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Discussions

Interactive comment on "An ice core derived 1013-year catchment scale annual rainfall reconstruction in subtropical eastern Australia" *by* C. R. Tozer et al.

Anonymous Referee #1

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"An ice core derived 1013-year catchment scale annual rainfall reconstruction in subtropical eastern Australia' by Tozer et al presents a ∼1000 year palaeoproxy record of annual rainfall in eastern Australia. There are few annually resolved palaeo records for mainland Australia in general, so the addition of ∼1000 year record is of great value to climate science in Australia, and indeed, in the Southern Hemisphere. In general the paper reads well. With some additional analysis and discussion, the paper could make a valuable contribution to the literature on Australasian rainfall variability. There are several issues that should be addressed by the authors, however. The two interrelated issues major issues are the lack of presentation of statistical evidence of reconstruction skill and the apparent non-stationarity of the relationship (beyond a sim-

ple positive/negative IPO phase difference). Firstly, the authors explain that they use a regression approach to estimate the relationship between LDSSS record and the spatially averaged AWAP precipitation in the region of interest. Nowhere are any 'quality' statistics of the model presented. The only statistics relevant to the modelling process that are shown are the Pearson correlations coefficients for the relationship between LDSSS and rainfall over the catchments (gauge 6010 and AWAP catchment averaged data). The existence of multiple reconstructions lends itself to the development of interval estimates, for example. I feel that the authors need to address the lack of (statistical) evidence shown for the skill of the reconstruction. The authors should also explain the regression technique used in more detail – e.g. tell the readers why this regression technique was used, its strengths and weaknesses. This is particularly pertinent given the non-linear relationship shown between LDSSS and rainfall (Figure 3), and the non-linear relationship between rainfall and the IPO often commented upon in the literature. Is the lack of variability captured in the reconstruction, relative to the AWAP areal average (Figure 4), related to the use of an inappropriate technique for example? Following on from this, the difference in the variability of the reconstruction vs the instrumental data requires a fuller discussion in the paper, and this is again linked to reconstruction skill. Secondly, the authors' own work demonstrates that the relationship between rainfall and LDSSS is not time-stable (Figure 3, Table 1). If skill is essentially limited to IPO positive phases, might it be feasible to limit model development and testing to IPO positive phases? Can these positive phases be used to demonstrate skill? Could, for example, the reconstruction be calibrated against some positive phases and then verified against other positive phases, using procedures similar to those used by dendrochronologists, or using leave-one-out validation procedures? Could a mdoel developed along these lines then be compared with the model obtained using the Marquardt-Levenberg regression framework as currently presented? Should other methodologies also be investigated? Given the clear non-stationarity of the relationship, I think that the authors need to demonstrate the skill of their reconstruction – even if they focus on the IPO positive phases only. A fuller discussion of this non-stationarity is unavoidable, especially because results in Table 1 suggest that the relationship between LDSSS and annual rainfall in IPO positive phases, for which greater reliability is indicated by the authors, may not be stable over time. Can the authors address point this with the instrumental data? Is the relationship in IPO positive phases 'stable enough' to provide a skilful reconstruction? Why or why not might this be the case? At the very least, these points deserve a much fuller consideration. They are critical, because if the relationship varies 'too much' over time, then the reconstruction is less valuable than it could otherwise be. That the existence of an asymmetric (and hence non-stationary) relationship between the IPO and precipitation is clearly known HAS to be considered at the outset here. The authors suggest that east coast low (ECL) activity may be responsible for a changing relationship between precipitation and LDSSS over time. However, although the 1950s was in a period in which ECL activity was high, and correlations between precipitation and LDSSS (Fig 3f) were low, other factors should not be discounted. In their introduction for example, the authors acknowledge that multiple factors influence precipitation in the region. Is a period of intense ECL activity nearly always going to be the cause of high annual precipitation? What other factors might have an important influence? Although ECLs are an important synoptic feature, the authors should not ignore other possible influences on annual rainfall in the region. What happens to the correlations between precipitation and LDSSS if the very few values that cause the strong negative correlation in the 1950s are removed? Is there a significant difference in the correlation between LDSSS in IPO positive and IPO negative phases if this data is temporarily removed? Also be aware that the use of running correlations may introduce apparent trends that do not actually occur. It would also be helpful if the authors explicitly compared their reconstruction with the little existing information that currently exists for central-southern Queensland through to central coastal NSW – eg. with Lough 2011, McGowan et al. 2009, Ho et al. 2015. Gallant and Gergis 2012 and Gergis 2012 are mentioned. This could form part of supplementary material. p. 12487, l. 28 "...no local ..." Depending on your version of 'local' this isn't correct, e.g. Heinrich et al 2009, possibly Lough 2011 and Heinrich et al . 2008. The existence of

other proxies along the Qld/NSW coastal strip should be recognised. p. 12490, l. 10 A very minor issue. A dating accuracy of +/1 1 yr for the Law Dome core from 894 – 1807 and then accurate to the year beyond that. Later in paper the authors indicate that they are identifying individual years of dry/wet conditions – but prior to 1807 dating accuracy is +/- 1 year. Perhaps another short sentence can be added to clarify. p. 12492, l. 9 A test for low frequency modulation of the signal could also be done –see Gershunov 2001. p. 12493, l. 6 – 20. What about the different seasonal window used? This is likely to be important. This paragraph could be simplified. Although it is true the area Vance et al. 2015 examined was a bit further north, it was not a lot further north.

p. 12493l. 21 – p. 12494 l. 12. While no-one would deny the importance of ECLs to precipitation over the region, it seems that the authors do not wish to acknowledge the existence of other influences on precipitation of the region. This can be simply remedied by adding a few sentences here that acknowledge the relative importance of other systems. Perhaps a bit more detail about the proportion of rain over the area that is due to ECLs in various months (c.f. Pook et al 2006; 2010) p. 12495, l. 1 – the authors state that the reconstruction captures around 10% of the variance in precipitation. . ."Nonetheless. . .there are periods when a stronger relationship. . ..exist." Can this be explored a bit more? How does the relationship differ in the IPO positive vs the IPO negative phases for eg? (See above) p. 12496, l. 20 – 25. Again ECLS are invoked as sole possible cause. Rewrite slightly. Table 2 - why are there longer duration events as the criterion becomes stricter? Figure 6 – We would expect different centuries to differ in terms of the numbers of wet/dry events, but are there large differences? What about the changes in duration of wet/dry events in different centuries? Some further analysis/discussion would be useful here. Same for Figure 7 – the importance of this information is not sufficiently drawn out of the figure.

p. 12497, l. 14-15 -"mid-range'? Mid-range in the context of values chosen, not in absolute terms.

p. 12497, l. 16. . .. Reference to table is confusing. Table suggests 8 years on line 3.

Presumably authors are comparing this value with on further down the table? Needs clarification. p. 12498, l. 5 "Results suggest..." perhaps, but the important features of the study are. . .? Rework the conclusions to highlight the most important findings once additional discussion/analysis of non-stationarity and presentation of some model statistics shown in earlier sections. p. 12498, l. 24 ". . .and anywhere else with similar teleconnections with East Antarctica." This needs to be reworded. As presently written, it seems to indicate all 'answers' to the climate of regions that have apparent teleconnections with the Antarctic will be explained by those teleconnections alone (and hence that LDSSS will be representative of climate in any of those locations). I don't think this is what the authors intend to convey, and it also ignores the important implications of non-stationarity in teleconnections (as shown in authors' own work, as well as elsewhere). Figure 3f Marker for site location is barely visible – especially on insets. Modify colour of marker. Figure 3f - it would be useful to show what a significant correlation is. Figure 4 A mean/median line for the reconstruction would be useful. Figure 5 If the reconstruction demonstrates considerably greater skill in IPO positive phases, these positive phases should shown on Figure 5, e.g., can the IPO-positive phases shown by Vance et al. 2015 be overlaid on this figure? (Some explanation of how/why Vance et al. concluded they were IPO positive or negative phases would need to be included). The inclusion of these phases would help a reader to better gauge (at least in a qualitative sense) when the reconstruction is likely to be more reliable, if indeed it is more reliable in IPO positive phases. References Gershunov, A., Schneider, N., Barnett, T. (2001) Low-frequency modulation of the ENSO-Indian monsoon rainfall relationship: signal or noise? Journal of Climate 14: 2486 - 2492

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