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Hydrology and Earth System Sciences Discussions

Interactive comment on "A framework for global river flood risk assessments" *by* H. C. Winsemius et al.

Anonymous Referee #1

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The authors presenting a well written and interesting study dealing with the scale problem in flood risk assessments. While hydrological analysis can be scaled from the hillslope to the presented global scale, this is not feasible when flood hazard and risks are to be assessed, as the authors correctly state. Relevant local scales have to be used for this purpose. The proposed model chain and downscaling are certainly a step towards flood risk assessments on a large scale reconciling the different scale demands. In general terms the proposed framework is appropriate, in particular the applied simple volume distribution for the estimation of inundation extend and depths for a global scale application. However, there are some points were the study, in particular the validation, is not very convincing. These issues should be addressed by a moderate revision of the manuscript.

1. Validation of hazard/inundation modeling

The presented validation for Bangladesh is not really convincing. If it is assumed that the maps are comparable, one actually would have to reject the simulation, as the DFO map shows from a visual inspection an inundated area of almost the double extend compared to the GLOFRIS simulation (Fig. 5). I would also disagree that the simulated maps show the inundation along the major rovers appropriately. In particular along the Brahmaputra I cannot support your argumentation. However, as you correctly state, the comparison is not really appropriate for validation, as it compares your empirical 30-year hazard map with the maximum observed inundation extend, for which the time period of observation or an estimate of probability of occurrence is not given. This comparison does not lead to a meaningful conclusion. In order to show the capabilities of the hydraulic scheme (the downscaling, which is the essential feature of the study), you should conduct an event based comparison, e.g. comparing the simulated flood extends for the floods of 1998 and 2004 in Bangladesh with the observed inundation extend of these floods. This is a standard procedure in hydraulic model calibration/evaluation and would yield a much better and meaningful evaluation of the efficiency of you hydraulic scheme. In order to underline the applicability of the proposed method in applied studies beyond this feasibility assessment, the validation of the hydraulics should be improved. Of course, this does not yield any information on the validity of the probability of occurrence. But this is a) a different story and should be treated separately, and b) is hard to achieve anyway, even in small scale applications. Additionally an improved validation of the hazard aspect would improve the final risk assessment and enable a better validation of the uncertainty/validity of the vulnerability and exposure side of flood risk. Currently this is impaired by the weak validation of the flood hazard analysis.

2. Time period modeled

I am wondering why you did not use the full ERA40 data set? This would give you 45 years of simulation results (1957-2002). This would have two major benefits: The statistical significance of the probabilities of occurrence is better, at least for the approximately up to 30 year events. And you could validate the inundation modeling against DFO event based flood images. As far as I can see there are some floods in 2000 and 2002 covered, but maybe you can also get images of the 1998 flood. One could also think of combining the ERA40 with the ERA-Interim data set to obtain long time series of inundation. In any case the restriction to 30-years of simulation should be explained.

3. Volume to water level

Section 2.2.4 deals with the important step of downscaling the hydraulics. However, the essential step of deriving water levels from the flow volumes of the grid cells is not described (last paragraph). As this is critical for the determination of the inundation extend and depths, this has to be corrected in the revised manuscript. Only if this is given along with an improved validation of the inundation simulation the feasibility of the proposed approach can be judged. Also, the authors should mention that this method does not consider hydraulic gradients along river reaches. As far as I understand a single water level is derived for the outlet of a model cell, and the DEM depression upstream are filled up to this level without considering a flow gradient. For large lowland rivers this effect might be negligible, but the authors should include a not and best also an estimation of this effect (e.g. compare the expected rise in upstream water levels from a typical lowland river flow gradient to the cumulative elevation curves or the expected error in elevation of the DEM). The last paragraph should also include a note on if and how connectivity is considered in the DEM-filling algorithm.

4. Use of the term “bias”

In section 2.2.4 the authors use the term “bias” in a way that easily leads to confusion or misunderstanding. More appropriate would be the term “error”, as a bias is defined as the difference of the means of two time series, i.e. a systematic error in a broader sense. However, in any kind of modeling and particularly in hydrology the differences between the observed and modeled system behavior is typically more than or not only

a difference in means. The wording in the manuscript should reflect this.

5. Consideration of embankments

The authors correctly state that embankments such as dikes alter the natural inundation dynamics and change the flood hazard. Thus I would ask them to elaborate more on how they would include the effects of dikes on a global scale analysis, firstly from a technical point of view, and secondly from a practical point of view. The first aspect is already involved in the assumption of non-impact floods (section 2.2.4), but it needs more emphasis in the relevant section and also conclusions. For the second aspect the authors should give some statements of how this local influencing factor can be considered in a global analysis. Typically embankments/dikes are not represented in global elevation models, thus a strategy has to be defined how to deal with this problem in practice.

6. Empirical vs. estimated probabilities

Currently the authors work with empirical probabilities of exceedance derived from the short 30-year period of simulations. In flood risk assessment typically extreme value distribution functions are fitted to the time series of observations enabling an extrapolation towards less frequent and likely events, although with quite some uncertainty. A similar procedure could be followed within the proposed framework by automatically fitting distribution functions to the time series of annual maximum flood volumes per grid cell. This is quite a computation al effort, but in comparison with the modeling performed already and with modern computational facilities this can be done automatically without supervision in practically no time. I would thus ask the authors to comment on this option and the consequences that could be drawn. I would argue that the statistically derived probabilities of occurrence are more bust than the simulated empirical. And additionally extrapolations toward less likely global flood events would be possible, although with uncertainty. Nevertheless, this could be an extension of the scope and applicability of the framework.

7. Conclusion on use of study outputs

I would appreciate if the authors include some statement on the use and applicability of global scale flood risk analysis in the conclusion. Who would profit from this analysis in which way? Would is the benefit with respect to more regional or local flood risk assessments?

In Addition to these general comments I made some minor remarks directly annotated in the attached pdf of the manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C4792/2012/hessd-9-C4792-2012-supplement.pdf

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