Response to the comments of Referee #2 on "Impacts of spatial resolution and representation of flow connectivity on large-scale simulation of floods" by Cherry May R. Mateo et al.

Note: This document provides authors' responses to the comments of Referee #2. We have formatted the comments made by the referee in italic and black-colored fonts and our response in upright and blue-colored fonts. To distinguish the modifications made in the manuscript from our response to the referee's comments, modifications are formatted in a different font type (Times New Roman). The page and line numbers within brackets or parentheses indicate the location of the modifications in the revised manuscript.

The revised manuscript as well as the manuscript with all revisions tracked are provided as supplementary documents. A document that provides our responses to the comments of the two referees is also provided as a supplement.

Response to the comments of Referee #2

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.

We would like to thank Referee #2 for thoroughly reviewing the paper and for giving constructive comments and suggestions which are useful for improving the paper.

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.

Thank you for recognizing the significance of the paper and for raising the issue about the extrapolation of our results to global simulation. We do recognize that a statement in the abstract about extrapolating our results to global flood simulations may be too bold. Referee #1 raised a similar concern and we have responded by revising a statement in the abstract to:

"While a regional-scale flood was chosen as a test case, these findings should be universal and can have significant impacts on large- to global-scale simulations especially in regions where mega deltas exist." [page 1, lines 24-25]

We argue, however, that because the mechanism behind our findings is physically-based, our findings should theoretically be applicable globally, but the impacts will be more evident in river mega deltas. The reasons for the decline in the capability of GRMs using *SDC* scheme to simulate flooding in floodplains and mega deltas in finer spatial resolution have been discussed in 6.1. We have added a few lines in the section to further improve the discussion (please see page 10, line 17 to page 11, line 5 of the revised manuscript).

As for the issue about calibration, we understand that the model still needs to be validated in other river basins, as we have pointed out in section 6.3:

"More extensive tests in large river basins and at global scale have to be conducted to further validate the model."

[page 13, lines 8-9]

While the CaMa-Flood model with *MDC* scheme has not been applied globally, prior to this study, it had been applied to the Mekong River Basin (Yamazaki et al., 2014b) and to the Ganges-Brahmaputra-Meghna Delta (Ikeuchi et al., 2015). In both studies, the *MDC* scheme proved to result in significant improvements in the simulation of flood inundation as compared with the model with *SDC* scheme, particularly by allowing the flow of water to smaller streams in the deltaic regions. These studies, along with the results presented in this study, suggest the importance of incorporating the *MDC* scheme in simulating floods, particularly in mega deltas.

The SDC version of the model had been applied globally in several studies (e.g. Yamazaki et al., 2011; Hirabayashi et al., 2013). Those studies, however, did not employ any calibration of the parameters due to practicability. Even without calibrating the model, those studies show that the model can be applied globally to allow the analysis of floods from a global perspective. As we have pointed out in the paper, the calibration of the model may limit its application in other large basins but there are several developments that may ease the difficulty of calibration such as the availability of a global width database of large rivers (GWDLR by Yamazaki et al., 2014a).

Thank you for bringing up the issue of automatic calibration. We have included a few lines in the discussion, section 6.3, to recognize the need for a calibration tool:

"One of the caveats of this study is the tedious calibration of the sub-grid channel parameters when applied to regional basins. At the global scale, this can potentially be addressed by the global width database available for large rivers (GWDLR developed by Yamazaki et al., 2014a). A database of channel depths of large rivers, however, do not exist; hence, the parameters characterizing the channel depths in the model may still have to be calibrated. To ease the difficulty of calibration, the development of an automatic calibration tool or a simpler or more efficient parameterization of sub-grid channels (e.g. Neal et al., 2015) may be helpful." [page 12, lines 28-33]

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.

Thank you for the really helpful suggestion. We have added the following at the end of section 6.2:

"To harness the benefits from using GRMs and catchment-scale hydrodynamic models, the development of hybrid approaches, where outputs from CaMa-Flood with *MDC* scheme are used as initial or boundary conditions of

catchment-scale hydrodynamic models, may be developed and assessed in the future. Hybrid approaches using relatively simpler GRMs have been shown to be feasible in the continental to global scale mapping of flood hazards and risks in fine spatial resolution (e.g. Ward et al., 2013, Winsemius et al., 2013, and Dottori et al., 2016)." [page 12, lines 22-26]

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?

Related comment: *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?*

Thank you for asking about the sensitivity of the model to the sub-grid channel parameters. The sensitivities of the model to river bank height, channel width, and Manning's coefficient have been shown and discussed in Yamazaki et al., 2011. The sensitivity of the simulated river discharge and flooded area to the parameters are discussed in a full section in the said paper. To summarize the section, a deeper bank height, wider channel width, or smaller Manning's coefficient result in larger fluctuations and advanced peaks in the simulated discharge, and less flooded area. The topic is interesting but as it is already published, we decided not to include a sensitivity analysis in the current paper to avoid duplication and to focus the discussion on the impacts of spatial resolution and representation of flow connectivity in the model.

To briefly discuss the topic in the paper, we have added the following in the appendix:

"The model is quite sensitive to the sub-grid channel parameters. A sensitivity analysis done by Yamazaki et al. (2011) showed that a deeper bank height, wider channel width, or smaller Manning's coefficient result in less flooded area and larger fluctuations and advanced peaks in simulated discharge." [page 15, lines 28-31]

As for the confidence bounds in the values of Manning's coefficient, we have found that the value used in this study is close to values published in other studies. Published values for river channels are: 0.03 – 0.035 in Visutimeteegorn et al., 2007; 0.026 – 0.035 in Keokhumcheng et al., 2010; 0.03 – 0.055 in Tingsanchali and Karim, 2010; 0.03 in Hunukumbura and Tachikawa, 2012; 0.03 in Sayama et al., 2015; and 0.025 – 0.40 in Wongsa, 2015. While the estimated value used in this study (0.024) is a little lower than the values published in other papers, it should be noted that some of these papers lumped the Manning's coefficient for the river channels and floodplains. Three of these published studies used a separate Manning's coefficient for plains and/or floodplains: 0.05 – 0.07 in Visutimeteegorn et al., 2007, 0.062 in Keokhumcheng et al., 2010, and 0.35 in Sayama et al., 2015. The estimated value for the Manning's coefficient in floodplains (0.10) used in this study falls within the published ranges.

Although we did not include an uncertainty or sensitivity analysis in this paper, for the purpose of applying the model in other river basins or regions, we believe that such analyses on the model parameters may be worth doing in the future.

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?

Thank you for allowing us to clarify the sentence. As described in the text, we are using the same daily runoff dataset for all experiments. The runoff dataset is available at a spatial resolution of 5-arcminute grids (approximately 10k). As we move to finer spatial resolutions, there will be an increase in the

number of CaMa-Flood unit-catchments which will be found in an area covered by one runoff grid cell. To ensure that the mass of water between the gridded runoff inputs and CaMa-Flood is conserved, whenever the boundaries of a runoff grid cell do not match the boundaries of the CaMa-Flood unit-catchments, area-weighted averaging is used to determine the amount of runoff that will have to be routed in each unit-catchment. The adjustment is made at the beginning of the simulation process to make sure that water is not created nor destroyed before being routed within CaMa-Flood. No adjustments are made to the outputs of the model. We hope our modified sentence is clearer:

"To conserve the mass of runoff inputs, CaMa-Flood uses area-weighted averaging to distribute the coarse, gridded runoff among the unit-catchments in CaMa-Flood." [page 6, line 33 – page 7, line 1]

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 . . ."

Thank you for the suggestion. We have made the necessary changes in the revised manuscript.

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.

Thank you for pointing this out. We have toned down the statement to "With this verification exercise, we attempt to answer two fundamental questions with regards to large- to global-scale simulation of floods…" [page 4, lines 6-7]

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".

Thank you for pointing this out. We have changed the wording to "The latest and most efficient version of CaMa-Flood (Yamazaki et al., 2014b) which uses the "local inertial equation" (Bates et al., 2010…" [page 5, lines 18-19]

We moved the citation of Yamazaki et al., 2014b next to "CaMa-Flood" and cited the first paper that presented the "local inertial equation." We think that it is necessary to cite "Yamazaki et al., 2014b" here to clearly identify which version of CaMa-Flood we are using.

Line 16, page 5: either "represent the backwater effect" or "represents backwater effects" Line 27, page 5: either "the" or "a" before "bifurcation scheme"

Thank you for the suggestion. We have made the necessary changes.

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.

Thank you for this comment. The referee is correct, the objective functions are evaluated at a site-by-site level. It is also true that there is a simple relationship between NSE and RMSE, and we see the point being made by the referee – that it may be redundant to show both metrics. Using NSE alone may be sufficient to prove our point – that the predictive capability of the model slightly increase with finer spatial resolution in the *MDC* scheme and significantly decrease with finer resolution in the *SDC* scheme. However, in some of the sites (gauging stations) that we have analysed, the differences in NSE coefficients between simulation results from 10km to 1km resolutions are usually smaller than those in RMSE. For example, in Figure 5, the upward trend in the NSE of discharge in C13_MDC (about 5% change) is less evident than the downward trend in the RMSE of discharge in the site (about 10% change). Hence, we find it useful to show both metrics in the paper.

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"

Thank you for pointing this out. We have made the necessary changes and used "difference" consistently in the text.

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.

Thank you for pointing out the confusing parts of the paper. Similar with most models that have multiple parameters, calibrating CaMa-Flood is time consuming. The amount of time required for calibration significantly increases when we move from coarse to fine resolutions. In the test basin, we have found that the optimal parameters calibrated at a coarse spatial resolution (10k), do not significantly change when we move to finer spatial resolutions (as shown in the appendix and briefly mentioned in section 4). Hence, we can avoid the extra costs of re-calibrating the model at finer spatial resolutions (as the parameters were found to be transferable across spatial resolutions). Nonetheless, time and effort still have to be spent to calibrate the model at some point.

Hopefully, the modified statements are clearer:

"One of the caveats of this study is the tedious calibration of the sub-grid channel parameters when applied to regional basins. At the global scale, this can potentially be addressed by the global width database available for large rivers (GWDLR developed by Yamazaki et al., 2014a). A database of channel depths of large rivers, however, do not exist; hence, the parameters characterizing the channel depths in the model may still have to be calibrated. To ease the difficulty of calibration, the development of an automatic calibration tool or a simpler or more efficient parameterization of sub-grid channels (e.g. Neal et al., 2015) may be helpful.
In the test basin used in this study, it was found that the parameters calibrated at a coarse spatial resolution are transferable across finer spatial resolutions. This significantly reduces the time required to re-calibrate the model at finer spatial resolutions. Once the initial difficulty of calibrating the necessary parameters at a coarse spatial resolution is hurdled, CaMa-Flood with *MDC* scheme can be used for more realistic, consistent, and robust simulation of large-scale floods across varying spatial resolutions. It should be noted, however, that the *MDC* scheme of CaMa-Flood had only been validated in three test basins – Mekong delta (Yamazaki et al., 2014b), Ganges-Brahmaputra-Meghna delta (Ikeuchi et al., 2015) and Chao Phraya River Basin in Thailand (this study). It should also be noted that the transferability of calibrated parameters from a coarse spatial resolution to finer

spatial resolutions have to be validated in other river basins. More extensive tests in large river basins and at global scale have to be conducted to further validate the model." [page 12, line 28 – page 13, line 9]

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

Thank you for the suggestion. We have made the necessary changes in the revised manuscript.

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