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

Anonymous Referee #2

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The paper from Doring et al uses a newly developped algorithm to reconstruct past surface temperatures from nitrogen and argon in ice core, and apply it to previously published GISP2 data.

This version of the paper is much improved in readability compared to the previous submission, but the major flaws in the experimental design are still there, and a lack of climatic interpretation of the results still dramatically limit the value of the work presented here.

My main criticisms are as follow: 1. The authors use a rich dataset, with d15N and d40Ar measurments, but make no use of it, and choose to use only one type of data

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at a time. This is regretable, because they are in fact unable to produce a solution that satisfies all of the constraints (see Fig 4 on the right pannels), even though they could, people (Kobashi, for instance) have done it before. Because of this, their reconstructions are no better than what is already published. They do not bring the different existing reconstructions in better agreement. As a result, I do not see the value of the results for non specialists of this particular method. And for specialists, still, they would need a version of the algorithm that can fit all the data at once (which exists and has been published by data producing groups).

2. The authors fail to understand, or explain clearly that, if you fit just d15N, you are basically infering temperature from firn thickness. If you fit d15N excess, you are inferring temperature from the thermal fractionation in the firn. These are almost independent processes, no wonder they produce different answers.

3. When infering temperature from firn thickness (either d15N only or d40Ar only), there are two very important assumptions: 1. firn densification models are perfect, 2. the accumulation scenario is perfect. In this instance of the paper, these two hypotheses have been discussed a bit better, by using two different densification models, and by looking at different accumulation scenarios. When I look at figure 5, I do not conclude that the accumulation scenario does not have any impact, but I al also a bit confused by the fact that the full temperature reconstruction is not shown.

4. An evaluation of the results, not just in comparison to Buizert and Kobashi, but compared to external validation data, like d18O, borehole temperature reconstructions, other sites etc would be needed to demonstrate the value of this work. Here, we are left hanging with inconsistent results that are not fully interpreted.

To sum up, I don't think that there is enough added value in the work presented here compared to what is already published (the data, the inverse method, temperature reconstructions at the same site) to justify publication. I recommend this article to be rejected.

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