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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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Response to reviewers' comments

First of all, we would like to thank both reviewers for their helpful comments and suggestions. We will now respond to the comments by each reviewer.

Reviewer #1, Etienne Boucher The reviewers' main (only) objection is that the relationship between the sediments' clay content and the environmental variable driving this (according to the paper; summer temperatures) is not clearly demonstrated and that other parameters may also be important:

'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 inïňĆuence 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 accelerate the melt / delay the freezeup

We certainly agree that other variables other than summer temperature can potentially influence the duration of ice cover. However, we tried to assess these potential influences as well as we possibly could, considering the scarcity of data in this region. We calculated correlation coefficients for temperature as well as precipitation data for all months and possible combinations of (consecutive) months. However, we did not find a significant correlation for winter temperatures (point 1), snow precip (point 2) or rainfall during specific seasons (point 3). We are aware that in particular the reanalyses data for precipitation may be unreliable for mountainous regions; however, we have no other datasets to compare our results to. So from the data we do have, we can conclude that the other variables mentioned by the reviewer did not have a significant influence on the sediments clay content. We have now clarified this in the text. We really appreciate the suggestion by this reviewer to do a short-term analysis on the link between degree days of frost or heat and compare these to the CRU data. However, such data are not available for this site. . . So, in short; we now clarified that the other variables mentioned by the reviewer could not explain a significant proportion of the proxy – variability, and that therefore, based on the available data, summer temperature is currently the only 'known' (and significant) driver of the clay content in the lakes sediments. We also think that this finding makes sense; the lake is situated at such a high altitude that the ice

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free period is limited mainly to the summer months. The lake is relatively small and shallow, so the heat retention capacity will be low, which implies that as soon as air temperatures fall below zero, the lake will freeze quite rapidly. Therefore, the temperatures in December (ice melting) and in March (freezing) together likely determine the length of the ice-free season.'

response to Reviewer # 2

The reviewer lists a number of technical/typing errors, which we have now corrected. Moreover, the reviewer suggests that the age model (the older part based on radiocarbon dates) could be improved. A figure was provided with an alternative age-depth model. However, we used the ShCal04 Southern Hemisphere calibration curve (McCormac et al., 2004), which, to our knowledge, is still the current standard calibration curve to use. The final age model (with uncertainty ranges) intersects those dating points in the parts with the highest probability. We therefore don't see why the current age model would need to be replaced. Unfortunately, the reviewer did not provide a reference or explanation to the alternative model, which makes it difficult to respond to the alternative suggestion.

With kind regards and once again, thanks to the reviewers for their helpful comments and suggestions!

Rixt de Jong and co-authors

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