Author Reply to the Review Comments by Dmitry Divine (Referee #1)

on the manuscript

cp-2018-112: What climate signal is contained in decadal to centennial scale isotope variations from Antarctic ice cores?
by Thomas Münch and Thomas Laepple

Thank you very much, Dmitry Divine, for the time you spent on reading and reviewing our manuscript; we are happy about the positive outcome. Below we include a point-by point response to both the general and to all specific comments. The original referee comments are set in normal font, our answers (author comment, AC) are typeset in *green italic font*.

Overall:

In this manuscript the authors present a method for calculating a timescale dependent SNR for an array of climate proxy records with a common climatic signal and a physical mechanism(s) behind.

For a particular case presented, the study uses ice cores based isotopic records and accounts for the effects of stratigraphic noise, diffusion in firn and timescale uncertainties elaborating the respective power spectral densities for the background climate signal and the aforementioned contributing noise factors. The proposed technique is then applied to ice core arrays from DML and WAIS. Opposite timescale behavior of SNR for the two core networks is linked to the homogeneity/heterogeneity of distillation trajectories between the two regions associated, for example, with the effects of sea ice on isotopes in precipitation.

In general the paper is clearly written and results are well presented. Moreover, my general impression over this study, is that this was one of the rare cases I had so far as a reviewer that can be published almost "as is". When reading the manuscript, I left a number of remarks/suggestions/question marks that I planned to list later when writing this review, yet it turned out in the end that almost all of them the authors have already addressed in Discussion and Conclusions.

This study is certainly recommended for those who deals with multiproxy archives – this is an explicit demonstration of a value of a single proxy (ice core) record and a clear warning against overinterpreting single spikes/events on the shorter timescales. Therefore, I consider the manuscript deserves to be published after some very minor modifications to the content if the authors/editor finds them relevant.

AC:
Thank you very much for this positive evaluation of our work. We are happy that you find our study relevant, well presented and deserving publication.

Minor Comments:

Page 2, line 5: "…to a first approximation, changes in isotopic composition are only recorded in the ice if there is snowfall." Recent studies suggest the effects of air (and hence water vapor) exchange across the firn –air interface in between the precipitation events may have a larger impact on the final d18O in snow than previously thought, see for example Stenni et al., 2016 , 10.5194/tc-10-2415-2016. It actually increases the role of SAT variability throughout the accumulation season even given the intermittency of precipitation itself.

AC:
We agree that there are indications that vapour exchange processes possibly further affect the isotopic composition of the surface snow and, if connected to SAT variations, could actually increase (again) the role of SAT variability for the isotope variability. Nevertheless, we could show for the Kohnen Station region in DML that, even after averaging away the local stratigraphic noise, the near-surface isotope variability is still very discrepant from the local interannual SAT varibility (Münch et al. 2017). This suggests either precipitation intermittency to be a main driver of the remaining (after reducing the

stratigraphic noise) isotope variability, or isotope modifications occurring directly at the surface which are not controlled by SAT variations.

In order to briefly reflect this discussion in the manuscript, we suggest to change the respective sentence to: "To a first approximation, changes in isotopic composition are only recorded in the ice if there is snowfall, while the role of water vapour exchange processes in between precipitation events is still debated (Steen-Larsen et al., 2011; Stenni et al., 2016; Casado et al., 2018; Ritter et al, 2016; Münch et al, 2017)."

Page 6, line 4: Please provide a ref to eq. (7).

AC:
The expression directly follows from the definition of the correlation coefficient as shown in Fisher et al. (1985). We will add this reference to the paper here.

Page 6, line 9: "… for display purposes… smoothed using a Gaussian kernel". Still the motivation is not clear, would it be possible to see an unsmoothed signal (in the letter of response for example). What is the kernel bandwidth used?

AC:
We agree that the formulation in the respective sentence was unclear. We do not apply the smoothing only for display purposes. In general, a strong smoothing is necessary in order to improve the quality of the spectral estimates. If one assumes that the spectrum of the climate signal (which one aims to reconstruct with proxy data) can be described by piecewise power laws, smoothing in logarithmic space is reasonable, which we have adopted here. This logarithmic smoothing is applied by taking weighted averages of spectral power over a Gaussian smoothing window with a scale factor in log units (Kirchner et al., 2005) which we choose to be 0.1 for the WAIS data and 0.15 for the DML data. The scaling in log units is proportional to the frequency at which the smoothing is applied, thus, at the higher frequencies more data points are averaged resulting in a stronger smoothing. We will add a more thorough motivation for the logarithmic smoothing at this point of the paper.

Below you see an unsmoothed version of paper figure 1 where you can observe the strong spectral uncertainty when no smoothing is applied.

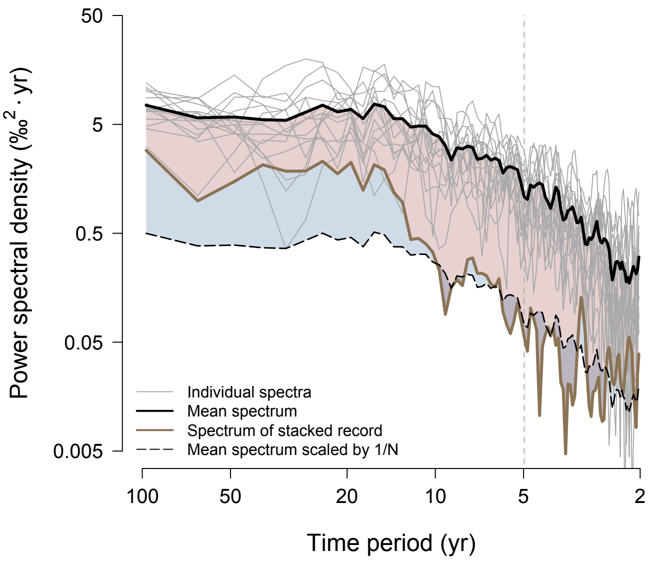

Figure 1: *As Fig. 1 from the paper but without logarithmic smoothing applied to the spectral estimates.*

Page 11, lines 10-20. Quality of ERA precipitation needs to be briefly discussed. How reliable are the estimates based on this variable?

AC:
We agree with the reviewer that the quality of the used ERA reanalysis data should be discussed but we think that the appropriate place for this would be the beginning of Appendix C. However, since our results do not critically depend on the accuracy of the estimated decorrelation scales, we suggest to only include a short and general discussion of the ERA-Interim quality of temperature and precipitation in Antarctica at the beginning of Appendix C.

Page 14, lines 30-33. The effects of sea ice on the modelled isotopic composition of precipitation in Antarctica can be found in the studies by Noone , e.g. Noone, D., and I. Simmonds (2004), Sea ice control of water isotope transport to Antarctica and implications for ice core interpretation, J. Geophys. Res., 109, D07105, doi:10.1029/2003JD004228. The authors are recommended to see if these results can be used to elaborate more on the potential controls of the different patterns in SNR found between the two study regions.

AC:
Thank you very much for pointing us at this work. We will include a reference to Noone and Simmonds and a short discussion of their findings at this point of the manuscript in order to improve our discussion of the role of sea ice changes for the isotopic composition of precipitation.

Page 19, lines 3-5. The authors present the winter and summer precipitation results. It is highly recommended to do the same analysis for the fall and spring seasons. The semi-annual oscillation (SAO) tends to modulate the seasonal distribution of precipitation depending on the strength of the semiannual harmonic. In addition, for West Antarctica (though shown for Faraday only in Broeke et al., 2000, part 4) the sea ice extent in the Amundsen and Bellingshausen seas (also linked with SAO strength) was shown to modulate the seasonal precipitation too. One can speculate that a long term variability in the strength/position of the low in contraction phase of the SAO (March and September) can actually be one of the mechanisms responsible for disruption of the coherence between the isotopic records on the longer timescales.

See in the series of earlier publications by Van den Broeke.

AC:
Thank you very much for pointing us at these interesting results. We extended the decorrelation analysis of the ERA-Interim precipitation data for the fall and spring seasons. This, however, does not change the results: also for the distribution of spring or fall precipitation (e.g., spring minus fall or spring/total precipitation, etc.) the decorrelation scales lie roughly between 300 and 500 km for both regions. Thus, whatever season drives the precipitation intermittency, the typical spatial scale of the intermittency should be of the order of 300-500 km, and thus a factor of 3-4 below that of the temperature fields.

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<u>References to the author comments:</u>

Casado, M., et al., The Cryosphere, 12, 1745–1766, https://doi.org/10.5194/tc-12-1745-2018, 2018.

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Steen-Larsen, H. C., et al., J. Geophys. Res., 116, D06108, https://doi.org/10.1029/2010JD014311, 2011.

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