Geoscientific Model Development Discussions

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Comment on gmd-2021-140

Anonymous Referee #2

Referee comment on "Computation of backwater effects in surface waters of lowland catchments including control structures – an efficient and re-usable method implemented in the hydrological open-source model Kalypso-NA (4.0)" by Sandra Hellmers and Peter Fröhle, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-140-RC2, 2021

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. Specifically I have the following comments.

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.

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.

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.

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.

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

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.

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

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! 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.

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?

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.

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.

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