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Discussions

Interactive comment on "HESS Opinions: Improving the evaluation of groundwater representation in continental to global scale models" *by* Tom Gleeson et al.

Anonymous Referee #3

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I am not completely sure about the work. First, it is an "opinion paper", and this is exactly what it is, a recollection of the thoughts of very relevant well-known researchers that gathered together and, after a discussion, put together some thoughts. The problem is that these thoughts are controversial and I would say that many of us (hydrogeologists) would not share them.

So, the point I want to make is asking what is the significance of the work. The problem of including groundwater in global models lies on the problem in scales, mostly the temporal ones. This is probably why the authors talk about geological eras, but do not talk about geology. Temporal dynamics are a benefit or a curse, as they run on different

timescales that are strongly related to spatial scales of models. But also, the amount of information that should be included in the models is size and purpose dependent. And this is also well known. Even the processes that should be considered depend on the size and purpose of the model. Processes that are inherently non-linear at the local scale can be linearized at the regional scale and maybe take as constant (or disregarded) in a global model; a clear example is GRACE, where groundwater is treated in a way that would be considered shameful for all groundwater modelers, but that is capable of producing some quite good results at the scale of a full country (obviously difficult to be used in predictive models). This can hardly be considered the core of a new contribution.

And then, for each problem we can use the simplest model (or approach) we can think of, but this depends exclusively on what do we want the model for, as correctly pointed out by the authors. Expert opinion models are fantastic when you have zero data and thus just to get an idea of how to manage resources at some global scale (what the authors call a sustainability-focused purpose), or how to design a global network. But this cannot help you at the local scale (designing a new well). It is the same with water quality, that can be inferred from general geogenic conditions, but again cannot be used to assess the water quality at a point. But my question here, what is new about this that deserves being published? The authors do not talk about water resources quality at all, and this is a key point in global management.

I like the sentence "all three strategies (observation-based, model-based, expert-based) should be pursued simultaneously because the strengths of one strategy might further improve others". I fully disagree; more, I do not see any other way of modeling that is not based on: You check all available data, you postulate some potential models using expert opinion and all existing data, and you build the model, including or not uncertainty and model selection criteria, but always based on calibration and model selection criteria. So, I do not sympathize with this idea of having 3 connected but separated strategies. In my opinion they are three faces of the same strategy: do the best you can with what you have and with your client needs.

Anyway, to summarize, the paper is perfectly fine. It is the contribution of excellent researchers that have their personal views based mainly on their own work. Maybe the contribution will be useful for young researchers, despite some comments/ideas are questionable. I do not recommend publication, because I do not see the point, but if the Editor considers that it should be published, it will be “in present form”.

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