Coupling a three-dimensional subsurface flow and transport model with a land surface model to simulate stream-aquifer-land interactions (CPv1.0) [MS No.: gmd-2017-35]

Responses to review comments

Anonymous Referee #2

In this manuscript the authors document development and application of a coupled Richards' equation solver (PFLOTRAN) with a land surface model (CLM 4.5) and apply it to a test problem developed from an intensely observed floodplain system. This manuscript is generally clearly written but in my opinion needs to better articulate its contributions given the prior work on this topic. I have specific comments below that need to be addressed before the suitability of this work for GMD can be assessed.

The larger comments are ones of contribution, what does this work want to contribute to our understanding of coupling models? Given that the main contribution (as I see it) is the coupler yet this is not novel i think the authors have the challenge to clearly articulate what their contribution is. I encourage them to revise their manuscript accordingly to do this.

1*. Introduction. The background provided in the introduction is a nice overview.*

Response: Thanks for the positive comments.

2. *Lines 91-97, the authors should also include TerrSysMP system (Shrestha et al MWR 2104) in this list and perhaps the numerous follow up studies using this platform. The platform is particularly important as it couples the same LSM as used in this study (CLM 3.5, now 4.5) coupled to an integrated hydrology model. As examples, the authors of this platform have used it for fully-coupled studies over all of Europe (Keune et al. JGR-A 2016) and for high resolution simulation (Gebler et al JoH 2017). It strikes me that these studies are much more advanced than the current effort and should be used to demonstrate how the current study is advancing the science.*

Response: Thanks for the great suggestion. We have added reviews on these studies in the introduction section of the revised manuscripts, and added discussions on how such coupled models could advance science in section 1 of the revised manuscript as follows:

"The developments of the integrated models have enabled scientific explorations of interactions and feedback mechanisms in the aquifer-soil-vegetation-atmosphere continuum using a holistic and physically based approach (Shrestha et al., 2014;Gilbert et al., 2017). Compared to simulations of regional climate models coupled to traditional LSMs, such a physically based approach shows less sensitivity to uncertainty in the subsurface hydraulic characteristics that could propagate from deep subsurface to free troposphere (Keune et al., 2016), while other physical representations (e.g., parameterizations in evaporation and transpiration, atmospheric boundary layer schemes) could have significant effects on the simulations as well (Sulis et al., 2017). Therefore, it is of great scientific interest to further develop the integrated models and benchmarks to achieve improved understanding of complex interactions in the fully coupled Earth system."

3. *Lines 103-104, the sentence is confusing. Do you mean that sometimes models agree and sometimes they don't?*

Response: Thanks for pointing this out. We have modified this sentence as follows in the revised manuscript:

"However, as a result of difference in physical process representations and numerical solution approaches in terms of (1) the coupling between the variably saturated groundwater and surface water flow; (2) representation of surface water flow; and (3) implementation of subsurface heterogeneity in the existing integrated models, significant discrepancies exist in their results when the models were applied to highly nonlinear problems with heterogeneity and complex water table dynamics, while many of the models show good agreement for simpler test cases where traditional runoff generation mechanisms (i.e., saturation and infiltration excess runoff) apply (Kollet et al., 2017;Maxwell et al., 2014)."

4. *Paragraph starting at line 107. This paragraph should be re-structured. One of the main criticisms I have of this work is the lack of novelty. This paragraph is one of the main places the authors can distinguish their work from prior studies. They don't in fact show scalability of either code and the other two points are somewhat weak science goals. I think restructuring this paragraph will help the authors develop a manuscript that is better organized and articulates the contributions made by this work.*

Response: Thanks for the constructive comments. We have revised this section significantly to include discussions on the scientific potential of integrated models based on recent studies. We also revised the coupling section to provide more details on how the coupling was achieved. Please check the revised manuscript to see if the revisions are satisfactory.

5. *Integrated hydrology models are such (and not just Richards' solvers) because they solve a form of the shallow water equations and Richards' equation in a globally implicit manner. It is unclear that PFLOTRAN has a surface component, so is it an integrated code?*

Response: In this study, a prognostic model for simulating river stage dynamics is not included. Instead, a river stage boundary condition was applied to PFLOTRAN to capture observed and hypothetical river stage scenarios. Even though a shallow water equation implementation in PFLOTRAN is under testing, it is premature and warrants a standalone study to assess its performance. Therefore, we have modified the text throughout the revised manuscript to remove ambiguity in this regard. We also added the need of implementing a surface flow component to qualify the model as an integrated hydrology model into the discussion section. Please check the revised manuscript for detail.

6. *Lines 205-220. As I see it, the coupler is the only potential contribution made in this work. The description needs to be much more detailed. What fluxes and states are passed between the two codes? How is the gridding handled? How is the parallelization accomplished for tiling in CLM and cells in PFLOTRAN? How is the grid overlap between soil column and 3D mesh approached, is the 3D Richards' formulation used in place of CLM or is there some other point where the codes couple? What time integration strategy is used? These are all critical points that should be addressed.*

Response: In response to all reviewers' comment regarding model coupling, we have significantly revised the technical details about model coupling in Section 2.3 and added a new schematic (Figure 1) to describe the two-way model coupling. We attempted to answer all the specific questions of this comment. The models are coupled two-ways and we have updated Figure 1 to better represent model coupling as follows. Please section 2.3 of the revised manuscript for detailed description on this coupling.

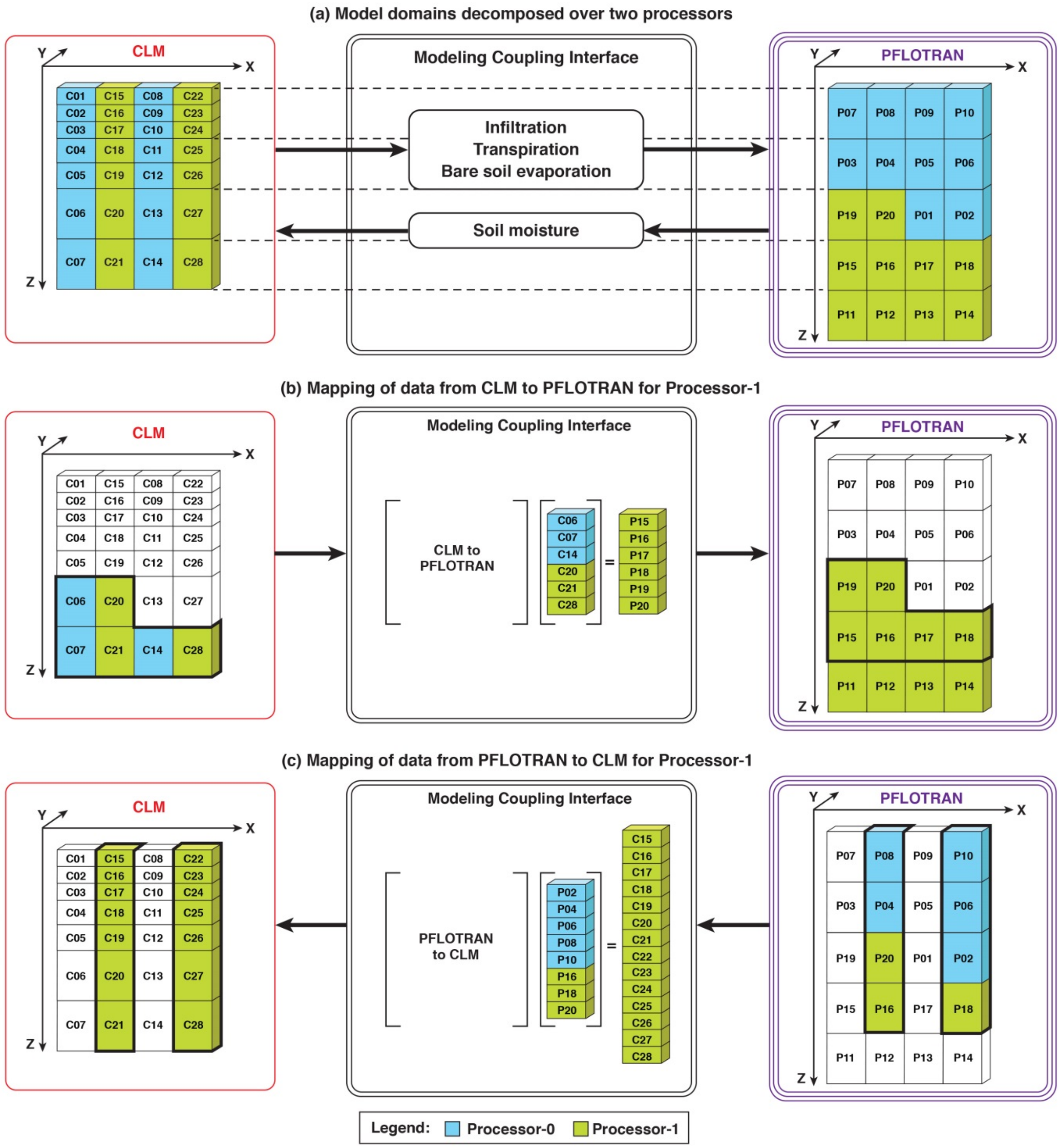

Figure R1. Schematic representations of the model coupling interface of CP v1.0. (a) Domain decomposition of a hypothetical CLM and PFLOTRAN domain comprising of 4x1x7 and 4x1x5 grids in x, y, and z directions across two processors as shown in blue and green. (b) Mapping of water fluxes from CLM onto PFLOTRAN domain via a local sparse matrix vector product for grids on processor 1. (c) Mapping of updated soil moisture from PFLOTRAN onto CLM domain via a local sparse matrix vector product for grids on processor 1.

7. *Lines 218-220. Surely the authors don't mean this is the first study to couple 3D Richards' equation to a land surface model, that has been documented in the literature for more than a decade. Do the authors mean the PFLOTRAN CLM 4.5 coupling? That isn't novel. This*

sentence makes the authors sound either disingenuous or naive, either way I think it should be restructured or removed.

Response: Thanks for the suggestion. We have deleted this sentence.

8. *Verification. There is no section describing the verification of the modeling approach. Prior studies have carefully calculated the energy and water balance of the individual and coupled systems to ensure that nothing in the original formulations has been altered by the coupling and that the coupled system balances water and energy between models. This is a critical missing aspect of the work. It's important to distinguish this from model validation, where a system that is poorly constructed could still be tuned to match observations.*

Response: We agree that a thorough evaluation of the energy and water balance terms is needed for the system. To address the reviewer's concern, we have (1) verified that the subsurface solver by evaluating the mass balance errors for each time step and added the figure to the supplementary material; and (2) verified that the surface energy balance and added the figure to the supplementary material. Discussions on these figures were added in section 3.1 of the revised manuscript.

9. *PFCLM. The abbreviation PFCLM has been used widely in the literature for the coupled codes ParFlow and CLM. The use here is confusing and a different acronym should be chosen. Also, given the order of calling (PFLOTRAN is a subroutine of CLM 4.5) it seems the CLM component leads, not the hydrology one.*

Response: Thanks for the constructive suggestion. We have changed the model name to be CP v1.0 to be consistent with the sequence of coupling, and to differentiate the model from ParFlow-CLM and previous coupled versions of CLM4.5 and PFLOTRAN. We have modified all occurrences of the names in the text and figures. Please check the revised manuscripts for details.

10. Scale. The Hanford test case appears to be at very fine spatial resolution (2m) which violates most of the assumptions made for land-energy fluxes in CLM. The M-O stability and ET formulations use a single-column approach which would almost assuredly break down at this resolution. Studies that do consider this type of fine scale usually use LES formulations for the atmosphere to relax this assumption. The authors need to discuss this and perhaps discard the 2m case.

Response: As noted by the reviewer, CLM uses the M-O similarity theory to compute friction velocity and other exchange coefficients that provide the basis to estimate surface heat and moisture fluxes. It is also common knowledge that the M-O similarity theory is only valid when the surface layer depth z>>z0, where z0 is the aerodynamic roughness length. As reviewed in Basu and Lacser (2017), It is highly recommended that $z > 50z_0$ to ensure that the lower atmospheric level is higher than the size of surface wakes in the roughness sublayer, which should be proportional to horizontal grid spacing to guarantee the validity of the M-O similarity theory (Arnqvist and Bergström, 2015).

In our simulations, the majority of the Hanford 300A domain is covered by bare soil (z_0 = 0.01 m), grass (z_0 = 0.013 m), shrubs (z_0 = 0.026-0.043 m), and riparian trees (varies across the seasons, z_0 = 0.008 m when LAI = 2 in the summer and z_0 = 1.4 when LAI = 0 in the winter). Therefore, under most condition a 2-m resolution is sufficiently coarse to ensure the validity of the M-O similarly theory, except for the grid cells covered by riparian trees in the winter. However, in our simulations, the wintertime latent heat and sensible heat fluxes approach zero due to extremely low energy inputs in the winter. In addition, the 2-m resolution simulations are valuable for verifying subsurface simulations. Therefore, after careful considerations, we decide to keep the 2-m simulations, but added discussion on potential issues when the model is run at such a resolution section 5 in the revised manuscript. We hope such a treatment could alleviate problems associated with the scale of model applicability.

Reference:

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