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Geoscientific Model Development Discussions

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Interactive comment on "BioRT-Flux-PIHM v1.0: a watershed biogeochemical reactive transport model" *by* Wei Zhi et al.

Anonymous Referee #1

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Dear authors, I am rather torn on this paper. On one hand, I strongly believe some of the results of the paper are significant and interesting and without a doubt worth publishing. On the other hand, I am fairly disappointed with the presentation of the manuscript.

Section 3 is poorly written. A lot of terms are not defined, and it seems that there are some inconsistencies. Notations are poor and make the equations hard to read. The structure of the paper is somehow chaotic. Section 5 includes a series of description of new processes, definition of parameters, rates, ... The most important associated issue is the fact that I do not see what motivated the choice of the content of section 3. The hydrology part could be summed up by one or two equations and adequate references (it's basically Darcy and Richards equation on which multiple pages fo-

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Discussion paper

cus). The reactive transport equation is fairly basic as well. And somehow, these fairly simple concepts are presented in a confusing way and most of the terms are poorly defined, and a lot of equations are redundant. I feel that that section could almost be entirely removed with adequate citations. On the other hand it seems that a significant proportion of the methods are lacking description (and are included within section 5). Another problem to me is that it seems that some key processes (evapotransporation) are barely discussed in the modelling part), despite some modelling results showing a comparison between ET model and data.

This work is obviously built within a collaborative effort but the paper fails to present what was actually done within this research. A good example is how all the complexity from figures 1, 2 and 4 are completely ignored within the model description.

A last important issue regards how the objectives are stated. Reading the title, abstract and introduction and even the equation section really gives the feeling it is a modelling/numerical paper, while the results section goes into important and interesting details about these coupled multi-phyiscs dynamics and predictive applications for large-scale field data. I feel that the first half of the paper does not constitute a proper description of what is coming next.

There are multiple good things about this very relevant work: - model results are interesting - model verification are good - interesting modelling approach - multi-scale multi-physics problem, - ....

and i'm confident this is worthy of a very nice publication. However, this paper, as is, is a poor representation of the work which was performed. In my opinion, an important work of structure has to be done. Considering how rich the results section is, i would maybe suggest to emphasize these very interesting features of the work as the main part of the article. I am sorry if my review sounds very negative. I was somehow upset by the quality disparity between the work itself and the paper writing. I guess it's better in this way.

In the attached files, you can find some detailed line by line comments. These are comments which occur to me "as i am reading", please forgive the tone within that text file. Hopefully, this can help you.

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2020-157/gmd-2020-157-RC1-supplement.pdf

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