Clim. Past Discuss., 9, C391–C399, 2013
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Climate of the Past Discussions

Interactive comment on "A 1500 yr warm-season temperature record from varved Lago Plomo, Northern Patagonia (47° S) and implications for the Pacific Decadal Oscillation (PDO)" *by* J. Elbert et al.

Anonymous Referee #1

Received and published: 10 April 2013

This paper presents a new temperature reconstruction from Northern Patagonia based on varved lake sediment data. Given the sparcity of climate proxy data from South America, this is a very welcome and important study. Also, the fact that the authors could derive two climate signals (winter rainfall and summer temperature) from the same sediment in a high temporal resolution is very remarkable and a significant step forward in the field of paleolimnology. Therefore, I think this record deserves publication in CP. However, I have some concerns, particularly regarding the analyses related to PDO in the last part of the paper, which, in my perspective, require major revisions of

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the manuscript. As explained in detail below, the analysis in this part of the paper is not clearly explained, has some conceptual problems and leads to partly contradictory interpretations. It needs to be noted that I am not an expert in paleolimnology, so cannot judge about the correctness of the methods used to derive the temperature reconstruction from the lake sediment.

In the following I provide my detailed review of the manuscript, beginning with my major concerns and followed by minor points and detailed explanations for the major concerns.

Major points:

1. In many parts of the manuscript it is not clearly distinguished between the two proxies MAR and brightness/reflectance. Although the latter is used for the temperature reconstruction presented herein, MAR is often used for physical explanations and statistical procedures. For the climatological interpretations to make sense, it needs to be clearly explained that the two proxies are independent.

2. One major achievements of the high resolution record from Lago Plomo is that it allows quantitative analyses and comparison with instrumental and proxy data unlike lower-resolved records where comparisons between records are often limited to visual comparison. Unfortunately, this kind of non-quantitative comparison is often used in this study. I think some “hard” information like correlations could significantly strengthen the results and clarify whether the match between some records is good or not.

3. The PDO-analysis is not very clearly described and confusing. Figure 4 shows a nice agreement between the negative running correlation of the two proxies from Lago Plomo and PDO (based on visual comparison, see point 2). I interpret this in a way that PDO+ coincides with a disconnection of summer temperature and winter rainfall, independent from their individual sign/anomaly. In the analysis presented in Figure 6, PDO+ is interpreted as warm summers (defined as C1) and wet winters (C2)

CPD
9, C391–C399, 2013

are used as an analogue for PDO-. But according to the running correlations, PDO- should represent years of parallel temp.-precip. fluctuations. Figure 6 suggests a direct positive relationship of PDO with summer temperature and a negative relationship with winter rainfall. Apart from that it, is not clear how the years used to define PDO+ and PDO- were selected and the cluster analysis (Figure 6d-i) suffers from circular reasoning.

Minor points

1. Abstract Line 1ff: suggest including the word “climate” before “proxy” and “changes”

2. L. 22ff: Do the data allow to compare the late 20th century with the 19th century warm phase and make statements about a “delayed recent global warming” in the SH? In the text it is stated that the loessfilter applied removes variations on lower than multi-decadal scales.

3. Page 1775 l. 3: The latitudes indicated do not match with the numbers provided in line 19.

4. Page 1775 l. 6ff: The entire part after “In this work. . .” until the end of section 1 is rather a summary/discussion and does not belong into the introduction in my point of view.

5. P. 1776 l.10: replace “suggest “ with “suggested”

6. P. 1776 line 13: Maybe one or two sentences on the climate of the region would be helpful to provide some climatological background on the “less windy conditions in winter” and “windy summer”.

7. P. 1776.l. 24: N/S and W/E gradients are mentioned in the same sentence and it is not clear how exactly the two different gradients are expressed.

8. Section 3: In my understanding the MAR was used as a rainfall proxy in Elbert et al. 2012 and is not used for the temperature proxy record. I found it a bit confusing that

CPD
9, C391–C399, 2013

MAR is mentioned repeatedly in the methods section (e.g. p.1778 l.1 or p.1779 l.11).

9. P. 1778, l.10f. I did not fully understand the definition of the composite core. Does it consist of the mean of the two cores in the overlap period, the data of the long core before that and the data of the short core after the overlap period? Please clarify.

10. P. 1778 l. 14ff: The overlap statistics of the two cores are good. Could a systematic shift or different trends during the overlap period be a potential problem for the calibration and climate interpretation? Adding the data of the individual cores to Fig. 2 (left panel) could illustrate the agreement of the two cores.

11. P. 1779 l.10f: long-term temperature effects on MAR are not of interest given the proxy used herein is reflectance. Do these effects also affect reflectance?

12. P. 1779, last paragraph of section 3. I would move this paragraph describing the instrumental data further up. It should be placed before the description of the calibration and reconstruction.

13. P.1780 l.1: remove “The”

14. P. 1780 second para.: I am a bit confused about the dating uncertainties. Please indicate the total uncertainties accounting from 14C dating and varve counting. I understand that this is 30+37=67 years in the floating section. This is different to the error bars provided in Fig. 3a. These error bars in Fig. 3a suggest a smaller error for the early 14C date than for the later one. Please clarify and state what the error bars represent in the Figure caption.

15. P. 1780 line 15ff: Is it necessary to mention MAR here and show it in Figure 2? I understand it is not used as a temperature proxy and may introduce confusion.

16. P. 1781 l.15: “from 1980 AD onwards” there is another shift to positive values around 1985.

17. P. 1781 l. 9ff: The running correlations are based on reconstructions. How do

CPD
9, C391–C399, 2013

the same correlations look like for instrumental data? This could help showing that the agreement with PDO is not a reconstruction artifact but a true climatological phenomenon. Instrumental data should be included in Figure 4c.

18. P. 1781 l. 21ff: some hard numbers regarding the agreement between the running correlations and PDO would be interesting. For example, are they correlated?

19. P. 1782 l. 1ff: I suggest re-writing this sentence as 1920 and 1960 do not belong to the 1650-1860 LIA period.

20. P. 1782 l. 14f: Please specify in what terms the two sediments are similar.

21. P. 1783 l.3ff: Here the details about uncertainties requested in my minor point 14 are provided. I think this should be moved up to avoid confusions. To me as a non-expert in dating techniques it is not clear how the 140yr uncertainty provided here is different to the $\pm$30 years on page 1780 line 9. Where does the number $\pm$139yr come from? Shouldn't the 140 years and 37 years be combined? Please clarify for non-experts.

22. P. 1783 l. 14ff: This entire explanation of low-frequency impacts on the proxy should be moved up to where the loess filter is first mentioned so that the reader understands the reason for its application.

23. P. 1783 l. 14ff: Again the entire argumentation is based on MAR but reflectance is used as a proxy herein. Does the whole concept also apply to this proxy. In this case the low frequency evolution of the two records should be the same. Is this the case? In line 18, a positive relation between MAR and temperature is mentioned for interannual to multi-decadal scales, but MAR is used as a rainfall proxy in Lago Plomo and reflectance for temperature. If MAR is also affected by summer temperature via melt water or any other process, this means that the following analyses need to be completely re-interpreted. Please make clear if the two proxies are independent.

24. P. 1784 line 25: what is the "good agreement" based on? Correlations? Visual

CPD
9, C391–C399, 2013

comparison?

25. P. 1784 l. 27: I can't see any warm peaks around 710 and 800 AD in the Laguna Escondida Figure. These peaks are later around 750 and 850. Either provide exact dates only when the timing really agrees or be clear the these are very rough comparisons with "agreement" allowing lags of several decades.

26. P. 1785 l.6: Suggest replacing "warming" by "warm phase".

27. P. 1785 last 2 paragraphs: It is a strong point of this dataset that it can be used for quantitative comparisons with other records. However the comparison with the other regional records mentioned and the multiproxy reconstructions is only qualitative. For example, if the Plomo data are not correlated to the Neukom et al. reconstruction this could indicate that the Neukom dataset indeed suffers from the absence of local proxies. The regional tree ring based reconstructions from Villalba et al. (2003, available on NOAA) are independent from all other studies mentioned and may have a better agreement with the Lago Plomo data. I think a bit more interpretation would be adequate here, particularly as favourable comparisons and "significant regional differences" are mentioned in the abstract and also in the conclusions. This need to be elaborated on or removed from the abstract.

28. P. 1785 l. 23: I Suggest putting Medieval Climate Anomaly into quotes as the warm period identified in South America 1200-1350 is clearly delayed to the typical MCA defined from the NH.

29. p.1786 l. 3-4: "negative decadal trends" Please explain. Is PDO related to the values of the running correlation or to the trends? The trends are not mentioned elsewhere so could be removed here.

30. p.1786 l. 12: I assume panels e,f,h,i of Figure 6 are also based on UDel data, not CRU? Please specify.

31. p.1786 l. 10ff: What are the criteria to define years for the PDO+ and PDO-

CPD
9, C391–C399, 2013

phases? From visual comparison they do not fully match the signs of the curves in Figure 4c or 4d. What are the thresholds used? What dataset?

32. p. 1786 l. 24ff: How were the clusters calculated? Please describe the method. The years provided for C1 and C2 don't agree with the anomalies in Fig. 4a and 4b (e.g. the "warm" 1922 summer falls into a period of cooling in Fig. 4b. similar disagreement for many other years). Were instrumental data used or the proxy reconstructions?

33. P. 1786 l.26: What is the "whole" period? 1901-2006? The reconstruction period?

34. p. 1786 l. 24ff: In the text four clusters are mentioned. What are C3 and C4? Maybe a more detailed explanation of the analysis could be included in the methods section.

35. The clusters are defined using temperature and rainfall data. Then C1 and C2 were allocated to PDO+ and PDO-. How was this done? By counting the years with PDO+ and PDO- for each cluster? Given that these "PDO+" and "PDO-" years are defined based on temperature and rainfall data, it is not surprising that they correspond to temperature and rainfall anomalies: Figures 6e,f,h and i and their interpretation are circular reasoning. PDO data need to be used to define PDO phases.

36. P. 1787 l.10ff: The dynamical explanation provided and the lower two rows of Figure 6 suggest a direct relationship of PDO to temperature and rainfall, not a decoupling/coupling of the two as in Figure 4 and the top row of Fig. 6. Please reconsider the analysis and make sure to provide a consistent interpretation.

37. P. 1787 l. 20: Fig. 6g does not show "less cold advection", rather non-anomalous conditions (white color in the plot).

38. P. 1787 l. 18ff: Several other PDO reconstructions exist; see e.g. the recent study of Grove et al. (2013) in Climate of the Past. A discussion of the different records for interpretations in different regions is also provided in that paper, which may be helpful.

39. P 1788: The Macdonald & Case (2005) record is shown in Figure 5 but not dis-

CPD

9, C391–C399, 2013

cussed in the text. Either remove it from the plot or discuss its agreement with the Plomo data.

40. P. 1788 l.2ff: Again, the comparison of two records is based on visual comparison only. A “relatively positive PDO” is mentioned for the Mann et al. reconstruction within 1530-1610 but Fig. 5d shows negative anomalies during that phase. Adjusting the reference period between the Figure and the explanatory text would help avoiding confusions. “a number of similar features jointly found in various independent datasets” is a very imprecise statement. What kind of features? Which datasets? Any statistical significance?

41. P. 1788 l. 15: "very large“ for what?

42. P. 1788 last paragraph of section 5: Argument (ii) questions the entire analysis: If a single site PDO reconstruction such as the one from Lago Plomo is really “not recommendable” then why is it performed? I would remove that statement. For the discussions of the difficulty of regional interpretations of PDO I would rather follow the discussion in Grove et al. 2013.

43. P. 1789 l. 14ff: I would remove all text between “For. . .” and “..implying that” as it is more discussion than conclusion.

44. P. 1789 l. 22. I would remove the word “Thus”.

45. Figure 1 caption: Mention that the coring sites in the left panel are represented by asterisks. Suggest rewriting the second sentence as Follows: “SONDJF instrumental temperature Pearson Correlation map between Lago Plomo grid cell (asterisks). . .” What do the white areas with no contour lines represent?

46. Figure 3: Mention the error bars in the caption and replace +0.85 with =0.85

47. Figure 6: Suggest using green color or similar to improve visibility of the Lago Plomo location. Add variables to figure titles in panels d-i (e.g. JJA: C1-C2 wind / temp. / precip.) for an easier understanding of the panels.

CPD

9, C391–C399, 2013

References: Grove, C. A., Zinke, J., Peeters, F., Park, W., Scheufen, T., Kasper, S., Randriamanantsoa, B., McCulloch, M. T. and Brummer, G. J. A. (2013). Madagascar corals reveal a multidecadal signature of rainfall and river runoff since 1708. Clim. Past 9 (2): 641-656.

Villalba, R., Lara, A., Boninsegna, J. A., Masiokas, M., Delgado, S., Aravena, J. C., Roig, F. A., Schmelter, A., Wolodarsky, A. and Ripalta, A. (2003). Large-scale temperature changes across the southern Andes: 20th-century variations in the context of the past 400 years. Climatic Change 59 (1-2): 177-232.

Interactive comment on Clim. Past Discuss., 9, 1771, 2013.