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Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "On the contribution of groundwater storage to interannual streamflow anomalies in the Colorado River basin" *by* E. A. Rosenberg et al.

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I. General Comments:

"This paper presents interesting work and is well put-together. The research seems well-conceived for the most part, and very well-executed. Presentation of the results is also clear to follow and maps well to the ultimate conclusions."

Authors: We very much appreciate the reviewer's comments.

"I have a couple minor concerns. One is about the authors' experimental design. I

wish the authors had chosen a basin where it might be more reasonable to expect more substantial groundwater interaction with surface water. My expectation for the Colorado basin is that streamflow interaction with soil moisture and shallow groundwater would be most significant in snowmelt-dominated headwater sub-basins and during the winter through snowmelt season; for the rest of the calendar and the rest of the basin, I'd expect groundwater to be largely disconnected from the streamflow generation process. Their results don't confirm my suspicions, but they do land on the view that groundwater interaction with surface water is not that important for streamflow predictability in the basin. However, I'm left wondering what if the authors had chosen the less-arid basins like the Columbia or the Missouri. I think application of their procedure to those types of basins would have shown more groundwater interaction with streamflow and perhaps revealed more interesting results. (If I'm missing some key points on why investigation of a less-arid basin doesn't bear more merit than investigating the Colorado, it would be good for the authors to explain.)"

Authors: While we agree that one might reasonably expect groundwater to be largely disconnected from streamflow in the Colorado, we argue that the issue of groundwater's influence on interannual Colorado streamflow anomalies was still, prior to this study, an open question. The Colorado has been the subject of numerous studies involving climate change, yet, as stated in Sect. 1, modeled projections of Colorado River streamflow have been remarkably variable (an issue that is discussed in detail by a BAMS paper by Vano et al. – draft available at www.hydro.washington.edu). This suggests a misunderstanding of catchment processes, and some in the hydrology community have proposed that groundwater, which is typically not represented in macroscale hydrologic models, acts as a modulating influence on annual streamflow volumes and therefore might explain some of these disparities. On an anecdotal basis, there were claims that groundwater depletion was a cause of large under-forecasts of basin runoff in water years 2002 and 2005, for instance. Furthermore, because of the grim picture painted by some of the climate change studies (one of which predicted a 45% reduction in streamflow by 2050) and the need for water managers to establish a cogent basis for water supply planning, we argue that the Colorado is a particularly appropriate study basin.

“Another minor concern is that while the conclusions are well-supported, I wish they might have been more substantial in the context of views that I perceive to be held by the Colorado Basin hydrologic forecasting and water management communities – namely that groundwater interaction with surface water is not a significant factor modulating seasonal to annual streamflow predictability. In other words, the conclusions of this paper seem to be reinforcing previously held views, which is useful to know but not really substantial in nature.”

Authors: As noted above, there has in fact been some question among these communities as to the role of groundwater storage at seasonal and interannual time scales. We feel that the conclusions of our study are important in that they rule out this element as a modulating factor of streamflow forecast anomalies.

II. Specific Comments:

“Section 2 discussion of aquifer systems – suggest focusing reader attention only to aquifer systems that stand a chance of interacting with streamflow, and not focus on deeper, disconnected aquifers.”

Authors: Please note that the aquifers mentioned in Sect. 2 are all surficial aquifers, and the areas where each aquifer is the uppermost water yielding unit are shown in Fig. 1.

“Section 3.1 – I’m wondering whether the WTD simulation arrived at a spatial WTD distribution that is representative of actual WTD at the start of the analysis period. Did you explore how findings were sensitive to this WTD spin-up procedure?”

Authors: We did explore the sensitivity of our simulations to WTD, and found them to be largely insensitive. The most likely reason is that those grid cells that contribute the most runoff, and experience the largest changes in hydrologic storage, also have

the shallowest WTDs, and hence spin up relatively quickly. On the other hand, those grid cells with the deepest WTDs, which are most affected by the spin-up procedure, contribute the least runoff and experience the smallest changes in hydrologic storage. Thus, the spin up procedure had little overall impact on the results, and we have now noted this detail in Sect. 4.1.

With regards to how representative simulated WTDs were of actual WTDs, please refer to the response provided to the comment below.

“Section 3.1, Figure 2 – I notice from the right panel that WTD is only several meters for many arid locations in the middle to lower basin... does this make sense?”

Authors: This raises a good point that merits further discussion in the text. Simulation of WTD is complicated by the relatively coarse spatial resolution of our model implementation (1/8-degree, or roughly 10–15 km). Because the model does not account for subgrid variability in topography, it also neglects subgrid variability in groundwater recharge. Studies have shown that when the spatial variability of topography is increased, enhanced interactions between the water table and land surface reduce flow paths and increase subsurface flows, serving to deepen water tables (Huang et al., 2008). Thus, it is possible that this lack of subgrid topographic variability is resulting in a shallow bias for the WTDs. We have now commented on this point in Sect. 3.1 and added Huang et al. (2008) to the list of references.

It should also be noted that Niu et al. (2007), in the paper introducing SIMGM, only indirectly validated modeled WTDs through comparisons between globally modeled and observed runoff. Because runoff is mainly dependent on WTD in SIMGM, they viewed the agreement between these two datasets as evidence of the model’s ability to simulate the global water table. They further noted, however, that this agreement did not speak to the comparability of modeled WTDs to well observations, which can display great spatial variability over model scales. Apparent inconsistencies such as the one noted by the reviewer are possible, and we have now added an explanation to this effect to Sect. 3.1 as well.

“Section 3.3 – How does GRACE handle changes in surface storage contents (Lake Powell, Lake Mead, etc)?”

Authors: GRACE data reflect total change in terrestrial water storage, including reservoir and other surface water storage. According to data for Lakes Mead and Powell (which account for about 85% of the basin’s total reservoir storage capacity), we conservatively estimate maximum interannual deviations in basin-wide reservoir storage at about 10–15 mm, or about 10–20% of the maximum interannual storage change observed by GRACE. Given that most of the reservoir storage change occurs in the Lower Basin, while most changes in hydrologic storage occur in the Upper Basin, the effect of reservoirs is likely negligible in our area of interest, and we have now noted this point in Sect. 4.2.

“Section 4.4, Figure 10 (top row), and interpretation statement on p. 13209: "annual streamflow volumes appear to bear little relation to interannual hydrologic storage". I realize that simulation results as shown on Figure 10 support this statement, but why do you accept this result as plausible? What’s a physical explanation for this? One might expect that in positive-anomaly runoff years, it was likely that we had positive anomaly precipitation years, which should partition into positive anomalies for runoff and recharge. However, your results don’t support this. Please offer physically-based explanations on why this might be the outcome.”

Authors: The most likely explanation for this result is that the storage conditions at the beginning of the water year are constraining the subsequent changes in storage that are possible. So, using the reviewer’s example, it is indeed likely that positive-anomaly runoff years are the result of positive-anomaly precipitation, which we agree should partition into positive anomalies for surface runoff and groundwater recharge. However, there is a limit to the height of the water table, and if the water table at the beginning of the water year is already high, the higher value of groundwater recharge

will simply result in a higher value of groundwater discharge rather than an increase in groundwater storage. Only if the water table at the beginning of the water year is low will the higher value of groundwater recharge result in an increase in storage. This explanation is substantiated by plot (b) in Fig. 10, which shows a relatively strong correlation between October 1 storage anomalies and water year storage change. We have now elaborated on this explanation in Sect. 5.

“Your analysis is based on an evaluation during a relatively dry decade in the Colorado River Basin. Suggest commenting on how evaluation of a "wet decade" might have affected your results and conclusions.”

Authors: For the parts of the analysis that were based on the 2000–2010 decade, we would not expect an analysis of a wet decade to yield substantially different results. Our reasoning is that, in either case, changes in hydrologic storage are likely to be primarily dependent on initial storage conditions. For a dry decade, initial conditions are consistently low, and even low values of recharge are unlikely to lower them further. For a wet decade, initial conditions are consistently high, and so (as described in the response above) high values of recharge are unlikely to raise them. On the other hand, we might expect the results to be different for a decade with greater interannual variability (i.e., wet years interspersed with dry years). In that case, fluctuations in storage are more likely to correspond to the degree of water year runoff, which may lead to the conclusion that groundwater storage provides a greater contribution to interannual streamflow anomalies. We have now included a discussion of this issue in Sect. 5.

Additional References:

Huang, M., Liang, X., and Leung, L. R.: A generalized subsurface flow parameterization considering subgrid spatial variability of recharge and topography, J. Hydrometeorol., 9, 1151–1171, 2008.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13191, 2012.