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Geoscientific Model Development Discussions

Interactive comment on "GSFLOW-GRASS v1.0.0: GIS-enabled hydrologic modeling of coupled groundwater–surface-water systems" *by* G.-H. Crystal Ng et al.

Anonymous Referee #1

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General comments: The manuscript presents the development of a suite of tools for preparing the input, submitting the simulation runs, and visualizing the output of the groundwater-surface water coupled GSFLOW model. The proposed suite of tools is developed exploiting the functionalities of the open-source GIS software GRASS and ad-hoc Python scripting. Authors tested the developed toolkit presenting test cases based on three catchments having different physiographic features. The manuscript is generally well written and with a logical and easy-to-follow structure.

While I concur with the authors on the potential of such kind of efforts to encourage the use of complex surface-subsurface coupled hydrological models, I question the actual

novelty and technical advancements presented in their work. Besides a suite of GIS extensions and scripts, the manuscript does not propose new technical solutions for the problem at-hand. For this reason, and for those elaborated below, I consider this contribution not suitable for GMD standard.

Specific comments:

1. In presenting/justifying their work, I think authors overlooked a bit too much the key technical issues preventing the widespread use of complex, physically based surface-subsurface coupled hydrological models in a decision-making framework. Here, I would argue that preparing the input is certainly a necessary and important step in the modeling exercise but not the most challenging one. In fact, if we agree that computationally efficient and numerically stable codes are needed to "promote science-driven decision making" then ad-hoc tools allowing a dynamic (e.g. in-situ visualization) inspection of such physical and numerical model response are probably much more needed, especially when we approach big-data problems. Saying that, I do not see the positioning of the effort presented in this manuscript with respect to these grand challenging tasks.

2. The outcome of the presented developments is clearly reflected in the results section. Here, authors describe three test cases illustrating the physical settings of each study area and discussing the potential outcome of a surface-subsurface coupled modeling approach. However, these results appear the repetition of the same exercise without much insight on the novelty of the proposed approach. For instance one could argue that such kind of plots can be simply obtained with some visualization scripts developed from scratch.

3. In a similar vein to the previous point, at the end of the introduction authors argue that the developments of such automated toolkit will enable rigorous testing. Absolutely true but a concrete path forward and tangible results are not presented in this context. Wouldn't it be an interesting way to demonstrate the utility of such kind of tools?

4. In several parts of the manuscript, authors refer to a similar work, i.e., Gardner et

al., which is currently under review for another journal. As the content of the cited work cannot be evaluated, these statements are unverifiable by the reader/reviewer, which is obviously not acceptable. Moreover, considering the potential overlap between the two contributions, as also acknowledged by the authors, it is not possible to weight the actual contribution of this work. For instance, one may ask if moving from ArcGis to GRASS or using ungridded versus gridded data would be enough to motivate an additional publication.

5. It appears that for some of the most critical parameters (e.g., Manning's parameter) authors present their approach referring to homogeneous values. In so doing, they advocate that field data on channel geometries come in a variety of forms difficult to accommodate in a generalized approach. Wouldn't it be the motivating reason for such geoscientific developments as the one presented here? Data fusion tools are in my opinion the key for facilitating the coherent ingestion of large source of information into a distributed model input data structure. An example along this line is represented by the work of Leonard and Duffy, 2013.

References:

Lorne Leonard, Christopher J. Duffy, Essential Terrestrial Variable data workflows for distributed water resources modeling, Environmental Modelling & Software,50, 85-96, doi: 10.1016/j.envsoft.2013.09.003, 2013.

Technical corrections:

1. Authors argue that models using triangulated irregular networks show better water balance performance over steep catchments. This is a quite interesting statement but ad-hoc citation is needed to substantiate this.

2. According to the author's opinion, PRMS does not implement Richards equation but instead applies an 'efficient' calculation to determine input and output for HRU. What's the meaning of 'efficient' here?

3. I do not see the precipitation lines in Figure 5-6-7.

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