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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Understanding the potential of climate teleconnections to project future groundwater drought" *by* William Rust et al.

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We would like to thank Anonymous Referee #1 for their detailed review comments. We found them to be insightful, and, through our responses to them set out below, we believe that they have resulted in a much improved paper.

Major Comment 1: Referee #1 states that "it is not correct to say that this paper quantifies the teleconnection contribution to the absolute groundwater variability for the first time (line 346, 392, 449, 509). The authors claim that all previous studies performed low-pass filtering or some averaging of groundwater level time-series before wavelet transform or PCA methods. This is not so, at least in the case of Tremblay et al., 2011 and Neves et al., 2019. The proportion of groundwater variability driven by teleconnec-

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tions in the UK seems indeed much lower than in other parts of the world. Blaming the amplification of low frequencies in other studies (that does not happen) is therefore not valid, and the authors should seek other explanations."

Response to Major Comment 1: The Reviewer is correct that there may be many other contributing explanations in some cases which we have now outlined in Lines 375 - 386. However, we do also think that previous studies that have sought to quantify the proportion of extra-annual cyclical variability in groundwater level and that may have used pre-processing steps that might have altered the strength of extra-annual periodicities within the groundwater spectra. A key example is cumulative departure from the mean (CDM) which has been undertaken by Neves et al., 2019. While not explicitly designed as a low-pass filter, CDM is a process that amplifies low frequency periodicity and suppress higher frequency periodicities. This is, for example, exemplified in figure 4 in Neves et al., 2019 where we can see little annual variability in rainfall; which we would not expect from a 'raw' dataset. As a result of this, the strength of extra-annual periodicities may be misrepresented when compared to the raw groundwater level data. Another example is given in Tremblay et al., 2011., while no preprocessing of the data is apparent, periodicities reported have not included the strength of seasonality. As such, we cannot tell the actual strength (and therefore importance) of the extra-annual periods, as we cannot tell how they compare to seasonality (known to be a major component of hydrological processes). As such we believe this paper provides an explicit assessment of the percentage of cyclical variability to the unaltered groundwater level data spectrum. We have amended the text in the locations highlighted by Referee #1 to make this clearer, e.g. Lines 375 - 386, 495 – 502.

Major Comment 2: Referee #1 states that "The results may probably be a consequence of the specific climate and hydrogeologic conditions in the UK, but may also be a consequence of the different methodology used to compute the percentages of variance. Do the authors get the same results using SSA or PCA? One alternative method should be used in order to be sure."

Major to General Comment 2: We appreciate why Reviewer #1 has made this observation and suggests additional analyses. However, SSA/PCA (which the co-authors applied to groundwater level observations in Holman et al.(2009)) requires removal of trends (non-stationary) before any meaningful information on principal components can be extracted and therefore implies stationarity. In addition, the aim of this paper was to identify specific periodicity bands that are shared between groundwater hydrographs, and with SSA/PCA there is no guarantee that eigenvectors between datasets will be comparable or even periodicities of these can be confidently estimated (as one would have to again assume stationarity to identify frequencies from principal components). Nevertheless, we have extended our literature review to include potential other sources for these signal strengths in light of the Reviewer's comment. E.g. Lines 479 – 492, 375 – 386

Major Comment 3.1: Referee #1 states that "A closer look at Figure 4 shows time intervals between droughts of approximately 2.5, 3, 5, 6 and seven years. Therefore, it seems excessive to declare that the approach presented in this paper can be used to predict droughts with a recurrence of seven years (line 492)."

Response to Major Comment 3.1: We agree with the comment that the wording around the recurrence of drought events is too strong and does not account for the variability in the time intervals between recorded droughts. To address this concern, we have now added a further review of drought mechanisms and have updated the text to refer to reflect drought risk, rather than the definite timings of drought in Lines 450 – 492. In addition, Figure 4 has been modified to better illustrate the drought start/end dates, although there is inevitable spatial uncertainty in these.

Major Comment 3.2: Referee #1 states "Moreover, the authors do not even mention the non-stationarity of teleconnections and ignore the effects of global warming on the predictability and statistics of extreme events. The authors need to elaborate more on these issues."

Response to Major Comment 3.2: We agree with the Reviewer that more elaboration is needed on these issues, although we also note that the effects of global warming on the predictability and statistics of extreme events is a very broad and still developing subject. It is mentioned in the text that the varying strength (and therefore the non-stationarity) of the NAO does not directly appear to influence the occurrence of historical drought, therefore wide-spread droughts appear sensitive to the NAO phase, rather than its overall strength. However, we have now also added additional text to clarify these issues in Lines 479 – 492.

Minor Comment 1: Please increase the font size of text and labels in the pictures – Figures have been updated

Minor Comment 2: Line 283: can you explain better why the 7-year cycle has greater significance values in rainfall than in groundwater? Text has been updated in Lines 287-289

Minor Comment 3: Line 315: do you mean misalignments amongst borehole records? Are there consistent misalignments amongst aquifers? Text has been updated at line 319

Minor Comment 4: Line 321: figure 6 instead of figure 4? Text has been updated

Minor Comment 5: Lines 342-354: the whole paragraph is redundant and would better be omitted. We agree that this paragraph is not required and have removed the text

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