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Discussions

Interactive comment on "Impacts of spatial resolution and representation of flow connectivity on large-scale simulation of floods" *by* Cherry May R. Mateo et al.

Anonymous Referee #2

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This paper explores the impact of spatial resolution and flow connectivity on the prediction of flood inundation at basin scales. The aim is to explore whether increased spatial resolution improves the prediction of flood levels by global-scale river models. The paper tests this at one site (Chao Phraya River Basin, Thailand), and while the results show that increased spatial resolution can lead to improvements in the prediction of flood levels, this requires consideration of multiple connections in regions like river deltas. Overall, the paper presents an interesting result which would be of value to the community, and should be published after the authors have considered the comments below.

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Discussion paper

Specific comments

Only having one site means that extrapolating the result to global-scale models is more difficult. The authors could argue that the problem with single downstream connectivity in a model will impact many (if not most) floodplains globally, on the basis of 1 study site it is difficult to evaluate how common the problem will be. To reach this conclusion, a SDC and an MDC model need to be compared globally to see exactly how significant the conversion to an MDC model would be. The problem with doing this is that, as the authors state, calibration of the parameters of the MDC model is a tedious process. This will limit global applications of such a model. Ideally what is needed as the next step is a means of more easily (i.e. automatically) calibrating the parameter values. This could be by developing a way of doing this, or by reformulating the model to enable this to be done. This doesn't subtract from the significance of this paper. The paper clearly shows that in the case considered, the MDC approach is necessary in order to improve model predictions.

The authors mention that more detailed hydrodynamic models are still needed to model the behaviour of floods at smaller scales (line 21, page 11). The authors might want to comment on the possibility of the development of a hybrid approach, where the GRM result is used as a starting point (either boundary or initial conditions) of a finer scale model – possible at the end of that paragraph.

It would be good to see a sensitivity analysis of these model. How important are the sub-grid channel parameters in terms of the modelled output?

Lines 19 to 21, page 6: The authors might need to clarify this sentence a little. Does this mean that the CaMa-Flood values were adjusted to ensure that the catchment scale output matched the gridded runoff within the catchment? If so, how significant was this adjustment? Line 14, page 7: What are the confidence bounds in the values for Manning's coefficient? How do these confidence bounds impact on the model output? Is uncertainty in these values captured by other parameters?

Technical corrections

There are a significant number of typographical and grammatical errors in the paper (see the comments from the first reviewer for a fairly comprehensive list of these).

Line 28, page 3: I would suggestion “CaMa-Flood is currently the only GRM . . .”

Line 7, page 4: I think “can be answered” is a bit strong here (see comment above). Actually, all that has been done is investigate these questions from the point of view of a case study.

Line 14, page 5: I think “Yamazaki et al., 2014b” is the wrong citation here. This makes it seem that the “local inertial equation” was first presented in this paper. A couple of lines later, the authors refer to a 2010 paper that talks about it. Options are to remove the citation (already given elsewhere in the paper), or give a more appropriate citation to the “local inertial equation”.

Line 16, page 5: either “represent the backwater effect” or “represents backwater effects”

Line 27, page 5: either “the” or “a” before “bifurcation scheme”

Line 2, page 8: Are the objective functions evaluated at a site-by-site level, or are the aggregated across a number of sites. Judging by the rest of the text, it is done at a site level. If so, then there is a simple 1-to-1 relationship between NSE and RMSE, so using both would not give any additional information.

Line 3, page 8: Here the authors use “difference in discharge peak timing”. Elsewhere (e.g. line 23, page 8; line 33, page 9) they use “delay”. The authors should be consistent. In my view “difference” would be the better term to use as it is the difference in the delay in the peak that is being measured.

Line 33, page 9: change “time to peaking” to “time to peak”

Line 4 to 7, page 12: There is an apparent contradiction in these two sentences. The first says that the calibration of the sub-grid channel parameters is tedious – implying that this was a time consuming process. Later the authors state that the calibrated parameter values were found to be consistent and transferable across spatial resolutions – implying that calibration wasn't that difficult.

Line 2, page 13: change "it significantly declined" to either "this significantly declined" or "it declined significantly"

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