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Climate of the Past Discussions

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 #3

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This manuscript describes the temporal extension of an ice core record of accumulation and hydrogen isotope ratios from Kamchatka in the northwest Pacific/eastern Siberia. From the description in the manuscript, this appears to be an extension of an earlier dD record that previously extended back to 1823. The current dD record is extended to 1735 (∼90 years new) but now includes accumulation data as well.

The primary analytical result with respect to climate here is a set of smoothed correlations between dD/accumulation and ERA40 2m temperatures over the Pacific. Local correlations against meteorological data are carried out, and wavelet analysis is used

to infer the existence of and changes in multidecadal scale variability reflected in the ice core proxies.

My concern is that the results of the manuscript rely heavily on a series of statistical analysis whose robustness seems uncertain. There are generally weak and insignificant correlations on year-to-year timescales with accumulation (P2162,L23-27), although there are significant correlations between precipitation-weighted temperatures and dD. The running mean correlations (P2162,L25 onward) are unconvincing, since the reduced degrees of freedom from the moving average will inflate the correlation coefficients and the period of comparison is already quite short. Despite the weak correlation to local climate variables, however, the authors make inferences about remote controls on their ice core proxies using correlations to Pacific 2m meter temperatures from ERA40 reanalysis data. First, this seems like an odd field to use – since the authors invoke decadal mechanisms like the PDO and 'NPGO', why not compare to sea surface temperatures and to SLP?

Second, the correlations are done on smoothed data (Figure 7b) in the case of accumulation – were significance levels adjusted for the high degree of autocorrelation introduced to the short instrumental record by the 3-year mean? Why was a 3-year mean used for the field but a 5-year mean used in the text?

Third, most of the significant correlations in the North Pacific are found to the east of the dateline (likewise, the weigh of the 'NPGO' is to the east of the dateline in the northern Pacific)– what is the mechanism whereby northeastern Pacific air temperatures influence eastern Siberian dD and accumulation? Numaguti (1999) actually differentiates between western and eastern Pacific sources, and only 25% of the water in eastern Siberia is from the eastern Pacific, as they classify it. Numaguti's Figure 4 would also suggest local western Pacific sources would dominate, so perhaps what is seen in the correlation fields is the spatial covariation of local temperatures with the large-scale field. In any case, though, it would be substantially more convincing if the relationship could be shown to be consistent using more than just 2m temperature from a particular

reanalysis field and if a more robust mechanism could be suggested for the remote influence of climate in absence of a clear connection to the local climate.

Finally, the authors use wavelet analysis to identify the existence of multidecadal power in their proxies. One omission in the plots is the 'cone of influence', however, which indicates those parts of the temporal-spectral space that has been influenced by padding during analysis. The authors should re-draft Figure 8 with the cone of influence plotted. In terms of analyzing the existing plots, however, I have two concerns: Much of the power in the record is at the 32-64 year period in the earlier part of the record – but only appears for $\sim$100 years, or at most 2 or 3 cycles at these periods? This is another reason the cone of influence should be plotted, so that we can assess the significance of that early multidecadal power. Also, at least in the dD the multidecadal power seems to terminate abruptly around 1850 or so – is it possible the change in the spectra characteristics of the proxies is related to the join of the previous (to 1823) and new (extended to 1720s) part of the core? Also, is it possible that changes in the nature of the ice core with depth (particularly the thinning rate) could change the spectral characteristics?

Minor Comments:

P2155L14: There is a more recent tree-ring analysis of the PDO by D'Arrigo and Wilson 2006, 'On the Asian expression of the PDO' – it would be good to compare their PDO reconstructions to the ice core time series developed here. How do they compare? Likewise, the authors should compare and contrast their findings to those of Solomina et al. 2007 in Climate of the Past. P2162L24-25: What does this sentence mean ('The annual minimum ...')? It also seems in contrast to the paragraph above, where a relationship between dD is established.

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