The Cryosphere Discuss., 5, C1770–C1773, 2012
www.the-cryosphere-discuss.net/5/C1770/2012/

The Cryosphere Discussions

Interactive comment on "Borehole temperatures reveal details of 20th century warming at Bruce Plateau, Antarctic Peninsula" *by* V. Zagorodnov et al.

Anonymous Referee #2

Received and published: 21 January 2012

The manuscript presents new temperature data from the Antarctic Peninsula, a region of rapid changes with notoriously few direct temperature records, and is an important contribution to the field. The authors measured borehole temperature with state of the art equipment in 2 boreholes, and inverted the record to produce a 200 year temperature history at the site, giving context to the recent warming trend. It fits well within the scope of "The Cryosphere". They present an important observation, in a fast-changing part of the Earth system, and hence it is appropriate for this journal and should be published after minor revisions.

The conclusions of the manuscript are very likely to be robust to any small revisions,

however, for methodological reasons I would still like to see a slightly more formal treatment.

The main thing that I would like to see addressed concerns the reconstructed temperature history. I was not sure why the match between the model-based reconstruction and the measured temperature profile was not better. In principle, it should be possible to match the measurements almost exactly with some arbitrary history (not that this history would necessarily be accurate). So it seems as if there was a choice, to do something different, but I could not tell from the manuscript exactly what that choice was. At the least the authors should explain exactly what choice was made. Even better would be that the authors find a solution that fits the data within their respective measurement errors.

Of course, the inversion of borehole temperature is a classically underdetermined problem and it would be helpful for the authors to explain this a little to set the stage. One way to explain the nature of the problem in plain language is to say that a borehole temperature inversion produces the average temperature over some time interval, and the length of this time interval itself increases with age. Thus the solution for 1900 CE is actually the average temperature between something like 1850 CE and 1930 CE, with non-Gaussian weighting. In contrast, the solution for 1000 AD is the average temperature between something like 500 and 1300 CE.

The authors used established methods in their measurements and modeling. They devoted a lot of effort to the discussion section, which is particularly interesting. However, I am disappointed that the impact of the choices made (said accumulation rate, or initial conditions) on the surface temperature solution is not more explicit.

Major comments: 1. Section 6.2 emphasized that the accumulation rate may have changed dramatically over the last 50 years, maybe as much as 100%. However, the inversion was made using a constant accumulation rate. I agree that in the absence of information, it is best to keep a constant, but it would be interesting to show the inver-

TCD

5, C1770–C1773, 2012

sion with a few different accumulation rates. If possible, it would be interesting to show the inversion exercise using variable accumulation produced by the RACMO2 model for this site (mentioned in section 6.2). I suspect that changes in the accumulation rate may give an additional error bar on the date of the warmest year (1995), stated in section 6.4. In other words, at the very least a sensitivity test is demanded by the known deviation of the real accumulation from a constant value.

2. More generally speaking, several versions of (surface T, accumulation, basal heart flux) were discussed for the initial conditions, but the inversion was run with only one value, and the impact the initial condition have on the inversion is not clear. If the model would be run with several initial temperatures, and several solutions plotted, the reader would have a better idea of the impact of the initial condition on the surface temperature reconstruction. The model was run for 200 years, but it is not said how many of these 200 years feel the impact of the initial conditions. In a recent paper by Muto et al (GRL 2011), the inversion of a 90m profile was run for 500 years, and only the last 100 years were plotted. I do not disagree with using a 200 year inversion, but I don't think that it is accurate to show all 200 years as a solution to the inverse problem/data.

3. In section 6.4, page 3070, line 18. The warmest temperature corresponds to approximately 1995, +- 5 years. It would be more accurate to show an age spread on that date: the inversion of borehole temperature shows an average temperature, and the averaging window widens with time. It would be more accurate to say that a decadal mean centered around 1995 was the warmest decade, if the averaging window was a decade at that time. Muto et al (GRL 2011) show that 10 years before measuring, the spread is already 25 years, and 20 years before measuring, it is 43 years.

4. At page 3070 line 19, it says " Our precision is limited to +-5 years due to thermal diffusion and precipitation rates". This value conflicts with Muto et al (GRL 2011), and is not explicitly justified. More justification, or a citation is needed.

Minor comments: 1. abstract, line 19 "derived by an inversion technique". Maybe you

TCD
5, C1770–C1773, 2012

could say a few words about what type of inversion technique: Monte Carlo, linearization? Otherwise, this sentence is not very useful. 2. Section 4, page 3059 line 4, and page 3061 line 21. Precision (how one measurement differs from another) and accuracy (systematic bias) are not distinguished in this section. They are considered to be the same. The authors could be more precise. 3. Section 6.3 equation 7: It seems that Delta T should actually be Gamma, the lapse rate. 4. Section 6, page 3068 line 25. The section 6 is very interesting, and it contains enough information to give an error bar to the estimate of 0.60°C/100m. It would make the comparison with other estimates more clear. 5. A temperature maximum is found in 1995 (page 3070). However, no such temperature extremum is visible in the closest weather stations, Faraday and Rothera. Could the Authors comment on it? 6. I find figure 2.b. misleading. At first sight, I thought that the reconstruction in blue should match the data in black, and that the point of the figure was to show a misfit. Is there a logic to the scaling used, logarithm maybe? If your point was to show the similarity, then why not align the extrema? If the Authors are convinced by the value of this figure, they should justify their creative presentation a little more clearly in the legend, or in the text. (e.g.: The scale in figure 2.b. was adjusted to show ...)

Overall, the paper is very well structured, and the discussion presents information not compiled anywhere in such a clear manner. Figures are well presented and support the main point of the paper well. Tables are welcome. It is not very common to see data explicitly presented, but it is here a very efficient way to convey the author's point. I recommend the paper for publication with revisions outlined above.

Interactive comment on The Cryosphere Discuss., 5, 3053, 2011.