Clim. Past Discuss.,
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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 #1

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Review: Döring, M. and Leuenberger, M. C.: Comparison of Holocene temperature reconstructions based on GISP2 multiple-gas-isotope measurements, Clim. Past Discuss., https://doi.org/10.5194/cp-2020-132, in review, 2020

This manuscript presents the first application of a recently developed algorithm/firn model scheme to the problem of Greenland temperature variability recorded by ice core gas isotope data during the Holocene.

This is my second time reviewing this manuscript (previously as reviewer 2). I commend the authors on taking on board many of my suggestions and carrying out a major restructuring of the paper. I find the manuscript somewhat improved, particularly in

terms of readability. That said, the manuscript is still very long and filled with technical details and close examination of model output that is often deemed highly uncertain. There are a number of long sections that end with conclusion to say that a particular effect is small (e.g. accumulation history) or the authors do not fully understand why their model-based prediction do what they do (e.g. differences in absolute temperature between Schwander and Goujon model as well as the uncertainty in the 15Nexcess fitting between the two models).

After a close reading, a gas isotope specialist will likely be able to gather some useful information. However, I suspect that the paper will be impenetrable for a non-specialist (e.g. a climate modeller interested in examining Holocene temperature variability). Given the specialist nature of Climate of the Past, this is perhaps not a barrier to publication. I mention it here so the authors keep this mind during the response to reviews and it brings up my only major comment.

For a non-specialist, the paper does not provide a clear answer as to which of the various methods will most accurately reconstruct surface temperature. One is even left with the impression that this is perhaps an intractable problem. If this is indeed the overall point of the authors I can understand their hesitancy to push any one particular reconstruction. However, if they see more utility in some more than others, or believe that all should be considered equally, then they should discuss this with the general user in mind. Moreover, they should make their reconstructions available for use, if they believe their results robust. Otherwise the community will continue to use the previously published reconstructions that the paper (sometimes) attempts to argue are not fully robust.

On the other hand, if they believe all their results are not robust then they should state this explicitly. For example, the final sentence of the abstract reads "However, all three reconstruction strategies lead to distinct temperature realizations". What are the implications of this? Should we use just one method or none at all?

Will the author's be making the temperature reconstructions publically available? Similarly, is their code available upon request?

I am surprised not to see any comparison climate model predictions. One way to frame the question of which temperature prediction is realistic would be to compare to model predictions (on centennial to millennial-timescales in Holocene). This would be an entirely different paper, but would show the implications of the different methods. This would also highlight the problem of seasonality changes on d18O.

Specific comments Page 2, line 8. A reference to the original Dansgaard study is appropriate here. The reference to Gierz et al., 2017 is more appropriate if discussing water isotopes during the last interglacial, where even then the literature is quite broad.

Page 2: line 25. The author setup of the paper by suggesting the Buizert approach could be complicated by the d18O "side effects" related to seasonality. Please consider using a different term than "side effect". But moreover, this is an important point, but is never followed up on in the text.

Page 4, line 14. Remove capitalization of "First".

Page 4, line 35. Can you provide a reference for this calculation of the signal smoothing based on resolution? I don't quite follow based on the equation shown why 5.3 is sensible. Are you trying a rough calculation of standard error based on the number of samples within a given window?

Throughout. . .define "cop" in the figures and other parts of the text.

Page 6, line 5. Replace "Non-sufficient" with "insufficient"

Page 6, line 18. A reference to figure our table is needed here as it not "obvious" what the authors a referring to.

Page 6, line 19. Change wording to remove use of "neither". Sentence is complicated because of the double negative

Page 8, lines 11. Please define the phrase "gained data".

Page 14, line 31. Depended should be dependent.

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