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Interactive comment on "The European 2015 drought from a groundwater perspective: estimation in absence of observed groundwater data" *by* Anne F. Van Loon et al.

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General comments: The paper was enjoyable to read, is clearly written and addresses an important set of related research questions on the topic of the predictability of heterogeneous groundwater systems to drought under conditions of incomplete information. My general comments on the paper relate to two points: a.) the framing of the paper, i.e. need for near real-time groundwater drought data and the ability to develop calibrated modelling systems in advance of groundwater droughts, and b.) the title of the paper. As an aside, the authors should be congratulated on seeking to publish a negative finding (their observation that, with regard to the present study, GRACE –TWS did not appear to be suitable for use in real-time groundwater drought assessment) –

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such findings can be very useful, but are often under-reported.

The need for near real-time groundwater drought data. The paper explores two possible approaches to estimating, in near real-time, spatially varying groundwater levels under conditions of drought by: i.) establishing correlations between a standardised hydrological index (in this case SPEI, although SPI "showed similar results") and observed groundwater levels, and ii.) estimating groundwater anomalies by subtracting modelled surface water stores from GRACE-TWS data (that represents variability in both groundwater and near surface water). Once calibrated or established such approaches could in theory inform near real-time decisions on groundwater management during future droughts in the absence of groundwater level observations.

In this context, the authors note in their conclusions the following: "We recognise that our approach of using the pre-determined relationship between meteorological conditions and observed groundwater levels is crude and has uncertainties. It does, however, provide a first-order look into the spatio-temporal patterns of current and recent groundwater droughts based on meteorological indices". An alternative approach to modelling the spatial variability of groundwater droughts in near real-time using the available a priori meteorological and groundwater level data could have been developed. Simple lumped parameter models are increasingly used to model groundwater levels; make almost no assumptions about hydrogeological setting; and, if there are sufficient sites, capture and reflect spatially varying responses of groundwater systems. Lumped parameter groundwater models are already used for operational hydrological services, such as UK Hydrological Outlooks http://www.hydoutuk.net/methods/groundwater/. In addition it is possible to constrain the uncertainty in such models as well as the success of their predictions. For example, see work using such models from the UK (Mackay et al., 2014; 2015; Jackson et al., 2016; Marchant et al., 2016). The paper would have a more rounded context if the introduction includes a discussion of such lumped parameter models and the pros and cons of the adopted approach compared with a lumped parameter modelling approach.

It should be noted that the SPEI/SGI correlation approach described in the paper produces a calibrated but un-validated correlation and that any SGI values modelled using SPEI driving data will have unconstrained uncertainties. Whereas, using the same calibration and driving data, if multiple lumped parameter groundwater level models are produced they can be both calibrated and validated and uncertainties estimated for each of the predictions of groundwater levels. The main 'cost' in this latter case would be the time involved in producing multiple individual calibrated lumped-parameter models although the process can to some extent be automated.

The authors also note in their conclusion: "With this work, however, we also want to promote more long-term groundwater measurement and international sharing of groundwater level data". I entirely agree with this statement. For example, throughout Europe groundwater levels and spring discharges are extensively monitored by a wide range of organisations and institutions for a variety of purposes. Some of this information is freely available on the web, however, much of it is not readily available and certainly not in near real-time. Significant advances in the effective management of groundwater resources during droughts could be achieved with better co-ordination and sharing of groundwater data at the European scale. Such a freeing-up of information would in one step obviate the need to model 'near real-time' groundwater levels as described in the current paper and would enable more effective modelling of 'near future' groundwater levels using more sophisticated lumped parameter-type models.

Title of the paper. The paper aims to establish an approach for estimating near real-time groundwater levels during episodes of groundwater drought in the absence of groundwater observations. It uses the expression of the European drought of 2015 from two regions, in Germany and the Netherlands, to test two alternative modelling approaches. However, I don't feel that it is appropriate to suggest that it provides a coherent insight into the groundwater aspects of the European of drought 2015. Consequently, I'd suggest an alternative title such as: "Estimation of near real-time groundwater drought status in the absence of observed groundwater level data".

Specific comments: P3., last para – given my comments above regarding the title of the paper, I don't think that the statement "In this paper, we aim to analyses the 2015 groundwater drought in Europe . . ." is quite right. I suggest re-phrasing to something like "In this paper, we asses two alternative approaches to model near real-time groundwater drought . . ."

P4., last para - when working with standardised indices such as SPI or SGI it is common practice to produce standardised values on a common time period and with a minimum record length of 30 years (McKee et al 1993 and others). What errors have been introduced into the analysis due to differences in record lengths within and between the two study regions and do these errors effect the conclusions of the study given that "The length of [the groundwater level] records varied from well to well with a minimum of 10 years, starting from the year 1951 for the German wells and 1988 for the Dutch wells and ending in the year 2013"?

P5-6. Section 3.1 and Fig 2 (top and middle panels) – the SGI data is the same as Kumar et al. (2016) and the difference between Fig 2 of Kumar et al. (2016) and Fig 2 (top and middle panels) of this study is that the latter uses a more refined grid for the analysis. What are the implications, if any, of the reduced number of groundwater level time series observations within the smaller grid cells of the present study on the averaging procedure to obtain a representative SGI for each cell?

P6., last para – "To reduce the noise from individual GLDAS model outputs, we use the ensemble mean of the groundwater anomalies in our analysis". It would be nice to have a bit more information on the scale and nature of the noise in the GLDAS model outputs, perhaps scaled as a function of the GRACE-TWS data? Is there any temporal or spatial structure in the noise relevant to the two study areas and the periods of calibration and modelling?

P8, para 2 - Once an optimal accumulation period has been established for each cell, why has the maximum (point?) correlation between pairs SPEI/SGI of time series been plotted in Fig 2, wouldn't a representative or (grid) average correlation corresponding to the optimal accumulation period be more instructive than the maximum correlation? It would of course be likely to be lower than the reported correlations.

P10., para 1 of Section 5.1 – as above, I suggest re-drafting to the first sentence to "We assessed two alternative approaches to model groundwater drought in the absence of . . .. . ."

P10., para 2 of Section 5.1 – It is stated that "The analysis using SPI instead of SPEI to represent meteorological conditions gave very similar results. This means that precipitation is the main driver of the optimal accumulation period of meteorological conditions to influence groundwater. This may be different in more arid regions where PET is a more important component in the water balance. For regions similar to the ones we analysed here, we expect that in absence of PET data SPI can be used instead of SPEI". Although not critical to the paper, this is an interesting observation, but I'm not sure that the interpretation is correct. Bloomfield and Marchant (2013) demonstrated that the optimal accumulation period for SPI/SGI correlation scaled as a function of the autocorrelation range of the groundwater level time series (mmax) (Bloomfield and Marchant, 2013, Fig.10), which in turn was shown to be a function of unsaturated zone thickness and log-hydraulic diffusivity of the aquifer (Bloomfield and Marchant, 2013, Fig. 13), i.e. that it was necessary to invoke aquifer and catchment processes responsible for attenuation of meteorological signals to explain the optimal accumulation period. Assuming that similar relationships hold for SPEI/SGI then I don't think that precipitation is the main control on the optimal accumulation period as stated, rather it is catchment and aquifer characteristics. PET would be expected to have a very limited effect on groundwater levels once a drought has been established when the main cause of groundwater decline would be natural groundwater recession due to groundwater discharge in the absence of precipitation. Note that under drought conditions soil moisture deficits are likely to be very high so limiting the effect of PET.

P12., end of first para of section 6 – again the statement that "The analysis of both

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SPI (representing accumulated precipitation anomalies) and SPEI (representing accumulated anomalies in precipitation minus potential evaporation) showed similar results, indicating that precipitation is the main driver of the optimal accumulation period of meteorological conditions to influence groundwater in our study areas". See my comments above. I don't think that this interpretation is correct and that the optimal accumulation period is a function of catchment and aquifer characteristics not precipitation.

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