Author Reply to the Review Comments by an anonymous reviewer (Referee #2)

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

We appreciate a lot the very thorough and detailed reading and reviewing of our mansucript by the anonymous reviewer. The many comments and suggestions will be of great help to improve our first version of the paper. Below we include a point-by point response to both the general and to all specific as well as technical comments raised by the referee. The original referee comments are set in normal font, our answers (author comment, AC) are typeset in *green italic font*.

General comments:

The article submitted deals with quantification of climate signal versus noise in ice cores from Antarctica. It is therefore well within the scope of Climate of the Past, and addresses an important issue for climatologists. Its aim is not to present new ice core data, but to present a methodology to evaluate (quantify) the climate signal contained in a series of records.

The methodology is based on a spectral analysis of the dataset, where the spectrum of the stacked record is compared to the mean spectrum and to white noise. The method also includes a correction for diffusion and for time uncertainty. The Methodology section is concise, because details are described in Appendixes. The paragraph 3.1 in Results is a useful complement to the methodology section, as it applies the method to an example, and provides a figure where the various steps are represented. It is well suited to an article that aims a large audience, not necessarily with statistical background, and who might overlook the equations in the methodology section and Appendixes.

AC:
We are happy that the structure of the manuscript and the steps at which we present the results are positively evaluated. Since not every reader of CP might be familiar with the details of the applied spectral analyses, it is indeed our intention to still present our results in a way that is understandable to a broader audience, as we envision our approach to be applicable in many fields of paleoclimatology/proxy research.

In the continuation of the Results section, the figures are described in less details. Some more precision is needed here, so that the main message is not obscured by unanswered questions on the parts of the figures that are not described. The results are different for the two studied regions. At EDML, the signal to noise ratio is found to increase for longer time scales (0.2 to 1), whereas at WAIS, it is relatively stable, and even seems to decrease at long (centennial) time scales. For the first region, the authors therefore recommend to use single cores only for multidecadal or longer timescales. For the second region, oppositely, they conclude that single cores yield good regional information at interannual and decadal scales, but give a more local information at longer time scales.

AC:
Thank you for these comments. Also in the light of what was said above, we will go through the second part of the results section again to ensure that the results are described in adequate precision in order to be comprehensible for a broad audience; see also our answers to the detailed comments.

In the Discussion, the authors consider the possible contribution of four processes to the climate signal, by looking at their spatial scale of coherence. They note that precipitation intermittency acts as noise or contributes to signal, depending on the

scale considered.
They also discuss the unexpected decrease of signal power at WAIS for longer timescales. This coastal region is particularly sensitive to the variability in atmospheric circulation. They suggest that slow processes modifying the topography of the region may reduce the spatial coherence of the signal over long timescales.

The conclusions of the article are important regarding the confidence that we can attribute to one or several ice core records. The results for WAIS are unexpected, and therefore may trigger more research in the area, or allow to consider differently the results from previous drillings. Lastly, as noted by the authors, the new methodology can be applied to other records, and therefore be of interest to a large audience.

The manuscript is well written and well structured, with a good use of appendixes to procure relevant information. Given that the conclusions of the article are interesting, and well supported by the Method and Results sections, I recommend that this article should be accepted by Climate of the Past with minor corrections.

AC:
We appreciate this positive evaluation.

Specific comments:

P2, l12: 'the diffusion of vapor through the firn column smoothes the isotope variations, reducing the power spectral density at high frequencies'
The first part of the sentence is very clear but the second part is less straightforward. Is it possible to add an intermediary step? '...smoothes the isotope variations, with stronger impact at short distances and therefore on the high frequencies.'

AC:
We will add the additional information, as suggested, and split the sentence into two parts for the sake of clarity: "Finally, once the snow is deposited, the diffusion of vapour through the firn column smoothes the isotope variations. This has a larger impact on short distances in the firn and therefore reduces the power spectral density of the variations strongest at the high frequencies (ref), which substantially shapes the isotope variability (ref)."

P2, l. 24: 'Furthermore, knowing the timescale dependence, and thus the spectral shape of the noise, would allow the correction of the isotope-inferred variability estimates for the noise contribution.'
This sentence is complicated, it takes some time to find the subject. Is it possible to split it or reverse it?
'Furthermore, in order to correct the noise contribution to the isotope inferred variability estimates, it is necessary to know the noise distribution over various timescales, i.e. its spectral shape.'

AC:
We will change the respective sentence to: "Furthermore, in order to correct the isotope inferred variability estimates for the noise contribution, it is necessary to know the variance of the noise across timescales, i.e., its spectral shape."

P3, l14: 'The time uncertainty of the record chronologies, ..., has been reported with 2 % of the time interval to the nearest tie point.'
This sentence is unclear, is there something missing? Is it 'has been reported to be 2 % of the time interval'?

AC:
The sentence indeed was misleading and it should read "...reported to be 2% of the time interval". For the sake of clarity, we will correct this and also rearrange the sentence to: "The record chronologies were established from seasonal layer counting of chemical impurity records constrained by tie points from the dating of volcanic ash layers (Graf et al., 2002a); their uncertainty has been reported to be 2 % of the time interval to the nearest tie point (Graf et al., 2002a)."

P5, l8: Equations 4

Although it was only a few lines above, please repeat here what M and S are (respectively the mean of spectra, and the spectra of the stacked record along time). Also, is it possible to get another symbol for the noise (or at least a very specific font) so that it is easier to distinguish between N and N?

AC:

We will repeat the definition of M and S here, as suggested, and will change the symbol for the number of records from upper case 'N' to lower case 'n' throughout the manuscript. We also note here that in Eq. (2), the lower limit of the sum was erroneously given by "i=0" instead of the correct "i=1". This will be corrected.

P5, l. 19: 'we restrict our analyses to the frequency region where G<2. This avoids large uncertainties and yields cutoff frequencies of...'

Based on Figure B1 in annex, G is always below 2. Its maximum value is one. This seems logical since a filter will eliminate some frequencies, not add more frequencies to the signal. Should it be 'restrict to the region where G>0.5'? (so that the short frequencies will not triple in power after correction)?
Please clarify this point in the text or in the Appendix B.
Please define the term 'cut-off frequency' (and maybe draw the limits on Figure B1).

AC:

We apologize for the confusion. At this point of the manuscript we mixed up $\overline{G}$ and $\overline{G}^{-1}$ (the inverse of $\overline{G}$, which is actually applied as the correction function in Eqs. (4)). We will correct the sentence to "restrict to the region where $\overline{G} \geq 0.5$" and clarify in the appendix that this means a correction factor less than 2 in Eqs. (4). Additionally, we will define the phrase "cutoff frequencies" by paraphrasing the sentence as "This avoids large uncertainties and translates to a maximum frequency that is used for the analyses (hereafter: cutoff frequency) of... (Fig. B1)", and we will add the respective frequency limits as vertical lines in Fig. (B1a).

P6, l2: 'the value of F is related to the correlation between...'

Is the correlation r providing more information than F (on the quality of the data) or the same information? Is the r value only for intercomparing with other studies?

AC:

The correlation is mathematically equivalent to the value of $\overline{F}$; however, we think the correlation in addition provides a more direct and intuitive way of expressing the amount of variability contained in a stacked isotope record that is related to the common (climate) signal in relation to its total variability. In order to stress the equivalence of both quantities, we will rephrase the sentence to "The value of $\overline{F}$ is used to obtain the correlation between the time series of the common signal c and a "stacked" record $\overline{x}$ built from the spatial average of n individual records:".

P6, l20-28:

1. Is it possible to insert here the symbols used previously in the methodology section (M, S) to facilitate the identification of the various terms?

2. The mean over all individual spectra, M, (figure1, black line)...

3. The mean spectrum divided by the number of records (M/15, figure 1, dashed line)...

4. ...averaging across record that contain noise and additionally... (S..., Figure 1, brown line)

AC:

We will insert the respective symbols here, as suggested.

Furthermore, please precise ‘averaging across records along the time dimension’ or ‘in time space’ to refer to the ‘time stack’ S, by opposition to the spectrum mean M.

AC:
We will clarify the sentence as follows: "In comparison, averaging in the time domain across records that contain noise and additionally a common (i.e. spatially coherent) signal, ...".

P6, l29: ‘For short timescales (2 to seven years), ..., is consistent with the null hypothesis of independent noise.’
The curves are closest between 3 and 5 years; they diverge again around 2. What causes this divergence from white noise? Is it an artefact?

AC:
This divergence from the white noise level close to the 2-yr Nyquist period is probably indeed an artefact caused by noise added in the measurement process, i.e. from the measurement uncertainty and more likely from the "jitter error", thus uncertainty in the definition of annual depth increments upon dating which translates into uncertainty of the annual averages. We will add a respective remark to the manuscript here.

P7, l6-7: ‘Unlike the average spectrum across all individual isotope variations (fig1), the corrected DML1 signal spectrum shows an increase of power spectral density with increasing time scale (fig. 2a)’.

1. Please precise which line is described here (color, symbol).

AC:
We will add the respective descriptions.

2. Both the black and brown line show an increase of power spectral density towards higher timescales on Fig1 (strongest between 2 and 20, then flat area at longer timescales). This proposition has to be nuanced.

AC:
Please note that both these spectra in Fig1 have not been corrected for the loss of spectral power by diffusion, so most of the increase in spectral power between the 2 and 20 year period is attributable to the decreasing diffusional smoothing towards longer timescales. What we wanted to stress at this part of the manuscript is the difference in spectral shape between the mean spectrum (M, black line in Fig1) and the estimated signal (blue for DML1 in Fig2) for longer timescales, which stems from correcting the isotope variability for the noise contribution. We will rewrite the respective sentence to clarify this.

3. For the brown line, there is again an increase at very long timescales (>50) still on Figure 1.

AC:
This is indeed expected given that the estimated signal (blue line in Fig2), which is contained in the spectrum of the stack (see Eq. 3), increases in power towards the longer timescales.

4. On Figure 2, the 3 DML1 curves are very similar for t>20. So, the correction does not seem to have a huge effect. Moreover, the increase of power between 50 and 200 years exist with/ without correction...

AC:
It is indeed expected for the diffusion and time uncertainty corrections to have no effect for periods >20 years, given their estimated transfer functions (Fig. B1). What is still relevant to correct on the longer timescales is the residual amount of noise still contained in the spectrum of the stack (since we only average a finite number of records). This residual noise is subtracted when calculating (solving for) the signal spectrum in Eq. (4a).

5. Lastly, regarding the increase between 3 and 20 years on Figure 2, the correction actually reduces the slope...

AC:
This is true and already noted in the manuscript (p.7 ll.11-13).

Possible correction to this sentence:
'Unlike the average spectrum across all individual variations (M, black line on Fig 1), the corrected DML1 signal spectrum (C, solid blue line on Figure 2a) shows an increase of power spectral densities with increasing time scales at time scales larger than 50 years.'

AC:
We will add the respective descriptions, as suggested, and rewrite the sentence to clarify that the corrections include both the correction for residual noise as well as diffusion+time uncertainty, and that they lead to a more steady increase in signal power as compared to the mean spectrum. For further clarification, we will in addition change the legend of the dashed line in Fig. 1a from "Raw" to "Uncorrected signal".

P7, l. 7-8:'This is confirmed by the three 1000-yr long records from the DML2 data set whose signal exhibits a similar power spectrum in the range of timescales that overlap.' The two curves are not really a perfect match.
There is common behavior between 60 and 100 years (increasing) and between 5 and 15 years (increasing) but the middle part (15 to 60) is different between the two curves. DML1 is slowly decreasing (or flat), while DML2 is strongly decreasing until 30, then strongly increasing. Maybe add: '...for time scales longer than 50 years.'
Or split the paragraph in two, one dealing with long time scales, and the other with short time scales.

AC:
We acknowledge that the two curves are indeed not a perfect match, but only show a similar slope. We will weaken the statement accordingly.

P9, l.6: '...decrease in signal power on centennial timescales'
Is it possible that this decrease is an artefact? How certain are we of the power of the 100-year frequency on a 200-year record?

AC:
It is indeed possible that this decrease is an artefact from the spectral estimates, since log-smoothing uses less data points for lower frequencies and thus leads to higher spectral uncertainties there. We will add this remark to the manuscript here.

P11, l1: 'In fact, the average correlation of 0.87 (0.94) over distances up to the maximal intercore distance...'
Please add here a reference to Appendix C, Figure C1. Otherwise the origin of these numbers (and their meaning) is unknown.
Based on the figure caption, this correlation is an average (for various intercore distances). Thus, maybe the formulation 'up to the maximum intercore distance' is misleading here. Why not say the correlation for average distance between cores instead?

AC:
We apologize for the missing figure reference here. We will add the reference to Fig. C1, and include the average correlation values as symbols in this figure. In fact, the values are the average across all correlations for distances smaller than the maximum intercore distances, i.e. the average of all grey dots within the shaded regions in Fig. C1 (so not the correlation at the average core distance). We will rephrase the sentence to clarify this.

P12, l. 11-13; 'The raw noise spectra derived from the two DML data sets exhibit a clear imprint from the diffusional smoothing in the firn, as suggested by the smaller PSD for periods <20 years of the raw DML2 spectrum, i.e. prior to correction, in comparison to DML1 (fig. 2b).'

This is too fast. Please split in two steps:

1. first, raw to corrected for both records (DML1 and DML2): the effect of diffusion is to decrease power at high frequencies;

2. second, comparison of the raw data for DML1 and DML2: DML2 is more affected by diffusion, since frequencies between 12 and 30 are affected (low power) only for DML2.

AC:
We agree that the sentence condensed many pieces of information and might be hard to follow. We will thus split the sentence into several steps: "The raw noise spectra, i.e. prior to correction, derived from the two DML data sets exhibit a clear imprint from the diffusional smoothing in the firn. This is suggested by their common decrease in PSD towards shorter periods (Fig. 2b), since diffusion acts stronger on higher frequencies. It is corroborated by comparing the loss in PSD between the two data sets, which for DML2 is stronger towards the high-frequency end and also extends further towards longer periods. This is due to the stronger diffusional smoothing in the older sections of the cores that are only contained in the DML2 records, since the diffusion process had more time to act there since deposition of the snow."

P12, l30: 'yields a slope of the DML2 signal spectrum of roughly beta=0.6'
Is it possible to add this fit to Figure 2 or 3?

AC:
This would of course be possible, but in our oppinion it impairs the visual appearance of Fig. 2a which already contains many different line plots. Furthermore, sine the slope is here used only as a diagnostic means, we do not want to place special focus on its value. Therefore, we would rather refrain from adding the fit to Fig. 2a (neither to Fig. 3 since it shows a different quantity than discussed at this point of the manuscript).

P13, l. 7: 'minimum averaging period constrained by the diffusion correction (2.5 years)'
Is it correct to compare r values at different averaging times (1 year and 2.5 years)? Could this also contribute to the higher correlation compared to previous study of the same data?

AC:
Of course you are right that we compare correlation values at slightly different averaging times, which also could contribute to the slight difference between the two values. We will add this as a remark here.

P14, l. 1-3: 'For WAIS, the higher SNR at interannual timescales as compared to DML is consistent with the general notion that higher accumulation rates result in higher SNR.'

1. Please insert the SNR values for both.

AC:
We will add the average SNR value for periods from 5-10 years for both DML and WAIS (as obtained from the data in Fig. 3) for better comparison here.

2. For N=1 and deltat=5 years, r=0.45 at DML and r=0.51 at WAIS. The difference looks very small (especially considering the 3-times accumulation at WAIS). Why is the increase in SNR not scaled to the large increase in accumulation?

AC:
Please note that we compare at this point the SNR values (Fig. 3) between the two regions and not the correlations in Fig. 4, since the latter cannot be compared one-to-one as the correlation values, which are currently shown in the figure, are based on different lower integration limits in Eq. (6) (DML lower limit of 500 yr period, WAIS lower limit of ~100 yr period), which is due to the different lengths of the data sets.

However, this review comment made us aware of the fact that it is more appropriate to use the same integration limits for both regions for Fig. 4. Thus, we will provide an updated version of the figure where we will use the same lower integration limits (i.e. 100 yr period), which we will also mention upon describing Eq. (6).

P14, l26-28: 'Together with the observed spectral shapes on the longer timescales (30-100y) our results therefore might indicate a true increase in local variability at the WAIS sites towards longer time scales, and a close-to-constant or even decreasing coherent signal variability.'
This sentence is strange. It looks unbalanced with a first part dealing with long time scales and a second part dealing with... probably shorter time scales?
Is it possible to correct this sentence?
To facilitate reading, maybe this paragraph could be limited to the issue of high frequencies at WAIS, since the discussion for the long timescales already existed in the previous one.
Is it possible to separate the issue of high frequencies for noise and for signal?

AC:
The sentence was intended to summarize both the findings on short and high frequencies: we argue before that deficiencies in the correction approach likely cannot explain the remaining decrease in noise level towards high frequencies. Taking into account additionally the results for the longer timescales, could then therefore suggest that the WAIS noise level tends to increase across the timescales studied and that the signal tends to stay constant or even decrease.

We suggest to clarify the sentence as follows: "By including the found spectral shapes of the signal and noise on the longer timescales (periods from 30-100 yr), together our results for the WAIS sites might therefore indicate that, across the timescales studied, there is a true increase in local variability and a close-to-constant or even decreasing coherent signal variability."

What are the hypotheses explaining this decrease in noise at high frequencies? Could it be something like wind blowing that would homogenize only the surface over large areas? Or large-scale evaporation of surface snow homogenizing its composition?

AC:
We see no obvious explanation for the decreasing noise at high frequencies and, given the uncertainty of our results, we would refrain here from suggesting such explanations which would be purely speculative.

2.1 Appendix A

P16, l 23: 'a Gaussian convolution kernel whose standard deviation is the diffusion length σ_i that is a function of depth (time) and depends on site i.'
This is very synthetic, but not very clear. Please add a reference to Appendix B where more details are provided. Please give some values of σ_i (and possibly the associated frequencies) at various depths. Is there a formula that relates σ_i to time (for each site)? If not, the use of the term 'function' might be misleading here. Indeed, σ_i seems to answer on density, temperature and pressure, and therefore to be more similar to an adjustable parameter in the densification model.

AC:
We will add the reference to Appendix B here and clarify the sentence as: "a Gaussian convolution kernel whose standard deviation is the diffusion length σ_i that is a function of depth (or, equivalently: time) and depends on site i through depending on the local temperature, atmospheric pressure and accumulation rate (Appendix B)."

Please note that to our understanding it is correct to state that σ_i is a function of depth: For constant temperature and pressure, the diffusion length is solely a function of firn density (Gkinis et al., 2014), and since density is a function of depth z, also $\sigma_i = \sigma_i(z)$. Assuming a Herron-Langway model for the firn density, there is an analytical solution which relates σ_i to the firn density (van der Wel, 2012).

We will add some typical diffusion length values (both in cm and years) to the manuscript, which we think is most suitable in Appendix B (p.18 l.16).

2.2 Appendix C

P19, l8: This sentence is complicated. It would be simpler to describe the procedure at EDML, and then say that the same method is applied to WAIS.

AC:
We will rephrase the sentence as suggested.

Technical corrections:

1. P5, l. 12: "inidividual" typo

AC:
Thank you for spotting this typo which will be corrected.

2. P6, l. 14: Please remove 'exemplarily' which is redundant with 'to illustrate our method'.

AC:
We agree and will rephrase the sentence to: "In order to illustrate our method (Sect. 2.2), we first use the DML1 data set to demonstrate the individual steps involved in the analysis."

3. P11, l. 8: 'estimated signal term' Please add a reference to the equation (5 or 6).

AC:
We do not see a motivation for referencing Eq. 5 or 6 here, since these provide different quantities (i.e. the signal-to-noise ratio (SNR) and its integrated value). Instead we assume that the reviewer would welcome a reference to the relevant spectral quantities in the methods section 2.2 again for clarification. Since the term "estimated signal" has already been mentioned earlier in this paragraph, we suggest to provide this reference not here but directly in the first paragraph of the current section 4.1 (p.11, l.6-7): "We presented a method and the results of separating the variability recorded by Antarctic isotope records into two contributions: local variations ("noise", Eq. 4b) and spatially coherent variations ("signal", Eq. 4a)."

4. P16, l. 12: 'core sites' typo

AC:
Thank you for spotting this typo which will be corrected.

5. P17, l.8: It is $F(\varepsilon_i)F(\varepsilon_j) \neq 0$ only for i=j, and hence... Maybe 'we have' instead of 'it is'?

AC:
We will change the sentence as suggested.

Thomas Münch and Thomas Laepple

References to the author comments:

Gkinis, V., et al., Earth Planet. Sci. Lett., 405, 132–141, https://doi.org/ 10.1016/j.epsl.2014.08.022, 2014.

van der Wel, L. G., Doctoral Thesis, University of Groningen, 146 pp., http://hdl.handle.net/11370/72cf3b0b-d258-44a1-8830-b0c355ddbd90, 2012.