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Hydrology and Earth System Sciences Discussions

Interactive comment on "Characteristics and drivers of baseflow response in 183 Australian catchments" *by* A. I. J. M. van Dijk

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FINAL AUTHOR RESPONSE TO COMMENTS BY THREE REFEREES AND EDITOR

First of all, I would thank the three referees and the editor for their comments, which have provided valuable suggestions to improve this manuscript. Below I offer a response to the main comments (where I did not respond to specific more minor technical or textual comments I accepted the suggestions in their entirety will simply revise accordingly).

RESPONSE TO REFEREE #1

COMMENT) The title does not reflect the real objective of the paper. In fact, baseflow

is just one of the streamflow response indices that is used. It should be more general. The abstract should be more appealing. All these numbers are meaningless for a reader that does not have read the paper!

RESPONSE) Point taken. The other referee furthermore suggested the number should be removed from the title. I propose to change the title to "Determinants of streamflow response inferred from hydrograph analysis for Australia". I will also remove some of the less informative numbers in the abstract.

COMMENT) The introduction does really a poor job in reviewing the huge literature on recession modelling. It should also refer to aspects of catchment classification, since this should be the main driver of the paper.

RESPONSE) As the referee states, there is a very considerable literature on recession modelling, including some reviews. For this reason, it was not part of my goals to summarise this literature. However, I appreciate some key relevant citations are welcome, including on catchment classification. Will add some; suggestions are welcome.

COMMENT) In my opinion, there is a big problem in the equations used for linear and non linear reservoir models to separate baseflow from quickflow (see my comments later). Note that in equation 3 you are subtracting fluxes from storages, forgetting the division by time. These errors may have a huge impact on the results (see later). Here my comments about linear and non linear reservoir equations: They should follow from mass balance and Q(S) constitutive relation: Linear reservoir: dS/dt=-Q Q=kS From this: dQ/dt=-kQ; and k=-(dQ/dt)/Q this is different from the equation that the author used (dt is missing!)

Non linear reservoir: S=aQËĘb (here I would prefer consistency with the linear reservoir exposition, either Q=f(S) or S=f(Q), anyway:) dS/dt=abQËĘ(b-1)dQ/dt dS/dt=-Q=ab dQ/dt QËĘ(b-1), from which a=-1/(bQËĘ(b-2) dQ/dt) also here there is some difference with the equation that the author used. Can he explain why?

RESPONSE) Indeed these equations have some notation inconsistencies and unstated shortcuts, for which I apologise. In principle, Eq. (2) (with addition of dt) should be integrated to enable its use with daily streamflow totals. In practice, use of the simplified Eq. (3) means that kBF is not independent of time step, as has been shown before (will add some references). It is however independent of Q or S and therefore the analysis is not affected. I also should have stated that the streamflow data are daily flow totals. Will revise the text accordingly. For derivation of the non-linear case I refer to the cited references of Wittenberg and Coutange. I stuck to their original notation in the sake of traceability but can see that some consistency would be preferable. To achieve this, I will rewrite the linear reservoir expression along the same lines. COMMENT) The methods section covers some aspects, but leaves other to the imagination of the reader. Paragraph 3.2: what model was exactly calibrated against what data?

RESPONSE) Not entirely sure I understand, but I think the answer is a linear reservoir model.

COMMENT) Paragraph 3.3, it is stated that other factors where considered to decide which model structure to chose. How was this done exactly? Are these criteria subjective or based on some numbers? In the 'results' section all these criteria seem to disappear. . .

RESPONSE) Basically through reasoning. For example, it was for a combination of these reasons, that ultimately a non-linear reservoir was considered to provide little benefit over a linear reservoir.

COMMENT) In section 4.1 it is stated that the k of the reservoir decreases substantially with increasing time window. Isn't it because dt was missing in the equations?

RESPONSE) No, the window only determines which data pairs are rejected from the analysis, but in all cases dt is one day for all data pairs used.

COMMENT) Among all catchment characteristics, there is nothing related to Geology,

which in my opinion should have the main influence on recession. I think the author should verify correlation of k to storage related properties.

RESPONSE) While I would also expect this, reliable hydrogeological information was not available for all catchments. I could add a brief discussion on some analysis that was done for those catchments with some generic categorical information on geology, but the results were somewhat disappointing (also referee #3 commented that the m/s is already too long). Happy to take the editors advice on this.

COMMENT) Overall, a paper should demonstrate the added value of something new with respect to what has been done before. What literature does this paper contradict, or what was missing in previous studies? What is the take home message?

RESPONSE) Will attempt to make this more explicit in the text, but they are basically listed in the conclusions. If I was to summarise even more concisely, I would say the take home messages are (1) for the purpose of catchment modelling a linear reservoir model adequately describes baseflow behaviour in these catchments; (2) both the recession coefficient and the relative and absolute contributions of baseflow to total streamflow are best predicted by considering climate indicators, with little discernable influence of terrain factors.

COMMENT) Page 5817 line 13: "to avoid over weighting on either larger values or (through logtransformation) smaller values, the value of k was optimised rather than directly inferred". (1) I don't understand the difference between optimized and inferred in this context. (2) if you optimize, the objective function used can put larger or smaller weights on different errors. So your approach does not solve the problem.

RESPONSE) The reasoning was that Eq. (10) (which uses the relative agreement between estimated and observed flow) avoids the fitted k value being influenced by absolute streamflow values. However, I recognise that any objective function will weight different types of errors differently, that is unavoidable. Will rephrase this.

RESPONSE TO REFEREE #2

COMMENT) The analyzed watershed covers a great extension of south-eastern Australia which has been the topic of other baseflow studies (Chapman, 2003; Chiew and McMahon; 1993). More references to these works should be added.

RESPONSE) Will do so.

COMMENT) Even if the hydrological approach (different to a hydrogeological study) doesn0t need an exhaustive analysis of the aquifer system, this study, similar to the conclusions derived with which groundwater systems are involved (extractions/recharge, existence of perched aquifers,. . .), still require a basic set of hydrogeological data to validate and provide an understanding of the results in terms of the baseflow coming from the groundwater system. In this sense, the author assumes the origin of the baseflow is the groundwater system (page 5813, lines 9-10). However, these contributions can vary in a wide range of sources, such as unsaturated soil, snowmelt or lakes drainage, which at times may be greater than groundwater contributions (Hewlett and Hibbert, 1963; Tallaksen, 1995). The subterranean origin of baseflow must be justified as well as the influence of the other contributions.

RESPONSE) Snowmelt and large lakes do not occur in these catchments. The catchment data set has been screened to exclude any catchments affected by water resources development (e.g. dams, regulation, groundwater extractions) although some effect is always possible. The greatest potentially confounding factors may be slowly draining wetlands in the streamlines of wetter catchments and perched groundwater tables. These are caveats that affect the definition of what constitutes baseflow, but otherwise do not affect the analysis. Can add some words to this effect if deemed useful.

COMMENT) In Page 5813, lines 16-17 the author proposes the utilization of the recursive filter described by Wittenberg (1999). Why this choice? Have other filters been considered?, such as those based on recession studies or BFI (e.g. Chapman y

Maxwell, 1996; Boughton, Chapman and Maxwell, 1996; Eckhardt, 2005).

RESPONSE) Other filters were considered and to also tested, but it was beyond the objective of this m/s to describe those results. Some analysis was done previous to this study to test how sensitive the choice of filter is. It was concluded that this is not a major factor, the main reason being that the greatest differences between filters occur in the first days after a storm event (although even then many filters produce similar results). These days were excluded from the analysis - precisely because confusion in separating storm flow from baseflow 'proper' is most likely to occur during these days. The Wittenberg filter was chosen because it does not require a priori assumptions of BFI, is amenable to parameter optimisation and automation, and comparatively simple yet conceptually sound.

COMMENT) Throughout the paper, major revisions in the descriptions of the equations and units are required. Page 5814, line 12, the _ parameter is dimensionless being _ =1 when the linear model is assumed. For the recession coefficient k_BF the dimensions are (LËĘ(-3+3_)âĂćTËĘ(-_)), in case of lineal model (TËĘ(-1)). In this study, is dt=1 day?

RESPONSE) Will fix this up. Indeed dt is 1 day.

COMMENT) Page 5814, lines 18-21, is the study of E. Kwantes a local study which justifies Tqf=10? In this case, which parameters has been studied for this assumption (soil characteristics, watershed area,. . .). Have other approximations been considered, e.g. Linsley et al., (1958)?

RESPONSE) No, the study of Kwantes used similar information as that in Figure 5, but for a smaller number of catchments. Probably this reference is not needed since the point is revisited further on in the text.

COMMENT) Page 5839, Figure 1 can be improved. I suggest changing this figure by extending the study site and adding additional information (geology, aquifers. . .) with

a small subfigure to detail the position of the study area within Australia.

RESPONSE) Appeciate these suggestions, will try to incorporate them.

RESPONSE TO REFEREE #3

COMMENT) Only one baseflow separation (Fig. 4) is shown in the whole paper though there are many other figures. However, Fig 4b shows clearly that also in semi-logarithmic scale flow recessions are still curve shaped and not straight lines. Thus storage is indeed nonlinear. As the paper claims to be a scientific one, this should be discussed. Of course, in a practical engineering study the linear reservoir could be adopted as is easier to be used. [. . .] Section 4.3: It shows that nonlinear model fits the streamflow data better than the linear model, which is the truth of the nonlinear nature. Therefore, I don't agree with the author who says ". . . these findings were considered insufficient basis to prefer the more complex non-linear reservoir model ...".

RESPONSE) This certainly occurred in some cases, as also can be inferred from the range of beta values (see also Section 4.2 and Fig. 6). However in Section 5.2 arguments are provided as to why a linear reservoir was ultimately considered preferable (in short: for most catchments beta was close to unity; the simplification produced very little deterioration in performance; some unrealistic beta values were obtained; there was comparatively strong parameter equivalence between kBF and beta; and the non-linearity may be attributable to in-stream evaporation rather than a function of storage behaviour). Ease of use was not really a criterion considered, however the ability to estimate parameters in ungauged basins was considered important. I do accept that many of these arguments are of a practical rather than a scientific nature, and could add a comment to emphasise this.

COMMENT) The title should be without the number of stations.

RESPONSE) Referee #1 also suggested a different title. I would propose to change the title to "Determinants of streamflow response inferred from hydrograph analysis for

Australia".

COMMENT) The manuscript is really too long. Some parts are repeated. Only as an example; Discussion is unnecessarily long and repeats number in the previous section. It has many acronyms after a while mixed. It is not easy to keep all these in mind. There should be a solution to this.

RESPONSE) I will try to reduce length.

COMMENT) Although it has been shown that seasonality has great importance in baseflow studies, it is not considered. Simply a statement is needed why? (See for instance papers by Wittenberg already cited by the author and Aksoy et al. (2001), Probabilistic approach to modelling of recession curves, Hydrol. Sci. J., 46(2), 269-285.)

RESPONSE) Will add such a statement.

COMMENT) I am concerned about the importance of the semi-variogram concept for this study. Can the study be made available without considering the semi-variogram? This concern might come due to the fact that the author has not discussed the results but only provide some numbers and percentages as they are calculated.

RESPONSE) The semi-variograms were included because my prior experience that some readers want to judge the appropriateness and fit between model and variogram data themselves. If the editor agrees, I will relegate them to supplementary material.

COMMENT) Under section 3.2, five different examples of linear regression are listed. The author does not need these very simple things to keep in his already very long manuscript although he does not use all in the manuscript. Then there is no need to have Figure 2.

RESPONSE) If the editor agrees, I will remove these sentences.

COMMENT) Page 5817 Line 10: It is said that "Tests showed that . . .." What type of

test? Brief information should be provided.

RESPONSE) Figures similar to those in Figure 5 were produced for different methods. Perhaps this statement can be removed too.

COMMENT) Appendix A: It is too much information. Variables defined there can easily be inserted into the main text or they can simply be defined in a shorter appendix. As there are many variables, some have not even be introduced (For example; PWEP in Page 5825 Line 13).

RESPONSE) If the editor agrees, I will relegate this to supplementary material, but for the sake of traceability it is probably important that this information remain accessible.

COMMENT) Section 3.4: A simple flow chart can be provided instead of or together with the decision tree. A simple chart based on the decision tree in the manuscript is made available in this report. Two questions for the rising limb of the hydrograph: (1) why does the author prefer the backward filter? (2) In the decision tree, there is an open point. The case when neither the forward nor the backward baseflow is less than streamflow is not mentioned. Although it is physically clear that baseflow cannot exceed streamflow in the river, this should be mentioned for the completeness of the decision tree (See red part in the flow chart).

RESPONSE) This is an excellent addition, and the effort made by the referee is much appreciated. Will include this flow chart in lieu of the textual description in 5819-5820.

COMMENT) Sections 4.5-4-8: They can be combined and be shortened rather than pasting the template copied from Section 4.5 for the four sections.

RESPONSE) I will attempt to do so.

COMMENT) Section 4.3 and Section 5.2: What is/are the difference(s) between the two sections?

RESPONSE) It was simply an attempt to keep results and discussion separated. In

this sense, probably the last two sentences of 4.3 can be deleted.

COMMENT) Figure 2: These figures make me confused. Either the figures itself or Figure caption is erroneous. Otherwise they are not comparable. When we consider the Figure caption is correct, then we have two figures neither their models nor their scales are identical preventing us making a comparison. Plus note that the regression line is the same y =0.9442x.

RESPONSE) I do apologise for this oversight; what is inconsistent is the regression equation and line in Fig 2b. However I take the point that this figure adds little and will remove it.

AUTHOR RESPONSE TO EDITOR

COMMENT) I agree that the equations for the linear and non-linear recession seem to be inconsistent and therefore some clarification is needed. If these equations were actually used in the form they are presented in the paper a complete revision of the analysis would be necessary.

RESPONSE) I refer to my response to referee #1 for explanation why I don't think complete revision is necessary?

COMMENT) Also, I agree that the review of the literature is missing important contributions that analysed the possible links among low flows and hydrological features of the catchment.

RESPONSE) I will aim to expand the discussion somewhat and add citations without making the m/s much longer (as referee #3 suggested it is already too long). What citations to add is to some extent a matter of personal preference; I would happily consider any specific suggestions the referees or editor would like to make.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 5811, 2009.