

Interactive comment on “A consistent implementation of the dual node approach for coupling surface-subsurface flow and its comparison to the common node approach” by Rob de Rooij

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Received and published: 16 May 2017

I appreciate that Anonymous Referee #1 has carefully read the manuscript and I acknowledge that certain concerns raised by the referee should be addressed if this manuscript is to be revised.

In case of a revision, I will comment in more detail in how these concerns are addressed (i.e. one by one). However, at this stage of the peer review process I would like to focus on those concerns with which I do not agree as well as those to which I would like to add my opinion.

Overall I am a bit worried that the comments of Referee #1 can be interpreted as a rejection of the manuscript. That is also the reason why I think that going through all the concerns as raised by Referee #1 one by one is not very useful at this point.

Referee # 1 is not sure that the material is enough for a research paper. The Referee provides two reasons for this: 1) The research is not novel and its conclusions are already documented in the literature. 2) The differences between the coupling approaches are irrelevant because if properly implemented the different coupling approaches work reasonably well.

Regarding the first reason, the Referee point out that it has already been demonstrated that both coupling approaches yield similar results if a proper discretization is used. However, the fact that it is known that both approaches will yield similar results under certain conditions is acknowledged in the introduction by referencing Ebel et al. (2009) in line 69-70. Moreover, this fact is not presented as a new insight or at least this was not my intention (In a revision I can make some changes to make this more clear).

More importantly, however, it is not true in a general sense that that the conclusions of this paper are not novel. Namely:

1) Following existing literature, the common node approach is typically presented as the more general and more elegant approach, because contrary to the dual node approach, there is no need to specify a coupling length that lacks a physical meaning. In my work, I explain that the coupling length is not related to an unphysical separation between the surface and the subsurface, but to the vertical discretization of the topmost subsurface cells. Also I explain that the common node approach is only in agreement with the principle of head continuity if the topmost cells are very thin. Subsequently I present the case that if the dual node approach is properly implemented then it is actually more general as well as more elegant than the common node approach. These insights that conflict with the common consensus are novel and have not been discussed in existing literature.

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2) I present a different conceptualization of the dual node approach in terms of a first-order approximation of the vertical hydraulic gradient at the land surface which does not rely on introducing a distinct interface between the surface and the subsurface. This is a new insight.

3) Most studies that have compared the common and dual node approach have been based on improperly implemented dual node approaches. As such the findings of my manuscript are significant.

Maybe, I misunderstand the Referee. It may be the case that the Referee is not so much claiming that my manuscript does not contain new insights, but is instead making the argument that these insights although novel are irrelevant. The Referee makes the valid point that in order to capture the infiltration fronts the vertical discretization has to be small and it is true that for sufficiently fine vertical discretizations the different coupling approaches work reasonably well. So from an application point of view the findings in my manuscript are indeed somewhat irrelevant if and only if the vertical discretization is sufficiently fine. However, there are some important reasons why my findings are still relevant:

1) When choosing a coupling approach, the existing literature may point to the common node approach as the most obvious choice. My work provides some alternative insights into which approach is to be preferred.

2) An understanding of how these approaches work is relevant from a learning as well as a theoretical perspective.

3) There exist models in which the vertical discretization is not very fine. Even if those models violate the requisites for simulating steep infiltration fronts, it is still important to understand why different coupling approaches yield different simulation results.

4) If the dual node approach can be implemented more properly at no cost and may provide some gains in efficiency and accuracy, I fail to see why one would like to stick

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with inconstant approaches just because they don't pose a problem as long as the vertical discretization is sufficiently fine.

5) Except for accuracy, there is also the question of efficiency. Even if those differences become smaller when using a finer discretization, the overall findings seems to indicate that the dual node approach is more efficient than the common node approach [This seems to correspond with the experience of the Referee].

I also want to point out that the issue of efficiency as discussed in my work is actually quite novel. In Ebel et al. (2009) efficiency is linked to the tightness of the coupling, but exactly why a tighter coupling is less efficient remains unclear. I think, that I provide additional insights into why the common node approach is less efficient (i.e. faster changes in water depth at the moment of ponding]. I agree, however, that I may need to explain this better. In any case, the issue of efficiency in surface-subsurface models is typically ignored in most of the existing literature.

Finally, I want to respond to the Referee's concerns about the tone and phrasing in the paper regarding other models. To some extent I can try to change the tone. However, to highlight the advantages of a properly implemented dual node approach, I have to say something about alternative approaches. I am not directly convinced I could skip this altogether. It is also not my intention to denigrate other approaches, but the approaches simply compare as they do and I think that these comparisons are important. For example, the inconstant dual node approaches can easily be modified. In MODHMS, one could simply turn off the adapted pressure-saturation relationship. I think that is a valuable insight.

ParFlow is an important model to reference as it was the first model to implement a common node approach. Moreover, it is a popular and powerful code. However, I cannot simply reference the existing literature and then give the configuration of the common nodes in this model. Almost anyone will point out that this configuration does not simply follow the literature (there does not exist a clear picture and explanations are

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contradictory) and subsequently one may conclude that the common node approach as implemented in my work does not resemble the approach in ParFlow.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-168, 2017.

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