Clim. Past Discuss., 11, C2090–C2097, 2015
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Climate of the Past Discussions
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# *Interactive comment on* "Laurentide Ice Sheet basal temperatures at the Last Glacial Cycle as inferred from borehole data" *by* C. Pickler et al.

**C. Pickler et al.**

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Received and published: 21 October 2015

**Response to comments by Reviewer V.M.Hamza**

We thank the reviewer for the opportunity to further explain our work. We note that several of the points brought up in our first response have gone unacknowledged.

1. *Much of the variations in temperature gradient (and hence in heat flow), reported in the work of PBM, arise from inaccuracies in the precision with which small temperature differences can be measured at closely spaced depth intervals during log operations.*

If the reviewer had taken the trouble to read the references and the description of the data used in this study (e.g. Jaupart et al., 2014) to cite only the most recent one, he would not have failed to note that the sampling interval for temperature measurements is not small (10m), and his explanation for the noise is not valid.

2. *Consider now the argument of PBM that minor changes in lithology is responsible for high frequency variations in heat flow. Under steady state conditions, changes in thermal conductivity arising from minor variations in lithology lead to compensating changes in temperature gradients such that vertical heat flow remain unaltered...Local changes in vertical heat flow may occur as a result of 2D/3D thermal refraction effects arising from lateral variations in thermal conductivity and/or perturbation effects of groundwater flow. According to the results reported, high frequency variations in heat flow are present in the deeper parts of all boreholes, not just those in the mining areas, as claimed by PBM. Why such changes occur preferentially in the deeper parts of the thirteen boreholes remain a mystery. The argument that high frequency fluctuations arise from minor changes in lithology, occurring in the bottom parts of boreholes, appears to be ad-hoc.*

Let us reiterate that the vertical profiles of heat flux are obtained by simply multiplying the vertical gradient by an average conductivity, therefore there is no compensation. We probably should have shown the gradient profile rather than heat flux to avoid confusion as the reviewer failed to appreciate this point. If there are no systematic changes in thermal conductivity and if the lithological variability of the thermal properties is random, then, once again, the high frequency oscillations at depth are irrelevant as stated above. We do not know



CPD
11, C2090–C2097, 2015

where the reviewer got the idea that some of the boreholes are not mining exploration holes. The only hole that was not drilled for mining exploration is the one at FlinFlon which was drilled to be instrumented with seismometers by the US Air Force. (Nevertheless FlinFlon is a mining area). All the other holes were drilled for mining exploration. Furthermore, the inclination of some of these deep holes varies with depth, usually flattening near their target. There is no mystery for the increase of noise.

*It is more likely that smoothing procedures have been carried out only for data sets of the upper parts of boreholes.*

The reviewer is incorrect. As stated above, no smoothing was applied to the data.

*In this context, the reference to repeat measurements in Sept-Isles (in the reply comment of PBM) has only limited validity, in view of absence of data for the deeper parts of the borehole in the log of 1994.*

We disagree. We would not have repeated the measurements if we had logged the full depth of the hole in 1994. Where the two temperature logs are available, they show similar fluctuations.

3. *It is important to point out that the results illustrated in Figure 2 are inconsistent with those in Figure 3, in which the authors fit straight lines over 100m sections of the T-z data. This procedure has been used in obtaining an extrapolated surface temperature.*

Such an approach provides an estimate of the surface temperature history



CPD
11, C2090–C2097, 2015





independent of the inversion procedure as it assumes no model and simply applies the standard procedure to estimating the quasi-equilibrium state in the subsurface. The iteration from depth to the surface illustrates how the long-term temperature has changed over time. We do not expect this to be a robust reconstruction and the differences are not surprising.

*Why not multiply the temperature gradient so obtained with the corresponding mean thermal conductivity to obtain the mean heat flow density for the 100m section? The resulting Q-z profile would surely have been much smoother After all, Figure 2 is billed as a first order inverse results and not merely the presentation of raw data.*

In his statement above, the reviewer accused us of smoothing parts of the data. He now states that we should have smoothed the data!

4. *The value of To for Flin Flon, as estimated by the authors, is 3.8oC. The calculated GST at 100,000ybp is about 6oC. The significant discrepancy of 2.2K between To and the temperature at 100,000ybp suggests that the time period employed in inversion of data for Flin Flon, is too short to obtain a reliable estimate.*

This is incorrect, the 100K inversion span is suitable as demonstrated by comparison the 1,000,000 years inversion the reviewer posted in his first comment. If the time span is too short, the inversion will adjust the reference temperature.

*There is no harm in using a longer time period for inversion because the*



CPD
11, C2090–C2097, 2015

*inversion will simply taper to To when the T-z data contain no information about GST variations toward the remote past.*

This is generally true but depends on the regularization of the inversion procedure.

*On the other hand, using too short a time period is harmful because inversion would be trying to squeeze in temperature variations that do not belong in the time period. I encourage you to redo the inversion for Flin Flon with a longer time span.*

No. See above comments for response.

*By the way, the captions of Figures 4-7 indicate that the displayed GSTH has been offset by To. Clearly, this is incorrect.*

The captions are correct. The GST history is reconstructed using the temperature anomaly. This means that the initial temperature ($T_o$) has not been taken into account. Following inversion, in order to obtain a temperature not temperature change, the GST must be shifted with respect to the initial temperature ($T_o$). If this is not done, then we are neglecting the initial temperature ($T_o$) and cannot reasonably assumed a GST only a GST change.

5. *Using singular value cutoff for smoothing is adequate when equal time steps are used. However, singular value cutoff alone is not adequate when unequal or logarithmic time steps are used, because unequal time steps carry unequal weights.*



CPD
11, C2090–C2097, 2015





The reviewer is incorrect. The coefficients of the matrix do not depend only on duration but also on time. A ten year time step does not carry the same weight if it occurred 100 or 10000 years ago. Hence the logarithmic distribution of time steps to offset these differences which would otherwise cover several orders of magnitude.

*Let us consider an extreme example in which we have 15 large time steps and one very small time step. Then the temperature for this small time step would likely be associated with small singular values and hence poorly resolved or not resolved at all. An additional smoothing constraint is one way to ensure that the temperature for this small time step falls in line with the neighboring values.*

Singular values are not associated with individual time steps but orthogonal linear combinations of them. Besides articles on inversion of borehole temperature profiles provided in references (Mareschal and Beltrami, 1992; Beltrami and Mareschal, 1995; Clauser and Mareschal, 1995; Beltrami et al., 1997), there are many excellent articles and books on singular value decomposition (e.g. Jackson, 1972; Lanczos, 1961) that are worth reading.

6. *In concluding this reply, my recommendation again is to verify the computational procedures used in the inversion program. ... It is also advisable to take a second look at the procedures employed in calculating first order estimates of heat flow and long-term surface temperature history.*

We thank the reviewer but as stated in our previous reply, benchmarks



CPD
11, C2090–C2097, 2015





have been run and the method has been checked thoroughly in at least a couple of publications with researchers using SVD, Fourier and FSI inversions (Beck et al., 1992; Shen et al., 1992).

We hope that we have answered the reviewer's comments. Although this dialog has not been particularly illuminating, it has given us a chance to clarify some points. We appreciate the time spent by the reviewer on preparing his comments.

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CPD
11, C2090–C2097, 2015