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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 #1

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This work describes a technical and mathematical variation on previously published work by Kobashi et al. (2010; 2011; 2012). This earlier work used nitrogen isotopes of N2 trapped in air bubbles in Greenland ice during the Holocene to reconstruct surface temperature, which gives complementary information to the classical oxygen-isotope paleothermometer. The added value of the gas isotope work is that the physics underlying this proxy is simpler than the classical oxygen-isotope method, being based on the fundamental physics of gaseous thermal diffusion fractionation. This well-known phenomenon causes heavy isotopes to migrate to colder regions in the firn layer on top of polar ice sheets, leaving a measurable signal in the air being trapped in bubbles at the base of the firn. Kobashi also used argon isotopes, measured in the same ice

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samples as the nitrogen isotopes. Because argon and nitrogen have constant atmospheric isotope ratios over the time scales considered here, the only processes that affect their isotopic composition in trapped air are those processes such as thermal diffusion that occur in the firn column.

One of the challenges that Kobashi et al. faced was translating the observed gas isotope data into a surface temperature history. Kobashi et al. innovated by creating a novel hybrid firn densification-thermal diffusion model, much the same as is done here. The new work is different in that it uses an inverse Monte Carlo sampling and cubic spline filtering instead of Kobashi et al.'s iterative forward-model step-and-adjust method. In that sense it is likely an improvement mathematically over Kobashi et al.'s work.

The scientific advance represented by this work is useful but is very incremental, almost to the point of not standing alone as a publishable scientific paper. It is not clear to me that this work suffices as a "Least Publishable Unit", and reads more like an Appendix to a publication.

It would improve the paper if Kobashi's work could be compared to the results found here, and placed in a larger context. Additionally, it would be helpful if the present work were actually used to reconstruct Greenland temperature over the Holocene, much as Kobashi et al. did. I am somewhat surprised that Kobashi is not a co-author, considering how heavily this work relies on Kobashi et al.'s prior work. The synthetic data looks a lot like Kobashi et al.'s actual data. Why not show the actual data?

If this change is made, then the reader could judge for herself/himself how much of an impact the new inversion methodology has made. Since this journal concerns climate, it would be much more satisfying for the reader if the actual improvement in climate reconstruction is shown.

Minor comments:

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page 1 line 11 "The presented approach is completely automated..."

page 1 line 23 "since it represents a time of moderate natural variations prior to anthropogenic disturbance, often referred to as a baseline...."

page 2 line 6 "The studies of Dahl-Jensen et al. (1998) and Cuffey et al. (1995; 1997) demonstrate the usefulness of inverting the measured borehole temperature profile for surface temperature history....."

page 2 line 9 " unable to resolve..."

page 3 line 24. It is not clear from the wording here which thermal diffusion sensitivity value was used here. Is it the Grachev and Severinghaus (2003), or the Leuenberge et al. (1999)? This must be clarified.

A separate issue is that the Leuenberger et al. value is based on measurements that were made in pure nitrogen, not in air. It is well known, and indeed predicted from theory, that the thermal diffusion sensitivity (and thermal diffusion factor) is larger in pure gases than in air. For example, Grachev and Severinghaus measured these parameters in both pure N2 and in air, and found a substantial difference between the two (Figure 1). As can be seen in Figure 1, the thermal diffusion factor in pure N2 is 0.0037 whereas in air it is less than 0.0036. Even more troubling is the fact that the 1960s-era measurements made in pure N2 by the sources that Leuenberger et al. use disagree well outside the analytical error (0.0035) with the pure-N2 value of Grachev and Severinghaus, which was made with a modern mass spectrometer. This suggests that the 1960s era measurements by Boersma-Klein and De Vries (1966) were badly in error. Given the primitive technology of that time, this is not a criticism of these workers, but it is clear that their values should not be used for the present study.

page 3 line 26 "The firn model used here behaves purely as a forward model,....."

page 4 line 3 You must say which ice core was used here. Is it GISP2?

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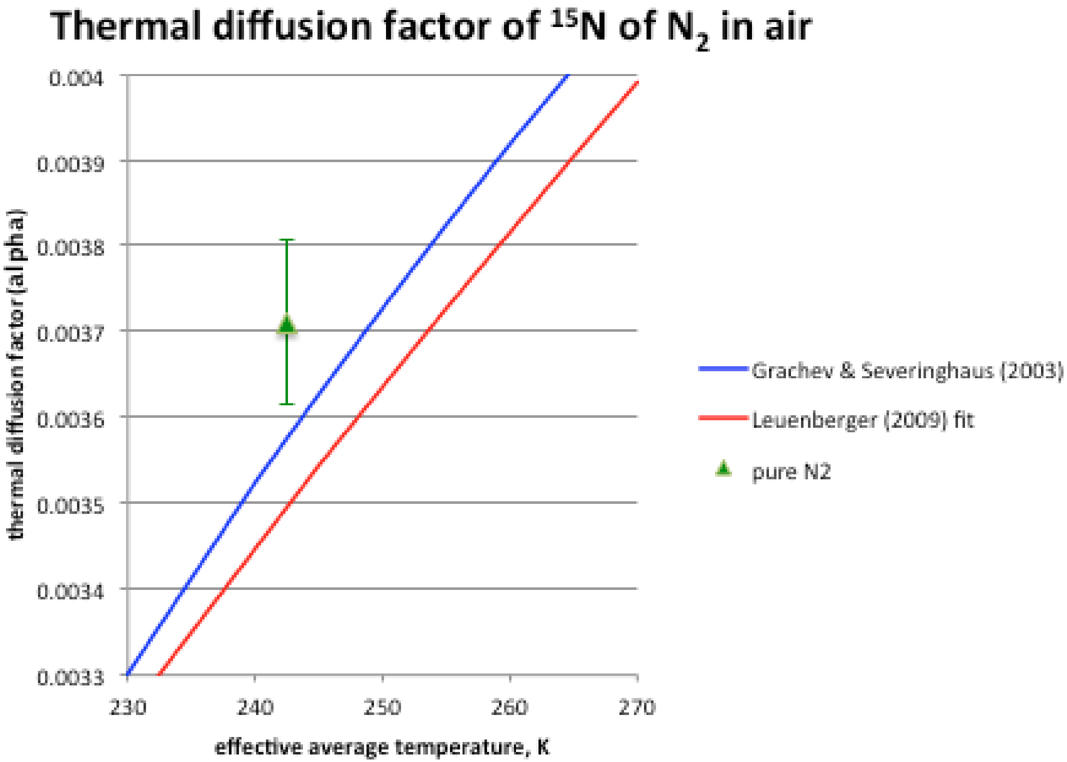

Fig. 1. Comparison of thermal diffusion factor measurements

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