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Interactive comment

Interactive comment on "Increased incidence, duration and intensity of groundwater drought associated with anthropogenic warming" *by* John P. Bloomfield et al.

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We would like to thank Referee #3 for their review comments. We enjoyed working though the challenges that the reviewer posed and think that a revised paper will be improved as a result of their contribution.

Response to the Specific comments

Comment 1. Referee #3 states that given the rise in temperature throughout the three periods, high temperatures will necessarily coincide with groundwater droughts more often in the latter period (see Fig 4), and that we have not investigated how many of

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the observed groundwater droughts are attributable to increases in temperature over time. We agree that the former is the case, and emphasise that this is the main point of the paper, i.e. to characterise the change in groundwater drought in the context of prior knowledge of anthropogenic warming. However, with regard to the latter comment we would like to take the opportunity here to re-iterate the aim of the work as described in Lines 76-81, namely: "We have not attempted to formally attribute any groundwater droughts to climate change. Rather, we follow the approach of Trenberth et al. (2015) and investigate how climate change may modify a particular phenomenon of interest. In our case, given the known centennial-scale anthropogenic warming over the UK described in section 2.2 (Sexton et al., 2004; Karoly and Stott, 2006; Jenkins et al., 2008), using an empirical analysis we address the question: how has the occurrence and intensity of groundwater drought, as expressed by changes in SGI, changed over the same period?". In short, we are searching for evidence of changes in groundwater drought incidence, duration and intensity given known climate warming and the absence of other major change factors, and we are not currently concerned with the formal attribution of individual groundwater drought episodes. In this context, establishing changes in the number and nature of droughts associated with warming (in the absence of systematic changes in rainfall deficits) in the third period is the key result of the paper. We are not for example, making any further inferences from Fig 4. We note that this approach is explicitly similar to one used in the analysis of Diffebaugh et al (2015).

Although simple in approach, we believe that such an essentially descriptive, empirical analysis is important because of the current lack of observations relating anthropogenic warming to groundwater systems, and to groundwater droughts in particular. For example, the IPCC noted as part of the Fifth Assessment (WGII AR5) "that there is no evidence that surface water and groundwater drought frequency has changed over the last few decades, although impacts of drought have increased mostly due to increased water demand" (Jiménez Cisneros et al., 2014). We believe that our work here directly addresses the very limited evidence base and provides for the first time evidence for the impact of climate change on groundwater droughts. We propose to make no changes to the paper related to this comment.

Comment 2. Referee #3 raises a number of questions related to the relationship between SGI and SPI. a. Referee #3 states that "When finding the highest correlating SGI to SPI aggregation periods, you get correlation coefficients between .7 and .8 at 6 and 7 months respectively. Even though these values are considerably high, showing the SPI/SGI on a cross-plot would reveal a considerable number of events where SPI does not predict SGI well". We feel that there is some confusion here regarding the purpose of aggregating the SPI to compare with SGI. SPI is known to be a poor predictor on it's own of groundwater drought events or SGI (e.g. Kumar et al., 2016; Van Loon et al., 2017), and this is not our aim in this study. Rather our purpose here is to identify an accumulation period that enables us to compare SPI and SGI in a consistent manner across the whole record. b. Referee #3 also states that "A longer aggregation period would possibly show a smaller change in precipitation-related droughts". Fig S4 shows that cross-correlation coefficients are relatively insensitive to accumulation periods for periods greater than about 5 to 7 months and for the purposes of our study it would not make sense to use longer accumulation periods for the analysis particularly where they have lower cross-correlations. c. Referee #3 states that "When looking at the study by two of the authors (Bloomfield and Marchant, 2013), the same locales were used among others, but DH had a longer aggregation period of 10 months, while using a shorter, more recent time period. Has a shift in the recharge regime occurred, which has been observed in other locations?". Fig 4 (and also Fig 7h in Bloomfield & Marchant, 2013) shows that differences in cross-correlation for accumulation periods between 6 and $\sim$12 months is very small. Consequently, we don't interpret the differences in significant accumulation periods between the present study and that of Bloomfield & Marchant (2013) to be indicative of a "shift in the recharge regime", but merely a reflection of the relative insensitivity of the cross-correlation to SPI accumulation period beyond $\sim$ 6 months. Our interpretation and the lack of evidence for a "shift in the recharge regime" is reinforced by the observation that the cross-correlations estimated for the first and last thirds of the records shown in Fig S4 are very similar.

Based on the above comments we propose to make a small clarification to the text at Lines 241-242, as follows: "The maximum cross-correlation between SGI and SPI was found for SPI accumulation periods of 6 and 7 months for CH (0.77) and DH (0.78) respectively. In addition, the maximum cross-correlation between SGI and STI was found to for an STI averaging period of between 5 to 6 months at CH (-0.15) and between 5 to 7 months at DH (-0.35). As would be expected, the cross-correlation between SGI and STI is weaker than that of SGI and SPI, but in all cases Figure S4 shows that there is limited sensitivity to the SGI-SPI and SGI-STI cross-correlations once a maximum correlation has been achieved after an accumulation or averaging period of about 6 to 7 months. Consequently, in order to treat the SPI and STI standardised data in a consistent manner across the whole record at each site, 6 and 7 month common accumulation and averaging periods have been estimated at CH and DH respectively. Although SPI6 and STI6 have been estimated for CH and SPI7 and STI7 have been estimated for DH, for simplicity throughout the following reporting of results and discussions all references to SPI and STI are for those accumulation and averaging periods for each site". In addition, we propose to make a minor adjustment to Fig 4, as follows: we will colour the cross-correlations estimated for the first and last thirds of the records to highlight their similarity to each other and to the estimates based on the whole record and hence emphasise the insensitivity of the correlations to the period of data on which they are based.

Comment 3. We agree that the cross-correlations between SGI and STI are relatively low compared with the cross-correlations between SGI and SPI. However, this does not have any bearing on our analysis. We are not using SGI-STI relationships to understand individual recharge events or for forecasting purposes, rather we are simply characterising the relationship between the two variables across the whole record. Because we are interested in the relationships between SGI, SPI and STI and how they change across the record it is important that they are estimated for common periods in a consistent manner. Consequently, based on the correlations illustrated in Fig S4 we believe that we have adequately justified the common accumulation and averaging periods of 6 and 7 months for CH and DH respectively. We have amended the text at Lines 241-242 (see response to Comment 2 above) to emphasise these points.

Although it is outside the scope of the present study, we agree with Referee #3 that there would be merit in looking at relationships between individual groundwater droughts and characteristics of the antecedent air temperatures. However, we expect that any relationships would be complex and non-linear functions of a range of factors including antecedent groundwater levels and precipitation. Comment 4. There is a real paucity of observational data to constrain the height of the capillary fringe in the Chalk. The value of 30 meters for the thickness of the capillary fringe cited in the paper (Lines 420-421) is based on the theory of Price et al. (1993). This value is widely accepted in the absence of systematic observations. For example, while developing their Chalk unsaturated zone model, Ireson et al. (2009) also assumed that "the matrix [in the unsaturated zone of the Chalk] will generally remain saturated by capillary forces" and modelled changes in Chalk unsaturated zone pore pressure and water content as a function of variations in fracture incidence and aperture. Ireson et al. (2009) modelled field data from two sites from the Chalk of the Pang-Lambourn catchment in the Chilterns. Although the site was on the same aquifer formationas CH and DH, i.e. the Chalk, we agree that there is no reason to expect that results from those sites should necessarily be representative of CH and DH.

We propose to modify the text at Line 420 to clarify these points as follows: "The Chalk, however, is a dual porosity dual permeability aquifer with a thick capillary fringe. Due to the micro-porous nature of the matrix, the matrix theoretically remains saturated to at least 30 m above the water table (Price et al., 1993). Consequently, in the Chalk it is proposed that ET contributes to the formation and propagation of groundwater droughts at sites with water tables at least down to 30 m below ground level. If so groundwater drought formation and development may be particularly sensitive to the effects of changes in ET, and hence to anthropogenic warming. We note that there have been no systematic observations of this phenomena across the Chalk aquifer to date and the only detailed observational study of variations in unsaturated zone flow, water content and matric potential in the Chalk is that of Ireson et al. (2009). They also assumed that matrix in the unsaturated zone remained saturated, and explained their observations in terms of the weathering profile of the Chalk and specifically variations the frequency and aperture of fractures. Clearly, if changes in ET mediated by anthropogenic warming are contributing to changes in groundwater drought in the Chalk and other shallow groundwater systems, there is a need to characterise this phenomenon using new co-located long-term soil moisture, water potential and groundwater level observations (Huntington, 2006). This is something that should be addressed with some urgency if we are to better constrain the effects of warming on groundwater resources and on groundwater droughts into the 21st Century."

Response to 'Technical comments'

L234: Referee #3 comment as follows: "Clarify that the indices are calculated over the entire period". Agreed. text to be modified to read as follows: "A Standardised Temperature Index (STI) and Standardised Precipitation Index (SPI) have been calculated by applying the SGI method to the average monthly temperature (STI) and a monthly accumulated rainfall (SPI) time series over the entire observation period."

L240-241: Referee #3 comment as follows: "Put maximum correlations into text". Agreed. See new text proposed in response to Comment 2 (above).

L269-272: Referee #3 comments that it is "unclear what is meant by "probability of the difference", please specify what has been done here. Statistical signiïňĄcance?". The probability of difference can be thought of in the following way. If we define D as equal to the number of droughts in the last period minus the number of droughts in the first period (for example, but it could be any pair of periods) then the "probability of difference" is the probability, under the null model, that D is greater than the observed value. To clarify the text we propose to revise it at Lines 266-269 as follows: “Given that the standardised indices are normally distributed, a null model can be estimated where each standardised index is assumed to be a realisation of temporally auto-correlated Gaussian random function (with auto-correlation function estimated from the observed data). A ‘probability of difference’ for a standardised index between periods can be estimated as follows: if we define D as equal to the number of droughts in the last period minus the number of droughts in the first period (for example) then the probability of difference is the probability, under the null model, that D is greater than the observed value. Estimated in this way, the probability of the difference in the number of hot months . . .”.

Fig 2: Referee #3 comment as follows: “Very information-dense. The percentage values mean different things in the different panels, it should be possible to clarify within the figure”. Agreed. The figure will be modified to be explicit regarding the %age exceedence in each figure.

Fig 3: Referee #3 comment as follows: “Instead of using integers 1-3 for periods, use the interval of years on the y-axis”. We tried this in an earlier iteration of the figure however the text becomes too small to be legible at any sensible scale of reproduction. A similar comment was made by Referee #2. In response their comment and this comment we propose to modify the figure caption as follows: “Percentage of monthly STI, SGI and SPI as a function of six ranges of standardised values from $\leq$ -2 to $\geq$ 2 for the first (1891-1932), middle (1933-1973) and last (1974-2015) thirds of the records from CH and DH, denoted by columns 1, 2 and 3”. In addition, we will label the x-axes as “Periods 1, 2 and 3”.

Fig 4: Referee #3 comment as follows: "Add location to the figure (CH, DH) so it becomes clear directly what the reader is looking at." Agreed, the figure will be amended as suggested.

Fig 4: Referee #3 comment as follows: "Additionally, it would be beneficial to see which

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of the non-drought months come from the specified period". Currently, the grey closed symbols denote all months data across all three periods. We will revise the figure to show this data as open symbols and use grey closed symbols to show non-drought months for the specific period.

Fig 6: Referee #3 comment as follows: "Why not include the second period? I get the impression from Fig 5 that drought durations are not dissimilar for the second and third period, especially for CH". Referee #1 raised a similar point. In response to their comment we have proposed to include the middle period in Fig 6 and make the following additional changes: "we will revise the text in Section 3.3 to reflect the observation that there is no clear shift in the duration probability distribution at the two sites; and, we will revise other references in the text related to drought duration in line with the above" (Bloomfield et al., 2018).

L412-416: Referee #3 comment as follows: "Difficult sentence to digest". We agree that the phrasing is clumsy and propose to re-draft as follows: "Based on analysis of data from shallow North American aquifers, Maxwell and Condon (2016) described a transition from a regime where T is groundwater dependent and E is water limited, to regime where both E and T are water limited. Under the latter regime groundwater is effectively disconnected from the land surface resulting in relatively low T and E that are limited by precipitation. They estimate that the transition between these regimes is of the order of 5 m below ground level".

Supplement, Fig S4: Referee #3 comment as follows: "Add locations CH/DH to the figure". Agreed, location details will be added to the figures.

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