Author comment to the Referee comment by Matthew H. England on the manuscript

Extending and understanding the South West Western Australian rainfall record using a snowfall reconstruction from Law Dome, East Antarctica

by Yaowen Zheng, Lenneke M. Jong, Steven J. Phipps, Jason L. Roberts, Andrew D. Moy, Mark A. J. Curran, and Tas D. van Ommen,

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We are glad that the reviewer enjoyed reading the manuscript. We thank him for the time that he spent reading and reviewing it. We respond to each of the general and specific points below. The reviewer's comments are shown in **bold text**, replies are shown in normal text, text from the original manuscript is shown in blue, and proposed changes to the manuscript are shown in red.

This paper extends a previous estimation of Southwest Western Australian (SWWA) rainfall using a snowfall reconstruction from Law Dome, East Antarctica. Overall the paper is well written and thorough, with appropriate use of statistical methods and suitable conclusions given the analyses obtained. It's a nice contribution to the field, and I very much enjoyed reading the paper.

Nonetheless, I have some suggestions that the authors should take into consideration when preparing a revised manuscript. Once suitably revised, I expect to recommend the paper for publication in Climate of the Past. I am happy to look at the revised manuscript when it's available for review.

General comments:

1 The square of the correlation coefficients (R2) reveals the fraction of variance in SWWA rainfall explained by the snowfall reconstruction at Law Dome. So this explained variance maxes out at only around 25-35% (i.e. 0.5 - 0.6 squared). I think this needs to be acknowledged in the paper (for example, added as a new column in Table 3). This relatively low explained variance suggests that tropical and subtropical influences also play a significant role in driving rainfall variations over the region. Implying that the rainfall estimates from the Law Dome snow proxies carry considerable uncertainty. It's important to expand on this point in the discussion / conclusions.

We thank the reviewer for this comment. We agree that the explained variance of ∼25-35% should be acknowledged in the manuscript and we have revised the text accordingly at Lines 169-171: "...showing the consistency with the significance of MASK. The square of the correlation coefficients have shown the explained variance are maximum at around 30-40%. The tropics and subtropics can play an important role in driving rainfall changes in SWWA (Smith et al., 2000; England et al., 2006; Ummenhofer et al., 2008). Using the Law Dome ice core snow accumulation proxy to reconstruct the SWWA rainfall has non-negligible uncertainty. However, in general the proxies are rare in such area suggesting this 30-40% makes a valuable contribution to our ability to reconstruct past climate. Therefore,..."

2 Related to this point, there are several studies that are not yet cited in the paper that make this link from the tropics and subtropics to SWWA rainfall, including (but not limited to) the following:

England, M. H., C. C. Ummenhofer, and A. Santoso, 2006: Interannual rainfall extremes over southwest Western Australia linked to Indian Ocean climate variability. J. Climate, 19, 1948-1969.

Ummenhofer, C. C., A. Sen Gupta, M. J. Pook, and M. H. England, 2008: Anomalous rainfall over southwest Western Australia forced by Indian Ocean sea surface temperatures, J. Climate, 21, 5113- 5134.

Smith, I. N., P. McIntosh, T. J. Ansell, C. J. C. Reason, and K. McInnes, 2000: Southwest Western Australian winter rainfall and its association with Indian Ocean climate variability. Int. J. Climatol., 20, 1913–1930.

The authors need to expand their discussion of the tropical and subtropical influence on SWWA rainfall, perhaps around lines 35 - 50 of the Introduction, or after that paragraph. And more clearly acknowledge that the SAM is not the sole driver of SWWA rainfall variability. Mention is made of this, but it needs to be expanded in relation to tropical and subtropical influences.

We thank the reviewer for these suggestions and have added these references to the manuscript.

We have also expanded the discussion of the tropical and subtropical influences on SWWA rainfall from Line 47, as follows:

This shift, in conjunction with the increase in anthropogenic greenhouse gases over this period, may be responsible for at least part of the reduction in SWWA rainfall (Cai and Shi, 2005).

Negative sea surface temperature anomalies in the eastern Indian Ocean and positive sea surface temperature anomalies in the central subtropical Indian Ocean are related to dry years in SWWA (Ummenhofer et al., 2008). Some indication was found that mean sea level pressure anomalies over the Indian Ocean drive Indian Ocean sea surface temperature anomalies and SWWA rainfall, but this link did not appear robust at the interannual time scale (Smith et al., 2000). A link between SWWA rainfall extremes and large-scale Indian Ocean climate was found due to moisture advection onto the SWWA coast (England et al., 2006). This is subject to influence from the large-scale wind field over the eastern and southeastern Indian Ocean, which may contribute to SWWA rainfall extremes (England et al., 2006). This link between the Indian Ocean and SWWA rainfall may be influenced by the IOD–ENSO link (England et al., 2006). Thus, the tropical and subtropical Indian and Pacific Oceans are both likely to play a role in SWWA rainfall variations, but not the only role. Changes of a similar magnitude to those observed can potentially also arise through natural multidecadal climate variability (Cai and Shi, 2005). Thus the drivers of the winter rainfall attenuation in SWWA are still unclear.

3 It's curious that the relationship between snowfall at Law Dome and SWWA rainfall is maximised with a 5–6 year low pass filter. This surprised me; I would've thought the annual signal would dominate. Is there any climatic reason for this? Dominant frequencies of variability of both the SAM and SWWA rainfall do not include a 5-year signal as far as I know. It would be good for the authors to expand on this discussion a little — the statistical analysis is clear, but what is the climatic interpretation?

Thank you for this comment. We used the same methodology as van Ommen and Morgan (2010), who found that smoothing was required due to noise introduced by surface processes at the ice core site. We have revised the text accordingly at Lines 134-136: "Low-pass filtering (or smoothing) the data increases the correlation of the precipitation time-series between SWWA and Law Dome (van Ommen and Morgan, 2010). Annual-scale noise arises from site surface processes and snow accumulation variability, and is ameliorated by the smoothing."

4 The analysed rainfall data only extends up to 2015 in this study (lines 270-272). I realise that the snowfall accumulation data may not be available after this year, but the reference to whether the drought and prolonged dry period might be continuing after 2015 is made without being quantified here. I suggest that the authors at least clarify this for rainfall post-2015? Did the dry spell continue during 2016-2020? Otherwise the reader is left hanging here.

Thank you for this comment. We have downloaded and plotted (Figure 1) the latest rainfall data from the Australian Government Bureau of Meteorology for the growing season in southwestern Australia from 1900 to 2020. The data is available at: http://www.bom.gov.au/climate/change

The mean growing season rainfall in southwestern Australia from 1900 to 1970 is 564.97 mm, from 1972 to 2014 is 481.26 mm and from 2016 to 2020 is 431.35 mm. The drought continued during 2016–2020.

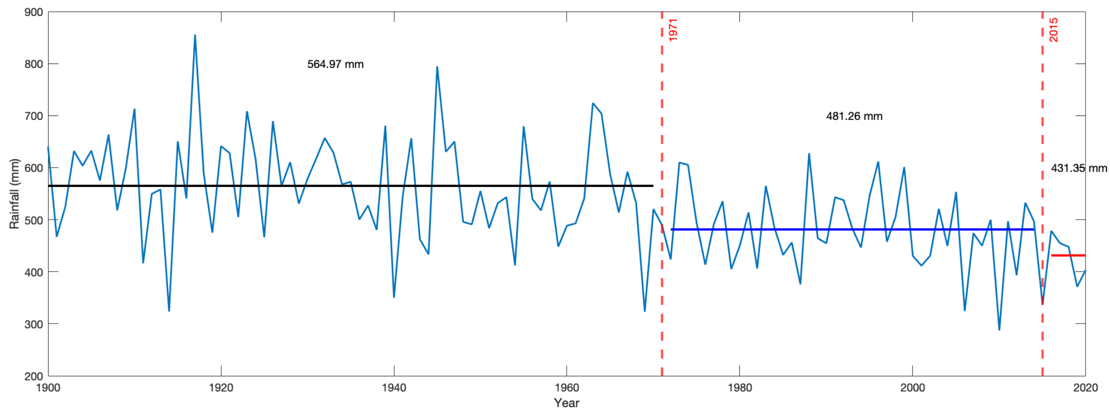

Figure 1. Time series of BoM rainfall in southwestern Australia at growing season from 1900 to 2020.

5 Figure 6 is very nice!

Thank you!

6 The authors note that the model has a dry bias (lines 284-285 and also Table E1). With SWWA rainfall impacted by the Southern Annular Mode, and with the westerly winds and the storm tracks generally having an equatorward bias in climate models, this is kind of surprising (this bias should lead to a wetter than observed SWWA region). Can the authors speculate on the possible reasons for the model's dry bias?

The model has a relatively coarse spatial resolution: for example, the SWWA region is represented by a single point on the atmosphere model grid. The model is not therefore able to capture finer-scale topographic variations, with the consequence that precipitation can tend to be too low along the western margins of continental landmasses. We speculate that this accounts for the dry bias that we find in the current study.

7 The use of the single and multiple forcing coupled model experiments is very nice, allowing the authors to separate out various climate forcings, including solar, greenhouse gases, volcanic and orbital. I liked this analysis and the associated discussion is very clear. An experiment with just stratospheric ozone depletion forcing would have been interesting as well, given the SAM link, but if this is not available to include, no problem.

We thank the reviewer for this comment.

In regard to ozone depletion, the CSIRO Mk3L climate system model does not include atmospheric chemistry and so no simulations exist that include changes in stratospheric ozone. While we acknowledge that we could include additional models, such as the CMIP5 or CMIP6 ensembles, we feel that this is beyond the scope of the current study.

8 The figures are generally of good quality, but the font sizes of all axis labels and figure legends etc. is often far too small. Definitely worth fixing this before publication.

We have modified the figures accordingly.

9 The final sentence or two of the paper is devoted to mentioning the two (possible) previous prolonged drought events over SWWA in the proxy-estimated record. To me this is an interesting finding, but not the major "take home" finding of the study. I suggest the authors add a final sentence beginning "However,..." wherein they describe the finding that GHG forcing is the likely driver of the SWWA rainfall decline since the early 1970s.

Thank you for your suggestion. We have accepted your suggestion and added a final sentence accordingly: "However, forced climate model simulations indicate that anthropogenic greenhouse gases are the dominant driver of the rainfall reduction in SWWA since the early 1970s."

10 Figure B1: There are peaks in both panels (a) and (b) at two years. What is the climatic interpretation of this?

This is likely to be a combination of many different processes. We are not aware of any specific climatic interpretation.

Minor suggestions:

11 Lines 43-44: Thompson and Solomon 2002 do not show analyses of the Southern Annular Mode link to SWWA rainfall. This reference should be changed to Thompson et al. 2011:

Thompson, D. W. J., S. Solomon, P. J. Kushner, M. H. England, K. M. Grise and D. J. Karoly, 2011: Signatures of the Antarctic ozone hole in Southern Hemisphere surface climate change, Nature Geoscience, 4, 741-749.

Thank you for pointing this out. We apologize for this mistake. We have changed the reference accordingly at Lines 43-44: "...is a large-scale mode of climate variability that is correlated with rainfall in WA (Gong and Wang, 1999; Thompson et al., 2011; Fierro and Leslie, 2013)."

12 Line 58: Add a citation to Goodwin et al. 2004 here alongside the reference to van Ommen and Morgan 2010:

Goodwin, I. D., , T. D. van Ommen, , M. A. J. Curran, , and P. A. Mayewski, 2004: Mid latitude winter climate variability in the South Indian and southwest Pacific regions since 1300 ad. Climate Dyn., 22, 783–794.

We have added this citation to the text at Line 58: "A relationship between rainfall in SWWA and the snowfall recorded in Dome Summit South (DSS) ice core drilling site on Law Dome, East Antarctica was found by Goodwin et al. (2004); van Ommen and Morgan (2010)."

13 Lines 104-105: England et al. (2006) also analyse the quality of the Mk3L model simulations in relation to observed interannual variability of SWWA rainfall. Perhaps cite their findings here.

Thank you. We have included these references, making the following change at Lines 104–105: "Both CSIRO Mk3 and CSIRO Mk3L produce credible simulations of large-scale precipitation, including over Australia (Cai et al., 2003; Cai and Shi, 2005; England et al., 2006; Phipps et al., 2011)."

14 I found the shorthand term "MASK" a little obtuse for the region of statistical significance of the rainfall signal over SWWA. I think it is okay to identify that region upfront as the authors have done, but then just say that hereafter, when describing SWWA rainfall, it is taken to indicate the region delineated by this area in Fig. 1. I think the resulting text will be clearer that way.

Thank you for this comment. We agree. We have made the following changes at Lines 164-166: "In order to quantify the MASK correlation coefficient and evaluate the statistical significance, we multiply the mask matrix (in the region has a value of 1 and outside the region has a value of 0) of the MASK with the AWAP gridded data to generate the MASK rainfall. Hereafter, the SWWA rainfall we are describing, is the MASK rainfall where the MASK region is delineated in Figure 1. Then we calculate the ~~MASK~~ SWWA rainfall correlation coefficient with DSS and test its statistical significance."We have changed all the "MASK rainfall" to "SWWA rainfall" after Line 166.

15 The amount of explained variance R2 should be added to Table 3, expressed as a percentage. I think this would add to the information provided in that table.

Thank you for this comment. We agree. We have made the following changes to Table 3:

Table 1. The Pearson correlation coefficients for the ~~MASK~~ SWWA rainfall and the four BoM stations rainfall with the DSS snow accumulation. R^2 is the square of the correlation coefficient. All the correlations are statistically significant (6-year window, $p < 0.05$).

Sample	Correlation coefficient	R^2	Year (CE)
~~MASK~~ SWWA	-0.597	0.356	1900–2015
Arthur River	-0.548	0.300	1891–2015
Boyanup	-0.623	0.388	1898–2013
Cranbrook	-0.540	0.292	1891–2015
Wonnaminta	-0.546	0.298	1905–2015

16 Lines 169-170: are there also non-significant stations within this MASK region? If so, the authors should point this out.

Thank you for this comment. There are five non-significant stations within this MASK region (Figure 2). We have revised the manuscript accordingly at Lines 170–171: "... four stations are all geographically located in the MASK region (Figure 1) showing the consistency with the significance of MASK. Five other stations within the region have correlations of a similar magnitude, but these correlations are not significant at the 5% probability level."

17 Typo, Figure 2 caption, line 1: plor -> plot

Thank you for pointing this out. We have made the following change to the caption of Figure 2: "(a) The scatter ~~plor~~ plot for AWAP rainfall in MASK region and DSS snow accumulation (both 6-year window) of period 1900 CE to 2015 CE with their linear model and 95% CI. (b) The histogram for model residuals using probability density function scaling. (c) The scatter plot for model residuals and fitted data with their linear fit and 95% CI."

18 The term "model outputs" is used often in the paper, I would suggest changing this to terms like "the model simulations", "the model runs", "the model experiments" (etc.) throughout the manuscript.

Thank you for this comment. We agree using the term "the model simulations" is better than "model outputs". We have changed all the "output/ outputs" to "simulation/ simulations" throughout the manuscript.

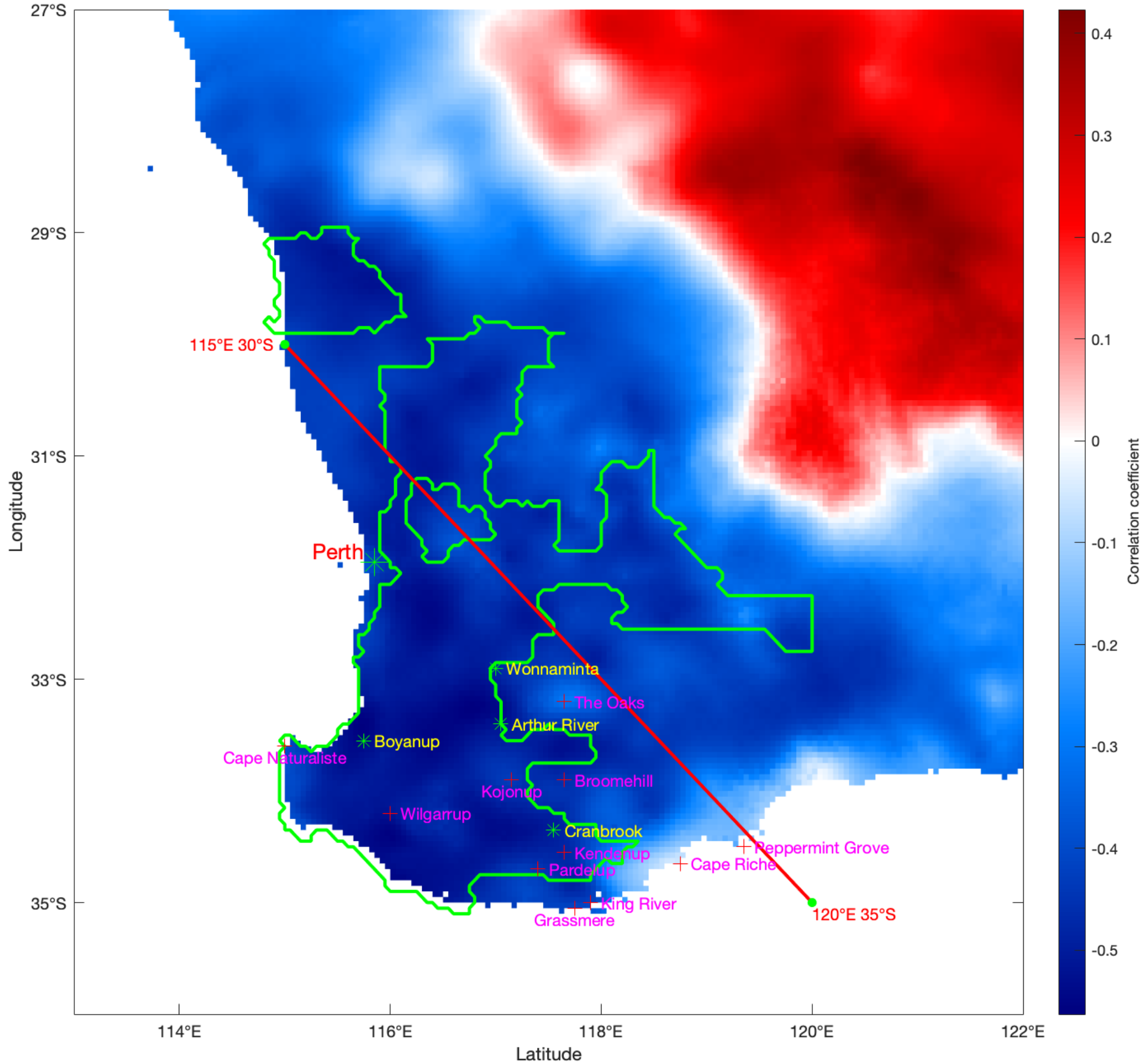

Figure 2. The correlation map for the southwest part of WA region for 6-year window AWAP rainfall and DSS snow accumulation from 1900 CE to 2015 CE. The outline area (green line) is the MASK region where the correlation is statistically significant ($p < 0.05$). Red diagonal line connecting 115°E 30°S and 120°E 35°S is the boundary of SWWA (van Ommen and Morgan, 2010). Perth is the capital city of WA. Boyanup, Wonnaminta, Arthur River and Cranbrook are the four significant (6-year window, $p < 0.05$) stations. Red "+" marked stations are the other 11 non-significant stations.

19 Figure E1: I understand why the authors may wish to include this diagram for completeness, but each of the panels looks basically the same as each of the other panels. So I wonder if there is any utility in this diagram being included?

Thank you for this comment. We do not fully agree. Figure E1 supports the statement in the manuscript at Lines 285-288: "The simulated rainfall for each individual ensemble member is shown in Figure E1, with the corresponding CUSUM time series shown in Figure E2. Within each forced ensemble, there are considerable differences between the CUSUM time series for individual ensemble members. This highlights the role of unforced internal variability in driving SWWA rainfall, consistent with the findings of Cai and Shi (2005)." However, in regarding to this comment and also in regard to a comment by Referee 1, we will move all of the appendices to a supplementary document.

References

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van Ommen, T. D. and Morgan, V.: Snowfall increase in coastal East Antarctica linked with southwest Western Australian drought, Nature Geoscience, 3, 267–272, 2010.