

## ***Interactive comment on “Multi-annual droughts in the English Lowlands: a review of their characteristics and climate drivers in the winter half year” by C. K. Folland et al.***

**C. K. Folland et al.**

jaha@ceh.ac.uk

Received and published: 9 January 2015

We would like to thank the reviewer for their very positive review of our manuscript, and their constructive suggestions for minor improvements to the paper. We address these specific comments as follows:

Specific Comments:

1. We appreciate the reviewer’s support for the decision not to accumulate streamflow. Our choice of nomenclature was indeed driven by the fact we use exactly the same non-parametric approach published as the SGI by Bloomfield et al. However,

C6006

on balance we agree this is confusing. We suggest adopting the Standardized Flow Index (SFI) terminology, because there is no particular formulation nor key reference for standardization for river flows, rather a number of different approaches (see our references); any approach that applies SPI-like concepts to flow could be justifiably named SFI. We think that it is better to refer to the index by the hydrological variable in question (SPI, SFI, SGI, etc) rather than by the methodological approach. We would not want to develop (yet another!) new index name for our approach. Note we propose using SFI rather than SRI as the latter has more typically been applied to modelled runoff (Shukla & Wood, 2008) rather than observed flow.

2. We agree that snowfall is not likely to be a major factor in lowland England. Some winter drought periods (e.g. 1963/4, 2010/2011) will have had major snowfall but typically snow makes up a very minor proportion of precipitation and is a minor runoff generation component (even in cold winters) at the monthly to seasonal scale. So this is not likely to be a confounding factor in our analysis of links with ENSO. However, for an international audience we agree we need to state this. We will add a short paragraph recognising that ENSO temperature anomalies could be important on winter snowfall but noting the generally minimal effect of snow in our target region. We will also point out that the relative importance of snowfall has decreased over the study period. We will highlight that cold-season temperature anomalies and their influence on snowfall is a much more significant factor in drought development in other parts of Europe shown in our analysis (e.g. Van Loon et al. 2014).

3. We agree that we have chosen ambiguous terminology here, and that we are really talking about the relationship between instantaneous SGI and the SPI accumulation period with the highest correlation. This still represents attenuation of the rainfall signal and demonstrates the importance of drought propagation, but we agree that referring to lags in this way can cause confusion given the low lagged correlations (particularly for streamflow; Fig 4a). We will amend this in the discussion and abstract to refer to propagation and attenuation (depending on context) rather than lags. We note also

C6007

that for groundwater the maximum correlation is a concurrent one but 1- and 2-month lags are also important. We will add clarification of this in our description of the results, in Sect. 2.3

4. Meteorological drought events are defined according to Table 1, using the methodology outlined in 2.2. We will make this clearer and we will amend the table to only show “YES”.

5. We agree and the requested split will be introduced

6. We agree. A consistent orientation will be created for Figs 8, 9 and 11.

7. We agree that the variance of drought indicators is influenced by a number of these drivers, including QBO as highlighted by the reviewer. We propose not to elaborate on this in the paper (other than to note different variances for high/low values of some drivers) as we agree that this would be a very productive avenue for follow-up – we thank the reviewer for this suggestion.

Technical Corrections:

Page 12947, Line 6 – It appears part of this sentence is missing.

Reply: The reviewer is in error here. There is no truncated sentence. We have also checked the original text against the version sent to the reviewer and they are identical and complete. The sentence on the first part of line 6 ends correctly and the next sentence starts and continues correctly: “The character and physical causes of the influences differ between moderate and strong El Niños (Ineson and Scaife, 2008). Folland et al. (2012), their Fig. 7b, show that the overall effect of El Niño on English Lowlands rainfall in December–February is towards modestly wetter than normal conditions, while La Niña (associated with significantly colder than normal SST in the tropical east Pacific) gives modestly drier conditions than normal conditions, consistent with the model results of Davies et al. (1997) and the observational results of Moron and Gouirand (2004).”

C6008

Page 12947, Line 8 – There is an accidental space in La Nina.

Reply: We will correct this

Additional references you may also consider “Fraedrich, K. and Müller, K. (1992), Climate anomalies in Europe associated with ENSO extremes. *Int. J. Climatol.*, 12: 25–31. doi: 10.1002/joc.3370120104” as an original paper that examined anomalies in Europe tied to ENSO

Reply: we thank the reviewer for the reference suggestion; we are planning to add papers by Fraedrich in response to the comments of Wilby (see Short Comment 1) so will add this to the list of papers to review and consider adding.

References:

Van Loon, A., Ploum, S.W., Parajka, J., Fleig, A.K., Garnier, E., Laaha, G., and Van Lanen, H.A.J. 2014. Hydrological drought typology: Temperature-related drought types and associated societal impacts. *HESS Discussions*, 11, 10465. <http://www.hydrol-earth-syst-sci-discuss.net/11/10465/2014/hessd-11-10465-2014.html>

---

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 11, 12933, 2014.

C6009