

Interactive comment on “Post-depositional changes in snow isotope content: preliminary results of laboratory experiments” by A. A. Ekaykin et al.

Anonymous Referee #1

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The authors present an interesting experiment in post-depositional effects of water vapor transport within snow on the isotopic content of snow. They find that mean isotopic change is a function of the relative amount of mass lost due to sublimation, regardless of temperature. The importance of ventilation on the post-depositional effects is also indicated by an experiment that allowed different air circulation within the sample chambers.

I believe that this research is important to paleoclimate studies, and an appropriate subject for publication in “Climate of the Past”. Unfortunately, it seems that this research is not quite finished. The authors have some very interesting results and synthesize

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quite a bit of literature to explain isotopic transport in snow. However, their results also beg questions that they seem capable of answering with the experimental setup described in the paper, but have yet to do so.

To the authors: I recommend that they complete the experiments required to thoroughly prove their point, as I believe you have the expertise and equipment to do so. However, if that is not possible, I recommend publication of this manuscript after major revisions, some of which include more small laboratory experiments to provide post-experiment controls on the study performed here.

Major revisions:

0. I am not comfortable with the word “preliminary” in the title, and the text, of the manuscript. Either the data set tells a complete story and the authors are certain of the quality of the data they present, or there is more work to do. I suggest removing occurrences of preliminary from the manuscript and title.

1. Much of the literature review of the first two sections can be condensed into one introduction section. This will provide room for a much needed expansion of the discussion motivating the present research. The current motivation is insufficient. We must understand better the gap that prior experiments have left, and where these experiments fill them in.

2. It appears that the isotopic content of the moisture source for this experiment may not be well-controlled. Simply using a big box of snow may not be sufficient to “know” the isotopic content of the water vapor that comes from the box of snow. The same effect that the authors are observing in the work chamber will occur in the moisture source. Neumann et al. (2008) provide strong evidence that the air coming out of a thin disc of snow is saturated with water vapor, but they also show that the moisture coming out of the snow has changed in isotopic content. Is it possible do some controlled tests to measure the isotopic content of the vapor as it leaves the moisture source?

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Further, there also may be frost deposited along the inlet tube from the moisture source to the “work chamber”. This latter effect may have been avoided already (by heating of sample lines), but it was not clear in the text if that was the case.

These two effects may significantly affect the isotopic modeling results presented in the paper.

3. The isotope model is presented, but not independently verified by any experimental data. It is then used to imply that the gross mass flux in the snow is 1-2 orders of magnitude larger than the net sublimation rate. This is an interesting use of the model, but the model requires more vigorous verification before we can trust its results.

4. The mass model disagrees strongly with the measurements. The authors should decide which explanation they prefer for the discrepancy. If it is instrument noise, then do an error analysis and prove that the error bars on the model calculation are very large. If it is humidity gradients in the “work chamber”, then maybe another experiment should be done to see what sorts of humidity gradients are possible with different amounts of snow in the “work chamber”, and then possibly use these experiments to correct for the initially flawed experimental setup.

Specific revisions:

0. The prose is readable and clear, but requires some tightening (in terms of the English grammar, and in terms of precision) before publication. I have given some small suggestions below, but they are probably not sufficient to bring the manuscript to publication quality.

1. In general, be clear about which ‘isotopes’ you are referring to in the manuscript. I.e., lines 14-15, you are referring to isotopes of water as a paleo-temperature proxy. There are many other isotopes that are studied in ice cores. Similarly on line 21.

page 2240, line 1. Change “assumed” to “thought”.

page 2240, line 25-26. I would also include the relationship between temperature
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during precipitation events and mean annual temperature (Krinner et al., 1997). This is different than, but related to, the relationship between condensation temperature and near-surface air temperature.

page 2241, line 11-16. Process (2) should also include the idea of “forced smoothing” of the isotopic record. Smoothing of the isotopic record can occur from forced interstitial (snow layer to snow layer) vapor transport, as opposed to the molecular diffusion cited in Process (1), and atmospheric water vapor transported to depth in the snow pack.

This section here is an example of where the authors can reword and expand the discussion a bit, rather than have to repeat themselves a bit later in section 2 as they add more detail.

page 2242, line 20-25. This concept is incomplete. Large snow grains growing at the expense of small snow grains does not guarantee isotopic fractionation. This can only occur if some of the light isotopes from the small snow grain are advected away from the neighborhood of the larger, growing snow grains. Otherwise, conservation of mass would not allow a net isotopic change in the snow just because some small grains shrank and large grains grew.

page 2243, line 1. Please clarify that by “layer-to-layer”, you mean “intra-ice-grain diffusion”. If you don’t mean that, then this sentence is really confusing to me.

page 2243, line 7. Temperature defines the “saturation” water vapor pressure in the “pore spaces”.

page 2243, line 5-19. The “active layer” depth is dictated by the combination of wind speeds and form and size of the micro-relief. These concepts should be grouped together at the end of this paragraph.

page 2244, line 9-14. This result is also consistent with more snow falling during the summer than the winter. Must rule out seasonality of precipitation, or include it as an equivalent hypothesis.

page 2244-2245, line 28-29+1-2. This is not enough motivation for your work. You have presented a thorough general outline of the literature, but not why/where your work is filling some specific gap in the literature. Simply lowering the lowest experimental temperature from -30C to -35C is not enough progress to warrant publication. I am confident that citing the supporting material to expand this section will not be hard for you, but it is absolutely necessary.

Sections 3.1-3.2. What precautions were put in place to minimize (or monitor) isotopic fractionation of water vapor from the moisture source until it hits the sample? Why would the same effect of isotopic fractionation not occur in your moisture source that happens in the work chamber? I believe it should occur in your moisture source, and for these two reasons I don't believe you know the precise value of R_{in} . This presents a serious problem for modeling of isotopic content shown in Fig 2.

Why not use pure, dry air in the inlet instead? Then you know that m_{in} and R_{in} are zero.

You may also consider heating the sample tube between the moisture source and the "work chamber" so that there is no worry of frost depositing in there.

page 2248, line 6-10. what are the grain size, density, and permeability of the snow? (Things that Neumann et al., 2008, consider important to this process).

page 2248, line 19-25. How do you determine τ ? What is τ for each experiment?

Figure 4. The disagreement between the model and the measurements is extremely bad. This does not inspire much faith in the models presented in this paper. The models are potentially very useful. More effort should be put forth to refine them, or refine our understanding of their weaknesses.

Figure 5. Leave out mention of the data from Exp 7 and 8. Further, renumber/rename experiments appropriately. Readers do not care what the chronological order of the experiments was.

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Figure 6. The results from Exp 4 beg for repetition at a different temperature to prove that ventilation effects are always independent of temperature, as implied in this work.

Section 4.4

It seems too soon to apply these results to the LGM in literature. In fact, the results of this thought experiment are similar to Town et al. (2008), but for completely different reasons. Town et al. (2008) don't allow mass to leave their snow pack, and they don't include snow-grain-to-snow-grain deposition, something that certainly happens in your experiments. This warrants either an extensive discussion of the uncertainties inherent in Town et al. (2008)s approach relative to the uncertainties in this approach, or it is a subject for another study. I recommend either carefully (but significantly) expanding this section, or leaving this section out until some more significant comparison of results can be made.

Interactive comment on Clim. Past Discuss., 5, 2239, 2009.

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