

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

### **Anonymous Referee #2**

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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.

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

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reader to understand what the aims are and the summary of information necessary to move to the next stage.

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.

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.

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.

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.

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

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Results presented in the methods section need to be moved.

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.

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.

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

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

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.

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.

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

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

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

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