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Climate of the Past Discussions

Interactive comment on "A millennial multi-proxy reconstruction of summer PDSI for Southern South America" *by* É. Boucher et al.

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Dear Dr. Boucher and coauthors,

The second review of your paper by Dr. Villalba was published last week and in my opinion it complements very well the detailed comments posted by Dr. Neukom some time ago. I think both reviewers make very important, major points and corrections that should be addressed in detail before publication of this manuscript in Climate of the Past.

In both cases they recommend the paper should be reconsidered only after major revisions.

Therefore, in order to proceed, I would suggest you incorporate their comments and

suggestions into a revised manuscript and submit it again for consideration. I have also a number of major concerns and a few minor points that would like you to address in addition to the referee comments. Some of my comments discuss issues that were also pointed out by the referees, but I think it is important that the paper clarifies and expands these relevant aspects as they relate to the methodology used and the interpretation of results in this study.

Editor's comments

Page Line Comment

Abstract First you say recent changes were significant but rarely excepcional. Then you say some extremes are unequalled over the last 1000 yrs. Please clarify?

It is not clear how do you quantify the influence of AAO, ENSO and PDO prior to the calibration period without using reliable reconstructions of these features covering the past millennium?

155 5 The claim about Northern Patagonia having a wetter climate seems strange. This region has experienced a marked decrease in precipitation that is also clearly reflected in streamflow records over the past 100 yrs, see e.g. Masiokas et al. 2008.

157- 158 The description of the climate and conditions in the four study areas is quite simplistic, please improve and provide appropriate references.

158 16-17 Please explain better, you selected individual series longer than 250 yrs and then developed 82 tree-ring chronologies?

Fig 1c. Please indicate which line corresponds to which set of data.

159 23-25 The AM method appears promising but only if you have a matrix of series which are largely similar so that if one is missing the remaining series can be used to estimate the missing values. But in a region as large and climatically diverse as SSA, and with the scarcity of climate and proxy data used as input of the AM matrices, I

have trouble understanding how any statistical model can estimate a missing year for a given series (say a PDSI value for the year 1098 in Mendoza, Argentina ca. 69°W, 33°S) when there are no records with a physically plausible relationship with the PDSI in Mendoza in that particular year.

I am not saying this method has no merit, I am merely pointing out that unless the available proxy-instrumental data has a strong covariability, for me it seems quite difficult that any method can estimate missing values where there is little or no data to realistically make the estimations. Of course, if forced, the method will provide an estimation from any data available, but I wonder how reliable this estimation might be if it was estimated from very poorly correlated, distant proxy records?

In addition, I think that in order to use a set of proxies for reconstructing a given variable, one first has to demonstrate a time-stable, strong relationship between the predictand and predictor series. Otherwise, one could be using series which are only weakly or randomly related, i.e. not really recording or reflecting the same climatological feature (PDSI here), and therefore the reconstructed exercise is questionable.

Please explain better why 1993 was selected as the end of the calibration period? According to Table 1, only 2 HF proxies reach the year 1993: La Esperanza-tree rings; and Laguna Aculeo-lake sediments (please correct the end date for the Lago Quillén tree-ring chronology as it was presented by Villalba in 1997 and therefore cannot end in 2003 as shown in the table). 1991 seems like a much better choice as there are many more proxy series that have data up to that year. For the LF calibration the number of proxies that extend at least to 1993 is only 4 (Table 1).

161 15-27 The validation of the reconstructions seems rather weak. You only mention the RE statistic plus R and R2. These latter two measures are pretty basic calibration statistics, and do not measure the skill of the models outside the calibration dataset. I strongly suggest to perform additional statistical calibration/validation tests together with an analysis of the residuals (Durbin Watson test, autocorrelation and/or trend in

residuals, etc).

Validation using the Mar Chiquita data may be appropriate for the grids located nearby the site (and maybe the Pampas region) but it can hardly be used as a validation of PDSI reconstructions in other sectors of SSA. So far these sectors have only been validated by the RE statistics discussed above.

160 6-7 You indicate that you used the first 12 PCs that explain 77% of PDSI variance over SSA. But then you reconstruct only 4 series from the 4 regions shown in Fig. 1. How do the 12 selected PCs relate to the 4 regions being reconstructed here? What is the weight of each PC in each region? Would it make more sense to reconstruct these 12 PC series since these are the records that reflect more objectively the main PDSI patterns over SSA?

Figs 3-4. In these figures you present a SSA series and make a comparison between observed and predicted values. Although it shows a fairly good agreement, what is the basis for averaging all PDSI grids in such a diverse area as SSA? As discussed in the Introduction and elsewhere, the climate regimes in this subcontinent are certainly quite varied and, for any given year, it is quite difficult to know what these regionally averaged observed-predicted values really represent. I would suggest not merging the subregional records into one large scale average which has such complicated interpretation.

162 15 As the reviewer Ricardo Villalba indicates, climate data in SSA are quite scarce and of questionable quality for such a long period as 1930-93. Therefore I would strongly suggest considering this important factor when discussing model results.

164 20 How does the AM method deals with the decrease in the number of proxies available as one moves back in time? Obviously not all proxies available cover the whole 1000 years, and I wonder how can all reconstructions cover the whole millennium if only a handful of proxy records go as far back as the 11th and 12th centuries. If only very few, distant proxies are available in these centuries, what is the reliability of the

reconstructed values during the first centuries presented for the 4 study areas plus SSA as a whole in Fig 8?

A similar comment applies to the most recent period (1994-2005). If there are only very few up-to-date proxy series, how reliable can the estimated values be for these years? You indicate correctly in the text that not all proxies extend beyond 1993. But in the text several analyses are indicated as using the year 2005 as the end date. As shown in my comment above and in Table 1, the end date for the calibration period could well be 1991 instead of 1993 as the number of proxies decreases dramatically after 1991 not 1993. And for the first years of the 21st century there are only 2 proxies (2 LF lake sediment proxies, Lago Frías and Potrok Aike; and no HF proxies). So what is the reliability of the estimated PDSI values for the 4 regions in SSA for 2000-2005 with so few proxies?

From what I can see this appears to be a main weakness of the method, in that it does not recognize which proxies are really reliable predictors but estimates PDSI values for all the reconstruction period (to the earliest and to the most recent period) regardless of the existence of appropriate proxies in these periods. This should be addressed if possible or clearly stated in the text so that the reader can better assess and interpret the results from this reconstruction method.

One possible way to tackle this key issue would be to introduce a previous step where a subset of proxies which are good predictors for each region′s PDSI patterns (e.g. that show strong, time-stable correlations) are selected prior to entering the proxies into the AM routine. I think that using only the proxies selected specifically for each region/grid will improve the reliability of the PDSI estimations and by using a nested approach it may even be possible to extend the reconstructions as far back as the selected proxies are available (clearly not the whole millennium for all regions).

I think that these questions and issues require a careful and detailed assessment prior to entering into the discussion of the main temporal patterns and relationships with

large scale features such as AAO, ENSO and PDO.

Fig. 8. Please make the red lines thinner as they do not allow seeing the estimated values for most of the 20th century.

166 13-20 The number of LF proxies does not vary greatly over time and therefore the reconstructions using all or only the ones that extend beyond AD 1250 will likely give similar results. The number of HF proxies is markedly smaller prior to AD 1250 and I wonder which ones contributed (and to what degree) to each of the 12 PDSI PCs from the 4 regions? In my opinion, more important than the number of proxies available, is which proxies are available for reconstructing which region prior to 1250? Is there a strong, physically plausible relationship between these series that warrants their use in these different 1000-yr long reconstructions (and the interpretation of results made from these reconstructions)?

I have one question regarding Figs. 6 and 13. The similarity between the observed and reconstructed PDSI values (Fig. 6), and that between reconstructed PDSI and the AAO, ENSO and PDO (Fig. 13) suggests a good match between observed and reconstructed PDSI values, and between these and the three large scale features (AAO, ENSO, PDO). However, it is not clear to me how these correlations were performed, as the authors indicate that the reconstructions were based only on 12 PDSI PCs, not on all grid cells in SSA (the maps shown in these figures suggest the existence of a series of gridded reconstructions).

In addition, in the text it says that only 12 reconstructions were performed and then somehow (it is not clear how) they were merged to form the reconstructions for the 4 SSA regions. Please clarify this point.

Finally, the analyses of the influence of AAO, ENSO and PDO are mostly based on the results from the correlations of these features with the Dai et al. (2004) gridded, instrument based PDSI series. It is clear that these large-scale ocean-atmospheric features have a marked influence on SSA 20th century′s climate (e.g. Garreaud et al.

2009). However, extending these relationships to earlier centuries seems risky as it is very difficult to determine if the influence of these large scale features has remained unchanged over the past millennium. This would require, in my opinion, the integration of reliable multi-century reconstructions of AAO, ENSO and PDO for comparison with reliable PDSI reconstructions in SSA. This has not been included in the manuscript and therefore the interpretation of the inter-relationships in earlier centuries needs, at least, a clear cautionary comment about the limitations of this approach.

Interactive comment on Clim. Past Discuss., 7, 153, 2011.