Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-632-RC1, 2021


Hydrology and Earth System Sciences Discussions
Open Access



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

**Anonymous Referee #1**

Received and published: 1 February 2021

—- Summary —- This study investigates the sensitivity of a linear reservoir-based model for groundwater pumping from unconfined aquifers and streamflow depletion, as well as applies this model for discretized cells of ∼100 km2 globally. Steady-state hydrologic parameters are input into the model with the outputs tracking the changing groundwater and surface water heads, presumably only considering a single model cell. A definition of sustainability is applied to the conceptual framework to explore the global spatial distribution of various model outputs and metrics.

—- Comments —-

1. I strongly disagree with the definition, and connected implications, of physical sustainability in this manuscript. According to the definition that groundwater pumping is sustainable so long as it doesn't cause the water table to disconnect from a sur-



Printer-friendly version

Discussion paper

face waterbody is extreme. This definition means that nearly the entire flow of the river (i.e., Qi) could be extracted from the groundwater pumping and still be considered sustainable. A similar argument could be made that any streamflow lost due to groundwater pumping would be not sustainable, but the opposite is not necessarily the definition of a sustainable pumping regime (physical or otherwise). A dry or reduced flow river is not emblematic of sustainable abstractions in my opinion. Figure 1b can represent a physically unsustainable system, as the lost streamflow could lead to negative environmental effects downstream and could also cause feedbacks with downstream groundwater-surface water interactions. It would be fair to state that qcrit in this analysis is indicative of certain unsustainable hydrologic conditions, but it is not the threshold between sustainable and unstainable in either this conceptual framework or the real world. The opportunistic simplification of "capture" in this study is not complete, and the water budget and simplicity of the approach do not address capture in a sufficiently meaningful way to allow the application at the global scale to inform pumping management plans. As a somewhat connected note on this topic, the study does not need any definition or use of sustainability. If the study were instead posed on the potential disconnection of groundwater from surface water, then there would be no need for the value-loaded aspect of sustainability definitions. The "critical" outputs could be relabeled as "disconnection" or extreme flow reversal outputs. 2. What are the hydrologic restrictions of the constant hydrologic inputs? Importantly, it appears that the streamflow velocity remains constant while the depth and discharge can change. This suggests that the Q was not connected between cells, such that the pumping analysis was only providing information for each cell individually, such that Qi is constant and unaffected by pumping. This was not stated clearly in the text. This is important for then later calculations of depletion, comparisons with observational data (i.e., GRACE depletion rates), the delineation of "sustainable" vs "unsustainable" areas or watersheds, and the "global limit to sustainable gw pumping". These calculations represent nearly all of the location-specific results, and the lack of hydrologic connectivity is especially concerning for the calculation of qeco (Eq 4). 3. This study needs





Printer-friendly version

Discussion paper

to connect more clearly with the Zipper et al. (2019) paper rather than an offhanded statement on “a single well-network” method. This study is also applying a one well-one stream methodology that fits within the levels of complexity tested by Zipper et al. Treating the aquifer as an infinitely deep linear reservoir with uniform drawdown is less informative when applied to real locations (i.e., in the spatial analysis in this study) than the analytical approaches in Zipper et al. The distance a pumping well is from a stream is critical to calculating the streamflow and aquifer depletion, and the Zipper paper certainly serves as a foundation for global hydrologic studies that already have basically all of the information needed. Similarly, superposition was not mentioned in this study, but it could surely provide a very simple but powerful tool for calculating more realistic drawdowns. Forcing all drawdown across the model cell to be equal with this conceptualization also sets a very optimistic limit for what is being inappropriately labeled critical metrics for “sustainability”. 4. Also of concern, relating to the Zipper paper, is the rather haphazard definition of the interaction term in this study, F. It includes the streambed conductance, a very difficult to constrain and important parameter, while also adding other geometries. If depletion is “often highly heterogeneous and incorrect estimates can lead to errors in estimated streamflow depletion (Fleckenstein et al., 2006; Irvine et al., 2012; Lackey et al., 2015)”, as stated in Zipper et al., then I have a lot of trouble trusting the two versions of F (and J) used in this study, as neither sources were meant to provide such information on streambed connectivity to an aquifer. As such, the two sets of maps are pretty samples from an unknown distribution with unknown uncertainty. Also, the maps only show the actual values and never provide any information on the relative similarity/dissimilarity of the two calculations (other than being “striking”, but not explained which is more realistic). Subtracting the two datasets and providing a map and histogram would give a sense of how important the unknown response time input for J and F is. These inherently include a length that may be inconsistent with the way this study was discretized. This again makes me question the utility of the many global outputs in this study 5. More information on the input datasets would be useful. For example, a description of the dataset used to apply “realistic” pumping rates for the unconfined aquifers needs to be at least stated rather than requiring the reader to track it down elsewhere. The validity of these pumping rates sets the validity of all of the spatial results. Uncertainty in these pumping rates and resulting uncertainty in the results would also be useful, as the focus on mapped outputs implies the targeted impact of the global analysis is site-specific rather than global. 6. The connection between the PCRGLOB-WB (2) model needs to be stated in the beginning rather than in the discussion. The differences and novelty of this study needs to be presented at the beginning with the full context, rather than stating the similarity between this analysis and the previous modeling work "is not as surprising as it seems". The differences need to be VERY CLEARLY presented. Along this reasoning, the comparison of the depletion rates between this study and the former work needs to be more detailed. How many of the inputs between the models were different? How many of the equations? Are the integrated depletion rates for the globe smoothing over larger differences? 7. The comparison with GRACE data needs further development. How were the averages of depletion upscaled for these aquifers and some identification of the target areas would be useful? What are the unlabeled dots in Fig 5? What areas do they represent? What do the large misfits between the depletion rates, especially for the low rates from this study, indicate about the model performance and limitations? The issue of total water storage changes and an infinitely thick unconfined aquifer could be discussed in more detail. 8. The focus of the discussion of uncertainty on confining conditions is not all-encompassing, nor does it even assuage my concerns on the way the aquifer system was developed. Insufficient description of the various geometries and model inputs make it difficult to fully question the role of confined vs unconfined aquifers. An infinite depth unconfined aquifer system as the domain with an area the size of the grid cell is somewhat clear. Are the pumping rates only for the unconfined aquifer? If so, then why compare to GRACE TWS, as those are heavily tied to confined aquifer pumping in many areas? Justifications are lacking and explorations of the uncertainty of the effect of unconfined aquifer with infinite depth/storage on the results is missing from the analysis. 9. "...likely the simplest analytical form that can be devised" is amazingly pompous and immediately false. Bragging at its finest. (Line 448). 10. The definition of F is different between Figure 2 and Equation 2. Reversing the inequality with a negative sign in Eq 2 results in problems. Figure 2 appears to be the correct definition, where negative F represents streamflow depletion and positive as baseflow. With Eq 2, h > hs leads to –F whereas h<hs leads to +F. In Eq 3, it appears that +F should lead to more streamflow, such that Fig 2 has the correct definition of F. A statement that +F is inflow into the surface water or something to that effect could help the reader follow this definition. Fig 2 should match the equations in the text and be consistent with the rest of the math. Similarly, some variables in Table 2 are capitalized when they are not in the text. 11. Numerous typos and misspellings throughout the paper. Lines 65, 68, 85, 101, 139, 149, 263, 283 (? or are tenths of years impressive?), 347, 388, 486, 717. 12. Ln 299 – inflow is flow in or out of the stream? Unclear here and elsewhere as this depends on perspective (towards surface water or towards groundwater?). 13. Ln 277 – Eq A30 mainly states that these fluxes negate each other, but the relationship of the ratio of these components is not known as q appears in this equation twice, unless additional assumptions are made (i.e., the ratio of the non-q components are equal to zero). 14. Ln 806 – distance, not difference 15. Ln 812 – it can also be set to other elevations, such as is implied in this study where pumpable groundwater exists below the streambed elevation. 16. All map figures are clipped to middle latitudes in the pdf I reviewed. I am unsure if this was intentional or not, but it seems arbitrary given the global extent of the analysis. 17. Separately on Qi, depending on the size of the watershed/catchment of interest, it seems strange to attribute the need for these to mountainous areas. Zero-order watersheds seem to also be depicted in Fig A1, which is absolutely not expected.

---

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