

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

Anonymous Referee #1

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I have carefully read the manuscript called “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. This paper raises important issues regarding the application of integrated hydrological models through the examination of the possible influence of the coupling strategy and the vertical discretization. It especially investigates the following scientific questions (i) what is the proper coupling length to be used for the so-called dual node approach; (ii) how to formulate the dual node approach to conserve the physically based nature of the model; (iii) how does the coupling strategy influence the simulated dynamics when the vertical resolution is coarsened and (iv)

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how do the common node and the dual node approaches compare on synthetic test cases.

Before going to my comments of the paper, I want to stress out that these issues are critical and barely discussed in the integrated hydrologic modeling literature. Integrated hydrologic models are more and more used to investigate hydrologic behaviors but the questions of the appropriate scale, spatial resolutions (both horizontal and vertical), the crucial modeling choices that are to be made (coupling length for instance) and their effect on the simulated dynamics are too often forgotten although in my opinion of primary importance. I especially believe that there is a need to keep the physical meaning of integrated hydrological models through the use of appropriate spatial resolutions. This point is made very clear in the paper and is in a way the starting point of the research presented.

The consistent dual node approach proposed in the paper is clearly exposed and is a way to properly account for infiltration, especially in partially ponded cells. This approach for coupling allows preserving the physics of infiltration across the land surface if numerical parameters and spatial resolution are chosen adequately. A detailed analysis on the surface and subsurface pressure values, on the infiltration flux and on the time to ponding is provided. This analysis demonstrates the added-value of this method mainly (and only?) to describe the infiltration excess process. Although the issues tackled are of interest and the method proposed seems appropriate, I have serious concerns with the paper and I am not sure that the material presented is enough for a research paper. It seems that the added value of the approach proposed is not so important compared to the classical coupling approaches if the classical approaches are used in a relevant way. I hope that the following comments will somehow help improving the manuscript and maybe help in the publication process.

Major comments:

(1) One of my major concern deals with the fact that most of the conclusions of the re-

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search proposed in this paper are not novel and already documented in the literature. For instance, it has already been demonstrated that when using a proper discretization both coupling approaches gives very similar results and that a relatively small coupling length needs to be used with the dual node approach to conserve the physical meaning. It is true that integrated models tend to be used out of their proper application domain with coarse vertical discretization but it is more than intuitive that the vertical resolution should be small to properly capture the non-linear dynamics of infiltration fronts (especially when infiltration excess occurs). If the integrated models are properly applied, most of the questions that are tackled in the paper are not a problem anymore. In a way, the paper aims at determining which method is the less inaccurate (see line 554 to 556) when using a coarse vertical discretization, which is in a way irrelevant as both approaches are acceptable when using a proper resolution. These comments are illustrated through the conclusion that is short and not so much informative.

(2) The second main concern is linked to the tone and the phrasing of the paper that are not always adapted especially when reference models of the literature – i.e. Hydrogeosphere, MODHMS or Parflow – are criticized. I acknowledge that the coupling in Parflow is not well described in Kollet and Maxwell (2006) and that as a consequence some important aspects of Parflow turn out to be unclear. But I don't feel like there is a need to point out in details what the author think is not done properly by others. Once again, if an integrated model is used carefully with proper discretization and coupling length, it will produce consistent (with the physics) results regardless if it is a common node or a dual node approach. As a consequence, it is preferable to highlight what the consistent dual node approach brings than to denigrate the other approaches. I think that part 5 should be removed or at least strongly modified.

(3) I have serious concern about the result regarding the numerical efficiency. First I don't understand the arguments presented at the beginning of the part 7.2 that directly link the infiltration rate and the gradient across land surface with the numerical efficiency. It is a problem for me as all the following discussion on the efficiency is related

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to that argument. I feel like this point should be explained better. Moreover, the efficiency of the resolution is highly linked to the numerical procedure (numerical scheme, time integration, ...) that is used to solve the common node approach. In the paper by De Rooij (2013) it is explained that the model uses a dual node approach. But the common node approach is not described. Either I missed something or this should be detailed somewhere so that the reader can have all the needed information. Finally, for some test cases the difference in the number of Newton iteration is rather limited when using a proper discretization and coupling length making it difficult to say in a general way that the dual node approach is more efficient than the common node approach.

(4) Regarding the efficiency, I also believe that the tighter the coupling, the more difficult the resolution will be. Considering the experience I have in the domain, it is much harder to impose continuity through a common node type of approach than to impose a first order coupling through a dual node approach (if the numerical resolution is the same). As a consequence, it is for me logical that convergence is harder to obtain for some test cases with the common node approach.

(5) The paper is quite clear but some parts are too long. This makes the paper sometimes hard to read. Part 4 is an example. This part is very long and the first conclusions are deceiving – i.e the proper implementation has already been proposed by other (Line 240) and the proposition of a numerical trick to properly implement dual node in vertex-centered scheme (line 256 to 259). Maybe this can be improved.

(6) The part that presents the results is also hard to follow. I believe that there are too many test cases presented and that all of them are not needed. The saturation excess test cases may be removed as they are only illustrative for the efficiency. Maybe only the infiltration excess should be kept as it is for this process that the added-value of the method proposed is the most important. The consequence of multiple test cases per hydrological processes is that the reader has to jump from one figure to another which is not convenient at all. The number of figure presenting the results is also quite high.

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(7) Regarding hydrological processes, it seems that the differences between both approaches are very small when dealing with the saturation excess process, which is the dominant process of streamflow generation in most temperate region. The main problems/conclusions are linked to the infiltration excess process. The findings for both processes are rather limited as (i) for saturation excess both approaches are OK and (ii) it is well-known that using the Richards equation infiltration excess cannot be properly capture with a 20 cm or a 50 cm resolution.

(8) The coupling between surface and subsurface strongly depends on the numerical schemes use for resolution. This point is clear on the paper (especially through the explanations related to figure 1) but the paper – although using 2 different schemes – is not exhaustive. Some published models using other resolution schemes are built using a properly implemented dual node approaches and this point should be fairly mentioned somewhere.

(9) I am a bit uneasy with the concepts of elegance and generality when considering physically-based modelling. In my opinion, the main question is whether the modelling approach chosen allows for a proper description of the physics considered. I believe that it is an endless debate to determine which approach is the more elegant or the more general and I would suggest the author to remove the sentences related to that and focus on the accuracy and/or the efficiency that are can be somehow measured.

Other comments:

- Some parts of the paper are only about interpretation and as a consequence are very subjective. See for instance from line 274 to line 283.
- Line 45: hillslopes not hill slopes
- Line 50: the reference paper for CATHY is rather Camporese et al, WRR, 2010 than Weill et al, AWR, 2011.
- Line 60: the interface is not always saturated. Its property is constant but saying that

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it is always saturated can be misunderstood regarding the infiltration process.

-From line 191 to line196: this part is not clear and needs to be improved. To my knowledge and in most of the integrated models mentioned in the paper, when a cell is not ponded, all the rainfall infiltrates. When the cell is ponded or partially ponded, infiltration occurs under the ponded area. I agree that infiltration under the non-ponded fraction of a partially ponded area should be theoretically accounted for, but the sentences in the paper could lead to misunderstandings.

- Line 223: I don't understand why it is mentioned here that the surface head can be used as a Dirichlet boundary condition. I agree that it can be done but not in the context of a coupling through a dual node approach. Maybe this is linked to the implementation of the common node approach.

- Line 326: typo - Figure 1c

- Line 365-368: Repetition of things already said from line 274 to 283

- Line 395-397: I quickly checked in de Rooij et al (2013) and this paper only describe the dual node approach for coupling. Some results with the common node approach are presented later in the paper. The way the common node approach is implemented should be presented somewhere.

-Line 464 to 478: this part does not bring anything to what is already well known and described in the literature. Just say that the reference is computed using a fine resolution.

- Line 498-500: Please explain before in the paper how the inconsistent dual node approach was implemented.

- It is strange that figure 2 d and 4d shows so different results. We would expect that the behavior between different coupling approach/resolution provides same trends regarding the reference and it's not the case. Can you explain?

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- Test cases with excess infiltration: even though the dual node approach displays “more desirable behavior” (line 521), the results with coarse discretizations are far from the reference. Meaning that a consistent implementation of the dual node approach is not sufficient enough if the resolution is not well chosen.
- Figure 10 c and 10 d: it is hard to say who the best is between the common node and the dual node. Needs to be discussed.
- Figure 13: why is there so much difference for this test case only? When the discharge are so close and match pretty well, the efficiency seems very different between the coupling approaches.
- Line 538-539 (excess infiltration): all the simulations are far from the reference. The argument presented in this sentence is not valid in my opinion.
- Line 553: typo “underestimates or overestimates”
- Line 671: Figure 9 not 10
- Line 635: Figure 6 not 7

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