Dear Reviewer,

We appreciate the time and effort that you have dedicated to providing your valuable and constructive feedback on our manuscript. We have considered all comments and outline our response and the changes we propose below.

The paper analyses the propagation of uncertainty in the estimation of future flood quantiles associated with climate change in four Irish catchments. It uses the ANOVA method to quantify the relative importance of different sources of uncertainties, including the choice of the Global Climate Model, the bias correction approach, the parameterisation of the hydrological model to transform climate variables (rainfall and temperature) into flows, and the choice fo the extreme value distribution for the flood frequency analysis. The analysis is solid and the paper is clearly written and enjoyable to read.

Many thanks for these positive comments

The main finding is that the dominant sources of uncertainty vary between catchments. This is not a completely new result, as previous studies already showed that different uncertainty sources are dominant in different places (as also noted in the discussion, L. 390-395). In the discussion, the authors attempt at linking some of these differences to catchment characteristics and dominant rainfall-runoff processes in each catchment. However, a comprehensive quantification and evaluation of these links is left to future studies. This is a bit regrettable, as I think the identification of these links is not only the logical next step but also the most challanging and potentially most interesting for future climate change impact assessments - if some of those links could be identified, then one could give a priori indications on which uncertainties should be targeted/reduced first, depending on the characteristics of the catchment under study. This said, I still think the manuscript offers an interesting contribution and is worth publishing after some revisions. Below are some suggestions for improvement.

Thank you. We agree that these are the logical next steps from our findings. As outlined in response to Reviewer 1 we agree that in an ideal case we would require to have much more catchments to more fully interrogate the links between catchment characteristics and the uncertainty cascade. However, we also argue that research is always limited by resources and time. We have used the resource and time available to us to highlight important findings that can be used to inform future work that can more fully unpack these issues and therefore contribute to the science. Our research question aimed at examining the portioning and interaction of uncertainties considered. The link with catchment characteristics was hypothesized, and even though we have a small sample the results suggest that this may be a very fruitful way forward for the reasons you outline. We can unpack additional aspects of how and why doing this might be useful in our revisions. We will also add this point that by linking catchment characteristics to uncertainty sources, it can lead to less effort/time by indicating which uncertainties should be targeted/reduced first.

The title may be a bit more specific. "Flood risk" may be interpreted as relating to the product of hazard, exposure and vulnerability, whereas this paper only deals with the hazard component. Pereaphs referring to "flood frequency analysis" or "extreme flow magnitudes" in the title would help readers get a better idea of the actual paper content.

We agree and will revise to avoid use of risk.

I think the paper could be made more coincise, in particular the number of figures reduced. I am not sure that all of them are needed to support the key points the authors want to make. Below are some specific suggestions of paragraphs and figures that I think could be removed from the paper and possibly given in Supplementary Material.

Thank you for the suggestions and considered advice.

The Introduction could elaborate a bit more about the ultimate goals of the analysis and previous studies that had similar goals. On Line 69, the authors say "an important step ... is the development of techniques that allow the contribution...". I find this a bit confusing. First, it suggests to me that this paper will deal with "developing" some new technique for sensitivity anaysis, whereas the point here is not to develop techniques but rather use existing ones for a specific application - identify the dominant uncertainties of flood frequency analysis under climate change.

This is a fair point and we will elaborate the introduction with the valuable suggestions of papers from each reviewer and will also clarity in the aims that we deploy existing methods.

Second, it would be good to discuss here what is to be learnt from such sensitivity analysis, how SA results may inform future climate change impacts assessment. These implications were mentioned in the Abstract, I would have expected to read more on this here too, including the results of previous studies that looked at a similar problem in different catchments (some of these studies are cited in the Discussion on L. 390-395) or studies that used SA to identify dominant uncertainty sources of other hydrological variables. Indeed, that such sources may differ from catchment to catchment has been shown already although pereaphs in different context / for different purposes (see for instance about the variations in dominant parameters of a hydrological model across US cathcments: van Werkhoven et al 2007, Characterization of watershed model behavior across a hydroclimatic gradient, WRR, doi:10.1029/2007WR006271).

Thank you for the point raised. We want to be careful to highlight we do not use sensitivity analysis to identify dominant uncertainty sources. We present here two ways of analyzing uncertainty in future flood hazard, i) an additive chain, which is more similar to sensitivity analysis, and ii) we used ANOVA to separate the variance contribution of each main factor and their interaction. Werkhoven et al., 2008, work is pure sensitivity analysis on hydrological model parameter and hydroclimate regime using SOBOL approach. i.e, our focus is more on the assessment model outputs uncertainty that derives from an ensemble of four factors. However, we agree that sensitivity analysis helps to understand the contribution of individual factors to uncertainty in output. We will update this aspect of the introduction.

I do not understand what the "additive and multiplicative approaches to uncertainty estimation" are and how they work. These are mentioned for the first time on L. 240 ("unlike additive or multiplicative approaches to uncertainty estimation, ANOVA....") but no reference is given. I am familiar with SA literature (for example I know ANOVA) and I would not say it is self-evident what the authors are referring to! The results of this "additive" approach are reported in Fig 12. I am not clear how these resultes (the shaded areas) were obtained (Did you let each uncertainty source vary while keeping all others fixed to their 'reference' value? But if so, how did you choose the reference values? Or maybe you progressively added uncertainty sources one at the time? But if so, the order of adding uncertainty probably conditions your results?).

Either way: is this analysis really needed? From a methodological point of view, it seems less robust then the following ANOVA and from a practical point of view it lead to the same conclusions (unless I am missing something?) that the relative importance of the uncertain inputs is catchment dependent. So, does it need reporting?

thank you. It is more related to the third comment. We will add in our methods more detail about additive and multiplicative approaches to uncertainty estimation. The additive approach is the sum of the four selected factors without considering their interaction. The ANOVA is an excellent example of a multiplicative approach, which considered both the variance from the main factor and their respective interaction.

Similarly to previous point, I found the explanation of how PCI works and what it is used for (L.210-220) a bit too concise and hence unclear. On the other hand, I am not really sure mentioning it is really needed, as it does not seem to be used in the Results section 3.2. Please clarify or remove.

Thank you, we will remove this from the manuscript.

Other figures that the authors may consider moving to Supplementary Materials: Figure 6: is it needed? what for? Indeed text to comment it on L. 282-287 is not particularly informative. Figure 8: again, what's the point? It shows the ensemble median, so it hardly tell us about the spread of uncertainty, and it brings together past and future behaviour, so it doesn't tell us about the effects of climate change... So, what is the reader expected to learn from it?

We will remove Fig 6 and associated text and move Fig 8 to supplementary material.

The authors discuss this already, but I think it is really worth emphasising even more: not considering structural uncertainty in the hydrological model is a major limitation of this study. Results show that parameter uncertainty has small impact on the final output variability, but really we do not know whether that is because the chosen model structure already condition the range of flow predictions that one can get, and whether maybe those would significantly change with a different model structure. This may not be too problematic if the uncertainty about the model structure was small - in other words, if the authors had a strong justification of why that model structure is particularly adequate for the catchments under study. This could be further elaborated in the revised manuscript.

We agree and will further emphasise this point as a limitation in the study. We will also do more on examining the contribution of parameter uncertainty (see response to Reviewer 3).

Sec. 3.1: I got a bit lost and was not completely sure what the key point is here. It seems to me that, regardless of all details, the key point here is that it is not possible to declare one bias correction method to be the best at improving performances in all catchments, so it remains unclear how to choose one; but the choice matters as the projections for the future change quite a bit depending on the method employed.

Exactly, we will clarify this narrative.

OTHER SPECIFIC POINTS

We will address/clarify the following technical points.

Squaring in Eq. (10): is it needed? (I would think not)

L. 333: is it sensible that changes are greater in the 2050s than in the 2080s? Isn't it odd?

Figure 13: maybe could be made easier to read by inserting the return period in the circle and explaining acronyms in the legend

We will examine how this might be done, either directly in the figure or through a supporting table.

Reference

Werkhoven, K., T. Wagener, P. Reed, and Y. Tang (2008), Characterization of watershed model behavior across a hydroclimatic gradient, Water Resour. Res., 44, W01429, doi:10.1029/2007WR006271.