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Climate of the Past Discussions

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Interactive Comment

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

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Manuscript Review, E. Boucher

Dr Masiokas, editor of CP;

I am delighted to send you this revised version of our manuscript entitled: “A millennial multi-proxy reconstruction of summer PDSI for Southern South America”. The present version is greatly improved by comparison to the previous one. Each and every comments made by the reviewers were considered in detail and in most cases, substantial revisions were made. The detail of each and every modification made to the original manuscript are listed below and discussed in the text that follows. Among the most important changes, you should notice that:

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1) Many figures have been modified / redrawn / created for more clarity (figs. 1, 3, 6, 7, 8, 9, 10, 13). 2) The text is now much shorter (23 MS Word pages by comparison to 36 for the previous version) 3) Some figures (e.g. wavelet analysis) were removed because they were redundant and not really useful to understand the main message in the article. 4) Nested reconstructions were performed in each regions in order to testify that our reconstructions can be interpreted back to A.D. 1000. 5) The frequency analysis was completely recalculated with more robust and conventional methods that will be better understood within the scientific community. 6) Many changes were brought to the text, including the removal of most passages concerning the "exceptional character" of extremely dry and wet spells, as we recognize that the method does not allow to extrapolate over the observed variability. I hope that the manuscript will now meet the standards of your journal. If anything should be done to improve this new version, please don't hesitate to contact me. I'll be happy to make the necessary changes.

Cordially.

Étienne Boucher

MAJOR COMMENTS

——Dr. Neukom's Comments——

1. Temporal evolution of reconstruction skill.

Dr. Neukom asked to provide a temporal evolution of the reconstruction skill. The reviewer rightfully argues that our RE values are only valid for the period where all proxies are available. We therefore provide RE values for nested reconstruction that go back until the beginning of the reconstruction period. The following lines were added along with a complementary figure that presents the evolution of RE values in each area of SSA:

//EB//L. 261// Before exploring millennial summer PDSI fluctuations, is important to establish that our reconstruction is reliable over the full period. To do so, we provide

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a series of nested reconstructions and their corresponding verification (RE) statistics. Nested reconstructions were computed exclusively from the subset of proxies that are older than AD. 1000, 1025, 1200, 1222, 1388, 1505, 1649 respectively corresponding to the calendar dates to which a new LF proxy is added to the reconstruction. We present the evolution of RE statistics in each sub-region on figure 8. Even with a limited number of proxies at the beginning of the last millennium, the RE statistics remains positive in all regions, meaning that the spectral analogues have good predictive skills over the last thousand years. As an example, we provide a graphical comparison between the full reconstruction and the AD 1000 nested reconstruction (Fig. 9) for two regions: PG and PM. Our analysis shows that LFs are comparable between reconstructions over the last thousand years. However, HFs are less similar and this probably relates to the fact that higher frequencies contain a lot of the local climatic signal (i.e. noise) that cannot be adequately reconstructed from a very limited number of HF proxies. Nevertheless, the correlations for the full spectra remain acceptable, suggesting that the long-term trends can be interpreted since AD 1000.\\

2. Comparison with other datasets

In addition to the comparison with Mar Chiquita, we added a comparison with Neukom's temperature (Neukom 2010a) and precipitations (Neukom 2010b) reconstructions. This addition interestingly revealed that our summer PDSI reconstructions are closely linked to temperatures at least in PG and PM. We added the following text and an accompanying figure:

//EB//L.192//Finally, despite the fact that a number of proxies are shared between the studies, we compared our PDSI reconstructions with the precipitation and temperature reconstructions of Neukom et.al. (2010a, 2010b). This was done to analyze which climatic parameter (P or T) relates most, over time and space, to summer PDSI in SSA.\\

//EB//L.322// In order to better interpret that antiphase, we compared our reconstruc-

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tions to those of Neukom et al. (2010a and 2010b). It is very interesting to notice that, at the scale of the last centuries, the response of summer PDSI to precipitation and temperature varied throughout the study area. We provide a graphical comparison of two contrasted regions: PG and PM (Fig. 7). In PG, summer PDSI seemed to be more responsive to temperatures than to precipitation. In this area, warm summers and winters were associated with a wet climate (positive relationship). By contrast, in PM, PDSI seemed closely linked to DJF precipitation. Moreover, DJF and JJA temperatures related differently to summer PDSI. Warm summers were associated with a dry climate while warm winters were generally coupled to wet summer conditions. These contrasting dynamics underline the fact that PDSI might not have responded similarly to precipitation or temperature in every region but instead that variations in the drought index were probably driven by different parameters whose importance are likely to vary through space.\\

3. Statements about the unprecedented nature of climate //EB//These statements were generally removed because we agree with the reviewer: our analog method, by definition, provides a conservative reconstruction and yet, past extremes are just "at least as important in magnitude as those observed during the modern (analog) period". Past extremes years might in fact have exceeded the PDSI value of the more recent ones and we would not be able to reconstruct a higher value. However, we must underline here that, although this might appear as a flaw of the SAM method, all reconstructions method are imperfect in that regard. No matter which statistical technique is used (e.g. linear and non-linear regression), extrapolation over the observed variability remains a hard task and no method can guarantee a success.

4. Proxy data

//EB//Dr. Neukom suggested that we include other proxies in our reconstruction. Among those identified by Neukom, we added the following:

-106 KL Enso-sensitive sediments (Rein, 2007)

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-Puyehue Lake sediments (Boes and Fagel, 2008)

-Several ice cores from West Antarctica / Antarctic Peninsula

-Dyer Plateau (Thompson et al. 1994)

-Berkner Island (Mulvaney et al 2002)

-DML (Graf et al, 2002)

//EB//In addition to those identified by Dr. Neukom, we also added

-Moy et al. (2008)'s Lago Guanaco lake sediments

-Christie et al. (2009) Andean PDSI reconstruction (from tree rings)

//EB//We could not add the following proxies (reasons are explained in [])

- Newkom's (2009) documentary proxys [Although those are high quality data, the proxy records are semi-quantitative and our spectral decomposition method should preferably be used on quantitative data].

- Ice core from Illimani [After several email exchange with G. Hoffmann, owner of the data, we concluded that the chronology was too imprecisely dated (+- 20 years) and too short to be used, even for its low frequency component].

- Other ice cores from Antarctica: [could not find them on the web, or could not establish a contact with the owner of the data].

——Dr Villalba's comments——

Dr Villalba's concerns were not with the method itself, but with the quantity / quality of both proxy and meteorological records used to reconstruct the climate.

//EB//Regarding the meteorological records. We agree with Dr. Villalba's comment. It is true that the source meteorological records are probably scarce and inhomogeneous. Hopefully, more research will be conducted and will allow for a better homogenization of the meteorological records. Of course, it is not the scope of this study to check these issues and, considering the fact that we used a generally ”approved” gridded PDSI dataset (Dai et al (2004)), we can't do much to correct this situation. We are forced to assume that the work of Dai et al. and the Climate research unit (that provided the source P and T data) have done a good job at checking inhomogeneities and removing problematic source meteorological records. However, as mentioned in the paper, we tried to reduce that effect by choosing only the post-1930 PDSI data even though the data was available since 1870 (very few stations were actually recording at that time).

//EB//Concerning the proxies. It is also true that the proxy matrix do not fill the entire SSA area. Some areas like ST have no proxy at all, and the reconstruction there is performed only from teleconnections. Fortunately, teleconnections are quite strong in that area of the world (AAO and ENSO together explain about 40 % of the variability in most part of SSA). Thus, it can be argued that using remote proxies to predict climate in proxy-lacking areas makes more sense in SSA than it does elsewhere, e.g. in areas where ocean-atmosphere forcings play only a small role overall. Moreover, the strength of the spectral analogue method should also be recalled. Within the SAM, low-frequency (high-frequency) proxies are used to reconstruct low-frequency (high-frequency) components of the PDSI. While it is clear that high frequency PDSI variations are noisier, low frequencies are much easier to predict and are much more similar between contiguous areas (e.g. each 2.5 x 2.5 pixel have a lot in common when it comes to the low-frequency component). This is why we can actually have a reasonably good success in predicting low-frequency components of the PDSI, even with a small number of proxies. For example, fig. 9 shows a comparison between the 1000-yr nested reconstructions (computed only form proxies that cover the full millennium) for PG and PM. We clearly see that the low frequency component is adequately reconstructed even from a small number of low-frequency predictors. However, this is not the case for higher frequencies. Because they are noisier and much more variable through space, they are much harder to predict from a small number of high frequency proxies. So, in the light of these results, we produced a little text to reinforce Villalba et

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al. 2009's argument.

//EB//L. 451// Our analysis finally shows that past low-frequency PDSI variations can be reconstructed quite successfully in most regions of SSA. We have shown that even with a reduced number of proxies, low frequencies reconstructed using the SAM remain quite similar to the low frequencies reconstructed from the full dataset. However, we have also shown that noisier high frequency PDSI variations are less well reconstructed from a reduced set of proxies. In order to better reconstruct high frequency variations of PDSI in SSA, more highly resolved proxies are needed as stated by Villalba et al. (2009), especially in proxy-lacking areas such ST area and eastern PM.\\

—–Dr. Masiokas (editor) comments:—–

The most important issue highlighted by the editor was that the reconstruction did not estimate the reliability of the predictors through time while estimating PDSI values for all the reconstruction period. This comment is similar to the first comment of reviewer #1 (Dr. Neukom), and we recognize that a solution to that problem is imperative.

//EB//The solution is to provide nested reconstruction along with their corresponding validation statistics on the calibration period (1930-1993), as suggested by Dr. Neukom. That procedure enables to verify that the proxies have predictive potential over the entire reconstruction period, even at the very beginning of the millennium, where fewer proxies exit.

//EB//Whether or not we should have provided a detail examination of the predictive potential of each and every proxy used here should however be discussed. The fundamental question to ask is: what is a reliable predictor? 1) a predictor that explains a small amount of PDSI variability in each pixel throughout the studied area? or 2) a predictor that explains a lot of the local variability but that is of no use to explain regional or continental PDSI variations?. The answer is simple; both types are needed. So, in our opinion, it would become very subjective to reject an individual proxy based on a statistical criteria such as its R, R2, RE etc. It's better to include them all and

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to examine the evolution of validation values through time. And this is what we have done.

This specific comment from Dr. Masiokas on the analogue method (AM) is relevant and merits a little discussion :

[From Dr. Masiokas' review] Âń 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. 69W, 33S) when there are no records with a physically plausible relationship with the PDSI in Mendoza in that particular year Âż.

//EB//Dr. Masiokas is right. In a sense, all statistical models, when forced, can yield predictions for past years and at each location, even when the proxies are remotely located and totally noisy. But to determine whether or not these predictions are reasonable is not a simple task. In the case of the AM method, as underlined by Dr. Masioskas, the reconstruction makes sense only if the links between the proxies and the predictands have a physical (climatological) meaning. This is particularly true for the AM method. This physical meaning can be measured by the computation of nested reconstructions. Without a strong climatological linkage between the predictors and the predictands, the RE values would become negative. This is not the case in our work. The computation of nested reconstructions has shown that the reference period can be well reproduced even when we use a subset of proxies that covers the full millennium.

//EB//Nevertheless, that does not fully guaranty that the predictions in e.g. 1098 in Mendoza, Argentina are accurate. No method can actually provide that guaranty. Some advantages of the SAM must however be recalled here. First, the low and high frequencies of the PDSI are reconstructed independently from one another and then

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summed up at the end of the reconstruction process. Thus, as we argued on a comment by Dr. Villalba (see preceding discussion), low (PDSI) frequencies are easier to reproduce from remotely located LF proxies because they are, by definition, less noisy and probably more influenced by large-scale climatic features then higher frequencies that are spatially more variable and noisy. Consequently, to follow-up on the example chosen by Dr. Masiokas (yr 1098 in Mendoza), we could argue that it is highly probable that year 1098 is placed in the right low-frequency context because of the SAM method chosen here, but that its high frequency component is much more uncertain because too few proxies exist locally to allow for an adequate estimation of the HF spectrum. Nevertheless, we must underline that, since the SAM method is based on similarity (i.e. not regression-based), the AM cannot reproduce patterns or variance that do not exist in the reference period. Yet, the error would not be due to extrapolation, but to a misestimation (the HFs of proxies are unrelated to the HFs of PDSI producing spurious PDSI values) or an underestimation of the amplitude (the HF components are strongly linked, but no analogues exist in the reference period to correctly model the extreme PDSI value of year 1098. In both cases, low frequency variations are correctly estimated.

MINOR COMMENTS

——Dr. Neukom's minor comments——

OTHER POINTS 1. The first sentence in the abstract may be revised, as Christie et al. (2009) already performed a PDSI reconstruction over most of the last millennium. The greatest novelty in the Boucher et al. manuscript is that their reconstruction is spatially explicit, and covers all SSA. This should be included in this sentence for clarity.

//EB//Done//

2. Abstract, line 15. The authors state that "The AAO was an important climatic driver during the calibration period (1930–1993) in SSA". However, the AAO time series they use starts only in 1949. The authors should clarify or correct this.

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//EB//Done//

3. P 157, line 13: It should be clarified what reference period the PDSI grid is based on. What period do the "local average conditions", i.e. a value of 0 correspond to? This is very important because the y-axes of most figures are based on this scaling.

//EB//Done//

4. P 157, line 21: An explanation of why the summer season has been chosen would be helpful here. Did the authors do any sensitivity tests with other seasons? Or did they just expect DJF to yield the best results?

//EB//Done//

5. There are several things that should be explained in more clarity in the proxy data and methods section: - The tree ring series used have all different starting and ending years. So how do the authors define the period for the calculation of the PC's? Was this done after infilling the missing data?

//EB//Done//

- Is the East Antarctica ice core record a composite or are all cores used individually? Please clarify. In the table it is only listed as a single proxy, but on the map and on figure 1c they are all plotted individually.

//EB//Done//

- Please make clear what time period is covered by the reconstruction. 1000-1993 (which would need some adaptations and clarification in section 3.3 where the climate indices are used until 2005, see below)?

//EB//Done//

6. P. 162, line 13. I think the smaller proxy density cannot be used as an argument for the weak results over the "southern Part of the Andes", because the region they refer

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to with the lowest R2 (Figure 5, left and middle) corresponds to the area where most of the tree ring records come from (Figure 1b).

//EB//Done//

7. P. 166, line 10: How can the findings of this study "support the idea that the MWP was probably a global phenomena"? This statement should either be removed or clearly explained how a SOUTH AMERICAN DROUGHT study can say something about GLOBAL TEMPERATURE variations. The correct word would be "phenomenon" not "phenomena" (which is plural).

//EB//Done// sentence removed

8. P. 166, lines13ff: Again, a more detailed explanation of how the analyses were performed would be appropriate. Which data series were chosen for the alternative reconstruction based on the long proxies only: the long series before or after infilling the proxy matrix? (I argue that taking only the long series and performing the filling based on these only would be the correct way). Were again PCs taken for the now much smaller tree ring network (If yes, how many PCs?), or the tree ring chronologies themselves? Again, the correlation analysis here is interesting, but cannot be used to state that the reconstruction was skillful in the early period. Verification measures for the alternative reconstruction are required.

//EB//Clarifications brought here//

9. P. 167 line19: I do not understand why the authors choose only 50 (randomly selected) years to define the 1 and 99 percentiles of the preceding 100 years for each year. please explain the reason for not taking all 100 years to identify the percentiles.

//EB//Bootstrap to calculate confidence intervals around return period values. Corrections done, see L. 333 and further down.

10. P 168 line 19ff: Please indicate which datasets were used to derive the ENSO, PDO and AAO indices and cite them properly.

//EB//Done//

11. P. 168 line 21: The authors claim that the common period between AAO and PDSI is 1949-2005. Was the PDSI reconstruction extended to the post-1993 period? The authors should clearly indicate in the methods section which time period was covered by the reconstruction. Or was this analysis performed on the instrumental data? Please be clear.

//EB//Done// corrected to 1950-1993

12. P 171 line 5: Is it possible to say that low frequency periodicities are "probably AAO-related"? The observational time period of the AAO covers only around 50 years and PDO is also active at lower frequency domains.

//EB//Done// this hypothesis was relaxed

13. P 171 lines 13f: Again please make clear that this is only the first gridded PDSI reconstruction in the SH.

//EB//Done//

14. P 171 line 21: Please be more specific here. What is an "important" PDSI variation?

//EB//Not sure what is meant here//

TECHNICAL COMMENTS 1. The correct term for a 1000 year period is "millennium" not "millennia" (which is plural). This should be replaced in many sentences in the manuscript.

//EB//Done//

2. P. 156, line 22: Maybe change the wording or at least include "also" between "PDSI" and "needs". Christie et al. (2009) IS a very valuable study although it's "only" focusing on the regional scale and it IS able "to place the recent changes in water availability into a broader perspective".

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//EB//Done//, 2nd comment : not necessary

3. I suggest you cite the original data paper for each ice core record instead of (Vimeux et al., 2009), which is a review paper.

//EB//Done//

4. P. 163, line 9: The 95% percentiles of RE as shown in Table 2 for all SSA are 0.18 and 0.51, which disagrees with 0 and 0.52 in the text. Please correct.

//EB//Done//

5. P. 163 line 12: The correct number to use here for comparison with Neukom al. (2010a) is 0.73 (to be found in line 12 of section 3.3), which corresponds to their average RE value for the full predictor set, as done by Boucher et al.

//EB//Done//

6. P. 166, line 4: The end of the MWP period as reconstructed by Neukom et al. (2010a) is around 1350 not 1400 (which was found to be already in the coldest phase of the millennium).

//Done//

7. P 169 line 20. The authors forgot to mention the very strong ENSO correlations in the area Uruguay, SE-Brazil, which is at least as strong as in the eastern coast of Argentina.

//EB//Done//

8. P. 170 line 10: I suggest omitting the word "western" as PDO and ENSO are dominating in all indicated points in PM. However in ST they are not; so better say "parts of ST".

//EB//Done// this part of text was removed

9. P. 172 line 20. Replace "act" by "acted".

//EB//Done//

10. Table 2: Maybe make clear in the caption that R and R2 are calibration statistics and only RE is a validation statistic.

//EB//This is already said in the text//

11. Figure 1: In Figure 1b there is a tree ring series at around 60âŮęW and 43âŮęS. I doubt that a tree ring record of more than 250 years exists in this area, and there is no such record listed in Table 1. Please correct this error; this is important because it must be clear to the reader that there are no proxies in the areas east of the Andes.

//EB//Done// This series was accidentally dragged there, sorry

12. Figure 3: It would be helpful to indicate what kind of filter was applied for the smoothed series. It appears that the bold lines are not always in the middle of the uncertainty bands, e.g. at the very beginning in the second panel (PA) or between 1950 and 1960 in the fourth panel (ANDES). Is this an error or is the method able to calculate uncertainties, which are of different magnitudes in the two (positive and negative) directions?

//EB//Done//

13. Figure 4: Maybe include “over the 1930-1993 calibration period” in the first sentence to be clear.

//EB//Done//

14. Figure 5: Make clear that these values belong to the full predictor set, i.e. the period 1736-1993. The maps shown suggest a higher resolution that the 2.5âŮęx2.5âŮę of the PDSI grid. Please clarify that the data were interpolated and name the kind of interpolation method used.

//EB//Done//

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15. Figure 7: Please remove the “à” in the caption of the left y-axis.

//EB//Done//

16. Figure 8: I suggest removing the bold red lines (observations), as they mask the reconstruction in the 20th century. The comparison between reconstruction and observations were already shown in Figure 3. Maybe indicate the frequency of the loess smoother that was applied.

//EB//Done// Figure was changed

17. Figure 10: Consider replacing “The lower part of each graphs” by “The bottom panel” for clarity.

//EB//Done//

18. Figure 11: The black lines in the middle and right columns are hardly visible. Consider showing only the period of interest (i.e. the 1000-1250), so that the differences between the two reconstructions are visible.

//EB//Figure changed and enlarged

19. Figure 13: Please indicate for which time period the correlation analyses where performed. I strongly suggest removing the word “extreme” from the caption and the plot, because all positive and negative values are considered, not only the extreme ones.

//EB//Done// Maps of extreme years were removed, because they were redundant with the top 3 maps of figure 13

20. Figure 14: Please explain why some of the grid cells are omitted, i.e. none of the three indices was considered as dominant.

//EB//Done// Those are values with negative REs

21. Figure 15: Clarify in the caption, that the plots belong to the PDSI reconstruction.

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//EB//Figure removed//

22. Figure A1: Please reverse the time axis in order to be consistent with all other plots in the manuscript.

//EB//Done//

——Dr. Masiokas's minor comments——

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?

//EB//The statement was removed

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?

//EB//This passage was clarified. We simply state that if modern patterns of response are transferable to the past (and they seem to be, because of the antiphase), then AAO could be an important forcing on PDSI over the last 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.

//EB//Sentence corrected\\

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

//EB//It is not the scope of the present paper to describe the climatology of each and every region in SSA, nor has any multi-proxy study ever described that in detail (see e.g Neukom 2010a and b); -especially if the text is to be reduced in length. Some studies already describe SSA's climatology pretty accurately. We decided that it wasn't

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necessary to add additional details in the present article.

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

//EB//Done//

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

//EB//Done//

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).

//EB//The chronology was considered until 1993 because 1991 and especially 1992 are El Nino years. Moreover, there are no particular problems associated with the modeling of these years (see figure 4). We decided to keep them.

P. 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).

//EB//Durbin Watson statistics were added

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

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PDSI reconstructions in other sectors of SSA. So far these sectors have only been validated by the RE statistics discussed above.

//EB//Comparisons with Neukom temperature and precipitation were done. Duncan Christie's PDSI reconstruction in the ANDES is focused on high frequency variations (Autoregressive modelling of tree rings. . .). Instead of using it in a comparison analysis, we decided to include the series as a tree ring predictor (see table 1).

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?

//EB//We reconstruct the 12 PCs, but we redistribute these PC series over the full 101 pixels to estimate. This is done using and inverse PC (PC-1) procedure. The 4 regions are just a summary (mean & 95% conf. Intervals) of the PDSI variability in each region.

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.

//EB//The answer is simple: we wanted to compare directly with the other multiproxy reconstructions (e.g. Neukom et al. 2010a) that also used this subdivision.

How does the AM method deals with the decrease in the number of proxies available

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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.

//EB//See the section on nested reconstructions. (Neukom's comment #1)

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?

//EB//The 1994-2005 recent periods are not predicted. When 1994-2005 PDSI series are used, Dai et al. (2004) original values were used to complete the series. That was mentioned in the text

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

//EB//Done//Figure was redrawn

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.

//EB//The correlations were made on each pixel individually, once they were reconstructed and after the PC-1 procedure. Then the correlation coefficients were krigged though space using a thin plate regression.

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 centuryâĂšs 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 interrelationships in earlier centuries needs, at least, a clear cautionary comment about the limitations of this approach.

//EB//True, but a reconstruction of AAO, ENSO, PDO would suffer from the same problems; we would be forced to assume that the relationship between the proxies and the AAO has not varied through time. A better way to reach that goal would be to conduct model-data comparison studies. Many sentences were added throughout the text to highlight this issues and the importance to conduct such an analysis.

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