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Interactive comment on "Coupling a three-dimensional subsurface flow and transport model with a land surface model to simulate stream-aquifer-land interactions (PFLOTRAN_CLM v1.0)" *by* Gautam Bisht et al.

Anonymous Referee #2

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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

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Discussion paper

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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.

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). I 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.

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

4. Paragraph starting at line 107. This paragraph should be re-structed. 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.

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?

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 paralleliza-

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tion 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.

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.

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.

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.

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.

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