

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

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This paper is a most welcome contribution to the field of paleoclimatology in South America over the last centuries. It provides a spatially explicit reconstruction of summer PDSI over the last millennium using multiple proxy records from different archives and a novel reconstruction approach. It provides new insights into the large-scale drought/moisture history of southern South America (SSA). Also, it relates observed and reconstructed PDSI variability to large scale driving factors of southern hemisphere climate variability. I think this paper fits well into the scope of the journal, but I have a few major concerns that should be addressed before the manuscript may be published. Also, there are a relatively high number of small errors and unclear points that must be corrected and clarified. I can not comment on the reconstruction method ap-

C1

plied, as I do not have experience in using that method, but the results suggest that it performs similar than other state of the art climate field reconstruction techniques.

MAJOR POINTS

1. Temporal evolution of reconstruction skill.

Although missing values in the proxy matrix are filled in prior the reconstructions, this infilling is already a reconstruction and the true proxy availability considerably decreases towards the past (Figure 1). The calibration and verification statistics provided are therefore only representative for the period where the full proxy record exists (1736-1993). The authors do provide a comparison between the reconstruction based on all and the long records only, but no verification of the performance of this smaller subset. I think it is absolutely necessary to provide such an evaluation if the authors want to claim that their reconstruction is reliable over the entire millennium and that “the long-term trends can be interpreted over the full reconstruction period” (p. 166, line 23). Ideally this would be done by providing the verification statistics (in this case R, R2 and RE) for each nest (i.e. proxy combination that occurs over the reconstruction period), but must be provided at least for the nest at the very beginning of the reconstruction period and for the spatial means.

2. Comparison with other datasets.

The comparison of the PDSI reconstruction with the Mar Chiquita lake level reconstruction is very interesting (Figure 7). However I somehow miss a comparison (of not only the average REs but also the reconstructions) with the existing regional PDSI reconstruction of Christie et al. (2009). Although that reconstruction focuses on high frequencies, I think some sort of comparison would be possible (e.g. looking at extreme years) and very helpful to assess the performance of the reconstruction in the ANDES. As the precipitation reconstruction of Neukom et al. (2010b) covers exactly the same area and precipitation is probably in many regions very strongly related to PDSI, some sort of spatial comparison with this dataset would be interesting (data are available at

C2

<http://www.ncdc.noaa.gov/paleo/pubs/neukom2010b/neukom2010b.html>). However, I may be personally biased when requesting this, so I leave it to the editor to judge on this point. However, as the authors already do a comparison with Mar Chiquita Lake levels and exactly the same comparison was also done in Neukom et al. (Neukom et al., 2010b), I think including the data from that study into Figure 7 would be appropriate, of interest to most readers and only a small effort (I can provide the data, already extracted to the catchment area).

3. Statements about the unprecedented nature of recent climate.

The reconstruction methodology applied “does not have the capacity to extrapolate over the observed variability” (p. 13, line 9). Therefore, I have some doubts that the method allows to draw conclusions about how exceptional recent decades are in the context of the last millennium. There might have been extreme years in earlier periods that exceeded the amplitude of the calibration period. This would also affect the calculation of 100-year droughts etc. I suggest that the authors remove statements about the unprecedented nature of 20th century climate (especially in the abstract) unless they either provide some quantitative estimates of whether the methodology allows such statements (e.g. by monte carlo simulations) or very clearly mention the methodological caveats every time such a statement is made.

4. Proxy data.

The authors use many, but not all of the available proxy records from SSA and surrounding areas that cover the last millennium. I wonder whether this selection is more or less arbitrary (i.e. the authors did not know about / find / request some records) or did the authors exclude some records for particular reasons (e.g. the records do not enhance the skill of the reconstruction or do not meet some quality or resolution criteria.). I think this should be clarified. In terms of tree rings, I acknowledge that the authors use the records that are available on the ITRDB and obtaining the (quite numerous) published updates and new series which are not publicly available may require

C3

an unproportionally large effort at the current state of the manuscript. But apart from tree rings there are a few other records that belong to the same archives and regions as some of the proxies used, so they may be considered (some examples listed from north to south):

- 106 KL ENSO sensitive marine sediment record from Peru (Rein, 2007), available at: <http://www.klimaundsedimente.geowiss.uni-mainz.de/128.php>

- Ice core from Illimani: Deuterium (Ramirez et al., 2003) and NH₃ (Kellerhals et al., 2010)

- Various drought related documentary records (Neukom et al., 2009), available at: <http://www.ncdc.noaa.gov/paleo/pubs/neukom2009/neukom2009.html>

- Puyehue Lake sediments (Boës and Fagel, 2008).

- Ice cores from West Antarctica and the Antarctic Peninsula, which are probably more directly related to SSA climate than the cores from East Antarctica (is there a reason why the authors choose only ice cores from East Antarctica?). For example James Ross Island (Aristarain et al., 2004), Dolleman (Russell et al., 2006), Dyer Plateau (Thompson et al., 1994), Gomez (Thomas et al., 2008), Berkner Island (Mulvaney et al., 2002; available at PANGAEA), DML (Graf et al., 2002; available at PANGAEA) and some of the ITASE cores (Schneider et al., 2006; the last 200 years of two cores are available at <ftp://ftp.ncdc.noaa.gov/pub/data/paleo/icecore/antarctica/antarctica-temp2006.txt>).

It would be interesting to see, how the chosen predictor set influences the results. This has been done to a limited extent in Figure 11, but could be extended in order to provide an estimate on which predictors are important/dominating and how robust the results are to changes in the predictor set. Especially as a large fraction of the PDSI grid does not contain any proxy records (e.g. the full PM region and most of the ST region) it would, for instance, be helpful to see, which records provide the signal

C4

for these regions. Also, the fact that there are much more high- than low-frequency proxies may influence the results and may be addressed in the paper.

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.

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.

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.

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?

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?

- 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.

- I think figure 1c should be adapted so that only the number of predictor series, which

C5

were finally used for the reconstructions are shown. I.e. only the seven PC's from the tree rings (instead of all single records) and the true number of time series from Antarctica.

- 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)?

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 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).

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).

8. P. 166, lines 13ff: 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.

9. P. 167 line 19: 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.

C6

10. P 168 line 19ff: Please indicate which datasets were used to derive the ENSO, PDO and AAO indices and cite them properly.
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.
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.
13. P 171 lines 13f: Again please make clear that this is only the first gridded PDSI reconstruction in the SH.
14. P 171 line 21: Please be more specific here. What is an “important” PDSI variation?

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.
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”.
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.
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.
5. P. 163 line 12: The correct number to use here for comparison with Neukom et

C7

- 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.
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).
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.
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”.
9. P. 172 line 20. Replace “act” by “acted”.
10. Table 2: Maybe make clear in the caption that R and R2 are calibration statistics and only RE is a validation statistic.
11. Figure 1: In Figure 1b there is a tree ring series at around 60°W and 43°S. 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.
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?
13. Figure 4: Maybe include “over the 1930-1993 calibration period” in the first sentence to be clear.

C8

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 than the $2.5^{\circ} \times 2.5^{\circ}$ of the PDSI grid. Please clarify that the data were interpolated and name the kind of interpolation method used.
15. Figure 7: Please remove the “à” in the caption of the left y-axis.
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.
17. Figure 10: Consider replacing “The lower part of each graphs” by “The bottom panel” for clarity.
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.
19. Figure 13: Please indicate for which time period the correlation analyses were 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.
20. Figure 14: Please explain why some of the grid cells are omitted, i.e. none of the three indices was considered as dominant.
21. Figure 15: Clarify in the caption, that the plots belong to the PDSI reconstruction.
22. Figure A1: Please reverse the time axis in order to be consistent with all other plots in the manuscript.

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C9

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C10

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C11

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