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

Anonymous Referee #1

Received and published: 21 November 2006

This manuscript reports repeated temperature-logs in boreholes made over the time span of about two decades (1986-2005). Because some sources of curvature in borehole temperature profiles are steady state, the difference between the temperature logs can be more confidently taken as due to surface temperature change. The authors back up this claim by investigating the predicted subsurface warming based on nearby surface air temperature records. This is a unique and important dataset. This study is particularly important because the boreholes are in an area of seasonal snow cover, and seasonal snow cover has been asserted to decouple air and ground temperatures. This manuscript therefore has the potential to be important and reach a wide audience. As outlined in the comments below, I think the paper needs to be better focused and

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should be communicated in a more transparent way. Additionally, some of the claims in the paper need to be examined in a more quantitative way. While there is the seed of a good paper here, my judgement is that this manuscript needs to be substantially rewritten and should probably be re-reviewed.

Overall the authors analyze a few temperature logs from some of the 24 boreholes for which they have made repeated temperature logs and compare those differences against differences constructed from SAT data that have been projected into the sub-surface. Strictly they are validating the borehole technique over an approximately 20-year period. The authors conclude that the differences in the temperature logs are in general agreement with those from SAT data. I would agree, except there appear to be important and systematic misfits. These results needs to be put into context. The authors appear to waffle (as detailed later), and this should be straightened out, or caveats should be added to the conclusions. Finally the authors should discuss how this work fits in with the previous work. For example in the introduction we are reminded that GST warming has been significantly greater than SAT warming in this area. In an earlier paper, using similarly located if not the same boreholes, Skinner and Majorow-icz (1999) argued that air and ground temperatures were showing different warming responses because of land-cover change, and specifically that temperature logs showed greater warming than SAT records. This result has gained prominence because it has been used to question the validity of warming estimates based on borehole climatology (e.g. IPCC, 2001; Jones and Mann, 2004). Are the current data from the same boreholes as the Skinner and Majorowicz (1999) data? How do the authors reconcile the present results with the differences in trends between SAT and GST records and earlier studies? Are the authors now arguing that GST and SAT warming trends were different in the past, but similar now? If they are, how does this validate borehole climatology that is typically at the time scale of centuries?

Improvements to the paper can be made by focusing the paper and striving for a better organization of the material. This study includes three important topics but does not

seem to describe any of them adequately. These topics include changes in surface temperature as inferred from repeat logs, comparing transients between repeat logs and profiles constructed from SAT data, and finally GST histories. This last topic has been covered elsewhere and could be deleted without adversely affecting the other two topics.

Detailed comments follow.

1. On line 5 page 1077, the authors cite themselves for the observation that the warming determined from temperature logs is about 0.4° C greater than warming inferred from proxy sources. This observation has been around for years, and in fact with the new proxy reconstructions of Esper et al.(2001), Moberg et al. (2005), and now Hegerl et al., (in press, Journal of Climate) is not strictly correct. This point is not trivial as it has been the subject of much research (e.g., Mann et al., 2003; Pollack and Smerdon, 2004; Rutherford and Mann, 2004, among others)

2. In the Introduction the authors reference Mann et al., (2003) as suggesting that snow cover significantly biases GST histories. The correct reference is Mann and Schmidt (2003). This seems by now a bit of a straw man. Chapman et al. (2003) published a comment pointing out some serious flaws in the study, and the results of Mann and Schmidt have now been questioned by Gonzalez-Rouco et al (2003), Pollack and Smerdon (2004), and Bartlett et al. (2005). These references should be cited so that readers are not left with the impression that this question has not been investigated. This is important because the authors come to a similar conclusion later in the paper.

3. The statement that the magnitude of difference between the mean annual SAT and GST varies according to the number of days with snow cover and soil moisture is an oversimplification, as is the statement that GSTs are normally higher than SATs because snow cover insulates the subsurface from temperatures below 0° C. Even in areas where there is no snow cover ground temperatures are warmer on average than

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air temperatures.

4. I think the paragraph starting on line 8, page 1078, should be cleaned up. GST reconstructions are an important climatic indicator on their own, independent of SAT [e.g. Taylor et al., 2006]. Assumptions 1 and 2 strictly only apply when GST histories are being compared or are interpreted in terms of SAT records.

5. The POM should be defined when it first appears, not a page later. The authors define it as the period prior to SAT monitoring but later redefined as the mean of the first 16 years (1895-1910) of the SAT record. How is this period chosen? How sensitive are the results to this assumption? The authors state the POM is a free variable but if it is defined based on the SAT curve, I do not understand how it is a free variable. In Figure 3c no data is shown between 1985 and 1910. How is the POM determined in this case? The figure 4 caption implies that the uncertainty in the POM is large, but in fact the uncertainty is not discussed.

Method Section

1. The method section leaves me confused and probably needs to be rewritten. My interpretation of what the authors are doing follows. They want to compare differences between temperature logs with differences between synthetics computed from nearby SAT data over the same time period as that of the temperature logs. This methodology is actually best described in lines 230-236 of the results section. The comparison between these signals helps to validate the assertion that the difference in temperature logs is due to changes in the SAT.

2. The authors state that the early series was adjusted based on Pincher Creek (lines 227-228). The adjustment should be described. How sensitive is the synthetic to the adjustments?

3. I am confused and concerned by the use of the FSI algorithm. The authors state they are using it to compute transient components of the temperature logs. This seems

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overly and unnecessarily complicated. The FSI algorithm has many free parameters and as such it is difficult to assess the results of the algorithm and its sensitivity to these free parameters. An explanation and justification of why the FSI is being used would be appreciated. I would think the transient component of the temperature log is simply the difference between them. Why is the FSI algorithm needed and how it is applied?

4. The authors discuss how the thermal conductivity structure is derived for the FSI, but it seems to me that the thermal conductivity structure is part of the steady state signal and is removed when the logs gets subtracted from each other. The thermal conductivity structure does not change between the logs. What is the sensitivity of the solution to the thermal conductivity structure? I think the solution should be independent of the thermal conductivity structure.

Results section

1. Appendix Figure 1 seems like an integral part of the paper and I wonder if it should be included in the paper instead of left for an appendix. Also the scale of the plots is too small to see the difference between the logs. Why not plot the differences since the authors are implying that the differences are a direct result of GST change. It seems to me that the focus of the paper is the repeat logs and these do not seem to be adequately discussed or interpreted.

2. The 10 year running mean for the SAT series are shown in Figure 3. How is this running mean used? In Figure 3b the record is based on an ensemble of five records. How is this ensemble constructed?

3. The sentence beginning on line 10, page 1083 seems out of place and should probably be in the conclusions.

4. I do not seem to understand Figure 4. It shows the difference in temperature logs at TSA13 and a series of differences between synthetics constructed from nearby SAT

records. Presumably the difference between dashed and solid lines corresponds to the different thermal diffusivities used. The authors show a number of different results as a function of thermal diffusivity and POM. The text implies a single POM based on the 1st 16 years of SAT data. What is the difference between the solid and dashed lines, and what specifically do the colors mean.

5. Figure 4. Presumably the diffusivity also varies with depth because the thermal conductivity varies with depth. It is not stated that the same diffusivity is being used when the SAT is projected into the ground. Doesn't the FSI algorithm give a set of a posteriori diffusivity values? It seem these diffusivities should be used when projecting the SAT series into the ground? It doesn't make sense to use one set of diffusivities for the FSI result and a different set for the SAT projections. It seems that the advantage of using repeat logs is that one doesn’t need to know the curvature induced by the steady state part, but this figure implies otherwise. Why not solve for the best fitting diffusivity and use that? In any event this figure is too busy to decipher.

6. I am confused about the difference between the measured temperature logs. It appears that there is only data to a depth of ˜120 m. I would think that the difference here should be zero. It appears there is some sort of DC shift. The DC shift is negligible, but its presence should be explained. Is this a product of the FSI algorithm?

Additionally, the time interval over which the temperature logs were made is 20 years (1995-2005). When the logs are subtracted from each other one would expect to only see the 20 year difference ś noise. A simple skin depth argument indicates a step change only propagates to about 50 m. $z = 2*sqrt(32*20)$. Yet the systematic misfit from zero extends considerably deeper.

In terms of Figure 4 why are the synthetics shown to a depth of 250 m when the data only extends to 120 m?

7. Comparisons between TSA 13 and Carway seem to provide the best fit. This figure is discussed in lines 238-243, but no synthesis or interpretation of the results is given.

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This figure warrants more discussion. How do the authors interpret the better fit relative to Carway than the other SAT records. How do the authors interpret the poor fits shown in Figures 3a and 3c? The met. stations forming the comparison seem to be about equidistant from the borehole site, yet only Carway provides a resonable comparison. Why?

8. Figure 5 shows examples of other comparisons. Something seems wrong with the well locations or Table 1. If I understand correctly Well T9c is number 14 in Table 1 and Figure 1, and is also labeled Riverhurst in Figure 5. I suggest replacing "Riverhurst", with "T9c". Well T8c is number 8 in Table 1 also labeled Riverhurst in Figure 5. There is no Riverhurst in Table 1. The text (line 249) implies that T9C is well number 8. Boreholes number 8 and 14 in Figure 1 are clearly more than 10 km away from each other as implied in the text.

9. The authors conclude this section by stating the comparisons between the logs and SAT curves provide a good fit (lines 255-258). What is the misfit? The authors state it is within the 0.03° C of the measurements but I am not sure if this is the case, especially because much of the misfit appears systematic. Should the measurement error be more random than systematic? Is there an interpretable signal in the difference between these comparisons? My own conclusion from this figure is that the SAT record overestimates the amount of warming observed in the repeat temperature profiles, but this is backwards from the previous studies indicating greater GST warming than SAT warming.

Analysis of Snow Cover

1. This section is qualitative at best and follows a somewhat confusing trajectory. If the authors are satisfied that the SAT records explain the differences in the repeat temperature logs to within measurement errors (lines 255-258), why make arguments about how the snow cover may explain the misfit (line 275)? After all, and as cited above many studies have looked at this and found minimal effect. In the end they argue

little change in snow cover characteristics, but this is qualitative at best and seems a little circular since the argument is based on the "good" fits with the temperature logs. These arguments should be made more quantitatively.

The authors reference the Todhunter and Popham (2005) AGU abstract. It seems to me that the abstract is a discussion of the mean offset between air and ground temperatures, as opposed to how the mean changes through time which is what this paper explores. It seems these issues may be jumbled together somehow.

Section 4.2

I am not sure what section 4.2 adds to the paper. To me the manuscript is about the importance of relogging boreholes for temperature and how the differences between logs can be explained in terms of SAT data. Section 4.2 instead seems to be about GST change in Alberta and the northern plains of the US over the last 100 years. I believe much of this has been previously published.

Summary

1. Line 317 states that there are two wells where the difference between logs is greater than can be explained with SAT data. Do these refer to Gull Lake and Sundrea? As far as I can tell there temperature logs are not presented or analyzed in the manuscript. This is merely an assertion. This conclusion does not really tell us anything except that some other process than SAT changes can influence the thermal state of the shallow subsurface.

2. Likewise the statement that there has been a decrease in the influence of snow cover on ground temperatures across the region is also only asserted in the paper, and seems to contradict the claim that differences between the temperature logs is explained by the SAT data and not biased by snow. This statement implies that the influence of snow cover was greater in the past. This is not shown or discussed adequately in the manuscript. This is potentially an important result at it would contradict

the studies of Gonzalez-Rouco et al (2003), Pollack and Smerdon (2004), and Bartlett et al. (2005).

Interactive comment on Clim. Past Discuss., 2, 1075, 2006.

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