Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-132-RC1, 2019

Climate of the Past Discussions

Interactive comment on "Comparison of Holocene temperature reconstructions based on GISP2 multiple-gas-isotope measurements" *by* Michael Döring and Markus Christian Leuenberger

Anonymous Referee #1

Received and published: 18 December 2019

Scientific significance: Poor

Does the manuscript represent a substantial contribution to scientific progress within the scope of Climate of the Past (substantial new concepts, ideas, methods, or data)?

This manuscript uses already published data (d15N and d40Ar at GISP2 over the holocene), but a different algorithm (also already published by the authors) to reconstruct the surface air temperature. Although this algorithm presents some advantages, it does not lead to a better fit to the data, and the temperature reconstruction is not better than the 2 already published, and does not provide any new insight on climate.

Scientific quality: poor

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?

The consideration of related work is appropriate. The method is valid in theory, but the implementation has some serious flaws.

The problem here is essentially the resolution of a sytem of 2 equations with 2 unknowns. We have 2 datasets : d15N and d40Ar, plus a third one which is the borehole temperature profile, and 2 input parameters: temperature and accumulation rate. The data (d15N and d40Ar) can be computed to derive 2 variables : the lock-in depth (LID) and the firn temperature gradient DeltaT. A firn densification/heat diffusion model can be use to relate the climate variables (T and acc) to the LID and DeltaT. The authors use 2 good densification models, but strangely, they decide not to use both datasets together in the inverse problem. So they are left with an under-determined system. They are proud to fit the data perfectly, but it is always possible with an under-determined system, and they do not find a coherent answer when they fit different targets (d15N, d40Ar, d15Nexcess). This means that the problem is not well posed. If you have data, you should use them. As a result, the reconstructed temperature makes no sense, especially for d15N excess, but also for the others, as is visible in figure 8 g, h, and i, where the output of the inverse model produces solutions that do not match the borehole temperature data, or known LID (which is unfortunately not shown).

Presentation quality: fair

Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?

The text is long, sentences are sometimes very complicated with improper english. I recomment a full rewrite of the paper when the scientific objectives are better defined. General comment on the form of the figures: it would be much better to have the time axis going always the same way. Figure 9 is the worst, with 2 different directions in the subplots of the same figure.

As a result, I suggest that the paper be rejected, and rewritten for future submission.

The work is not entirely uninteresting, but as it stands, it looks like a student project, and does not add to the litterature.

If I understand the author's motivation correctly, they wanted to use their inverse method to better quantify the uncertainties in temperature reconstructions from inert gases. This is still a valid goal in my mind, but it needs to be tackled differently from what is being done here.

First, the authors should adopt the thinking framework of considering gravitational and thermal fractionation separately, and evaluating both of them. It would avoid the tragedy of Figure 7 or 8h, where inapropriate firn thicknesses are derived.

Regarding gravitational fractionation, here are some questions that are worth investigating:

1. How well is it known from the data? What is the impact of the uncertainty in gas loss on the LID estimation? What is a reasonnable uncertainty range when a convective zone is considered? The output of such a section would give you a LID with uncertainty bars that represent not only the analytical uncertainty, but also other sources, that are more tricky to quantify.

2. Can the densification models accurately reproduce LID variability? This is a big question. Lundin et al 2017 have shown that there are substantial offsets in the mean steadystate LID already, this is why you need a T offset between the 2 models. But what about the variability? If you use a reasonnable range of temperature and accumulation rate change, can you reproduce the high frequency variability in the LID? I bet you will not be able to get a perfect fit, and you'll find that the LID changes more slowly in the model that what the data suggest.

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What is shown on Figure 8 g and H is the compensation between the LID and DeltaT. So all the uncertainty in our knowledge of densification is automatically compensated by a temperature adjustment so that thermal fractionation compensates the misfit in gravitational fractionation when you fit d15N only. This is an essential point, that is causing you a lot of trouble, but you have not discussed it, or even shown a model data comparison with LID and DeltaT data. You approach supposes that the firn densification model is perfect, and that is a difficult assumption to make.

Temperature influences both gravitational fractionation (through its influence on firn thickness) and thermal fractionation. Kobashi et al., 2008 have already shown that d15Nexcess alone cannot be faithfully used to reconstruct temperature change, because of the problem with drift. It is not new. Your hybrid solution is a good idea, but why not also use the borehole temperature record, or compare with d18O, which is also sensitive to temperature? Here several options are possible. But what you did does not include enough constraints to give a meaningful result. In theory, your inverse method can be used to fit multiple datasets at the same time. I suggest that you would do that.

Finally, there are some interesting differences between the Kobashi and the Buizert temeprature reconstructions, particularly around 8.2ka, where Kobashi has a big overshoot after 8ka, but Buizert does not. It would be interesting to understand where these differences come from. It would also be interesting to see whether using their temperature histories with the two densification models, you find a good fit to the data. By comparing the different reconstructions, and the d18O record, you could have a more in-depth discussion about climate history at GISP2. It would give more meat to the paper, and justify its presence in CP.

Tu sum up, as it stands, this paper does not reach substantial conclusions that justify its publication, but with a bit of work, the authors could submit a new manuscript on this general topic.

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