Responses to reviewers

Line numbers correspond to the original paper, not the corrected version. Responses are given in blue following each point.

Reviewer 1

General Comments

The overall concept of this paper is neatly done: a 12-member ensemble of baseline and future climate (12 km resolution) is input to a grid-based hydrological model (1 km resolution) to characterise the impact of climate change on flood events. The strength of the paper is in its focus on areal flood events, where the joint interaction between the factors that cause floods over a range of temporal and spatial scales is implicitly accommodated by the use of a gridded daily continuous simulation model. All inferences about changes to flood risk are made using 30-year sequences of daily floods, as derived from the 12-member ensemble of climate projections. Differentiating impacts by the areal extent and duration of floods of varying severity is novel, as is the exploration of possible changes in their spatial dependency.

There are, however, some aspects to this work which are potentially problematic, and these need to be addressed by further explanation and/or revision.

Specific Comments

The key issues that I am struggling with are as follows:

- It is difficult for a dynamically downscaled rainfall products to reproduce rainfall quantiles over the temporal and spatial (meso-) scales relevant to catchment flooding, and I was surprised to read (lines 62-63) that "due to the focus on … extremes rather than the whole regime in general" that no bias correction was applied. Bias-correcting projected extremes is as important, if not more important, than a central tendency measure. The rainfall-based simulation of floods is critically dependent on the correct representation of the frequency distribution of areal rainfalls, and I think it important to provide evidence that the frequency of areal rainfall extremes derived from the UKCP18 data compare reasonably well with observations. To this end, providing evidence that distributions fitted to n-day maxima extracted from UKCP18 (preferably for a range of areal extents relevant to the adopted spatial limits) are reasonably consistent with those based on observational data. I searched for any such evaluations in the Met Office documents (the citations provided for these need to be improved and corrected in the manuscript) but I could not find anything specifically relevant to the rainfall behaviour of most interest.
    - There are many issues and assumptions inherent in the bias-correction process, including the assumption that the same 'biases' seen in baseline climate model data are also present in data for future periods, concerns that correction can alter the spatio-temporal consistency of individual variables or break important physical relationships between variables, and the fact that typically-applied daily rainfall corrections can fail for multi-day rainfall totals (e.g. Ehret et al. 2012, Addor and Seibert 2014). The application of bias-correction can even introduce artefacts into the 'corrected' data (Maraun et al. 2017). Attempts to 'correct' rainfall extremes are especially difficult, as by their nature they have limited occurrence in observation-based datasets and are also strongly affected by natural climate variability (e.g. the well-known presence of prolonged flood-rich and flood-poor periods), whereas ensembles of climate model data will not necessarily present the same 'states' of natural variability through time. Thus application of bias-correction, rather than reducing uncertainty, represents a considerable source of uncertainty in itself (e.g. Lafon et al. 2013, Ehret et al. 2012). In this application we instead chose to use only the raw climate model data, to maintain the spatio-temporal properties of precipitation, temperature and potential evaporation imposed by the dynamic downscaling of the RCM. We then determine what constitutes an "extreme" level of flow by selecting a threshold based completely off the climate model runs, not observations and hence any bias in threshold selection is matched by bias in the events. The upshot of this is that the key features and results are not impacted by the bias.
- On the basis of the information provided it is difficult to be comfortable with the reported probabilities of exceedance (PoE). In concept the approach of adopting a merged CDF on the basis of empirical and fitted distributions is fine, my difficulty is with the inferred annual PoEs. I suspect that there is a problem with the way that the Poisson approximation is applied, and I suggest that the authors compare (or replace) their analysis with the more straightforward approach based on fitting the GPA distribution to the POT2 series, where the annual quantiles are obtained by the simple expedient of factoring the exceedance probabilities

by N/M, where N is the number of years in the record and M is the number of maxima extracted. The key reason for my discomfort with the PoEs reported is the severity of the identified events. For example, in Figure 2 it appears that 3 (possibly 4?) events with return periods of 1000 years have been observed in a single 30 year sequence. I appreciate the need to consider the influence of spatial dependency and the trading space for time issues here, but still, this number of extreme events is higher than expected (and higher than I suspect would be extrapolated by Tawn et al, 2019). A crude estimate of the likelihood of this could be obtained by estimating the notional number of largely independent catchments across the UK. If we adopt a spatial dependence limit of 120km (from line 220 in the paper) then the notional upper limit of the spatial extent of an event might be around 45000 km2, which yields around 5 or so independent catchments (or "trials") in each year. Given that the likelihood of a 1 in 1000 event occurring in a 30-year period is 0.029 (from the Binomial distribution), then there is about a 13% chance you would see a single 1000-year event in one of the five independent catchments somewhere across the UK in a 30-year period. However, we would actually need to have around 50 independent catchments in the UK to see three 1000-year events occurring in a 30-year period with any likelihood, and this corresponds to an asymptotic dependence limit of only around 40km, which is very low given the information presented in Figure 7. The number of exceedances shown in Figure 5 is larger again, but this may be due to how the ensemble members are combined (discussed in the next point).

  - This is a really interesting point, and quite an insightful way of estimating the number of very extreme events within a given period. The merged CDF is required for the copula method to be applicable, however, the empirical component of the distribution is not actually used in the figures since the threshold for using the GPa exactly corresponds to our threshold for delineating the event extents. A preliminary investigation suggests that your alternative greatly reduces the return periods of the most extreme events, reducing most of the >1000 year return periods to under 1000 years. However, we have a second paper in publication building on this work, and the authors feel that a consistent presentation of return periods across the two papers would minimize confusion. As we feel this is an important point to make, we will include a paragraph at line 156 outlining the alternative approach.
  - Line 156: "An alternative approach is to simply use $F_{GPA}(x)$ directly, and scale probabilities by N/M, where N is the number of years in the record, and M is the number of exceedances extracted. Due to the small probabilities involved, these don't line up for the largest values of flow. This approach leads to smaller estimates of return period, which might potentially align with discussion of the "frequency of 100-year events in the UK" (Tawn et al., 2019)."

- If my understanding is correct (lines 175-177), the 12-member ensemble from UKCP18 has been lumped together and used in the preparation of the results as summarised in Figures 3 to 7. I think this approach confounds the absolute interpretation of the reported frequencies and return periods, and I suggest that it would be more useful to treat each ensemble member as a source of aleatory uncertainty over a 30-year period. Thus, rather than reporting, say, that there are 17 events larger than 1000-year event in DJF (Fig 5) under baseline conditions, it would be more useful to report on the average (or median) frequency/quantile across the 12-member ensemble, where the highest and lowest ensemble member provides an indication of the upper and lower bounds of the sampling uncertainty in each 30-year period.
  - This is a good point. Aside from Figure 3 which is already split by ensemble member, we can easily include uncertainty bounds on Figure 4, and include some measure of variance in Figures 5, 6 and 7 by including three supplemental figures showing how the spread of the distribution changes between ensemble members using convex hulls of the underlying points on each figure. Including upper and lower bounds in addition would result in a lot of extra figures, or much more complex ones, at a cost to readability. At line 177 we replace the sentence "In the rest of this section, the event sets …" is replaced by "In the rest of this section, the event sets from all ensemble members are combined and given equal weighting. In the supplementary material, ensemble members are treated as separate sources of equal weighting. Ensemble members are shown in different colours and have the convex hull of the points from each ensemble member highlighted to show in particular variation in the extremes."
  - We also add three figures into a new supplementary material, corresponding to showing variance related to figures 5, 6 and 7.
  - Line 200: "Supplementary material figure 1 shows that there is some variability between ensemble members, particularly in the extremes, but the overall pattern is preserved throughout, as expected from Figure 3."
  - Line 206: "Supplementary material figure 2 shows that this pattern is matches between ensemble members, but there is some variability in the relative dynamics of duration and rarity in the extremes."

- Line 227: "This is also mirrored in Supplementary material figure 3, which shows this split by ensemble member, where spatial variation in coherence is strongly preserved between ensemble members."

- Lastly, no discussion is provided on how the asymptotic independence metric varies with distance (lower panel, Figure 7). I think the metrics used by Coles to explore asymptotic behaviour would benefit from additional explanation here as they are not intuitively obvious; specifically, the way in which the independence metric is defined is easily misinterpreted and without explanation it appears odd that the degree of independence is decreasing with increasing distance, which is exactly the opposite of what one would expect (and as shown in the dependency metric in the upper two panels of Figure 7, which is consistent with intuition).
  - This is a reasonable point to make. On the one hand, we do not wish to just repeat Coles, however we agree that intuition may be misleading. We edit the text at line 136 to the following: "…are calculated between pairs of points. For two points i and j,

$$\chi_{i,j} = \lim_{x \to \infty} P[Q_i > x | Q_j > x]$$

If $C^*(u,v) = 1 - u - v + C(u,v)$ for a copula C between two points i and j, then

$$\bar{\chi} = \lim_{u \to 1} \frac{2 \log(1-u)}{\log C^*(u,u)}$$

χ describes the level of asymptotic dependence, if $\chi = 0$ then the variables are asymptotically independent, otherwise they are asymptotically dependent. In the asymptotically independent case, $\bar{\chi}$ describes the dependence for large but not asymptotic values of flow. In the asymptotically dependent case, $\bar{\chi}$=1."
  - To comment on both panels of Figure 7, we change the sentence on line 220 to: "The figure suggests that asymptotic dependence decreases as distance increases. In the asymptotically independent case (Figure 7b), we see a similar pattern in dependence for large values of flow, with high dependence at short distances, even if they are independent in the limit."

Reviewer 2

The authors present a study on flood changes under climate change in the UK. The focus lies specifically on changes in modelled flood return periods based on an ensemble climate projection. The main point of analysis is the changes in widespread flooding. The authors find that there is more widespread flooding in winter and less in summer in the future projected climate. Further analysis included changes in return period, area covered and duration of events between current and future climate.

Overall, I like that the article focuses specifically on simultaneously occurring flood events under climate change. The analysis and results presented here show thorough, good work. I would have wished for a bit more focus on how the uncertainty of the climate ensemble translates into the results and more discussion of the results regarding potential drivers of change. While I have lots of comments and open questions, all of them are minor and should be quick to address.

Introduction

- Since a large part of your results section talks about spatial dependence, can you motivate this analysis in the introduction? Especially since several people have already written about flood coherence/synchrony (Brunner et al. 2020) including results for the UK (Berghuijs et al. 2019).
  - On line 39 we add the following: "… more frequent and severe. Spatial coherence of flooding events – whether flood timings at different locations have become more correlated – is of key interest to national-scale actions to mitigate the associated loss. The dependence structure of river flow has been analysed on a Europe-wide scape (Berghuijs et al., 2019) and focusing on the United States (Brunner et al., 2020), focusing on synchrony of events within a given range."

Methodology:

- Can you elaborate on why you chose the Grid-to-Grid model for this analysis and how well it performs in streamflow/flood prediction under the current climate? This would allow some estimate how reliable future projections might be.

- We will add some further information to line 74 about previous studies using Grid-to-Grid: "Grid-to-Grid has been widely tested and applied to explore climate change impacts on river flows across GB, for both floods (Bell et al., 2009, 2012; Kay et al., 2018) and droughts (Rudd et al., 2019; Kay, 2021; Lane and Kay, 2021). It has also been used by the English Environment Agency for flood forecasting (Price et al., 2012). "

- I like that you thoroughly elaborate on your choice of thresholds regarding POT and inundation extend. Can you supplement this with a sentence along the lines of "Widespread events are defined as…".
  - Agreed. We add the following at line 95: "We define widespread events as timepoints for which a large number of locations experience very high flow (i.e. above the POT threshold) simultaneously."
- Can you elaborate more on the method chosen for asymptotic dependence and, more importantly, elaborate on what that means? I have not encountered this method before, nor did I understand by the end of the paper, what it actually tells me. If you are interested in using an established method, I can refer you again to the papers by Brunner et al, (2020) or Berghuijs et al. (2019). Their results should also be discussed in line 254 since it relates to your proposed further work.

- The measures of asymptotic dependence are well established but often misunderstood. To alleviate this, We edit the text at line 136 to the following: "…are calculated between pairs of points. For two points i and j,

$$\chi_{i,j} = \lim_{x\to\infty} P[Q_i > x | Q_j > x]$$

If $C^*(u,v) = 1 - u - v + C(u,v)$ for a copula C between two points i and j, then

$$\bar{\chi} = \lim_{u\to 1} \frac{2\log(1-u)}{\log C^*(u,u)}$$

χ describes the level of asymptotic dependence, if χ = 0 then the variables are asymptotically independent, otherwise they are asymptotically dependent. In the asymptotically independent case, $\bar{\chi}$ describes the dependence for large but not asymptotic values of flow. In the asymptotically dependent case, $\bar{\chi}$=1."

  - To comment on both panels of Figure 7, we change the sentence on line 220 to: "The figure suggests that asymptotic dependence decreases as distance increases. In the asymptotically independent case (Figure 7b), we see a similar pattern in dependence for large values of flow, with high dependence at short distances, even if they are independent in the limit."
  - On line 254, we add the following: "…event length determination. Brunner et al. (2020) make use of a spatial dependence function (F-madogram) and hierarchical clustering to determine events for which points are mutually dependent to a sufficient degree. This would be an interesting direction to go in to improve event identification. To highlight spatial dependence in a simpler way than χ, Berghuijs et al. (2019) use a metric of flood "synchrony", measuring how often extreme floods occur at the same time within a given radius of a target point. The gridded data set we have available here could be evaluated using this metric, or one like it."

Results:

- There seems to be a mix between results and discussion in the results section (e.g. lines 184-190 are discussion, not results). You could either call the results section "Results and Discussion" or move any discussion from the results section to "Discussion and Conclusion". Generally, the discussion could be more elaborate (see below).
  - We call it Results and Discussion.
- You quite often talk about an "increase in the range", "little change", "less asymptotic dependence", "extend slightly". Can you support these statements with numbers?
  - We add percentage changes where appropriate throughout the results and the discussion, and add change in χ and χbar as well.
  - Line 197: "In the future time-slice, event duration seems to be slightly shorter on average, and this is more pronounced in spring (MAM) and summer (JJA) , reducing from 3.54 to 2.99 days in spring, 2.20 to 2.04 in summer."
  - Line 202: "The changes between the two time-slices are subtle, but there is an overall trend towards an increase in the range of peak return period: the 95th percentile of return periods increases in all

seasons, from an increase of 10 years in spring to 205 in summer, with the 5th percentile being ~1.2 in all seasons and timeslices."

- Line 220: "There seems to be little change in dependence between the two time-slices, although the asymptotic dependence appears to extend slightly further in the present time-slice (a maximum distance for which $\bar{\chi} = 0$ of 300km in the present versus 260km in the future). If the events are subdivided by season, subtle differences can be observed (Fig 8). Overall, spring and summer shows less asymptotic dependence (lower values of χ and $\bar{\chi}$) than autumn and winter. In spring and summer, mean χ is 0.641 and mean $\bar{\chi}$ is 0.222, compared to mean χ is 0.673 and mean $\bar{\chi}$ is 0.363 for autumn and winter. Also, the 50% contour for $\bar{\chi}$ is longer in spring (max distance of 495km in present, 545km in future) than summer (max distance of 431km in present, 462km in future) in both time-slices, suggesting that the variance in $\bar{\chi}$ exhibits seasonal variation."

- Line 205: "On the right of some panels (future winter and autumn) is a set of events with a peak return period of at least 1000 years." From what I see, all panels have events up and over a return period of 1000 years.
  - We change this sentence to "We see that in all seasons are a small number of events with return periods exceeding 1000 years, particularly in winter and autumn."
- Even though you use a climate ensemble as input data for the hydrological model, the presented results mostly do not give an overview of the uncertainty the different climate projections introduce. Can you please give an indication of how the ensemble spread demonstrates uncertainty in the results? Especially since you state in the abstract: "Results were consistent across ensemble members, with none showing significant difference in distribution." Since the two main conclusions are about the seasonal shift and spatial dependence, the results in Figure 3 are not enough to support this statement across all findings.
- Aside from Figure 3 which is already split by ensemble member, we can easily include uncertainty bounds on Figure 4. On line 186 we add: "Between ensemble members, variability is higher in Summer, even more so in the future time-slice, and one member actually saw an increase in summer events in the future time-slice (Figure 4, right)." A second panel is added to figure 4 to describe variability between ensemble members.

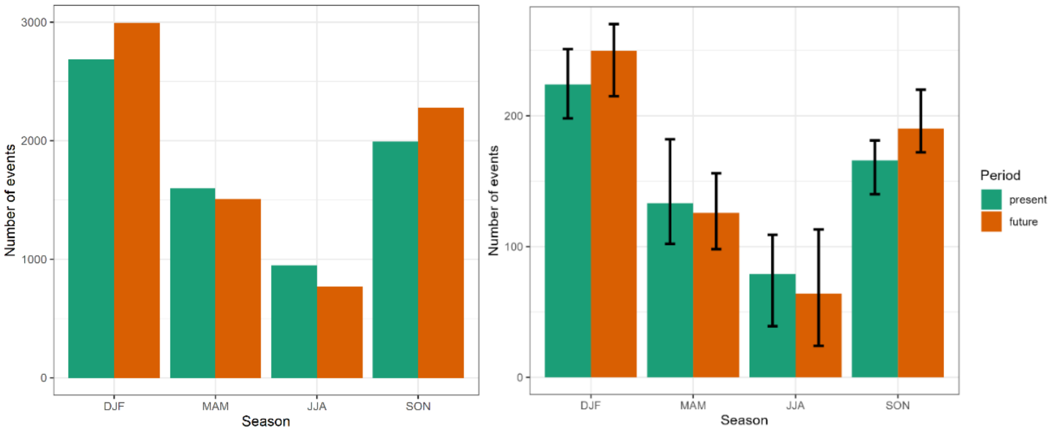

Figure 1 Number of widespread events, summed across ensemble members, split by season and time-slice. Left: total events, Right: mean number of events per ensemble member with error bars showing minimum and maximum across ensemble members.

- Including upper and lower bounds in addition would result in a lot of extra figures, or much more complex ones, at a cost to readability. At line 177 we replace the sentence "In the rest of this section, the event sets …" is replaced by "In the supplementary material, ensemble members are treated as separate sources of equal weighting. Ensemble members are shown in different colours and have the convex hull of the points from each ensemble member highlighted to show in particular variation in the extremes.
- Figure 2: Can you include in the caption what the percent inundation refers to? Is this percent grid cells or percent land area?
  - We add the following on line 160: "The percentages show refer to the percentage of the number of river grid cells, not a fraction of UK land area."

- Figure 3: I would prefer if you would present a summary figure for the different model ensembles. After all, since the ensemble runs represent uncertainty, only presenting, comparing and analysing individual ensemble members does not make sense.
  - As mentioned above, we added Figure 4, and three supplementary figures. However, Figure 3 is important to highlight that we have not erroneously included ensemble members which differ significantly from the others. The specific statistics are covered more broadly in the subsequent figures, so we feel adding to this information-heavy figure would reduce readability.
- Figure 4: Since you are using ensemble results, can you include uncertainty bars into the event count? Secondly, the caption says that you take the sum of all ensemble results. I would think that the mean or median (and potentially even the range) is the more appropriate measure. This is the case for Figures 5 and 6 as well.
  - See above for our response on this

- Figure 5+6: Is there a specific reason why you have return period once on the x-axis and once on the y-axis? If not, I would recommend choosing one or the other, not both.
  - This is a formatting choice. In Figure 5, duration is much narrower in range than return period, so the heat maps have a "vertically" stretched profile. Conversely, the key features in figure 6 vary more by return period, and so again, due to the number of panels, the order was chosen to keep the figures clear.

Discussion:

- Although the analysis itself does not focus on drivers of change, there have been several published articles on how hydrology and specifically floods are changing in the UK. I think the discussion would benefit from discussing the results of this study in the context of previous findings. For example, there is a projected increase in winter atmospheric rivers in the UK which are likely to bring widespread flooding (Lavers et al, 2013). Furthermore, floods in the UK are strongly associated with soil moisture timing (Blöschl et al, 2017). Do changes in the soil moisture influence in the increase/decrease of widespread flooding in the UK?
  - We will add a couple of sentences to the discussion (at line 244) to provide some context for our results, in terms of projected changes in precipitation seasonality, soil moisture etc., referring to Kay et al. (2022) which discusses soil wetting dates
  - Line 239: "This matches with some work done by Lavers and Villarini (2013) which shows the possible increase in atmospheric rivers, especially in Western Europe, which drive extreme precipitation events. The typical spatial extent of events was found to be fairly consistent between time-slices, but summer (June-August) events appeared smaller in the future across all return periods. Event duration decreased on average in all seasons between the two time-slices. This pattern was the same across all RCM ensemble members. Kay et al (2022) show that projections of soil moisture changes point towards wetter winters and drier summers. In conjunction with Blöschl et al (2017) suggesting that UK floods are closely linked to soil moisture timing, this gives confidence in the results in the present work"

General comments:

- There seems to be an issue with your referencing system. I found at least three references cited in the text to be missing in the reference list (Coles, 2001; Jiminéz Cisneros et al, 2014; and Paz et al, 2006). I did not check all of them, so there could be more. Furthermore, the reference list is not always sorted alphabetically (e.g. Robson et al and Rudd et al should be before Sayers et al) and some references do not start on a new line (e.g. Chen et al.).
  - These, and those mentioned throughout these responses have been edited in the references.
- Data availability: What is EIDC?
  - EIDC is "Environmental Informatics Data Centre". We have edited this sentence.
- There are missing spaces in lines 201, 223, 225, and 227, and an "s" missing in asymptotic in line 218.
  - We have corrected this.
- Line 181: There seems to be a word missing after "widespread".
  - The missing word is "events" which we have corrected.

Editors remarks

This is an interesting methodology with a focus on spatial floods. The reviewers have made useful suggestions, and from what I can read in the responses they are addressed well.

Additionally, I suggest that the authors emphasise the methodological aspects of the paper and downplay the findings of the simulations. The findings are surely of interest to flood managers in the UK, but HESS is an international journal, and for an international audience the methods are of much more relevance.

- This point around methodological relevance is indeed important to an international audience. We feel that the addition of the supplementary material, and the embellishment of several methodological parts (such as the definition of asymptotical dependence, and the issue of using a Poisson approximation for daily-to-yearly exceedance probability conversion) increases the emphasis on the methodological. The authors are somewhat reticent to change too much more as there is already a second paper (Adam Griffin, Alison Kay, Elizabeth Stewart, Paul Sayers; Spatially coherent statistical simulation of widespread flooding events under climate change. Hydrology Research 1 November 2022; 53 (11): 1428–1440. doi: https://doi.org/10.2166/nh.2022.069) which focuses on the methodological. We feel the abstract and conclusions do a lot to emphasise the method over the UK-centric implications, and demonstrate its applicability to a global stage.

Please submit a document detailing how you have addressed all comments along with the revised paper.

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