Interactive comment on "The El Niño event of 2015–16: Climate anomalies and their impact on groundwater resources in East and Southern Africa" by Seshagiri Rao Kolusu et al

Reviewers' comments in BLACK
Our responses to comments in BLUE

Anonymous Referee #2

Reply to Anonymous Referee 2:

Overall review

This paper by Kolusu et al. examines different climate and groundwater anomalies in East and Southern Africa related to the period of 2015-2016, which corresponded with one of the worst droughts that occurred in Southern Africa. This study puts in context what were some of the major factors leading up to and affecting the severe drought in Southern Africa and the rainy conditions in East Africa, during this period. The paper overall contributes relevant science questions and results, within the scope of HESS, and presents relevant results that address a key water resource issue (i.e., groundwater depletion and recharge) in a vulnerable climate changing region. Major conclusions are reached in this work, but there are some points that the authors may want to consider addressing in their results and discussion. Some examples are provided below in the "Specific comments" section. The abstract and overall presentation of the paper is clear, however, having so much of the background material in the Supplementary Information document requires the readers to continuously refer to the separate document, interrupting the flow of reading the main manuscript at times. Scientific methods and assumptions are outlined and described well, in both the manuscript and Supplementary Information. The results are overall sufficient to support the authors' conclusions, and most of the dataset and method descriptions are well explained. Also, proper credit is given to previous studies and data providers.

Specific comments 1. The authors place much of the paper's background and details in the Supplementary Information section. At times, placing some of the information in the main manuscript would actually help the flow of the paper more, instead of the reader having to constantly refer to the supplementary material. Some examples include the background discussion of the SPEI, which almost all is placed in the Supplementary Information section. However, the SPEI is one of the more crucial metrics used to address their science question on the relationship to the groundwater datasets and anomalies.

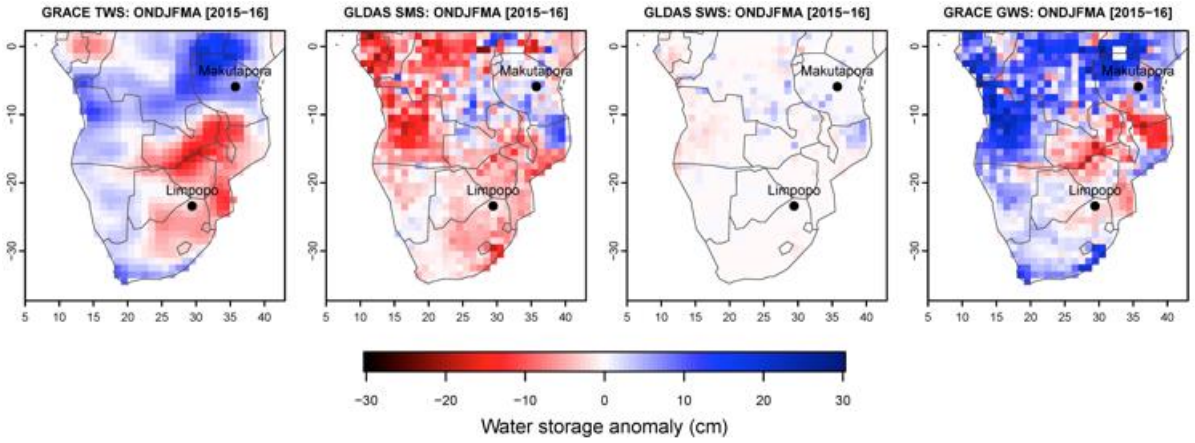
We agree (as does reviewer #1) and have now moved much of the important detail from the Supplementary Information to the Methods section [see lines - 117-169 and ;174-243 in the revised manuscript].

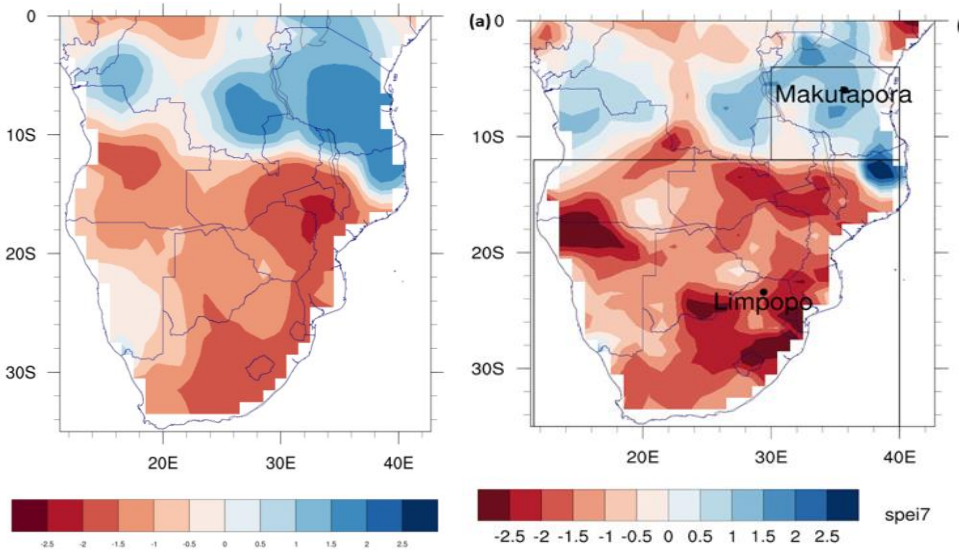
2. Lines 230-233: Authors may want to be careful in stating with such certainty that the "2015-2016 magnitude of the SPEI-7 drought over SA . . . increased two times due to the effects purely of anthropogenic warming. . .". Though anthropogenic warming may be contributing to greater magnitudes, expressed with such drought metrics, other effects such as persistent drought or dry-land-atmospheric feedbacks could have greatly contributed as well.

We agree that attribution of changing climate risks is challenging. For that reason we consider only the contribution of anthropogenic changes in regional temperature to drought risk, for which we have much greater confidence in attribution than we do for anthropogenic influence on regional rainfall (Bindoff et al. 2013). We hope this is clearly explained in both the description of the attribution method (SM Section S2) and in the results section i.e. 'We estimate that the risk of a 2015-16 magnitude SPEI-7 drought over SA to have increased by approximately two times due to the effects purely of anthropogenic warming, ignoring changes to other climate variables and variability' (lines 324-7). The role of dryland-atmosphere interaction is to some extent accounted for in our method, since the magnitude of the estimated anthropogenic effect on temperature trends (which drives our SPEI) includes those effects at least as simulated in the climate models. We feel that the greatest uncertainties are likely to be associated with the estimation of the return period values for extreme events and we have now strengthened the caveats around our estimate in Section S2.

3. Lines 259-291: The authors report that there are discrepancies between the groundwater water storage (GWS) estimates, involving their three-member GRACE dataset ensemble mean and with the ensemble of SPEI-7 datasets, in both meteorological fields and PET methodologies (e.g., Penman-Monteith vs. Thornthwaite). The question comes up about the different datasets that are used in the SPEI metric methods, i.e., GPCP and CRUTS3.24.01, and different water storage term ensembles, using GLDAS inputs, which use different meteorological datasets, e.g., the GDAS and CMAP-based forcings. Could these factor into the differences seen between the SPEI anomalies and deltaSMS and deltaGWS anomalies? Also, the SPEI is derived based on the data record from 1901 to present, which would be a different period from the GRACE measurements (2002-2016) and then again for the GLDAS datasets (2000-present, if using GDAS). Authors may want to address these possible discrepancies as well.

The reviewer raises a similar point to the first comment of reviewer 1 and we refer to our response to that. In specific response to the query about the potential difference between SPEI-& derived from GPC vs CMAP data we have now done that comparison (see below) and note that our the pattern of inconsistency between SPEI-7 and GRACE remains, which we consider in Section 3.2.1. The issue of the differing relative magnitudes of SPEI-7 and TWS anomalies over South Africa is less apparent with SPEI-7(CMAP) and we note that in point (i) in para 2 of Section 3.2.1 (lines 389-391)





Finally, in relation to their results and discussion on this topic, the authors may want to consider that the time windows relevant to the SPEI fields and those of GRACE, and other LSM-based fields, can be different and that the recharge or other drawdowns of groundwater can vary and take time in response to the rainy season. The authors point out this lag in lines 304-306 in relation to figure 5. The October-April SPEI timeframe may not have exactly aligned with the GRACE-ensemble (e.g., deltaTWS) and LSM ensemble (e.g., deltaSMS), as the response to the lower layers may be better reflected in a lagged timeframe (e.g., December-June). Also, trends in the TWS may already have been present that the SPEI-7 may not have captured, given the differences in datasets. Authors may want to look at other studies that have addressed such issues, such as Hassan and Jin (2016), Rodell et al. (2018), and Zhao et al., 2017:

We note this point now in Section 3.1.2 (lines 385-390) and cite the suggested references.

4. Lines 323-325: The authors mention here that the GRACE ensemble-based deltaGWS in the early part of the 2015-2016 drought had a high amount of uncertainty and did not correspond well with the piezometry data for the Limpopo site region. It would be of interest here if they

could identify which of the three GRACE TWS anomaly products contributed to the higher blue shaded region in the last half of 2015. Note the lower minimum values of the ensemble spread show some steep decline from late 2015 into 2016. Though the authors do point to the Scanlon et al. (2018) study in lines 302-303 of the Supplementary Information document, it would be of interest to the community to know which product contributed to this GWS reduction.

We thank the reviewer for highlighting the uncertainty in GRACE TWS signals for the Limpopo Basin particularly for the period of 2015-16. We have now looked at the individual TWS time-series data for 3 GRACE products (CSR, JPL-Mascon and GRGS) and found that differences among the three GRACE products for the 2015-16 period are substantial (Fig. S3). The JPL-Mascon (MSCN) shows negative anomalies whereas both CSR and GRGS feature slightly positive anomalies. We provide this new plot of GRACE TWS time-series data for both Limpopo and Makutapora basins in the revised supplementary material (Fig. S3).

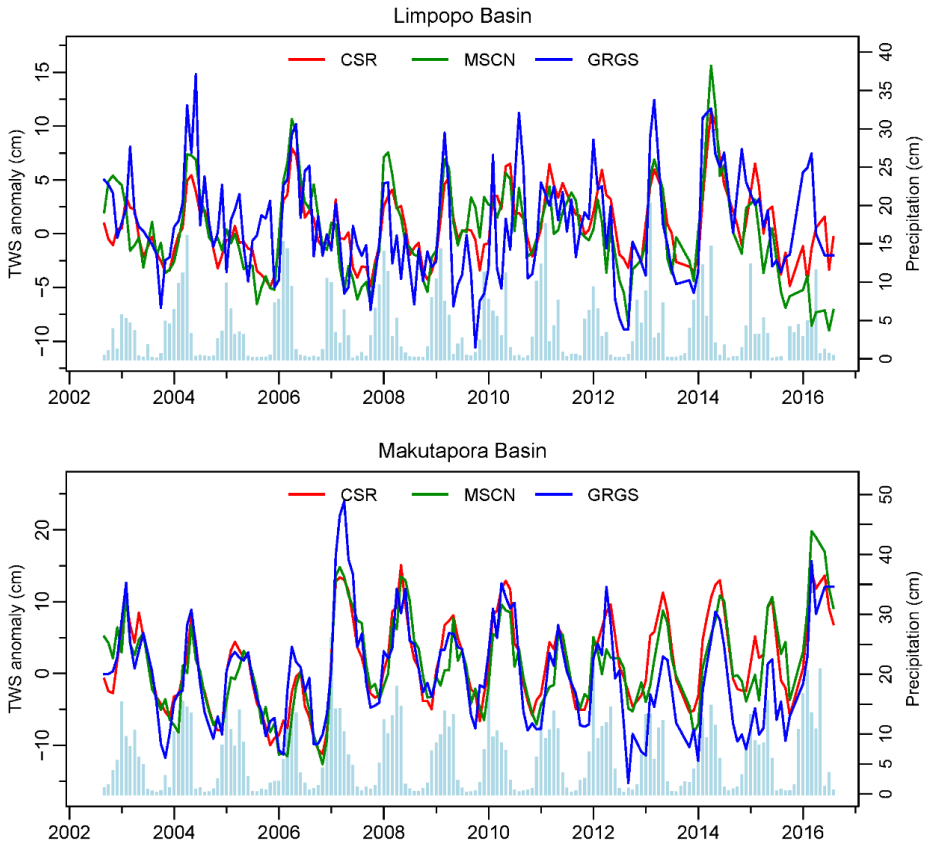


Figure S3: GRACE TWS time-series data for Limpopo and Makutapora basins.

Technical corrections Main manuscript: 1. Lines 96 and 111: Noticed that authors use "EASA" instead of "EASE" for the northern of the two domains in these two lines. They should be replaced with "EASE"?

Corrected

2. Line 161: Should the reference to “figure S1(b)” actually be to “figure S1(d)”, if highlighting the SST anomalies associated with this 7-month period?

Corrected

3. Line 217: Authors may want to replace the article “an” in front of “East Pacific” with either “than” or “in” here.

Corrected

4. Lines 221-222: The last phrase of this sentence is not fully clear: “and statistically this 2-year drought event remarkably unlikely”. Please clarify what is meant here.

The return period estimates for a consecutive 2-year SPEI-7 IAF curves for 2014-16 are extremely high such the uncertainty is too poorly constrained to have confidence. So we prefer not to provide the absolute values

5. Line 315: Remove the comma after “GWS” and before “suggests”.

Corrected

6. Lines 341-344: This sentence is a bit awkward in places, e.g., “The magnitude of major GRACE increases in deltaGWS”, or “with no response apparent in piezometry.” It is recommended to improve these phrases and overall clarity of the sentence.

Corrected

7. Lines 358-362: This is a long run-on sentence, and it is recommended to break this sentence in to two separate ones to improve its readability.

Corrected

8. Line 404: Place the period after the “l” in “et al., 2018).

Corrected

9. Figure 1b: It is unclear about how the 80th percentile of the rainfall anomalies is established. Is this constructed relative to the EASE box? Please clarify further how the positive and negative anomalies are established in Figure 1b (in the main text) relative to the 80th percentile.

This is now explained in the methods section

10. Figure 3 caption, line 619: “men” should be changed to “mean”.

Corrected

Supplementary manuscript:

1. Line 107: “EASA” occurs here as well.

Corrected

2. Line 116: “Penman-Montieth” should be spelled: “Penman-Monteith”.
Corrected

3. Line 124: Can remove either “use” or “derive” in front of “percentiles”.

Corrected

4. Line 125: The authors may want to provide the full name for TRMM 3B42 product, not just the acronym for the satellite and precipitation product. Also, it may be helpful to specify here which years of the TRMM product were used.

Corrected

5. Caption for Figure S1: The final sentence description for S1-d seems incomplete. What period was the anomalies derived from?

Information provided

6. Figure S2a, for Limpopo location, the shading in the top four panels is missing, unlike that for S2b, which shows the shading in those panels for Makutapora. Also, recommend placing the word “and” between “(a) Limpopo” and “(b) Makutapora”.

Shading is included in both (a) and (b).