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Interactive Comment

Interactive comment on "Uncertainty propagation in a cascade modelling approach to flood mapping" *by* J. P. Rodríguez-Rincón et al.

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We would like to acknowledge the prompt revision of our work entitled "Uncertainty propagation in a cascade modelling approach to flood mapping". We thank the reviewers for their constructive comments which very much helped to improve our manuscript. We have digested the two reviews and made considerable changes to our manuscript (please see the new version in the attached supplement pdf file). We made an effort to incorporate precise changes in the manuscript that acknowledged their comments, and are confident they considerably improved the quality of the final version of the paper. Please note that following the suggestion of reviewer two, the title was changed to:

Hydro-meteorological uncertainty propagation in a model cascade framework to inun-

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Interactive Discussion

Discussion Paper

dation prediction.

Based on the two reviews, the main tasks in the revision were to 1) address in more detail the sources of uncertainty in our work; and 2) to improve the interpretation of the results. In response we indicate our reply with an R:, providing line numbers in the new manuscript which is attached in the supplement file.

I hope this works proves to be of interest to you and the referees. I look forward to hearing from you in due course and take this opportunity to thank you for such a quick and efficient process of review.

Best wishes,

Adrian Pedrozo-Acuna on behalf of all coauthors.

——————— Anonymous Reviewer #1 (replies indicated with R:) ———————

This paper deals with the propagation of uncertainty from numerical weather prediction to flood mapping through a distributed hydrological model. The topic fits the goals of the journal. The paper is generally well structured, and overall sufficiently well written, although it does include a number of typos, difficult sentences or imprecise terminology (see e.g. predictions vs forecasts). However, the introduction gives a lot of emphasis on the potential impact of climate change on floods (which is actually much more complex than what the authors state in this manuscript) while the rest of the paper, as well as the conclusions, are not connected with this premise. Anyhow, I could not find any scientific novelties in this manuscript. It describes a mere numerical exercise with reference to a specific case. The exercise is too simplistic as too many sources of uncertainty in the model cascade are neglected (structure and parameters of the rainfall-runoff model, structure and parameters of the hydraulic model, etc...). This is a major issue because different parameterizations/structures, for instance, would likely lead to different propagation of uncertainty. Comprehensive methods in this field have already been developed, and abundantly described in the literature: Pappenberger et al., 2005; 2012; Di Baldassarre et al., 2010; Beven et al., 2011; etc... to only men-

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tion the few ones cited in this paper. In my opinion, this manuscript does not add anything, from a scientific viewpoint, to these studies. Also, this exercise is described without attempting to interpret the outcomes. What would a reader actually learn? In the abstract, one might find intriguing (though not necessarily counter-intuitive, and not entirely correct from a grammatical viewpoint) that “uncertainty do not necessarily increase within a model cascade”. However, this study does not demonstrate that. The specific outcomes only result from the strong assumptions made by the authors, and the absence of a rigorous and comprehensive uncertainty analysis (based on model results as well as observations) exploring the impact of the different sources of epistemic and aleatory uncertainty. These are the main reasons why I think that this paper lacks the "substantial contribution to scientific progress", which is a prerequisite for publication on HESS.

Beginning of reply: R1: We would like to warmly thank the referee for the observations and time spent during the thorough revision of our work. Following the referee′s general comments, the whole manuscript has been revised and thus some sections of the paper have been rewritten. This reviewer pointed out an apparent emphasis on the potential impact of climate change on floods. This appeared in the first paragraph of the manuscript Lines 2-7 Page 1. We have erased the climate change discussion and incorporated a detail description of the different sources of uncertainties that are associated to the flood risk assessment process (the new introduction now appears from Page 1 L2 to Page 2 L2). We believe that the framing of the introduction along these lines has helped to highlight the contribution of our work.

R2: In order to differentiate our contribution from those presented in the literature, which indeed are excellent pieces of work, we have also included in the introduction a brief description on the use of ensembles and multi-ensembles in hydrological forecasts (Page 4 L18-L25). However, it should be noted that the purpose of our study is not to characterize all the possible uncertainties there are in a model chain, but to study the error propagation from the meteorological model (due to model parameters), to the determination of an affected area. We believe that although simplistic, it is important to report how an error originated in the first model of the chain is propagated to a result of interest to decision-makers (flood map). Assuming that both hydrological and hydrodynamic models are indeed representing the system at its best. It should be borne in mind that there is, however, a basic problem in estimating the uncertainty associated with the predictions of models for real-world problems: there is no general agreement on what methods should be used to estimate that uncertainty. This is because there are too many sources of uncertainty in the modelling process that cannot easily be disaggregated when we can only evaluate model predictions against a limited number of observations. Thus, it is necessary to make assumptions about how to represent uncertainty, and there are sufficient degrees of freedom in doing so that different methods based on different types of assumptions (including purely qualitative evaluations) cannot easily be verified or rejected. This is the reason why none of the cited works have evaluated all possible sources of uncertainties, not to mention that most of them stop their model cascade at the hydrological level (e.g. streamflow, in contrast to the hydrodynamic level used in our study). Finally, we would like to thank the reviewer for pointing out the typos and sentences hard to follow. These have been revised.

R3: With regards to the interpretation of the outcomes, we have made an effort to provide a better discussion of the results. A new paragraph has been added in the discussion and appears in Page 14 L7-L30 as follows: "BIAS and NSC/TSS error metrics (Fig. 9) revealed discrepancies between observations and simulations throughout the model cascade. For instance, an increase in the NSC from the rainfall to the flood hydrograph has a double implication: first, it implies that the hydrological model is more sensitive (wider uncertainty bars) to its main input (precipitation) than the WRF model is to the set of micro-physics parameterisations. Second, despite such large amount of uncertainty, the ensemble of flood hydrographs is closer to the reality (high NSC) than the ensemble of hyetographs provided by the NWP model. On the other side, it implies that the hydrological model used in this study is quite sensitive to climatic forcing. Such attenuation in the error could be explained by the fact that the mean flood hydrograph obtained from the ensemble members is quite close to the measured hydrograph as shown in Fig. 6. This type of error and uncertainty propagation within the first step in the model cascade (a simultaneous rise in the model accuracy and uncertainty), suggests that the error in the hydrological model is reduced as a consequence of the non-linear rainfall-runoff transfer in the watershed. Whereas the error reached in the meteorological model may reflect a spatial scaling issue (comparing observations from rain gauges to simulations at the meso-scale) and thus widening the gap. The propagation of uncertainty and error from the hydrological model to the inundation area reveals a reduction in the uncertainty but also an increase in the error. This last modelling step is quite important given the consequences for issuing warning alerts to the population at risk. This work shows that the estimated inundation extent is strongly insensitive to the input flood hydrograph. While this can be explained by the limited effect that the volume overflowing the riverbanks and reaching the floodplain will have on the maximum inundation area, the difference between the observed and ensemble of the flooded area remains important (TSS=0.65)."

——————— Anonymous Referee #2 (replies indicated with R:) ———————

This paper presents an interesting case study application of uncertainty propagation through a model cascade consisting of a NWP model through a hydrological model to a flood inundation model. The paper is well structured and well referenced and provides an interesting read. The standard of English is acceptable, although the paper would benefit from a further proof reading. Each of the three modelling techniques applied within the cascade are found in current flood risk management practice and there is nothing new in their application to this problem other than the fact that simulations have been undertaken using multiple boundary conditions. On page 7988 the background to Mike 21(FM) is discussed and it is emphasised that continuity, momentum, temperature, salinity and density equations are involved. From my experience it is most unusual to use equations involving temperature, salinity and density in flood inundation simulations. Can the authors expand on why they are required in this instance? From my perspective the use of flood mapping in the title is confusing. The modelling approach is best suited to flood warning where one is interested in the prediction of flood inundation extents that allow emergency intervention to take place, whereas, flood mapping is normally undertaken for a specific return period flood for planning and risk analysis purposes. The key difference being that flood warning is best undertaken starting with a rainfall prediction from step 1 in the model cascade and flood mapping is best undertaken with a river flow input to step 3 in the model cascade. I would much prefer if the term inundation prediction were substituted for flood mapping throughout the paper. The paragraph on page 7979 discussing the growth in the frequency of flooding limits examples to the UK and Mexico. This is rather limited in scope and the authors should extend this discussion to other international examples. No information is provided on the computer resources used or model runtimes. This is an omission which requires correction. It remains the case that common application of uncertainty analysis is limited by the resource required to undertake the ensemble of simulations required. At the end of each step in the cascade the skill in model prediction is estimated through a variety of statistical comparisons with observed data. This is a worthwhile exercise however no account is taken of the uncertainty in the observed data and how observed data uncertainty varies from rainfall depth, to river flow to inundation extent. Indeed, no information has been provided on how river flow was measure in the field or how inundation extent was estimated. Presumably for the inundation extent were extracted from some remotely sensed data set however this process can include miss-classification of wet and dry areas of flood plain. The abstract concludes that "..the error associated [with] the determination of the runoff, is [shown] to be lower than that obtained for the precipitation estimation suggesting that uncertainty [does] not necessarily increase within a model cascade." This is an interesting point and I was hoping to read an analysis of why this might be in the conclusions, however, this point is discussed further by the authors, which is a significant omission.

Beginning of reply: R: We warmly thank the reviewer for a thorough revision of the manuscript. Indeed, we are certain that the consideration of his/her suggestions have improved our manuscript. All the suggestions have been taken into consideration.

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R2: As pointed out by the reviewer, we used multiple boundary conditions in the chain of models. However, it is important to highlight that we have also made an effort to characterize the behavior of different error metrics at each step within the model chain. We think that this exercise is also worthwhile and interesting in itself, as it provides a first step to the disaggregation of errors involved in the model chain.

R3: With regards to the description of the model including the equations of temperature, salinity an density, we concur with the reviewer with the fact that these were not used in our investigation. These where mentioned in order to provide a full description of the model capabilities. For clarity, and following this comment we have removed this description from the text (see Page 11 L13- L17).

R4: We concur with the criticism of the title of our study. Following this comment, we have made an effort to change the title to be more precise on what is actually presented in the manuscript. The new title of the manuscript reads now as: “Hydro-meteorological uncertainty propagation in a model cascade framework to inundation prediction”. The word “hydro-meteorological” has been included as not all uncertainties are considered in the analysis but only those associated to the prediction of rainfall with the NWP model.

R5: Additionally, as suggested we have also made an effort to include examples from more regions in the world not only the UK and Mexico. These are included in the introduction in Page 3 L5-L9. R6: Computer resources and model runtimes are now reported at the beginning of the Methodology and Results section. These details can be read in Page 6 L13-L17. In this sense, the hydrodynamic model was the most computationally expensive modeling step.

R7: It is certainly true that different sources of errors are also associated to observations with rain gauges. These are mostly ascribed to the quantification of the spatial variability of rainfall (Morin et al., 2006; Yilmaz et al., 2005), and errors in the mechanical operation or in the ability of the tipping bucket to catch the rainfall in the device,

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preventing raindrops from entering the gauge (Molini et al. 2005). Most of the time these errors affect the conveyance of rain into the gauge body, resulting in underestimation of the true rainfall by the gauge. These sources of error are important for quantifying rainfall accumulations over long times, but as pointed out by Molini et al. (2005), they have limited influence on rainfall intensity estimates, such as those used in this study. An acknowledgement of this is now included in Page 8 L12-L16.

R8: The last point of the second review, is also associated with the discussion of results and the interpretation of outcomes. As pointed out in the reply to reviewer 1, we have made an effort to provide a better discussion of the results. These appears in Page 14 L18- Page 19 L310.

Finally, we believe that the incorporation of all the comments from both reviewers have resulted in a better version of the manuscript. Therefore, both reviewers criticisms and comments have led to a clarified version of our contribution, for which we have included an explicit acknowledgement to both.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C4968/2014/hessd-11-C4968-2014-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 7977, 2014.