

## ***Interactive comment on “Inconsistencies between observed, reconstructed, and simulated precipitation over the British Isles during the last 350 years” by Oliver Bothe et al.***

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Received and published: 16 July 2018

Dear referees, dear editor,

Once more, thank you for your candid and helpful judgment, comments, and suggestions.

Let us start with a preliminary note. We will follow the advice by the editorial office and are not going to prepare a revised version right now but rather wait for the editor's recommendations. While a structure of a potential new version is clear and presented below, our replies to the referees' comments depend on whether the point in question

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will be included in a new manuscript.

Next we shortly repeat and slightly extend on our initial reply. Detailed responses to all comments follow below.

Intended changes in document:

Regarding the aims of the study we would like to point out once more that we a) aim to motivate the advantages in using the SPI for comparing various sources of information in paleoclimate research on precipitation and b) find for the case of a rather small domain on the British Isles that the sources of information lack consistency.

Our aim is not a comprehensive analysis of all available data for the British Isles. Our intention is to show for a small region the (in)consistencies between the various information sources. For the moment, we do not plan to include additional reconstructions or regions on the British Isles but rather to optimise our presentation of our chosen focus.

Your comments made it clear that we have to increase the clarity and improve the structure of the manuscript. Therefore we will rewrite abstract, introduction, methods section, and conclusions. This hopefully clarifies the motivation, the expectations, the aims, the methods, and the implications of our results. We will also improve the visual presentation. The results-section will require a profound revision, too, considering that we will likely add new analyses and remove some of the original analyses.

Regarding the results, the new version will concentrate on the analyses of the distributional precipitation properties, i.e., the SPI. We will add a comparison to further regional observational information sources. A manuscript asset, e.g., an appendix, is going to present shortly the Weibull distribution fits.

In turn, we will minimise the comparison between precipitation and temperature data and overall the analyses of temperature data. We aim also to regard the global simulations only in passing. Both parts will be moved to manuscript assets but these will be

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truly supplementary to the scope of our manuscript.

Detailed responses to all your comments follow below. Referee comments are put in red font and our replies in blue font. Intended changes in the document follow in default font.

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## 0.1 Referee 1:

Overall, I found this paper to be very confusing to read and also not surprisingly hard to follow. Also in this form I don't think that it is worth publishing.

Author reply: Thank you for your honest evaluation. We aim to remedy your concerns in a new version.

Intended changes in document: We intend to rewrite abstract, introduction, methods section, and conclusions. This hopefully clarifies the motivation, the expectations, the aims, the methods, and the implications of our results. We will also improve the visual presentation.

I think this is because it is attempting to do too much in a single paper. I would be more interested in one where the observations and reconstructions are compared, then a separate paper or a separate part of a single paper, where the climate simulations are compared.

Author reply: We aim at this point to mostly remove the global models from the manuscript. The discussion of the regional simulation is going to be included in a new version of the manuscript.

Intended changes in document: The discussions and analyses of the PMIP3-ensemble is moved to a purely supplemental manuscript asset.

It seemed to me that every time there was something interesting, the discussion went on to a discussion of the models and initially a list of the models and all the necessary details about them in terms of refs/names/resolution/forcing etc. It would have been better if all this latter part was in a separate Appendix.

Author reply: We are not quite sure what you are referring to. However, descriptions of the PMIP3-ensemble will be removed from the main manuscript.

Intended changes in document: Remove descriptions of the PMIP3-ensemble.

The basic premise of the paper is that regional scale precipitation (here for England and Wales) should show some impact of external forcing, but it seems that not surprisingly that internal variability dominates. Maybe the authors should spend some more time looking at long observational series, and less time looking at climate model simulation output.

Author reply: Most of the analyses are on reconstructions and observations. We will add comparisons on additional Met Office data. We will discuss our expectations more. Indeed our basic premise is shortly described as: we need consilience of evidence from all sources of information to reach a robust understanding of past and future climate variability and climate changes. This also involves external forcings. Agreement about forced and unforced signals may signal consilience. Internal variability is likely to dominate the mean signal on regional scales, the SPI-transformation allows to compare quantiles of precipitation data more easily as well as other precipitation distribution properties.

Intended changes in document: We will add discussions on the data we use by comparing to additional data. These are the subdivisions of the Met Office Hadley Centre England-Wales precipitation data, i.e. the data for South West, South East, and Central England. Additionally, we use the instrumental precipitation data from Kew Gardens and Pöde Hole. We will also specify our basic premises more clearly.

There are long precipitation records for the England and Wales region (including also the Central England Temperature series) and they have been analysed for a long time (i.e. there is a vast literature on analyses of these series, that this paper doesn't consider at all).

Author reply: We are not sure to which papers you refer specifically - or how they refer to the current analyses. We will however screen again respective databases in case we have overseen high quality and long data series over our region of interest and papers

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discussing them.

Intended changes in document: Some additional references may be added.

Figure 2 clearly realizes the seasonal nature of the reconstructions plotting seasons such as MAMJJ and JJA.

Figure 1 though appears to look at annual averages for CET. So when in Figure 4 running correlations are shown for CET with precipitation observations, drought/precipitation reconstructions what season is being used. Is this CET for MAMJJ or JJA or is it annual CET? I couldn't decide what it is from the text or the captions. If it is CET annual then this is wrong. CET correlates with England and Wales rainfall in winter positively (warmer winters tend to be wetter) and inversely in summer (warmer summers tend to be drier). So relationships change with the season. Need to specify for every season what season is being used, otherwise people will assume annual like I did. These relationships ought to be captured by models, which is what I think you say, but this is buried in text somewhere else.

Author reply: All analyses use MAMJJ except when explicitly stated that it is JJA. We will clarify this point.

Intended changes in document: The results section will be more clearly formulated, Figure captions revised, and connections between text and Figures optimised. We aim to specify the season for every analyses described in the text.

This is another problem with the paper, that there appears little structure to it and the text doesn't flow in a logical order, and there is no summary at the end of the Introduction of what to expect in each of the subsequent sections.

Author reply: We will try to remedy this structural issues.

Intended changes in document: The structure will be revised. We do not plan to add a redundant summary of following contents at the end of the introduction.

## Some specific points

1. SPI. Using a distribution for this is discussed. Whatever is chosen, the parameters ought to be compared.

Author reply: As clearly stated in the manuscript, we fit and use Weibull distributions. We will shortly present the parameters.

Intended changes in document: Manuscript assets are going to present the parameters of the Weibull distribution fits.

Tree-ring based reconstructions generally explain only a portion of the variance, so these are likely to have a lot less variance than the observations. This issue needs to be discussed.

Author reply: We will discuss this in more detail in the new version.

Intended changes in document: We are going to discuss the variance issue and how the original authors of the reconstructions rank this issue.

Comparison of series at SPI doesn't let the reader see the effects of the differences in explained variance.

Author reply: We are unsure on the point raised by the reviewer. The pure reconstruction series do not show this either in our original Figure 2.

Intended changes in document: As mentioned above, we will be more clear about the variance issue.

2. When you compare the reconstructions with England and Wales precipitation series in Figure 2, you seem to think that they will agree well. It is essential to look at how well SW England and also East Anglia compares with England and Wales. You can get the observed data here <https://www.metoffice.gov.uk/climate/uk/summaries/datasets> for periods since 1910. The correlations will not be as high as you imagine, partly because East Anglia is dry and also how the England and Wales series is put together.

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See the brief discussion in Kendon and Hollis (2014).

Author reply: As you may have seen, we obtained data from the Met Office homepage. We will clarify these points. England-Wales does correlate very highly with South-West, South-East, and Central England on interannual time-scales and also for lower resolution data. The reconstructions are much less related to the observational sets on both scales. We do not see the relevance of Kendon and Hollis (2014) for this discussion.

Intended changes in document: We will include a more extensive comparison to the observational data series.

3. There are odd bits of discussion almost on every page. On p7, why do you think the late-18th century dip in temperatures is due to the Laki Eruption? The references don't look at CET and the eruption did not put material in the stratosphere. I've assumed you're referring to CET as the paper is about this region, but the Laki eruption has been postulated as causing cooler weather in central Europe in 1784 and 1785, but as said this isn't very convincing at all (compared to say the eruption of Tambora in 1815).

Author reply: We will clarify these points. Especially, we will discuss why we think the high latitude eruption of Laki could have an influence on European climate. You state yourself that the effect has been postulated, which by itself warrants inclusion of this date. However, we have to discuss in more depth how likely the eruption may have had an impact on European and British Isle temperatures in an extended spring season.

Intended changes in document: We will discuss the inclusion of this date if the new version includes this discussion.

4. You should state that all you expect with the models for this small a region is to get the precipitation amount right. You would need ensembles of runs to see if any of the low-frequency might agree. You seem to be expecting too much, or you need to explain

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why you're expecting as much as you are.

Author reply: We will clarify our expectations. Indeed we absolutely do not expect to get the amount right. This is one reason why we think the SPI may help. If there is a forced response we may see it in the mean. Assuming the internal variability dominates the mean series, the SPI additionally allows to have a look at other properties of the precipitation distribution to see whether these may show a signal. Indeed, the suite of PMIP3-simulations and our regional simulation represent an ensemble.

Intended changes in document: We will more clearly discuss our expectations, why we think we do not need an ensemble of simulations, and why we in the end remove the global simulation ensemble.

5. As stated the text is difficult to follow. Much of p10 comes into this category. The bottom line of Figure 2 shows Weibull standard deviations, but what does this mean? Surely this is showing what I was talking about in 2? The whole running numbers are confusing. It doesn't help putting too many coloured series on the already quite small plots.

Author reply: We aim to provide a new version which is easier to follow. We will try to clarify all these points. The Weibull Standard deviation is the square root of the Weibull distribution variance as, e.g., presented at <http://mathworld.wolfram.com/Variance.html> (Weisstein, Eric W. "Variance." From MathWorld—A Wolfram Web Resource) or in a number of textbooks.

Running numbers allow displaying easily the changes in the distribution properties. We aim at improving the visualisations of the data.

Intended changes in document: Figures will be redrawn. Text will be clarified.

6. The opposite evolution in East Anglia and SW England might be correct (p11)? You need to look at the observations to check this. There is an outof-phase correlation between SE England and NW Scotland.

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Author reply: We are not quite sure how NW Scotland relates to our analyses, but we have checked with the observationally derived series. We will further discuss this point in the new version. A comparison between more data series will be included, but it doesn't change the point here.

Intended changes in document: We will extend on the discussion.

7. If series (p12) have the order of one degree of freedom, then what are you doing showing them.

Author reply: The new version likely will not include this analysis.

Intended changes in document: The correlation analyses will be mostly or even completely absent from a new version.

Parts of this page are very difficult to read and follow.

Author reply: We will try to clarify this.

Intended changes in document: We will rewrite the results section.

8. No seasons are given with Figures 4 and 5.

Author reply: We will clarify the seasons throughout the manuscript. It is MAMJJ except when we additionally use JJA.

Intended changes in document: Captions will be clarified.

9. Trouet et al (2018) would have done better to have used the 300-year long instrumental records from the British Isles instead of going straight to tree-ring reconstructions. There are large variations across the British Isles with the size of the influence of the westerlies on precipitation amounts in the spring and summer. For example (p16) the NAO has no influence on East Anglian precipitation amounts in the winter half year. The NAO effect is much stronger on the western and northern areas of Britain, and it is mainly in the winter season. When you talk about spring/summer and the NAO are

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talking about the same NAO as in winter? It would be useful to discuss how the North Atlantic Jet that Trouet et al (2018) talks about relates to the NAO, if it does?

Author reply: We will discuss the large scale circulation influence more extensively.

Intended changes in document: We will extend on the discussions of the large scale circulation.

10. P17 states that standardization of precipitation goes beyond comparing means and deviations. I'm not sure that you have shown anything other than just the means and SDs.

Author reply: Obviously, we disagree. We will clarify this point in the manuscript. Our analyses allows to compare the full distribution including measures that cannot be evaluated using the mean and the SD like the asymmetry of the distribution and its tails.

Intended changes in document: We will try to clarify the benefits of the SPI.

References Kendon M and D Hollis, 2014: How are UK rainfall-anomaly statistics calculated and does it matter? *Weather* 69, 37-39.

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## 0.2 Referee 2:

While I think that this paper has merit and could provide interesting insight it is my view that it is not yet ready for publication. I encourage the authors to rethink the structure and layout of the paper and the key messages to be delivered. I think that such a paper would be welcomed by the field and of interest to the readers of the journal. But to reach a standard for publication significant work remains.

**Author reply:** Thank you. We are going to restructure the manuscript completely and clarify the key messages.

**Intended changes in document:** We will restructure the manuscript to provide more focussed information on our main points.

From the outset the specific aims of the paper are rather vague; the introduction section needs clearer structure. At the moment it jumps from one topic to the next without really unpacking where the state of knowledge it at in any aspect. The authors need to structure the introduction much more clearly, building the necessary context for the reader to understand what the aims are and the summary of information necessary to move to the next stage.

**Author reply:** We will more clearly structure the manuscript.

**Intended changes in document:** Introduction and also subsequent sections will be more clearly formulated.

If the focus is on the British Isles why just use the EWP series and not the Island of Ireland monthly series from 1711 or the Scottish regional series. I realise the latter is shorter, but to talk of the British Isles and not use the other available regional series is confusing. Murphy et al. (2018) cited in the introduction show that CET is also strongly correlated, at least at decadal scales with the Irish series.

**Author reply:** The focus is a small domain on the British Isles, not the whole of the

Isles.

Intended changes in document: We will clarify our spatial focus.

Why did the authors choose these tree ring reconstructions? To the best of my knowledge these are based on ring width reconstructions which have been shown to be less reliable for precipitation. Why not incorporate the oxygen isotope reconstructions done by Rinne et al. (2013) for southern England. Indeed in their discussion, if I recall correctly, they identify interesting points of departure from both EWP and Kew precipitation series for the summer months. Again in providing this suggestion as I am reading it is not clear what the time focus is of the paper – spring/early summer, spring?? The study design needs clearer thought, signposting and explanation.

Author reply: We will clarify the seasonal focus of the manuscript and the additional points you raise. We will argue for not using Rinne et al. in this context. Among other reasons: to our knowledge the data from Rinne et al. is not publicly available. The focus of the manuscript is an extended spring season. We will ensure that this becomes clear everywhere.

Intended changes in document: We will clarify our scope.

Regarding the selection of ensemble members from model reconstructions, why not use the entire ensemble? In the next paragraph it is noted that the selection is rather arbitrary and it is assumed that the domain sufficiently represents EWP domain. Some kind of table to help the reader interpret the different forcings used would be helpful.

Author reply: We are not quite sure what the reviewer is referring to but we will try to clarify this. We agree that the selection of the domain within a simulation is in a way arbitrary. We are going to move the analyses of the PMIP3-ensemble to a purely supplemental manuscript asset.

Intended changes in document: As far as this point is still relevant to a rewritten manuscript, we will try to clarify this. Analyses of the PMIP3-ensemble will be removed

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from the main body of work.

The use of the SPI to investigate the 6.7 and 93.3 percentiles is a very stringent test of models and reconstructions is it not? The EWP is essentially a composite series and extremes are likely smoothed out. Also, is it a fair ask to expect climate model reconstructions to be able to represent these, especially if not employing a large ensemble? I am only asking out of curiosity here and would like to be informed of how stringent the comparison you are setting up is.

Author reply: We will discuss why we think the comparison of the distributions makes sense even for area average or composite series. By using distributions we essentially compare climate states which in theory should account for a part of the internal variability. Thus, assuming there is a common signal the evolution of the climatological properties could agree between data sets even without employing a large ensemble.

Intended changes in document: We are going to clarify the limitations and the stringency of our proposed method.

Any bias correction applied to the models? Does SPI negate this?

Author reply: No. We don't use statistically downscaled data. Bias-correction is not the scope.

Intended changes in document: We will clarify this.

Results presented in the methods section need to be moved.

Author reply: Will be changed.

Intended changes in document: Results from the methods section will be moved to later sections.

The paper is badly let down by plots that are very hard to decipher and methods applied that are not appropriately, or sometimes not at all, explained in the methods section.

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Author reply: We will pay attention to clarify the visual presentation and the methods used.

Intended changes in document: We will take care to describe all methods and to present the results more clearly.

Fig 1 – no detail of the types of smoothing applied covered in the methods. What is a ‘first impression’, not a scientific term. What CET time step is the smoothing applied to? Monthly or annual series. Why not plot as an ensemble rather than 11 sub plots? Line types in legend do not match the plots. Use of sunspot data is not covered in the data section so far as I recall.

Author reply: We will try to remedy all these points. We use a 51 point Hamming filter. We use the extended spring data here as well. We decided to use the 11 sub-plots since we regarded the ensemble plot to be even less visually helpful.

Intended changes in document: This Figure or a similar representation is likely going to be moved to a purely supplementary manuscript asset. Discussions of the sunspots will either be added or the data will be completely removed from the manuscript.

A table detailing the various data sources compiled is badly needed.

Author reply: We will present the used data in a clearer manner.

Intended changes in document: We will present the used data in a clearer manner.

The use of differing periods is confusing, how can this be comparative – which is the primary aim of the paper.

Author reply: We will try to be more clear in our thinking on how to compare the used data sets. However, we are unsure to what part of the manuscript this comments precisely relates.

Intended changes in document: The methods section will give more details on the comparisons.

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Please think about presenting results in a clearer way. I literally spent hours trying to figure out what the figures were showing and in many aspects am no clearer.

Author reply: Thank you for spending so much time on the manuscript. We aim to clarify the presentation including a clearer outline of the spatial, methodological and dynamical considerations of the manuscript.

Intended changes in document: The new methods section will clearer spell out what is shown later, and Figures will be optimised.

There needs to be a more systematic approach to this work in terms of presentation and some sub sectioning in the results and discussion to help the reader.

Author reply: We will try to lead the reader more clearly through our thinking.

Intended changes in document: We will try to structure the results-section more clearly and to more systematically direct the reader through the manuscript.

The title of the paper concerns precipitation. It is confusing to start the results off with temperature.

Author reply: Discussions of temperature will be minimised in a new version and not start the results.

Intended changes in document: The results section will be restructured.

I find it next to impossible to interpret the caption of Figure 2.

Author reply: We will clarify the presentation of Figures and captions.

Intended changes in document: Figure captions will be clarified.

It is difficult to comment in much depth on the nature of the results and the points made in discussion and conclusion given how difficult it is to decipher what was done.

Authors need to revise the structure of the paper to systematically consider the inconsistencies of interest.

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Author reply: We will try to make our points more clearly in a new version.

Intended changes in document: A rewrite of the manuscript is necessary.

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### 0.3 Referee 3:

**Summary:** The manuscript involves a comparison of climate model simulations with an ensemble of global and one regional model to long observationally-based records and two paleoclimate reconstructions. Little consistency is found between time histories of these records, suggestive of a large role for internal atmosphere-ocean variability. Importantly, while there is little agreement between the characteristics of the model simulations and the observationally-based records, these differences do not appear to be systematic across models and cannot be explicitly linked to model bias. Likewise, there appears to be even less agreement between the characteristics of the observationally based records and the reconstructions. Together this work is consistent with mounting evidence that regional hydroclimate is largely “unforced”.

**General Remarks:** While the manuscript is interesting and highlights some important results, it is at times unclear what should be taken away from the results. This is, in part, an issue with the introduction and a refocused introduction that clearly describes the motivations and goals of the study would greatly improve the manuscript. Below are a number of specific and more general comments.

**Author reply:** A new version will clarify the introduction not only with respect to the motivation and the conclusions but also related to the general focus and intention and peculiarities of our approach in comparing different sources of information.

**Intended changes in document:** The complete manuscript is going to be restructured.

**Page 1, Line 8:** and in the standard deviations seems a weird statement.

**Author reply:** To be changed.

**Intended changes in document:** We will modify the abstract.

**Page 1, Line 18:** add “of” before “whether”.

Author reply: Will be changed.

Intended changes in document: Will be changed

Page 1, Line 19: what is meant by requires consistency?

Author reply: We will clarify our idea of requiring consistency.

Intended changes in document: Introduction and discussions are going to be more explicit about what we mean by consistency.

Page 1, Line 21: suggest changing to “over approximately the last 350 years”.

Author reply: Will be modified.

Intended changes in document: Will be modified.

Page 2, Line 2: suggest removing “in particular”.

Author reply: We will rephrase the sentence.

Intended changes in document: Will be rephrased.

Page 2, Line 6: change “base” to “basis”.

Author reply: Will be changed.

Intended changes in document: We will change the sentence.

Page 2, Line 10: change “compare directly” to “directly compare”.

Author reply: Will be changed.

Intended changes in document: Will be changed.

Page 2, Line 12: Cooper and Wilson et al. are the reconstructions. I would be careful here and throughout with the semantics of “data”.

Author reply: We will try to be clear in how we refer to the various

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sources of information. However regarding the longstanding discussions on what may be named “data”, the Wiktionary writes, slightly paraphrased, at <https://en.wiktionary.org/wiki/data>English: data: a) Information, especially in a scientific or computational context, or with the implication that it is organized. b) Recorded observations that are usually presented in a structured format. c) A representation of facts or ideas in a formalized manner capable of being communicated or manipulated by some process. Data in the context of our writing is generally any set of information.

Intended changes in document: We will carefully consider how we describe the various sets of information.

Page 2, Line 16: You argued in the paragraph above that you do not want to use gridded reconstructions. I understand that this paragraph is addressing a new issue but the reference to the OWDA thus seems unusual here. In general, this paragraph does not seem necessary. I might instead start at the beginning of the next paragraph and add a statement at the end of that first sentence saying that you are doing the standardization to make the reconstructions directly comparable to SPI.

Author reply: We will try to more clearly justify the choice of method and data.

Intended changes in document: We will rephrase the introduction to ensure a logic reading.

Page 2, Line 24: Suggest changing “their data” to “the utilized archives”.

Author reply: We will rephrase the sentence.

Intended changes in document: Will be phrased differently.

General comment: A lot of the above reads much more like a methods section than an introduction. I suppose this is more of a personal preference but the paper might be more impactful with a standalone introduction that does not include this methodological information.

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Author reply: We will try to better separate introduction and methods.

Intended changes in document: We will take care to clearly distinguish between introductory comments and the description of the methods and the data..

Page 2, Line 24: The sentence about Murphy et al. (2018) feels out of place. I would try to tie this into the paragraph above or remove it.

Author reply: We will try better to embed the point of Murphy et al. (2018).

Intended changes in document: Introduction will be rephrased.

Page 2, Line 28: Suggest removing “than in periods that are more recent”.

Author reply: We will restructure the sentence.

Intended changes in document: We will phrase the sentence more clearly.

Page 2, Line 29: Suggest splitting the sentence after the Maunder Minimum dates. I would then reword as: “Instead, they generally start around the late 18th century, when sunspot numbers indicate a period of relatively strong solar activity (Clette et al., 2014), and thus also include the transition. . .”

Author reply: We will clarify the point.

Intended changes in document: The paragraph will be modified.

Page 2, Line 35: Suggest changing “in European subdomains” to “across Europe”.

Author reply: We will modify the sentence.

Intended changes in document: We will make the point more clearly.

Page 3, Line 1: Change “extend” to “extent”.

Author reply: Will be changed.

Intended changes in document: Will be changed.

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Page 3, Line 10: This sentence is long and the second half I had trouble understanding. Perhaps you could split this up into two sentences and expand on the point that you are trying to make in the second half of the sentence.

Author reply: We will try to clarify the point.

Intended changes in document: The paragraph will be clarified.

Page 3, Line 20: Suggest “using the global model ECHO-G for boundary conditions” instead of “externally forced”. I am also not sure what this part of the sentence means: “and reconstructions over larger regional domains.”

Author reply: We will clarify the sentence.

Intended changes in document: We will make the point more clearly.

Page 4, Line 1: Suggest changing “and the simulation data representing” to “and simulations that often represent”.

Author reply: We will adapt the sentence.

Intended changes in document: Will be changed

Page 4, Line 2: Suggest changing “evaluation” to “comparison”.

Author reply: Will be changed

Intended changes in document: Will be changed.

Page 4, Line 17: Change “allows comparing” to “allows for the comparison of”.

Author reply: Will be changed

Intended changes in document: Will be changed.

Page 4, Line 19: Change “allows evaluating and comparing” to “allows for the evaluation and comparison of”.

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Author reply: Will be changed.

Intended changes in document: We will rephrase the sentence.

Page 4, Line 22: Suggest changing “extends the available metric for assessing the agreement in” to “allows for the rigorous comparison of”.

Author reply: We will try to discuss this point more clearly.

Intended changes in document: We will extend on this point in a new version.

Page 4, Line 23: Suggest changing “not only for periods without but also with” to “for periods both with and without”.

Author reply: We are going to change the sentence.

Intended changes in document: Will be changed.

General comments on introduction: I am unsure about the relevance of short-term (decadal) relationships between temperature and precipitation with those expected as a result of climate change (first two sentences of the introduction). The relationship between hydroclimate and temperature at the end of the 21st century in climate models is largely due to evaporative demand, which has a first order impact on water storage but not necessarily on precipitation. These changes are also very large in magnitude, and co-occurring with large magnitude changes in plant physiology, making deeper-time paleoclimate comparisons more appropriate for evaluating climate models (e.g., Scheff et al., J. Clim., 2016). I do not think this precludes such analyses being useful, I am just unsure of using the relationship between temperature and precipitation with an eye towards climate change as the motivation.

Author reply: We are going to adapt the motivation to address this point and to provide a more focussed impetus for our study. The link between temperature and precipitation is more complex, and not only restricted to long time scales. It may be modulated even at interannual timescales by other processes, for instance, through the link between

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temperature and cloud cloud cover during the extended summer season. Thus, an analysis of the covariability between temperature and precipitation even at interannual and decadal time scales serves as a validation of both reconstructed variables on the one hand, and of the corresponding link between these two variables in climate models on the other hand.

Intended changes in document: The motivation will be rephrased.

I would be careful with the semantics of the word data to make sure that things are as clear as possible. Likewise, I would refer to reconstructions, observations and simulations each with a single consistent term. This applies to the entire manuscript.

Author reply: We will try to be consistent in the descriptions of the various sources of information.

Intended changes in document: As mentioned above, we will take care to be clear in our use of the term data and its application to the various sources of information.

The introduction bounces around a lot, with quite a bit of methodology (see general comment above). I think that as cast it will leave the reader uncertain about the motivations and goals of the study. I suggest that the authors revisit the introduction with an eye towards clarity.

Author reply: We are going to try to motivate our study more clearly and to provide the reader with a better picture of from where we start and where we try to go.

Intended changes in document: The introduction will be reformulated.

I made an effort to make grammatical edits in the introduction but likely missed some. I will not be able to make this effort in subsequent sections but suggest that the authors revisit the manuscript with an eye towards grammar and syntax.

Author reply: We are going to try to improve the language, once more.

Intended changes in document: We are going to improve the language.

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It might be worth explicitly outlining how what you are doing here is different from Gómez-Navarro et al. (2015). Along with what is described the methods there would appear to be quite a bit of overlap.

Author reply: To be clear, we are co-authors on GN15. GN15 look at a regional simulation and gridded reconstruction over the European domain. They, among other things, compare both for a variety of regional sub-domains and a number of different datasets. They do not consider the small regional scale, they do not consider the SPI, they always have the spatial reconstruction step.

Intended changes in document: We will clarify the difference between Gómez-Navarro et al. (2015) and our manuscript.

Page 4, Line 28: Change to “in the form”.

Author reply: We will modify the sentence.

Intended changes in document: Will be modified.

Page 5, Line 15: Suggest adding “In particular,” at the start of this sentence to link it to the previous sentence. Suggest also changing “different means” to “systematic differences in the values of”.

Author reply: We will adapt the paragraph

Intended changes in document: We will change the paragraph.

Page 5, Line 16: Suggest “While model-biases may also contribute to these differences,. . .” and change “bias” to “source of differences”.

Author reply: We are going to adapt the paragraph.

Intended changes in document: The paragraph will be clarified.

Page 5, Line 17: I doubt it matters but why the different domain here?

Author reply: The domains for CET and EWP differ, thus we also adapt different model

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domains. However, a new version will have a smaller role for the temperature data.

Intended changes in document: We will remove much of the temperature discussion from the manuscript but also discuss the different domains more clearly if necessary.

Page 5, Line 30: Change “to include” to “the inclusion of”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 6, Line 5: Change “allows to compare” to “allows for the comparison of”.

Author reply: Will be changed.

Intended changes in document: To be changed

Page 6, Line 19: Change “allows considering the changing amount of precipitation” to “allows for a robust quantification of changes in precipitation amounts between subsequent periods, for instance”.

Author reply: We will clarify this paragraph.

Intended changes in document: Will be clarified.

Page 7, Line 2: Remove “just”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 7, Line 3: Add “the” before “time series”.

Author reply: Will be changed.

Intended changes in document: To be changed.

General comments on methods: The half-degree simulations are course resolution for a regional climate model. At least one of the last millennium simulations analyzed is

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one degree (CCSM4), how much added information do we expect from a regional simulation at this course resolution and what physical processes is it capturing to provide that information?

Author reply: We will comment on this. See, for example, the most recent papers by Ludwig et al. (2018) and Sørland et al. (2018).

Intended changes in document: We will more clearly discuss the benefit of even a slight increase in resolution and why a regional simulation adds more benefits than just an increased resolution.

Page 7, Line 13: Change “tentative” to “qualitative”.

Author reply: Will be changed.

Intended changes in document: Will be changed.

Page 7, Line 20: Suggest change the last sentence to “This is likely to also impact our analyses of precipitation”.

Author reply: We may modify the sentence.

Intended changes in document: We will clarify this paragraph.

Page 7, Line 32: What is the European domain?

Author reply: We will detail the domain.

Intended changes in document: In case it is still relevant in a new version, we will be specific about this larger European domain.

Page 8, Line 2: Suggest removing the first sentence.

Author reply: We are going to restructure the description of our results

Intended changes in document: The results section is going to be rephrased and re-structured.

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Page 8, Line 5: Suggest changing “representations” to “time series”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 8, Line 7: Suggest removing “but the Southern-Central England data enters it later”.

Author reply: We will clarify the description of the results.

Intended changes in document: The results will be clarified.

Figure 2, caption: Why call the Southern-Central England record SW England in the legend?

Author reply: Will be changed. Thank you for pointing out this oversight.

Intended changes in document: To be changed.

Page 10, Line 23: Change “allows evaluating” to “allows for the evaluation of”.

Author reply: We will clarify the sentence.

Intended changes in document: This will be clarified.

Page 10, Line 24: Change “gliding” to “sliding”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 10, Line 25: Suggest removing “partially”.

Author reply: Will be removed.

Intended changes in document: To be removed.

Page 10, Line 27: Change sentence to read “The moving window transformations show

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the percentiles represented by a given amount of precipitation over time (Figure 3).“

Author reply: We are going to clarify the procedure.

Intended changes in document: This part of the manuscript will be clarified.

Page 12, Line 1: Suggest changing “We pointed above at” to “In the previous sections we described”.

Author reply: We are going to modify the sentence in question.

Intended changes in document: This part will be clarified.

Page 12, Line 6: Suggest changing “gliding” to “sliding”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 12, Line 11: Suggest combining these two sentences.

Author reply: We are going to make the point more clearly.

Intended changes in document: We are going to change this paragraph.

Page 12, Line 12: Suggest changing “Considering” to “In”.

Author reply: We are going to modify the sentence.

Intended changes in document: To be changed.

Page 12, Line 15: Suggest removing “correlation”.

Author reply: Will be removed.

Intended changes in document: To be removed

Page 12, Line 20: Suggest changing “highly” to “strongly”.

Author reply: Will be changed.

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Intended changes in document: Will be changed.

Page 12, Line 21: Why “CET” here and not elsewhere?

Author reply: We are going to be more consistent in the use of abbreviations.

Intended changes in document: The new version will be consistent in use or avoidance of abbreviations.

Page 12, Line 31: Change “very low frequent variability” to “low frequency variability”.

Author reply: Will be changed.

Intended changes in document: To be changed.

Page 15, Line 8: Again why the use of “CET” here and not elsewhere?

Author reply: We are going to be more consistent in the use of abbreviations.

Intended changes in document: A new version is going to be consistent in use or absence of CET, EWP, and other abbreviations.

Page 15, Line 15: Why just atmospheric circulation when coupled variability can also do this?

Author reply: Indeed. We will change this and discuss more extensively factors influencing the regional domain.

Intended changes in document: We will clarify this discussion.

Page 15, Line 24: I found this paragraph difficult to understand. The final sentence is seemingly important but I was unclear on what it means. Likewise, I would clarify what is meant by unfortunate earlier in the paragraph.

Author reply: We are going to clarify our thinking on regional climate variability, natural forcing, the relation between temperature and precipitation, and the precipitation distributions.

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Intended changes in document: A new version will discuss this more clearly.

Page 16, Line 7: Suggest changing “appears” to “is”.

Author reply: Will be changed

Intended changes in document: Will be changed.

Page 16, Line 23: While this is true, it is unclear how it relates to the other discussion.

Author reply: We are going to better connect the discussion on changing teleconnections to the discussions on internal variability and the representativeness of data sources.

Intended changes in document: Discussions of a new version will be more clear in this discussion.

Page 17, Line 19L Change “source” to “sources”

Author reply: Will be changed

Intended changes in document: To be changed.

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A note is in place on potential added references. There are a number of topics, which may need discussing additional references.

First, there is the SPI. We have not yet decided which of the previous studies using the SPI in paleoclimatology are essential for our argumentation. Candidates include Domínguez-Castro et al. (2008, doi:10.1016/j.gloplacha.2008.06.002) Machado et al. (2011, doi:10.1016/j.jaridenv.2011.02.002), the SPI use by Lehner et al. (2012, see original references), Seftigen et al. (2013, doi:10.1002/joc.3592), Yadav et al. (2015, doi:10.1016/j.quascirev.2015.04.003), and Tejedor et al. (2016, doi:10.1007/s00484-015-1033-7). These, however, mainly deal with the SPI as original reconstruction target.

Second, as we noted above, we have to discuss why we think the used regional climate model has indeed a chance to improve on the representation compared to the PMIP3-ensemble. Recent publications by Ludwig et al. (2018, doi:10.1111/nyas.13865, Sørland et al. (2018, doi:10.1088/1748-9326/aacc77), and Pinto et al. (2018, doi:10.1002/joc.5666) allow to make this point. These may also become relevant in discussing how our work differs from Gómez-Navarro et al. (2015).

Third, while we in principle think that our references for contextualising regional climate variability and the large scale are sufficient, we may include additional discussions on the relation between the large scale climate dynamics and precipitation (e.g., Jones et al., 1993; Mayes, 1996; Wilby et al., 1997; Osborn and Jones, 2000; Murphy and Washington, 2001; Wedgbrow et al., 2002; Kingston et al., 2006; Lavers et al., 2010; ).

Fourth, there remains the question, how much of the literature on the British observational datasets is relevant to the discussions. Our initial assessment was that the main references for the datasets are enough. Possibly, additional references will be added (e.g., Wigley and Jones, 1987; Gregory et al., 1991; Jones and Conway, 1997; Kilsby et al., 1998; Osborn et al., 2000; Croxton et al., 2006; Marsh et al., 2007; Simpson and Jones, 2012; Simpson and Jones, 2014).

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It remains to be seen to what extent discussing the issues of the used and not considered reconstructions requires additional references.

Once more, thank you for your help.

On behalf of the authors

Yours sincerely

Oliver Bothe

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-27>, 2018.

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Interactive  
comment

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