

## Review of a manuscript for *Climate of the Past*

**Climatic variability in Princess Elizabeth Land (East Antarctica) over the last 350 years** by Ekaykin et al.

### Overall:

In this manuscript the authors present an ice core based reconstruction of 350 years of temperature variability in Princess Elisabeth Land, Antarctica. The reconstruction is derived using scaling of the smoothed  $\delta D$  stack of 6 ice cores from the plateau to the stacked instrumental annual mean temperature series from Vostok, Mirny and Davis stations. The authors present evidence of a persistent positive trend in the reconstructed regional temperatures observed over the length of the stack together with the pronounced variations on bidecadal and multidecadal timescales. This work is a contribution to Antarctic Pages 2K initiative aimed at reconstructing and understanding regional climate variability over the last 2000 years.

In general the paper is clearly written and results are well presented; it fits the scope of the special issue very well and deserves to be published after some modifications to the content have been made. I have some relatively moderate methodological comments that the authors are encouraged to address/answer before the paper can be accepted.

As a non-native English speaker I can not comment much on the language quality. However, my impression was that some style improvement/language check by a native speaker would be highly desirable to improve the manuscript readability and eliminate some language flaws.

### Major comment

- 1) My first major comment concerns the method the authors used to estimate the isotope to temperature gradient and its STD on the smoothed data. More specifically, it is not demonstrated that a reduced number of degrees of freedom (DOF) in the data due to smoothing is taken into account. The same applies to significance of the correlation coefficients reported for the smoothed series. For a 27-year low pass filtered instrumental series of a length of about 60 years one have to expect about 5 independent data points only, implying that a simple sample variance (or STD) of the slope presented in the manuscript is a biased estimator of an underestimated true variance. For a very simplified case of AR(1) model of serial correlation in the data, taking the effect of autocorrelation into account to estimate the confidence intervals (CI) on the slope estimate was summarized in Nychka et al., 2000 (available from <http://citeseerx.ist.psu.edu/viewdoc/download;jsessionid=A30C325B3A1E36EAB30B126EF74F974E?doi=10.1.1.33.6828&rep=rep1&type=pdf> ). To reassess the significance of correlation coefficients simple adjustment for a number of independent samples in the t-distribution quantile can be applied as a simplistic remedy of the problem.
- 2) Some discussion on precipitation types/seasonality, and moisture origin that can be different for the coastal and inland locations in the study area would be highly relevant in the context of the observed discrepancies between the core series and the instrumental data.

### Other comments

Page 1 last line: “the only source of climatic data”. Please use “primary” instead; there are alternative though sparse sources of instrumental data such as earlier expeditions to Antarctica, observations from ships logbooks etc.

Page 2 Line 5: “...moreover unevenly distributed...”, “.reflecting heterogeneous efforts...”, “still remain white spots”. Awkward sentences, please check the language.

Page 2, Line 15: “Classically” can be omitted.

Page 2, Line 29: “...down to a 150 m depth...”

Page 2: “Individual records” can be modified to “ice core data”

Pages 2-3, Section 2.1: Q. on ice core dating. Did the authors use, wherever possible, counting the seasonal peaks in  $\delta^{18}O$  to establish and/or support their core chronologies?

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.

Page 3, Line 27: “...values were reduced in terms of mean and STD...”. Awkward sentence, better to refer to the procedure as a “mean and variance adjustment” or a “variance scaling” (see e.g. Esper et al., 2005, GRL 32, L07711, doi: 10.1029/2004GL021236).

Page 3, Line 27: “...to avoid an artificial dominating...”, please check the language.

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.

Page 4, Line 2: “...due to a very low SNR...” ...and non-temperature effects on isotopes in precipitation including post-depositional alterations.

Page 4, Line 4: “...despite (some) common features...”

Page 4, Line 8: “...observed discrepancies do not arise from chronological uncertainties alone...”

Page 4, Line 9: “...significant level of noise event in the filtered series”. ...and other than the ambient temperature -related controls on the isotopic composition of precipitation.

Page 4 Section 2.3. Subsection title can be changed to “Instrumental temperature data”

Page 4 Line 17. “The data are available from...”. Please mention explicitly that the annual means were constructed from the monthly means.

Page 5 Line 2: “...considered as a prevailing mode of atmospheric circulation in the SH representing about 35% of the extratropical SH climate variability”.

Page 5 Line 2: “The monthly AAO index is available from...”

Page 5 Line 22: “...to assess whether uniform climate variability pattern is monitored...”. Awkward sentence, consider revision.

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?

Page 5 Line 27: “...that the region encompasses between these 3 stations...”. Please check the language and consider revision.

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.

Page 6 Line 14: “...have a 30-year periodicity...”. Due to a shortness of the data being analyzed, referring to a “quasi-periodic variability” would be more appropriate. Mind also the edge effects of any filtering procedure that in the zone of influence equal to a filter length at a specified timescale.

Page 7 Line 5: “...reflects a larger pressure gradient...”

Page 7 Line 15: please see my major comment 1.

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.

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?

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).

Page 10, Line 27: "...suggested to modulate..."

Page 11, Line 8: please provide STD on the estimated slope.

Page 11, Line28: "field technicians" or "field engineers" would be a more appropriate term.

Page 12, Line 1. "...in the framework...", please indicate what abbreviation "LIA" stands for.

## Figures

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.