Clim. Past Discuss., 9, C1052–C1054, 2013
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Climate of the Past Discussions

Interactive comment on "Late Holocene summer temperatures in the central Andes reconstructed from the sediments of high-elevation Laguna Chepical, Chile (32° S)" *by* R. de Jong et al.

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Received and published: 8 June 2013

Dear Dr. Guiot

The article entitled Âń Late Holocene summer temperatures in the central Andes reconstructed from the sediments of high-elevation Laguna Chepical, Chile Âż by De Jong et al is a nice contribution to the field of paleoclimatology. It is well written, and it brings crucial data, in an area where both long-term and short-term climate data are either lacking or very scarce. Overall, I think that this article uses a novel sedimentary proxy (R570/R630), which apparently reflects clay mineral content of lacustrine sediments. The study also provides adequate dating (Pb and Cs + radiocarbon) of the

sedimentary sequence and presents an accurate discussion of potential dating errors. For these reasons, I recommend that this article should be published in Climate of the Past, after minor revisions.

On the methodological aspect, I can't comment, as these techniques are far from my field of expertise. My main comment will therefore concern the nature of the climatic signal extracted from the sedimentary chronology. The authors perform a series of correlation on the modern period and identify moderately strong relationships (r= -0.67) with summer temperatures. Based on this correlation, the authors perform a reconstruction of NDJF temperatures (austral summer) for the last 3000 years. This is a classic approach in paleoclimatology, but I think that, it this case, the main flaw is that it relies on a weak demonstration that NDJF temperatures are actually a good (the main?) driver of the vast array of sedimentary processes that occur at Laguna Chepical, Chile. The authors argue that this relationship probably reflects, though indirectly, the interactions between sedimentary processes and ice cover dynamics. Following this argument, the authors suggest that "the longer the ice-covered period and the later the timing of ice-break-up (. . .) the more fine sediment particles will settle Âż. While I agree that the duration of the ice covered period is an important process in such a high-altitude lake, I am not convinced by the demonstrations proposed here. What is the evidence that warmer summer temperatures translate to a longer ice-free period? How are ice covered dynamics related to summer temperatures in anyway? So many variables other then summer temperatures influence ice cover dynamics in a much more direct way (to name a few, independently of the site): 1) Winter temperatures (degree-days of frost). Low winter temperatures are associated with a thicker ice cover due a deeper penetration of the freeze-up front. 2) Snow precipitation during winter: The less snow there is, the thicker the ice cover will be, due to a reduced insulation effect. 3) Early spring / late autumn rain showers and high temperatures: they transfer heat to the waterbody and accelarate the melt / delay the freezeup. Considering all these processes, I would recommend that the authors try to better demonstrate the links that exist between summer temperatures, ice cover dynamics and the varia-

tions in clay content in their core. Is there any data on freeze-up / breakup dates in the area? Can the authors actually use, even for smaller time scales, daily temperature data to calculate degree-days of frost (<0°C) or degree-days of heat (>0°C)? Such data could be compared to the monthly reanalysis data extracted from the CRU to confirm that warm summer temperature means are effectively evocative of processes or metrics that control ice cover dynamics at their site, or in the area. I think that this would reinforce both the argumentation that their sedimentary proxy (R570/R630) is a valuable indicator of summer temperatures, but also the reconstruction. We must not forget that this paper builds on such interpretations to propose a 3000 years reconstruction of summer temperatures. My impression is that this reconstruction would be even more credible if the processes linking sedimentary sequences to summer temperatures would be clarified more fully.

For instance, this is my only comment, and I hope it is perceived as useful to improve the quality of the manuscript that is already pretty high. I congratulate the authors for providing such a high quality, long-term paleo-temperature in this part of the southern Hemisphere that clearly needs to be documented more intensely.

Cordially

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Interactive comment on Clim. Past Discuss., 9, 2277, 2013.