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The Cryosphere Discussions

# *Interactive comment on* "Borehole temperatures reveal a changed energy budget at Mill Island, East Antarctica over recent decades" *by* J. L. Roberts et al.

**Anonymous Referee #3**

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**General comments:**
The study by Roberts et al. presents a surface temperature reconstruction based on a numerical heat transfer model which is fitted to the temperature measurements of a borehole. The study gives a brief overview of the used methods including two fitting procedures, provides some analysis of potential uncertainties of the temperature reconstruction, and gives some interpretation of the obtained results. The authors point out that such studies are important since long-term climate records are very spars in Antarcica. The major finding of the study is that surface temperatures have increased since 1980 which is most likely associated with changes in the surface energy balance. The study will contribute to a better understanding of the local climate



evolution at the study region in east Antarctica. However, there are some major concerns about the structure, the methods, and the results which should be addressed before final publishing.

The introduction is very short and gives only little specific information about the study and its motivation. In contrast, there are a lot explanations about paleoclimate reconstruction which is not part of this study. The introduction should be more focused on the specific aims of the study and set them into a wider context.

There are only little information about the study site. A more detailed site description about e.g. climate conditions and landscape features would make it much more easier for the reader to put the results into a geographic context. Also a map of Antarctica with a marker on the study site would support this. The description of the performed temperature measurements might be better placed in a separate method section together with the description of the model and the fitting procedures.

The method descriptions should be better structured. On the one hand there are several redundancies while on the other hand there is a lack of important information (see specific comments).

In general, there are some structural inconsistencies which make it hard to follow the manuscript. It might be better to clearly separate methods, results, and discussion (see also specific comments).

The discussion is very general. Instead of explaining what processes could have changed the surface energy balance, the authors should focus on the interpretation of the results. Are there any studies which support that e.g. sea ice coverage or cloudiness has changed after 1980? Are there any known environmental changes that correlate with the timing of the temperature increase?

I suggest to put this study into a much wider context than just showing that the temperatures have increased after 1980. The obtained results could be compared more extensively with other data sources and studies. Although direct measurements from climate stations are not available at the site, there might be other sources such as reanalysis products (as suggested by the authors themselves) or the drilled ice core.

**Specific comments:**
p. 2576/l. 4: I would call this “zone of zero annual amplitude”

p. 2576/l. 22-p. 2571/l. 3: This sentence could be written more understandable.

p. 2578/l. 22: “contributing up to 0.06 K” This is a result and should be placed there.

p. 2579/l. 6: EQ. 1 is perfectly correct but could be written more comprehensive so that the single heat flux terms can be directly distinguished.

p. 2579/l. 13 The equation should be written separately as EQ. 2. Also some further information on the equation would help to better understand the approach. Please provide some references at least.

p. 2579/l. 8-13: This is not a comprehensive and satisfying description of the applied parametrization especially of the thermal conductivities. A realistic parametrization of the thermal conductivities especially of the upper most layers is very crucial for the calculations. Please explain why the used assumptions are adequate and if possible give uncertainty ranges.



p. 2579/l.18-22: It would be helpful to have some further information on the boundary conditions. What exactly is a “time varying prescribed surface temperature”? What kind of function is assumed – a polynomial?

p. 2579/l. 22 “see below” Please refer to a specific section. This statement is not a method but a result.

p. 2570/l. 23-26: Why are the assumed velocity profiles reasonable? Are there any other studies supporting this?

p. 2580/l. 8-12: How is the initial temperature condition set? I recommend to check whether a 10yr initialization period is long enough for a 130yr run down to a depth of 100m. The initial temperature conditions could strongly determine the temperature evolution and affect the fit. Hence, a sensitivity test on the initial conditions is strongly recommended. It is also recommended to extend the sensitivity tests to all parameterizations including the thermal properties of the firn layer.

p. 2580/l. 21-22: What is meant by initial temperature history? Is it the used initial temperature condition or is it the initial temperature time series for the fitting procedure? If it is the initial time series, what is the intension behind the “depth to time” transfer function? Temperature transport by vertical ice advection is much more inefficient than heat diffusion. The authors state this by themselves (p.2579/l.24). Hence, the question arises how sensitive is the fit to the used starting condition?

Sect. 3.2.2: Please provide a more precise description of this method. E.g. it is hard to understand what parameter of a piece wise linear function is considered a particle.

p. 2582/l. 18-23: This is repeated several times in the manuscript.

p. 2583/l. 7: It would be helpful to provide a figure with the temperature profile. Also the results from the fitting procedure could be illustrated. This would help to gain a better impression of the quality of the fit.

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Interactive comment on The Cryosphere Discuss., 6, 2575, 2012.