Submission of revised version of the manuscript:

TC-2017-35:

Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles

by Thomas Münch et al.

Dear Joël Savarino,

enclosed you will find the revised version for our paper along with a version that highlights the changes that we made compared to the first version, created with `latexdiff`. Once more we would like to thank you as well as both reviewers for the many helpful comments that helped to improve our work.

In the following, we first list a summary of the implemented changes, followed by a one-to-one response to all review comments together with the changes accompanied with each of them. We marked answers where we made additional changes compared to our published review replies by using *italic font*.

Kind regards, on behalf of all co-authors,

Thomas Münch

Summary list of changes:

- We revised the introduction in order to improve its structure, and to elaborate more on the relation of expected effects from post-depositional processes and the discrepant interannual isotope and temperature variations observed Kohnen. We also include now the description of precipitation intermittency, but also argue why we focus our paper on post-deposition.
- Section 2.4: We added the standard error of the annual accumulation rate measurements from snow stakes.
- Section 2.5: We stated the limit of our diffusion modelling approach to isothermal firn.
- Results 3.2: We clarified that the effect of diffusion and compression on minimization of the T13/T15 profile deviations is much less than the effect of downward-advection, but nevertheless significant.
- Results 3.3: We improved the description of our results concerning the KS tests.
- Discussion: We enhanced the discussion of the difference curve (Fig. 6b) in relation to firn ventilation. We now emphasize that we can significantly constrain additional post-depositional effects, but that qualitatively effects could still be present but are not clearly detectable.
- Abstract, discussion and conclusions: We slightly toned down our main conclusion to reflect that overall our study cannot exclude additional post-depositional effects but only constrain their magnitude to be below the (residual) stratigraphic noise level.
- We edited the text throughout the paper to improve language and wording.

Reply to the Review Comments of Anonymous Referee #1

on the manuscript

TC-2017-35: Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles

by Thomas Münch et al.

We are very grateful for the enormously careful and thorough review of our manuscript and for the many detailed and constructive comments that will help to improve the work. Below there is a point-by point response to both the general and all specific comments raised by the referee. The original referee comments are set in normal font, our answers (author comment, AC) are typeset in blue.

General comments

This article presents new measures of isotopic compositions (d18O, d-excess) in the first 2 meters of snow at Kohnen (Antarctica). These measurements are used to evaluate how the isotopic signal is modified with time (over a two-year interval), after deposition, at this site. The authors also present a simple model including 3 post-deposition processes, and use it to simulate the evolution of d18O values for the same period of time. The model and data results are coherent with each other. The authors conclude that no other processes (besides these three) are necessary to account for d18O evolution in the snow layers. Besides this study of post-deposition, the authors compare the spatially averaged d18O profile in the snow to measured temperature evolution (AWS) and note a strong discrepancy. Since post-deposition processes do not explain this discrepancy, they propose that processes before or during deposition have to be investigated.

I recommend that this paper be accepted with moderate revisions.

1) The data presented here are crucially needed at the moment. They not only represent a huge amount of field work and analysis, but also respect a carefully designed set-up to ensure the quality of the signal retrieved by minimization of horizontal noise. Such high-quality data are exactly what is required to evaluate quantitatively the impact of post-deposition processes.

2) The quantitative evaluation of the three processes studied through minimization of RMSD is clear, and the magnitudes obtained are coherent with independent estimates.

3) However, the articulation between the strategy of the field experiment and the broader issue of the discrepancy between interannual temperatures and interannual d18O could be more detailed in Introduction.

4) The authors could nuance their conclusion that post-deposition processes are unable to produce the interannual variability of d18O observed. Only three processes have been evaluated quantitatively, the others are rejected based only on qualitative observations (and are still subject of research).

AC:
We are happy that the referee considers our data and the work to be significant and important. Nevertheless we also acknowledge the mentioned generally weaker points of our manuscript. We will revise the work to improve the introduction (improving the elaboration of strategy of field work vs. broader issue of discrepant inter-annual temperature and d18O variations + including the discussion of precipitation intermittency (see specific answers below)). Further, we will tone down our conclusions to account for the stated detection limit of additional post-depositional changes (see also our reply to Anonymous Referee #2) and for the qualitative nature of some part of our results.

__________________________Specific comments___________________

ABSTRACT

O_____'Here we reject the hypothesis of post-depositional change within the open-porous firn beyond diffusion and densification.' This sentence is unclear. Is it possible to use affirmative form?

AC:

We apologize that the sentence was not sufficiently precise. We will rephrase it to: "Here, we investigate the importance of post-depositional processes within the open-porous firn and find that further modifications besides those arising from diffusion and densification are unlikely."

We have split the suggested revision in two parts (first part here at the mentioned point, second part in 2nd half of abstract): "Here, we investigate the importance of post-depositional processes within the open-porous firn (> 10 cm depth) by separating spatial from temporal variability." + "Beyond that, we find further modifications of the original isotope record to be unlikely, or small in magnitude (<< 1‰ RMSD)."

O_____'These results show that the discrepancy between local temperatures and isotopes most likely originates from spatially coherent processes prior to or during deposition, such as precipitation intermittency or systematic isotope modifications acting on drifting or loose surface snow.' Why did you choose to evaluate post-deposition processes and not precipitation intermittency in this study? The latter is a strong candidate for the observed discrepancy. Is it due to a lack a measurements?

AC:

Yes, it is indeed the lack of measurements that prevents a quantitative evaluation of the precipitation intermittency. Over the year, measurements of accumulation are only available from the automatic weather station, the data however are strongly influenced by noise due to dune movements and snow drift. Snow stake measurements are only obtained in summer (thus, only record annual mean accumulation rates or at most the summer snowfall over the relatively short periods of the field seasons) and in addition are not available over the complete period of the trench records. Finally, the reliability of reanalysis precipitation amounts is unclear. We think that all these information are too detailed for the abstract. However, we will add a summary to the introduction in order to motivate why we focus our study on post-deposition.

INTRODUCTION

O_____When you say that diffusion and condensation 'only smooth and compress the original signal', you should precise that you are talking about vapor diffusion against isotopic gradients.

AC:

It is indeed a good point to precise to which diffusion process we refer here. However, to our knowledge the term "against isotopic gradients" is not common in the literature. Diffusion rather acts "down" the (concentration) gradients. We will change the sentence to "The isotope ratios of buried snow are affected by firn densification (…) and by diffusion of interstitial water vapour driven by gradients in the isotopic composition (…)".

We now have formulated the sentence more generally to account for the respective editor comment on this issue: "(...), however, these processes to not lead to any net change in the isotopic composition."

O_____ 'In contrast, the low local annual accumulation rates and potential seasonal intermittency of precipitation increase the time the surface is exposed to the atmosphere (Town et al., 2008; Hoshina et al., 2014) and therefore to processes that might alter the snow's original isotopic composition.' The intermittency of precipitation does not only favor post-deposition processes through exposition to the atmosphere; it can also shape the d18O signal because of irregular accumulation.

AC:

This is of course correct. We will add the discussion of precipitation intermittency to this paragraph (p.2, ll. 10-27) of the introduction.

O_____‘These processes can act either on loose snow in the post-condensational phase (falling or drifting snow), . . .’ Could you precise which processes are active then? It is not wind redistribution, since these processes have to be spatially coherent.
AC:
Falling or drifting snow at the surface might be already influenced by fractionation due to sublimation and condensation processes, similar to deposited surface snow as mentioned later in this paragraph of the introduction. We agree with the reviewer’s apparent impression that the logical order of the paragraph is not optimal and we will revise the entire paragraph to improve this.

O_____‘This discrepancy stresses the importance of contributions other than regional temperature alone to the formation of the isotope signal. /// In this study, we investigate whether post-depositional isotope modifications in the open-porous firn contribute to the observed discrepancy between isotopes and local temperature at Kohnen Station.’ This transition is very short. Could you indicate briefly what are the other contributions and why this study is dedicated to post-deposition?
AC:
We will provide a link here to the processes discussed in the first part of the introduction (see comments above) and then explain why we now focus on post-deposition (basically since we lack precise measurements to evaluate precipitation intermittency, see also our comments above).

Figure 1.

O_____Do you have information on precipitation amounts over this period? Or on summer d18O in the snowfall? Does the summer d18O in the snowfall follow the evolution of summer temperatures? If precipitation amounts are unknown, please state it here, not later in the Discussion. . . It will be easier to understand why you focus on post-deposition processes.
AC:
No, unfortunately we do not have information on precise precipitation amounts over the period of the trench data as explained in our reply to your second comment on the abstract. We also have no precise information on seasonal timing of precipitation, only the qualitative observation from the field seasons that there is little accumulation in the sommer months. For this reason, we also lack systematic measurements of summer snowfall which could be compared to temperature observations. We will add a summary of these information to the introduction in order to motivate why we focus the manuscript on post-deposition (see above).

O_____‘. . .we have designed our study such that it allows for the first time to quantitatively follow the isotopic changes and thus to test for post-depositional effects over a time span of 2 years.’ What do you expect for the evolution of the variability over 2 years? An attenuation or an amplification? If you expect only an attenuation, then post-deposition is obviously not responsible for the discrepancy between temperature and d18O interannual variations (attenuating a flat profile will not lead to increased variability). If you expect amplification, then why do you simulate only ‘attenuating’ post-deposition processes?
AC:
This is a very good point. However, we do not expect only attenuating effects. Post-depositional processes depend on other climatic features than temperature alone, such as wind speed, time the surface layer is exposed to the influence of the atmosphere, radiation, humidity, surface topography, etc., and could imprint these features to the isotopic signal in the firn. For inter-annual variations of these climatic features one would then also expect post-depositionally driven inter-annual variability of the isotopes. We will add these ideas to the manuscript at the end of the 2nd paragraph of the introduction.
In addition, we simulate the known influences of downward-advection, diffusion and densification – processes which are certainly at play and of which only diffusion is attenuating – not because we only expected attenuating effects but to disentangle the effects of these three processes from any further post-depositional effects.

We have added this additional information on our analysis strategy to the last paragraph of the introduction.

RESULTS

Table 2.

O_____ 'The higher variances in vertical direction of the T15 records are partly expected for autocorrelated data in combination with a larger record length,' It seems also stronger for the horizontal variability. Do you have an explanation for that? There is also a strong increase of the signal-to-noise ratio. Does it mean that the mean profile in 2013 is less well known?
AC:
We will add the confidence intervals of the variance estimates (using the effective degrees of freedom to account for the autocorrelation of the data). This shows that the variance estimates are not significantly different from each other. The uncertainty of the signal-to-noise ratio estimates is given by one standard error. The different trench valus are hence also likely not significantly different. We will add the relevant information to the table caption.

Figure 3.

O_____Considering only the part of the profiles that is complete, there seems to be an increase of d18O with depth. The shallowest winter (24 cm) has a very low value compared to the deepest winter (153 cm). There is a similar trend for summers (-37‰ for the summer at 173 cm and -44‰ for the summer at 33 cm). Is it possible to test this trend with a linear regression? Do you have information on the continuation of this trend at greater depths? If this trend is verified, what process could be responsible of such an increase?
AC:
Yes, testing on trends is of course possible. We tested the average T15 trench summer maxima and winter minima for a linear trend. The seeming increase in isotopic winter minima is not strongly significant against depth nor time ($p = 0.1$). In contrast, the trend in isotopic summer maxima is significant both against depth and time ($p < 0.01$). This trend cannot be explained by summer temperatures (see Figure 1), but it could be caused by changes in other climatic parameters such as the amount of summer snowfall. For greater depths, we only have preliminary data from our trench campaign which however do not show any continuation of the summer trend.

We have added the finding that the summer maxima show a significant trend over depth that is not captured by the evolution of summer temperatures recorded by the AWS.

Figure 4.

O_____It is really difficult to compare quantitatively the two curves on this figure, because they are not superposed. Could you put them on the same d18O scale, and shift the 2013 curve 'optimally'?
«<Figure 4: superposed»> see attached figure (Figure 1)
AC:
We do not agree with the reviewer on this point. It is certainly correct that the visual comparison of the profiles would be improved by superposing the plots. However, putting the plots on top of each other using only one y axis could visually imply that the profiles originate from the same expedition which is not the case. For that reason we decided to offset the plots vertically with respect to each other and use separate y axes (we noticed however that both axes do not have the exact same scale; we will change this to facilitate the comparison). Putting the plots on the same y axis and in addition using the optimal shift, as suggested by the reviewer, would preempt part of our results at this point of the manuscript. By contrast, our aim here is to show both mean profiles on their original depth scale, and from this point on discuss the different processes (downward-advection, diffusion, densification) that finally lead to Figure 6b. In summary, we suggest to leave this part as it is but leave it to the editor to decide on this issue.

We left the figure as is, considering the editor comment on this issue, but enforced same y-axis scaling for both plots.

O_____ 'In the 2 years, the T13 isotope profiles are advected downwards, compressed by densification and smoothed by firn diffusion.' Attenuation is not very clear here. There is attenuation between 75 and 120 cm depth (blue zone). However, between 60 and 75 cm depth and also between 125 and 150 cm depth the profile after two years (2015) has larger amplitude (red zones). Adding attenuation to the initial d18O profile from T13 would increase the agreement in the blue zone, but decrease the agreement in the red zones.

AC:
We admit that this part was ambiguous and thank the reviewer for pointing towards that. The cited sentence was not intended to express a result but the expectation that these three processes are at play and must be quantitatively investigated first before one can assess the significance of further post-depositional changes. We will rephrase the sentence to make this clear: "Within the 2 years, we expect that the T13 profiles are advected downwards, compressed by densification and smoothed by firn diffusion."

Figure 5

O_____ 'For the downward-advection, we apply vertical shifts between Δ = 40 and 60 cm,' This range is too large to stay within the bounds of the first winter minimum (47-53 cm would be enough) and too small to permit the shifting of the curve by one cycle (shift of 25-75 cm required). How is it possible that 60 cm become an optimum (it should lead to anti-correlation)?

AC:
The referee's estimates are totally valid and correct but are based on the trench data. However, to find the optimal parameter set of advection, diffusion and densification, we want to be as independent of the trench data as possible and therefore choose the values of vertical shifts accordingly. We will add this argument to the manuscript. A spaciously choice for the possible downward shifts are those values that cover the estimated range of annual accumulation rates observed in the wider vicinity of Kohnen Station (20-30 cm), as given in section 2.4. We see no motivation for further narrowing or enlarging the range of tested downward-advection values.
A vertical shift of the T13 profile by 60 cm can in fact be locally an 'optimal value' but only in combination with strong diffusion and densification. Shifting the profile by 60 cm alone indeed leads to anti-correlation and a high deviation from T15 (rmsd > 3 ‰), see attached Figure A1. However, for the combination of a large diffusion length (8 cm) and a strong compression (10 cm), this shift leads to the smallest possible deviation from T15 (rmsd ~1.3 ‰, upper right corner of Figure 5) since the strong diffusion essentially flattens the profile and the strong compression counteracts most of the anti-correlation that results from the vertical shift alone (Figure A1).

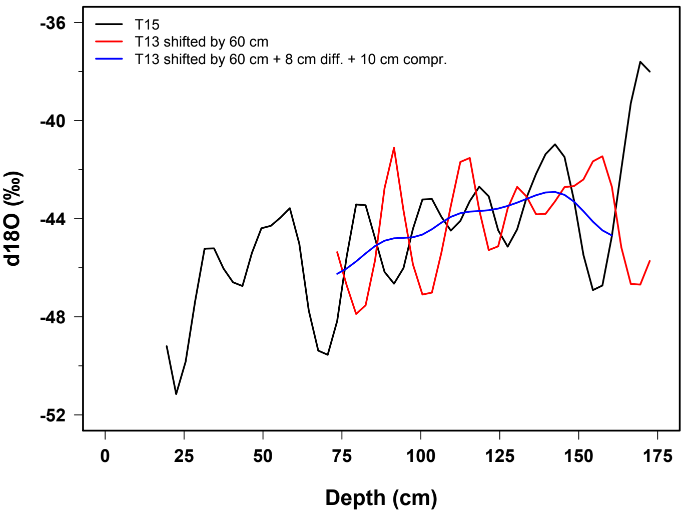

Figure A1: The T13 mean profile shifted alone by 60 cm (red), and shifted by 60 cm, diffused by a differential diffusion length of 8 cm and compressed by 10 cm (blue), as compared to the T15 mean profile (black).

O_____ Compression higher than 6 cm or diffusion length higher 4 cm leads to RMSD

higher than ‘doing nothing’ (1.05 at the point of origin). This is interesting as it gives an upper bound for the impact of these processes. It also confirms the estimates from independent datasets.
AC:
Yes, this is absolutely correct. It is also mentioned in the text that the optimal parameter values found from varying the parameters across their ranges are close to the ones that we obtained fully independently from the trench data (p. 10 ll. 10-14).

In addition, we now explicitly added to the text that the 1.07‰ contour line represents an upper bound for the magnitudes of diffusion and densification.

Figure 6

O_____ ‘We obtain the best agreement (RMSD = 0.92 ‰, Fig. 5; r = 0.93) between the T15 and the modified T13 mean profile (= T13*) for the optimal parameters Δ_{opt} = 50.5 cm, σ_{opt} = 2.3 cm and γ_{opt} = 3.5 cm (Fig. 6).’ Even if adding attenuation generally increases agreement with 2015, is it really the best scenario to apply here (considering red zones)? If the diffusion length was computed only on the zone where attenuation is evident (between 75 and 120 cm) would it have the same value?
AC:
This is a very good point. Of course it is possible that the best-possible fit we obtained does not represent the correct physical processes, thus is right for the wrong reasons. However, the agreement of the parameter values between our best-fit and the independent estimates argues against this possibility. To compute the differential diffusion length σ only for the “blue zones” certainly yields a different result as the one found in the manuscript but this value would (1) be more uncertain since fewer data are used for the estimate, and (2) it would represent a subjective choice based on knowledge of the trench data. We consider it important to reach our statistical conclusions with the least possible amount of data-based presuppositions.

O_____ Did you try to move the profile of T13 vertically (more or less enriched in heavy isotopes) to get a better fit? Of course the processes tested here would not lead to a change in the mean value, but it could give information on other processes (maybe for discussion).
AC:
We repeated the analysis for Figure 5 looping over different isotopic mean shifts of the T13 mean profile (from -1 to +1 ‰ in steps of 0.1 ‰). Indeed, shifting the mean value of the T13 mean profile results in a further reduced RMSD with the T15 mean profile (Figure A2). We find a new minimum RMSD for a shift in mean value of -0.4 ‰. The associated optimal parameter values of downward-advection, diffusion and densification are with Δ_{opt} = 50.5 cm, σ_{opt} = 2.4 cm and γ_{opt} = 3.4 cm equal or similar to the ones obtained without shifting the mean. However, we have no possible explanation for such a change in mean value, but still we think that this finding is worth adding to the discussion and thank the referee for raising the issue.

Indeed, we aded this finding to the disussion (p16, ll17-20).

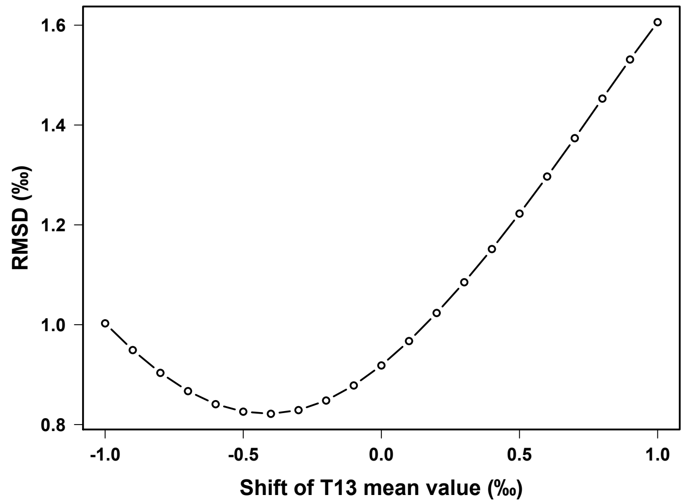

Figure A2: The RMSD between the T13* and the T15 mean profile as a function of the shift in the mean of the T13 profile.

O_____ Could you give us an estimation of the attenuation due to diffusion? It could be useful for future comparisons (to other data or models). Roughly from the graph (T13*), the half-attenuation seems to be of ~0.6 ‰ and the initial half-amplitude of about 2.2 ‰ which would correspond to a quite strong attenuation, of the order of 25 % over two years. What would be the attenuation in the 'blue zone': 75-120 cm depth?

AC:

Many thanks for these estimates. We have attached the Figure A3 showing the typical exponential decline of the seasonal amplitude with depth according to the local depth-dependent diffusion length (Kohnen Station parameters) and a seasonal cycle with wavelength of 25 cm (range of 20—30 cm). At 1m depth, the seasonal amplitude has been reduced to ~ 75—85 % of its initial value at the surface, at 1.5m depth the reduction is ~ 60—80 %. These results are well captured by your rough estimate from the data.

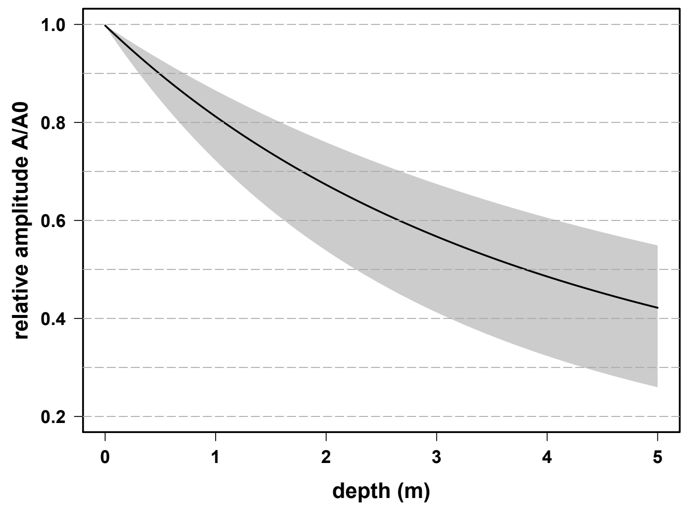

Figure A3: The relative decline with depth in seasonal amplitude due to diffusion for Kohnen Station parameters for the upper 5 m of firn. The black line shows the amplitude reduction for a seasonal cycle with a wavelength of 25 cm, the shading gives the reduction for wavelengths from 20 to 30 cm (lower to upper bound).

O_____ p10 l20: 'can be seen' and l21: 'clearly': It would be easier to see the improvement if there were somewhere a figure showing T13 (unmodified) and T15 superposed. Without this figure, the term 'seen' should be avoided/replaced.

AC:

We agree that in this part it might be a bit hard for the reader to follow our statements. However, all the necessary information is contained in Figure 5 – just shifting T13 optimally is represented by the lower left corner of the figure, adding diffusion and densification improves the RMSD towards the black dot. We will add these information to the manuscript to guide the reader more carefully. In addition, we will rephrase the sentences to avoid the terms 'can be seen' and 'clearly'.

The entire part has been reformulated in the revised version.

O_____ 'Nevertheless, both processes play a significant role in explaining part of the temporal changes. This can be seen if we only shift the T13 mean profile vertically to find the maximum correlation with T15. . .' Is the RMSD of 'only compression' different from the one of T13*? How much improvement is obtained by adding the diffusion to the 'compression only' experiment?

AC:

Yes, according to Figure 5, the minimum RMSD of 'only compression' is 0.98 ‰ (Figure 5, along the vertical axis the minimum RMSD is found for a compression value of 3 cm). Thus, adding diffusion improves the match slightly by 0.06 ‰. This is a small value but nevertheless we think that diffusion explains at least some part of the temporal differences between T13 and T15, especially when taking into account that the diffusion model near the surface could very likely be further improved by accounting for different diffusion lengths for summer and winter layers (see discussion). However, this approach is beyond the scope of our study. In any case, we will weaken the statement at this point of the manuscript to reflect that the gain in RMSD by adding diffusion is small (which is also the case for adding compression alone).

We explicitly note now that the gain in RMSD is small, but elaborated why we think it is still significant.

O_____ 'deviations especially remain around the isotopic extreme values, in particular for the first overlapping cycle and the depths around 100 and 125–140 cm.' As expected, the deviation after post-deposition is high mostly in the red zones (first cycle, 125-140 cm), where the amplitude in 2015 is larger than the amplitude in 2013. For these zones adding diffusion leads to higher deviations than doing nothing (and the term 'remaining difference' is maybe not the best adapted).

AC:

Yes, it is correct that our model increases the deviations in some parts of the profile overlap, however, in total it minimizes the root mean square deviation and thus is overall still the best-case scenario. We will improve the discussion of our results at this point to take that into account, especially we will emphasise that the partially increased profile deviations are expected since some T13 amplitudes were already initially smaller than for T15, and thus diffusion cannot lead to an improved profile match here. We will thus replace the inappropriate phrase 'remaining differences'.

O_____ What do you call 'extreme values'? All the extremums? Or only the summer at 175 cm and the winter at 70 cm? If you are talking about the extremums, then there is a contradiction with p. 14: 'Furthermore, the difference curve (Fig. 6b) does not show any clear seasonal timing. . .'

AC:

This is a good observation, thank you for pointing towards that. Indeed, our statement here was too generalized and thus in contradiction to the statement on p. 14. We will weaken the statement and rewrite the sentence to replace the term 'especially remain at'.

O_____ 'This gives a best shift of 48.5 cm, but clearly the agreement is less pronounced (RMSD = 1.1‰ r = 0.88) compared to. . .' On the Figure 5, at the point of origin (no diffusion, no densification), the RMSD is 1.05. It reaches 0.92 ‰ for optimal compression and diffusion. Thus these two processes are a step in the right direction, but finally do not improve the RMSD very much.

AC:

We agree that the gain in RMSD is not particularly high when adding diffusion and densification (see also comments above). As a consequence, we will weaken our statement here by stating 'This gives a best shift of 48.5 cm, but the agreement is slightly less pronounced (…)'.

The entire part has been restructured to account for the issues raised in the last five comments.

O_____ p. 10, l25: 'Taking these processes into account leads to a good match of the trench mean profiles (Fig. 6b). However, deviations on the order of 0.9–1‰ remain.' What were the deviations before taking them into account?
AC:
We agree that it is a good idea to guide the reader more carefully here. We will change the sentence in order to reflect that just accounting for downward-advection already yields a good match of the profiles, which is further improved slightly by adding diffusion and densification (with a gain in RMSD by ~0.15 ‰).

O_____ 'These can have two causes: firstly, additional temporal changes driven by unaccounted post-depositional processes;' Could you precise what other processes you are thinking about? Or maybe just make a note toward the section where these unaccounted processes are discussed? Listing possible processes could help to research specific features expected in the remaining variability.
AC:
We refer here to the processes discussed in the introduction (post-deposition such as sublimation and ventilation) and will add a link to emphasise that.

O_____ 'secondly, remaining spatial variability since we average a large but finite number of records which do not originate from the exact same position.' It seems coherent to evaluate the remaining variability as spatial noise, if this variability is random. However, it may not be the case here (slight trend toward higher values with depth, see below).
AC:
This is a very good point. However, we see no indication for *not* assuming that the spatial variability is random, thus white noise. Firstly, the slight increase of the difference curve towards higher values with depth (Figure 6b) is not significant (see below). Secondly, diffusion smoothes the noise and thus leads to autocorrelation, meaning that even white noise shows autocorrelation after diffusion. In other words, the autocorrelation of the difference curve hence does not invalidate the white noise assumption.
In addition, we use the statistical noise model as a first test to assess whether the differences between T13*/T13** and T15 can be explained by spatial variability. This test indeed assumes white noise between the trenches. However, as a second test we use the formal statistical KS test which yields the same result as the first test but makes no assumption about the coherence of the noise between the trenches.

We added that the second test makes no assumption about the covariance of the noise.

O_____ 'The agreement of both estimates indicates that the remaining profile differences between the modified T13 mean profile and T15 (Fig. 6b) can be entirely explained by spatial variability through stratigraphic noise. We note however that the squared RMSD lies at the upper end' If there is still a doubt in your mind after the mathematical demonstration, why do you use the term 'entirely' in the first sentence? This term also seems in contradiction with the end of the paragraph. To facilitate reading, you could add a layer of uncertainty such as: 'At first order, the agreement of both estimates indicates'
AC:
Thank you for spotting this inconsistency in our language. We will remove the word 'entirely' and add a layer of uncertainty to our statement, as suggested.

Figure 7

O_____ 'We find that the distributions of the spatial differences between the mean profiles of each trench campaign (T13–1 vs. T13–2 and T15–1 vs. T15–2, Fig. 7a) are statistically indistinguishable' Could you explicit the results of these tests with simple words? What is the more general conclusion of this first test? That the sampling strategy has no influence on the results? That the uncertainty is the same for T13 and T15?
AC:

We note that the cited phrase 'distributions (…) are statistically indistinguishable' is the formal and correct statement for the obtained result. In more simple words it means that there is no significant difference in spatial variability between each trench pair, thus between the two seasons. Alternatively, one can state that the spatial variability estimates from the trench pairs belong to the same underlying distribution (regarding mean and variance / location and width). We will add a more thorough explanation to the manusript to facilitate the interpretation of the result.

O_____ 'More importantly, the combined distribution of spatial variability is also indistinguishable from the distribution of the temporal differences between the T15 and the modified T13 mean profile' Does this test evaluate if the difference between T13** and T15 is more than just the difference between T13** and T15 that comes from having a different location?

AC:

This test evaluates if the (temporal) differences between T13** and T15 belong to the same underlying distribution as the (combined) spatial differences. Thus, the null hypothesis of the test is that the differences between T13** and T15 just arise from the fact that the trenches have a different location, thus that the temporal differences can be explained by spatial variability alone. We find that we cannot reject this null hypothesis. We will rephrase this part of the manuscript in order to facilitate the interpration of our results (in line with our last comment).

O_____ How do you 'combine' spatial differences between trenches? The distances considered are not exactly the same (~350 m between T13-1 and T13-2; ~500 meters between T15-1 and T15-2; ~200 m between the mean T13 position and the mean T15 position). Do you apply a weighting by distance?

AC:

No, we do not apply a weighting by distance. This is motivated by the fact that the change in spatial (horizontal) correlation of the stratigraphic noise is large in the first metres, but only small or even zero for larger distances (> ~10 m) (Münch et al., 2016). Thus, it does not matter if the trenches are separated by 300, 400 or 500 m. This is in fact also underpinned by the first KS test: we find that the spatial variability between the trench pairs belong to the same underlying distribution (see above), thus do not depend on the distance between the trenches. We will add this information to the manuscript at this point.

DISCUSSION

________Densification, diffusion and stratigraphic noise________

O_____ 'We found a strong resemblance...' This 'strong resemblance' is largely brought by moving downward the profile (advection). The impact of compression and diffusion, even if it is significant, is still very small.

AC:

Yes, this is correct. We will take this into account by emphasising that the major portion of the agreement achieved by our model is a result of the downward-advection.

O_____ 'our assumption of a linear profile compression with depth is certainly a rough approximation given the actually observed seasonal firn density variation (Laepple et al., 2016).' In what direction would that process intervene? Preferential compression of summers or winters?

AC:

This is a good question and worth mentioning in the manuscript. In Figure 6 in Laepple et al. (2016), there seems to be stronger densification (change in density) of summer compared to winter layers. However, the short data do not allow to estimate if this is a robust feature. In general, the seasonality of densification in Antarctic firn is very unclear (Laepple et al. 2016 and references therein).

O_____ 'In detail, the diffusion correction improves the match of the trench mean profiles in the medium depth range but also results in higher deviations of the profile minima at the top and bottom part of the overlap (Fig. 6).' This observation is much welcome but should have come earlier in the manuscript, when the deviations are first described.

AC:

Yes, absolutely. We wil change the manuscript accordingly as already outlined in our above answers to the comments relating to the same issue.

O_____ 'Part of this mismatch might be reduced by accounting for the seasonally varying firn temperature resulting in stronger (weaker) diffusion for summer (winter) seasons (Simonsen et al., 2011).' How exactly? Does this mean that summers would be more attenuated than winters (due to stronger attenuation when they are still at the surface)? What about temperature gradients? They might not only favor attenuation, but also redistribute heavy and light isotopes vertically.
AC:
Yes, indeed summers would be more attenuated than winters due to the higher surface temperatures, especially close to the surface where the difference in diffusion length between summer and winter is largest (see Figure 1c in Simonsen at al., 2011). We will enhance the discussion regarding this point. The effect of temperature gradients is subject of open research.

______________Additional post-depositional modifications________

O_____ '. . .any additional post-depositional changes of the isotopic composition of the firn, below 10 cm, must be on average clearly below the residual stratigraphic noise level, thus « 1‰˙' Thus the change can be of more than 1 ‰ as long as it goes on opposite directions at top and at bottom (the average being zero)?
AC:
No, this is not correct. The limit of 1 ‰ stated here refers to 1 ‰ RMSD. The RMSD is independent of the sign of the actual differences, thus opposite changes at top and bottom with the average being zero would still result in a non-zero RMSD (e.g., changes of +1 ‰ at the top and -1 ‰ at the bottom would result in a RMSD of sqrt(2) ~ 1.4 ‰, larger than the limit set by the stratigraphic noise level.) We will add the therm 'RMSD' to the cited statement in order to clarify this ("thus « 1‰ RMSD").

O_____ 'This conclusion is also supported by comparing the qualitative nature of the differences between the mean profiles (Fig. 6b)' Regarding this difference (violet curve): is it possible to add the zero line, to discriminate between positive and negative differences? Is it possible to add the difference T15-T13 (with optimum downward advection), to see where the post-deposition has been most effective?
AC:
This is a good idea. We will add the zero line and the second difference curve for the T15-T13 differences accounting only for the optimal downward-advection.

O_____ 'the T15 mean profile shows, if anything, more depleted 18O values compared to the T13** record (Fig. 6b).' Is this negative difference significant (see below d-excess)? If it is significant, does this mean that post-deposition, at this site, is characterized by a decrease of d18O values? What process could be responsible of this decrease?
AC:
A t-test accounting for the autocorrelation of the data shows that the overall mean difference (~ -0.45 ‰) is not significantly different from zero (p = 0.4). We will add this information to the manuscript. In consequence, we refrain here from discussing potential mechanisms that might be responsible for an overall negative difference since the differences are more likely just a random expression of the diffused stratigraphic noise.

O_____ 'Specifically for South Pole conditions (annual-mean temperature −50°C, accumulation rate 84mmw.eq.yr−1, surface wind speed 5ms−1), the firn isotopic composition showed annual-mean enrichment by firn ventilation after several years of ~3‰ (Town et al., 2008).' In as much as the first cycle of T15 reflects undisturbed isotopic cycle (-44 to -52‰ and annual average of -48‰), the annual average value after postdeposition (between 70 and 150 cm depth) is indeed enriched (-45 to -46‰) by nearly 3 ‰
AC:
Please see our author comment below (marked by an asterisk *).

O_____ 'For the first overlapping annual cycle, T15 exhibits an average difference from

T13** of −1.6‰ for the other annual cycles the averages are −0.4, ±0 and −0.1‰˙’
There seems to be an increase in values with depth, with the difference between T15 and T13** getting closer to zero. Is that trend significant?

AC:
The trend is not strongly significant ($p = 0.12$) when accounting for the autocorrelation of the data. We will add this information to the manuscript.

____i____ The T13 profile, and its derivatives (T13* and T13**) do not show this trend. If it is significant, it could mean that this trend is a result of a post-deposition process yet unknown, that could also be responsible for the overall depletion of T15 relative to T13 (or T13*, T13**). This process would be oriented, and would bring preferentially light isotopes to the top and/or heavy isotopes to the bottom.

AC:
Thank you for these considerations. Although the trend is not significant (see above), it is indeed an interesting observation which calls for further studies.

____i____ Qualitatively, sublimation (Sokratov and Golubev, 2009) is unlikely to produce this result; it would instead bring enrichment in the top layers. Oriented diffusion is also unlikely, because when it is active in summer, vapor moves downward, and would bring light isotopes to the bottom.

AC:
Thanks once more for these thoughts. We will consider to add these discussion points to the manuscript, depending on how well they can be incorporated into the present discussion.

On p16, ll15-21 we added that qualitatively, these observations might still indicated some post-depositional process, but that this is speculative.

____i____ The ventilation process as described by Town et al. (2008) could contribute to this trend: Town et al. (2008) show that the winters become more and more enriched after burial, at least until the influence of the wind becomes null (40 cm). Looking at Figure 6b, there seems to be indeed a trend toward higher winter values when depth increases (especially in the original ‘first 40 cm’ located between 60 and 100 cm depth).

____i____ Regarding the summers values, they are too low for the first two summers (T15 relative to T13*) and too high for the next (deeper) summers. This could be explained by ventilation too. The summers at shallow depth are first depleted because of condensation of ‘winter’ vapor during the winters. But later on, they can be enriched again by ‘summer’ vapor entering during subsequent summers. Since more vapour is available in summer, this influence would become preponderant when layers are buried more deeply. (In winter the atmospheric air would contain only little vapour that would condensate quickly/entirely in shallow layers and not reach these deeper layers).

AC(*):
Combined response to the last two comments as well as the one not answered above (*):

Thank you very much for these detailed considerations. You are partially right but mix two different observations. Indeed there seems to be a trend in T15 winter layers towards less negative values over depth. You are also correct in saying that the Town et al. ventilation study also shows enrichment of winter layers over time. However, the conclusion that the observed T15 winter trend over depth can thus be explained by post-deposition following Town et al. must assume that initially all winter layers looked like the first layer of T15 (or at least had similar initial minimum values). We have no means to validate this assumption. All we can do is directly compare the winter layers of T13* to T15 which are direct counterparts of the same season. If we only look at the first winter layer which was closest to the surface at the time of excavation of T13, thus presumably being under strongest influence of the atmosphere, we see a rather strong depletion of the layer after 2 years (the layer in T15 is more negative than the layer of T13* by about 2 ‰). This is just the opposite of what is suggested by Town et al.! Also the other winter layers either show no change (2nd and 3rd layer) or also more depleted values after 2 years.

Regarding the summers: Your observation is right that here indeed the change of the first three summer layers appears consistent with the Town et al. results. However, looking at annual mean differences, again we find no support for the possibility of ventilation (as stated in the manuscript, the first annual cycle is overall more depleted after 2 years by 1.6 ‰, not enriched as suggested by ventilation, since the winter layers don't show the "expected" strong enrichment overcompensating the summer depletion). In addition, we see no motivation for the hypothesis suggesting that ventilation at Kohnen Station should be only active in summer.

In summary, we see no clear support for post-depositional changes from firn ventilation that would be in line with the Town et al. study. However, of course we cannot directly reject the hypothesis since the effect might still be there but just too small to be detected by our analysis. Therefore, we will elaborate the discussion of the difference curve in more detail, as outlined here, and also attenuate our conlusions.

____i____ Of course all of this is very theoretical as long as we ignore the vapor isotopic composition in the atmosphere, and the direction of air fluxes.
AC:
Yes, indeed. That is why we also suggest in our final conclusions that future studies should combine measurements and analyses similar to ours with measurements of the atmospheric vapour isotope composition.

O_____ 'We note that the RMSD corresponding to the first value is above our stated detection limit.' See above («1‰
AC:
We are sorry but we do not fully understand what you mean with your comment.

O_____ 'Furthermore, the difference curve (Fig. 6b) does not show any clear seasonal timing which might be expected for a systematic post-depositional modification.' This affirmation could be nuanced. The maximum deviations (from zero) generally occur in phase with the extremums. The only case where the maximum deviation is not in phase (in front of the T15 extremums) is when the two curves T13** and T15 are not in phase with each other (110-120 cm) probably due to linear compression.
«<Figure 6b: annotated»> See attached figure (Figure 2)
AC:
We do not fully agree with these statements. This is almost true if one considers only the extreme values in general. If one looks in more detail (see attached figure A4, which is Figure 6b from the manuscript with the zero line added) only 3 out of 5 winter minima in T15/T13$^{*/**}$ isotope values coincide with a minimum in the difference curve, and only 2-3 out of 5 summer maxima coincide with a maximum in the difference curve. The remaining extremes in both cases either coincide with the opposite extreme in the difference curve or with a difference of around 0 ‰. For us it is thus reasonable to conclude that the 'difference curve does not show any clear seasonal timing'. We rather think that the difference curve appears to exhibit some kind of seasonality due to the smoothing effect of diffusion. However, the shortness of the data does not allow a formal test of this hypothesis. We will modify the discussion at this point of the manuscript to elaborate on our reasoning.

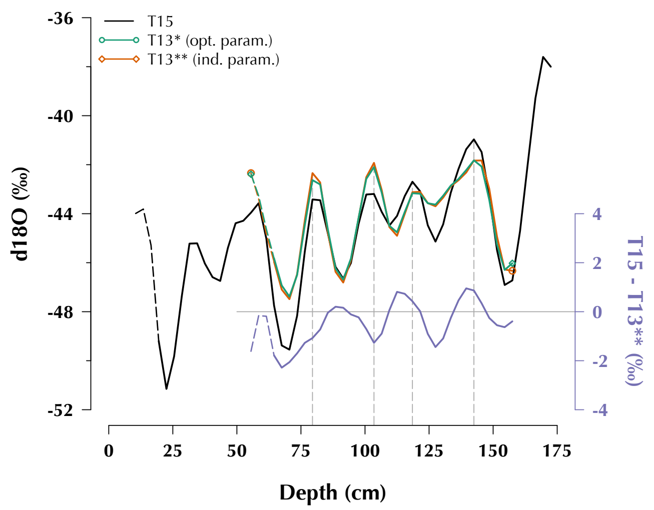

Figure A4: Same as Figure 6b from the manuscript but with zero line added on the difference curve (T15-T13).**

O_____ 'We nevertheless note the possibility that post-depositional changes by wind-driven firn ventilation are present at Kohnen Station but that their effect is unexpectedly weak and thus masked by the stratigraphic noise level.' See above («1‰

AC:

We are sorry but we do not fully understand what you mean with your comment.

O_____ 'Finally, we note the small tendency towards negative values of the differences between the T15 and T13** mean profiles (Fig. 6), What do you mean by 'negative tendency'? Is it the increase with depth or just the average of the differences between T15 and T13**?

AC:

We meant the seeming (thus insignificant, see above) increase of the differences with depth between T15 and T13**. We will rephrase the text to clarify this point.

O_____ '. . .we cannot reject the null hypothesis that both spatial and residual temporal differences originate from the same distribution,' This sentence is unclear, could you be more explicit ?

AC:

We apologize that this sentence is unclear. However, it is the correct expression of the result. Nevertheless we will rephrase/amend the respective passage to clarify the statement. Basically it means that all residual temporal differences are very likely attributable to spatial variability alone, and hence no further post-depositional processes need to be invoked to explain the mean and amplitude of the differences.

O_____ On Figure 7b, the 'spatial' difference and 'temporal' difference seem to have the same mean value (which seems negative). Did you made a test to evaluate if the average value is statistically different from zero, for the two variables? The fact that they 'originate from the same distribution' does not really prove that the average value is null for both, just that their averages are not statistically different from each other.

AC:

No, we haven't included this information in the manuscript. However, applying the t-test, taking into account the autocorrelation of the data, we find that the average differences are statistically not different from zero ($p = 0.4$). We will add this information to the manuscript.

This information has been added to the caption of Fig. (7).

O_____ Is the negative difference between T15 and T13** significant? (See above). If it is the case, then there is a contradiction between the two tests. If not, this negative difference cannot be used as an argument to select processes.

AC:

As given in our above answer, the average difference between T15 and T13** is not significant and there is thus no contradiction between the two tests.

O_____ On Figure 7b, the 'spatial' difference appears to have wider distribution than the 'temporal' difference. Does your statistical test include the width of the distribution?
AC:
Yes, the KS test is sensitive to differences in both location (mean) and width (variance) of the empirical distribution functions of both samples. We will add this information to the Methods section 2.5.

O_____ 'the histogram of the temporal differences is even more symmetric than for 18O.' This clearly supports the absence of new deposition processes. Is there a trend with depth for the d-excess values?
AC:
No, the difference curve for the d-excess values (T15 – T13**) does not show a significant trend with depth ($p = 0.4$, accounting for autocorrelation). The average difference is, as for d18O, also not significantly different from zero ($p = 0.9$, accounting for autocorrelation).

O_____ '(1) Seasonal variation and intermittency of precipitation cause the discrepancy between isotope and local temperature data (Sime et al., 2009, 2011; Persson et al., 2011; Laepple et al., 2011).' This hypothesis could have come earlier (in the introduction or when the discrepancy was described).
AC:
As we stated in our replies to the referee comments on the introduction, we will discuss the effect of precipitation intermittency on the isotope signal already in the introduction.

O_____ 'At Kohnen Station, a large part of the annual accumulation is assumed to occur in winter since little or no precipitation is observed in the summer field seasons. However, the exact seasonal and inter-annual variation of accumulation is still unclear due to the lack of sufficiently precise, year-round observations (Helsen et al., 2005).'
Idem
AC:
As described above, also the fact of our imprecise knowledge on seasonal and inter-annual variations of precipitation and accumulation will be included already in the introduction.

In addition, we added here that the surface height changes derived from the AWS sonic altimeters are difficult to separate into contributions from drifting snow and true snowfall.

CONCLUSIONS

O_____ 'The trench records show a pure downward-advection of the isotope signal within the open-porous firn, further influenced only by firn diffusion and densification, with no evidence for substantial additional post-depositional modification.' This conclusion is largely supported by the data, and the statistics. Quantitatively, the remaining difference can be accounted for by spatial noise, and thus there is no proof of another process active (and no need for it). Qualitatively, ventilation may still be happening.
AC:
We agree with the referee that our results still leave room for post-depositional changes – either with very small magnitudes so that their effect is "masked" by stratigraphic noise in our analysis, or occurring directly at the surface where we do not have sufficient data to assess this possibility. In consequence, we will weaken our statement here.

O_____ 'Year-long isotope studies (e.g. in seasonal intervals) focusing on the near-surface would help to constrain isotope modifications at the interface of surface snow and atmosphere.' Yes, more field campaigns, especially at this interface are acutely needed to understand what is happening.
AC:
We fully agree with you.

________________Technical comments________________________

p8 line 7: ‘T15-2 profile’.
AC:
We are not sure what you mean here. Reading the entire paragraph, we noticed that “T15-x profile” or “T15-x mean profile” has not been used in a consistent fashion. We will replace all occurrences of “T15-x profile” by “T15-x mean profile” throughout the text.

p10, l16: ‘deviations especially remain’ remove ‘especially’
AC:
We will remove the word ‘especially’.

This part has been rewritten in the revised manuscript.

p10, l30: “variability” miss an ‘a’
AC:
The typo will be corrected.

p12, l28: ‘occured during the 2 years’ misses a ‘r’.
AC:
The typo will be corrected.

p12, l3: ‘modified T13’ which one? Is it T13* like in the previous sentence or T13** like on the figure 6b? If it is T13**, could you also check the previous sentence, and give RMSD for T13** (for consistency)?
AC:
‘(…) modified T13 mean profile’ here indeed referred to T13*. However, for consistency we will also mention the squared RMSD value for T13** and will rephrase the sentence as follows:
“For comparison, the square of the RMSD between the T13* (T13**) and T15 mean profile is 0.85 (0.88) $(‰)^2$. The agreement of the estimates indicates that (…)”

P16, l2: verify ‘focussing’
AC:
We will change this part to “Year long istotope studies (…) *with a focus on* (…)”

p 16, l8: ‘averaging’ needs a second ‘a’
AC:
The typo will be corrected.

Figure 2. The labelling is too small for longitude, latitude, and for the core and trench names. Is it possible to add the general wind direction?
AC:
We will increase the label fonts and add an arrow indicating the mean wind direction (57°).

Figure 5.

O_____ ‘For each parameter set of compression and diffusion, we record the minimum root-mean square deviation of the profiles (contour lines) for the optimal downward-advection value (colour scale).’ From this legend it seems that only the (diffusion; compression) couples were tested (while in the main text it seems that all the parameters are varied independently). Could you clarify this point? Is the downward advection the parameter with the less impact on RMSD? This is suggested by not treating the parameters equally in this figure.
AC:
We apologize for the fact that the figure caption has been misleading. In fact, we independently varied all three parameters (downward-advection, diffusion, compression) according to the given ranges, as stated in the text. However, in this figure we show the results projected onto the optimal advection values, thus for each pair of diffusion length and compression value we only show the minimum RMSD from varying across the range of advection values. This choice was necessary in order to be able to display the three-dimensional parameter space in only two dimensions. In fact, the downward-

advection has the largest influence on the RMSD. We will improve the caption description in order to clarify these points.

References:

[1] Laepple, T., Hörhold, M., Münch, T., Freitag, J., Wegner, A., and Kipfstuhl, S.: Layering of surface snow and firn at Kohnen Station, Antarctica: Noise or seasonal signal?, J. Geophys. Res. Earth Surf., 121, 1849–1860, doi:10.1002/2016JF003919, 2016.

[2] Münch, T., Kipfstuhl, S., Freitag, J., Meyer, H., and Laepple, T.: Regional climate signal vs. local noise: a two-dimensional view of water isotopes in Antarctic firn at Kohnen Station, Dronning Maud Land, Clim. Past, 12, 1565–1581, doi:10.5194/cp-12-1565-2016, 2016.

[3] Simonsen, S. B., Johnsen, S. J., Popp, T. J., Vinther, B. M., Gkinis, V., and Steen-Larsen, H. C.: Past surface temperatures at the NorthGRIP drill site from the difference in firn diffusion of water isotopes, Clim. Past, 7, 1327–1335, doi:10.5194/cp-7-1327-2011, 2011.

Reply to the Review Comments of Anonymous Referee #2

on the manuscript

TC-2017-35: Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles

by Thomas Münch et al.

We thank the referee for carefully reviewing our manuscript and for the constructive comments that will help to improve it. Below there is a point-by point response to both the general and the specific comments raised by the referee. The original referee comments are set in normal font, our answers (author comment, AC) are typeset in blue.

The manuscript "Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles" by Thomas Munch and co-authors is devoted to the study of post-depositional changes of snow isotope composition in central Antarctica using the huge dataset of recently obtained data. The authors clearly demonstrated, using robust statistical methods, that the observed evolution of the vertical profiles of snow isotopic composition can be explained without significant influence of the post-depositional processes. In general, I enjoyed reading the manuscript and suggest that it may be published as it is, or with minor corrections.

I think the authors could slightly modify the main idea of the conclusion of the manuscript. In the current version they state "no evidence for substantial additional post-depositional modification", meaning that they do not expect post-depositional modifications stronger than 1 per mil for oxygen 18. Indeed, 1 per mil is a very small value comparing to the spatial variability due to the stratigraphic noise. But on other hand, if considering the post-depositional modifications of the whole annual snow layer, 1 per mil is rather big value – it's an equivalent of about 1.25 *C of air temperature change! Thus, the obtained results still give some room for the post-depositional modifications of the snow isotopic composition, although they are less than 3 per mil as expected from the modeling (Page 14).

AC:

We totally agree with the reviewer that our results make post-depositional changes in addition to diffusion and densification unlikely at our study stite, but still leave room for such effects of the order of <1 ‰, and also very close to the surface where our data are insufficient. But we have to bear in mind that this limit of 1 ‰ refers to the root mean square deviation of the T15 and T13 profiles (after accounting for diffusion and densification) calculated over the entire record's overlap, thus on the seasonal scale. If we consider annual means, this value should be much smaller. However, still we will tone down our conclusions by stating that additional post-depositional changes appear unlikely, but that we can only constrain this to changes down to the order of less than ~1 ‰ RMSD on a seasonal basis.

Other comments or corrections:

Figure 1 would be more informative if you add a wind rose, or just an arrow showing the prevailing wind direction.

AC:

(We assume that Referee #2 refers to Figure 2 here). We will add a wind rose and an arrow indicating the prevailing wind direction (57°) to this plot.

Page 14, line 11, "Sublimation led in lab studies. . ." – the sentence looks somewhat awkward, please consider revision.

AC:

We apologize for the fact that this sentence was poorly formulated. We will rephrase it as follows: "In lab studies it was shown that sublimation leads to isotopic enrichment (Sokratov and Golubev, 2009); the modelling of post-depositional modification as a result of wind-driven firn ventilation by Town et al. (2008) yielded overall annual-mean enrichment from the enrichment of isotopic winter layers."

The entire passage has been rewritten to improve structure and language.

Page 16, line 8: averaging
AC:
This typo will be corrected.

Page 16, line 10: did you want to say that the spatial separation should be well above the spatial decorrelation length?
AC:
No, indeed we mean well *below* the decorrelation length of the stratigraphic noise. If you compare two isotope profiles that are spaced above the decorrelation length, the contribution of stratigrahic noise to the overall variability of the profiles will be different between the profiles since the noise is spatially no longer correlated. As a consequence, the resulting spatial variability between the profiles will likely mask any temporal changes you want to detect. By contrast, for a spacing below the decorrelation length, the noise contributions will show high similarity and therefore it will be easier to discriminate temporal and spatial variability. The downside of such an approach is that you have to make the second measurement as close as possible to the first one, making disturbances or contaminations of the second profile by the previous measurement(s) more likely. In the manuscript, for the sake of clarity, we will amend the cited sentence as follows: “Alternatively, single records can only be compared faithfully for temporal changes when their spatial separation is well below the spatial decorrelation length of the stratigraphic noise, minimising the amount of spatial variability between the records.”

Reply to the editor comments:

We thank the editor for his comments for which our author comments (AC) are given below.

O_____When you say that diffusion and condensation 'only smooth and compress the original signal', you should precise that you are talking about vapor diffusion against isotopic gradients. AC: It is indeed a good point to precise to which diffusion process we refer here. However, to our knowledge the term "against isotopic gradients" is not common in the literature. Diffusion rather acts "down" the (concentration) gradients. We will change the sentence to "The isotope ratios of buried snow are affected by firn densification (...) and by diffusion of interstitial water vapour driven by gradients in the isotopic composition (...)".

EC: Sorry but I don't understand the statement "by diffusion of interstitial water vapour driven by gradients in the isotopic composition (...)". It appear to me that the sentence is claiming that isotopic gradient is a driving force of change. Vapour diffusion in snow is driven by T gradients not by isotopic composition. The isotopic gradient will drive the diffusion only if the system under consideration was isothermal, purely diffusional. In the present situation, the change of enthalpy induced by the T gradient is orders
of magnitude greater than the change of enthalpy induced by the isotopic gradient. Isotopic composition change is thus a result, not a driving force. Please rephrase so that the reader is not confused by which process is responsible for the change in the isotopic composition.

AC:
We thank the editor for clarifying this issue. It is indeed true that only for isothermal firn diffusion is driven alone by the different isotopic composition of the layers. For non-constant temperatures, the main driver of diffusion are temperature gradients since the temperature directly affects the vapour concentration above the ice and thus also the concentration of heavy and light isotopologues in the vapour. In this case, the diffusion does not necessarily lead to a pure smoothing of the isotopic composition in the firn. This is probably also what referee #1 referred to in the original comment. We overlooked this fact in our answer since we approximate the effect of diffusion in our study assuming isothermal firn even at the trench depth scale. We will add a respective note to the revised manuscript in section 2.4 where the diffusion model is described in order to clarify this and the fact that pure smoothing only occurs for isothermal firn. Regarding the introduction, since in polythermal firn the effect of diffusion may not be a pure Gaussian-like smoothing and not only driven by different isotopic composition of the layers, we suggest to rephrase the cited sentence to a more general statement: "The isotopic composition of buried snow and firn is affected by diffusion of interstitial water vapour (...) and by densification (...); however, these processes do not lead to a net change in the isotopic composition."

EC: Regarding the shift of the curves in fig4. I will suggest to keep the original plot. Eventually, the curves can be x-axis shifted for taking into account the accumulation between the two samplings but no superposition is required in my view.

AC:
Thank you for your comment on this. We will keep the original plot in the revised mansucript.

Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles

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Abstract. ~~Stable water isotopes in firn and ice cores are~~ The isotopic composition of water in ice sheets is extensively used to infer past climate changes. In low-accumulation regions their interpretation is however challenged by poorly constrained effects that may influence the initial isotope signal during and after deposition of the snow. This is reflected in snow-pit isotope data from Kohnen Station, Antarctica, which exhibit a ~~clear~~ seasonal cycle but also strong inter-annual variations that contradict local temperature observations. These inconsistencies persist even after averaging many profiles and are thus not explained by local stratigraphic noise. Previous studies have suggested that post-depositional processes may significantly influence the isotopic composition of East Antarctic firn. Here, we ~~reject the hypothesis~~ investigate the importance of post-depositional ~~change~~ processes within the open-porous firn ~~beyond diffusion and densification~~($\gtrsim 10\,\mathrm{cm}$ depth) at Kohnen Station by separating spatial from temporal variability. To this end, we analyse 22 ~~stable water~~ isotope profiles obtained from two snow trenches ~~at Kohnen Station~~ and examine the temporal isotope modifications by comparing the new with published trench data extracted 2 years earlier. The initial isotope profiles undergo changes over time due to downward-advection, firn diffusion and densification in magnitudes consistent with independent estimates. Beyond that, we find ~~no evidence for additional modification~~ further modifications of the original isotope record to be unlikely, or small in magnitude ($\ll 1$‰ RMSD). These results show that the discrepancy between local temperatures and isotopes most likely originates from spatially coherent processes prior to or during deposition, such as precipitation intermittency or systematic isotope modifications acting on drifting or loose surface snow.

1 Introduction

~~Stable water isotopes from ice cores are important climate proxies. Their abundance ratios~~ The isotopic composition of water measured in firn and ice cores is an important climate proxy. The abundance ratios of the stable water isotopologues in falling snow are shaped by different fractionation processes in between the moisture source and the precipitation site, including evaporation (Craig and Gordon, 1965), air-mass advection and Rayleigh distillation (Dansgaard, 1964), and snow formation (Jouzel and Merlivat, 1984). Hence, ~~stable water~~ isotope ratios can be linked to the climatic conditions at the local or moisture source site. For instance, physical modelling of the large-scale hydrological cycle and the fractionation processes has validated the link between the isotopic composition of precipitation and local temperature (Jouzel et al., 1997, 2003, and references therein) ~~. For~~ previously inferred for polar ice sheets, where observational evidence has suggested a robust relationship at large spatial scales (i.e. continental) between the isotopic composition of snow and annual-mean temperature at the sampling sites (Dansgaard, 1964; Lorius et al., 1969; Masson-Delmotte et al., 2008). Isotope data archived in polar ice cores have therefore become an invaluable means to infer past site temperature variations (e.g. Petit et al., 1999; NEEM community members, 2013) or changes in the moisture sources (e.g. Vimeux et al., 2001; Uemura et al., 2012), and show, at least qualitatively, a globally consistent picture of glacial–interglacial to millennial-scale climate changes (EPICA community members, 2004, 2006; NGRIP members, 2004). However, it is questioned whether the assumption that pre-depositional fractionation processes alone are the main influence on the isotopic composition of firn and ice~~isotope ratios~~, while seemingly fulfilled for large spatial and temporal scales, holds in general. Particularly in low-accumulation areas for which the snow surface is exposed to the atmosphere for a substantial time, a variety of processes are thought to considerably modify the original atmospheric isotope signal during or after deposition of the snow, thus from seasonal to inter-annual timescales (e.g. Ekaykin et al., 2014, 2016; Hoshina et al., 2014; Touzeau et al., 2016; Casado et al., 2016).

For the East Antarctic Plateau, ~~isotopic modifications during and after deposition are generally expected. The isotope ratios of buried snow are~~ modifications of the original isotope signal that is imprinted in precipitation are generally exptected. In buried snow and firn, the isotopic composition is affected by diffusion of interstitial water vapour ~~and firn densification~~ (Johnsen, 1977; Whillans and Grootes, 1985; Cuffey and Steig, 1998; Johnsen et al., 2000; Gkinis et al., 2014) and by densification (Hörhold et al., 2011, 2012; Freitag et al., 2013b); however, these processes ~~only smooth and compress the original signal without changing the net isotopic composition of the snow~~do not lead to any net change in the isotopic composition. In contrast, the ~~low local annual accumulation rates and potential~~ seasonal intermittency of precipitation ~~increase the~~ and accumulation can bias the original signal, induce variability, or lead to a combination of both (Sime et al., 2009, 2011; Persson et al., 2011; Laepple et al., 2011). In combination with the low accumulation rates on the East Antarctic Plateau, precipitation intermittency also increases the time the surface is exposed to the atmosphere (Town et al., 2008; Hoshina et al., 2014)~~and therefore to processes that might~~. These conditions might favour fractionation, diffusive and advective processes that can considerably alter the snow's original isotopic composition~~. These processes can act either on loose snow in the post-condensational phase (~~, acting either post condensation (on falling or drifting snow), ~~on deposited surface snow or on buried snow in the~~ or post-depositionally on snow at the surface or within the open-porous firn column which is no longer subject to erosion but still in contact with the atmosphere. For instance, exchange of water vapour between the first metre of firn and the overlying atmosphere through diffusion and wind-driven ventilation (Waddington et al., 2002; Neumann and Waddington, 2004; Town et al., 2008) can introduce vapour with a different isotopic signature to the firn and significantly change the isotopic composition. Isotopic exchanges between the top layer of snow and the lower atmosphere have been observed on daily scales at the NEEM site in Greenland (Steen-Larsen et al., 2014) and on diurnal scales at Kohnen Station in East Antarctica (Ritter et al., 2016). Isotopic fractionation associated with sublimation, condensation and recrystallisation processes ~~at or~~ within the near-surface firn might change the initial isotope signal, as indicated by observations (Moser and Stichler, 1974; Stichler et al., 2001) and lab experiments ~~,~~ (Hachikubo et al., 2000; Sokratov and Golubev, 2009). Since these post-depositional processes depend, besides temperature, also on other climatic variables such as wind speed and relative humidity, any seasonal or inter-annual variations in these variables would induce additional variability in the isotope record. However, for East Antarctica, a quantitative assessment ~~based on field~~ of the individual processes based on firn-core data is still outstanding, and ~~the importance of these processes~~ their importance for shaping the ~~isotopic~~ isotope signal in the near-surface firn remains poorly constrained.

~~One way to address the question of post-depositional modification is to compare two~~ An additional, important source of variability in low-accumulation firn-core ~~isotope profiles obtained at different times and to measure the nature in which the first profile has been modified. However, this approach is complicated by~~ records is the spatial variability from stratigraphic noise (Fisher et al., 1985), caused by uneven deposition and the constant wind-driven erosion, redistribution and vertical mixing of the snow surface. ~~Thus, the comparison of two single records sampled at different times will always confound temporal isotope changes and spatial variability.~~ A previous study from ~~East Antarctica~~ Kohnen Station in Dronning Maud Land, East Antarctica, has shown that the spatial variability can be overcome by averaging across a suitable number of single profiles extracted from snow trenches ~~drilled at Kohnen Station in Dronning Maud Land~~ (Münch et al., 2016). This yielded a spatially representative isotope signal on a horizontal scale of approximately 500 m. However, contrasting the isotope data with instrumental observations from a nearby automatic weather station ~~(AWS,)~~ (AWS, Reijmer and van den Broeke, 2003) suggests that this regional signal does not necessarily represent a regional temperature signal (Fig. 1). Whereas the isotope record shows strong year-to-year variability, the observed temperature variations are characterised by a regular seasonal cycle and small inter-annual changes. This discrepancy stresses the importance of contributions other than regional temperature alone to the formation of the isotope signal~~.~~

~~In this study ,~~ , such as precipitation intermittency and changes during or after deposition. Since quantitative knowledge on seasonal and inter-annual variations of accumulation amounts is still sparse on the East Antarctic Plateau (Reijmer and van den Broeke, 2003; Helsen et al., 2005), in this study we investigate whether post-depositional isotope modifications in the open-porous firn contribute to the observed discrepancy between ~~isotopes and local temperature~~ the isotope data and local temperatures at Kohnen Station. ~~We present new data from an extensive field work campaign , yielding 22 profiles of stable water isotope ratios obtained from two snow~~

One way to address the question of post-depositional modification is to compare two firn-core isotope profiles obtained at different times and to measure the nature in which the first profile has been modified. However, due to stratigraphic noise, the comparison of two single records sampled at different times will always confound temporal isotope changes and spatial variability. Therefore, in this study we present and use data from a new extensive snow trench campaign yielding 22 profiles of isotopic composition from two trenches, and compare these with the data of the previous trench campaign conducted 2 years earlier. By ~~enabling~~ generating representative records from the spatial averaging of single profiles, ~~we have designed our study such that it allows~~ together with the theoretical understanding of stratigraphic noise, our study allows us for the first time to quantitatively follow the isotopic changes ~~and thus to test for post-depositional effects~~ over a time span of 2 years. We use independent knowledge on firn diffusion and densification to subtract these effects from the observed temporal modifications. Therefore, beyond simply stating the problem of local isotope–temperature discrepancy, we go further and can quantitatively estimate ~~, based on the data and our theoretical understanding of stratigraphic noise, the~~ the influence of post-depositional

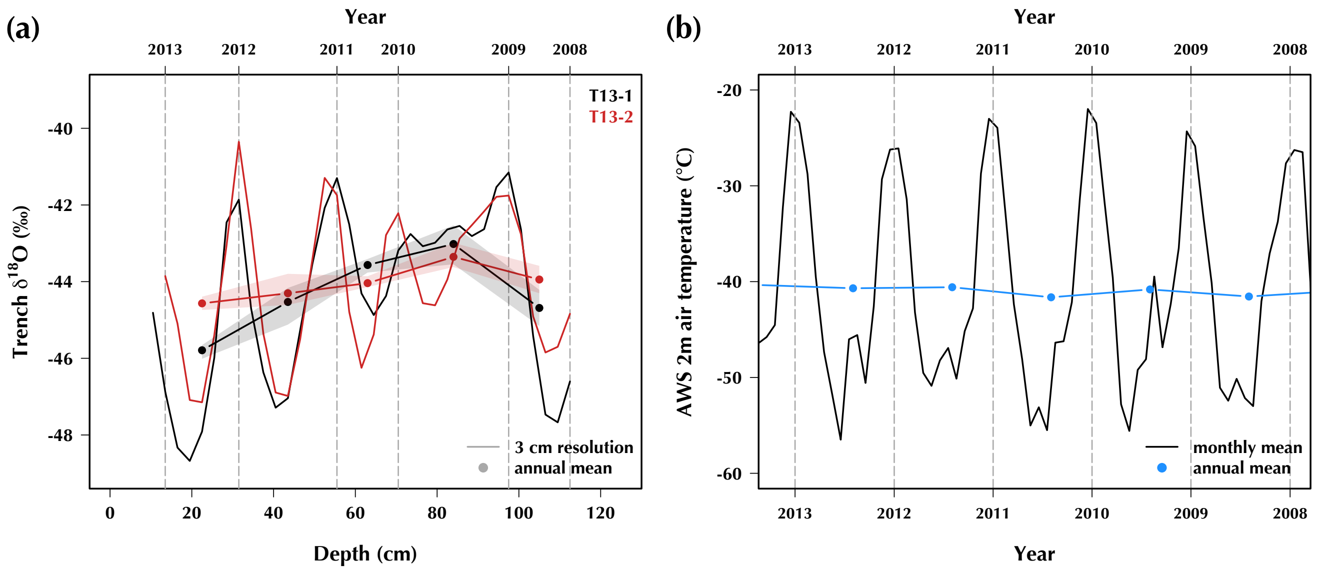

Figure 1. Comparison of oxygen isotope data and 2 m air temperature at Kohnen Station, Antarctica. (**a**) Mean δ^{18}O profiles of trenches T13–1 (black) and T13–2 (red) (modified after Münch et al. (2016)) on original 3 cm (lines) as well as annual resolution (points with uncertainty shading from shifting the range of the annual bins). (**b**) ~~2 m~~ 2 m air temperature (black lines: monthly means, blue points: annual means) recorded by the automatic weather station AWS9 located at Kohnen Station < 1 km from the trench excavation sites. Note the different timescales (**a**: based on counting and binning the extrema of the isotope data, **b**: true calendar dates). The mean of the 2 m air temperature shown here lies about ~~3.5 °C~~ 3.5 °C above the published local ~~10 m~~ 10 m firn temperature (Table 1).

change for our study site. This is an important step towards better constraining the isotope signal formation in East Antartic firn.

2 Data and methods

2.1 Sampling and measurements

A pair of firn trenches, each with a horizontal length of 50 m and a depth of 3.4 m, was excavated using a snow blower in the austral summer field season 2014/2015 near Kohnen Station (Alfred-Wegener-Institut Helmholtz-Zentrum für Polar- und Meeresforschung, 2016), the location of the EPICA Dronning Maud Land deep ice core drilling site (Fig. 2 and Table 1). This campaign extends the published oxygen isotope data set obtained from two shallower (~ 1 m) trenches in 2012/2013 (Münch et al., 2016). From the new trenches, we present the top 1.75 m of the data which are ~~estimated~~ expected to cover the period imprinted in the trenches of the first campaign. To avoid contamination, the new trench positions were shifted relative to the previous ones by 160 m and 300 m, respectively, and are separated by 550 m (Fig. 2). In the remaining part of the manuscript, "T13" will refer to the pair of previous trenches from 2012/2013, "T15" to the pair of new trenches from 2014/2015.

Table 1. Information on the EPICA Dronning Maud Land (EDML) drilling site at Kohnen Station, Antarctica. Listed are approximate position (latitude, longitude), elevation, 10 m firn temperature $\overline{T}_{\mathrm{firn}}$, mean annual accumulation rate of snow $\overline{b}$, and mean ~~monthly~~ daily wind speed $\overline{v}_{\mathrm{wind}}$ (± 1 standard deviation).

Drilling site	*Latitude*	*Longitude*	*Elevation*	$\overline{T}_{\mathrm{firn}}$	$\overline{b}$	$\overline{v}_{\mathrm{wind}}$
	°N	°E	m a.s.l.	°C	mm w.eq. yr^{-1}	m s^{-1}
EDML	-75.0[a]	0.1[a]	2892[a]	-44.5[a]	64[a] / 82.5[b]	~~4.2 ± 1.1[c]~~ 4.4 ± 2.3[c]

[a] EPICA community members (2006). [b] Mean of snow stake measurements 2013–2015. [c] AWS9 data 1998–2013 (Reijmer and van den Broeke, 2003).

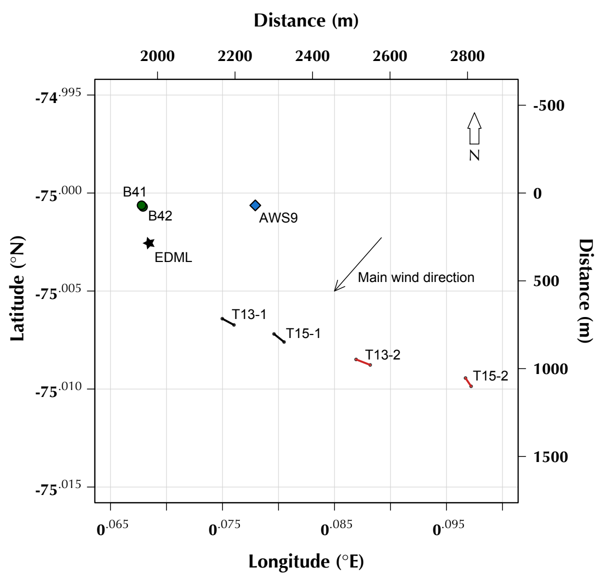

Figure 2. Map of the study area at Kohnen Station. Snow trenches are shown as black and red lines, firn-core sites as green filled circles. The drilling site of the EPICA Dronning Maud Land (EDML) ice core is shown as a black star, the position of the automatic weather station (AWS9) as a blue filled diamond. The main wind direction (57° from geographic North, Birnbaum et al., 2010) is indicated with a black arrow. The trenches were aligned perpendicularly to the local snow-dune direction.

~~Field works of~~ Fieldwork for the new T15 trench campaign ~~were~~ was conducted as follows~~.~~: Horizontal profiles of the surface height variations were obtained along each trench using a levelling instrument. The uncertainty of these profiles is estimated from the reading accuracy of the levelling rod of 0.5 cm. The windward walls of the trenches were cleaned after excavation by slicing off a thin firn layer. Firn profiles were then sampled directly off the wall with a vertical resolution of 3 cm and a horizontal spacing of 5 m, yielding 11 profiles in each trench. The vertical resolution is small enough to evaluate the seasonal cycle of the istope data of ~ 20 cm (Münch et al., 2016); the inter-profile distance of 5 m corresponds approximately to three times the decorrelation length of the stratigraphic noise observed in the T13 record (Münch et al., 2016). At both trenches, excavation and subsequent sampling of the profiles was conducted in two consecutive stages (2 times $\sim 1\,\mathrm{m}$ depth); each stage was completed within 24 h. All firn samples ($N = 1214$) were stored in plastic bags, tightly packed, transported to Germany in frozen state and analysed for ~~stable water~~ oxygen ($^{18}\mathrm{O}/^{16}\mathrm{O}$) and hydrogen ($^{2}\mathrm{H}/^{1}\mathrm{H}$) isotope ratios at the isotope laboratory of the Alfred Wegener Institute (AWI) in Potsdam, using a cavity ring-down spectrometer (L2130i, Picarro Inc.). The results are reported in the usual delta notation (oxygen isotopes: $\delta^{18}\mathrm{O}$, ~~deuterium~~hydrogen isotopes: $\delta\mathrm{D}$) in per mil (‰) relative to the international V-SMOW/SLAP scale. Calibration and correction of the raw measurements was performed as described in Münch et al. (2016). The mean combined measurement uncertainty is $0.08\,‰$ for $\delta^{18}\mathrm{O}$ (root-mean square deviation ~~, RMSD)~~(RMSD)) and $0.8\,‰$ for $\delta\mathrm{D}$, assessed by evaluating a standard not used in the calibration and correction procedure.

2.2 Trench depth scale

Following Münch et al. (2016), we record and display the trench isotope data with respect to an absolute height reference given by the respective maximum of the surface height profile of each trench. Note that the ~~near-surface~~ surface layer of the trench records is incomplete on this depth scale (up to $\sim 10\,\mathrm{cm}$ for T13 and $\sim 18\,\mathrm{cm}$ for T15) due to the surface undulations. Averaging of trench profiles is performed relative to the absolute height reference. Therefore, the number of data points contributing to a mean profile is lower and varies in the ~~near-surface~~ surface layer. This part is marked by dashed lines for all mean profiles and is excluded from all quantitative calculations. Our conclusions are therefore limited to firn depths below $\sim 10\,\mathrm{cm}$ but are however, as will be shown, not essentially changed when including the ~~near-surface~~ surface layer.

2.3 Spatial variability of average trench profiles

Spatial variability arising from stratigraphic noise is a major contribution to the overall variability of individual trench isotope profiles (Münch et al., 2016). Its magnitude ω can be estimated from the horizontal variability of the trench isotope record. Averaging across individual trench profiles reduces the total noise variability. Specifically, stratigraphic noise can be modelled by a first-order autoregressive process with a horizontal decorrelation length for the study region of $\lambda \simeq 1.5\,\mathrm{m}$ (Münch et al., 2016). Then, the residual noise variability of a mean profile built by averaging across N individual records is

$$\varepsilon_{\mathrm{res}} = \frac{\omega}{N^2}\Big(N + f(N, d\cancel{;}, \lambda)\Big) \equiv \frac{\omega}{N_{\mathrm{eff}}}\,, \tag{1}$$

where ~~$f(N, d; \lambda)$~~ $f(N, d, \lambda)$ is a function of ~~the number of averaged profiles,~~ N, λ and of the inter-profile distances d~~and of the horizontal decorrelation length of the stratigraphic noise, λ~~. Eq. (1) equivalently can be expressed through the effective number of records, N_{eff}. For independent noise (zero autocorrelation, $\lambda \to 0$), $f(N, d, \lambda \to 0) \to 0$ and thus $N_{\mathrm{eff}} \to N$~~, as expected~~.

2.4 Quantification of downward-advection, firn densification and firn diffusion

We expect that within 2 years the original T13 isotope profiles have been compressed through densification of the firn, advected downwards due to new snow fall and ~~smoothed~~ affected by firn diffusion. To quantify these effects, certain site-specific parameters have to be known.

Table 2. Sampling and statistical properties of the trench δ^{18}O records from the field seasons 2012/2013 ~~(T13,)~~ (T13, Münch et al., 2016) and 2014/2015 (T15, this study). Listed are: Number and distance of sampled profiles, δ^{18}O values and ~~variability~~variance, correlation of mean trench profiles, and estimated signal-to-noise variance ratios (SNR) after Münch et al. (2016). Correlations are maximised through allowing relative vertical shifts (optimal shift given in brackets). 67 % confidence intervals (CI) for the variance estimates account for autocorrelation of the data. Average signal-to-noise ratios are given with an uncertainty of 1 standard error (SE).

Trench record:	*T13–1*	*T13–2*	*T15–1*	*T15–2*
number of profiles:	38	4	11	11
profile distances (m):	$\sim$ 0.1–2.5	10, 20	5	5
<u>δ^{18}O (‰):</u>				
range: min/max	$-54/-34$	$-50/-38$	$-56/-32$	$-55/-33$
mean (SD)	$-44.4\,(3.1)$	$-44.0\,(2.7)$	$-44.7\,(3.8)$	$-44.5\,(3.8)$
<u>δ^{18}O variance ((‰)^2):</u>				
mean horizontal (67 % CI)	~~5.9~~ 5.9 (5.2–7.0)	~~5.3~~ 5.3 (4.2–7.0)	~~7.0~~ 7.0 (6.1–8.3)	~~6.6~~ 6.6 (5.7–7.7)
mean vertical (67 % CI)	~~9.5~~ 9.5 (8.3–11.1)	~~7.3~~ 7.3 (5.9–9.6)	~~13.8~~ 13.8 (12.0–16.3)	~~14.2~~ 14.2 (12.3–16.8)
mean profile correlation (optimal shift)	0.81 (+3 cm)		0.91 (−0.5 cm)	
~~signal-to-noise ratio~~ SNR (±1 SE)	0.9 ± 0.4	0.5 ± 0.5	1.0 ± 0.3	1.5 ± 0.5

Firn densities are provided independently of the trench data by high-resolution X-ray Computer Tomography data (Freitag et al., 2013a) of the firn cores B41 and B42 (core distance ~ 10 m, Laepple et al. (2016)) drilled in vicinity to the trenches (~ 1 km, Fig. 2). The average firn density in the first metre is $\sim 330\,\mathrm{kg\,m^{-3}}$. The densification rate relative to the surface is $\sim 2\,\%\,\mathrm{m^{-1}}$ when regressing density against depth over the first 2 m, $\sim 7\,\%\,\mathrm{m^{-1}}$ when regressing over the first 5 m.

The local annual accumulation rate of snow was ~~29 cm~~ 28.8 ± 0.4 cm (±1 standard error) in the year 2013 and ~~21 cm~~ 20.8 ± 0.3 cm in 2014, which was estimated from an array of snow stake measurements conducted near the trench excavation sites. In general, the recent local accumulation rate strongly depends on the measurement site, with values ranging from 20–30 cm of snow per year which is up to 50 % larger than the published longtime mean (Table 1).

~~The smoothing effect of firn diffusion on~~ In case of isothermal firn, diffusion of interstitial water vapour leads to overall smoothing of an isotope profile which can be described as the convolution with a Gaussian kernel (Johnsen et al., 2000). The amount of smoothing (the width of the Gaussian convolution kernel) is controlled by the diffusion length σ which increases monotonically in the upper firn layer (Johnsen et al., 2000). We model σ according to Gkinis et al. (2014) with diffusivity after Johnsen et al. (2000). Firn density is a main input to the depth dependency of the diffusion length. For the calculations we smooth the stacked B41/B42 density data by fitting a quadratic polynomial in the square root of the depth. For the concept of differential diffusion, we consider a firn layer which is located at the average depth z_1 and has thickness Δz over which the increase in diffusion length ($\Delta\sigma$) is small compared to the layer thickness, $\Delta\sigma/\Delta z \ll 1$. Now the firn layer is advected downwards to the depth z_2. The total amount of diffusion that acted since the layer has been at the surface is the convolution of the layer's initial isotope profile at the surface, δ_0, with a diffusion length $\sigma(z_2)$. Equivalently, since the Gaussian convolution is a linear operation, we can express this as the diffusion of δ_0 with $\sigma(z_1)$, followed by diffusion of the resulting profile with the differential diffusion length

$$\tilde{\sigma} = \sqrt{\sigma^2(z_2) - \sigma^2(z_1)}. \tag{2}$$

For the T13 isotope profiles, we account for an approximate average effect of differential diffusion over 2 years by considering the average diffusion lengths calculated over the depth of the T13 profiles before and after downward-advection, neglecting the seasonal variations in firn temperature.

2.5 Statistical tests

We use the Kolmogorov–Smirnov (KS) test to assess whether distributions of differences between mean trench profiles ~~are identically distributed~~vary. Autocorrelation of the data is accounted for with a modified version of the standard test adopting effective degrees of freedom of $n(1-a)$ (Xu, 2013). Here, n is the total number of data points for each profile and a the estimated autocorrelation parameter at lag 1. The KS test compares the empirical cumulative distribution functions of the data and is thus sensitive to differences in both mean and variance.

3 Results

3.1 New T15 isotope data and qualitative comparison with T13

The two new T15 δ^{18}O trench records measured in 2015/16 (Fig. 3a, b) are qualitatively consistent with the T13 data (Münch et al., 2016) measured 2 years earlier. The isotopic variability within the first metres of firn is characterised by roughly horizontal, alternating layers of enriched and depleted isotopic composition (Fig. 3a, b) which are separated on average by ~~approximately the local~~ the annual layer thickness of snow (20–30 cm) and thus ~~representing~~ likely indicative of the climatic seasonal cycle. In addition, stratigraphic noise leads to significant horizontal variability, ~~becoming~~ visible through discontinuous and inhomogeneous layering as well as patchy features, for example at the surface of trench T15–2 (Fig. 3b). We ~~also~~ find similar statistical properties for the data of each trench campaign (Table 2). The higher variances in vertical direction of the T15 records are partly expected for autocorrelated data in combination with a larger record length, in addition to the contribution by the strongly enriched layer around 170 cm depth.

Averaging across all individual profiles of each T15 trench reduces the noise level and yields mean profiles that are highly correlated (correlation $r = 0.91$, RMSD ~ 1.2‰, Fig. 3c) and thus spatially representative. We maximised this match by allowing vertical shifts of the T15–2 mean profile. Using linearly interpolated data on a resolution of 0.5 cm, we find an optimal shift of -0.5 cm. We note the exceptionally high delta values at the top of the T5–2 mean profile which stem from a prominent dune at the trench surface (Fig. 3). However, on the absolute depth scale this top part has no overlap ~~on the absolute depth scale~~ with the T15–1 mean profile and therefore does not contribute to the total T15 mean profile discussed below. Despite

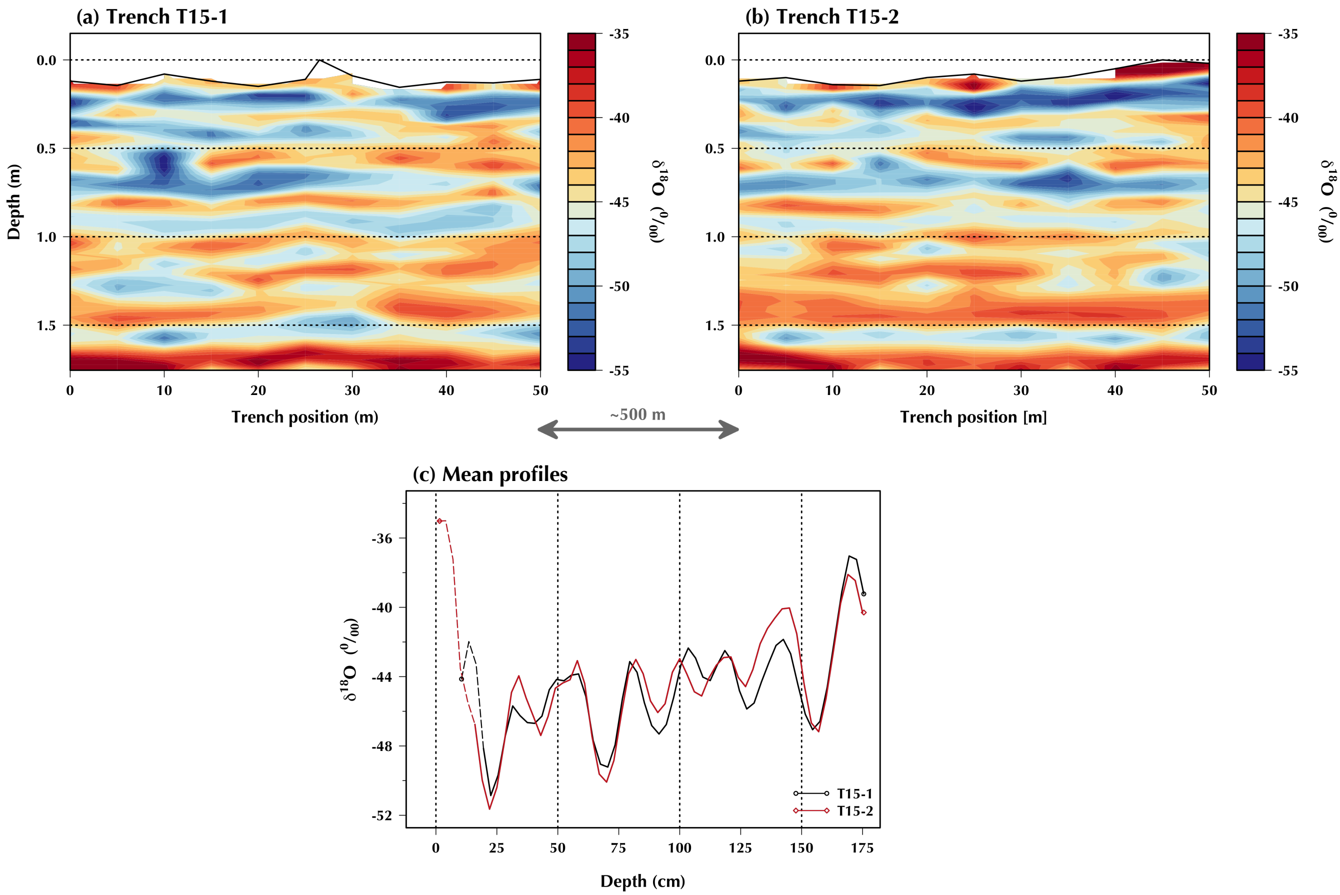

Figure 3. The new T15 δ^{18}O data set. Displayed are the isotope records of trench T15–1 (**a**) and trench T15–2 (**b**) as two-dimensional colour images, and the mean profiles from averaging across the individual profiles of each trench (**c**), displayed ~~after~~ for the optimal ~~shifting~~ vertical shift of the T15–2 mean profile (see text). The trench surface height profiles are given by solid black lines, the near-surface part of each mean profile is marked by dashed lines since the trench data are incomplete there (see Data and methods). The vertical scale in **a** and **b** is strongly exaggerated.

their representativity, the T15–1 and T15–2 mean profiles show strong year-to-year variability confirming the discrepancy to local temperature previously found for T13 (Fig. 1). This also becomes apparent through the increase in average T15 summer maxima (Fig. 3c) which is statistically significant ($p < 0.01$) but not captured by the evolution of local summer temperatures (Fig. 1).

Our first findings show that at our study site both the nature of the regional isotope signal and the stratigraphic noise are comparable between the two trench campaigns. In the following sections we quantitatively assess to what extent the original T13 signal can be recovered with the T15 trenches obtained 2 years later. For this task, we use a single data set for T13 and

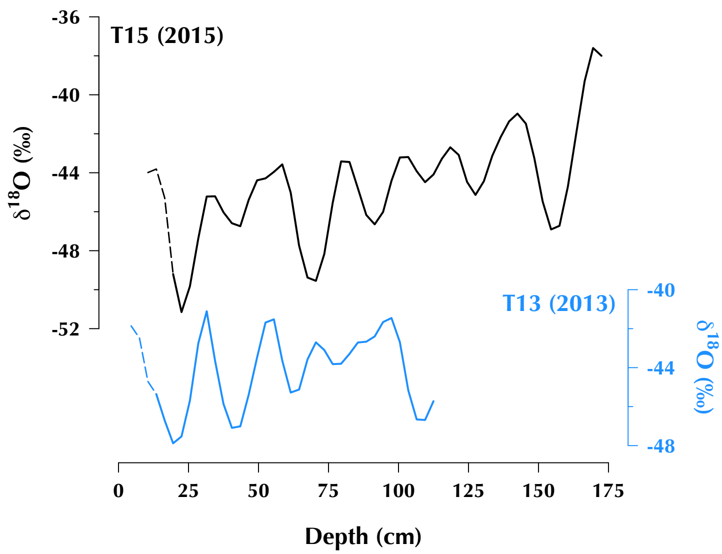

Figure 4. The mean oxygen isotope profiles of the T15 (this study) and T13 (Münch et al. (2016)) trenches on their original depth scale. The incomplete ~~near-surface~~ surface layer of the trenches is marked by dashed lines.

T15 from averaging across each pair of mean profiles (Fig. 4), accounting for the optimal vertical shifts that maximise each inter-trench correlation (Table 2).

3.2 Expected isotope profile changes between 2013 and 2015

We analyse to what extent the T13 record can be recovered from the new T15 data, and which changes have modified the original record. ~~In~~ Within the 2 years, we expect that the T13 isotope profiles are advected downwards, compressed by densification and smoothed by firn diffusion. Testing for additional ~~isotopic~~ isotope modifications hence requires estimating at first the magnitudes of those ~~known~~ expected processes. We do this in two ways: Firstly, we use data independent of the trench records. Secondly, to check consistency with the first estimate, we determine the optimal parameter set that ~~optimally quantifies the processes by minimising the~~ minimises the difference between the T13 and T15 mean profiles.

Using the available independent snow stake and density data~~sets~~, we obtain the following estimates~~.~~: The annual accumulation rates suggest a downward-advection of the T13 profiles after 2 years of ~~50 cm~~ $\sim$ 50 cm. Further, we expect ~~, based on the theory of firn diffusion, an additional diffusion~~ additional diffusional smoothing of the T13 δ^{18}O profiles ~~over the course of 2 years~~ according to a differential diffusion length (Eq. 2) of $\tilde{\sigma} \sim 1.9$ cm. ~~This value is obtained by comparing the average diffusion length over the depths covered by the T13 record before (0–100 cm) and after the expected downward-advection (50–150 cm).~~ The estimated densification rate ~~of $\sim$ 2–7 % m^{-1}~~ at the study site of $\sim$ 2–7 % m^{-1} implies a compression of the T13 profiles after 2 years of approximately 1–4 cm.

For the second estimate, we vary the three parameters (downward-advection Δ, differential diffusion length $\tilde{\sigma}$, compression γ) in order to minimise the root-mean square deviation between the T15 and T13 mean profiles. To avoid an influence on our results, we choose the range of tested parameter values independently of the trench data: For the downward-advection, we apply vertical shifts between ~~$\Delta = 40$~~ 40 and 60 cm, comprising the snow-stake based range of the recent annual accumulation rates~~; additional diffusion~~. We vary the differential diffusion length from 0 to 8 cm, which is equivalent to additional diffusional smoothing of the original T13 mean profile ~~is modelled with differential diffusion lengths $\tilde{\sigma}$ from 0 to 8 cm; and~~ from zero to the maximum possible amount at the firn–ice transition. Finally, compression is applied for values between ~~$\gamma = 0$~~ 0 and 10 cm (equivalent to 0 to ~ 5 times the observed average densification rate). We obtain the best agreement (~~RMSD = 0.92 ‰~~RMSD = 0.92 ‰, Fig. 5; $r = 0.93$) between the T15 and the modified T13 mean profile (= T13*) for the optimal parameters $\Delta_{\mathrm{opt}} = 50.5$ cm, $\tilde{\sigma}_{\mathrm{opt}} = 2.3$ cm and $\gamma_{\mathrm{opt}} = 3.5$ cm (Fig. 6). These trench-based parameter estimates agree reasonably well with the independent estimates from above, showing that the trench data are compatible with our assumptions and parameterisations for downward-advection, densification and diffusion. Indeed, using the independent parameter estimates ($\Delta_{\mathrm{ind}} = 50$ cm, $\tilde{\sigma}_{\mathrm{ind}} = 1.9$ cm, $\gamma_{\mathrm{ind}} = 2.2$ cm from mean over estimated densification rate) to modify the original T13 mean profile (= T13**), results ~~only in a slightly higher~~ in a deviation from T15 (~~RMSD = 0.94 ‰~~RMSD = 0.94 ‰, $r = 0.93$) that is only slightly higher compared to T13*.

We note that the largest portion of optimising the fit between T15 and T13* is accounted for by the downward-advection. This is obvious from only shifting the T13 mean profile vertically to find the maximum correlation with T15, without accounting for diffusion and densification. We find an optimal shift of 48.5 cm ($r = 0.88$) with a minimum misfit of RMSD = 1.07 ‰ (Fig. 5). Thus, the gain in RMSD is only small when adding diffusion and densification according to T13* (black dot in Fig. 5) or T13**, but still appears significant given the above found consistency in magnitude of the trench-based and independent estimates. This is further supported by the fact that no second minimum in RMSD exists outside the region bounded by the contour line of only downward-advection (RMSD = 1.07 ‰, Fig. 5) where the magnitudes of diffusion and densification are unrealistically high.

The visual agreement of ~~both~~ the trench mean profiles after modifying T13 according to downward-advection, diffusion and densification is remarkable regarding cyclicity and, to a lesser extent, the amplitude of the ~~isotopic~~ isotope variations (Fig. 6b). However, deviations ~~especially remain around the isotopic extreme values, in particular for the first overlapping cycle and the depths~~ occur throughout most of the record's overlap (Fig. 6b) and are even amplified there where the amplitude of the T13 profile prior to diffusion was smaller than for T15 (depths of ~ 70 cm and around ~~~ 100 and ~ 125–140 cm. These differences~~ ~ 125–140 cm). Here, locally additional diffusion does not lead to an improved match, although overall it reduces the mismatch between the profiles (Fig. 5). In general, the profile deviations are relatively large compared to the influence of firn diffusion and densification on the original T13 profile (Fig. 6a)~~. Nevertheless, both processes play a significant role in explaining part of the temporal changes. This can be seen if we only shift the T13 mean profile vertically to find the maximum correlation with T15, without accounting for diffusion and densification. This gives a best shift of 48.5 cm, but clearly the agreement is less pronounced (RMSD = 1.1 ‰, $r = 0.88$) compared to using T13* or T13**.~~ which calls for studying further processes in order to explain them.

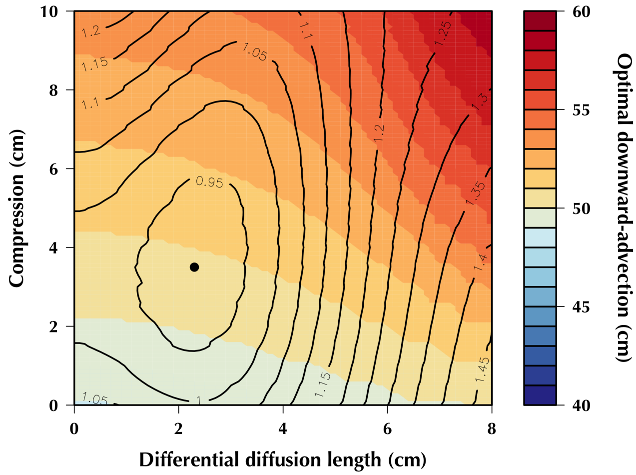

Figure 5. Effect of downward-advection, firn diffusion and linear compression due to densification on the misfit (root-mean square deviation, RMSD) between the T15 and the modified T13 mean profile. ~~For~~ We record the RMSD for each point in the three-dimensional parameter ~~set~~ space of downward-advection, compression and diffusion. For each diffusion–compression pair, ~~we record~~ the ~~minimum root-mean square deviation of~~ figure shows the ~~profiles~~ local minimum in RMSD (contour lines) from varying accross the range of advection values, hence the RMSD for the optimal downward-advection ~~value~~ (colour scale). The global minimum in RMSD is marked with a black dot. Varying the downward-advection has in fact the largest influence on the RMSD.

3.3 Do the remaining differences represent temporal or spatial variability?

We have shown that downward-advection, firn diffusion and densification contribute ~~as expected~~ to the temporal modification of the original T13 profiles as expected from independent data and theoretical considerations. Taking these processes into account leads to a good match of the trench mean profiles (Fig. 6b). ~~However, deviations~~ Most of the match is achieved by accounting for the downward-advection; adding the effects of diffusion and densification yields a slightly further improvement (gain in RMSD of ~ 0.15 ‰). However, still deviations between the profiles on the order of ~~0.9–1 ‰~~ ~ 1 ‰ RMSD remain. These can have two causes: firstly, additional temporal changes driven by unaccounted post-depositional processes such as firn ventilation or sublimation; secondly, remaining spatial variability since we average a large but finite number of records which do not originate from the exact same position. We can thus deduce the importance of additional post-depositional change for our study site if we quantify the contribution of spatial ~~varibility~~ variability. In the following, this is done in two ways: (1) by using the statistical model for stratigraphic noise, and (2) by analysing the distributions of the profile differences.

According to the statistical noise model, the effective number of profiles that contribute to the T13 and T15 mean profiles (Eq. 1) is $N_{\mathrm{eff}} = 13$ for T13 and $N_{\mathrm{eff}} = 20$ for T15. The residual noise of the mean profiles arising from spatial variability is thus the noise level before averaging (~~$\omega \sim 5$–$7\,(‰)^2$~~ $\omega \sim 5$–$7\,(67\,\%\,\mathrm{CI}{:}\,4$–$8)\,(‰)^2$, Table 2) divided by 13 and 20, respectively. We ~~can~~ assume that the residual noise terms are independent of each other. Therefore, the uncertainty of the difference between the T13 and T15 mean profiles due to stratigraphic noise is the sum of each residual spatial variability,

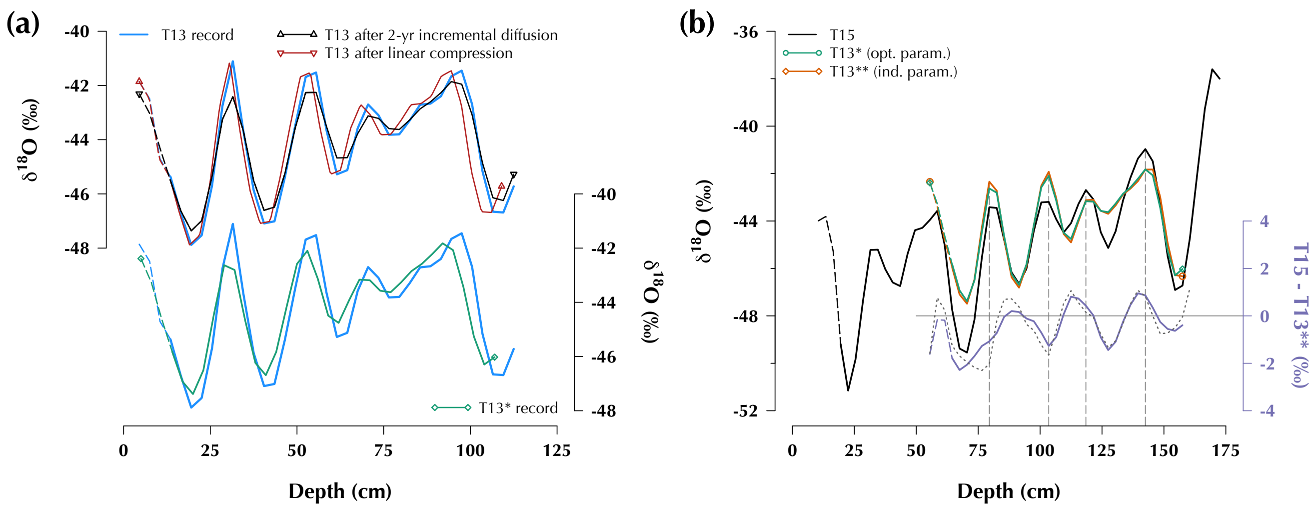

Figure 6. Expected changes of the T13 and comparison to the T15 mean profile. **a**: (Upper panel) The original T13 mean profile (blue) and its modification by diffusion (black: 2-year diffusion with differential diffusion length $\tilde{\sigma} = 2.3$ cm) as well as densification (red: linear compression of $\gamma = 3.5$ cm). (Lower panel) The original T13 mean profile (blue) compared to the joint effect of 2-year diffusion and linear compression (green, T13*). **b**: The T15 mean profile (black) in comparison to the T13 mean profile after modifying the latter according to (1) the optimal parameters for downward-advection, incremental diffusion and linear densification (green, T13*), and to (2) the corresponding parameters obtained independently from the trench records (orange, T13**). Additionally, the difference between T15 and T13** is shown (violet lines, axis to the right). For comparison, the grey dotted line marks the difference between T15 and T13 only shifted optimally ($\Delta = 48.5$ cm). Vertical dashed lines ~~mark~~ indicate the isotopic summer maxima which are not in phase with the difference curve.

or ~~~ 0.6–$0.9\,(‰)^2$~~ $\sim$ 0.6–0.9 (0.5–1.0) (‰)^2. For comparison, the square of the RMSD between the T13* (T13**) and T15 mean profile ~~is 0.85 (‰)^2. The agreement of both estimates~~ (the "temporal varaibility") is 0.85 (0.88) (‰)^2. This agreement between RMSD and estimated residual spatial variability indicates that the remaining profile differences between the modified T13 mean profile and T15 (Fig. 6b) ~~can be entirely explained by spatial variability through~~ might be likely consistent with stratigraphic noise. We note however that the squared RMSD lies ~~at~~ towards the upper end of the estimated range of residual stratigraphic noise~~, which~~. This also applies to the RMSD between the T15–1 and T15–2 mean profiles (square of RMSD of 1.44 (‰)^2 vs. uncertainty from residual stratigraphic noise of ~~~ 1.0–$1.4\,(‰)^2$~~ $\sim$ 1.0–1.4 (0.8–1.6) (‰)^2). This could indicate that part of the spatial variability on the scale of the inter-trench distances (~ 500 m) is not explained by our stratigraphic noise model.

We therefore make a formal statistical test comparing spatial and temporal variability, which accounts for the full extent of spatial uncertainty and makes no assumption about the covariance of the noise, by analysing the deviations between the mean trench profiles. We find that the ~~distributions of the~~ spatial differences between the mean profiles of each trench campaign (T13–1 vs. T13–2 and T15–1 vs. T15–2, Fig. 7a) are statistically indistinguishable ($p > 0.5$ from modified KS test, combining all possible directions of calculating the differences)~~. This suggests that the spatial variability on~~, meaning that

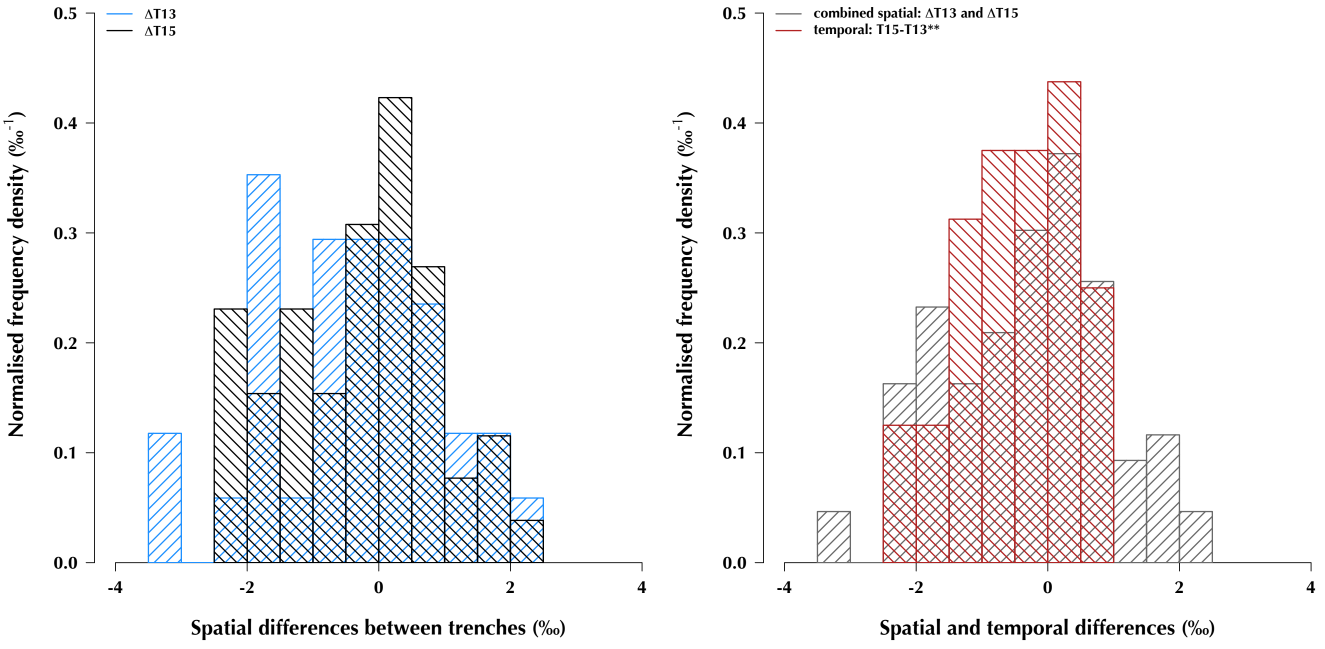

Figure 7. Variability of the trench data sets. The histograms depict (**a**) the distribution of the spatial differences between the two mean profiles of the T13 (~~T13-1~~ T13–1 vs. ~~T13-2~~T13–2, blue) and ~~of~~ the T15 trenches (~~T15-1~~ T15–1 vs. ~~T15-2~~T15–2, black), and (**b**) the combined distribution from **a** (grey) compared to the distribution of the temporal differences between the T15 and the T13** mean profiles (red). All distributions' mean values are not significantly different from zero (all $p \geq 0.4$, accounting for autocorrelation).

the statistical distributions of the inter-trench scale ~~is comparable between the two field campaigns. More importantly, the combined distribution~~ spatial variability are not significantly different between the years 2013 and 2015. This suggests that we can combine these spatial differences as a joint measure of spatial variability ~~is also indistinguishable from the distribution of the~~ and compare them to the temporal differences between the T15 and the modified T13 mean profile (Fig. 7b). Applying the modified KS test once more, also here the null hypothesis that both differences follow the same distribution cannot be rejected (all $p > 0.5$ for using T13** to avoid overfitting~~, Fig. 7b).~~). Thus, the temporal differences between T13** and T15 likely just arise from the fact that the trenches have different locations and can be therefore explained by spatial variability alone.

In summary, both methods show no evidence for any temporal changes of the trench record over the course of 2 years apart from downward-advection accompanied by firn diffusion and densification. The remaining deviations that are observed between the mean profiles of the two trench campaigns can be entirely explained by residual spatial variability arising ~~to a large extent~~ from stratigraphic noise.

4 Discussion

We presented and analysed a new extensive data set of ~~22~~ 22 oxygen isotope profiles obtained at Kohnen Station from two 50 m long and ∼ 180 cm deep snow trenches. The new trench campaign was designed such that it allows for a direct comparison with a trench data set obtained from the same site 2 years earlier in order to test for post-depositional effects. In the following, we first discuss our results concerning the expected processes that have influenced the trench isotope profiles over the observed time period, then our findings regarding the possibility of additional post-depositional changes.

4.1 Densification, diffusion and stratigraphic noise

We found a strong resemblance between the mean oxygen isotope profiles from the trench field campaigns of 2013 and 2015 ~~after taking into acount the effects of~~ (Fig. 6b), achieved mostly by accounting for the downward-advection ~~,~~ and further improved by adding the effects of water vapour diffusion within the firn and firn densification that ~~occured~~ occurred during the 2 years ~~.~~ (Fig. 5). The estimated magnitudes of these processes obtained from matching both records are consistent with independent estimates from snow stakes, diffusion theory and independent density profiles.

The estimated small compression of the T13 profiles is reasonable given the low densification rate observed in the top metres of nearby firn cores. However, our assumption of a linear profile compression with depth is certainly a rough approximation given the actually observed seasonal firn density variation (Laepple et al., 2016), which might indicate a stronger density change with depth of summer compared to winter layers. However, in general the seasonality of densification in Antarctic firn is largely unclear (Laepple et al., 2016, and references therein).

Our data-based estimate of differential firn diffusion agrees with theoretical expectations and in total ~~reduces the~~ leads to a further reduction in RMSD between the T13 and T15 mean profiles ~~.~~ compared to the case of downward-advection and densification alone (Fig. 5). In detail, the diffusion correction improves the match of the trench mean profiles in the medium depth range but also results in higher deviations of the profile minima at the top and bottom part of the overlap (Fig. 6), where the amplitude of the T13 profile had been smaller than for T15 already prior to diffusion. Part of this mismatch might be reduced by accounting for the seasonally varying firn temperature resulting in stronger (weaker) ~~diffusion for~~ attenuation of summer (winter) ~~seasons~~ layers caused by the seasonal difference in diffusion length which is largest close to the surface (Simonsen et al., 2011). In general, firn diffusion is still an active area of research (van der Wel et al., 2015), and progress in this field could conceivably result in an improved understanding of our data.

Stratigraphic noise is a major contribution to the overall variability of isotope profiles (Fisher et al., 1985; Karlöf et al., 2006; Münch et al., 2016). Our large trench data set allows a significant reduction of the noise level by averaging across the single profiles. This is done in two steps: First, we average across the local (intra-trench) scale; then, we average the resulting mean profiles to account for potential uncertainties on the 500 m (inter-trench) scale. Furthermore, we can estimate ~~, based on our theoretical understanding of stratigraphic noise,~~ the remaining uncertainty of the trench mean profiles based on our theoretical understanding of stratigraphic noise. As a result, we ~~have~~ found that the difference of the T13 and T15 mean profiles still exhibits an uncertainty of ∼ 0.77–0.95 ‰ (SD). Thus, the trench data allow us to detect any additional post-depositional changes of the T13 profiles that exceed ~~, on average,~~ a detection limit of ~ 1 ‰ RMSD. Obviously, a lower detection limit would be beneficial but is in practice constrained by the amount of field work, given the high local stratigraphic noise level ~~(assessed with~~ as observed from the mean horizontal isotope variability ~~,~~ (Table 2).

4.2 Additional post-depositional modifications

Based on the above results we have shown that the remaining differences between the 2013 and 2015 data sets are, after accounting for downward-advection, firn diffusion and densification, likely consistent with spatial variability from stratigraphic noise. In other words, we conclude that at our study site the impact of any additional post-depositional changes of the isotopic composition of the firn, below ~ 10 cm, ~~must be on average clearly~~ is on average below the residual stratigraphic noise level, thus $\ll 1$ ‰ RMSD. We limited our conclusion to this depth range due to the applied absolute depth scale resulting in a lower and varying number of available data points in the ~~near-surface~~ surface layer. However, looking at ~~the near-surface~~ this part of the modified T13 mean profile (dashed lines of T13* or T13**, Fig. 6b) also does not show any solid evidence for additional post-depositional changes~~to have occurred~~.

~~This~~ Our conclusion is also supported by comparing the ~~qualitative~~ nature of the differences between the mean profiles (Fig. 6b) with the expected effect of post-depositional modification ~~. Sublimation led in lab studies~~ processes. Studied processes all point to isotopic enrichment~~; modelling of post-depositional modification by~~ , such as sublimation (Stichler et al., 2001; Sokratov and Golubev, 2009) and wind-driven firn ventilation ~~resulted in~~ (Town et al., 2008). Specifically, the latter modelling study showed that firn ventilation can result in isotopic annual-mean enrichment from the strong enrichment of isotopic winter layers~~. Specifically for~~ , compensating an observed slight depletion of summer layers. For South Pole conditions (annual-mean temperature ~~−50 °C~~ −50 °C, accumulation rate 84 mm w.eq. yr^{-1}, mean surface wind speed 5 m s^{-1}), the ~~firn isotopic composition showed annual-mean enrichment by firn ventilation~~ effect amounts to ~ 3 ‰ for firn ventilation until the layers are advected below the influence of the atmosphere, thus after several years ~~of ~ 3 ‰~~ (Town et al., 2008). The environmental conditions at the South Pole are comparable to ~~the observations at~~ Kohnen Station (Table 1), suggesting a similar influence of ventilation on the isotopic composition of the firn. The higher temperatures at Kohnen ~~would even lead to slightly stronger enrichments by wind-driven firn ventilation~~ Station would even imply a slightly stronger enrichment (Town et al., 2008). However, ~~contrary to these expectations, the~~ if we analyse the difference curve of the T15 ~~mean profile shows, if anything, more depleted δ^{18}O values compared to the~~ and T13** ~~record~~ mean profiles (Fig. 6b) ~~. For the first overlapping annual~~ we find no evidenve for firn ventilation. Comparing the direct seasonal counterparts, the first winter layer, which was closest to the surface at the time of excavation of T13 and thus presumably being under strongest influence of the atmosphere, is more depleted in isotopic composition in T15 than in T13**, in contrast to the expectation from firn ventilation. Moreover, despite the fact that the first three summer layers exhibit more depleted values, which would be in line with ventilation, the remaining summer layers do not confirm this finding, and none of the average annual differences show enrichment: for the first annual cycle, T15 exhibits an average difference from T13** of -1.6 ‰ (-1.3 ‰ including the surface region), for the other annual cycles the averages are -0.4, ± 0 and -0.1 ‰. ~~We~~ Also in general, the difference curve (Fig. 6b) does not show any clear seasonal timing which might be expected for a systematic post-depositional modification. Instead, minimum and maximum differences appear rather randomly across the seasons. In addition, the global average difference of about -0.45 ‰ is not significantly different from zero ($p = 0.4$, accounting for autocorrelation). We nevertheless note that the RMSD ~~corresponding to the first value~~ of the first overlapping annual cycle is above our stated detection limit ~~. This limit applies , however,~~ for post-depositional change. However, this limit applies to the average over the record's entire overlap and does not account for the ~~likely possibility of seasonally varying and thus autocorrelated stratigraphic noise levels. Furthermore, the difference curve~~ possibility of autocorrelated differences. Finally, we note the seeming increase with depth of the annual-mean differences towards more positive values (Fig. 6b)~~does not show any clear seasonal timing which might be expected for a systematic~~ , which is also indicated by the slight skewness of the corresponding histogram (Fig. 7b). However, the trend is not strongly significant ($p = 0.12$, accounting for autocorrelation), and the KS test of the distribution of the differences showed that mean and variability of the residual temporal differences are likely explained by the spatial distribution alone. In addition, we obtain similar results (not shown) when we apply our analysis to the trench d-excess ($d := \delta\mathrm{D} - 8 \cdot \delta^{18}\mathrm{O}$) data, a second-order parameter potentially more sensitive to post-depositional ~~modification. Instead, minimum and maximum differences appear rather randomly across the seasons, which supports our conclusion that the effect we see here~~ fractionation processes (Touzeau et al., 2016). The spatial and residual temporal differences between the corresponding d-excess mean profiles follow the same distribution ($p > 0.5$), and the histogram of the temporal differences is even more symmetric than for δ^{18}O.

In summary, all evidence suggests that post-depositional modifications from firn ventilation, or sublimation, are unlikely to contribute to the deviations between the T15 and the modified T13 mean profiles, and that the shape of the difference curve only arises from the statistical nature of stratigraphic noise, smoothed by diffusion. We nevertheless note the possibility that additional post-depositional changes ~~by wind-driven firn ventilation are~~ are still present at Kohnen Station but ~~that their effect is unexpectedly weak~~ not detectable in our analysis. Wind-driven firn ventilation might exist but its effect being much weaker than expected and thus masked by the stratigraphic noise level. One possible explanation for the discrepancy between the firn ventilation model results and our data could be that the model misrepresents the isotopic signature of the surface vapour advected into the firn. Another possibility are weaker firn temperature gradients at Kohnen Station compared to the South Pole, preventing significant vapour deposition. ~~However, assessing~~ Assessing these possibilities in detail is however beyond the scope of our study.

~~Finally, we note the small tendency towards negative values of the differences between the T15 and T13** mean profiles (Fig. 6), which is also indicated by the slight skewness of the corresponding histogram (Fig. 7). However, from the statistical analysis of the differences we cannot reject the null hypothesis that both spatial and residual temporal differences originate from the same distribution, underpinning our conclusion that the temporal differences are unlikely to arise from additional post-depositional modifications~~Interestingly, if the seeming trend in difference values were significant, it would suggest an oriented post-deposition process that is yet unknown. In any case, the stronger profile differences for the first overlapping annual cycle might indicate modification processes that are constrained to the very surface layer. In addition, ~~we obtain similar results (not shown) when we apply our analysis to the trench d-excess ($d := \delta\mathrm{D} - 8 \cdot \delta^{18}\mathrm{O}$) data, a second-order parameter potentially more sensitive to post-depositional fractionation processes . The spatial and residual temporal differences between~~ the ~~corresponding d-excess mean profiles are indistinguishable ($p > 0.5$) , the histogram of the temporal differences is even~~

~~more symmetric than for δ^{18}O~~RMSD between T15 and T13* can be further minimised if one allows shifts in the mean value of the T13 profile (new minimum RMSD of -0.82 ‰ for a shift in mean of -0.4 ‰) which is an interesting observation, yet without any obvious explanation. However, based on the presented evidence, these possibilities are speculative and further field studies are needed to test them.

Our study underlined the pronounced discrepancy at Kohnen Station between inter-annual variations of ~~stable water isotopes and local temperature~~ isotope ratios in the firn and local temperatures and showed that this feature is not only spatially (over distances of ~ 500 m) but also temporally representative over a ~~time span~~ period of 2 years. Furthermore, given the sum of our above findings, it is ~~highly~~ unlikely that post-depositional modifications of the isotopic composition of the open-porous firn (below depths of ~ 10 cm, and ~~likely~~ probably also not in shallower depths) ~~,~~ are the cause of the observed discrepancy. Since a strong relationship between isotopes in precipitation samples and local temperature has been observed at different sites of the East Antarctic Plateau (Fujita and Abe, 2006; Touzeau et al., 2016), this cause must instead be seeked in processes working directly at or above the firn surface. At least two explanations for this seem possible. (1) Seasonal variation and intermittency of precipitation cause the discrepancy between isotope and local temperature data (Sime et al., 2009, 2011; Persson et al., 2011; Laepple et al., 2011). At Kohnen Station, a large part of the annual accumulation is assumed to occur in winter since little or no precipitation is observed in the summer field seasons. However, the exact seasonal and inter-annual variation of accumulation is still unclear due to the lack of sufficiently precise, year-round observations (Helsen et al., 2005). The available surface height changes derived from sonic altimeters of automatic weather stations are difficult to separate into events of drifting snow and true snowfall (Reijmer and van den Broeke, 2003). (2) ~~Isotopic~~ Isotope modification occurring directly at or above the surface is the key driver for shaping the inter-annual isotope variations. Such processes might be acting on falling, loose or drifting snow, or on the top layer (first few centimetres) of deposited snow (Ritter et al., 2016; Casado et al., 2016). The fact that our trench records are reproducible on spatial scales of at least 500 m implies that the atmospheric parameters and conditions controlling potential processes would also need to be spatially coherent.

5 Conclusions

Many studies, including our present one, show that inter-annual isotope records from the dry East Antarctic Plateau are inconsistent with local temperature variations. However, beyond simply stating the problem, we take two steps further: (1) We use the average over 2×11 isotope profiles to obtain a spatially representative record. (2) We designed our study such that it allows testing for post-depositional effects over a time span of 2 years.

Our results provide important constraints on the formation of the stable water isotope signal and its propagation with depth in East Antarctic firn: The trench records show a pure downward-advection of the isotope signal within the open-porous firn ($\gtrsim 10$ cm depth), further influenced only by firn diffusion and densification, with no evidence for substantial additional post-depositional modification. Hence, once the signal is archived at this stage, we do not expect any significant change of the mean values deeper down, reinforcing the credibility of palaeoclimate studies using ice core isotope data. However, ~~by ruling out~~ from our analyis we can constrain post-depositional changes ~~in the open-porous firn we~~ only down to the level of stratigraphic noise.

Therefore, qualitatively, firn ventilation and sublimation might still be present but their effect being very small, or constrained merely to the surface layer for which the lower number of data points in our study prevents quantitative analyses. These constraints lead us to conclude that the observed discrepant isotope–temperature relationship on the inter-annual timescale must be caused either by processes prior to or during deposition.

To improve our understanding of the inter-annual isotope signal, we suggest a mixture of field and modelling efforts. Year-long isotope studies (e.g. in seasonal intervals) ~~focussing~~ with a focus on the near-surface would help to constrain isotope modifications at the interface of surface snow and atmosphere. Further, the role of precipitation and accumulation intermittency has to be clarified, e.g. through measuring wet-depositioned tracers and improved accumulation measurements. These studies should optimally be accompanied by monitoring and modelling of the atmospheric water vapour isotopic composition as well as modelling of the potential exchange and fractionation processes between the loose or deposited snow at the surface and the overlying atmosphere.

Our results again underline the role of stratigraphic noise for the total variability of isotope records. Spatial ~~averging~~ averaging is thus essential to improve the signal-to-noise ratio and thereby to separate spatial from temporal variability. Alternatively, single records can only be compared faithfully for temporal changes when their spatial separation is well below the spatial decorrelation length of the stratigraphic noise, which minimises the amount of spatial variability between the records. The effects of potential ~~isotopic~~ isotope modifications depend substantially on the time the surface ~~layer~~ is exposed to the atmosphere, thus on accumulation rate and seasonal timing of precipitation. Comparable recovering efforts at other ice-coring sites are hence highly needed. Our data indicate that present models might overestimate the expected influence of wind-driven firn ventilation; however, regions with higher wind speeds and lower accumulation rates might still be susceptible ~~for~~ towards post-depositional changes within the open-porous firn. A deeper understanding of the isotope signal formation in Antarctic firn is, beyond holding intrinsic interest, essential to decipher the temperature signal archived in ice core records and thus crucial for their palaeoclimatic interpretation.

Data availability. The trench stable water isotopologue data presented in this study are archived at the PANGAEA database (http://www.pangaea.de) under doi (link will follow). PANGAEA is hosted by the Alfred Wegener Institute Helmholtz Centre for Polar and Marine Research (AWI), Bremerhaven, and the Center for Marine Environmental Sciences (MARUM), Bremen, Germany.

Author contributions. Thomas Münch, Thomas Laepple, Sepp Kipfstuhl and Johannes Freitag designed the trench campaign, Sepp Kipfstuhl and Thomas Münch led the field work. Thomas Münch and Thomas Laepple designed the analysis. Thomas Münch performed the research and wrote the manuscript. Hanno Meyer supervised the isotope measurements. All authors contributed significantly to the discussion of the results and the refinement of the manuscript.

Competing interests. The authors declare that they have no conflict of interest.

Acknowledgements. We would like to thank all scientists, technicians and the logistic support who worked at Kohnen Station in the 2014/2015 austral summer, especially Holger Schubert and Tobias Binder, for assistance in creating the trench data set. Thanks are also due to Christoph Manthey of the isotope laboratory in Potsdam for assistance in measuring the isotope data. We are grateful to Gerit Birnbaum and Carleen Tijm-Reijmer for providing the AWS9 data. This work significantly benefited from discussions with Mathieu Casado and from proofreading and many valuable comments by Andrew Dolman. All plots and numerical calculations were carried out using the software R: A Language and Environment for Statistical Computing. We acknowledge support of this work by the Initiative and Networking Fund of the Helmholtz Association Grant VG-NH900.

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