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Discussions
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Interactive comment on "The Heterogeneous Discrete Generalized Nash Model for Flood Routing" *by* Baowei Yan et al.

Anonymous Referee #1

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General comments

The authors present a new flood routing model, "HDGNM", which is a further development of the "DGNM" model, which was developed by the primary author. The authors expand on the DGNM model, a Nash-cascade model, by incorporating a heteregeneous S-curve. The motivation of the authors is to improve flow routing in rivers that exhibit changes in the slope and geometry along its reach. They apply their model for flow routing in a 105 km stretch of the Hanjiang River, and demonstrate that the HDGNM model provides smaller error statistics.

I want to preface this discussion by stating that I am not an expert on the mathematical development of Nash-cascade models, and I recommend to the editor to rely on a different referee to judge the novelty or necessity of this development within that branch

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Discussion paper

of study.

First, I want to complement the authors for presenting their study in such a concise format. Although the authors should expand on a few sections to ease understanding, the manuscript has a very respectable size. However, I would strongly recommend for the authors to let their manuscript be proofread by a native speaker.

I think my main comment on the manuscript is that, in its current state, I fail to see the benefits of the proposed approach. The quantified improvement over the DGNM model is well described in the case study (although it does seem a marginal improvement at best), but I'm not convinced that the entire approach is conceptually ill-conceived. This may in part be due to a lack of context or well defined objective in the introduction, but points to some deeper concern as well.

First a small note on the literature review. The authors discuss a wide arrange of literature (starting from Nash's original paper) from L23-L54 on Rainfall Runoff modelling, even though the manuscript focuses on flow routing. Only from L55 onward the authors turn toward the relevant literature. Perhaps a restructuring to better lead up to the main objective would be advisable. Regarding relevant literature, I feel the authors focus to much on the Nash-cascade types of models and developments thereof, at the expense of other state-of-the-art literature on distributed hydrological modelling (e.g. see Imhoff et al., 2020 and references therein. DOI:"10.1029/2019WR026807").

Second, if I had to distil an objective from this manuscript it would be (Paraphrasing from L16) "To adapt the DGNM for flow routing to better deal with river reaches with varying geometry". This objective overlooks other, perhaps better suited, methods to deal with flow routing in river reaches of varying geometry. Conceptually, I would expect models derived from the shallow water equations to provide strong competition indeed. A literature review discussing alternatives outside from Nash models, would help to persuade the reader that the proposed alternative is worthwhile.

Third, building on the previous section, I'm having trouble seeing the inherent conceptual benefit for the broader scientific community. Applying Nash models to flow routing in rivers like the Hanjiang is really stretching the conceptual interpretation of the model to (in my opinion) untenable limits. The authors state that introducing heterogeneity would theoretically improve the model, but this is not supported by a rigorous analysis of the physics of river flow that their modification tries to alleviate. An interesting addition could perhaps be found in discussing how, from a physical point of view, changes in slope and cross-sections are expected to influence travel times and distortion of the flood wave, highlighting the flaws in the DGNM and hypothesizing how the HDGNM addresses these flaws. In its current form, I lean toward seeing the HDGNM model as an (overly) complex data-based model, more akin to machine-learning models than to process-based models - which have their applications as well, but if seen as such, require proper introduction and review of relevant literature.

The case study itself is interesting and well defined, although some expansion on the case study (see specific comments) is required. The application of the HDGNM model is clear and results are well described, although somewhat marginal compared to DGNM. I would encourage the authors to publish the source code of their model and test data along-side the manuscript as well.

In summary, I think the manuscript needs extensive revision before publication in HESS would be advised - mainly to better place it in light of the state-of-the-art and highlight the academic advancement made. Although to be fair, I fear the inherent academic progress made by this manuscript, even if thoroughly revised as advised above, may remain too little to be considered for publication in HESS, and that a different journal may be better suited. I include some specific comments below, in the hope they will be useful to the authors.

Specific comments

L10: "The heterogeneous... the DGNM". This is very vague wording: I did not understand what the authors meant by 'conceptual interpretation of the DGNM' until much later on. Consider rephrasing this.

L16: "The HDGNM ... change greatly". Be more specific (here, but certainly later in the manuscript) what is meant by 'greatly'

L53: "All of these ... Runoff modelling". Be more specific which improvements are relevant for the objective

L73: "The DGNM ... topograpy too". Check language

L83: what is a combination formula?

L106: "another way to deduce the HDGNM": what is the first? Why is another way required?

L111: "But for the basins with large topographical changes": Some form of conceptual sketch of what the authors mean by 'large topographical change' would be appreciated.

L125: it is unclear why this formula is introduced, nor how it follows from (4)

L133: what is a sub-river?

L150: "but it seems impossible..." I'm not sure I follow why it is supposed to seem impossible.

L211: please specify on what basis the river is subdivided into these reaches.

L211: please use scientific notation for the slopes (1.76*10**-4)

L212: It is indicated by whom?

L213: please make clear what sub-reaches 1 and two are (Huangjiagan-Guanghua and Guanghua-Taipingdian?)

L219: The selection criteria of floods should be better described. Are these all the flood waves that fulfil the stated criteria? What do the authors mean by (delta t = 3)?

L221: I don't understand what the authors mean by 'the simulation effect'

L221: The forecast capability of HDGNM cannot be tested by comparing to DGNM. Improvement over DGNM can be tested, but any forecasting prowess should be based on evidence (measurements)

L223: please specify how ’flood data’ was obtained and what it consists of.

L226: please specify which parameters were optimised

L229: It would be very helpful if the authors could expand on the outcome of their optimisation exercise. Specifically, assuming that n=3 is an optimised value, is this an expected value? The authors state the the HDGNM is better suited to deal with topographical change, and this case study indeed shows four subreaches, of which the first one has a shallower slope than the final three. So, based on this information, would n=2 not be a more expected value? Or perhaps n=4, based on the number of subreaches the authors divide the river into.

L267: "The heterogeneous ... the DGNM". I think the way this sentence is phrased does not help the author’s case. Would ’The HDGNM was derived by implementing a heteregeneous S curve into the DGNM model’ not be more to the point?

L295: What would constitute a reasonable request?

Technical corrections

Figure 2: The size of the labels is a bit small and difficult to read.

Figure 2: Please indicate which of these flood are the calibration events and which are the validation events

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