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

Anonymous Referee #2

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The manuscript present an analysis of a very deep borehole temperature profile retrieved from the Ural region. The borehole temperature profiles measured today can be converted to a history of near-surface temperature or near-surface heat flux by applying different statistical methods that invert the heat diffusing equation . The problem is mathematical not well constrained and the usual methods so far used some sort of regularization. For instance, Beltrami put forward a method based on Singular Value Decomposition. 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. Later it is a applied to derive the surface heat flux history from the Urals borehole data. The main conclusion is that the surface heat flux over the last 35 thousand years has been driven by orbitally modulated insolation forcing and the influences of the changes of greenhouse gas concentrations between the glacial and interglacial states are negligible.

My impression of the manuscript, is, unfortunately, not positive and a publication in Climate of the Past would require very substantial revisions and extensions.

1) My first comment is related to the English, which is really poor. We non-native English speakers often wrestle with the writing, but a minimum standard has to be reached, if needed with the help of a colleague of a copy-editing service. This standard is unfortunately not reached here.

2) 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. There are now quite few simulations with climate models spanning several thousand years that provide a simulated near-surface or soil temperature and surface heat flux. Specially at the timescales considered in this manuscript, 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 a history of surface temperature and/or surface flux can be directly linked to the external climate forcing. To what extent does this new method provide different results than more traditional methods, which are the advantages, the drawbacks, the limitations ? 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 ?

3) 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 ? Additionally, the reconstructed heat flux record is then tuned to the insolation forcing by chaining the value of the diffusivity. Many questions arise from this tuning, which are not really addressed in the manuscript. Is the new value of the diffusivity still within reasonable bounds ? Could the diffusivity be not constant over time ? (I guess that the properties of soil changing as the the climate worms would also influence the diffusivity. 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. Even the tuned heat flux record strongly disagree with the orbitally-modulated insolation over the last 5000 years . What is the explanation for this ?

3) 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 CO2 records resembles better the heat-flux record. Can we conclude that CO2 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 CO2 record matches better the heat-flux record ? Could it be that all three records agree within their uncertainty bounds ?

In summary, the manuscript leaves too many questions open and requires a complete

copy-editing.

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