Clim. Past Discuss., 9, C759–C766, 2013
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Climate of the Past
Discussions

CPD
9, C759–C766, 2013

Interactive Comment

Interactive comment on "Accumulation reconstruction and water isotope analysis for 1735–1997 of an ice core from the Ushkovsky volcano, Kamchatka, and their relationships to North Pacific climate records" *by* T. Sato et al.

Anonymous Referee #2

Received and published: 14 May 2013

General comments:

Sato et al. present an extended Ushkovsky ice core record of δD and accumulation back to AD 1735, which they say provides evidence of major North Pacific ocean-atmosphere changes in the late 19th century that are also recorded in alpine ice cores of N.W. North America. Alpine ice cores are an important paleoclimate archive, and few of these records have been developed in the North Pacific region. On those grounds, this study represents an important contribution, and the subject matter is certainly relevant to the readers of Climate of the Past.

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Interactive Discussion
Discussion Paper

Overall, the paper is well written. I have made suggestions in my specific comments on how to improve the readability further. The methods used to develop the proxy records are sound, and they are interesting records for sure. These positives aside, I have a number of concerns that the authors should consider carefully before this paper goes any further. These concerns are mainly on statistical analyses, presentation of findings, and conclusions made that I do not find are well supported. Also, the discussion is seriously lacking. In my opinion, this paper is not acceptable as it is, but could be acceptable after moderate to major revisions. I wish the authors all the best in preparing a revised manuscript. I have summarised my more serious concerns just below, and my specific comments/suggestions below that.

1) Discussion is seriously lacking, a point that was also raised by the first reviewer. There was little context provided for this study and its findings, in terms of previous studies and how this study is distinct. Furthermore, the physical mechanisms that govern climate signals in the proxy records were largely ignored. These points should be addressed in the revised manuscript.

2) The authors claim that these proxies are valuable records of North Pacific climate variability, but often the statistical analyses lacked rigor and did not always support the conclusions. Here are some examples:

-Based on visual inspection of the δD record, the authors conclude that multi-decadal variability is diminished during the latter part of the record (P2161/L19-21). I disagree. I count five distinct cycles, peaks and troughs. The first peak occurs at ca. 1790 and peak 5 occurs at ca. 1980. The amplitude of peak 5 is comparable to peaks 2, 3, and 4. Peak 1 is the only peak that is abnormal in amplitude. Another abnormality is the periodicity of the cycle associated with peak 4, which is longer than normal. But overall, there is little evidence to support the claim that multidecadal variability is weaker during the latter part of the record. This is a sample size (N) problem, only 5 cycles.

-Correlations with local climate data are made, but significance (p) values are not reported in most cases. These must be included. Where correlations are non-significant, the abbreviation 'n.s.' could be used.

-Correlations are provided as a measure of association between local climate and proxy data, often based on short time periods (e.g., 1961-1991). As noted by the other reviewer, longer climate records are available for this region since 1920 (Solomina et al., 2007, CP). I agree that longer climate records should be used to better characterise the climate sensitivity of the proxy records. Correlations based on shorter periods are more susceptible to sampling error and false-positive results.

-Longer instrumental records would allow for an analysis of the stability of the climate-proxy relations. This is missing from the manuscript, and I wonder how robust some of the highlighted climate-proxy relations really are. One example is the relation between δD and NPGO. This was a major finding highlighted in the abstract, but the authors have done little to support the claim that δD is a good proxy for NPGO. The NPGO-δD correlation is strong over the 19 year period 1979-1997 ($r = 0.70$), but weak from 1950-1997 ($r = 0.27$; i.e., δD explains only 7% of the NPGO variability). Based on this result, I cannot accept the conclusion that δD is a suitable proxy for NPGO. The authors need to do more to argue their case. I would suggest providing a (smoothed?) time-series comparison of δD and NPGO, and correlations for the first and second halves of the longer instrumental period (e.g., 1920-1958 and 1959-1997). If similar correlations are observed for both periods, then it is fair to conclude a stable climate-proxy relation exists. Ditto for the accumulation record.

-Based on the 'N-issue' discussed above, it is difficult to make conclusions about multi-decadal variability. A wavelet analysis was used to support the conclusion that a significant multi-decadal (40-60 year band) change happened at the end of the Little Ice Age, but this finding is not well supported. The 'Cone of Influence' (COI) was omitted from the wavelet diagrams (see Torrence and Compo 1998), which delineates the area of the wavelet spectrum that is meaningful. Some of the power spectrum (more so at longer periods) is artificial due to zeros used to pad the ends of the time-series and, thus, this information is uninterpretable. Based on Fig. 8a, it seems that δD record is energetic in the 40-60 year band from ca. 1735-1875. However, if the COI is considered, it is not possible to interpret the 40-60 year band prior to ca. AD 1800, and since ca. AD 1925. Use of the wavelet analysis to argue for multi-decadal changes is problematic, and I recommend it is excluded from the revised manuscript.

3) The authors suggest their δD record provides evidence of a major ocean-atmospheric regime change, consistent with a mid-19th century regime change recorded in the Mt. Logan ice cores in Yukon, Canada. There are some very important differences between the two records. One is that the shift in the Logan ice cores is represented by a marked step change in water isotopes that is unique since at least AD 800. Also, this step change was towards more depleted values, which is contrary to the direction of warming since the Little Ice Age. The Ushkovsky δD record shows a positive trajectory since the late 19th century, which is an important difference between these two records that was not discussed (hint!). The Logan δD/δ18O step change was linked to changes in atmospheric circulation (as the authors correctly note), and subsequent studies involving isotope-GCM experiments (Field et al., 2010, GRL; Porter et al., 2013, Climate Dynamics) confirm that meteoric isotopes in this region are sensitive to circulation. Comparatively, the δD change in the Ushkovsky record is consistent with secular warming since the LIA, and there does seem to be a moderate temperature-δD correlation during the instrumental period. By raising these points, it is my hope that the authors will more fully interrogate the dataset and discuss all possible driving mechanisms, which may be completely unrelated to the circulation shift that affected the Mt. Logan ice cores (see also Anderson et al., 2005, Quat. Res.).

4) I am surprised the authors decided not use their δ18O record to calculate d-excess, which can be a good indicator of changes in atmospheric circulation, precipitation seasonality, and moisture source. This would be a valuable record to include in the revised manuscript, especially since the authors are interested in major climate system changes.

CPD

9, C759–C766, 2013

Specific comments:

P2155/L1-2 – Yasunari et al. (2007) is the only reference that pertains to an Alaskan ice core. Holdsworth et al. (1989), Wake et al. (2002), Shiraiwa et al. (2003), and Fisher et al. (2004) refer to Mount Logan ice cores, which are from the St. Elias Mountains in Yukon Territory, Canada. Maybe revise the sentence to "...from ice cores in Alaska and Yukon ...".

P2155/L16 – Delete "shows that the record"

P2155/L20 – Suggest adding "...Shiraiwa and Yamaguchi, 2002), a timing that coincides with a major PDO shift (Hartmann and Wendler, 2005)." – or find another reference, there are many.

P2156 /L2 – ...with a maximum depth of $\sim$ 240 m...

P2156 /L11 – ...preserved in the core...

P2156/L17-21 – The format used to reference the two flow models is awkward. Suggest creating a second sentence, e.g. "The two flow models that were used were the Salamatin (Salamatin et al., 2000) and Elmer/Ice (Zwinger et al., 2007) models."

P2157/L6-8 – Confusing sentence. Suggested revision, "The intervals varied in thickness from $\sim$100 mm near the top of the core, $\sim$50 mm near the middle of the core, and $\sim$30 mm near the bottom of the core."

P2157/L11 – Change to "The water isotope samples from the first..."

P2157/L13 – Spurious precision. Only one significant figure should be reported. Change 1.02‰ to 1‰Ȧlso, is this the 1 or 2 sigma precision? Please specify the 2 sigma precision.

P2157/L15-17 – The precision you have reported seems impossibly low. Typical precision is 2‰ for δD and 0.2-0.3‰ for δ18O. I doubt your numbers are correct. Ensure you are specifying the 2 sigma precision.

CPD
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P2157/L17 – Does “substandard water probes” = “internal (or secondary?) water standards”?

P2161/L18 – The average δD value...

P2161/L19 – “The average value increased by 6.0‰ from 1880 to 1910.” This is misleading. In the previous sentence you provide the mean δD values for 1735-1880 and 1910-1997, which are offset by exactly 6.0‰. This does not mean that δD increased by 6.0‰ between 1880 and 1910. Coincidentally, the 20 year mean does change by roughly 6.0‰ from 1880 to 1910. You need to revise this and the previous 2 sentences. It is more accurate to say “There is a change in mean δD values in the late 19th century. The 1735-1880 and 1880-1997 periods have a δD offset of 6.0‰ with mean values of -162.1‰ and -156.1‰ respectively.”

P2162/L13-15 – Why compare the proxy records with January-December climate indices? Is this the interval that most likely corresponds with the sampling interval? Please explain in the revised manuscript.

P2162/L24-25 – The sentence about δD is out of place. Relocate to the previous paragraph, and elaborate. Specifically, it is not clear what you mean by the statement “The annual minimum δD cannot be dated to form a climate dataset?

P2163/L13-14 – State the correlation period with the ERA40 data (e.g., 1957-1997).

P2163/L15 – Insert the following, “For δD, there is a significant...”

P2163/L15,16 & 19 – It is more correct to say there are correlations for the latitudes 20-30° and 40-50°. Also, you should indicate the latitudes for the δD correlations.

P2163/L21-22 – “extratropical Pacific” is more correct, since δD correlates significantly at 20-30°?

P2164/L2-3 – How much smaller is the accumulation-NPGO correlation?

P2164/L4-6 – Confusing. Consider revising to “...almost 80% of winter precipitation and 50% of summer precipitation over Eastern Siberia originates from the North Pacific."

P2164/L20-22 – What do you mean by "de-trended"? How does this affect your wavelet results?

P2165/L1-2 – Some of the terminology used is inappropriate. For example, the opening sentence explains that the record has significant coherences at various frequencies. Better to say that the record expresses variability at these frequencies, and these results are statistically significant.

P2165/L18-21 – Based on the evidence you present, I am not convinced that the Pacific Northeast experienced the same climate regime shift demonstrated in the Mt. Logan ice cores; see also the Jellybean lake δ18O record from that region (Anderson et al., 2005, Quat. Res.).

P2169/L3-5 – The journal abbreviation is incorrect. The full journal name is Palaeogeography, Palaeoclimatology, Palaeoecology. The abbreviated title should be Palaeogeogr. Palaeoclimatol. Palaeoecol.

Concluding section – most of this is simply a re-hashing of the points made prior to this section. I would advise the authors to write a more critical (and concise) synopsis here, establish why this work is important, and finish with recommendations for future research, as was already done in the final paragraph of this section.

Fig. 1 – Based on your map, readers who are not familiar with the Kamchatka region might have trouble understanding the geographic context of your record. I would recommend using a small scale map that includes the entire North Pacific region. You may also want to show locations of other North Pacific ice cores, to demonstrate the paucity of these records and need to develop more ice core records from this region.

Figs. 8a and 8b – Plot the 'Cone of Influence' (COI) on your wavelet diagrams (refer to Torrence and Compo, 1998).

Interactive comment on Clim. Past Discuss., 9, 2153, 2013.