29. November 2021

Answer R2:

Thank you very much for the detailed and well worked out review. Your comments, questions and remarks are very valuable for our revision of the preprint. We are pleased to discuss your points of the review in more details to make our paper and statements more clear. For that purpose we cite your comments in the review (with *"", italic, underlined*) and answer each comment thereafter. The specific comments are answered first, before responding to the overall comments.

- Reviewer Comment 1:

"Line 23-24: "Open demand exists in hydrological modelling of rainfall-runoff regimes in lowlands which are distinguished by complex flow routing in mostly intensively drained catchments by manifold control structures." I think this sentence tries to say too many thigs, consider splitting up the points being made."

Answer to comment 1:

Thank you for the comment. We are pleased to split up this sentence as follows: (Line 23-24) "Open demand exists in hydrological modelling of rainfall-runoff regimes in lowlands. The flow routing in lowland catchments is characterised by artificially drained catchments using manifold control structures."

- Reviewer Comment 2:

"Introduction. Traditionally backwater and inundation process would be simulated by a coupled hydrodynamic model (of which many are available). I think this needs to be discussed and then a clear reason for including such processes within the hydrological model can be set out. At the moment the introduction only discussed modelling of rainfall runoff as an isolated field of research. As a reader I immediately ask why not couple to another model. I appreciate that this is visited later in the manuscript."

Answer to comment 2:

Thank you for the comment and advice. We agree in your point of view and your arguments. We are looking especially from the hydrological model point of view on this topic. Improving the functionality of a hydrological conceptual model is our objective in this paper. Our proposed conceptual method doesn't intend to substitute the application of hydrodynamic numerical models for computing, for example, flood inundation maps. Therefore, we agree in explaining the intention of the proposed hydrological conceptual method in detail already in the introduction. The discussion of applying hydrodynamic models and/or hydrological models is shifted from line 73 – 84 to the introduction in line 36 ff. In this way, the reader is informed earlier about the intention of the proposed hydrological method. Additionally, the limitations of our proposed conceptual method will be explained in a new additional chapter in the paper. In this way a more nuanced conclusion will be given at the end about the applicability and the limitation of the proposed method.

The text of the shifted lines in the introduction (36- 49):

"Simulating backwater effects, velocity fields and the spatial distribution of water depths for flood inundation maps demands for 2D or 3D hydrodynamic-numerical models with the numerical integration of the partial differential equations describing the flood routing processes. To compute spatial detailed simulation results in river streams and flood plains, coupled hydrological and hydrodynamical model approaches fit well to meet the required modelling objectives. But, hydrodynamic-numerical models require larger effort to parameterise river streams and simulation times, which are at least one to two orders of magnitudes longer in comparison to conceptual hydrological flood routing approaches to model river streams. High resolution data describing the topography of the main channel and the natural flood plain in the case of bank overflow is necessary. Hence, the availability of suitable detailed profile data from measurements is significant for hydrodynamic-numerical modelling. The larger effort in data resources and runtime for hydrodynamic-numerical model simulations is no limitation for answering special research questions and to create detailed inundation maps. However, applying a coupled hydrological-hydrodynamic model shows disadvantages in the application on meso to regional catchment scales (>100 km²) and for operational forecast applications. Therefore, it is proposed in this article, that a stand alone hydrological approach can be beneficial in flood forecasting models to enable parsimonious and efficient modelling of flood routing and backwater effects in lowlands, by a conceptual hydrological method producing less detailed results."

- Reviewer Comment 3:

"There is some rather vague language used in places that detracts from the writing. For example, on line 46 "new concepts are required." What are new concepts? And then with regard to "this article fulfils five objectives in hydrological modelling" it would be more normal to set out the four objectives and then discuss the success of meeting them after the results have been presented."

Answer to comment 3:

Thank you for your remark. We agree in substantiate the sentence in line 46 and agree to revise the structure of the text. In the revised structure, the objectives are described first to meet the revealed shortcomings in hydrological modelling. Additionally, we discus the met objectives and point out the limitations in more detail in a new chapter (7 discussion of results).

- Reviewer Comment 4:

"Line 55 "Most promising to accomplish the defined five objectives for a re-usable, open, efficient and parsimonious hydrological model, is the development of an extension approach for state-of-the-art flood routing methods (for instance Muskingum-Cunge or Kalinin-Miljukov), which can be transferred and implemented in different hydrological numerical model approaches and on different model scales." Could this be more specific to your study objective, which I think are to have a scheme that can simulate the backwater effect of river and floodplain flows. This might just be my take on it but the objectives seem broader than those set out in the abstract and title."

Answer to comment 4:

We appreciate your remark about this sentence and agree in your comment that the describtion of objectives require a revision. This goes along with the answer to comment 3. The sentence is revised as follows: “To accomplish the defined five objectives for a re-usable, open, efficient and parsimonious hydrological method to model backwater effects, the authors suggest to develop a conceptual extension approach for state-of-the-art flood routing methods (for instance Muskingum-Cunge or Kalinin-Miljukov).”

- Reviewer Comment 5:

“Line 67-69. These statements could do with some references.”

Answer to comment 5:

Agree. References are added.

- Reviewer Comment 6:

“Line 76 “(2) future impacts of climate change and urbanisation are not directly parameterised in the model approach” I don’t agree with this statement. They are included to the extent that they are included in whatever forcing is coming from the models boundary conditions. It’s also common to adjust friction values in such model or edit the topography to explicitly represent urbanisation – if anything urbanisation is more explicitly represented in a hydrodynamic model than what its being compared with. I agree with points 3 and mostly with point 1 - although there are examples of hydrodynamic models being applied in quite data scare settings with limited parametrisation and topographic data.”

Answer to comment 6:

Thank you for the remark and discussion. Our intention is to apply parameters of landuse maps like the sealing rate of partially impermeable surfaces and parameters of the spatial distribution of vegetation types (root depth, LAI) and spatially distributed rainfall data series as input. When considering stand alone hydrodynamic models, we agree in the argumentation that indirect parameters are derived (e.g. friction values) to represent the impact of urbanisation. We see here a dependancy on coupling the hydrodynamical model to a hydrological model for representing the catchment characteristics. We agree, that this argumentation remains vague and that it can not be given without further explanations. Therefore it is not given in this context anymore.

- Reviewer Comment 7:

“Line 100: I didn’t understand the use of the word ‘decisive’. Furthermore, the rest of the sentence lacked context for me.”

Answer to comment 7:

Thank you, we agree in revising the sentence. “In (Waseem et al., 2020), a review of models is published with regard to simulate important hydrological processes in coastal lowlands. This review shows weaknesses in the model SWIM (soil and water integrated model) and HSPF (hydrological simulation program—FORTRAN). The approaches in the models SWAT (soil and water assessment tool) und MIKE SHE show good conformity to simulate processes in lowlands while both are not applicable to model backwater effects in the river, on floodplains or other adjacent lowlands and backwater effects caused by control structures (sluices, pumping stations and tide gates).

- Reviewer Comment 8:

“Section 4: I found the method difficult to follow because it is split over several sections and the supplement. If I understand correctly when the downstream level exceeds an upstream level volumes of water are moved to the upstream cell in increments of Wmin until the excess height downstream is less than Wmin? Water can be further rooted onto floodplain storage (linked areas) via the same method in a sub loop. If this is wrong then I haven’t understood the method! “

Answer to comment 8 (Part 1):

Your description of the conceptual method is fine.

“*Section 6 seems quite critical to the method to me so it is a bit odd that its not in the main text, but I’m happy to listen to justifications of why this should be in the supplement. What I think is missing here is a description of the hydraulic assumptions being made and how these might differ from reality. I think the main assumption is that the backwater profile is flat (termed “final balanced stage” in the text I think) and what this means is that as the water level downstream increases the components upstream progressively become part of the same flat pond or bathtub. How does this differ from the hydrodynamic backwater effect? Does this mean that any tidal signal will be instantaneously routed upstream rather than propagating like a wave? I think this is fundamental to any discussion around general applicability.”*

Answer to comment 8 (Part 2):

Thank you for your opinion. Our purpose was to simplify the reading flow of the main text and thought that the mathematical description of the method interrupts the flow of reading. After your comment we see that providing the mathematical details right away in the text is more important. We will move the mathematical details of section 5 and 6 of the supplementary back into the main text. The text is added were the references were given.

- Reviewer Comment 9:

“The method seems pragmatic and sensible to me, but I’m not sure I fully appreciate the assumptions and limitations relative to a shallow water wave simulation and where this method might become inaccurate. Could be added?”

Answer to comment 9:

We agree in pointing out the limitations and assumptions of the proposed conceptual method in more detail. This text will be given in an additional chapter (7) and the text in the summary (chapter 8) will be reduced.

- Reviewer Comment 10:

“Line 333 “The compiled code is freely available at http://kalypso.wb.tu-harburg.de/downloads/KalypsoNA/ and the source code of the modified part of the model presented in this paper can be provided upon request to the corresponding author.”

This doesn’t fit with the journals code availability policy. Code is recommended to sit in a repository such as zenodo. It’s also duplicated at the end of the document so could probably be removed at this point in the text.”

Answer to comment 10:

We totally agree in that. The preprint was published on 4th of May and the code was sucessfully published as open source code under the following link in the TORE system of our university: https://doi.org/10.15480/882.3522 at the same time. But the GMD procedure didn’t give us the possibility to adjust the text and add the link.

- Reviewer Comment 11:

“Line 408: “(1) applicable to model complex drainage systems in tidal backwater affected lowlands,” The application is to one test case and the backwater profile is assumed flat. I don’t think this is sufficient to claim applicability to all complex drainage systems – especially those with greater tidal ranges and long backwater profiles. The authors might disagree but I think this need to be a more nuanced conclusion recognising potential limitation of the approach and the summary needs to include a critical view on the limitation of the method.”

Answer to comment 11:

Thank you for the comment. We agree in the need to work out a more nuanced discussion and conclusion. As described in the answer to comment Nr. 9, we will work out an

additional chapter 7 to point out the limitations and applicability of the proposed conceptual method.

- Reviewer Comment 12:

Line 410: "(3) open for further model development" depends on code availability section, don't claim if not open."

Answer to comment 12:

As explained in the answer to comment nr. 10: the availabilty of the code is given since May 2021, but it wasn't possible to change the text in the preprint.

- Reviewer Comment 13: (overall comment)

"This article presents a new method for including a simple backwater effect in hydrological models that might act as a quick substitute for full hydrodynamic simulation in some lowland systems. The study is well motivated and generally well presented. The methods get a bit tricky to follow in places due to being split between the supplement and main text, but the authors might have good reasons behind this.

The performance of the new model is evaluated for a test case on the Dove-Elbe and shows promising results. My only significant issue is that the assumptions made by the method relative to taking a hydrodynamic approach are not really discussed in detail and the conclusions thus find that the model is generally applicable to all lowland settings and scales – I think this is unlikely to be the case. "

Answer to comment 13:

Thank you for the comments, remarks and description of your point of view. We agree in your comments and are open for improving our preprint. The requirement for a more nuanced conclusion and a more detailled discussion about the limitations of our conceptual method will be added in an additional chapter. (see answer to the comments: 9 and 11)

As described in the answers to the comment nr. 8: Two paragraphs about the mathematical description of the method are shifted back from the supplementary to the main text.