

Interactive comment on “On the similarity and apparent cycles of isotopic variations in East Antarctic snow-pits” by Thomas Laepple et al.

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General Comments

In this paper the authors investigate an interesting observation that water isotope profiles in shallow firn cores and snow pits show rather constant peak-to-peak wavelengths across East Antarctic sites despite very different accumulation rates. The authors suggest that the source of largely uniform water-isotope cycle length is water isotope diffusion acting on a mostly uncorrelated white noise background, which leads to a characteristic wavelength of water isotope excursions that is linear with the diffusion length. Similar diffusion lengths across East Antarctica thus leads to the similar cycle lengths observed.

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This is an excellent paper. The problem is well laid out in generally clear language. The study is very well designed. In particular I found the build up of the minimal forward model and the comparison between observations and synthetic model based expectations to be excellent and informative. The figures are clear and attractive. I found the arguments in this study to be compelling.

There is an increasing body of work on the diffusion of water isotopes in firn and the interpretation of such records, largely in the spectral domain. This study takes insights from that body of work to inform traditional time-domain interpretations of water-isotope records in snow and firn. This study provides a much-needed caution to the community in interpreting “annual”, interannual, and perhaps decadal variability from these records - in particular single records from the high plateau. It will make a useful and novel contribution to the field.

Generally the paper is well written though both the abstract and introduction have some confusing phrasing or language. There were also some minor errors throughout.

Specific Comments and questions: I have a couple larger questions and a few minor ones.

1) From Equation 2 and the diffusion lengths estimated in Figure 3, it appears that the expected peak to peak distances ought to increase quite substantially in even shallow firn cores (10m depth or so), perhaps up to double the 20cm discussed throughout the text. Figure 7 shows this quite nicely for a couple of cores up to about 15 m. But certainly by the bottom of the firn the wavelength of peaks ought to be substantially longer, be well developed, and presumably locked in. Is such an increase in peak-to-peak distance observed in deeper firn cores or indeed the main deep ice cores from these sites? Such a prediction should be easily verifiable. Is there any other processes not discussed that would alter such characteristic spacing of isotope peaks at greater depths? Perhaps firn compaction and eventually thinning will begin to take over? But it seems that an increase in the peak-to-peak distance in the 20 to 50 m depth range is

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a prediction of this work? Can this be verified?

2) On page 20 (line 17) you state that the correlation of impurities and water isotopes should survive the diffusion process. I completely agree with the arguments about the redistribution of both isotopes and impurities. But the impurities are not subject to the same diffusion process (as you quite nicely discuss in the appendix). Are you arguing that the correlation survives the diffusion of just the water isotope record? If so, how much (if any) is that initial correlation degraded with increasing diffusion? If initially correlated water isotope and impurity records are both similarly whitened by redistribution (or whatever process) and then the water isotopes are smoothed by diffusion, the impurities ought to have higher power high frequency content than the water isotopes. Is this observed? Further the impurities ought to have very little seasonal cycle surviving (at least for some sites since you find the water isotopes to be best represented by a high initial noise fraction per Figure 7). Is this observed?

I also don't really understand why in Appendix A and Figure 9 you apparently diffused the impurity-like record. As you state, the impurities are not subject to diffusion. Perhaps I have misunderstood what was done for that particular profile, if so the description on page 22 could be a little more clear.

The following is an observation rather than a question. An underlying assumption in water isotope diffusion based studies is that the initial background spectrum of the variability is white noise (or close to it and with the possible notable exception of the annual cycle). Your results, in particular those in Figure 7, seem to be a nice confirmation of this. Your Appendix A seems to suggest that (at least some) autocorrelation in the initial signal is possible though doesn't change the resultant signal much.

Minor comments, questions:

Figure 1 is nice. But it would be much more convincing to see several of these example time series (even just in a supplementary figure) than just the two shown here.

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Section 2.2: Line 10: “temporally independent” – so no annual cycle? (This seems like a good null hypothesis, if so.) Line 16: Is the local diffusion length not also a free parameter? Or is that independantly estimated? Section 2.3: This was fascinating, thanks. Section 2.4: Line 13: How does 6cm compare to the minimum accumulation rate at a site you investigated? This is worth mentioning.

Sec 2.5 (page 4), Line 13: This is a fine first assumption or starting place. However was this slope not estimated from data that may contain seasonal or multi-seasonal averages as well as data from depth, and thus including the post depositional processes of interest here? I’d be curious if the final slope of T_{air} d18O_{snow}, after your forward model, is the same as this initial value. This is mostly just curiosity; this does not appear to influence your results in any meaningful way.

Figure 6 is excellent and informative.

Technical Corrections:

Abstract:

Line 4: “depict” is likely the wrong word. Perhaps “record”.

Line 5: Reorder sentence for clarity. “. . . variation in accumulation rate between sites”. Also the meaning of the ending of the sentence is not very clear: “matched by variation in the number of seasonal cycles”. Presumably you mean the number of seasonal cycles for a given depth.

Line 6: “Accumulation conditions” is vague. Do you mean “accumulation rates”?

Line 15: It’s confusing to put “Finally” in the middle of the paragraph. (Though the line makes an excellent point!)

Line 17: “similar power” is two words.

Section 1: (Page 2) Line 10: “which extent” should be “what extent”. Line 17: “the the”

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Line 22: Is “annual snow layer thickness” the same as annual accumulation rate? If so this might be useful to state.

Section 2.4 (page 7) Line 6: Reword for clarity: “. . .in a way similar to . . .”

Pg 12, Line 25: The depth dependency of what about the isotope profiles?

Pg 20, line 21: Missing “it”. Should be “. . .it has been argued. . .”

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