Reply to the reviewers, December 16, 2014

"Geothermal evidence of the Late Pleistocene-Holocene orbital forcing (example from the Urals, Russia)" by D. Y. Demezhko and A. A. Gornostaeva

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Reply to the reviewers

We are grateful to both J.-C. Mareschal and Anonymous Referee #2 for their positive response to our paper and helpful suggestions. We appreciate the constructive feedback. We have tried to answer your questions in detail and incorporate all of your suggestions into our revised paper.

J.-C. Mareschal (Referee #1)

One needs to go back to the first paper to see the original temperature profile data, which is unfortunate.

We will include the figure with the original temperature-depth profile from borehole SG-4 in the revised paper (Fig 1).

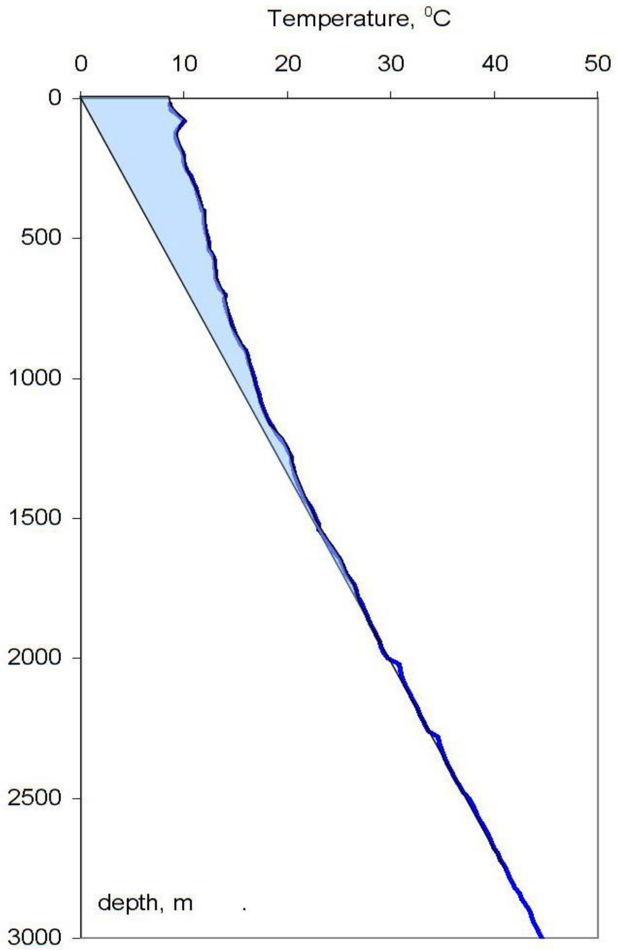

Fig. 1. Temperature-depth profile from the borehole SG-4 (Demezhko and Shchapov, 2001)

The GSTH that is used as input is smoother than the one in the 2001 paper, and seems dominated by a single frequency which happens to be the same as that dominating the insulation curve. Is the suggested correlation an artifact of the smoothing that has selected the proper frequency in the GSTH?

We slightly smoothed the initial GSTH within the band of uncertainty of the reconstruction. This smoothing is necessary in order to avoid false climatic episodes in the SHFH. "Roughness" of the GSTH presented in 2001 paper results from the bad smoothing. The smoothing was needed to be done in the initial 2001 paper.

The GSTH covers 80,000 years while the SHFH covers only 35,000 years. Would the correlation remain for the entire 80,000 years? It might be that the resolution of the GSTH decreases with time and does not allow the reconstruction of the SHFH, but one would have a lot more confidence that the correlation displayed in Figure 2 is real if it could be demonstrated over a longer time interval.

Because of the decrease of the GSTH resolution with time the interval from 35 to 80 kyr BP presented in 2001 paper does not contain any noticeable GST variations. The SHF may be considered as a constant on this time interval. For correct comparison of SHF with insolation variations it is necessary to smooth the insolation curve in uneven running windows according to the resolution power of the SHFH. After this procedure, the insolation curve on the interval 35-80 kyr BP will also not contain any significant insolation changes. An addition the interval 35-80 kyr BP does not almost change the correlation coefficient.

We will add the corresponding comment into the final revised text.

Authors from the same group had also determined surface heat flux changes from a borehole in Karelia (Demezhko et al., 2013) which suggest a similar correlation between SHFH and solar insulation. Contrarily to the Urals, Karelia was covered by an ice sheet during the last glacial cycle and it is difficult to understand how the relationship between ground surface conditions and solar heat flux could be same for a site covered by an ice sheet and a site free of ice, with the ratio of heat flux to insulation being 0.0012-0.0013 for both sites.

Firstly, a little remark. The ratio of the heat flux to insolation is 0.012 and 0.013 for the Urals and Karelia respectively.
We also found surprising no visible traces of the ice sheet influence in the thermal field in Karelia. We discussed this issue in the mentioned paper (Demezhko et al., 2013). It is possible that the duration of Scandinavian ice sheet existence in Karelia (from 23 to 13 kyr BP - Lunkka et al., 2001; Saarnisto and Saarinen, 2001) was too short relative to the time resolution of GSTH or SHFH and thus it did not leave a detectable trace in contemporary temperature field.

Incidentally, we find about the same values for the ground surface temperatures during the last glacial maximum in Canada as in Europe (Chouinard and Mareschal, 2009). The difference between Canada and Europe is that present ground temperatures are much higher in Europe than in Canada with stronger perturbations of the temperature profile in Europe. Were we to apply a similar analysis to derive SHFH in Canada, would we find that the ratio between heat flux and solar insulation is only 0.0003?

If the amplitude of Pleistocene/Holocene warming in Canada is much lower than in Europe, one can expect much lower values of the amplitude of the SHF changes. We digitized the GSTH from Lockerby (Chouinard, Mareschal, 2009) and transformed it into the SHF history. The amplitude of the SHF changes here was found two times lower than that in the Middle Urals (Fig.2). Hence the ratio of amplitudes of the SHF and insolation changes is two times lower, i.e. 0.006.

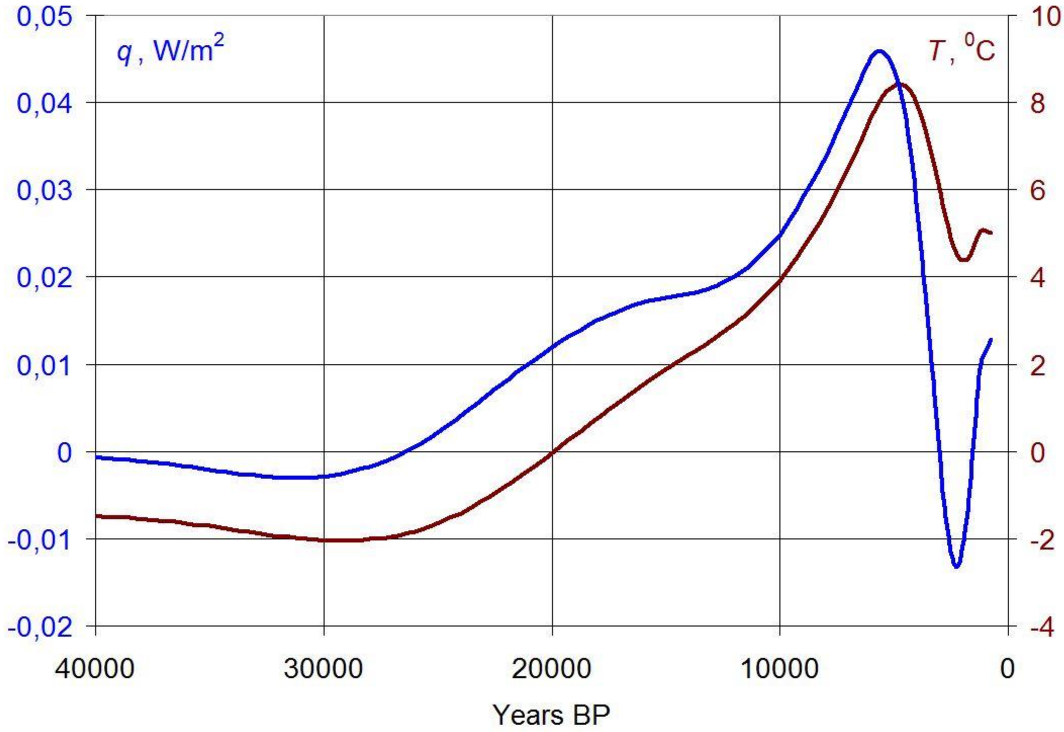

Fig. 2. The GST history T for Lockerby (Chouinard, Mareschal, 2009) (brown line) and the SHF history q calculated according to the method described in the paper (n = 3, E = 2500 $\mathrm{Jm^{-2}\,K^{-1}\,s^{-1/2}}$, blue line)

However, it should be noted that the relation between insolation changes and the absorbed heat may be indirect. Especially if we consider such a long time intervals. For example, orbital forcing variations could change the North Atlantic current system. The secondary heat source distributed in the atmosphere arose, which could significantly affect spatial distribution of the SHF-insolation ratio. Given the mainly westerly flow in the middle and high latitudes of the Northern Hemisphere one can assume that this heat source had a great influence on Europe and then on the Urals. However, such interpretation is beyond the scope of the paper.

It would be useful to state that there was no ice cover over that part of the Urals during the LGM.

We will add the corresponding mention in the revised paper.

The equation 3 in the paper was actually derived in Carslaw and Jaeger (1959, p. 63, C1513 equation 8).

We will include this reference in the revised paper.

Anonymous Referee #2

The authors of the present paper present another method, reminiscent of the Green function approach, in which the temperature profile is decomposed as a sum of step-wise temperature changes, for which the diffusion equation can be analytically inverted. The response is then linearly added. The method is tested in a simple setting of a periodic surface temperature change.

Some confusion arose here. We do not consider the method of borehole temperatures to GSTH inversion in the present paper. We have developed this method earlier and since we obtained a number of GSTH reconstructions using it (Demezhko and Shchapov, 2001; Demezhko and Golovanova, 2007; Demezhko et al., 2007; 2013) including the reconstruction of GSTH from the borehole SG-4. In this paper, we present the algorithm of GSTH to SHFH transformation. This is directly written in the first line of the Abstract: "*We use early obtained in the Middle Urals geothermal reconstruction of the ground surface temperature (GST) history to determine the surface heat flux (SHF) history over the past 35 kyr. A new algorithm of GST-SHF transformation was applied to solve this problem*."

(1) My first comment is related to the English, which is really poor.

We will use Copy-Editing Services to improve English.

(2.1) The method has been tested in a very simple context - apparently just assuming a periodic temperature evolution. The details of this test are given in another publication by one of the authors, which is original written in Russian. Much more details have to be included here and, more importantly, the method has to be tested in a more realistic setting.

Finite-difference schemes are usually tested on synthetic examples, for which the exact analytical solutions are known. Our algorithm of GST-SHF transformation is based on the known solutions for polynomial GST changes (Carslaw and Jaeger, 1959; Lachenbruch, 1982) and then was applied to periodic temperature variations. It is mathematically correct. Besides a harmonic signal is quite real function. Any arbitrary temperature variations may be represented by Fourier series. In the paper, we present the result of testing, which an interested reader can easily verify. The testing procedure itself is trivial but very cumbersome. Therefore, we would not like to show it fully in the paper.

We will add a graph of the relative error of heat flux estimation versus the ratio of an oscillation period to a sampling time (i.e. a discretization density of a periodic temperature history) – Fig. 3.

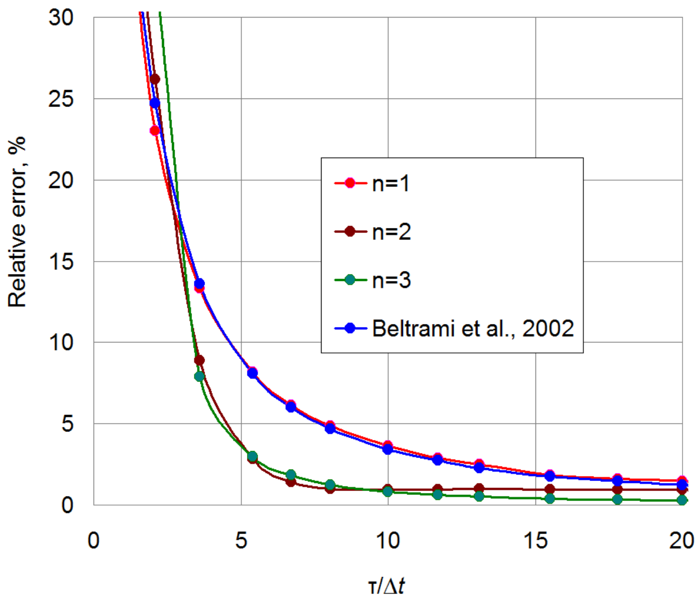

Fig. 3. Relative error of heat flux estimation versus the ratio of an oscillation period τ to a sampling time Δt

(2.2) I can imagine that the connection between near-surface temperature and heat flux is not stationary in time, for instance if that area was intermittently glaciated, so that I have reasonable doubts that a history of surface temperature and/or surface flux can be directly linked to the external climate forcing.

We assume that two different questions are mixed in this comment. The first one is about the relation between GST and SHF changes. The second one is about the ratio between the SHF and insolation (external radiative forcing). The relationship between the heat flux changes and temperature changes at the surface are described by non-stationary heat conduction equation and Fourier law. It also depends on the thermal properties of rocks (we represent these equations in the paper). This relationship is not affected by the existence or the absence of a glacier. An ice sheet can change the ratio between solar forcing and the surface heat flux. However, there was no ice sheet in Pleistocene in the region under study (Velichko, 1997; Svendsen, 1999; 2004).

We will add the corresponding mention in the final revised paper.

To what extent does this new method provide different results than more traditional methods, which are the advantages, the drawbacks, the limitations?

We do not fully understand what "***more traditional methods***" the Referee means. The method of GSTH-SHFH transformation cannot be called a completely novel. We wrote (P. 3318, Lines 24-26, P. 3319, Lines 1-3): "*Wang and Bras (1999) proposed the integral relation to estimate surface heat flux (SHF) changes from ground surface temperature (GST) variations. A finite-difference approximation of the relation between the GST (represented by a piecewise linear function of temperature), and the SHF was proposed by Beltrami et al. (2002). SHF history reconstructions based on borehole temperature data were made in timescales from several centuries to millennium (Beltrami et al., 2002, 2006; Huang, 2006)*". Our paper is devoted to further developing of the known method and extending its paleoclimatic interpretation. Compared to previous publications on this topic, we have:

1. improved the algorithm of GST-SHF transformation;
2. extended the temporal coverage to several tens of thousands of years;

3. applied the known procedure of orbital tuning (Martinson et al, 1987; Shackleton, 2000; Bender et al., 2002; Parrenin F. et al, 2007) to geothermal reconstructions of the surface heat flux;
4. obtained the SHF reconstruction in the Urals for the last 35 kyr for the first time.

We will extend the conclusion part of the paper and summarize what we have done there.

(2.3) The manuscript very briefly quotes ’relative errors’ when using the Beltrami method, but more details are needed. Is the relative error refereed to the variability of temperature or is it just the error expressed as percentage of degree C (why not degree K?) Is that figure the maximum or the mean relative error?

Neither degree C nor degree K since we are talking about a heat flux. Using the expression ***’relative errors’***, we had in mind the ratio of an absolute standard error of the SHF estimation (in W/m^2) to the real amplitude of heat flux variations (in W/m^2).

To clarify this point we'll include the corresponding explanation and a graph of the relative error of heat flux estimation versus the ratio of an oscillation period to a sampling time (i.e. a discretization density of a periodic temperature history) in the revised paper – Fig. 3.

(3.1) The authors compare the reconstructed surface heat flux with the orbital insolation forcing. This agreement is not perfect and the authors claim that this is due to ’inertial climate factors (feedbacks)’. The climate inertia are definitively not feedback processes. But independently of this, what are those factors producing a lag of several thousand years?

We agree with this remark and apologize for this misprint. Of course, we had in mind not *’**inertial** climate factors’* but *’**internal** climate factors’*.
In the response on the comments by J.-C. Mareschal (Referee #1) we already noted that the relation between insolation changes and the absorbed heat flux may be not direct (see our comments above). However, we consider that the shift of a few thousands of years between insolation and the SHF comes rather from the overestimation of the apparent thermal diffusivity.

Is the new value of the diffusivity still within reasonable bounds?

The experimental studies of the rock’s thermal properties in the Urals showed that the thermal diffusivity value varies within the limits of $(1±0.3)×10^{-6}$ m^2/s (Demezhko, 2001).

Could the diffusivity be not constant over time? (I guess that the properties of soil changing as the climate worms would also influence the diffusivity).

This remark is valid only for the upper part of soil (the first few meters, so-called 'active layer'). At the depths from several hundred of meters to several kilometers, which contain the Pleistocene/Holocene climatic signal the thermal diffusivity may be considered constant.

(3.2) Also the tuning destroys the independence of the records. The subsequent analysis including linear regression to estimate the sensitivity of surface flux to orbital forcing is thus flawed. This analysis does not include any statistical

uncertainty estimations arising from the regression analysis, that should anyway be modified because the records have been a posteriori tuned to agree better than they do. Related to this is the fact that many glacial-interglacial records display the same form as the ones shown in Figure 1. The conclusion that the driver of the surface heat-flux is the orbital insolation is thus difficult to prove or disprove.

This comment is valid. Indeed, after the procedure of tuning the curves become dependent.
The procedure of orbital tuning is commonly used to dating paleoclimatic ($\delta^{18}O$, δD) records represented global temperature changes (Martinson et al, 1987; Shackleton, 2000; Bender et al., 2002; Parrenin F. et al, 2007). The using of this procedure a priori assumes that temperature changes are resulting from orbital forcing. In contrast to the general approach we use other climatic characteristic (the SHF) for orbital tuning. Such approach is physically more correct (Peixóto and Oort, 1984; Pielke, 2003; Douglass and Knox, 2012) – we wrote about this in the Introduction of the paper. Figure 1 (in the paper) shows that the insolation curve is much closer by its shape to the SHF curve than to temperature curve. After the orbital tuning the correlation coefficient between insolation and SHF on the interval from 35 kyr BP to 6 kyr BP equals to R=0.99. Therefore the unexplained variance $1-R^2 = 0.026$. And such coincidence can be achieved by changing only one parameter (thermal diffusivity) within the range of its natural variability. This fact as such gives the evidence about the validity of a hypothesis of SHF changes, induced by orbital factors. Such a good coincidence is not possible when tuning the temperature curve.

We will include this speculation in Section 4.

...the tuned heat flux record strongly disagree with the orbitally-modulated insolation over the last 5000 years. What is the explanation for this?

This time interval of 2000 years (5-3 kyr BP) is too short in comparison with the entire reconstruction to be able to present a convincing explanation of the SHF behavior.

(4) The manuscript also looks at the match between the reconstructed heat-flux and temperature records with the atmospheric concentration of carbon dioxide. The authors find that the CO_2 records resembles better the heat-flux record. Can we conclude that CO_2 does not affect surface temperature? What is the mechanism by which heat flux is affected by CO2 and surface temperature is not? Related to this, what is the uncertainty range in the heat-flux reconstruction that allowed to conclude that the CO_2 record matches better the heat-flux record? Could it be that all three records agree within their uncertainty bounds?

A short remark. "***The authors find that the CO_2 records resembles better the heat-flux record.***" To the contrary, we wrote that *"...a character and a chronology of CO_2 concentration changes are much closer to temperature changes rather than to heat flux variations*" (P. 3623, Lines 22-24 and Figure 3 in the paper).
The mechanism of interactions between CO_2, heat flux and temperature is presented in paper too (P. 3623, Line 17). Shortly, CO_2 may produce the additional heat forcing through the greenhouse effect. Its variations would affect heat flux changes and then temperature changes. But in our case heat flux increases earlier then CO_2. It can be assumed that increasing of CO_2 does not cause noticeable additional forcing.

We will introduce this speculation in Section 5.

...what is the uncertainty range in the heat-flux reconstruction…

After the procedure of orbital tuning the chronologies of temperature and heat flux changes are fully determined by insolation chronology, which is calculated with much higher accuracy than temporal differences in the SHF and CO_2 behavior.

Could it be that all three records agree within their uncertainty bounds?

No, it is impossible. The differences between the SHF and GST changes are determined by physical relationship presented in the paper. Heat flux changes always occur before temperature changes. In the case of harmonic oscillations temperature lag is the 1/8 of the period. We can see from Figure 3 (in the paper) that these differences are much larger than the uncertainties of CO_2 estimations.

In order to avoid interpretation of "orbital forcing" as the only possible cause of the surface heat flux variations, we suggest the following title change:
"**Late Pleistocene-Holocene ground surface heat flux changes reconstructed from borehole temperature data (the Urals, Russia)**"

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