Anonymous Referee #1

Thank you for pointing out an important aspect of the paper where we could improve our discussion.

RC. The principal concern of the referee is the validity of our basic assumption regarding the one-dimensionality of heat transfer (vertical direction ony) for LARISSA Site Beta (L-B), as stated in the paper in the form of the boundary problem (2).

AC. The one-dimensional model can be applied if $w\frac{\partial T}{\partial z} \gg u\frac{\partial T}{\partial x}$. The horizontal and vertical advection rates are known: $w \approx 2\ m\,yr^{-1}$, $u \approx 10\ m\,yr^{-1}$ (Table X, see below). The vertical temperature gradient near the surface is $\frac{\partial T}{\partial z} \leq 0.05\ {}^{0}C\,m^{-1}$. The elevation difference between the divide and L-B site is 36 m. The local air temperature vertical gradient of $0.006\,°C\,m^{-1}$ allowed us to estimate the possible temperature difference T_{15}=T_s between the L-B site and the glacier divide, as 0.2°C, therefore $\frac{\partial T}{\partial x} \approx \frac{0.2}{2000}\ {}^{0}C\,m^{-1}$. Thus, the horizontal advection is about three order of magnitude smaller compare to the vertical advection, which supports our one-dimensional model assumption.

RC. A minor editorial correction is needed to put the right dimensions of the specific heat capacity in Eq.(4) and thermal conductivity in Eq.(5), i.e J/(kg K) and W/(m K), respectively.

AC. We made corrections in manuscript.

The authors are considering adding the following table to section 2 – Site characteristics.

Table X. L-B site characteristics

Surface elevation:	divide: 2012 *m*; ice core site: 1976 *m*
Thickness:	divide: 460 *m*; ice core site: 447 *m*
Distance east of the divide	~2 km
Surface slope	~0.025 at the core site
Surface velocity	10 (+/- 4) $m\,yr^{-1}$
15 m temperature (average annual)	-14.8 $°C$
Accumulation rate (1963-2010):	~2 $m\,yr^{-1}$ m/yr
Pore close-off depth	80 m
Bed temperature	-10.2 $°C$
Geothermal flux	88 $mW\,m^{-1}$

Anonymous Referee #2

AC. Dear referee, thank you for constructive critique and useful suggestions.

RC. 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.

AC. The difference between the measured and reconstructed temperature-depth profiles is in the range of the measurement errors; suggested version of the Fig.2 see below. The decision that was made during modeling was to use the simplest history that fit the observational data within the uncertainty. It is true that a closer match could have been made with a more complex history, but as the reviewer points out, these additional adjustments may not be real. They could obscure the main trends that the data support.

The inverted T_s for accumulation rate å=$1.9*10^3$ $kg\ m^{-2}\ y^{-1}$ is shown in Fig. 2b (new version) and 4. Fig. 2b also shows the error of the reconstructed temperature in the case of the measured temperature error equal to 0.1°*C*. The error of the reconstruction increases with past time (difference between black curves). For more details see AC in Minor comment 6.

We suggest modification of Fig. 2 as shown below.

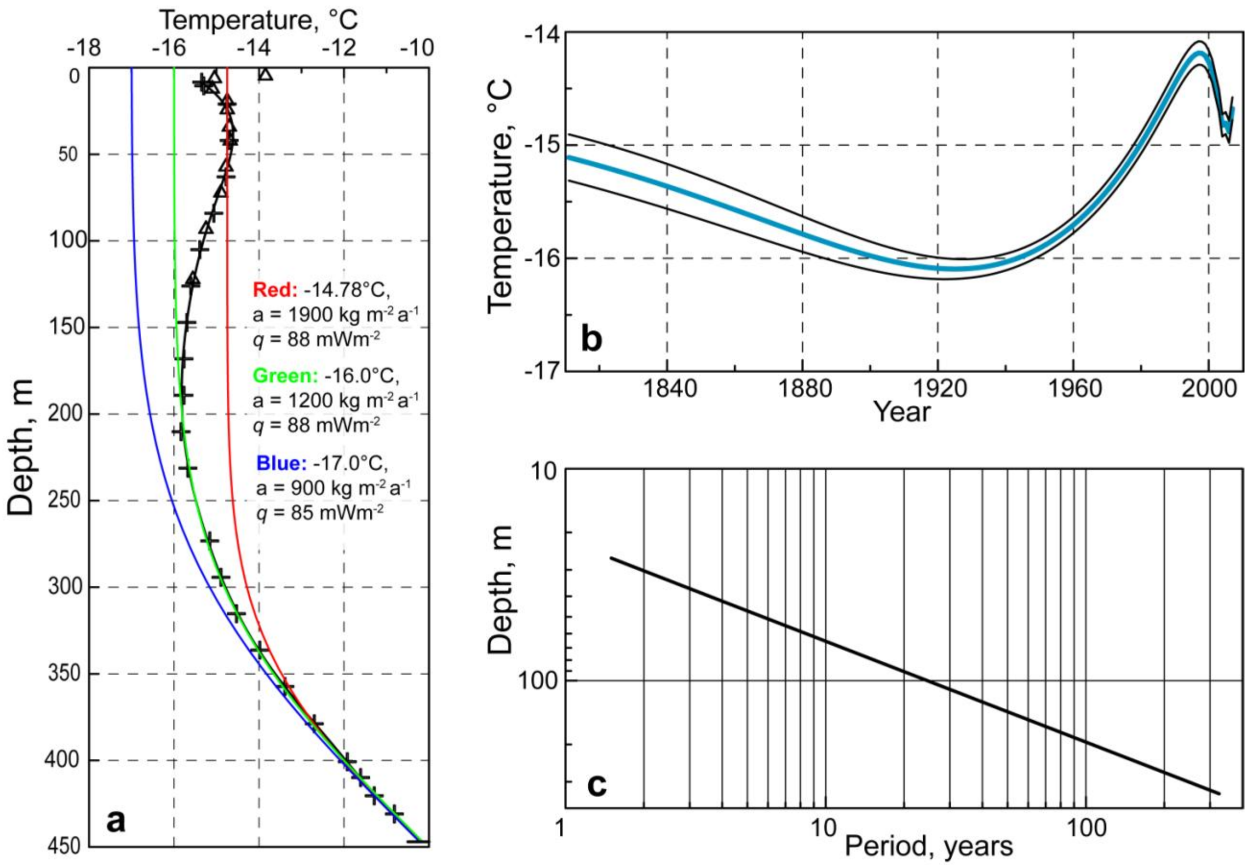

Fig. 2. Left panel (a), borehole temperatures measured at LARISSA Site Beta and steady state model results used for estimating the range of geothermal heat flux and mean accumulation. Crosses are the thermistor-measured values for L-Bb; triangles are PRT-measured mean temperatures in L-Ba spanning five months; the colored lines are steady state model results that bound the likely range of conditions for the profile (see the text for the parameters). Right panels: b - is the inverted T_s temperature history (blue) and possible error (black) proportional to instrumental inaccuracy of 0.1°C; c – is attenuation depth of surface temperature oscillations in glacier.

RC. 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.

AC. The glacier or rock filters high frequencies in the climatic temperature signal that propagate from the surface downward. In the case of a glacier the temperature oscillations propagate due to the combined effect of heat conduction and the vertical advection rate. Near the surface the main mechanism of heat propagation is the heat conduction while from some depth the heat propagation is due more to the vertical advection. Fig. 2c shows how the periodic climatic signal from the surface attenuates with depth. The region on the left of the curve derives which periods of the temperature oscillations at the surface do not penetrate at given depth. The region on the right of the curve shows periods that can penetrate at given depth.

Authors are considering an additional section 6.1 to 6 Discussion.

RC. Major comments:

RC 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 inversion 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.

AC. Certainly "the inversion of borehole temperature is a classically underdetermined problem". We approach the uncertainty of boundary conditions, specifically history of accumulation rate, by: 1) steady state model, 2) surface observations and atmospheric models data and 3) inverse modeling with different boundary conditions. The steady state model give us å=900-1900 $kg\ m^{-2}\ a^{-1}$ range of accumulation (Figure 2, left panel); the highest å value is possible at measured T_{15}=-14.78 *C*. Surface observations: a) atmospheric models, based on surface observations, provide å≈2000 1900 $kg\ m^{-2}\ a^{-1}$ for the B-L site region; b) ice core proxy data demonstrate a warm 18th century and a cooling trend during 19th century. We run inverse model at T_{15}=14.78 *C*, *q*=88 $mW\ m^{-2}$ and å=1000, 1500, 1900 and 2000 $kg\ m^{-2}\ a^{-1}$. The best compliance of T_{15}, *q* and proxy temperature trends take place at å≈1900 $kg\ m^{-2}\ a^{-1}$. In order to keep the paper less loaded with transitional results in final version of the paper we present the T_{15} reconstruction at å≈1900 $kg\ m^{-2}\ a^{-1}$ only.

RC 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.

AC. The T_{15} reconstruction was done for the whole data set from the BHb (about 1500 years). In the paper we present T_{15} for the last 200 years.

RC 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.

AC. The warmest period for the last 70 years is from 1990 to 2000. The average temperature in this period is -14.2°*C*.

As to the discussion about the averaging window, I would note that it does not lead us to a clear understanding of what it means. For example, let us consider periodic oscillations of temperature at the surface. Let us assume that the period of such oscillations is large enough. Then one can see that temperature even at large depths will have practically the same shape as at the surface. This means that words about an age spread of the temperature signal depend on the shape of the temperature variations at the surface. The words about an age spread of the temperature relate only to rectangular finite temperature signals. Better to think that high frequency harmonics of the climatic signal dissipate first and heat propagation is vary with the depth due to reduction of vertical advection.

RC 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.

AC. The precision is shown in modified Figure 2b.

RC. Minor comments:

RC 1. abstract, line 19 "derived by an inversion technique". Maybe you could say a few words about what type of inversion technique: Monte Carlo, linearization? Otherwise, this sentence is not very useful.

AC. Changes in abstract, line 19: …derived by the Tikhonov regularization inversion technique (Tikhonov and Samarskii, 1990).

RC 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.

AC. Thermistor data (deep borehole, precision +/-0.1 C) represent old climatic history and were used for T_{15} history reconstruction. Dry borehole, 120 m (precision +/-0.05 C) PRT data were used for determination of active layer depth and cross verification of thermistor measurements. We found that crossfire bias is in the range of instrumental and model precision.

RC 3. Section 6.3 equation 7: It seems that Delta T should actually be Gamma, the lapse rate.

AC. Correction in equation 7 is made.

RC 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 ^{o}C/100 m. It would make the comparison with other estimates more clear.

AC. To estimate vertical temperature lapse rate we have only one option - compare T_{15} in the glacier environment (L-B and DP) and air temperature in a marine-terrestrial environment at two weather station sites. We found that T_{10} at the Antarctic Peninsula region data is sporadic and possibly not precise enough for determination of vertical temperature lapse rate at glacial locations. Possibly either could be different than what we determine. For instance, DP-L-B lapse rate is 0.0079 $^{o}C\ m^{-1}$, or 0.79 *C*/100 *m*. It is likely that significant difference from other observations (DP-FV, DP-RO) is caused by instrumental error from the DP site. Because we are dealing with a relatively short period of observations at weather stations we compare 11-years average air temperatures with reconstructed T_{15} at the B-L site. We compare different types of data, obtained in different environments that represent close, but different periods of averaging. Therefore we are consider not estimate the error bar.

RC 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?

AC. We noticed the absence of 1995 air temperature maximum in the meteorological data and one possible explanation of the observed differences shown in Figure 4 could be a different energy balance regime or climate regime at marine-terrestrial and glacier environments at 2000 m elevation. Our study site is both higher elevation, and westward, of Faraday and Rothera. As we note in the text, although the warm period is centered on 1995, it refers to a multi-year period of greater warmth relative to decadal temperatures on either side of it. In the eastern Peninsula, 1995 itself was a very warm year, as evidenced by the break-up of the Larsen A ice shelf. Despite some further warm years in the early 2000s, recent weather records suggest some cooling relative to the 1990-2006 period. We found this fact of T_{15} maximum in ~1995 worth of mentioning in the paper.

RC 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 ...)

AC. Authors agree that original Fig. 2b is confusing. The Fig.2 is rearranged as above.