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Reply on CC1

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-AC3, 2021

Author reply to comment by Wing et al.

We thank Oliver Wing and colleagues for their short comment. The comment is helpful because it allows us to justify the scale of the approach that we have taken in this study. We take this opportunity to restate that the principal use case for this model is as a component within a land-surface climate modelling system where there is a pressing need to simulate flooding at our chosen resolution to correctly specify hydrological fluxes at the land-atmosphere boundary. Despite the many high-resolution datasets available in the UK, which of course underpin finer-resolution limited-area modelling applications, there remains a need for rapid, large-scale assessments for situational awareness in times of major flood.

The datasets used to constrain hydraulic geometry and for flood depth estimation were chosen for consistency with the benchmark validation data. In parallel work we are collating a large quantity of more recent data into a usable open-source form to support future analyses.

Performance metrics are presented in good faith at the scale of the analysis. We acknowledge the helpful suggestion to compare flooded fraction directly and will include such a comparison in a subsequent revision.

Below, in bold, we respond in more detail to the various points raised.

The Bates & Neal Flood Lab, part of the Hydrology Group in the School of Geographical

Sciences at the University of Bristol, reviewed this HESS Discussion paper during one of our meetings and provide the following comments that we hope are useful to the authors.

General comments

The general framing of the paper does not justify what place a model of this fidelity has in a country like the UK. Where metric-resolution inundation models with gauge-based flows, better parameterised channels, lidar terrain, and flood defences are already available, what is the need for a steady-state, 1 km, undefended model? Observations of flow, channel properties, elevation, and flood defences that are more accurate than the components used here are readily available for the UK.

The main motivation for this work is to test the ability of a newly-configured land-surface model component to simulate fluvial flooding. Its central purpose is to serve as one option within the JULES land-surface model, ultimately for coupled land-atmosphere-ocean simulation of flood inundation at 1 km resolution. Correct specification of the land boundary is important in such models because it controls the partitioning of water and energy fluxes at the surface.

Of course, we acknowledge the many excellent high-resolution datasets in the UK. We also acknowledge the existence of many important problems in flood hazard modelling at finer resolution. But, as we state in the paper, those are not the target applications for this study. We have taken great care in the manuscript to note that we do not expect our modelling approach to take the place of traditional fine-scale flood inundation modelling (p.2 lines 20-25) and will take the opportunity to strengthen those statements in revision.

The justification that simulations are quick does not outweigh the need for accurate models.

We agree. The purpose of our study is to ensure that our land-surface flood parametrisation is fit for its stated purpose. The trade-off in the present case is between speed of execution and spatial precision, not accuracy. Our study is intentionally structured to test whether such an approach provides the necessary accuracy at the scale for which it is designed.

The authors fail to discuss the merits of sampling from pre-simulated libraries of more accurate flood inundation maps when time is at a premium, for instance, or downscaling the 1 km model back to the native resolution of the DEM. The other justification that simplified models are yet to be evaluated fully is not the case. There is already a wealth of literature on the general inability of coarse and/or physics-lite models to replicate detailed validation data, some of which the authors themselves cite.

We appreciate the utility of simulation libraries and will include relevant references in revision. Such an approach would be unsuitable for use in a coupled land-atmosphere-ocean model though because the land boundary fluxes need to be updated at all model time-steps as part of the coupled simulation.

Channel and boundary condition configuration

We were unable to understand the treatment of channels in the model. In particular, whether estimated bankfull depths are "burned" into the DEM or retained subgrid. It appears that all channel variables are based upon a linear regression of ~30 observations collected 30 years ago. We question how representative such poorly constrained equations are for applications at national scales, particularly since there is no consideration of slope or discharge (the ultimate determinants of hydraulic geometry).

The extreme boundary condition is again based on a simple uplift of the same measurements from the 1991 paper, rather than any understanding of growth curves that the authors' own organisation sets out in the Flood Estimation Handbook. It is also unclear how this boundary condition is input to the model: by being steady-state, the model would struggle to simulate non-valley filling floods.

In a country as data-rich as the UK, there is little need to estimate these properties in the way the authors describe. The use of its rich network of river gauges and channel approximations based on discharge and slope would undoubtedly be a more justifiable approach than the one taken: it would not "introduce additional uncertainty" (P3/L15), it would decrease it. If the model could readily receive flows as input, as seems to be suggested through its intended coupling to JULES, how then would channels be parameterised?

Our rationale for using the original flood depth estimation procedure referred to in the 1991 study is to remain as close as possible to the method used to construct the benchmark. This is explained on p3. lines 13-14. The justification for this approach is so that errors and uncertainties diagnosed from our comparison can then be attributed to the structure of our model rather than to differences in the driving data used. During the development of this work, we did also use a later dataset produced by estimating flood discharges from flow records across the United Kingdom as part of the Flood Estimation Handbook. We will include a substantial section showing results from that analysis in the revised manuscript.

In parallel work we have collated additional width observations and we will include them in a revised version of this manuscript given the interest in updated width observations shown here and in RC1.

The model is not a steady-state model. It is a time-dependent model (see Eq 5), which is here evaluated for the steady-state case associated with the 100-year flood. Transient evaluation is amongst our planned future work pending acquisition of wide-area validation data.

To calculate river levels using statistically-estimated flows requires use of an additional flow resistance equation which adds uncertainty to the calculations. Channels remain fixed within the 1 km grid box and their properties are parametrised as described above.

Model validation

The model validation is questionable. Scaling up the high-resolution benchmark data to that of the coarse model is not a fair test of its skill – the appropriateness of low model resolution is partly what should be tested. Most channels and floodplains in the UK are <1 km wide, meaning (as is shown in Figure 9) the validation procedure simply discriminates whether channels exist in broadly the correct locations and whether water is input to them. To suggest that "these performance metrics are comparable with those obtained in previous studies" is disingenuous when such exercises exhausted the utility of the validation data rather than degrading it to fit the model. For the high-level conclusion to be 86% similarity to EA maps is patently false.

We disagree that the validation protocol is inappropriate for this study, given the 1 km resolution of our intended application. Validation metrics have been calculated according to equations given in the text and are presented in good faith at the scale of the study. We take seriously the important comment about the difference in resolution between ours and others' studies and will include clarification in revision.

The translation of inundated cell fractions to binary wet/dry grids with a very low threshold of detection (ε) is, again, a very forgiving comparison. It is not clear why the flood fractions are not just compared directly. A more useful test, however, would be to use the validation data at their native resolution.

Our stated aim was to evaluate a model of flooding at 1 km resolution (for the use cases indicated above). The suggestion to include direct comparison of flooded fraction is a good one which we will introduce in revision.

We are grateful to the discussants for their careful attention to our manuscript and for their helpful suggestions to improve the paper.