Reply to the reviewers, June 30, 2014

"Impact of the Last Glacial Cycle on Late-Holocene temperature and energy reconstructions from terrestrial borehole temperatures in North America"

Reply to Reviewers

We are grateful to both referees for their constructive and thoughtful reviews. Their suggestions and criticisms have improved our manuscript.

In addition to the changes suggested by the reviewers, we also corrected some typos, set the time units consistently to "a", and corrected some sentences that needed to be restructured. Major changes are marked in red in the revised manuscript.

Reviewer#1

In this paper, the authors assess the magnitude of thermal energy contributions from the Last Glacial period to the subsurface for regions that were characterized by significant ice coverage during that period. In the assessment, they couple the output from an ensemble ice sheet model to a subsurface temperature model. The basal temperatures from the ensemble model are used as a Dirichlet condition to the latter model. The authors find that subsurface disturbances to the semi-equilibrium geothermal gradient are relatively small at depths between the surface and 600m. However, in terms of quantity of heat differences between taking into account ice sheet variations on the Last Glacial period or not may differ up to 50%, which is of course a much more important factor to take into account. The paper is well written, to the point and covers an interesting subject worth publishing in Climate of the Past.
I only have a couple of minor comments to improve the manuscript. One of these is that I wonder why the boundary condition to the thermal subsurface model is taken as a Dirichlet boundary condition (basal temperatures), and not the basal temperature gradients stemming from the ice sheet model ensemble. Wouldn't the latter ensure continuity between both models? It would probably also be more consistent in its approach. The ice sheet model uses a Neumann condition at its base, based on a (supposed) value of the geothermal heat flow and extended with heat generated through basal sliding and it seems therefore logic that using the ice sheet model basal temperature structure as a perturbation to the subsurface thermal model continues from this condition. This aspect should be argued in the text/discussion.
There are good reasons for choosing a Dirichlet BC at the surface for our study. At the ice base, the temperature is continuous, but not the gradient, because of the phase change and energy sinks/sources at the boundary. These result from the melting/freezing phase change or advective flow. Heating from sliding and from ice deformation work are both source terms in the ice thermodynamics, as is standard for thermo-mechanically coupled ice-sheet models. Though it is possible to construct a Neumann BC at the ice base, we have much better control on the temperature, as the melting temperature of the ice is well known as a function of pressure, and thus ice thickness. In the water-covered times and areas, again the temperature is better constrained than the gradient, as the density maximum is well known for water of different salinity, and can be used to define the BC, though this may be a matter of discussion. For the shallow proglacial lakes (as opposed to oceanic transgressions), it is set to the melting temperature for the current study. For direct contact to the atmosphere, meteorological simulations use a complex parametrization of the boundary layer physics. For the long time periods involved here, however, it has been shown that the GST is related to the SAT following a simple relationship (see Smerdon et al. 2006a,b). The Dirichlet BC is further justified from a practical perspective because it is much better constrained and easier to implement. We have added a few sentences in the revised manuscript to address these points.

Another comment is that basal processes underneath (paleo) ice sheets are crucial in the determination of basal temperatures. These are especially regions of fast flow, sediment covered areas (hence saturated sediment deformation), subglacial lakes and basal hydrology (channels, cavities). These processes significantly alter the basal heat budget. How good are the ensemble model results with respect to these processes? How is basal hydrology represented in the ice sheet model? Were there large subglacial lakes underneath the Laurentide ice sheet (later on becoming proglacial lakes) influencing the heat budget and how are they taking into account?
There is currently no basal hydrology in the GSM used for this study, which employs a Shallow Ice Approximation and does account for fast ice flow due to subglacial sediment deformation. There is some evidence for the existence of sub-glacial lakes in the area of the Laurentide ice sheet (see Livingstone et al., 2013) however the totally area that had a > 50% chance of being covered by sub-glacial lakes in their analyses at any point over the last 32 kyrs amounted to <0.5% of the total area of LGM ice sheet. Futhermore, given all the other uncertainties, the lack of basal hydrology has probably limited relevance for the thermodynamics. On bedrock, it will have no impact except for some horizontal advection of heat (basal water will be associated with basal ice at the pressure melting point), which is likely negligible given the approximately 50-km grid cell scale. On sediment, one can likely assume that all subglacial sediment is saturated, as is done for the permafrost calculation in the GSM. Permafrost is represented via an effective heat capacity that includes a latent heat of fusion term following Osterkamp 1987 (see Tarasov et al., 2007).

P 2363 L 18: calving is also part of ice dynamics. Why putting this term P 2363 L 18: calving is also part of ice dynamics. Why putting this term separate?
In the glaciology community, ice (sheet) dynamics refers to flow of ice, and does not include calving.

Detailed remarks

P 2360: If the temperature profile is represented by a linear function, it should be added what the physical meaning is: this is the steady state temperature profile for vertical diffusion process only in which the slope depends on the basal heat gradient (geothermal heat, other processs?).
We have slightly reformulated the second part of this paragraph:

“This steady-state signal is approximated below a few hundred meters in BTPs by a linear function of increasing temperatures with depth, assuming that this signal represent the geothermal heat flow from below (see Section 2.2). The thermal effects of the LGC have been previously discussed as sources of bias in estimates of this background steady-state signal (e.g., Hotchkiss and Ingersoll, 1934; Jessop, 1971), but recent results have more quantitatively characterized the important impact of the LGC on BTP interpretations (Rath et al., 2012). Though known to be significant for a long time, LGC effects on heat flow density estimations, have only recently been used with appropriate care for general geothermal studies (e.g., Slagstad et al., 2009; Majorowicz and Wybraniec, 2011; Westaway and Younger, 2013).”

P 2361: development and demise? Aren’t there any simpler terms, such as buildup and melt, or waxing and waning. Demise is often used in a different context (more related to humans than to things).
The wording has been changed in the revised manuscript.

P 2362, L 6: is it "expand on the theoretical basis of BTP modeling and interpretation that are relevant to" or "... is relevant to" (depending what is meant. Not very clear phrasing.
The wording in the revised manuscript has been changed to:

"In the following section we briefly expand on the theoretical basics of BTP modeling and interpretation, as far as they are are relevant to our subsequent analysis."

P 2362: What is the permafrost resolving thermal model as part of the ice sheet model? How does this model cope with the temperature evolution in the subsurface? In what does it differ from the analytical subsurface model that is used later on? Are basal temperatures the only measure that is important for the subsurface model? Why are basal temperature gradients from the ice sheet model not used as a (Neumann) boundary condition? They seem to be available. See also P 2364.
Permafrost is represented via an effective heat capacity that includes a latent heat of fusion term following Osterkamp 1987 (see Tarasov et al., 2007). As already described in the submission, the model also includes a TTOP (temperature at the top of the permafrost) thermal offset for ice-free land to account for the thermal impacts of seasonal snow cover, vegetation, and the differing thermal conductivity of thawed and frozen land (based on Smith and Riseborough, 2002). For the question concerning the BC, see above.

P 2364: Diffusivity of rock is given. Is it only rock that matters? Are sediment layers not of importance (I can imagine that the heat budget of those, especially enriched with water) could be important.
In this study, "rock" is used for crystalline and sedimentary material. Our experimental set up did not take into account any geological differentiation vertically nor horizontally. In this respect it is even less developed than the subsurface module of the MUN-GSM, which includes the effects of a simplified geology (sediment vs crystalline), laterally varying heat flux, the TTOP thermal offset, and a simple permafrost model (see Tarasov et al., 2007). We agree that all of these should be included in a truly quantitative investigation, as variations in subsurface geological composition (not just sediment vs bedrock) has a significant impact on thermal diffusivity and permafrost related processes. Ideally we would have a 3D gridded digital map of subsurface porosity, heat capacity, and thermal conductivity, but none are available. Our study presents only a proof-of-concept, and the derived quantities should be seen as low order estimates. We have added a few sentences in the revised manuscript.

P 2367: Remove section 3 header. Make the two sub-sections separate sections.
Changed in the revised manuscript.

P 2374: top: something is missing here. Should be "since/from/at? 100 ka".
Corrected in the revised manuscript. The unit should not be "ka" but "a". Note that we also changed the unit consistently from "yr" to "a" when denoting a time (kyr for a time interval, "years" in the text)

Reviewer#2

I read this paper with great interest: it provides an analysis of borehole temperatures and thermal energy stored during the last 120000 years. A climatic ice sheet model designed for North America provides the ground surface temperature history for this analysis. Although I'm not a specialist, the topic of the paper seems suitable for publication in Climate of the Past. It is generally well-written, but section 3.2 could be expanded and clarified, as some of its statements are rather abrupt! In particular, the procedure that consists to add the geothermal gradient to the temperature anomalies obtained from a forward model and then remove a linear trend at the bottom of the temperature profile (line 8 page 2370) needs to be better explained and justified. I understand that it is supposed to reproduce the procedure used to invert the ground surface temperature history in the real-world, but I wonder why it is also applied to calculate the amount of energy instead of using directly the forward model results (figure 7). I guess that as the temperature anomaly associated with the LGC is mostly linear below 300m, it has been integrated in the geothermal gradient and therefore, I wonder what is the significance of the energy discussed in the paper?

The reviewer is correct as we are trying to follow the steps we normally use in real-word geothermal data analysis for the ground surface temperature and for estimating the continental energy. A comprehensive understanding of the energy distribution into the various climate subsystems is important for climate model simulations attempting to map the overall energy imbalance of the Earth, and to understand how the flow of energy impacts the behaviour of each climate subsystem. Records of past energy fluxes at the land surface from geothermal data are also important to help in the quantification of the rate of change of the energy content in the Earth's major climate subsystems (ocean, atmosphere, cryosphere, and continents) from the perspective of climate system dynamics (e.g. Hansen et al., 2005; Trenberth et al., 2009, Hansen et al, 2013). Some energy content estimates for the continents and other climate subsystems have been already incorporated into the global energy budget (Levitus, 2001; Beltrami et al., 2002, Levitus, 2005; Bindoff et al., 2007; Davin et al.,2007; Murphy et a;, 2009; Church et al., 2011; Levitus et al., 2012, Rhein et al., 2013). Thus, it is desirable that we consider the continental subsurface energy content when studying the influence of the LCG on the ground thermal state. An additional benefit of a good characterization of the thermal state of the ground subsurface may benefit other research areas dealing with near surface processes such as biogeochemical soil carbon stability models, permafrost stability dynamical models, as well as land surface modelling.

Detailed remarks

Page 2355 Title is ambiguous, as it can be interpreted in a way that the impact of LGC is derived from real data.

We suggest the following title change:

"A numerical investigation of the impact of the Last Glacial Cycle on Late-Holocene temperature and energy reconstructions from terrestrial borehole temperatures in North America"

Page 2365 line 6: Ts(z) "often called geothermal gradient" (????).

The concept of "geothermal gradient" can be ambiguous. Here, in a 1-D world, it is supposed to be of "geological" origin (i.e., originates from heat production and flow from below), and represents the steady-state background heat flow. The commonly observed gradient often contains other effects, as paleoclimate, anisotropy, or even advective flow. Additionally, as neither the temperature gradient nor the borehole are vertical in reality, it is not the full gradient, but an assumed quasi-vertical component of the gradient. These distinctions nevertheless are a digression in the context of the discussion in our paper and we reformulate the discussion as follows:

"...where Ts (z) represents the linear steady-state temperature profile depending only on thermal conductivity and basal heat flow density, while Tt (z,t) represents the perturbation by transient changes in the surface temperature boundary condition (Mareschal and Beltrami, 1992). The equilibrium BTP is written as..."

Pages 2386 and 2388: What is the difference between figure 3 and figure 5 (except the range)? It seems that conclusions of section 3-1 can be also drawn from figure 3.

We have included Figure 5 from frequent queries we have received from our collaborators. Although Figure 3a shows the mean heat flux perturbation for the interval between 30 m and 500 m which is the depth range of most of the borehole temperature profiles in the International Heat Commission Database for Borehole Climatology. We anticipate that most borehole climatologists would formulate the question regarding the details of the heat flux perturbation within that depth interval. This is because the recent warming period of the last 150 years is recorded in the upper 200 m or so, and the widespread existence of previous colder events sometimes associated with the Little Ice Age (found at some time between 1500 and 1800 CE), expected to be mainly recorded in the depth range of 400 to 600 m. Thus, we think that detailing some of the heat flux variation in parts of this interval would provide a response to our anticipated query. To reduce redundancy, however, we have moved figures 3 and 4 to a supplement to the article, keeping only Figure 5 in the main text. Thus, we think that detailing some of the heat flux variation in parts of this interval would provide a response to our anticipated query.

Page 2370, line 8: you should probably give the value of the linear trend removed from the synthetic T(z) compared to the 20 mK/m added to the temperature anomaly.
As the whole procedure is supposed to mimic the field procedure, it is an interesting suggestion to estimate the additional error from this approach. However, this would be a different study, as it becomes interesting, when you chose different levels of noise, estimating intervals, depths, or procedures, e.g., using the inverse code. The perturbation of the gradient from the synthetic 20K/km value, vary spatially as the deviation from the synthetic gradient is affected by the basal temperature history for the 120ka. Figure 3a shows the average perturbation of the heat flow (Δq0) for the range of 30 to 500 m. The average value of the gradient perturbation is given by (Δq0)/3, as we assumed a thermal conductivity of 3 W/K-m.

Page 2372, discussion and conclusions: say few words on the impact on heat flow estimates and lithospheric thermal regime in North America.
We added a sentence in the introduction of the revised manuscript:
"Though known to be significant for a long time, LGC effects on heat flow density estimations, have only recently been used with appropriate care for general geothermal studies (e.g., Slagstad et al., 2009; Majorowicz and Wybraniec, 2011; Westaway and Younger, 2013)."

Page 2374 equation 7: z^2 instead of z in exponential term?
This typo is corrected in the revised manuscript.

Page 2374 line 2: 100 years BP instead of 100 ka BP
This is corrected in the revised manuscript. The unit should not be "ka" but "a". Note that we also changed the unit consistently from "yr" to "a" when denoting a time (kyr for a time interval, "years" in the text)

Pages 2373-2374: discussion on the detection of a future 1K increase is not well related to the LGC
The passages concerning the future are not directly related to the LGC, however, this paragraph aims at encouraging more studies of energy content. We think this becomes clear in the following paragraphs.

Page 2375: In the beginning of the discussion, you mention that the effect of the LGC on the surface temperature reconstructions is not important, but conclude (top of page 2375) that it represents 60% of the energy and cannot be ignored! You should explain this paradox.
As mentioned in the text several times, the effect in the top few hundreds of meters is mainly manifest as a bias in the gradient. As with the manual determination of the gradient from the deep part of the borehole, standard inversion schemes for ground surface temperature changes will estimate the "equilibrium" or reference linear gradient including the effect of the LGC. Thus, the inversion for the ground surface temperature changes from the transient temperature term will only be slightly influenced. The linear part of the temperature signal, however, will not be the equilibrium one, but contains a much older transient signal. For more information, see Rath et al., 2012.