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

**Tom Gleeson et al.**

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RC3 - Anonymous Referee #3 received and published 8 September 2020: 'a well-written text and that's it'

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.

Thanks for asking this question of the significance. We argue that the significance is providing clear recommendations for the quest to improve the evaluation of large-scale models which are currently the only "coherent scientific framework to examine the dynamic interactions between the Earth System above and below the land surface, and are compelling tools for conveying the opportunities and limits of groundwater resources to people so that they can better manage the regions they live in, and better understand the world around them." (Section 1)

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.

As mentioned above, we were remiss in not focusing more on geology - we have added this to Section 1. We agree that temporal dynamics are related to the 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?

We generally concur with the above statements of the referee. What is new here are at least three elements: âŮŔ is that never before have the limitations of current evaluation practices for large-scale models (Section 2.3) been distilled; âŮŔ nor have a compelling vision for how to holistically improve the evaluation of groundwater represented in large-scale models been proposed (Section 3); and âŮŔ finally, our two next steps (Section 4) are forward looking and will hopefully be a useful nudge to multiple scientific communities. Together, these reasons suggest this is a good, worthwhile and timely contribution that deserves being published.

The authors do not talk about water resources quality at all, and this is a key point in global management. Yes, this is true. In Section 1 we have added: "It is important to note that we do not consider water quality or contamination since large-scale water quality models are in their infancy (van Vliet et al., 2019) even though water quality or contamination is important for water resources, management and sustainability." 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.

We understand that RC3 disagrees with our language of '3 connected but separate strategies' and would rather consider them 'three faces of the same strategy'. We acknowledge that our text in Section 3 and 4 (and Figure 1) gave the impression of three separate strategies even though we did write that the three strategies are "potentially

mutually beneficial" and later that they "should be pursued simultaneously because the strengths of one strategy might further improve others. For example, expert- or model-based evaluation may highlight and motivate the need for new observations in certain regions or at new resolutions. Or observation-based model evaluation could highlight and motivate further model development or lead to refined or additional hypotheses." After reading this critique we realised that we needed to clarify that we are not proposing a brand new evaluation method here but separating strategies to consider the problem of large-scale model evaluation from different but highly interconnected perspectives (or 'faces' as RC3 mentions above). All three strategies work together for the common goal of 'improved model large-scale model evaluation' which is what is at the top of the pillars in Figure 1.

Therefore we added this text to the beginning of Section 3: "We are not proposing a brand new evaluation method here but rather separating strategies to consider the problem of large-scale model evaluation from different but highly interconnected perspectives. All three strategies work together for the common goal of 'improved model large-scale model evaluation' which is what is at the top of the pillars in Figure 1."

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 being 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".

Thank you for summarizing that the paper is perfectly fine. We have done our best effort to improve or clarify that questionable comments or ideas. We hope our arguments above the referee would see the point of publishing the manuscript, especially with all the improvements from it's previous 'present form'.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-378/hess-2020-378-AC2-

supplement.pdf

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