Reply to reviewer 1 (Olga Solomina)

In this paper the annual resolution reconstruction of accumulation in Ushkovsky ice core (Kamchatka) is extended back in time up to AD1735. The reconstruction of accumulation (mass balance) is based on the stable isotope stratigraphy and the two ice flow models. The correlations of the $\delta 18O$ and δD with local meteorological data is found to be generally weak, though the δD correlates with the North Pacific surface temperature and NP Gyre Oscillation. The reconstructed accumulation variations agree well with other proxies (e.g. age of moraines) and the recorded in 1970s peak of Very important new empirical data and some interpretation of these results are provided in this publication.

At the moment the discussion is virtually absent in this paper, but in my opinion it would be important to include it and discuss more deeply the following questions:

1. The results have to be discussed in the context of previous findings. For instance it was reported that the 11 years running mean net accumulation in Ushkovsky correlates with the local Kliuchy station winter (r=0.75) and hydrological year precipitation (r=0.69). It was also found that the accumulation in Ushkovsky ice core correlates with the PDO (Shiraiwa et al., 2001). Are the shorter accumulation time series reported previously in Shiraiwa et al., 2001 the same as here or they are somehow modified? If they are the same, why the correlations established before were not found this time?

Accumulation reconstruction only changes the trend of the records. Therefore, correlation rate results are the same as before. Shiraiwa and Yamaguchi (2002) used the Salamatin model even though it considers only the vertical transfer. The result is essentially the same, particularly for the shallower part of the ice core, since thinning rates do not change significantly. However, there is a slight difference in the deepest part. We did not try the same analysis in this paper since it had already been conducted by Solomina et al. (2007). We will refer to the paper and discuss the results considering the previous study as suggested by the other reviewer.

2. Please provide your assessment - which of the model you used is more reliable and why?

One of the advantages of this paper over other accumulation reconstruction studies is that it uses firn dynamics within thermomechanically coupled flow models for the Gorshkov crater glacier, namely the Salamatin model (Salamatin et al., 2000) and Elmer/Ice (Zwinger et al., 2007), and we carried out the accumulation reconstruction considering its flow.

The results explicitly show the uncertainties of such a reconstruction. It is hardly possible to decide which models are better based on current knowledge. The consensus of the firn dynamics is not sufficient. Two different firn dynamics were applied: Salamatin model followed Salamatin and Duval (1997), and Elmer/Ice followed Gagliardini and Meyssonnier (1997). Elmer/Ice is a three-dimensional, compressive, full Stokes thermomechanically coupled model in which surface mass balance effect is not considered though basal effects are considered. The Salamatin model (Salamatin et al., 2000) is an analytical solution based on a 2D, compressible, higher-order flow line model, which includes longitudinal flow tube width, thermodynamics, and surface/basal mass balance effects. We will add some more description of the models and discuss the reconstruction results.

3. It would be important to add some discussion on the potential mechanisms governing the decadal variability identified in the records and the teleconnections. The authors used for their analysis the records from Kliuchi met.station for the period 1961-1989. The Kliuchy met station was open in 1920s

and as far as I know it still operates since that time (see Solomina et al., 2007 published in CP), so it makes sense to take a longer records for this comparison.

We have long temperature and precipitation records of Kljuchi station from 1908. However, there are some missing values, particularly in the precipitation rates. Continuous records of both precipitation and temperature rates are needed to estimate the precise precipitation weighted mean temperature.

It is also possible to reconstruct a mass balance index basing on this meteorological data and compare it with the accumulation (in fact - mass balance) retrieved from the ice core records for almost entire 20th century. It is also possible to compare the accumulation records of Ushkovsky ice core with the mass balance reconstructions of Kozelsky glacier which is available for a century long period (Vinogradov, Muraviev, 1992). In Solomina et al., 2007 it was shown that the accumulation of Ushkovsky and the tree-ring summer temperature reconstruction (11-years running mean) are anti-correlated in 20th century, but have a positive correlation in 1830s-1880s. It would be interesting to extend the comparison back in time, while the new longer accumulation time series is available.

We checked that the Ushkovsky accumulation records showed the same phase shifts as the regional tree ring chronology ("KAML") in 1730–1830s as in 1830–1880s found by Solomina et al. (2007). The mass balance phase of Kozelsky glacier is generally the same phase as the Ushkovsky ice core. We will add further discussion on the comparison between accumulation reconstruction and the other glaciological records in this region, and tree ring records as per your and the other reviewers' suggestions.

I would also recomend to add some additional details on the dating of the ice core records based on known eruptions from Muraviev et al., 2007 (I guess the paper is published in Russian and therefore it is hardly available for the international public).

Muraviev et al. (2007) is published in English and available at http://link.springer.com/journal/11711. According to Fig. 7 of Muraviev et al. (2007), four ash layers were identified based on chemical analysis of the ashes. They were also identified by mineral compositions and stratigraphic features (Ovsyannikov et al., 2001); from the oldest to the newest: Klyuchevskoi 1737, 1829, 1945, Bezymyanni 1956.

The tephrochronological interpretation is partly supported by Shiraiwa et al. (2001), who supported the dating of the four ash horizons by analytical calculations based on Salamatin et al. (2000). Shiraiwa and Yamaguchi (2000) confirmed the ages of Klyuchevskoi (1737, 1829) and Bezymyanni (1956) by annual counting of water isotope variations extracted from the same ice core. Therefore, we are confident that the four ash layers are reasonably dated to meet the purpose of this paper.

In my opinion the paper is scientifically sound and suitable for the publication in CP, but some extension of the interpretation of the results would be desirable.

Thank you very much for the positive conclusion. We will add more discussion about the accumulation reconstruction, comparison to local glaciological records, and paleoclimate proxies in each sections. In addition, we will change the structure of the paper such that previous sections 3 ("Results"), 4 ("Comparison with local climate data"), 5 ("Comparison with large-scale climate data") and 6 ("Wavelet analysis of δD and accumulation rates") will appear as subsections 3.1 ("Isotope (δD) record and accumulation-rate reconstruction"), 3.2 ("Wavelet analysis"), 3.3 ("Comparison with local climate and glaciation data") and 3.4 ("Comparison with large-scale climate data") of the new section 3 ("Results and discussion"). Former section 7 ("Conclusions") will be new section 4 ("Summary and conclusions").

Reply to reviewer 2 (anonymous)

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.

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.

Thank you for the encouragement and the many thoughtful suggestions to improve the paper.

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.

We will add more discussion about the accumulation reconstruction, the comparison to local glaciological records with accumulation rates, paleoclimate proxies and recent climatology papers to interpret the ice core data as climate signal. It will be added in the several sections, considering previous studies.

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.

- Based on the 'N-issue' discussed above, it is difficult to make conclusions about multidecadal 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.

This ice core does not have millennial scale records. However, there are some possibilities to discuss multidecadal oscillations. We will add the global wavelet spectrum and COI. The significance of the multidecadal signals (30–60 years) is checked by the global wavelet result.

COI only excludes the duration where frequency is lower than that value. We discuss the changes of frequency in the valid duration. We have applied other mother functions, which have low COI effects (e.g., Torrence and Compo, 1998) and checked that the change of the signal around 1900 for isotope and 1850 for reconstructed accumulation rates are common for different mother function. We choose the Morlet mother function because of its strength; it is good at the frequency resolution, it is possible to divide decadal and multidecadal signals and can resolve each peak in the global wavelet, and COI does not disturb this result. Although this result shows changes of dominant periods, it does not deny the multidecadal oscillation itself. There could be multidecadal signals in the 20th century as you suggested, but that might not be strong enough to find significance as before.

On the other hand, it is true that the long paleoclimate records are preferred to analyze multidecadal signals. We will mention the importance of further analysis explicitly in the discussion and conclusion parts of the revised manuscript.

- 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.

We will correct it as suggested.

- 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 characterize the climate sensitivity of the proxy records. Correlations based on shorter periods are more susceptible to sampling error and false-positive results.

We have the original data of monthly precipitation and temperature records of Kljuchi's weather station. They are older than what is stored in the Global Historical Climatology Network. Although they include continuous winter precipitation and temperature records (Solomina et al., 2007), some values are missing for late spring precipitation. This is a problem because both monthly temperature and precipitation records are needed continuously to estimate the precipitation-weighted temperature. We must therefore exclude the years with missing data and limit our analysis to the longest available continuous records.

- 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.

The NPGO index is defined by sea surface height anomaly (SSHa) and is defined only after the 1950s, which may be due to a lack of observation. Therefore, it is not possible to compare longer time records as suggested by you. One future possibility is the reconstruction of past SSHa. However, observational information is limited. The comparison of the ice core record (δD) with the continental SST record is not the same as that for the period after the 1950s (see the answer for reviewer 3). We will mention your comment, describe the changes of relationships and the future possibilities in 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.).

The cause of isotope ratio changes in this ice core would not be similar to the Logan site as suggested by you. It increases mean value in the termination of Little Ice Age to 20th century, like the south central Russian, Belukha ice core in the Altai Mountains (Henderson et al., 2007). It reflects warming from the LIA. We will cite the papers as per your suggestion in the revised manuscript, and described that the general trend and its mechanism is not the same as the Yukon region ice core.

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.

The standard deviation of annual mean d-excess (d-excess = δD – 8 δ18O) in this ice core is small (1.3‰) for all the periods. Considering the measurement errors of deuterium and oxygen isotopes, it is difficult to discuss something about it. However, this is also a result. We will explicitly add a remark on the small variance of d-excess, compared to the measurement errors.

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…".

We will correct it as suggested.

P2155/L16 – Delete "shows that the record"

We will correct it as suggested.

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.

We will correct it as suggested.

P2156 /L2 – …with a maximum depth of ~240 m…

We will correct it as suggested.

P2156 /L11 – …preserved in the core…

We will correct it as suggested.

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."

We will correct it as suggested.

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

We will correct it as suggested.

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

We will correct it as suggested.

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

We will add both 1 and 2 sigma precisions as suggested.

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.

The 2 sigma value for the measurements is 0.06‰ for Oxygen isotope and 0.3 % for δD. We will add the 2 sigma values as suggested.

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

It means secondary water standards. We will add this.

P2161/L18 – The average δD value…

We will correct it as suggested.

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."

We will correct it as suggested.

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?

This was a mistake, and we will correct it in the revised version.

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

It uses records data from 1958 to 1996 from ERA40; 1957 records only have three months, three year mean is used (1958 to 1996, 39 years). We will describe it.

P2163/L15 – Insert the following, "For δD, there is a significant…"

We will correct it as suggested.

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.

We will correct it and add the latitudes for the δD correlations.

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

We will correct it as suggested.

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

The value is 0.30 ($p < 0.1$) for 1950–1997, but 0.25 for 1979–1997 (n.s.).

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."

We will correct it as suggested.

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

The linear trend is removed from the ice core records as the preprocessing for other frequency analyses like FFT (Emery and Thomson, 2001). If it is not removed, there are strong spectra in the 100-year period, which would be caused by the changes of average value from the late 19th to the early 20th century.

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.

We will correct it as suggested.

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.).
We also considered that the cause of the change could be different. We will describe it as suggested.
P2169/L3-5 – The journal abbreviation is incorrect. The full journal name is Palaeogeography, Palaeoclimatology, Palaeoecology. The abbreviated title should be Palaeogeogr. Palaeoclimatol. Palaeoecol.
We will correct it as suggested.
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.
We will correct this part as suggested.
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.
We will add another figure of ice core sites around the high-latitude North Pacific.
Figs. 8a and 8b – Plot the 'Cone of Influence' (COI) on your wavelet diagrams (refer to Torrence and Compo, 1998).
We will add the cones of influence and also the global wavelet spectra to the plots.

Reply to reviewer 3 (anonymous)

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 δD record that previously extended back to 1823. The current δD 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 δD/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 δD. 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?

Water isotope ratio was considered to correlate with surface temperature and weakly anticorrelate with the source surface temperature. We considered that there is some association between them. This result of the comparison with the surface temperature refuses these possibilities and suggests the importance of large-scale Pacific climate conditions. As your suggestion, one of the possible implications is that it is the relationship between local temperature and the Pacific climate field.

We will also add a comparison with the sea surface temperature record ERSST v3b (Smith et al., 2008) from 1854 to 1997 in the revised manuscript. We have checked that the correlation patterns of SST and ice core records are consistent for the same interval with ERA40 surface temperature, 1958 - 1997. However, it shows different patterns if we consider the longer period 1854 - 1997. It shows the importance of the Kuroshio-Oyashio-Extension region, the California current and a part of the sub-polar Alaska current, like the first leading mode of EOF of SST, PDO. This implies a change of the relationship over time. We will add the corresponding figures plus some discussion.

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?

This was a mistake. Five-year running mean is not used for the comparison. Only the three-year mean is used. We have re-analyzed the correlation map with regard to the effect of the autocorrelation (Mitchell et al., 1966; Trenberth, 1984).

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 δD 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.

As per your comment, the NPGO index is defined by sea surface height anomaly of the east coast of the North Pacific. However, the California current is an element of the North Pacific Gyre, with Kuroshio and its extension (Kuroshio-Oyashio-Extension) and the North Equatorial Current. NPGO has the relationship between the second mode of EOF of SSHa, SSTa over the North Pacific region and SLPa, atmospheric conditions (Ceballos et al., 2009, Furtado et al., 2012, Di Lorenzo et al., 2012). We will add discussion on possible causes with previous comments reply.

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 ~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.

We will add the COIs to the wavelet analysis figures, and also add global wavelet spectra for the chosen mother function. Although the longer record is preferable as the other reviewers suggestion, the result is consistent with previous analysis.

Also, at least in the δD 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?

The change of the analysis, 110 m (1810s) is 40 years before the changing period of accumulation rates (1850s) and 70 years before that of water isotope (1880s). The sampling interval is significantly smaller than mean annual sample between two volcanic ashes (P2161, L12-13). It is not possible to change the annual layer thickness or decadal scale periods by mixing. Further, measurement errors are more than five to ten times smaller than the standard deviation of water isotope, 18.4‰. It also does not disturb dating.

It is hardly possible to change strain rates between them since both upper part and deeper part are dated by volcanic ashes. Moreover, foldings or disturbances of the ice core (like GRIP or NEEM) are not shown at the 140 m depth, though there is shear strain effects at more deeper part, below 180 m depth, suggested by inclinations of volcanic ashes (Shiraiwa et al., 2001). We will add the description about these things for readers in the revised manuscript.

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.

We have checked the Asian PDO expression reconstructed by D'Arrigo and Wilson (2006). There is an anti-phase relationship between Ushkovsky accumulation rates and Asian PDO expression. It is consistent with the anti-phase relation between accumulation rates and PDO index, as suggested by Shiraiwa and Yamaguchi (2002). We will add the comparison.

P2162L24-25: What does this sentence mean ('The annual minimum ...')? It also seems in contrast to the paragraph above, where a relationship between δD is established.

This was a mistake. We will correct it.