**I have really enjoyed reviewing this interesting paper by Pappenberger et al. dealing with global flood hazard maps. The paper is well written and the numerical exercise scientifically sounded. Also, I like the idea of producing consistent maps for the entire globe. This consistency in mapping flood hazard can be a first step, for instance, in start understanding flood risk. Anyhow, given my enthusiasm for this topic (e.g. Di Baldassarre et al., 2011; 2012), I have some comments.**
*We thank Giuliano for his encouraging opening and enthusiasm for this topic.*

**My main concern is the fact that this method was not validated against hard data, but only compared to another attempt to produce global flood hazard maps. Although the authors do recognize this limitation, I think that too much emphasis was given to this benchmark study, which (I feel I must point out) has never completed a peer review process**

*We acknowledge that the paper by Herold and others (2011) was never revised and thus never published in HESS. However, we also believe that the results of the paper do have considerable value and indeed the maps are currently used for decision making as well as in the operational management of flood hazard and risks (we will quote more reverences in the revised manuscript). Therefore, although it could be argued these data do not hold the total scientific rigour, they are de facto benchmarks and thus we would argue that one has to compare any results to them. Indeed, the open HESS-D review process allows any individual to make their own judgement on the quality of the paper thus we feel it justifies the extensive usage in this context. We know of no alternative global data set which provides similar information. We have not been able to access any regional data sets which are consistent across the spatial and temporal scales (and are large enough to make a comparison statistically meaningful).*

**Secondly, I see room for improvement in the rationale about the usefulness of global flood hazard maps. As I mentioned, I really like the idea of consistently estimating flood hazard at the global scale as this would improve our understanding (rather than "managing") of flood risk. However, this paper seems to underestimate this potential scientific relevance (e.g. understanding global links between flood hazards and global changes, human impacts on flood hazards, etc...), while overestimating the practical usefulness of these map for risk management, which in reality will always require local knowledge. For instance, it is stated that these maps can be used "for example deriving priority regions in which an upgrading of river defence structures may result in the highest return in terms of impact." Now, how can these maps suggest the need for updating defence structures when these maps are derived (according to the same paper) without considering the presence of defense structures? Also, how useful is (from a management viewpoint) such a coarse resolution map in most of the rivers of the world?**

*We partially agree in that those maps cannot be used for local planning and indeed we will provide an entire discussion point to it. The reviewer is right that there is some focus on getting an improved understanding, but we also believe that there is a far wider value. Planning is not just a local activity and in conjunction with risk a global overview will allow for broad regions in which investment (by for example an international body) has higher value. This by no means reflects the demands and pressures on local planning. We will add more discussions on the rational in the revised version.*

**Lastly, the paper seems to suggest that the implementation of physically based model cascades from precipitation to inundation) is the most appropriate approach to mapping flood hazard at the global scale. For instance, the word "successfully" is used twice in the sentence starting at the end of page 6617, without a clear explanation of these successes. In my opinion, if the final objective is to estimate inundated areas for large return periods, simpler approaches such as the mplementation of probabilistic envelope curves (see e.g. Castellarin et al., 2009; Padi et al., 2011) should also be considered as possible alternatives. Essentially, it seems to me that this paper does not give sufficient credit to regionalization techniques, which have proven to be useful tools in estimating design floods in ungauged basins (e.g. Blöschl and Sivapalan 1997; Merz and Blöschl, 2008).**

*We are not aware of any successful application of regionalisation and estimations of return periods for the scale we are considering. It is true though that the references mentioned and the possibilities should be discussed and we will amend a revised version accordingly.*