-way and two-way coupling in the revision.

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

Response: To clarify, the East River watershed is already highly instrumented due to the presence of the long-standing Rocky Mountain Biological Laboratory (RMBL), the SNOTEL network, and DOE Watershed Science Focus Area project which has been adding instrumentation to the watershed over the last ~7 years. While these observations focus primarily on surface and subsurface processes, the East River watershed has become even more instrumented in recent years (2021-2023) through the support of the U.S. DOE (SAIL campaign) and U.S. NOAA (SPLASH campaign) deployments of a comprehensive set of atmospheric instruments (e.g., radar and radiation measurements).

However, these SAIL and SPLASH campaign measurements were provided after our simulations were conducted and we began to prepare this manuscript. In future work, we plan

to build upon the knowledge learned from this manuscript to compare the most optimally configured IPM to SAIL and SPLASH campaign observations. We can revise this sentence to best reflect the added significance of SAIL and SPLASH campaigns in future IPM studies in the UCRB region, and look forward to using those datasets to improve mountainous hydrologic cycle process understanding and model development.

references

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