**General comments**

This is a review of the revised paper *"A promising new baseflow method and recession approach for streamflow at Glendhu Catchment, New Zealand" by MK Stewart.* I have also reviewed the first submission. The author presented a new recession analysis method claiming the need to begin with streamflow separation into quick and baseflow before analyzing the depletion of the components. In general, the Author (MK Stewart) did a great job during revision: The paper is much more structured, the introduction leads now clearly to the aim of the study. The paper benefits from removing parts of the first version (isotope an TT theory). The concerns of the Editor are also adequately addressed (e.g. important literature is added now). A suitable adjustment of the title has be done, it is, however, still difficult to accept the justification of the proposed method as long only data from one catchment is used. But in general, enough effort has been devoted to discuss the new method and to confirm the meaningfulness of a component-separated recession analysis.

However, due to the extensive revision, it is now a "new" paper and I suppose some improvements regarding the structure and the content of some sections (see comments below). Detailedness due to the introduction of a new method is feasible, but the paper seems to be very long. Discussion and Conclusion should be revised to gain a more comprehensive "Synthesis". Please check for repeated statements. I wonder why one sub-heading in section 4 is a question, while all other subheadings are not? I like the way the HESS-D paper (Stewart, 2014a) was cited in the new manuscript, because the new manuscript has significantly changed and valuable information from the first version will improve the impact of the new paper. However, please consider my comments below, which are mostly minor suggestions. I recommend publishing this work after moderate revisions.

**Major comments:**

1. I have found some improvements in the paper regarding the distinction between event-specific application of the BRM-method and a continuously application. Now two calibration possibilities are mentioned, it is thus worthwhile to explain more detailed when and why event-specific application of BRM is possible and what is needed for a continuous analysis.
2. Please move the catchment description from 3.1 in the Method section and rename Methods to something like Methods and Study Site/Data. Please also remove the explanations in L454-458 or move it to the very beginning of the results section.
3. At some points the Author referred to text *above* or *below* in the manuscript. It would be even more helpful if the exact sections were mentioned to guide the reader to the relevant information (e.g. L550).
4. L97-101: I have some concerns with this sentence in the Introduction. It sounds very much like a Discussion or Conclusion statement. It is partly also found in the middle of the Abstract (L28-31) suggesting (due to its position in the abstract) being more a description of a study's outcome than an introduction to the topic of the paper. Therefore the Author might consider moving this statement further down in the manuscript (or even remove it).
5. L259-261: I partly disagree with this statement. Other studies (e.g. Brutsaert and Nieber, 1977) have also introduced a separation of different baseflow response signals (short- and long-term, has been mentioned in L351-352)), the statement in the manuscript should therefore be slightly adjusted.

6. Collischonn and Fan (2013) suggested that $BFI_{max}$ can be estimated from FDCs, but limited their results to some regions in Brazil. Perhaps I have missed it, but the Author should therefore give some justification why the approach could be assign to the catchment in his study. Although $Q_{90}/Q_{50}$ seems to be highly sensitive to the period of streamflow record.
7. Fig 5d: It should be stated that the temporal connection between streamflow and baseflow and and quickflow is not the same. Otherwise the FDCs would have more scatter. Each flow sample (streamflow, quickflow, baseflow) is sorted separately to draw the analogous FDC. Normally I would expect that the time index of streamflow is also (consistently) be used for base- and quickflow values. This comment might have also implications for the statement in the conclusions (L902-904).
8. L614-617: The implication of the presented result (variability in FDC) is very vague in my eyes. The breakpoints and also the relevant outcomes from Pfister et al. (2014) have to be explained more detailed (at least as long the cited reference is not available for the reader). Otherwise I do not see the value of this reference.
9. Again regarding the structure of the sections: The seven outcomes in 4.1 look like conclusions from the result section before. In my opinion these statements are to a greater degree the outcomes of the discussion section, thus should be moved to the end of discussion or into the conclusions (or a section called synthesis).
10. L437-438 and L721-723: This constraint seems to be only possible if exactly one parameter has to be calibrated. But BRM is based on two (f,k) parameters, isn't it? Otherwise please state clearly which BFI (L437) is meant, the BFI from BRM or from the cited paper? Isn't it possible to derive the same BFI values with different values of bump and rise (L721-723)? Seems to me that the "optimum shape" (L724) is somehow meant to be a "soft-data" fitting criteria, if so, the Author should comment more detailed on this "by-eye"/"subjective" fitting (this has be done to some extend in L734-737).
11. L762-772: Very interesting and valuable discussion point! This would be even more convincing if the Author points out, that the paper (and the dQdt/Q method to fit an nonlinear S-D-model) from Kirchner (2009) is very often used to do recession analysis, not to "inferring rainfall from catchment runoff".
12. Perhaps an additional comment; but some reference of oxygen-18 and deuterium as stable isotopes could also be included into the paper (besides the - I guess- less common used Tritium), especially when discussing old and young water (point 3, Line 785-789). Other studies also have found significant differences in water ages between quick- and baseflow components using oxygen-18.
13. L 813-865: The drawn "observations" at the end of the discussion section are a little bit problematic in my eyes. I do not understand why the Author use "observations", obviously not enough evidence can be extracted from the "1-catchment"-results. However, the study is certainly not "limited" (L813) and the observations are discussion points, thus I encourage the Author to write these paragraphs in a more positive manner although this manuscript has earned some serious criticism during the first review. A lot of results fit quite well into the speculations and concerns that other authors have with conventional recession analysis!!!
14. L689+L887: "not critical" should be explained more detailed.
15. Additional comment: L917: "when baseflow becomes predominant"… Soil water is certainly not in the focus of this study, but it would be very interesting to learn more about timing of late recession (mainly controlled by baseflow) and soil water

or the soil wetness conditions of catchments (and the relationship of baseflow and soil water recession).

16. In the conclusions (L919-924) valuable improvements and applications of the proposed method are listed. The Author – as an expert – could give some more guidance (or speculate more) how the method should be applied in models or other catchments.

**Minor comments**

1. L68/69: Change to moderate flows. "middle flow" is rather a "state" or "transition" than an event. Perhaps moderate or intermediate (L833) flow fits better here. Or "mid-flow" (L828). Please check consistency of these terms.
2. Remove second full stop in L95.
3. Please give some reference(s) for the statements in L111-115 – otherwise it appears to be a look ahead to the results.
4. L136: better "mainly groundwater storages", because also wetland outflow or snowmelt can be seen as baseflow signal.
5. L139: add "exemplary" to recession curve.
6. L140-142: I fully agree with the Author's statement, but this is not easily visible from Figure 1.
7. L 151: Remove the break between the paragraphs.
8. L233-234: Please give an example or the range of suggested values.
9. L265: "also"?
10. Eq11: "a" instead of "e"? Eq13: Is "alpha" explained before Eq13?
11. L386-392: This is not fully appropriate in the Method section. Perhaps these statements could be moved to the introduction in a shorter form.
12. Section 2.3 (L395-404): Is this really necessary? Most HESS readers will be familiar with FDCs, otherwise a reference will be enough.
13. L436 two blanks.
14. L458: Add a period.
15. L498: Reference not needed again here. Is L499-503 an interpretation of the Author or a result from the cited study?
16. The introduction into Section 3.2 is confusing (the first sentence).
17. It might be helpful to color the two line types in Fig 5a-c or use different line types (solid and dashed) to give the reader (with less experience in dQdt-Q-plots) more guidance while reading the text (L586-590).
18. L622: Can the assumptions (no snowmelt or rainfall) be found in the cited literature or is this an additional assumption of the Author?
19. Misspelling in L691?
20. L832-836: Very good point! This is a step further on to explain why Brutsaert and Nieber (1977) have success dividing short- and long-term recession behavior and perhaps why Stoelzle et. al (2014) (L865) found that a "FLEX- model" (which incorporates with short- and long-term behavior) is generally superior to other groundwater model structures).
21. L838-844: Also snowmelt or wetlands or deeper groundwater systems may contribute. Please give a reference for "drainage from different aquifers in different dryness conditions" (L843).
22. L850: "The separation can be made" …?