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Reply on RC2

Simon J. Dadson et al.

Author comment on "A reduced-complexity model of fluvial inundation with a sub-grid representation of floodplain topography evaluated for England, United Kingdom" by Simon J. Dadson et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-60-AC2, 2021

Author response to Comment on hess-2021-60; Anonymous Referee #2

We thank the reviewer for their positive remarks on the presentation of the paper and for their constructive comments on how the work can be improved. We set out below (in bold) how we propose to make the suggested changes in a revised submission.

The paper is well written, covers a large literature review in the hydrology field, but according to me completely fails in providing a convincing motivation of the hydrodynamic structure of the proposed model. More specifically:

1) It is not clear if the model is a 1D or a 2D model. Eq. (3) is the continuity equation of a 1D model, where the flow along the direction normal to direction xi is zero. The same holds for the momentum equation (4). If Eqs (3) and (4) hold for direction x1, they cannot hold for direction x2. On the other hand, authors adopt a regular structured grid, with grid size of 1 km.

The model is a 2D model and we thank the reviewer for pointing out some oversimplifications in the way in which we described the local inertial approximation in the original submission. The original paper gave the model in abbreviated index notation and referred the reader to several papers in the literature where its details can be found. In the revised version we will include a full derivation in two dimensions including details of the numerical scheme.

2) I assume the water depth is updated at the new time level from the finite difference approximation of the continuity equation (3), but this is not discussed in the paper.

Yes, that is correct and we will add this step to the explanation of the model physics in revision.

3) If Eq. (4) is the momentum equation along the xi direction of a 2D model, the 3rd resistance term on its l.h.s. must be written in the form:

[Equation]

otherwise it depends on the grid orientation. If Eq. (4) is the momentum equation of a 1D model, the approximation of the hydraulic radius with the hydraulic depth is a very strong one, also because the channel width is a very arbitrary choice.

Yes, exactly. The resistance term requires the magnitude of the vector water flux to be calculated. This is denoted |q| in the original paper and the reviewer is correct to note that this has been computed as the vector norm. The approach that we have adopted is explained in Almeida et al., which we cite. We will give full details and a complete derivation in the revised manuscript.

4) The choice of a zero convective inertia model should be discussed against other possible approximations. It is known that the error of the zero convective inertia model is larger than the error of the zero model ([1]-[4]). A trivial example is the front of a sharp shock wave, where the local inertia is positive, but the convective inertia is negative. In this case to neglect only one of the two components is worse than to neglect both. The advantage of the zero inertia model is that it allows an easy solution in the case of small water depths, but there are also other options that can be applied for fully diffusive models ([5]-[6]).

We thank the reviewer for these suggested references. We proposed to include an additional paragraph discussing the motivation for our choice of model in the revised submission, in which we will refer to the suggested papers.

5) The authors carry on a sensitivity analysis of the model results for the choice of the Manning coefficient and of the channel width, but they should do the same also for

the topographic elevation z. Because the adopted value, in each computational cell, is the mean elevation computed over a 1kmq area, I assume that both the averaging technique and the measurement error lead to a very large uncertainty.

We thank the reviewer for the suggestion to investigate topographic uncertainty - it would be a useful and informative addition to the paper. We therefore propose to add a substantial new section to the manuscript in which we investigate the effect of the fidelity of the sub-grid topographic representation alongside the hydraulic parameters and quantify the associated uncertainties.

1) Perumal, M., and K. G. Ranga Raju (1998a), Variable-parameter stage hydrograph routing method. I: Theory, J. Hydrol. Eng., 3(2), 109 – 114.

2) Perumal, M., and K. G. Ranga Raju (1998b), Variable-parameter stage hydrograph routing method. II: Evaluation, J. Hydrol. Eng., 3(2), 115 – 121.

3) Ponce VM, Li RM, Simons DB. Applicability of kinematic and diffusion models. J Hydraul Div, ASCE 1978;104:353–60.

4) Ponce VM. Generalized diffusive wave equation with inertial effects. Water Resour Res 1990;26:1099–101.

5) Sinagra, M., Nasello, C., Tucciarelli, T., Barbetta, S., Massari, C., Moramarco, T. A self-contained and automated method for flood hazard maps prediction in urban areas (2020) Water (Switzerland), 12 (5), art. no. 1266.

6) Aricò, C., Filianoti, P., Sinagra, M., Tucciarelli, T. The FLO diffusive 1D-2D model for simulation of river flooding (2016) Water (Switzerland), 8 (5), art. no. 200.