Author comment to the Referee comment by Anonymous Referee #1 on the manuscript

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

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We are glad that the reviewer thinks the topic of the manuscript is very interesting. We thank them for the time they have 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.

The authors propose a reconstruction of precipitation in South West Western Australia (SWWA) based on snowfall estimate from an ice core in East Antarctica. The authors also compare this reconstruction with model results to identify the role of various forcings and natural variability in the rainfall changes during the past 2000 years. This allows them to conclude that the recent decrease in precipitation is likely due to anthropogenic forcing. The topic of the study is very interesting. The proposed statistical analyses are well described and justified. Precipitation reconstructions are rare in the region so a long time series as the one proposed in the manuscript is more than welcome and the analyses are useful to understand the potential origin of the reconstructed changes. However, I see several issues that need to be addressed, to reach stronger and more robust conclusions, before the paper can be accepted for publication.

General points

1 A previous study (van Ommen and Morgan, 2010) has proposed a link between precipitation in SWWA and snowfall at Law Dome. The authors confirm this link based on a least square linear regression. They discuss the validity of this statistical link but not the mechanisms that could explain it. A few lines are available in the introduction, based on the results of van Ommen and Morgan (2010) but we have now 10 more years of data and it would be instructive to see if this confirms the conclusions reached 10 years ago or not. A point that seems puzzling is the link between an annual mean record (Law Dome) and precipitation during the growing season in SWWA. This should be discussed and justified.

We thank the reviewer for this comment, however, this paper does not explore the mechanisms for the link itself. These were considered in van Ommen and Morgan (2010), which points to variations in meridional circulation south of Australia. We have added one sentence at around Line 61 to explicitly mention this mechanism of van Ommen and Morgan (2010) in the text. Line 61: "The relationship consists of a statistically significant negative correlation between winter JJA mean SWWA regional rainfall and Law Dome, East Antarctica, and the mechanism of this link is the variations in meridional circulation south of Australia(van Ommen and Morgan, 2010)."

The Law Dome ice core record has a well-defined annual chronology that allows annual snowfall to be determined, but a suitable method for extracting sub-annual accumulation from the ice core has not been developed. While an accurately registered snowfall accumulation series for winter would be desirable and might be expected to correlate more strongly with SWWA rainfall, such seasonal accumulation data are not available.

2 For the reconstruction, the authors assume the stationarity of the link between snow accumulation at Law Dome and precipitation in SWWA, as classically done. However, that stationarity needs to be tested. A classical test is to separate the time series in a calibration period, over which the statistical model is developed, and a validation period, over which its skill is evaluated. In this framework, it is important to test how the magnitude of the correlation is influenced by the post 1971 trend, calibrating for instance the model over the period 1900-1971 and evaluating then the correlation over 1971-2015. This is important for the reconstruction as, if the link is weaker before 1971 (because of a strong role of the anthropogenic forcing on the observed link), this could decrease the validity of reconstruction for the pre-industrial period. In this framework, this would be nice on Figure 2 to have also the observed time series of precipitation in SWWA and snow accumulation at Law Dome for a first visual comparison of the records.

Thank you for pointing this out. However, we think this might be a validation issue instead of a stationarity one. To validate the reconstruction, we have now performed a jackknife analysis over the period 1900-2015 for the mean AWAP data in the "MASK" region. Specifically:

1. We determine the autocorrelation length by calculating the autocorrelation at various time lags (e.g. lag-1, lag-2, lag-3, ..., lag-6, ...) on the unsmoothed observational data until the autocorrelation coefficient decreases to zero.
2. We use this autocorrelation length to perform a (modified) jackknife analysis.
3. For each subsample, we perform 6-year smoothing then calculate the correlation coefficient and construct the linear model.

The autocorrelation time-scale is 12 years. The results of the jackknife analysis are shown in Table 1. The correlation coefficients for the individual members of the jackknife ensemble are tightly clustered around the value of -0.597 for the full reconstruction. Furthermore, the overlap between the 95% confidence intervals for the gradient and intercept indicates that the models derived for each jackknife ensemble member are statistically indistinguishable at the 5% probability level. This demonstrates the robustness of the reconstruction technique.

Table 1. The correlation coefficient between the smoothed ice core record and the mean of the AWAP rainfall in the "MASK" region. x1 and Intercept are the coefficients for the linear model: Rain = Snow * (x1 $\pm$ 95% CI) + Intercept $\pm$ 95% CI mm/year. The 95% confidence interval (CI) is estimated by multiplying the standard error of the model by 1.96.

Period	Correlation coefficient	x1	Intercept (mm/year)
1912–2015	-0.536	-0.385 $\pm$ 0.118	667 $\pm$ 85
1900–1911 & 1924–2015	-0.513	-0.363 $\pm$ 0.118	651 $\pm$ 85
1900–1923 & 1936–2015	-0.532	-0.378 $\pm$ 0.117	662 $\pm$ 85
1900–1935 & 1948–2015	-0.524	-0.405 $\pm$ 0.128	684 $\pm$ 93
1900–1947 & 1960–2015	-0.545	-0.407 $\pm$ 0.122	685 $\pm$ 88
1900–1959 & 1972–2015	-0.499	-0.385 $\pm$ 0.130	671 $\pm$ 94
1900–1971 & 1984–2015	-0.506	-0.427 $\pm$ 0.141	699 $\pm$ 101
1900–1983 & 1996–2015	-0.492	-0.374 $\pm$ 0.129	662 $\pm$ 92
1900–1995 & 2008–2015	-0.512	-0.356 $\pm$ 0.116	651 $\pm$ 82
1900–2007	-0.593	-0.379 $\pm$ 0.098	670 $\pm$ 71
1900–2015	-0.597	-0.389 $\pm$ 0.114	672 $\pm$ 82

3 For the stationarity, this should also be tested using the model results to check if the model reproduce well the observed correlation for the 20th century and if the value remains stable for the previous centuries.

The ability of the Law Dome accumulation record to be used to make inferences about shifts in SWWA rainfall hinges upon the stability of the relationship between accumulation at Law Dome and the Southern Hemisphere atmospheric circulation. To explore this, we use the three members of ensemble OGSV to calculate the correlation between precipitation at Law Dome and

Southern Hemisphere mean sea level pressure. The results are shown in the following Figure 1, for the periods 1975-2000 CE (top row), 1851-2000 CE (middle row) and 501-2000 CE (bottom row).

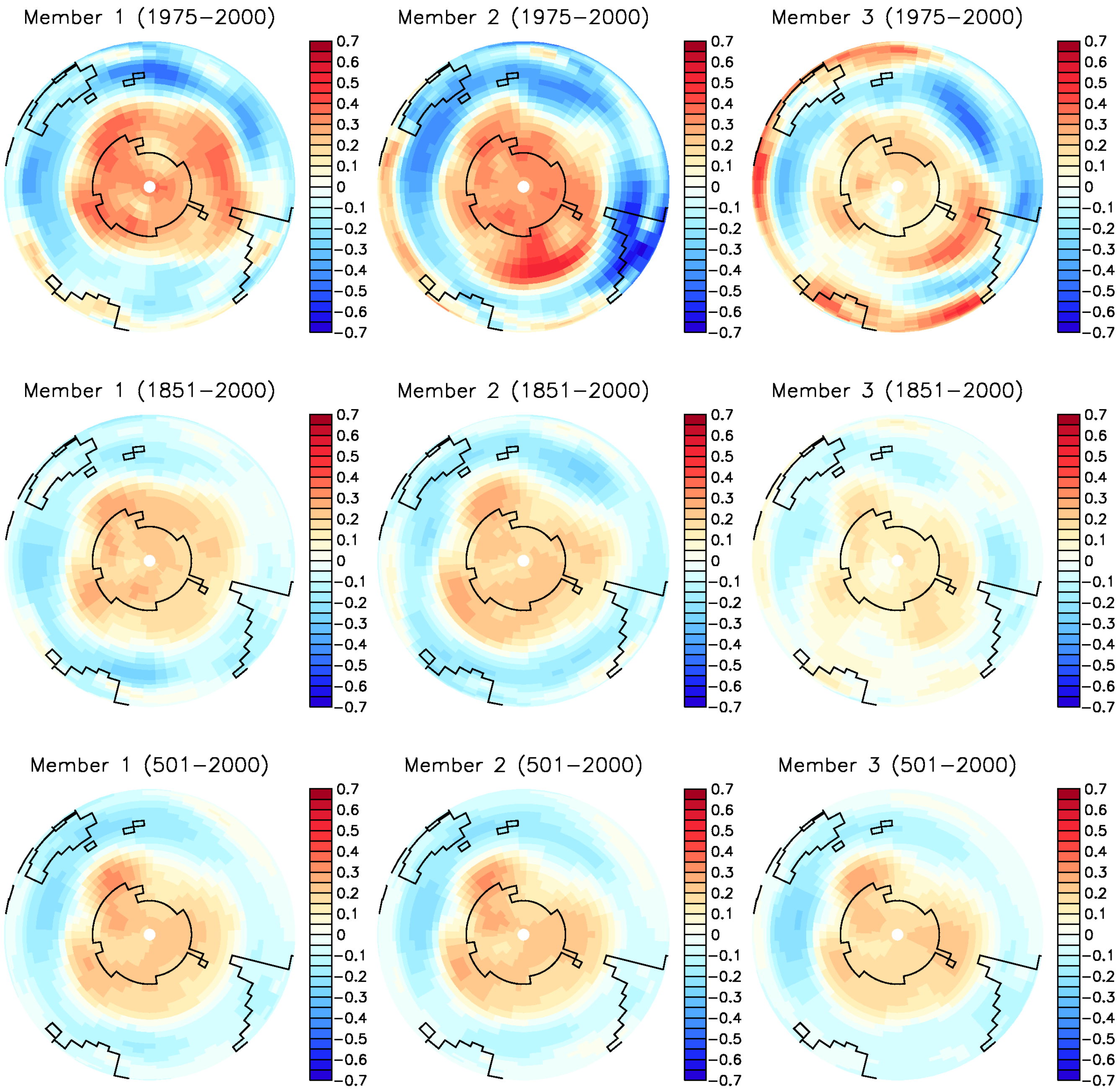

Figure 1. Correlation between precipitation at Law Dome and Southern Hemisphere mean sea level pressure for each member of OGSV.

Interannual variability gives rise to some differences between the individual model siulations, particular on shorter timescales. However, the pattern of positive correlations over the high-latitude Southern Ocean, surrounded by negative correlations at mid-latitudes, is consistent with the observed relationship (as shown in van Ommen and Morgan, 2010) and can be seen to be stable over the full 1500 years of the climate model simulations.

4 The authors compare the pre-industrial period (before 1850) with the recent past but the justification of this date is based on a commonly admitted value, not on the reconstruction itself. From a visual interpretation of the curve, it seems to me that not shift is present at that time in the series. This could be tested with the methods used to detect the shift in 1971. The differences between 22 BCE-1849 and 1851-2015, shown for instance on figure 4, may be only due to the period 1971-2015. This could be tested by checking if there is also a difference between the periods 22 BCE-1849 and 1851- 1971. If there were no difference, this would mean that changes really occurs after 1971, not after 1851 (as discussed for Figure 6). This would also have an impact for the interpretation and for the model-data comparison as the the shift in 1850 seems clearer in the model (this should be tested too).

Thank you. The BREAKFIT analysis on the CUSUM of the reconstruction showed the break-point is at 1971. However, the BREAKFIT analysis only picks the largest change in the gradient, with a single break-point as the output. This means that we did not know if there were other significant changes in the gradient, apart from at 1971. It is therefore possible that the differences between 22 BCE–1849 and 1851–2015 are due to the period 1971–2015.

To test this, we independently perform the two-sample KS test on the additional combination of periods. The results are shown in Table 2.

Table 2. The results for independent KS tests for different time periods from the rainfall time series. The result "same" means we cannot reject the null hypothesis (that the two samples of data are from the same distribution), while "different" means we can reject the null hypothesis ($p < 0.05$).

Sample	501–1849 VS 1851–1970
CONTROL	same
O	same
OG	different
OGS	same
OGSV	different
Rainfall reconstruction	different

The model simulations suggest an anthropogenic influence beginning to emerge after 1850, which is at the limit of detectability at first because of natural climate variability. The reconstuction shows a secondary change due to anthropogenic influence during the epoch 1850-1970.

5 There is a good justification to use results from the CSIRO Mk3L climate model for the past millennium as not many ensemble exists with different forcing. Nevertheless, a strong point in the discussion is the post 1971 shift. The model underestimate this shift. The authors rightly argue that this may be because the model does include stratospheric ozone forcing (lines 335-336). This can be easily tested using climate model results available for this period, but including ozone forcing. A main conclusion of the study is that anthropogenic greenhouse gases have contributed to the observed decline in precipitation but this is based on one model, which do not include one of the potentially dominant forcing to explain the changes. Comparing the reconstruction with other models results, in particular those including stratospheric ozone forcing, would allow reaching stronger and more valid conclusions. It would also allow discussing the potential reasons why the shift occurs in 1971 and not earlier.

We agree that using multiple models (especially those which include stratospheric ozone forcing) would be helpful in better understanding the rainfall shift in 1971. This is beyond the scope of the current study, but would be a very useful avenue for further investigations into the observed changes in SWWA rainfall.

We have added some text in the paragraph at Lines 364-367: "Both the reconstruction and the climate model simulations suggest that the drying trend began earlier than the 1970s. However, the model simulations do not capture the acceleration in the reconstructed drying trend at around 1971 CE. This suggests that this acceleration cannot be attributed to external forcings, or at least not any of the forcings considered in this study. Either natural variability or stratospheric ozone depletion are potential alternative explanations (e.g. Cai and Shi, 2005). A investigation into possible ozone forcing and a comparison against multiple climate models is a promising avenue for future work."

6 The way internal variability is addressed in not always adequate. The reconstruction should always be compared to an individual member (or the range of the members) not to ensemble means. Ensemble means are useful to identify the forced response. However, ensemble means should not be used for the control run (lines 304), as for Figure 7 (if I understand well the figure). The interest of the control run is to estimate the range of internal variability as simulated by the model and this is strongly reduced for ensemble means by construction. If you use the ensemble mean, you cannot reach any conclusion on the role of internal variability in the sentence 'Examining each of the ensembles in turn, we see that CONTROL does not capture the key features of the the rainfall' (lines 307-308, two time 'the' before 'rainfall'.).

Thank you, but we do not fully agree. In general, the ensemble mean has a better signal-to-noise ratio and is therefore more representative of the response to external forcings than any individual ensemble member. For this reason, the bulk of the analysis is performed on ensemble means. We intentionally processed the CONTROL simulations in the same way as the forced simulations to derive independent estimates of the amplitude of noise in the four forced ensembles.

Where we are considering internal variability, for example at Lines 286–288, we examine the individual ensemble members separately.

Thank you for pointing out this typographical error. We will make the change in Line 307–308: "Examining each of the ensembles in turn, we see that CONTROL does not capture the key features of ~~the~~ the rainfall reconstruction (Figure 7)."

7 The comparison between the recent drying and past dry periods is too short (see for instance lines 289-291) to reach strong conclusions (lines 369-371) This is an interesting point and the way this comparison is performed should be explained and justified much more extensively.

We agree and we have revised the manuscript accordingly. Following "...consistent with the findings of Cai and Shi (2005)." at Line 288, we have replaced the text with the following paragraph:

"A number of prolonged droughts that approach the magnitude of the current integrated rainfall deficit are apparent in the reconstruction (Figure 3c), as discussed in Section 4.1. These prior drought epochs occurred during the pre-industrial era, suggesting that they may have arisen through natural climate variability or natural forcings such as volcanoes. To explore this within the model simulations, the 45-year changes in the CUSUM for the pre-industrial control simulation are shown in Figure E3. A number of prolonged droughts are also apparent in all three ensemble members, for example at years 589–633 CE in Control1 (Figure E3b). This supports the hypothesis that prolonged droughts of a similar magnitude to the currently observed drought can arise to natural climate variability or natural forcings."

At Line 369, we have replaced "Droughts of similar duration and magnitude occur in an unforced pre-industrial control simulation, suggesting that these events may have arisen through natural climate variability." with " Droughts of similar duration and magnitude also occur in an unforced pre-industrial control simulation. The model simulations therefore support the hypothesis that these pre-industrial drought events may have arisen through natural climate variability. However, forced climate model simulations indicate that anthropogenic greenhouse gases are the dominant driver of the rainfall reduction in SWWA since the early 1970s."

Specific points

8 Line 19. Just by curiosity, the growing season in the region is winter (May to October)? Is it the growing season also for wheat that is mentioned line 23?

Rainfall in SWWA is seasonal with $\tilde{7}5\%$ of the annual total falling in May-October (Ludwig and Asseng, 2006). This corresponds to the growing season for wheat cropping in this region.

9 Line 97. What is the method used to obtain a reconstruction at a resolution of 0.05°? This should be mentioned as this could explain part of the spatial structure obtained in Figure 1 for instance.

The AWAP data is 0.05° resolution gridded data. Jones et al. (2009) describes the method used to generate the gridded dataset. We describe the method that we use to generate the reconstruction at Line 152 to Line 157, "To define a region with statistically significant correlation between observed (AWAP) and reconstructed (DSS) rainfall over SWWA, we independently calculate the correlation coefficient and test its significance for 110 Local Government Areas (Australian Goverment, 2020) (details in Appendix C) in WA. There are 9 Local Government Areas smaller than the 0.05° longitude * 0.05° latitude geospatial resolution of the AWAP data grid. Therefore, we actually calculate and test 109 (Sorry, this should be "101" instead of "109") areas. There are 52 areas that are statistically significant (6-year window, $p < 0.05$). We combine these 52 areas to define the significant region (Figure 1) over SWWA. For convenience, we name this significant region as "MASK"."We took out that 9 areas but looking at the rest of 101 areas. The Australian Government (2020) data we used for every single 101 areas has the same 0.05° resolution and grids as the AWAP data. For each area, there is a mask matrix– the value equals "1" within the area and "0" outside the area. So we did Dot Product between the AWAP rainfall data and every single LGA to generate the AWAP rainfall in each area and independently calculated the correlation coefficient and tested its significance between each of 101 areas' AWAP rainfall and snowfall record, then we did addition on 52 significant LGAs' mask matrices to generate the mask matrix of the region "MASK". Then, we did Dot Product between the AWAP rainfall data and the "MASK" mask matrix to generate the AWAP rainfall in "MASK" region and calculated the mean "MASK" region rainfall which was used to obtain a reconstruction at a resolution of 0.05°.

We thank you for pointing out the spatial structure obtained in Figure 1. We apologize for this. We think the spatial structure (e.g. some gaps between the green line and the map) was due to the difference between the Western Australia land mask and the LGA masks. We preformed a scalar multiplication between the AWAP rainfall and the Western Australia land mask before performing the calculations to get remove the rainfall data in ocean areas, increase the speed of calculations and reduce memory usage.

10 Line 136. This is probably not very important for your results but a running average is not a very good method for smoothing and other filters, simple to implement but with much better properties, are available.

We thank the reviewer for this helpful suggestion. We will take it into consideration for future work.

11 Line 208. I do not understand why the comparison is between ensemble means and the reconstruction and which estimation of uncertainty is expected (see main point 6).

We have addressed this in our reply to main point 6 above.

12 Line 212 'lalso' instead of 'also

Thank you for carefully pointing out this typographical error. We have changed in Line 212: "We also estimate the adapted degrees of freedom (DF) using the Welch–Satterthwaite equation:"

13 Line 222. I do not follow ‘has shown the feasibility of investigating’. Please reformulate.

We agree this wording is unclear. We have changed in Line 222 from "This rainfall reconstruction has shown the feasibility of investigating the longer-term context of rainfall variability over SWWA before the instrument-era." to "This rainfall reconstruction has shown that it is possible to use the ice core to investigate the longer-term context of rainfall variability over SWWA before the instrument-era."

14 Line 238-239. This role of recharging events seems very speculative as it is presented. Is there references to support this hypothesis?

Yes, there is a reference to support this hypothesis. Gallant et al. (2013) states "Soil moisture is regulated by multiple hydro-climatic processes, such as evaporation, run-off and groundwater recharge, which are potentially important drivers of drought variability."

We now cite Gallant et al. (2013) at Lines 238-239: "...and this lack of recharging events might have more of an impact than the shift in the mean(e.g., Gallant et al., 2013) ."

15 The way the variable is presented on Figure 3C seems a bit strange to me. If CUSUM gives the cumulative sum of the record, what is the interest of ‘the 45-year running change in the rainfall reconstruction CUSUM series’ compared to a running mean on the series itself? There are maybe differences (in the magnitude in particular) but, if this diagnostics does not bring strong new information, it is probably simpler to just show the smoothed reconstruction.

Thank you for pointing this out, but we do not fully agree. We think the 45-year running change in the rainfall reconstruction CUSUM series does bring important new information. As we state at Lines 272-276, we have found there was a prolonged drought in SWWA from 1971 CE to 2015 CE that might be ongoing. We calculated the 45-year running change in the rainfall reconstruction CUSUM series to "highlight dry epochs of an equivalent duration to the observed drought to date".Using this analysis, We found the important new information that there are "two comparable prior epochs during the past two millennia, lasting from around 385–429 CE and 732–776 CE (Figure 3c)" with similar duration and intensity.

16 Lines 244-245. I did not understood what is the exact difference between ‘the step changes’ and ‘changes of the gradient’ in the present framework.

Thank you for pointing this out. We apologize for this wording. We have changed at Lines 243-245: "In order to accurately determine the changes in rainfall after 1850 CE, we calculate the CUSUM time series (Equation 3) for rainfall time series (Figure 3a) ~~to identify the step changes~~ and use BREAKFIT analysis ~~on CUSUM~~ to identify any significant changes of the gradient."

17 Lines 251 'subsampled from the 501 CE to 2000 CE series'. Do you mean that you only take a part of the series?

Thank you for pointing this out. We agree that this text is unclear. We have changed at Line 251: "Figure 5 shows the rainfall reconstruction CUSUM time series from 1850 CE to 2000 CE ~~subsampled from the 501 CE to 2000 CE series~~."

18 Caption of Figure 5. What is 'the reconstruction in period 1850–2000 CE from the 501–2000 CE series', here too a part of the series?

Thank you for pointing this out. We agree that this text is unclear. We have changed the caption of Figure 5: "CUSUM time series on rainfall reconstruction in period 1850–2000 CE ~~from the 501–2000 CE series~~."

19 Table 5. What means 'Sample represents to each rainfall simulations'?

Thank you for pointing this out. We agree that this text is unclear.

We have changed the caption of Table 5: "The RMSE and difference in slope calculated between the rainfall reconstruction and simulations in the period 1972–2000 CE. ~~Sample represents to each rainfall simulations.~~ The difference in slope is the difference of the slope of the Ordinary Least Square linear fitted CUSUM time series data for each sample to the slope of the fitted CUSUM time series data for rainfall reconstruction in the period 1972–2000 CE."

20 For me, all the appendices could be moved to a supplementary material.

Thank you for this suggestion. We agree and will move all the appendices to supplementary material.

References

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