

## ***Interactive comment on “Differences between repeated borehole temperature logs in the southern Canadian Prairies-validating borehole climatology” by J. Majorowicz et al.***

### **Anonymous Referee #3**

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This might be an interesting paper, and it might be worth publication. At this point, however, the paper is not well presented, and it is very difficult for the reader to understand what the authors have done and what it is that is new in this the paper. I also have serious reservations about the quality of the data, and the analysis, at least as far as I could understand what the authors are doing. Temperature depth profiles in the Canadian prairies and in the Dakotas have already been interpreted to infer very strong recent warming. As far as I could see, the only new result in this paper is that repeat borehole temperature logs confirm the warming trends in the Canadian prairies. I think that the authors have confused the issues that the paper raises by throwing in too many old results and not focusing sufficiently on the analysis of the repeat logs.

The presentation of the data and the analysis are extremely careless and the paper reads like the result of cuts and pastes. Here are some specific points.

1. I do not understand why the authors do not present their data in a more systematic way. The numbering of logs is inconsistent between Table 1, Table A1, Figure 7, and Figure A1. All the repeat measurements from Canada are presented in Figure A1 which, BTW, is very difficult to read. Three logs are also shown on Figure 2; three of the North Dakota sites are shown on Figure 7. Concerning the Canadian data, some drill holes are extremely shallow (50-80m) and I doubt that they yield any useful information. Some drill holes are clearly affected by non conductive effects. The authors must also have edited their profiles to get the warming indicated in Table A1. The 2005 logs from TSA1 and TSA6 indicate cooling; yet, Table 1 gives 2.3 and 1.3 degrees warming since 1900. Some profiles were not used, I think for obvious reasons, but this is not explained in the text. Many of the profiles used are questionable.

2. I am really at pain to understand the author's use of the pre-observational mean. They define an SAT pre-observational mean from the SAT record between 1895 and 1910. I am not sure why it needs to be assumed and input into the predicted GST. The pre-observational mean is usually used as a free parameter adjusted to fit the temperature profile predicted from SAT to the observed profile. The authors must give a Table with the values that they have assumed or obtained for the POM. Is the inferred warming since 1900 on Table A1 relative to the POM? They should also list which weather station was used to input the SAT for each of the drill holes that they have interpreted. No weather station is indicated for the northern part of the study area. Where did the SAT come from?

3. The author's use of functional space inversion is even more obscure. It is mentioned in section 3, but not in the results and I wonder if it was used at all in this study. (BTW, in the abstract it is presented as functional state inversion).

4. The comparison between GST from repeat borehole temperature profiles and SAT

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is unfortunately very incomplete and relies only on weather stations from southern Alberta and Montana. The author present a good case that for two of the temperature logs that the SAT and GST were coupled. The paper does not present SAT records from northern Alberta to compare with the very large GST warming inferred there. Without an analysis of all stations where good data have been obtained, the paper conclusions are not supported.

5. There is no analysis of the effect of snow cover. The authors present a figure of yearly snowfall at three stations, but I am not sure what they do with it. The discussion of snow cover is very vague and sounds like a review of published work. Again, there is no data on snow cover in the northern regions where the GST warming seems to be highest. I do not think that the warming could be explained in terms of snow cover, but there is no ground for the author's claim that snow cover is irrelevant.

6. The pattern of northward increase in GST warming in Figure 9 is not quite as “clearly evident” as the authors claim. Without the South Dakota data, it would be very difficult to see any trend and a well marked SN gradient. There is a lot of scatter in the data at any latitude. If data were binned by latitude, the standard deviation within each bin would be three times larger than differences in the mean.

7. The caption on Figure 9 mentions Table 1 for information on the well sites used in this Figure. There are US sites on the Figure but not on Table 1.

There are a lot of misleading statements throughout the paper. For instance, the authors say that three basic assumptions are made to reconstruct GST from T-z profiles. The first two (GST follows SAT and the offset between SAT and GST is constant) are not required to reconstruct the GST from T-z data, but only to relate the GST to the SAT. This may sound picky but is just another example of the very careless presentation of this paper.

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