Response to the reviewers

CPD 11, 5605-5649, 2015: Regional climate signal vs. local noise: a two-dimensional view of water isotopes in Antarctic firn at Kohnen station, Dronning Maud Land

Thomas Münch et al.

11th February 2016

We thank both reviewers for their constructive comments. Based on these, the major points that we suggest for the manuscript revision are a shortening of the entire manuscript, a clarification of the used nomenclature and of the mathematical derivation of the noise model, as well as the rewriting of certain paragraphs. We would like to point out that part of the review comments are based on misunderstandings. We are sorry that our style in the manuscript was not concise enough at some points and will make efforts to improve this. Please find below our detailed answers. We will first reply to the general comments of both reviewers and afterwards answer the specific comments. The reviewer comments are typeset in *italics*, our author comments in normal font.

General comments

Anonymous referee #1:

First and most important I think that the manuscript does not read well. The writing feels overly complicated while the mathematical treatment, the description of the statistical noise model as well as the way the latter is used with the real data sets are not presented clearly. The manuscript will benefit from a clean-up and a clarification of the mathematical symbols as well as the terminology that seem to be used carelessly to some extent. After I read the

Appendix 1 and all sections relevant to the derivation and use of the noise model, it is still very unclear to me what exactly have the authors done. I can't claim that my math/statistics level is very high but can certainly relate to the average reader of CP and my problem in understanding the methods lies mostly in the rather confusing use of symbols and often in the absent explanations of how the noise model was applied.

AC:

We would like to express our apologies that the manuscript was hard to read and to follow. We will make an effort to improve its readability. This will include a shortening as well as a simplification of the manuscript. We plan to accomplish the shortening by removing the diffusion model and its discussion, by merging sections 4.1 and 4.2 and by condensing individual paragraphs. Simplification of the manuscript will be reached by reducing technical terminology and a clean-up of the nomenclature. For this, we will extend the Data and Methods section by an additional paragraph that introduces the coordinate system that is used throughout the manuscript (including a schematic figure) as well as relevant nomenclature. We will make sure that the nomenclature introduced there will be used throughout the rest of the text. We will give more space to the statistical noise model in order to clarify both its derivation in the appendix as well as its application in the main text. To improve the comprehensibility of the derivation, we will introduce a table of symbols including their definitions in the appendix.

I believe that the manuscript falsely presents an overly pessimistic view on the use of the water isotopic ratios obtained from single firn/ice cores. The reason for this is that the signal to noise ratios and variance estimations of the 1 m deep firn cores array are in a way "extrapolated" and used for evaluating the representativity of deeper cores thus falsely giving the impression that a minimum of N cores is needed for a robust isotopic signal to be estimated. Even though a study of the top 1 m of firn is very valuable one should expect isotopic diffusion and firn densification to heavily attenuate a lot of the variance caused by post-depositional (mostly surface topography) effects. This is of course not to say that the interprofile correlation is expected to approach 1 but certainly the low covariances the authors observe for the top 1 meter are not representative of the deeper parts of a firn core. I also fear that the results the authors present regarding the last 6000 years of isotopic data from the EDML core overestimate the importance of post depositional noise and neglect the recorded climate variability. This does not necessarily mean that water isotopic records are accurate proxies of *polar temperature over the Holocene; the problem of the low responsivity of the d18O signal to temperature still remains.*

AC:

The reviewer states his concerns about the fact that we use noise levels inferred from the first metre of firn also to assess the representativity of much deeper firn cores, and mentions that both densification and diffusion likely affect the noise level in the deeper parts. We are certainly aware of the fact that our approach of analysing the first metre is only a limitation, and we will ensure that this is also marked as such clearly in the manuscript.

However, regarding the influence of densification and diffusion we do not fully agree. In the first metre of firn densification does not occur at our study site which is shown by the density data obtained from the trenches. It is therefore not relevant for our data. Below the first 1-2 metres where densification starts, its effect on the lateral isotopic variability is probably dependent on the sampling resolution. However, the exact effect is yet unclear. We will add a respective remark at the end of section 4.1. In the case of diffusion and densification we also have to bear in mind that it acts equally on both signal and post-depositional noise. If the variance of the climate signal in the isotopic time series does not change on the time-scale considered (e.g. inter-annual), which is a reasonable assumption, the variance ratio of signal to noise will not be affected by diffusion nor densification, and our results of the representativity will not change for the deeper parts of a firn core.

However, we also expect that the climate signal has more variance associated with longer time scales, e.g., as seen on glacial-interglacial time scales. Therefore, the signal to noise ratio will improve considerably when analysing longer time scales (e.g. centennial or millenial variations). We will add these points to the discussion in sections 4.3 and 4.4.

Regarding the interpretation of the decadal variance seen in the EDML deep ice core over the last 6000 years, we admit that so far we have neglected diffusion at this point. However, even after a full forward diffusion of our trench noise level estimates with a (pessimistic) diffusion length of 8 cm water equivalent, the effect on decadal and longer variations is small. Our inferred noise levels for the decadal time scale are consequently not strongly affected (the inter-annual noise levels estimated from the trenches are reduced by a factor of ~ 0.095 in the diffusion case instead of a factor of 1/10 in case of undiffusing white noise; see also our more detailed answer to the the respective specific comment). Thus, our statement that the

EDML core decadal isotope variations might to a considerable part be noise is still valid after accounting for diffusion. We will add this discussion to the manuscript.

I have the impression that the authors tend to statistically treat the pre-deposition isotopic signal as a stationary stochastic process when in reality it is to a large extent a deterministic signal. Additionally, water isotope time series from ice cores are found to present a red + white noise behavior in the frequency domain, likely reflecting processes in the climate system that introduce a long-term memory. As a result the approach the authors use for example in section 4.4 when attempting to detect a warming trend is far from realistic. A warming signal in water isotopes can't possibly be just the sum of a linear trend and white noise.

AC:

While we do not agree that large parts of the pre-depositional signal are deterministic, we are also aware that it is a mixture of many processes. On the one hand, its temperature signal consists of deterministic components (the seasonal cycle, solar and volcanic forcing, anthropogenic trends) and of a stochastic component as result of the internal variability in the climate system (red climate noise). On the other hand, it exhibits a non-temperature part including meteorologic/atmospheric effects of stochastic nature that influence the isotope content of precipitation, noise due to a varying isotope-temperature relationship, post-depositional noise, etc. In our paper we examine therefore the most simple and also most optimistic case: an anthropogenic trend + white post-depositional noise. Our Monte Carlo simulation is hence valid as an upper bound of the detection probability since all other mentioned components of a real isotope time series will complicate the detectability of an anthropogenic trend. In our oppinion it is thus only necessary to formulate the underlying assumptions in the Monte Carlo simulation much more clearly and to mention the additional complicating issues, but not to refine the approach itself.

Based on their results regarding the minimum number of cores required for a satisfactory representativity, the authors suggest that it is preferable to sacrifice measurement precision (wrongly referred to as accuracy in the manuscript) to higher throughput in order for more cores to be analyzed using Cavity Ring Down Spectroscopy. This recommendation sounds tentative for two reasons. Firstly with the current Cavity Ring Down instrumentation one *injection is very unlikely to provide results free of memory effects regardless of the correction scheme used. I am personally not aware of a correction scheme that "behaves" when such a small number of data points are available per sample. The problem this generates is that intra-sample memory effects are notorious for modifying the color of the noise in high resolution water isotope records. This impacts any work utilizing spectral methods as power spectral densities become biased in the low frequency part of the spectrum. Secondly a higher analytical noise level results in inferior Deuterium excess records and impacts the accuracy of temperature reconstructions based on water isotope diffusion – the latter seeing a great benefit from measurements of as high precision as possible. I would argue that the authors should reconsider this message and at least stress out that there will be a cost in following a one-injection measurement approach.*

AC:

We agree with the reviewer that reducing the number of injections on Cavity Ring Down Spectrosopy instruments down to one per sample might affect the usability of the data for diffusion-based methods as well as for the interpretation of deuterium excess. On the other hand, it would improve single-proxy reconstructions if it allowed more replicate core measurements. In the revised version, we will better stress the limitations of our suggestion.

Last, though not as important, it would be nice presenting some of the d18O profiles from T1 so the reader has a feeling of how the time series look.

AC:

We do not think that this is an improvement of the manuscript since single T1 d18O profiles will not offer any new insights compared to the T2 profiles already shown. All data presented in the paper will be made public via the data base PANGAEA (http://www.pangaea.de/) so that anyone will be able to investigate it.

Anonymous referee #2:

The paper overall is very difficult to read. The writing is too complicated, often mixing nomenclature, or not defining it properly. The statistical model, especially, deserves more attention in the text, as well as more description in the Appendix. A major simplification of *the story is needed. As it stands, the reader is lost in technical and often unnecessary writing. The paper could be as much as* 25% *shorter just in this regard.*

AC:

Similar issues have been mentioned by the first reviewer. We therefore cite here our answer from above:

We would like to express our apologies that the manuscript was hard to read and to follow. We will make an effort to improve its readability. This will include a shortening as well as a simplification of the manuscript. We plan to accomplish the shortening by removing the diffusion model and its discussion, by merging sections 4.1 and 4.2 and by condensing individual paragraphs. Simplification of the manuscript will be reached by reducing technical terminology and a clean-up of the nomenclature. For this, we will extend the Data and Methods section by an additional paragraph that introduces the coordinate system that is used throughout the manuscript (including a schematic figure) as well as relevant nomenclature. We will make sure that the nomenclature introduced there will be used throughout the rest of the text. We will give more space to the statistical noise model in order to clarify both its derivation in the appendix as well as its application in the main text. To improve the comprehensibility of the derivation, we will introduce a table of symbols including their definitions in the appendix.

In section 4.4, the authors attempt to reconstruct a 0.5degC temperature trend using a Monte Carlo approach consisting of a signal (linear temperature trend) and random noise. Although the time period is short (50 years), this is far too simplistic a model for estimating isotopic variability. The approach must also include the atmospheric component of variability, because storm tracks and moisture sources can change over decadal time periods. At the very least, this should be clearly documented as a simplifying assumption. Water isotope signals do not only depend on noise and temperature!

AC:

We agree with the reviewer that our model neglects many contributions to the signal and noise as well as the processes causing these variations. Please see also our response to the similar issue raised by reviewer 1. However, our model, by purpose, examines a simple and also most optimistic case: an anthropogenic trend + white post-depositional noise. Our Monte Carlo simulation is hence valid as an upper bound of the detection probability since all other mentioned components of a real isotope time series will complicate the detectability of an anthropogenic trend. We will formulate the underlying assumptions in the Monte Carlo simulation more clearly, mention the limitations, and make clear that this is a thought experiment to estimate a lower limit of the number of required cores and not a realistic simulation.

The results presented largely focus on isotopic analysis in the depth/time domain, but I think it would be worth pointing out that analysis in the frequency domain of isotopic profiles would be informative, and an area of much needed research. It makes sense that post-depositional stratigraphic variations alter the isotopic signal, but is the frequency component of the data preserved? That is, do the spectra of nearby isotopic profiles in the vertical direction have the same power density values? In my opinion, this would be the major test of water isotope literature. At the end of the paper, this should be suggested (note: an analysis like this would require perhaps 100 years of data from multiple cores). Table 1 would suggest there may be large discrepancies in the frequency domain, but I also think the vertical scale of the study (~ 1 *m) prevents any useful conclusions.*

AC:

We agree with the reviewer that a spectral analysis of nearby firn cores is a very interesting approach. It is expected that temperature spectra (from climate models, for instance) will show deviations from d18O spectra of ice/firn cores due to post-depositional noise and diffusion. In fact, this is part of our ongoing research to obtain a better understanding of signal and noise in Antarctic cores. However, with respect to our manuscript we do not regard a spectral approach as meaningful due to the limited vertical extent of our data. In addition, for the rather nearby trenches we expect their spectra to be similar within uncertainty of the spectral estimate. In our data, we observe a quite considerable difference between variance levels of the mean trench profiles. For example, the estimated signal variance of the mean T1 profile on the inter-annual time scale of 1.15 (per mil)^2 is in contrast to the value of T2 of only 0.21 (per mil)^2 (see Tab. 1 in the manuscript). This discrepancy can be attributed to the fact that information is lost due to the stacking of the single profiles. We will add a sentence to the conclusions section that spectral analyses of firn cores would complement trench-like studies in order to understand the spectral shape of the noise.

Throughout the paper, an accumulation value for low-accumulation sites is poorly defined. The results of the paper are only valid for low accumulation sites, which I guess might mean something like less than 15 cm ice eq/year. It should be made clear at the beginning of the paper, and throughout.

AC:

As a reference throughout the paper, we will define a low accumulation rate to mean a value of $\leq$ 10 cm water eq./year. The East Antarctic plateau typically shows accumulation rates below this threshold.

Suggesting that only one injection on Cavity Ring Down Spectroscopy instruments be used for future multi-ice core studies, in my opinion, should not be included as a suggestion in the paper. Although throughput would increase, current CRMS instruments cannot give reliable results with a single injection - precision is lost - and this can alter the frequency component of the signal. Plus, the deuterium excess parameter requires good precision in both d18O and dD for useable results.

AC:

Also the first reviewer has critisised our recommendation in the paper to reduce the number of injections on Cavity Ring Down Spectrosopy instruments down to one per sample in order to be able to measure more cores instead. We will better state the limitations of our suggestion in the revised manuscript.

In Figure 4, seeing that the mean isotope profiles of T1 and T2 are correlated at 0.82 leads me to believe that clarification is needed in the text. Using a low accumulation site to extract temperature is problematic in many ways, and using up to 50 cores might be necessary to get some sort of temperature signal, but simply averaging a few isotopic profiles over some depth/time is still useful to pull out a common climate signal. This must be clarified to the reader.

AC:

The significant observed seasonal correlation of 0.81 is expected from our noise model for the seasonal time scale: The model shows that a number of five profiles at a spacing of 10 m is sufficient to obtain a representative (R>0.9) isotope signal. In T1, 38 profiles are averaged in the mean profile, thus a large number; in T2, four profiles at optimal spacings of at least 10 m are averaged. The recommendation of drilling 10–40 cores for a representative signal refers to the inter-annual case for which the signal-to-noise ratio is much smaller. Despite that, we observe a correlation between T1 and T2 for the inter-annual mean time series of 0.87. However, this value should be taken with care since its significance is doubtable as the value is only based on five observations. Both aspects will be clarified in the manuscript in section 4.3.

Answers to specific comments, anonymous referee #1:

RC 1, P5610–L15:

Based on the scheme you present the results of your measurements are not calibrated on the SMOW/SLAP scale. This is unfortunately a point misunderstood by many laboratories performing water isotope analysis. Technically a calibration of your samples on the SMOW/SLAP scale requires a two fixed-point calibration. This originates from the SMOW/SLAP scale definition itself where zero is defined by SMOW and the linear scale is defined by SLAP at -55.5 per mile (precisely). The problem with a three points linear fit is that despite the fact that often the R2 value of the linear fit looks excellent the actual offsets of the points from the calibration line are large enough to cause accuracy issues that are not easy to identify. I think your measurements will strongly benefit from fixing the two extreme water standard points, calculating a calibration line based on those two and using the 3rd mid point as an accuracy check. This in the end is a measure of your "combined uncertainty" and often it can be slightly higher than a precision estimate that is based on the of series of injections of a standard water. With this in mind the 0.09 per mile precision given in the manuscript is absolutely the upper limit of precision and very likely the combined uncertainty of the measurements is somewhat worse. Having said this, I do not think your actual results will vary significantly by choosing a 2-point calibration and thus if you make a proper comment on the calibration scheme it will be fine not readdressing all your measurement runs. It would however be very nice to apply it to one run in order to get a feel of how high your combined uncertainty is, as estimated by checking the offset of the middle standard from the calibration line.

AC:

Please excuse that, for the sake of brevity, we have apparently not adequately described our measurement and correction scheme. In fact, each measurement run includes three blocks of standard measurements, one at the beginning, one at the end and one in the middle of the run. The three-point calibration as well as the memory correction is performed with, or respectively based on, standards from the first block, the drift correction by additionally using standards from the last block. To check the precision of the entire calibration and correction scheme, an independent standard in the middle block is measured that is neither used for calibration nor memory/drift correction. Our given measurement precision is based on the deviation of this standard from its known value. It thus yields a measure of the combined uncertainty of the calibration and the measurement itself. In the revised version we will add that the given precision is based on the evaluation of an independent standard not used for calibration or correction and thus represents an combined uncertainty.

Regarding SMOW/SLAP scale we agree that, strictly speaking, the calibration is not performed onto the SMOW/SLAP scale. We will change the respective sentence to: “The isotopic ratios are calibrated by means of a linear three-point regression analysis with different in-house standards where each standard has been calibrated to the international V-SMOW/SLAP scale.”

RC 2, P5611–L8:

“Signifficantly higher density” Maybe an estimate?

AC:

According to the reference given, the dunes typically exhibit snow densities about 15–50 % higher than the mean value of the surrounding firn. We will add this information to the manuscript.

RC 3, P5612–L10:

The numbers you give for the RMS deviations seem very low after looking at the profiles in Figure 1b. Is there any chance you calculated mean of differences and not an RMS value?

AC:

This is a misunderstanding, please excuse that this has not become clear. For a specific layer profile, we calculate the root-mean square deviation (rmsd) for two cases: i) between the layer profile and the surface height profile, and ii) between the layer profile and the horizontal reference (a straight line). The numbers we state in the manuscript are the differene between the two rmsd values. We will rewrite the entire paragraph for clarification.

RC 4, P5612–L22 and Figure 2:

The P–P values of the T2 d8O profiles ar about 10 per mile lower than of those from T1. Can you maybe comment on this?

AC:

The peak-peak value is an instable metric and depends strongly on the sample size. In T2 only four profiles were sampled which likely causes the difference between both trenches (20 per mil in T1 vs. 12 per mil in T2). More stable metrics are for example the mean and the standard deviation which indeed show much smaller differences between the trenches (mean(T1)=-44.4 per mil vs. mean(T2)=44.0 per mil; SD(T1)=3.1 per mil vs. SD(T2)=2.7 per mil). These values are also stated in the manuscript or will be added (please see answer to RC 8 of referee #2).

RC 5, P5614–L11:

For the case of an AR-1 process one would expect the correlation to continuously drop until it reaches values close to zero for high lag values. Here you observe a plateau at the value of 0.5 for spacings $\geq 10m$ *Does this imply something for the choice of the AR-1 approach for your lateral noise?*

AC:

This is a misunderstanding as our model is not an AR-1 process alone, but the sum of a noise following an AR-1 process and a coherent signal. In P5614-L13-15 we state: “We assume that each profile consists of a common signal S and a noise component ε independent of the signal. The noise component is modeled as a first-order autoregressive process (AR(1)) in the lateral direction.” The inter-profile correlation then is the sum of a constant term and an AR(1) term that decorrelates with increasing distance between the profiles (see Eq. (2) in the manuscript):

$$r_{XY} = \frac{1}{1+\frac{\mathrm{var}(\varepsilon)}{\mathrm{var}(S)}} + \frac{\frac{\mathrm{var}(\varepsilon)}{\mathrm{var}(S)}}{1+\frac{\mathrm{var}(\varepsilon)}{\mathrm{var}(S)}} \times \exp\left(-\frac{|x-y|}{\lambda}\right) .$$

The constant term assumes for a variance ratio $\mathrm{var}(\epsilon)/\mathrm{var}(S) = 1.1$ as used in the manuscript a value of ~ 0.5. We will change the legend of Fig. 5 to "AR(1) noise + signal model" to make it also here immediately apparent to the reader that the model consists of a noise and a signal component.

RC 6, P5614–L18:

The term "signal to noise ratio" is normally used to describe the ratio of the powers of two signals. Is it appropriate to use this term when looking into the variance ratio?

AC:

The signal-to-noise ratio is indeed defined as the ratio of the powers of signal and noise. However, it is also routinely used in the related literature to describe the variance ratio (e.g., Persson et al., 2011, JGR; Wigley et al., 1994, Journal of Climate and Applied Meteorology). When both signal and noise are stationary stochastic processes, their respective power is equal to their mean-squared value; which is further identical to the variance if both have zero mean. An AR(1) process is stationary stochastic; however, this is not the case for the isotopic seasonal signal since it contains a deterministic signal, the seasonal cycle. To prevent misunderstandings, for the manuscript we will name it signal-to-noise variance ratio, as, e.g., in Fisher et al., 1985.

RC 7, P5617–L8:

Preferably replace "m-scale" with "meter–scale"

AC:

We will adopt this change in the manuscript.

RC 8, P5617–L11:

The relatively recent literature on vapor measurements and their interpretation has certainly showed that the isotopic composition of the upper snow is subject to change post deposition and similar changes can be observed in the vapor isotopic composition. However I do not think that the literature has showed any solid evidence that sublimation-condensation processes are the mechanism driving these changes in the upper firn (it is possible indeed). A rather simple diffusion model can show how an underlying winter layer can significantly deplete the isotopic composition of the overlying enriched summer layer in a period of hours to few days, some-

thing allowed by the extremely open porosity of the upper firn.

AC:

We agree with the reviewer but also think that our statement “Possibly, exchange of water vapour with the atmosphere by sublimation-condensation processes (Steen-Larsen et al., 2014), potentially accompanied by forced ventilation (Waddington et al., 2002; Neumann and Waddington, 2004; Town et al., 2008), acts as a further noise source.” clearly reflects that this is not a solid evidence but a possibility.

RC 9, P5618–L3:

Indeed firn diffusion plays a strong role. Do you not think that the densification process itself is also a mechanism that reduces the variance caused by surface topography noise?

AC:

In the sampling region no densification is observed within approximately the first two metres of firn (J. Freitag, personal communication), the densities measured in both trenches support this (T. Laepple et al., manuscript in preparation). Consequently, we do not consider densification to be important for our data set. Nevertheless we agree that below the first 1-2 metres, where densification starts, it may influence the noise variance given the firn is sampled in constant intervals.

RC 10, P5618–L23:

I guess that you need a sinusoidal d18O signal in order to cancel out at a shift of $\nu/4$*? Also, your observations show a plateau at a correlation of 0.5 so you do see something different in fact.*

AC:

The purpose here was to assign a physical interpretation to the observed decorrelation length of the noise. However, we agree with the reviewer that the attempt to relate a sinusoidal surface variation with the exponential decorrelation of the noise is too simplistic since the autocorrelation of a periodical function is again periodical, not exponential. We will remove this part and simply state that the observed decorrelation length of $\lambda \sim 1.5$ m is of the same order of magnitude as the small-scale surface height variations, suggesting stratigraphic noise to be an important noise component in our records.

RC 11, P5619–L2:

Is the 1km value an educated guess?

AC:

The value corresponds to the rounded up distance between the trenches.

RC 12, P5619–L5:

Your comments on the validity of the isotopic thermometer and the precipitation intermittency are certainly valid but I find them irrelevant here. Your study deals with local noise and further complicating the discussion with the long standing question on the validity of the isotopic thermometer can possibly be confusing at this point in the manuscript.

AC:

We agree that the additional comments on the isotopic thermometer and precipitation intermittency might confuse some readers at this point, and we will remove this part from the manuscript.

RC 13, P5619–L15-22:

The reader here is left guessing what you have done for this section. Which model parameters from T1 do you carry over for this calculation? You mention that an averaged set of T1 profiles is used and that those profiles are chosen if they fulfill the required criteria. Can you be more specific? Inspecting Fig. 7 I see a feature of your model that is hard to understand (it also appears in Fig. 8 actually). For $N = 2$ *and* $N = 3$ *there seems to be a discontinuity in your model. A "kink" is very clearly seen. I do not see any reason why your math produces such a feature (i am referring to the* r_{xy} *definition here). Can you explain why this is the case?*

AC:

i) We are sorry that this part was apparently not clearly written. We will thoroughly rewrite it to clarify what is being done here. ii) The "kinks" seen in the model curves in Fig.s (7) and (8) are not a discontinuity of the model itself, but due to the fact that the model (and also the data) can only be evaluated for an integer number of profiles. We will add points at N=1,2,3,... to the lines in each plot to make this clear.

RC 14, P5620–L20:

Again you refer to correlation to local temperatures. This is essentially a different study and your reference to weather station data sort of pops out of the blue here leaving the reader a bit confused.

AC:

We think it is important to assign a physical meaning to our term of representativity. For this we stick to the classic interpretation of d18O as a proxy for local temperature, thereby assuming that the coherent isotope signal identified in the trench record is related to local temperature variations. Bearing in mind issues such as meteorology and moisture source temperatures that complicate this interpretation, our representativity can then be interpreted as an upper bound for the correlation with a nearby weather station. True correlations will certainly be lower. We want to stress again our oppinion that a physical meaning of the term representativity is a benefit for the reader and suggest to keep this, but will of course rewrite the sentence to make our reasoning more transparent.

RC 15, P5620–L25:

Can you be more specific on the time scale here. Do you simply mean "time" and not "time scale"? Also keep in mind that nowhere in the manuscript a description on how you assigned a time scale is to be found. You calculate annual means but have not described how you assign years to your data.

AC:

i) We are afraid this is a misunderstanding. In our understanding the term "time scale" is common usage in climatology to denote a typical period of time: e.g., climate variations occur on different time scales, from seasonal over inter-annual to decadal, centennial and longer variations. ii) The construction of the age-depth relationship/assignment of annual means is described in P5616 L4-8: "In order to obtain annual-mean d18O time series we define annual bins through the six local maxima determined from the averaged profile of the two mean trench profiles. The mean peak-to-peak distance of these maxima is 19.8 cm, consistent with the accumulation rate. Three alternative sets of annual bins are derived from the five local minima as well as from the midpoints of the slopes flanking these minima.", but we will try to add a more detailed description in the results section.

RC 16, P5621–L10:

Would the simplest and best case scenario be assuming white noise?

AC:

Indeed, white noise would be more advantageous than autoregressive noise. However, firstly the detrended trench data are positively autocorrelated in the vertical direction, contradicting white noise. Secondly, white noise is physically quite unlikely. Since stratigraphic noise is the result of constant mixing, erosion and redistribution of the surface snow it is likely that adjacent layers show some inter-relation. We will change the wording to reflect that the first-order autoregressive noise is the best case, consistent with the available data.

RC 17, P5622–L10:

I guess you would have to agree that the study from Graf et al has completely different boundary conditions than yours. Low cross correlations between the records in that case can be due to other processes that are not apparent in your case.

AC:

We are aware that the results obtained by Graf et al. also include other effects than just the stratigraphic noise. This is reflected in our manuscript (P5622-L18-21): "However, this accordance does not necessarily mean that our worst-case scenario is the more realistic one since the measured cross-correlations [in the study of Graf et al.] are also subject to potential dating uncertainties and additional variability caused by spatially varying precipitation-weighting and possibly other effects." We disagree with the reviewer that the study of Graf et al. has completely different boundary conditions: It was conducted in the same area, the firn cores are annually resolved, and they cover isotopic variations at the end of the Holocene. In summary, we would leave this part of the manuscript as it is.

RC 18, P5623–L5:

I am not sure the term "significant challenge" is appropriate here considering you only use data from the top 1 m of firn.

AC:

The corresponding part in the manuscript is: "The noise level identified in our trench data poses a significant challenge for the interpretation of firn-core-based climate reconstructions on seasonal to inter-annual time scales." Hence, we already restrict the statement to apply to seasonal to inter-annual time scales only, and not in general. We will add "in our study region" to stress that we only make a statement for the area around Kohnen station.

RC 19, P5623–L21:

Replace "high-accuracy" with "high-precision". It is the precision that affects the variance of your noise in the isotopic profiles. Accuracy issues can potentially create biases but this is not exactly what you are looking at.

AC:

We will replace "high-accuracy" with "high-precision". We accidentally mixed up the two terms.

RC 20, P5624–L5-7:

I suppose you would require that the d18O signal is stationary in order to make this statement?

AC:

While we do not make any assumption about the d18O *signal* here, indeed we assume stationarity of the post-depositional noise (before densification and diffusion which does not influence the ratio of stratigraphic and measurement noise). However, we feel that this is a reasonable assumption, at least for the late-Holocene.

RC 21, P5624–L25:

I find it problematic that after you have used a certain color for the lateral and vertical noise in your previous calculations, now for the case of the detection of the warming trend you only assume a linear slope plus white noise for the whole signal. This is far from realistic. Take a look at high-resolution deep ice core data – there is a plethora of information in them and they certainly do not look like white noise even for the case of the relatively "boring" Holocene.

AC:

As outlined in more detail in our answer to the general comments, we do not assume at any point that the Holocene climate signal is white. The purpose of the "warming detection thought experiment" is to provide the reader with a simple demonstration what stratigraphic noise implies for the detectability of a temperature trend. Here we aim for the simplest, and also most optimistic model which is reflected in our assumption of a pure linear trend. Including any further signal components (internal climate variability, filtering and modification of the signal by meteorology etc.) would complicate the model and also the understandability for the reader, but also lead to more pessimistic results (thus requiring even more cores to detect an antropoghenic signal).

The white-noise component arises solely from modeling the post-depositional noise. It is correct that on the seasonal time scale the data suggests that the post-depositional noise is autoregressive in the vertical direction (thus in the time domain) with a decorrelation length of $\lambda \approx 6$ cm. However, on the inter-annual time scale the noise for such a λ can be well approximated by white noise as the power spectrum of an AR(1) process levels off on frequencies below the frequency associated with the decorrelation length. As an asset, white noise is more optimistic than AR(1) noise and here also simpler for the reader to understand. We will add some clarifying remarks about the relationship of the vertical noise covariance between seasonal and inter-annual time scales.

RC 22, P5626–L16:

I assume that with the term "noise" here you refer to post depositional noise. I personally have my strong doubts that this statement is true for three reasons. Firstly a simple spectral analysis of the EDML high resolution data over the last 6000 years will reveal clear information of the diffusion process and thus past temperature. The signal to noise ratio in this case (and of course this varies through the core) is roughly 20-30 dB. Secondly as I have explained above your results are based on values that are likely an overestimate of the final contribution of post depositional noise since you are focusing only at the top 1m. Lastly (and here I have to admit I am doubting myself a bit so take this with a grain of salt..) I am not sure that the use of the statistical variance is proper for a deterministic periodic signal like this of d18O.

AC:

Regarding the reviewer's first point we have to be cautios as the reviewer contrasts two different methods. There are several things to consider:

i) The signal-to-noise ratio (SNR) the reviewer gives in the case of inferring past temperature from diffusion is in our understanding the ratio of the measurement noise (the baseline in the d18O spectra) to the measured spectral signal. This cannot be compared to our SNR contrasting isotopic signal to post-depositional noise, but rather has to be compared to the ratio of isotopic signal to our measurement precision of 0.09 per mil. In the manuscript we use as an estimate for the annual signal variance a value of 0.68 (per mil)^2. This gives a SNR of $10\log(0.68/0.09^2) \sim 20$ dB, similar to the reviewer's lower bound. On longer time scales one should expect the signal to become stronger. However, in any case the SNR of isotopic signal to post-depositional noise is considerably smaller.

ii) We are afraid that it has not become clear that we refer all our implications for the ability of d18O firn cores to reconstruct past climate to the classical method of interpreting d18O as a proxy for (local) temperature. In this context we do not intend to say that there is no climate signal in the EDML record over the last 6000 years, but that it might be entirely masked by post-depositional noise (see below our answer to the second point). We will rephrase the respective passage to make this clear. We agree with the reviewer that the diffusion method is a powerful tool to reconstruct past temperatures. This is based on the fact that the temperature signal that is reconstructed is not inferred from the isotopic time series itself but by the diffusion acting on it. In fact, it is commonly assumed that, before diffusion, the d18O spectrum is initially white due to post-depositional noise (Gkinis et al. (2014), Johnsen et al. (2000)). We will add a clear statement to the manuscrip that all our implications refer to the classical d18O method, and mention that there are other means utilizing firn cores for climate reconstructions (such as the diffusion method or nitrogen/argon isotope ratios) to which our implications do not necessarily apply.

To the reviewer's second point: It is certainly a strong assumption to apply noise levels inferred from the first metre of firn to a time series covering 6000 years. We will carefully rephrase the respective parts to make this clear. Additionally, we admit that in the manuscript the effect diffusion has on the decadal post-depositional noise level has so far been neglected. However, even after a pessimistic estimate of the effect of diffusion, the change of our results is small: Taking the inter-annual post-depositional noise level inferred from the trenches (5.9 $(\text{per mil})^2$ in the worst-case, 1.25 $(\text{per mil})^2$ in the best-case scenario) and assuming the inter-annual noise to be initially white, the decadal noise level is obtained by the integral over the diffused spectrum. Accounting for full forward diffusion with a constant diffusion length of 8 cm water equivalent it turns out that the inter-annual noise level is reduced by a factor of ~ 0.095 instead of a factor of 1/10 for undiffusing white noise. This small difference is due to the fact that for the present accumulation rate at Kohnen station of 6.4 cm w.eq./yr, diffusion mainly acts on isotopic variations on sub-decadal time scales. For longer periods of time it becomes more and more negligible.

In summary, the decadal d18O variations observed in the EDML record can still not easily be interpreted as climatic variations but instead might be to a large extent post-depositional noise. For the revised manuscript, we will add our estimate of the influence of diffusion in the main text and update the noise levels given in Tab. 2 accordingly.

To the last point: We agree with the reviewer that in statistics, variance is strictly defined only in terms of random variables. However, generally climate is a mixture of stochastic and deterministic parts. This is exemplarily seen also in the EDML d18O time series over the last 6000 years which does not resemble a purely deterministic signal (see Fig. 2 of Oerter et al. (2004)). Using the variance in such cases is straightforward.

RC 23, P5626–L25:

Your phrasing on the intermittency of the accumulation may be misunderstood here. It may be a good idea to stress out that you are talking about post deposition (or redeposition) of snow causing the local variability of the accumulation.

AC:

Thanks for the comment; indeed we did not mean accumulation intermittency here but post-depositional redeposition. We will rephrase the sentence accordingly.

RC 24, Appendix A:

I would suggest that the authors spend some time to reread this section. A clean-up in the way symbols are used and what exactly do they mean (perhaps a table?) would be very helpful. In particular the use of the terms ε, $\tilde{\varepsilon}$, ε_x, ε_y, $\sigma^2_{\bar{x}}$, $\sigma^{*2}_{\bar{x}}$ *and what they represent has been very hard for me to follow when reading this section. I also think that since your data analysis is all performed in the depth domain you should substitute* t *with* z *in all the equations in Appendix A.*

Assuming one drills a vertical core and measures a signal $X(z)$ *then this signal can be seen the sum of an ideal signal* $S(z)$ *plus some noise* $w(z)$ *as:*

$$X_n(z) = S_n(z) + w_n(z) \tag{1}$$

where n the index for core n drilled at lag τ_n*. As far as I understand you consider* $w_n(z)$ *to be the sum of a white noise variance* $w_{vert}(z)$ *in the vertical direction and a variance described by an AR(1) process in the horizontal plane* $\overline{\varepsilon}_n(z)$*.*

So, $w_{vert}(z)$ *has a constant value and* $\overline{\varepsilon}_n(z)$ *is (simply definition of an AR(1) process):*

$$\overline{\varepsilon}_n(z) = \alpha \cdot \overline{\varepsilon}_{n-1}(z) + \overline{w}_n(z) \tag{2}$$

where $\overline{w}_n(z)$ *is white noise and for simplicity lets assume it is the same for all cores thus simply summing up eq.1 and eq.2 I combine the white noise components into one and get:*

$$X_n(z) = S_n(z) + \varepsilon_{vert}(z) + \alpha \cdot \overline{\varepsilon}_{n-1}(z) + \overline{w}_n(z) = S_n(z) + \alpha \cdot \overline{\varepsilon}_{n-1}(z) + w'(z) \tag{3}$$

Can you clarify where does the normalization parameter in your eq. A3 comes from? I can also not understand how you separate your Gaussian noise in the vertical and your AR1 lateral in the math. Can you be more specific as to what is the difference between your $\widetilde{\varepsilon_{n-1}}(t)$ *and* $\varepsilon_n(t)$*. In the text* $\tilde{\varepsilon}$ *is described as white noise but in eq. A3 it looks like AR(1).*

Additionally since $S(t)$ *represents an "ideal" noise-free signal how do you practically calculate the* $var(S)$ *quantity as seen in several of the equations in the manuscript?*

In the beginning of the derivation of eq. A5 you calculate the mean value $X(t)$*, you run the indexes from 1 to N but for some reason the variable n is kept in the subscript. Is this correct?*

AC:

We are sorry that the derivation given in the appendix was not presented comprehensibly enough. For the revised manuscript, we will re-write the entire derivation in a more concise and understandable fashion, including a clean-up of the nomenclature.

To the individual points:

We agree that it is more appropriate to use z as the vertical variable instead of t and will follow this advice. We will also add a table of symbols summarising the different definitions. The factor $\sqrt{1-a^2}$ is not a result of the derivation but was introduced as a normalization so that the variance of the AR(1) noise series is unity. However, this introduction is actually not necessary and unfortunately led to a small mistake in the manuscript regarding nomenclature of the noise variances which, however, does not affect the actual results. For the revised manuscript, we will not use the this normalization and better separate the nomenclature of the noise (see below).

The noise term $\widetilde{\varepsilon_n}$ of profile n was introduced to be following a first-order autoregressive process in the horizontal direction. Thus, according to the definition of an AR(1) process, this noise term splits into the term $a\widetilde{\varepsilon_{n-1}}$ arising from the autocorrelation of the noise with the previous profile, and a term ε_n which is noise drawn from random variables that are independent and identically distributed (white or Gaussian noise). For the revised manuscript, for the sake of clarity, we will change the notation as follows: The autocorrelated noise will be termed w_n, the independent white noise component of each noise profile ε_n. Then, w is the noise term that can be identified with the horizontal trench variance in the main text, and not ε as accidentally given.

It is unfortunately a misunderstanding that we separate the noise into a vertical and a horizontal component. The only further assumptions about the modelled post-depositional noise is that it is stationary in both the horizontal and the vertical direction, and that its variance is isotropic. Thus, the noise term of a trench profile can be described by a single term. We will state these assumptions more clearly in the revised version of the appendix. A potential depth-dependency of the noise becomes relevant for averaging the trench data from seasonal to lower (e.g. inter-annual) resolution. This depth-dependency is then represented by the covariance of the noise in vertical direction for which the two cases in the main text are discussed (autoregressive noise similar to the horizontal direction (best case), or complete inter-dependence of the noise on the sub-annual time scale (worst case)). We will also describe this discussion in greater detail in the revised manuscript.

An exact estimate of the signal variance, $\mathrm{var}(S)$, is not necessarily needed, since our model results depend only on the signal to noise variance ratio, $\mathrm{var}(S)/\mathrm{var}(\varepsilon)$. For the seasonal time scale, this ratio can be estimated from the inter-profile correlation (Fig. 5) as it is done in the manuscript, and is then used throughout the manuscript for the noise model on this time scale. However, for the inter-annual time scale, individual estimates of the annual signal and noise variance are necessary. The annual signal variance is approximated by the mean of the variances of the mean annual d18O trench time series. This assumes that the noise in the time series is sufficiently averaged out by the stacking of the profiles. We will clarify the respective parts in the manuscript to make our approach and the underlying assumptions more clear to the reader.

The reason why the variable n is kept in the subscript in the beginning of Eq. (A5) is that n denotes the horiztonal position of the profile along the trench; thus n_1 refers to the position of profile number 1, n_N to the prosition of profile number N. We will simplify the entire nomenclature in the revised version of the appendix to avoid such ambiguity.

Answers to specific comments, anonymous referee #2:

RC 1, P5607-L3-4:

The stated text "the strong relationship between the isotopic ratios in precipitation and local air temperature" should be clarified. This is valid at large distances (latitude scale). Variability at a single ice core site will also depend on the trajectory of individual storm tracks, and for example, the location of low pressure zones that influence meteorology. This means that there is both a local temperature effect and an atmospheric effect. This is also mis-represented later in the paper using the Monte Carlo simulation.

AC:

Thank you for this comment. We will remove the adjective "strong" from the cited sentence as the relationship between precipitation and local temperature depends both on the spatial as well as temporal scale considered – as you mentioned and as we describe later in the introduction. In addition, we will better clarify in the manuscript here that local d18O also depends on the specific trajectory of a given precipitation event and thus on meteorology. However, still we think that our approach for the Monte Carlo simulations is valid as we aim to provide the optimistic boundary case which provides an upper bound for the reconstruction of a local temperature trend. We will describe our underlying assumptions for the Monte Carlo approach more clearly – in this context please see also our answers to the general comments.

RC 2, P5607–L13-16:

It is mis-leading to say that outside of large-scale temperature shifts (how big? glacial-interglacial size shifts?) it is often too hard to extract climate information. There is still climate information, such as multi-year or decadal oscillations, but perhaps finding a temperature signal in a low accumulation site is too hard. Please clarify. What sort of temperature shift? What does low accumulation even mean (less than 15cm ice eq/yr perhaps)?

AC:

We are sorry that our definition in the manuscript of non-climate noise as “the part of the isotopic record that cannot be interpreted in terms of large-scale temperature variations” was ambiguous. We refer the term “large-scale” here to large spatial scales, not to the amplitude of the temperature variation. We will point this out more clearly by writing “in terms of regional or larger-scale temperature variations”.

From this interpretation it follows that any local effects on the isotopic record (meteorological and post-depositional influences) are interpreted as non-climate noise in our manuscript. To our knowledge there is so far no solid evidence that decadal isotope variations observed at a single low-accumulation site, for example in the EDML deep ice-core record, can be interpreted in terms of regional temperature oscillations (as evidenced by a significant correlation to independent climate data). Thus, we think that our statement “may often be too high to accurately extract a climatic signal” is appropriate.

We will define low-accumulation here as being less than 10 cm water eq./year, please see also our answer to comment RC 4.

<u>RC 3, P5607–L21-23:</u>

What are non-climate influences? Do you mean noise, that must be averaged to get climate over something like 30 years or greater? This is at least partially explained in the rest of the paragraph. Perhaps state “short-term processes” or “small spatial scale processes” instead of “non-climate influences”.

AC:

We do not limit our definition of “non-climate influence” to noise on small spatial or short temporal scales, but include any influence that leads to isotopic variations (or, respectively, variations of any other temperature proxy) that cannot be interpreted as a regional or larger scale temperature signal. We will rephrase our sentence here to point out that we refer again to our earlier definition of non-climate noise (see our comment on RC 2).

<u>RC 4, P5608–L23:</u>

Please define low-accumulation.

AC:

Albeit being a subjective choice, we will adopt as a definition of low accumulation a value of

$\leq$ 10 cm water eq./year – all the deep ice core sites on the East-Antarcic plateau exhibit less accumulation.

RC 5, P5609-L21:

Please state the accumulation rate in m ice eq./yr for comparison to other ice core sites.

AC:

As the unit m ice eq./year is dependent on the the value adopted for the density of ice we would prefer to change the unit to m water eq./year which is common usage in the ice-core sciences as well. The numerical value of the annual mean accumulation rate at Kohnen station would only change by order of magnitude then, being 64×10^{-3} m water eq./year.

RC 6, P5609-L27:

What is a "spirit level"?

AC:

A device with a glass tube filled with liquid and a bubble of air to test whether a surface is level by the position of the bubble.

RC 7, P5611-L5-14:

This paragraph is excellent and useful. Describing the structure of the surface of the snow, and at what locations along the horizontal trench line, allows the reader to form ideas about how this may affect the isotope profiles in the vertical direction.

AC:

Thank you.

RC 8, P5611-L15:

Please also include a standard deviation value, in addition to mean, max, and min.

AC:

The standard deviation of d18O values over the entire trench T1 is 3.1 per mil, over entire T2 2.7 per mil. We will add this information to the manuscript.

RC 9, P5611-L19:

What is a "high" d18O value? In the next line, please give standard deviation, not variance.

This sentence is important, but very confusing. Likewise in line 23, what is a lower d18O value. Please use enriched or depleted.

AC:

We meant “high” and “low” in relation to the respective mean value. However, using “enriched” and “depleted” instead is more appropriate – thanks for this suggestion.

<u>RC 10, P5612-L2:</u>

What is an “isoline”? Please define somewhere above this sentence for clarity. The rest of the paragraph is similarly confusing, and because of its importance, it should be carefully re-written. Give accumulation rate in m ice eq.yr. Do “lateral layer profiles” refer to isolines? The nomenclature is difficult to follow.

AC:

An isoline is a curve along which some variable (here, d18O) has a constant value. We will add this definition to the paragraph. The lateral layer profiles are thus not identical to isolines since the former follow the seasonal maxima and not a specific constant d18O value. We will re-write the paragraph for clarification.

<u>RC 11, P5612-L23-24:</u>

What are “inter-profile deviations” referring to? Deviations of isolines? Try to use one common description, rather than many types. In general, I can interpret what the author means over the preceding two paragraphs, but it should be defined more clearly.

AC:

This paragraph discusses the d18O profiles of T2 (Fig. 2) – we will add “d18O” in line 22 to clarify this. We will change “inter-profile deviations” to “differences between the profiles”.

<u>RC 12, P5613-L2-5:</u>

I cannot understand what this sentence means: “On the horizontal dimension of the trenches, the observed lateral variance (Fig. 3) reflects processes that are not related to variations of atmospheric temperatures as these are coherent on this spatial scale. According to the terminology adopted here, the lateral variance is non-climate noise.” Do you mean that local temperature and regional atmospheric circulation should cause variations in vertical isotopes profiles, while horizontal profiles are affected by something else, such as post depositional *movement superimposed on the natural climate variability? Also, please do not use "lateral", as this can mean "side-to-side" in the vertical or horizontal direction, and when used on its own, is confusing to the reader. Try to define nomenclature early in the paper, and stick to that nomenclature throughout.*

AC:

Yes, you understood it correctly. However, we will re-phrase the sentence to make it easier to understand. In addition, we will add a paragraph to the "Data and Methods" section introducing the coordinate systems used in the manuscript together with a corresponding nomenclature.

RC 13, P5613-L17-25:

For this paragraph: 1) The first sentence repeats previous rationale. 2) In line 22, a mean of what? Units? It is unclear what is being discussed at this point. 3) Why do you call this "classical"? Can you include a reference? 4) In line 25, the author mentions vertical shifting, but it is not entirely clear why this is introduced? Is this peak matching with a max shift of 12cm? The entire paragraph needs to be clarified.

AC:

We will re-write the entire paragraph. In detail we will make the following changes: 1) We will shorten the first sentence. 2) In line 22, we discuss the correlations between single profiles of T1 and single profiles of T2. Hence we will write "mean correlation of ..." instead of just "a mean of ..." for the sake of clarity. 3) We called snow pits "classical" opposed to our more extensive two-dimensional sampling in the trenches. However, as this might be mis-leading we will remove the word "classical" and will include the reference to McMorrow et al. (2002) as an example of a snow-pit study. 4) Allowing for a vertical shift before correlating a profile of T1 with a profile of T2 is necessary as we don't have an exact height reference of T1 relative to T2. We will introduce this at the beginning of the paragraph.

RC 14, P5615-L5:

By "independent of the signal", do you mean the climate signal?

AC:

Yes. We will add the word "climate" for clarification.

RC 15, P5615-L24:

It might be worth noting that the missing d18O winter values could have been a winter where very little precipitation fell (the seasonality effect).

AC:

This is indeed a possibility and we will add this to the manuscript.

RC 16, P5617-L14:

Spatial precipitation intermittency on scales of km's is not relevant to this study as the trenches are only spaced at 500m.

AC:

We agree to remove this part as we explicitly discuss possible causes of lateral isotopic variance only for the spatial scale of the trenches.

RC 17, P5618-L3:

*The attenuation of the signal with depth *must* be mainly explained by diffusion. Using the term 'likely' disregards physics. I think this paragraph can be shortened considerably to say: diffusion attenuates the signal with depth, and in the upper few meters, ventilation can cause even larger attenuation of the signal.*

AC:

We will shorten the paragraph considerably as you suggest (including an entire removal of the diffusion model).

RC 18, P5618-L28:

What do you mean by "the remaining correlation"?

AC:

We meant the correlation that remains after the small-scale stratigraphic noise is decorrelated. We will rephrase the sentence to make this clear.

RC 19, P5619-L22:

What "criteria"? You mean, "the following criteria"? Or something else?

AC:

We will thoroughly rewrite this part to clarify what is being done here; see also answer to

RC 13 of referee #1.

RC 20, P5620-L1:

At this point, I have become somewhat lost. While the larger picture remains clear, the details are confusing. For example, "representativity" is difficult to interpret in many instances.

AC:

We will shorten and simplify the discussion of Fig. 7 to make the general picture more clear to the reader. Regarding the term of representativity that is introduced, we will emphasize the physical interpretation of the term as being an upper bound for the correlation with local temperature. We bear in mind that meteorology (storm tracks, moisture source, etc.) and possibly other effects complicate this simple interpretation. Hence, the representativity can be at most an upper bound. Please see also our answer to RC 14 of referee #1.

RC 21, P5623-L5-7:

*You must state in this sentence that the interpretation of firn-core-based climate reconstructions is challenging for *low accumulation sites* and state what accumulation value(s). For high accumulation sites, the interpretation is quite straightforward. As this important sentence is written, it is mis-leading.*

AC:

We will add the information that this is true for low-accumulation sites ($\leq$ 10 cm water eq./year).

RC 22, P5625-L22:

It should be clarified that low accumulation firn cores do not show a coherent signal at high-frequencies (i.e. probably at sub-decadal scales, depending on the accumulation rate).

AC:

We will add to our statement "single isotope profiles obtained from low-accumulation regions are poorly correlated and do not show a coherent signal" that this applies, based on our data, at least to sub-decadal time scales.