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# *Interactive comment on* "Climatic variability in Princess Elizabeth Land (East Antarctica) over the last 350 years" *by* Alexey A. Ekaykin et al.

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The review by Dmitry Divine is in general positive, but contains two major comments.

The first major comment refers to the fact that when we deal with the smoothed series, the number of degrees of freedom should be reduced, which leads to less significant correlation and regression coefficients. This is also clearly demonstrated in the short comment by Thomas Laepple, who showed that two rows consisted of 52 random values and smoothed with a 27-year filter may occasionally exhibit high correlation coefficients (see also my reply to his comment). I agree with this remark, and will change the text accordingly. In particular, I will discuss the isotopic composition of precipitation as a parameter that "covaries with atmospheric circulation in a manner similar to temperature" (following Eric Steig) rather than simply a proxy of air temperature.



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Discussion paper

The corresponding changes will be made in Introduction. On the other hand, the link between the isotopic composition of precipitation and air temperature has strong and clear physical basis, which gives additional prove to the observed covariation (though statistically insignificant) between these two parameters in the studied region. Rather poor correlation between them may be explained by, first, "stratigraphic noise" in the ice core data and, second, by some climatological factors that may disturb the isotope-temperature relationship (see major comment 2). Thus, in the revised manuscript I will keep discussion of the quantitative temperature changes in the past in Section 3.3 and Conclusion, but with revised uncertainties. In particular, new estimate of the overall warming during the past 350 year is 1$\pm$0.6 °C instead of 1$\pm$0.2 °C.

The second major comment concerns the precipitation type, seasonality and the origin of moisture in the coastal and inland regions of the studied area. These factors may affect the observed relationship between snow/ice isotopic composition and air temperature, and thus should be discussed. Indeed, the coastal and inland areas of Antarctica are different in terms of precipitation regime: coastal sites receive relatively more moisture from high latitudes of Southern Ocean, and most of precipitation is snow from clouds, while inland sites receive relatively more moisture from lower latitudes, and much (or even most) of precipitation is "diamond dust" from clear sky. This may lead to biases in the isotope-temperature relationship, when the observed changes in the snow isotopic composition may be caused not only by the changes in local air temperature, but also, e.g., by changing conditions in the moisture source, or by changes in precipitation seasonality. Although these effects are widely recognized, they are usually not taken into account when interpreting the ice core data, simply because we do not know much about past changes in precipitation origin, type or seasonality. In our case, my opinion is that although the mentioned factors may play a role in the observed discrepancy between ice core record and instrumental temperature record in the PEL region, the influence of these factors is much less then the influence of the "stratigraphic noise". This can be clearly demonstrated by considering ice core records obtained in a short distance one from another (Ekaykin et al., 2014): even in this case we still have relatively low correlation between individual ice core records and relatively low correlation of the stacked ice core series with the instrumental temperature record. I will add the corresponding discussion to the Section 3.3 of the manuscript.

Other comments

"Pages 2-3, Section 2.1: Q. on ice core dating. Did the authors use, wherever possible, counting the seasonal peaks in d18O to establish and/or support their core chronologies?" The counting of the seasonal peaks was only possibly for the "105 km" ice core, where it was used as the basis of the dating. In other records the seasonal signal is not preserved. I will change the text in order to make it clearer.

"Page 3, Line 16: The age uncertainty associated with the Nye model alone can also be estimated directly from the Nye formula, please see Divine et al., 2011 (Polar Research, 30, 7379, DOI: 10.3402/polar.v30i0.7379, on page 3) for details." Thank you for this comment, I will use this approach for independent estimate of the age uncertainty. In our case the main source of the uncertainty is the error of the accumulation rate, which gives the age uncertainty <10%. This figure confirms our estimate.

"Page 3, Line 29: "...to cut off the variability with periodicities lower than 27 years...". Use "shorter" rather than "lower". Please provide some more detail on the filtering procedure you have actually used." I used a rectangular-shaped filter that cut-off all the frequencies > 0.037 (i.e., periods < 27 years). I will add the corresponding information to the text.

"Page 5 Line 26. High correlation coefficient reported for AWS LGB59, is it based on 5 annual values only or the authors used the monthly means for this particular case? If the latter is correct did the authors subtract the annual cycle from the data?" Yes, the correlation between LGB59 with Vostok and Mirny is 0.95 and 0.96, but is only based on 5-year record. Although it is statistically significant with a 0,05 confidence level, I realize that the conclusion made on 5-year series does not look very solid. But I included this in the manuscript, since this information is supplementary (not main) evidence that the climatic variability is uniform within the whole studied sector. Indeed, we have already demonstrated that climatic record at Vostok correlates with those at Mirny and Davis, so we may expect a high correlation between a point located in the middle of the sector with the mentioned sites.

"Page 5 Line 28: Just a comment: principal component analysis commonly used in climate sciences, could be considered a reasonable alternative to a cluster analysis" We agree that PC analysis could be used as well, but in this case we prefer to use the cluster analysis as it gives the result in a simple and intuitively understandable way.

"Page 8 Lines 3-5: since the presented slope estimate is based on the low-pass filtered series, a decreased number of DOF needs be taken into account. The STD on the estimated slope is presently underestimated and should be corrected; some more details on the method the uncertainty of the slope was calculated should be provided too." In our case, it was not possible to derive the isotope-temperature slope directly from the regression of the PEL2016 stacked series with the instrumental temperature record, since PEL2016 consists of normalized values. Thus, to calculate the isotope-temperature slope we used well-known relationship: slope (y,x) = r(y,x) * std(y)/std(x). where std(x) is the STD of temperature record, and std(y) is the mean STD of individual isotope records As an estimate of the uncertainty of the slope, we used the uncertainty of the mean STD value of individual isotopic records (as indicated in Page 8, Line 3). But this estimate does not take into account the uncertainty of the correlation coefficient. So, the revised value of the isotope temperature slope will be $9 \pm 6$ ‰°C.

"Page 9 Line 23: "…the IOD is expected to affect the inland Antarctic climate…" can the authors provide any relevant reference pointing to a link between IOD and cyclonic activity in the coastal Antarctica?" The heat and moisture is brought to Antarctica by cyclones, this is why we suggested that the correlation between isotopic content of precipitation and IOD could be due to modulation of cyclonic activity by IOD mode. But so far we could not find a proof of it in literature (which does not necessarily means that our supposition is wrong), this is why we used air pressure at the coastal stations as a rough proxy of cyclonic activity.

"Page 10, Line 4: A similar divergence in the longer term trends in d18O and accumulation was also observed for the coastal DML (see Divine et al., 2009, JGR,114, D11112, doi:10.1029/2008JD010475 ) but not on the plateau where both d18O and SMB showed positive trends (Altnau et al., 2015)." I will include this into discussion.

"Figure 5: please use different colors for 5b. The lines are difficult to discriminate with the presently used color palette. Correct the uncertainty interval on the reconstruction by adjusting for the number of DOFs." I will change the figure accordingly.

I agree with the other comments and will correct the text accordingly.

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