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Hydrology and Earth System Sciences
Discussions



# *Interactive comment on* "Can the two-parameter recursive digital filter baseflow separation method really be calibrated by the conductivity mass balance method?" *by* Weifei Yang et al.

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Understanding baseflow is important and not straightforward and papers such as this which compare techniques are valuable. This is an interesting paper that I consider is publishable following moderate revisions. I have made several comments below that I hope are helpful. The Conclusions are a little understated and for more impact, I suggest that the authors explain better what is new and useful here. Perhaps because I thought that the Eckhardt filter always would yield different information to the chemical mass balance, I was not surprised that it is not calibratable in this was at least on a daily timestep (although many studies still seem to try).



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Units. Flow is reported in cubic feet per second. I realise that the USA uses imperial units but SI units are preferable.

Although not suggesting that it needs to be included, the just published paper by Cartwright & Miller (2021, Journal of Hydrology, 593, 125895, https://doi.org/10.1016/j.jhydrol.2020.125895) also looks at the variability of stream EC and implications for water stores that sustain streamflow.

Specific Comments

Abstract

The abstract is a good summary of the paper, but as with the conclusions it could be more impactful

Introduction

Line 27: Should be rainfall not rainfall-runoff

Lines 34-39. It would be useful to expand on this. Baseflow does indeed contain regional groundwater but also contains the other delayed stores of water that you discussed earlier (interflow, bank storage and return waters, slowly draining pools on the floodplain). Many of the earlier papers in hydrograph separation (eg Nathan and McMahon, 1990) do not necessarily equate baseflow with groundwater inflows (see the first paragraph of their conclusions); however, many of the papers that have applied these techniques have. This is a subtle but important point whereby the assumptions with these techniques have changed a little over time.

Lines 60-64. Benchmarking baseflow separation methods is difficult due to the associated assumptions (especially that we are applying a two-component separation to a multi-component system). By what criteria did Xie et al. determine this? Perhaps it is best to leave out that statement and concentrate on the uncertainties. In practice because the Eckhardt method is “tuneable” it should always perform batter than the other filters with fixed parameter; however, that does not overcome the fundamental

problems with this approach.

Lines 70-74. The CMB approach goes back before Stewart et al. (2007). I think that Pinder & Jones (1969, Water Resources Research, 5, 438-445) introduced the technique and Yu & Schwartz (1999, Hydrological Processes, 13, 191-209) further formulated it.

Lines 75-95. All this is correct, but in the context of this paper you should think about what is important. You already have mentioned that rivers contain water from multiple sources, in which case two-component hydrograph separations are not ideal. So is it more likely that the comparison will reveal those intermediate water stores (as in the Cartwright et al. and Rammal et al. studies) rather that being a viable method to calibrate the BFI parameter. There is also a timescale issue here. It may be that the BFI parameter can be calibrated on a long timescale (seasonal or annual) but not on a daily or weekly timescale (i.e., is it an annual average baseflow or a daily baseflow that you are concerned with?).

Methods

Lines 131. See comment above about the origins of the CBM method.

Lines 131-135. Somewhere here or in the introduction you should mention that the conductivity is presumed to reflect the overall salinity or concentration of a conservative component (e.g., Cl).

Lines 136-141. Assigning the baseflow EC as the maximum (or 99th percentile) value assumes that at low flows the river is entirely fed by baseflow. This is probably fine as an assumption in drier areas but may not always be the case in high-rainfall areas. In many areas this maximum value is lower than the EC of regional groundwater – which is one of the lines or evidence that near-river water stores (such and bank return flows) may always contribute to the river (McCallum et al., 2010 and Cartwright & Irvine, 2020 both discuss this). This is also worth a brief discussion here.

Also did you assign a constant baseflow to each water year (or the whole record) or use the strategy outlined in Miller's papers where they interpolate between the maximum EC in each water year to assign a value of baseflow EC on individual days?

Lines 146-151. There is some repetition here with the introduction. Since this is the methods, just tell us how you did the calibration.

It would be useful in include a bit of QC information. How complete are the records and did you attempt to infill missing data?

Results

In addition to the constraints described above. Baseflow estimation based on hydrograph separation requires that the flow regime is not overly influenced by human activities (eg major dams or storages on the river). Both baseflow estimation techniques methods are best applies to streams that are uniformly gaining (both along their reaches and at all times). Can you be more specific as to whether the streams met these criteria.

Looking at Fig. 3, there is some difference with the results of Cartwright et al. (2014) in as much as there was a seasonal difference in that study – the estimates agreed better in summer than winter (proposed as being due to a higher proportion of transient water stores in winter). Do you see that in any of your studies?

Discussion

Line 240. Not sure what you mean by converge

Lines 243-246. Do you know whether that is really the case in your catchments? If there are the data it would be interesting to know whether the salinity of the stream ever reaches that of the regional groundwater as that informs us whether the transient water stores ever truly are absent.

Lines 270-275. It would be useful to briefly introduce hysteresis loops and the informa-

tion that they provide in the introduction.

Lines 283-290. Are any of these basins severely impacted by human activities and what steps did you take to exclude basins that might be unsuitable?

Lines 326-336 and Fig. 7. This works well as a general concept but I would just call the “low salinity groundwater” something like “low salinity transient water” to be consistent with the way that you have discussed it in the paper. Some of that input is from the saturated zone (eg the bank return flow) so is groundwater but there may also be interflow or water from floodplain pools here.

Conclusions

These are a little understated. It is not surprising, given the assumptions inherent in the two techniques and the previous work that there is disagreement. Many of these conclusions have been made before by the studies that you quote earlier. So what is new and important here? Is it possible to use the calibration of the Eckhardt method to estimate total annual baseflow and then use the differences to do multicomponent separation? Does your study help understand the timescales over which either or both techniques yield useful information? Are there river types (size, rainfall, topography) where the comparison worked better?

Strengthening the conclusions would give the paper more impact.

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