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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Contrasts between chemical and physical estimates of baseflow help discern multiple sources of water contributing to rivers" *by* I. Cartwright et al.

Anonymous Referee #2

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While the paper is interesting and reasonably well presented, I am not totally convinced that it adds a great deal to hydrological science understanding. It has been knw for a long time that mathematical baseflow separation approaches are not easily able to distinguish between processes contributing to low flows in rivers. I accept that this paper has the potential to contribute to the quantitative understanding of these differences and perhaps that is enough to make it a sufficiently valid contribution, but I am not totally convinced. Regardless of this point, there are a number of questions that I think the authors need to address before the paper can be accepted. I was not sure whether to identify these as major or minor revisions as they are somewhere between.

I am not sure that the authors identify all of the possible processes contributing to low flows, but there is not really enough information provided about the catchment (slopes, soils, etc.) to get an idea whether other processes are likely to be present or not. It would also be useful to have some idea of why the groundwater is so highly saline, when the surface water salinity is expected to be very low. On the issue of salinity observations, why are some given as both TDS and EC - this is confusing and it would better to use the same units throughout, even if some approximate conversions are necessary. I would also like to see more consistency in the wau in which numbers are presented - 3500 and 13,000 and 8.1x10^3, etc.

The paragraph at the end of section 3 is more detailed than it needs to be. It would be better to provide a brief summary of the flow and TDS variations instread of repeating the numbers that are clear from figures 2 to 6.

As far as I can see, the surface runoff TDS is based on the rainfall and therefore it is assumed that no salinity is added during the surface runoff process - is this a valid assumption and can it be justified? The authors look at the hysteresis effects and relate it to bank storage, but perhaps it is also necessary to consider the effects of surface runoff attenuation effects in the channels, particularly if the surface runoff is generated in the headwater areas. Will this effect influence the calculation of likely baseflow volumes. In addition, estimates are made of bank storage, but are these reasonable and can they be related to river lengths and likely bank storage availability? The conclusions suggest that banl flow and floodplain storage will have a geochemistry that is similar to surface water, but is this likely if the soils are also saline? However, this information is not supplied and this issue also relates to the source of high salinity in the groundwater.

Figs 2 to 4 show very large fluctuations in the GW contributions based on the CMB approach, but I did not find a great deal of discussion of this result and whether it can be physically justified in terms of time series variations in the hydraulic head of the groundwater feeding the river.

Overall, the conclusions and analysis of the results focus on the differences between the CMB approach and the different digital filter approaches. It would strengthen the paper if the focus shifted towards more verification of the CMB results. After all, most people would expect there to be differences between the digital filter approaches and more physical methods - we have know this for a long time - the important thing is whether relatively simple physical approaches can identify the sources of water accurately.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5943, 2013.