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Hydrology and Earth System Sciences
Discussions

Interactive comment on "New baseflow separation and recession analysis approaches for streamflow" *by* M. K. Stewart

Anonymous Referee #2

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This discussion paper presents a new method to separate base flow from rapid flow in River hydrographs. The author claims that the technique is based on evidence from tracer studies, and the approach essentially assumes that they slow response following facilitation events actually comprises two components, an initial "bump" followed by a subsequent "rise", rather than the simple one component which is normally used. A second key assertion of the paper is that base flow separation should be done prior to recession analysis, rather than the other way around which has always been the convention.

Whilst this discussion paper is generally well written and contains some useful review of the literature and several interesting ideas as to how baseflow separation approaches may be improved, there are several fundamental problems that I feel preclude it from

being accepted for publication in this journal. The level of discussion as to the detailed shortcomings of existing models is lacking, and a comparison between the declared new method and existing approaches is also absent. I think this material is certainly of some interest, but much more as a single case study with interesting results from a few hydrological events, rather than the basis of a widely-applicable method. I do not see how any amount of modification would change this, and therefore I recommend the paper be rejected, but invited for resubmission as a comment article or refocussed very much as a case study.

My recommendation is based on the following 4 major issues:

1) The title of the paper is inappropriate. The manuscript does not detail new approaches, it speculates as to how a particular approach may be applied, using a considerable amount of expert judgement, to particular events, in particular catchments. I do not see how this can be declared a new powerful method without considerable empirical evidence over a wide range of catchments and events, and without the need for highly subjective judgements (p7103: "the baseflow fractions during the periods tested were first estimated based on examination of the streamflow and previous experience with the catchment, and were kept constant during the optimisation process to give a constraint on f and k. A well-chosen estimate of baseflow fraction appears to be sufficient. . ."). It is also heavily reliant upon local knowledge and local tracer evidence which is not always available.

2) A major argument during the introduction literature review, largely drawn from other recent publications on baseless separation, is that more than one method should be used as different methods often give different answers. It is somewhat puzzling why the author chose not to demonstrate the difference between this new method and previous ones given the strength of argument for a multi-method approach set out in the introduction.

3) The method is a modified version of that proposed by Hewlett and Hibbert (1967),

which is a very simple form of baseflow separation. There are far more sophisticated methods, many alluded to in the text, but no justification as to why this particular method should be chosen and modified.

4) A central argument of the manuscript is that baseflow separation should be performed before recession analysis. However the paper does not explain why recession analysis is conventionally performed prior to baseflow separation. Eckhard (2005) clearly explains why recession analysis is required by linear filters, and he further demonstrates that most linear filters for baseflow separation typically require two parameters - a recession constant, and the maximum allowable base flow index. More theoretical approaches required a selection of an appropriate period with no surface run-off, and subsequent fitting of an assumed set of equations to the obtained hydrograph. I do not necessarily disagree with the author's assertion that recession of both rapid run-off and baseflow are of interest to the hydrologist, but the author has merely stated this assertion without any evidence. All approaches to separate streamflow hydrographs require assumptions. In the case of digital filters, it is the choice of a low flow period to approximate the recession constant, a, and then the selection of a BFI_max. In the case of more theoretical approaches it becomes the form of recession relationship, as detailed in the manuscript. However, in order to declare the conventional approach unsuitable, it is essential to first explain (and then demonstrate) why the old methods are inappropriate, and then present a new method that requires fewer assumptions and produces more physically-meaningful results, in comparison to the existing methods. This manuscript does not present such an argument.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 7089, 2014.