

Review of the paper: "Extending and understanding the southwest Western Australian rainfall record using a snowfall reconstruction from Law Dome, East Antarctica"

By: Zheng, Jong, Phipps, Roberts, Moy, Curran, and van Ommen

Submitted to *Climate of the Past*

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 (R^2) 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.

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.

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?

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.

5. Figure 6 is very nice!

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?

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.

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.

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.

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

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.

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.

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.

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.

15. The amount of explained variance R^2 should be added to Table 3, expressed as a percentage. I think this would add to the information provided in that table.

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

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

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.

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?

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