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Climate of the Past
Discussions

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Interactive Comment

Interactive comment on “Climate history of the Southern Hemisphere Westerlies belt during the last glacial-interglacial transition revealed from lake water oxygen isotope reconstruction of Laguna Potrok Aike (52° S, Argentina)” *by* J. Zhu et al.

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The authors are grateful to both referees and the editor H. Fischer for their constructive and valuable comments and criticisms. All the comments were carefully considered.

In addition to the changes suggested by the referees, we also named the updated age-depth model as version 3.1 (v 3.1) following the early version (v 3) provided by Kliem et al. (2013) and corrected some sentences that needed to be restructured.

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Anonymous Referee #1 This paper presents an oxygen isotope record from Laguna Potrok Aike in Patagonia, derived from cellulose and bulk organic matter found in the lake's sediments. The record dates back to the last glacial/interglacial transition and allows for a reconstruction of lake water delta 18O, which in turn is attributed to temperature-driven rainwater d18O variations and evaporative enrichment. Since the evaporative enrichment is to some extent a function of wind speed, it may allow for a reconstruction of past wind characteristic associated with the Southern hemisphere westerlies. The paper is very well written and easy to read and follow along. The analyses have been performed very carefully and all interpretations, to the extent that I can judge, appear logical and justified. However, I am not a geochemist and cannot assess the adequacy of the lab analyses. Some of the interpretations remain a bit speculative since the relatively minor enrichment in 18O since the last glacial are not easy to explain. The authors put forward several hypotheses to explain this conundrum. Ultimately, to answer this question will probably require a combination of proxy analyses and isotope-enabled climate modeling. Nonetheless this paper provides an important first step in the right direction. I only have a few small comments, listed below, and suggest accepting the paper after minor revisions have been incorporated.

Fig. 3 demonstrates that both temperature and d18O follow a seasonal cycle, but this does not prove a causal mechanism (both may simply be driven by a common forcing that also has a seasonal cycle). This lack of removal of seasonality is a common mistake in paleoclimate research. The correct analysis here would require using monthly anomalies (departures from long-term monthly mean) to verify that this relationship (which may be time-scale dependent) also holds on interannual time scales with seasonal forcing removed. Statements, such as in the first paragraph of section 5.1, that the analysis in Fig. 3 indicates an influence of long-term temperature change on d18O remain conjecture as long as they are based on raw monthly data without removal of the seasonal cycle.

Reply: We agree with the comment of the reviewer and removed Fig. 3 from the

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manuscript. As pointed out by the editor, Fig 3 is not really required for our manuscript as the calculations/inferences are based on a longer-term d18O/temp gradient derived by Rozanski et al. (1993). Fig. 3 was solely given to show that the principal temperature – isotope relation can be seen in the regional precipitation data and, therefore, that it is valid to use it here. The overall uncertainty introduced by the approach would apply for both Glacial scenarios and, thus, would not change the overall considerations, especially since we do not strive for a temperature reconstruction. That would of course be the case if the d18O/temp gradient would not be stationary in time. However, other studies have either further substantiated the Rozanski et al. (1993) gradient (Kohn and Welker, 2005), or found no convincing evidence for differences in spatial and temporal slopes in GCM experiments for the LGM (Jouzel et al., 2000). In the case where a difference was found in the GCM simulations for the LGM this was attributed to local conditions in Antarctica, especially near the ice edge (e.g. Lee et al., 2008). Overall, this is a much broader issue and well beyond the scope of this manuscript. Nevertheless, this question needs further evaluation.

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Section 5.3 and Fig. 10: The ITCZ may indeed have shifted southward prior to the onset of the last deglaciation, but Botuvera is not really an adequate record to discuss this phenomenon. Botuvera does not receive precipitation from the ITCZ, but from the South American summer monsoon (summer) and the SW Atlantic (winter) months. Hence it is primarily a recorder of the waxing and waning of the summer monsoon and not the ITCZ (see discussion in original publication of Botuvera Cave; Cruz et al., Nature, 2005).

Reply: We agree with the referee in part. However, as discussed by Cruz et al. (2005), the seasonal balance in precipitation at the location of Botuvera Cave is "controlled by the long-term mean location and southward extent of convective activity associated with the South American summer monsoon and southern boundary of the Hadley cell in the Southern Hemisphere". The Hadley cell circulation is undoubtedly related to the shift of the ITCZ. Other authors have indeed interpreted intensified South American summer monsoon (SASM) precipitation as a result of its response to the southward displacement of ITCZ during the Holocene and the last Glacial (Bird et al., 2011; Kanner et al., 2012). Accordingly, we consider it adequate to cite the data of Botuvera Cave as evidence of ITCZ migration during the Glacial-Interglacial transition. We improved the text as follows : Page 2440, Line 3 "About two millennia prior to the onset of the last deglaciation, the Intertropical Convergence Zone (ITCZ) also began to shift southward, as indicated by intensified South America summer monsoon precipitation in South Brazil and diminished rain falls in the drain regions of the Cariaco Basin (Wang et al., 2007; Deplazes et al., 2013; Fig. 10g and h)."

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Minor edits: Page 2447, line 11: 'tracers' Page 2448, line 12: 'referencing of journal 'Palaeo3' is incomplete. Page 2451, lin15: 'Arctic'

Reply: The typos have been corrected accordingly. The name of journal "Palaeogeography, Palaeoclimatology, Palaeoecology" is abbreviated according to the "ISI Journal Title Abbreviations Index".

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Anonymous Referee #2 This paper addresses the climate interpretation of a high resolution ca. 25,000 year long d18O of lake water reconstruction. The description of the site, reconstruction methodology and regional climate are described here and in other papers already published by the group. Overall the manuscript is well presented and figures clear. Hence should be published. My main concern about the paper is that given that the climatic interpretation of the results is not straightforward, different scenarios a given, and the end the message does not come across very clearly. Clarifications in the interpretation need to be addressed before publication. The climatic setting and the forcings on the lake show some variability, so that there is no one clear interpretation for a same d18O value. I might have misunderstood some of the reasoning, so please clarify.

Discussion: The discussion of the results is separated in 3 stages. 1. Glacial period: here the main discussion is how/why the mean d18O values are only 3‰ lower than at present, the authors present 2 different scenarios: (i) d18O of meteoric water and groundwater markedly lower than present: depleted d18O by colder temperatures would then imply that evaporative enrichment has to be even stronger that at present –> SHW were stronger during the LGM? I would state this hypothesis clearly at the end of line 25 of page 2436.

Reply: In the manuscript we discussed the two scenarios which could result in unexpectedly enriched glacial δ18Olw-corr. In scenario (i) we assume that δ18O of Glacial lake water was largely determined by strong 18O depletion of meteoric water. Considering this, strong 18O enrichment should be accounted for. We hypothesize that evaporative 18O enrichment could be even stronger than at present. Citing the work in the modern McMurdo Dry Valley of Antarctica, it is reasonable to imagine that high evaporation at Laguna Potrok Aike during the full Glacial could also be achieved by strong and dry regional foehn winds passing the ice-covered southern Andes. This hypothesis is indeed based on the existence of the SHW at the latitude of Laguna Potrok Aike during the Glacial. However, its strengthening compared to the present was not necessary for the interpretation.

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Page 2437, Line 16: “higher-than-expected” change to “higher than present”?

Reply: We have changed it accordingly.

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Page 2437, Line 25: I don’t understand this sentence: “In fact, paleoclimate studies from sites between 30 and 45S in southwestern South America have implied much higher precipitation during the Glacial compared to the present”. If your evidence re-

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quires stronger winds at 52S during the glacial, and other studies suggest stronger winds at 30-45S, then the SHW became a LOT wider? Please clarify what exactly you want to say here.

Reply: In scenario (i), we hypothesize the existence of the SHW at the latitude of Laguna Potrok Aike during the Glacial. Combining with other studies which indicate stronger SHW at 30-45S, it could be suggested that the SHW became wider during the Glacial. We have included this into the manuscript: Page 2437, Line 28 “Taken together, that would imply a broader latitudinal extension of the SHW during the glacial compared to the present.”

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(ii) Moderate change in d18O of source water compared to the present. Here the idea is that the SHW were shifted equatorwards, hence balance of easterly and westerly air masses at study site changes. Page 2438, line 2: “If the SHW is located” change “is” to “are”. Question: why is it assumed that the temperature / d18O relationship from Punta Arenas does not hold for precipitation from Atlantic origin?

Reply: Undoubtedly, the temperature/δ18O relationship also holds for precipitation from Atlantic origin. However, δ18O of modern precipitation brought from Atlantic origin is more enriched in 18O that those brought from Pacific origin. The enrichment is about 7‰İf this relative enrichment is retained during the Glacial, a largely increased proportion of precipitation from Atlantic origin as resulting from an equatorward shift of the SHW would lead to more 18O enriched meteoric water in scenario (ii) than is assumed in scenario (i).

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Page 2438, lines 9-27: I don’t understand the argument here. The discussion about permafrost and its effect on the hydrological balance of the lake is also valid for hypothesis (i), or not?

Reply: That is not necessarily so. First of all, the evidences for permafrost in the region are still scarce and neither information on its potential lateral extension nor on its penetration into the ground is available. Therefore, permafrost can be an important driver but its effect remains speculative. Thus, permafrost is but one factor for our argumentation. Second, hypothesis (i) leads to more arid conditions compared to hypothesis (ii) because we assume the predominance of the Westerlies during the Glacial. Since the Westerlies are per se dry and we further argue for a foehn-wind effect the climate setting would be even drier. It seems questionable if under these dry conditions sufficient moisture would be available to build up deep-penetrating permafrost. Based on that rationale, we did not include the factor permafrost in our hypothesis (i).

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2. Deglaciation: this discussion seems clear enough. Are these interpretation consistent with Moreno et al, 2012? (Deglacial changes of the southern margin of the southern westerly winds revealed by terrestrial records from SW Patagonia (52_S))

Reply: Yes, our interpretation for the SHW evolution during the deglaciation is to some extent consistent with Moreno et al. (2012). We added this reference in the discussion.

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Conclusions: Why are the SHW not mentioned in the summary for the glacial part of the record? Was the conclusion not that the SHW where further equatorwards?

Reply: Considering the occurrence of the deep permafrost and previous hypothesis for an equator ward shift of the glacial SHW, scenario (ii) is preferred to explain the observed glacial δ18Olw-corr. We improved and rewrote the conclusion accordingly: Page 2443, Line 15 "Our interpretation is based on the validity of the concept of applying the mainly spatial temperature – d18O-precipitation relation of the mid-latitudes to infer temporal d18Olw-corr variations." Page 2443, Line 16 "Considering two competing hypothesis, the δ18Olw-corr record between 26,000 and 21,000 cal. BP is currently

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best explained by a scenario of weakened Glacial SHW. With moisture from easterly directions and the occurrence of permafrost during the Glacial, the inflow into the lake would not be as depleted as under a strong SHW scenario. Moreover, reduced interchange between in- and outflows and generally decreased inflows would have prolonged the lake-water residence-time. Under these circumstances, the higher than expected δ18Olw-corr during this period could be achieved despite possibly weakened evaporation under Glacial conditions. This interpretation is consistent with the hypothesis of an equator ward shift of the SHW during the last Glacial." Page 2444, Line 2 "Such an early meltwater flow could be caused by an initial climatic amelioration phase preceding the genuine deglaciation." Page 2444, Line 7 "Thus, an early strengthening of the SHW is not a necessary prerequisite to explain the observed pattern."

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Other comments Page 2421, line 17: what do the authors mean by "variable precipitation"? Low correlation is also probably related to the low elevation of the Andes at 52S?

Reply: We refer to the high variability of annual precipitation in the semiarid environment. We have changed the text accordingly. It might be that the lower elevation of the Andes contribute to that observation, however, no studies have yet attributed this topic.

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Abstract: The description of the results could be done in a clearer way. I understand that the authors describe 3 stages: glacial, deglaciation and Holocene, but at this point little interpretation of the evolution/changes of SHW is provided, as you would expect from the title!

Reply: We agree with the reviewer and have partly rewritten the abstract.

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Figures: Figure 1a: include position of Punta Arenas. Could you also include topography to see the Andes?

Reply: The data from Punta Arenas of Fig. 3 has been removed from the manuscript following the recommendation of Reviewer 1.

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Figure 2: the color scales in both panels are not exactly the same, which makes the reading difficult. If you are only interested in the westerlies then I would just plot u > 4m/s (or something like this). This would help the discussion on the latitudes at which the SHW are significant.

Reply: The color scale in the right panel has been changed. Now both panels use the same color scale shown in the middle.

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Interactive comment on Clim. Past Discuss., 10, 2417, 2014.