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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.

Anonymous Referee #1

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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.

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 groundwa-

ter 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.)

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.

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.

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 senstive to this WTD spin-up procedure?

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?

Sectoin 3.3 - How does GRACE handle changes in surface storage contents (Lake Powell, Lake Mead, etc)?

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.

Your analysis is based on an evaulation 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.

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