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Climate
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Discussions

Interactive comment on "Repeated temperature logs from the sites of the Czech, Slovenian and Portuguese borehole climate stations" *by* J. Šafanda et al.

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Review of "Repeated temperature logs from the sites of the Czech, Slovenian and Portuguese borehole climate stations", by Safanda, Rajver, Correia, and Dedecek.

This paper presents exciting data showing changing subsurface temperature as a function of time. The authors show repeat logs from the Czech Republic, Slovenia, and Portugal. In all cases the ground is unequivocally warming. In addition the authors use some of the deeper boreholes for a formal functional space inversion and compare these inversions with nearby ($<$ 100 km) meteorological stations. I think after improvements this paper will be of interest to a wide range of people interested in our climate system.

I think this paper can be significantly improved in at least three ways. 1. The paper should be reorganized. I suggest showing the repeat logs and then showing the formal inversions (see below). 2. While the data has been analyzed, there is little analysis of the results. 3. The paper needs a substantial edit for grammar and figures need to be called out in order.

These comments overlap a bit but I will try and elaborate the above points.

Introduction. I like the term “borehole climate observatory” for these boreholes where repeat logs are made or could be made. These are certainly “legacy boreholes”. I would include the reference to Bartlett et al., 2006 near line 10, page 633. Later on in the discussion of repeat logs a reference should be made to Chapman and Harris, 1993.

Bartlett, M. G., D. S. Chapman, and R. N. Harris, 2006, A decade of ground-air temperature tracking at Emigrant Pass Observatory, Utah, J. Climate, 19, 3722-3731.

Chapman, D. S., and Harris, R. N., 1993, Repeat temperature measurements in borehole GC-1, northwestern Utah: Towards isolating a climate-change signal in borehole temperature profiles, Geophys. Res. Lett., 20, 1891-1894.

2.1 Borehole Descriptions. There is a great deal of discussion of thermal conductivities. Perhaps this numerical information can be put in a table. While the thermal conductivity data is important, the point should be made that since the thermal conductivity does not vary with time, subtracting successive logs removes temperature variations due to variations in thermal conductivities. Removing steady state sources of curvature is an important outcome of repeat temperature logs. This important point is not discussed, but perhaps it should be. When the boreholes are discussed the figures showing the logs should be referenced. Also a location map for each borehole or group of boreholes should be included. The paper would be much easier to understand if the figures of the

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temperature logs were referenced when the boreholes are presented and if the figures and boreholes were presented in the same order. Currently they are not.

2.2 Temperature logs In the context of climate studies, I am not sure what the authors mean by equilibrium logs. This term probably needs to be defined. These logs are not in equilibrium which is why they can be used for climatic studies. This section should probably be rolled into the borehole description, since it is really a description of borehole depths and temperature tools used for the measurements. As such it does not really describe the temperature logs.

3 Results I would start with an analysis of the repeat logs for a couple of reasons. If comparisons between repeat logs and SAT data are good, then we can be more confident air and ground temperature variations are tracking each other and then have more confidence in the long-term inversion of the deeper logs. The authors start by correctly emphasizing that the repeat logs all show that the ground has been warming over approximately the past 20 years. Temperature changes at a depth of 20 m are identified. This data should definitely go in a Table. Figure 2 is very difficult to read. It should also be made consistent with figures 1 and 3. The authors use a 1-D finite difference method to compare repeat temperature logs with the diffused version of SAT data. The model is not adequately discussed in this paper. The model is initialized with what the authors term the pre-observational mean, but in this context it is not a mean temperature prior to the meteorological observations as originally described in Chisholm and Chapman [1992] and later advocated by Harris and Chapman in a series of papers [1998, 2001, 2005]. In the current context the POM is the mean temperature of the first 50 years of SAT data. I wonder if it should simply be called the initializing temperature, so as to not confuse it with a pre-observational temperature. I also think that using the first 50 years is arbitrary. How is this interval chosen? This is an important point because the results strongly depend on the choice of initial temperature as shown in the Harris and Chapman studies. Why not follow Harris and Chapman and choose the initial temperature that optimizes the fit? Either way these decisions and

their implications should be discussed. It is true that by differencing the logs the influence of the actual POM is less important, but the results are still sensitive to the choice of the POM. This can be seen using standard analytical approaches, say in terms of step functions. The point is that in the differencing of initial conditions do not cancel because the time weighting is different. The discussion of the frequency dependence of the diffused SAT signal can probably be eliminated since as the authors conclude annual mean SAT data suffices. The frequency dependence is a main point in Harris (this issue, in review). Likewise for these time scales the results are relatively insensitive to the effective thermal diffusivity analysis. Finally I think the authors stop short. The repeat logs are analyzed with nearby SAT data to determine if variations in these two signals are correlated. There is no analysis or statement as to whether the authors think these two signals are coupled and if so to what degree. I think that without this step the paper is little more than a data report. To make the argument as to whether these signals are coupled may require some additional analysis. For some of the data pairs the present analysis does not convince me that the signals are adedquately coupled, and there are many processes that may decouple air and ground temperatures. If however the authors can demonstrate that the two signals are coupled, then I think the inversions of the deep temperature logs and comparison with longer SAT time series is warranted. However, if the authors cannot demonstrate that the signals are coupled then what is the point of the comparison of the inversion with the SAT record at a long time-scale.

3.1 While there is not much description of the FSI analysis, the authors have been using it for quite some time and it has widely been discussed in the literature. I know that one capability of this inversion algorithm is the ability to simultaneously invert repeat logs and this appears to have been but does not seem adequately discussed. Instead the authors spend a couple of sentences discussing the potential range of thermal diffusivities which appear to make little difference. My intuition is that the inversion is not very sensitive to the thermal diffusivity in the same way that it is not very sensitive to time. Both terms enter the equation in a similar way (e.g., see results in Harris,

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currently being reviewed in this special issue). Does the simultaneous inversion of repeat logs, which is the focus of the paper, make much difference, relative to a single log? Finally the authors overlay the inversion solution on SAT data after taking out an arbitrary shift. All solutions to the diffusion equation, and particularly the FSI solution represents a weighted average where the time window of the averaging function grows with time in the past. I think these overlays make a quantitative assessment of the two signals very difficult. For example the Portugal data is singled out as being particularly good, but in fact the SAT data dips at the same time that the inversion shows a rise (around 1880), and detracts from how confidently we interpret the more recent past where the two signals appears to be well correlated. Again, it seems that the analysis stops short. What do the authors think of these comparisons?

Interactive comment on Clim. Past Discuss., 3, 631, 2007.