

# ***Interactive comment on “Regional climate signal vs. local noise: a two-dimensional view of water isotopes in Antarctic firn at Kohlen station, Dronning Maud Land” by T. Münch et al.***

## **Anonymous Referee #1**

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## **Overview**

In this work Münch et al present a high quality – high resolution dataset of water isotopic ratios from an extensive array of cores covering the top 1 m of firn at the Kohlen station in Dronning Maud Land, Antarctica. By comparing the isotopic profiles the authors attempt to address the question: “how representative of the initially precipitated isotopic signal is the measured profile of a single firn core?”. Previous snow pit and firn core studies have demonstrated that isotopic profiles from neighboring locations can possibly present a significant inter-profile variability. This variability is due to post depositional effects that primarily depend on the local surface topography and the intensity

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of surface snow reshuffling. Similar effects have been observed for proxies other than the water isotopic composition of polar firn, like the electrical conductivity as well as several chemical impurities.

This work is innovative and deals with a problem that is significant for the interpretation of polar ice cores as paleoclimate records. It contains an impressive amount of high quality data as well as interesting statistical methods for the evaluation of the representativity of a single or multiple isotopic profiles from polar firn. As a result I believe it should certainly have a place in the Climate of the Past archive and recommend it should be published. Despite the excellent material however, the manuscript suffers from some important issues where the authors should further look into. In general these can be outlined in the following 4 points:

#### General comments

- 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.
- 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

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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 inter-profile 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  $\delta^{18}\text{O}$  signal to temperature still remains.

- 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.
- 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

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[Interactive  
Comment](#)

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.

- Last, though not as important, it would be nice presenting some of the  $\delta^{18}\text{O}$  profiles from T1 so the reader has a feeling of how the time series look.

Due to all the points above I recommend publication with some major revisions necessary. I do believe this material has excellent potential and should eventually be published in CP. However both the manuscript and the readers will benefit from a clean-up of the text and a consideration of some of the points I mentioned above.

### Specific comments

Here some more specific comments for the authors.

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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  $R^2$  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  $\sigma^2$  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.

“Significantly higher density” Maybe an estimate?

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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?

P5612–L22 and Figure 2

The P–P values of the T2  $\delta^{18}\text{O}$  profiles are about 10 per mile lower than of those from T1. Can you maybe comment on this?

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  $\geq 10$  m. Does this imply something for the choice of the AR-1 approach for your lateral noise?

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?

P5617–L8

Preferably replace “m-scale” with “meter–scale”

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Discussion Paper



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, something allowed by the extremely open porosity of the upper firn.

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?

P5618–L23

I guess that you need a sinusoidal  $\delta^{18}\text{O}$  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.

P5619–L2

Is the 1km value an educated guess?

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11, C2799–C2810, 2016

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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.

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?

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.

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11, C2799–C2810, 2016

Interactive  
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Interactive Discussion

Discussion Paper

C2806





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.

P5621–L10

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

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.

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.

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.

C2807

CPD

11, C2799–C2810, 2016

Interactive  
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P5624–L5-7

I suppose you would require that the  $\delta^{18}\text{O}$  signal is stationary in order to make this statement?

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.

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  $\delta^{18}\text{O}$ .

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11, C2799–C2810, 2016

Interactive  
Comment

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Discussion Paper



C2808

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 re-deposition) of snow causing the local variability of the accumulation.

## 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  $\varepsilon$ ,  $\tilde{\varepsilon}$ ,  $\varepsilon_x$ ,  $\varepsilon_y$ ,  $\sigma_{\bar{x}}^2$ ,  $\sigma_{\bar{x}}^{*2}$  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:

$$X_n(z) = S_n(z) + w_n(z) \quad (1)$$

where  $n$  the index for core  $n$  drilled at lag  $\tau_n$ . 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  $\bar{\varepsilon}_n(z)$ .

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

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

where  $\bar{w}_n(z)$  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:

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

C2809

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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  $\overline{X}(t)$ , you run the indexes from 1 to  $N$  but for some reason the variable  $n$  is kept in the subscript. Is this correct?

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