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Interactive comment on "Novel approach for ice core based temperature reconstructions – a synthetic data study for Holocene δ^{15}N data" *by* Michael Döring and Markus Leuenberger

Anonymous Referee #2

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Döring and Leuenberger present a new approach aimed at reconstructing temperature variations from Greenland d15N-N2 data, which is based on inverting an existing firn densification model. They test the model using synthetic data. The method optimizes the fit to the data in several steps.

Unfortunately, both the method and the paper have severe shortcomings, that I list below.

(1) The method assumes that the forward problem (converting surface temperature to d15N) is completely described by the firn model, and that all variations in d15N can be linked 1-to-1 to past surface temperature. It is thus no surprise that they can recon-

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struct the original temperature very accurately, because they know the exact accumulation rates and physics of the forward model. Unfortunately, that is not at all true in the real world. The d15N is influenced by variations in convective zone thickness (the CZ is ignored here altogether), firn layering that influences the lock-in process, melt layers and wind crusts, etc. Real data (as opposed to the synthetic data used) further suffer from analytical noise in the laboratory. All these things will reduce the ability to reconstruct temperature from d15N. Furthermore, our understanding of firn densification is incomplete, with several physical models giving different results, microstructure effects not included in models, and hypothesized influences of dust softening. All these effects remain unaccounted for, which further reduces the ability to use d15N. The authors use identical firn physics in the forward and inverse models, which is an idealization that is untenable.

Several studies have shown that on the cm-scale there is much variation in parameters like d15N and CH4, reflecting a staggered trapping of gas bubbles within the firn-ice transition zone. See e.g. Etheridge et al. (1992), Rhodes et al. (2016), and Mitchell et al. (2015). This may be relevant as the sample size is typically smaller than the average layer thickness.

Gas diffusion and trapping smooths out the d15N signal, which provides a fundamental limit on the time resolution at which surface temperature is recorded and could potentially be reconstructed.

From the above it is clear to me that the precision that the authors state for their method is a meaningless number, that teaches us nothing about how well d15N can reconstruct temperature. A more interesting approach would be to include these fundamental uncertainties in a stochastic way, and see how well the method works under realistic settings. The synthetic data could e.g. be generated with a different firn physics description, and should be subject to CZ fluctuations, LIZ thickness variations and analytical noise.

(2) There are 2 fundamental inputs into the model, namely temperature and accumulation rate. The authors assume the latter is known with zero uncertainty (both in values and age model). This is a very unrealistic assumption. Even if the layer-count were perfect (which it is not), correcting for ice thinning has a fundamental uncertainty. Especially in the early Holocene, this can easily exceed 10%. As the method fits the d15N data, all accumulation errors are mapped into the temperature reconstruction. This is not accounted for.

Exactly for this reason, the method by Kobashi et al. uses a combination of 40Ar and 15N data to isolate the thermal component. The authors do not give any justification why that approach is abandoned. [As an aside, the authors convert the accumulation record from Cuffey et al. onto the GICC05 scale, which makes it internally inconsistent because the accumulation rate is the derivative of the age scale, so changing the age scale should change the accumulation values. Since the method is sensitive to the decadal-scale accumulation variability, it may be insufficient to use this crude approach.]

(3) The authors have no way of validating that their Delta-age is correct, which is critical to constrain the timing of climate change. In all d15N modeling studies I'm aware of, the use of d18O as a temperature template ensures that Delta-age is correct. In particular during abrupt events, the timing of gas and ice signals gives you Delta-age. This information is lost in their method, which is completely independent of d18O.

If the modeled Dage is off by 50 years (which is easy to do in Greenland, particularly during the glacial), the timing of the temperature solution is also off by 50 years. It would be interesting to run their algorithm on data from the last deglaciation, and see whether it reproduces the timing of abrupt change as seen in d18O. Because Delta-age is underconstrained, the timing of all reconstructed high-frequency temperature variations is uncertain.

(4) I am surprised the authors don't event attempt to invert the existing GISP2 data

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(which are even plotted). This seems like a missed opportunity; especially given that it would allow comparison to existing reconstructions to estimate the accuracy of the method.

(5) The paper is overly long. I recommend section 2.3 be removed entirely, and other sections be shortened considerably. There are also 32 (!) figures in the manuscript, which is too many. Dividing the figures into main, appendix and supplement figures is annoying, as it requires a lot of going back and forth.

While the topic is of interest, and the method potentially interesting, I unfortunately cannot recommend publication of the work due to the severity of the flaws in the methodology, and the very limited scope of the presented work. Below are a few detailed comments should the authors decide to resubmit the manuscript elsewhere. I would recommend they first address the major comments listed above.

Page 1 Line 26: Give references for Holocene temperature reconstructions (there are many!). Page 3 Eq. (1): what about the convective zone? You should correct for that Eq. (2): The surface temperature should really be the temperature at the bottom of the convective zone where diffusion starts to dominate. This may smooth out some of the abrupt decadal-scale temperature variations. Line 17: Martinerie et al. (1994) gives the depth of the bubble close-off, whereas d15N is set at the lock-in depth instead. The LID is shallower than the COD. Is this difference accounted for, and how? Page 4 Section 2.2: what are the model parameters? What are the time and spatial step? How deep does the domain extend? What geothermal heat flux is used, etc. Section 2.3: I recommend this is removed completely. I don't see the point, especially the dynamic case which we know doesn't behave linearly due to memory effects. Page 6 Line 13: Not too robust. It'd be easy to have a 10% uncertainty in the thinning function.

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