

Interactive comment on “Technical Note: A measure of watershed nonlinearity II: re-introducing an IFP inverse fractional power transform for streamflow recession analysis” by J. Y. Ding

Anonymous Referee #2

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The author has recently been involved in some interactive discussions on HESS-D papers, which is to be applauded. The current manuscript is basically, in his own terms, “*a consolidation and extension of these two sets of comment, plus two additional ones, all on streamflow recession*”. Again, this is to be stimulated as well, especially because the comments involved do not really ‘count’ as a scientific publication (for one thing, they’re not peer reviewed). This ‘consolidated’ note therefore seems essential to convey the author’s results.

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The paper focuses on the use of a series of discharge transformations to assist characterization of streamflow recessions. Although I did find the subject as such interesting, I do have a number of concerns:

First, the proposed methodology requires that Brutsaert-Nieber parameter b is known in advance (i.e. the transform is a function of b). Although the author has solved this problem by testing multiple estimates of b , I found the results not convincing. A linear regression is used to quantify the goodness of fit for multiple values of b . The resulting R^2 is close to 1 for all cases, suggesting that it has no discriminative power. The methodology as applied thus is not able to select the ‘true’ b . This in contrast to ‘traditional’ Brutsaert-Nieber analysis which does find an \bar{b} (although different approaches result in different estimates of b , as recently shown by Stoelzle et al, but that’s another matter). This issue is not discussed in the paper.

Second, the proposed methodology is applied to individual recession events, resulting in event-specific estimates of a and b . Again, this is in contrast to ‘traditional’ Brutsaert-Nieber analysis which collapses all recession events to a single data cloud, resulting in unique a and b values. Especially when reasoning from the Boussinesq or the Manning equations, one should not expect inter-event variability in a , yet, this issue is not discussed at all.

Third, the proposed methodology is applied to a very limited duration dataset, for a large catchment. The author does acknowledge this, but the reasoning is rather weak (“it has been because the recession flow data available for analysis were published in an open access journal such as HESS”) and unconvincing because many streamflow data for smaller catchments, is freely available, e.g. the MOPEX data, or the LTER sites.

Above criticisms prevent me from recommending publication of the manuscript in its present form. In order to warrant publication, the proposed transformation method should be applied to a longer dataset from a more suitable catchment, using better

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indicators to distinguish 'good' from 'bad' b -values. Also the issue of temporal variability of parameters, and a more thorough comparison with 'traditional' Brutsaert-Nieber analysis should be included.

Also, the paper should be self-contained. References to HESDD-comments by the author should be eliminated from the paper. Arguments made in those comments should be consolidated into the present paper.

My final recommendation is therefore "major revisions".

Specific comments:

[sec. 1] — The significance of "variation and persistence of the low flow" is illustrated with a reference to the Bible. I'm not sure if such a reference is appropriate in a scientific article target at a religiously diverse audience.

— *"During the public comment period of ... I brought to their attention ... refined and elaborated in a later comment ... This note is a consolidation and extension..."* — Although I applaud the author's enthusiastic involvement in recent discussions in HESS, I don't think references to this activities should be part of a research paper.

[2.1] — *"The heading of my most recent comment was titled on purpose ... this hopefully has conveyed my view"* — Idem

[2.1.1] — The 'classic' Brutsaert-Nieber b -values 1, 1.5, 3 are mentioned, without mentioning too that they were derived from hydraulic groundwater theory (the nonlinear or linearized Boussinesq equation, applied to a flat aquifer, characterized by uniform conductivity).

[2.2] — *"This was discovered, ... the latter then a graduate student ... by me, then a graduate student at Guelph, Canada"* — The then status of the referenced authors is not relevant.

— *"a parameter b value of 1.5 characterizes late-time recession in Brutsaert-Nieber*

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model" — No, it characterizes late-time recession from a Boussinesq aquifer under specific assumptions.

[2.3] — *"Their b values range from a lower limit of one"* — What about the kinematic-wave process that in theory, following steady-state initial conditions, would lead to a $b = 0$?

— *"... to an upper limit of 2"* — What about the early-time $b = 3$ as derived from the Boussinesq equation?

— *"As an aside..."* — I see no reason why this aside should be in the paper.

[3] — *"IFP ... most suitable for low flow analysis"* — What about measurement noise and discrete values? These are most prominent at the lowest flows.

— *" R 's among all transforms for four events are similar, all close to or at 1.0"* — So there is almost no discriminative power in using R ?

— *"ranges from (1.33, 0.07) to (1.5, 0.23)"* — Given the intra-event variability in a (which is not discussed at all!) why would one favour the lowest a for $b = 1.33$ and the highest a for $b = 1.5$?

— *"For the Spoon ... linear storage ... un-transformed"* — which is $b = 0$ to 1. How is this consistent with the earlier $b = 1.33$ to 1.5 as derived from Manning's equation?

[4] — *"The range of b values varies from one to an upper limit of 2, rather than that of 3 adopted by Brutsaert and Nieber ... for the early-time lower-envelopes"* — The upper limit of 2 has been derived from a complete different approach (and assumptions) than the $b = 3$

— *"based solely on the highest correlation coefficients for each event"* — Again, differences in R are very small

— *"[This case study] has demonstrated only marginally the superiority of IFP transform over the conventional log transform method"* — If the differences are "only marginal",

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why is is then “superior”?

[Figure 1] — Is not mentioned in the manuscript

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 15659, 2013.

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