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Climate of the Past Discussions

# *Interactive comment on* "Climate trends in northern Ontario and Quebec from borehole temperature profiles" *by* C. Pickler et al.

**Anonymous Referee #1**

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This manuscript discusses several borehole temperature profiles in portions of two Canadian provinces. The authors use an inversion technique commonly used in borehole climate studies to determine past temperature histories for these sites, and interpret the results as showing little or no signal for both the Little Ice Age (LIA) and the presence of permafrost in the region.

My overall feeling of the paper is positive, but I believe that the authors gloss over several issues that either need to be addressed or that would make the manuscript, and hence the interpretations, stronger. Addressing these issues would lead me to recommend publication of this manuscript in Climate of the Past.

I will list my comments about these issues below:

1. Borehole sites: In adequately determining if borehole sites are appropriate for use



in climate reconstructions, several criteria are required. While the authors have addressed several of the sites and determined they were unsuitable (as presented in Table 2 of the manuscript), information regarding the other sites is not included that would aid a reader in understanding the conditions at the boreholes. For example, no discussion of slope, topography, vegetation or surface material is given, although the authors do reference previous studies. A discussion of vegetation and ground cover at the sites would be extremely useful, however, especially considering that the argument that one site (Thierry Mine) may have additional warming due to the removal of vegetation was put forth. Further, some sites are said to be "too shallow" or on the side of "steep" hills. What exactly is "too shallow" and "steep"? Can a quantitative discussion replace the qualitative explanation? Also, are all of the boreholes vertical? At least one site was excluded because it was plunging under a lake. It should be clear.

2. Temperature Anomalies: The determination of the geothermal gradient using the bottom 100 meters is usually sufficient, but I wonder if the results of the removal of the steady state gradient as shown in Figures 2 and 3 are different if the length (100 m) is modified?

3. Results: The authors state that only one site has a ground surface temperature (GST) that was affected by the LIA. However, based on the temperature anomalies shown in Figures 2 and 3, it would seem that other sites exhibit cooling at the same depths as Otoskwin. Mussellwhite, TM0608, and CC0713 all have temperature anomalies that indicate cooling at the same general depth. Is this not a LIA signature? Also, the anomaly shown in CC0712 (Figure 3, top left) has a very interesting profile. What is the cause? Other questions I have about the results that don't have any explanation – or that aren't adequately explained – include the assertion that the Thierry Mine signal may be amplified by the clearing of vegetation between 1934 and 1950. However, most of the GST histories show a large increase in temperature at this same time, indicating it may not be vegetation alone. Have the authors done any modeling or do they have any surface temperatures to support this hypothesis? Lastly, one site (Eleonore) has

warming that began considerably earlier than the other sites. Why might this be?

4. Interpretations: In the final section, the authors attempt to explain some of the anomalous results, but do so only qualitatively. The manuscript would be far stronger if there was more of an attempt to quantitatively make the same argument. This is done with the Thierry Mine example (previously discussed), as well as with Corvet, which "is located on the side of a 30 m hill." However, what is the slope? How much of an effect does this have? It is still being used, so the authors must think it isn't significant. My final criticism is of the qualitative nature of the LIA arguments. The authors discuss what the LIA surface signal should be for the region, but do not see a ground signal. Perhaps a simple forward model of driving into the ground a surface temperature time series with the appropriate LIA signal and making a comparison to the boreholes would be appropriate? Then, the authors could argue whether the signal is strong enough to actually be observed, or whether it is not seen due to snow or something else. This is similar to the arm waving argument used to interpret a possible ground warming due to longer/deeper snow cover in the region, but it seems that other authors have performed some analysis that may provide quantitative support to their arguments (perhaps Bartlett et al., 2005?)

5. References: I did notice that on page 11 in the reference section that Jaupart and Mareschal, 2011 was published in Cambridge, not Cabridge; also, the next two references following the previous reference are of Jaupart et al., 2014 and are a duplicate.

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