Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-115-RC2, 2019

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Technical note: Comparison between two generalized Nash models with a non-zero initial condition" *by* Baowei Yan et al.

Anonymous Referee #2

Received and published: 24 September 2019

The study is based on the comparison of two well-known models It seems that the objective is truly very narrow and does not contribute significantly to enhancing our understanding of hydrological system functioning. As such, I find it of very limited relevance to HESS. It might be more suitable to an application- or mathematically-oriented Journal, even as I am not sure that there are significant advancements in terms of mathematical developments.

A point which is not entirely clear to me is the basis for statements of the kind "The results show that the GNM provides a unique solution while the DLCM has multiple solutions depending on the estimate accuracy of the current state.". They then state that

Printer-friendly version

Discussion paper

observed values do not need to be estimated, thus implying that observed values are not associated with uncertainties. If a key difference is related to GNM being associated with observations (and not estimates), one could also claim that observations are always associated with measurement uncertainty/error. How do the Authors reconcile this aspect? There seem to be no mention of this aspect in the study.

In the example section, the Authors mention relying on an optimization approach to estimate model parameters. It seems to me that parameter estimation uncertainty is neither quantified nor considered and I am not sure why.

With reference to non-uniqueness of the solution, I am not sure why this is not compatible with typical uncertainty propagation analyses that are performed in environmental systems. Since there is uncertainty in some quantities, the latter should propagate to model outputs. Such an uncertainty can also be associated with initial conditions. The Authors should also comment on these aspects in future works.

In terms of comparisons, I am not sure about the point raised by the Authors. They claim that the results obtained by the DLCM approach are approximated (I guess when considering results obtained through the continuous counterpart of an otherwise discretely sampled signal). I am not sure about what elements we learn from this exercise with respect to other studies on signal analysis that are available in the literature.

Finally, it should be noted that the quality of the English is really substandard, thus posing difficulties to the reviewer. I am not providing specific examples simply because they are widespread throughout the text.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-115, 2019.