

Interactive comment on “Large-scale sensitivities of groundwater and surface water to groundwater withdrawal” by Marc F. P. Bierkens et al.

Anonymous Referee #3

Received and published: 7 February 2021

Bierkens et al. present a simplified analytical methodology for first order approximation of the impacts of groundwater pumping on streamflow and ultimately groundwater sustainability. In addition to summarizing the methodology they provide global mappings of streamflow depletions and sustainable pumping limits. While I appreciate that the authors were very clear throughout the manuscript that this methodology is intended to be approximate, I still have very significant concerns and I don't feel that the manuscript in its current form has demonstrated that this approach is adequate to support the types of groundwater sustainability findings that are presented in figures 7-9 for the following reasons:

1. The approach presented here relies on a myriad of simplifying assumptions. While the authors do try to be very transparent in these assumptions, this does not make

C1

them less concerning. Specifically, the steady state approach and the distributed well locations are big areas of concern in my opinion. For large scale aggregated analyses of declines this might be okay but for stream aquifer interactions well placement and timing is very important. The key advance of this paper is groundwater surface water interactions and therefore I think the bar is higher for some of these assumptions.

2. The authors present a sensitivity analysis for their approach which is a helpful illustration of the relationship between variables. However, for me this really only demonstrates that the general interactions are in the correct direction, which follows directly from the equations they used. Much more concerning to me is the uncertainty of the inputs to these equations at the spatial scales presented here and whether reliable estimates for some of the parameters can be generated at all. For example, how accurately can bed slope and bed elevation be captured at this resolution globally? How sensitive are the final results to the uncertainty in these values?

3. My biggest concern here is that the most important metrics that the authors are highlighting in their findings are not well validated. The authors present primarily comparisons to other global models which rely on similar assumptions and are working at similar spatial resolutions. It seems like it should be expected that the results here would be 'remarkably similar' (line 315-318). Before jumping to a global analysis I would like to see some rigorous evaluation of the model in some of the many heavily studied aquifers across the world comparing to regional models and observations. For example, observational groundwater ranges are reported for a few aquifers (Lines 379-382) but the authors only note that 'our estimates are in the lower end of those observed ranges' I think a much more quantitative comparison is need here.

4. Furthermore the validation that is provided here is really focused on groundwater depletions and I think the validation of the stream aquifer interactions or sustainable limits to groundwater withdrawals (the highlight of the paper) is lacking. If the main purpose of this work is to get to sustainability estimates and to connect to streamflow then these are the parts of the methodology which must be most thoroughly evaluated. I realize

C2

that this information is not available globally (hence the novelty of this work). However, I don't see any reason why these behaviors cannot be rigorously and quantitatively evaluated in some example locations for which data or models are available.

5. Finally, the sustainability language in this paper is concerning to me. First of all because sustainability is a very subjective topic and it's not clear that the first order type approximations used here can really get at true sustainability. Second of all because I think these results can easily be misinterpreted based on how they are presented here. The authors do try to specify that this approach is only for first order approximations, but if that is the goal here then I think they should focus on using this methodology to provide ranges of potential groundwater depletions and stream interactions, and not be using this to present things link groundwater limits which can very easily be misinterpreted and miss-used.

Overall, I do think this is a well written paper that is clearly presented and easy to follow. Unfortunately, I am not convinced about the validity of the approach, and as a result the findings that are presented. I think a much more rigorous evaluation of the methodology is needed including quantitative analysis of every metric that is going to be presented in the findings. I completely understand that this methodology is intended to be approximate and will not perform as well as regional integrated models or intensive observational studies. However, I think these should still be the bar for comparison so that users of this approach can fully understand its strengths and weaknesses of the simplified approach, and so that any metrics that are too uncertain are not included.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-632>, 2020.