Climate of the Past Discussions, 1, S52–S56, 2005
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Climate of the Past Discussions

# *Interactive comment on* "Paleoclimatic reconstructions in Western Canada from subsurface temperatures: consideration of groundwater flow" *by* J. Majorowicz et al.

**Anonymous Referee #2**

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Borehole temperature profiles may directly record the surface temperature evolution of the past centuries, a property that explains the growing interest in their analysis. In contrasts to other proxies used for the reconstructions of recent past climates, for instance tree-rings, they do not need to be empirically calibrated to translate their variations in variations of, say, regional temperatures. Borehole temperature profiles are, however, burdened by other other sources of error that may considerably hamper the retrieval of the climate information they may potentially contain. For that purpose one has to identify and understand all other important sources of variations of the underground temperature, for instance changes in geothermal heat flux, or in the heat conductivity of the surface due to snow cover or vegetation changes. Furthermore, the temperature profiles have to be inverted back to ground temperature histories with the help of

models that simulate the diffusion of heat.

This manuscript tries to estimate the relative influence of two factors on borehole temperature profiles in Canada: one is the the past history of surface temperature and the other is advection of heat by subsurface water. The authors conclude that in this region the influence of the latter is negligible and that surface temperature histories exert the strongest influence on borehole profiles.

One of the guidelines of Climate of the Past is that articles should be clear, but in my opinion, this manuscript is far from it. The general structure is quite confusing and the abstract does not provide a guideline that could orientate the reader in the most difficult passages. Also, the individual sections are not well organized. For instance, the leading paragraphs in the introduction, which mention the possible drawbacks of the borehole approach, are followed by a general description of borehole physics. I think this ordering should be reverse. One can find in the data section paragraphs already describing some results. The figures are not introduced in numerical order. Some sentences are not complete (page 96, line 25). Equations are not numbered. The reader can find too many irritating details of this sort that make this manuscript difficult to follow. In my opinion, the English can be improved in too many passages.

After reading the manuscript, I cannot say that I was convinced that the conclusions can be reached from the evidence presented in the text. To be honest, perhaps this evidence is hard to find in the flurry of irritating aspects of the presentation, which hamper a fluid reading. However, the message that I could understand from the manuscript is that the observed borehole profiles can be fitted to simulated profiles only by using a "boxcar" model" with at least one free parameter. The reader is left to figure out what this model could look like, what is the uncertainty in the simulated profile, what is the possibility of artificial over fitting, etc. The other evidence, shown in Figure 11, could be that SAT histories retrieved from temperature profiles could match the ones derived from tree-ring data. This could be a quite interesting point in itself, but unfortunately Figure 11 is not discussed at all: again no uncertainty ranges are given , no description



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of the inverse method is offered.

The title leads to the impression that the manuscript will deal with subsurface hydrology; however, the introduction mentions this point at the very beginning and only superficially. The main goal of the manuscript is, according to the introduction, to test if factors such as deforestation, seasonal snow cover, and others may affect the temperature profiles. However, the reader finds no traces of this afterwards. In my view, the only results in this direction would be Fig6, and the results of this figure show “significant disagreement”(page105,line13) between the measured profiles and the profiles that could be expected from the near-surface temperature history. One explanation for this disagreement stems from the application of a “boxcar” model, but no explanations given of what this model is actually representing.

In page 95, line 4: "snow cover warms the surface". Could one rather write "snow cover reduces heat loss" or similar?.

Fig 1 shows the warming trend in the last 200 years, apparently derived from borehole profiles. But how has this warming trend been calculated. From inversion of the temperature profiles? by which method? were all boreholes profiles measured at the same date?, which is the error in the estimation of those trends?

The same can be said of figure 2.

Incomplete sentence in page96, line 25.

Text apparently missing in page 97, line 25.

Paragraph centered on line 5 page 97 describes some results. It is however embedded in a Data section. The data description itself starts afterwards. This is quite confusing.

Paragraph around page 98, line 10 describes some results again in the data section.

Section 4. The method for the direct simulation of temperature profiles from SAT observations is described in some detail. However, the method used for the inverse mod-



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elling (reconstructing the SAT history from the temperature profiles, in principle much more complicated), is worth just a reference.

Page 99, line 19: I think the most important factor in limiting the resolution of T(z,t) is heat diffusion itself, and not rock properties or three-dimensional effects.

In section 5, the FSI method is used to obtain the temperature profiles. The results of this method are compared with a boreholes profiles estimated from SAT, and the differences discussed. The reader may want to know which are the main drawbacks and uncertainties of the FSI method, since the problems of the synthetic simulations of the profiles are indeed discussed in much more detail.

In page 101, line 7, the “boxcar” period is mention for the first time. What is this?.

In the same section, it is stated that the results of both approaches (FST method and synthetic profiles) disagree. In which sense they disagree?

In page 102, line 2, the FST profiles are fitted to a “line”. A straight line?

Is it not possible to formulate the last paragraph in page 102 in a simpler manner?

At the beginning of section 6, “the typical profile associated with downward flowing groundwater creates a lower downward curvature”. Apart that the reader has to double-guess what is meant here, I guess that the curvature will be “downward or upward” depending of how the plot is made. Please, describe more clearly.

In the same paragraph: “This tends to show up as a recent warming in the upper part of the profile and cooling at depth when reduced”. What is reduced here?

Before discussing Figure 10 in page 104, it could be useful to present shortly the main message conveyed by this figure.

In page 105, line 22 the tree-ring based temperature reconstruction by Luckman and Wilson is a summer reconstruction. Can it be applied without restriction as representative of the annual means?



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Figure 6, please label panels a, b, c

Figure 11 shows 5 curves, but only one of them is mentioned in the text. Why include the other ones, without discussion and description of how they have been calculated?

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Interactive comment on Climate of the Past Discussions, 1, 93, 2005.