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Climate of the Past Discussions

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Interactive Comment

Interactive comment on “Borehole climatology: a discussion based on contributions from climate modeling” *by* J. F. González-Rouco et al.

J. F. González-Rouco et al.

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The authors would like to thank the reviewers for their constructive suggestions and the time they devoted in reading and proof-reading the manuscript. We have tried to integrate all suggestions and think that the manuscript has improved with them. We do appreciate their contribution.

The next sections contain a detailed point by point response to the reviewers comments. Most editorial (minor) comments have been implemented and are not discussed herein for the sake of shortness in this response.

Anonymous Referee 1

General comments:

REVIEWER'S COMMENT:
The present manuscript is an important overview about the potential use of borehole data to

reconstruct past temperatures. In particular, a thorough review about the interface of borehole and climate model data is given, which has been lacking as far as I know. The authors provide a very useful discussion on external forcing and problems with the zero flux condition. Likewise, the discussion of the relative roles of snow cover and soil water content will prove useful.

I recommend the manuscript for publication in CP given that the authors address the minor remarks below.

AUTHORS' COMMENT:
The authors welcome the positive perspective of the reviewer on the paper. We are grateful for the reviewer's comments.

REVIEWER'S COMMENT:
The language is too diverse. There are several paragraphs with inferior English compared to the remainder of the text, e.g. page 9-12, 36, 42. Please proof-read these (and other) sections. Some suggestions are given in the specific comments below. There is also an inconsistency in labelling model simulations in text and accompanying figures. The choice of the vertical axis is less than optimal in Figs. 1c, 11 and 12b.

AUTHORS' COMMENT:
The reviewer has identified a large series of errors and omissions that had been bypassed in the first version. The text has been proof read and acronyms and labels corrected. As for the figures, there was in fact some reason behind this arrangement of the vertical axis. In Fig. 1, the purpose was to highlight the long term coherence of the SAT and GST signals in the model, that is why emphasis was put on showing the time series together (as indicated in SC100) rather than on the optimal selection of the Y axis and achieving detail in the representation. Fig. 11 is shown for both panels with the same range for the temperature anomaly in both locations, the intention being to highlight the difference in the range of anomalies and variability between both locations. However this does make the plots not optimized to show the variability in Fig. 11b. As for Fig. 12c, the (-180,180) or (0,360) vertical scale is the standard way of plotting phase diagrams; however, it has been changed in the last version ot optimize the plot to the used (0,-180) range suggested by the reviewer.

We hope that the reviewer finds the changes satisfactory and the quality of the manuscript improved.

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REVIEWER'S COMMENT:
The authors claim (page 8) that borehole profiles cam deliver useful information about low frequency climate changes. Given the numerous difficulties (which all are nicely discussed in the text), it would be desirable to give an error estimate here.

AUTHORS' ANSWER:
See for the answer to this issue also comment 5 of reviewer 3.

The question of error bars is a complicated one and relates to the problem of uncertainty estimation. From the point of view of uncertainties the manuscript discussed a number of factors that may hamper the ability of borehole profiles to convey information about past climate. This has been treated in the manuscript in a qualitative and descriptive way or from the perspective of methodological testing in order to discriminate whether the impacts of a given factor (snow cover, depth distribution ...) can be important (see answer 7 to reviewer 3).

A more complicated question is how to translate all these uncertainties into error bars. In the present study we have preferred not to deal with this issue. As in the case of many other proxies, the development of fairly realistic uncertainty intervals that embrace the most important unknowns is a complicated issue that needs to be addressed in the future. The inversion methods used in this manuscript offer the possibility of including methodological uncertainty intervals. This is detailed in the literature dealing with such methodological issues. For the case of the inversion method considered herein some estimation of the uncertainty band related to noise is possible (see Beltrami et al., 1992; Beltrami and Bourlon, 2004). Since we are using simulated profiles without noise we decided not to include such uncertainty estimates here. We also think that including them would obscure the main messages around Fig. 6 and unnecessarily lengthen the section in order to incorporate an issue that, even if relevant, would relate to a pure methodological question than model-proxy interaction.

A brief comment in this direction has been included though, thus placing the results in Fig. 6 in the perspective of such methodological uncertainties.

REVIEWER'S COMMENT:
The conclusions presented here are based on the use of a single model. It would be very interesting to see how other models behave in this respect. In particular, it has been shown that ECHAM4, the atmospheric model used here, behaves quite differently when run in higher

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resolution than T30. Are there any higher resolution simulations?

AUTHORS' ANSWER:
Reviewer 3 raises a very similar question in point 2). Please, refer to the answer there for some discussion and indication of changes in the text.

Related to the resolution issue, there is indeed one 500 yr experiment, also indicated in the text, that uses the ECHAM4 model coupled to the OPYC ocean model (Stendel et al., 2006). The resolution of the ECHAM4 is in this experiment T42 and indeed the model shows less amplitude of changes in comparison to the ECHO-G along the last 5 centuries of the millennium. It is somewhat tricky to argue about this, and discuss the differences from the perspective of a different resolution. On one hand the forcings used in the ECHAM4-OPYC experiment and those in the ECHO-G are different. Also, the ocean component is different and this may well have an impact in low frequency resolution. In addition, there is also an ECHO-G model simulation shown for comparison in Stendel et al. (2006). This run is the one described in (Zorita et al., 2004) and (Zorita and González-Rouco, 2002), i. e., the first 500 yr simulation made with the ECHO-G. This simulation shows larger multi decadal and centennial variability than the FOR1,2 simulations shown in this manuscript. The model code is a f77 version while a newer f90 version was used later in FOR1,2. Therefore, it is hard to ascertain the origin of the differences.

The impact of resolution on these type of simulations is an issue that certainly needs attention. We would say that particularly with respect to changes in large scale dynamics this may be of importance, and therefore specifically their impacts on the modulation of regional climate.

SPECIFIC COMMENTS:

Page 35, line 7-11:
Even though the authors present an overwhelmingly extensive list of references, they are actually missing one. In the approach by Stendel et al. (2007), boundary conditions are provided by a GCM to an RCM, and the RCM output is used to drive a sophisticated permafrost model on the same grid as the RCM. The authors demonstrated that this approach is superior to using only GCM data.

Answer: This was a very good suggestion. The reference has been briefly commented now in Sec. 4.2 and 5, both in the context of GST validation and scenario simulation

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exercises.

Referee 2

V. Cermák

REVIEWER'S COMMENT:
Generally, this is a good contribution and I want to recommend it for publication in the Climate of the Past Discussions. As a coauthor (with Louise Bodri) of the recently published book on a similar topic, namely Borehole Climatology (by Elsevier, 2007), I welcome this contribution. The work suitably extended the problem how to apply a relatively new powerful method to reconstruct the past climate changes by inverting the observed temperature versus depth profiles measured in the shallow subsurface. The authors concerned their effort to describe the interaction between models and results of borehole loggings, they focus on explaining how models can be used as a validation tool for paleoclimate reconstructions and compared practical borehole information and model simulations. They go even further and discuss the potential realism of estimating the future climate changes by simulating subsurface climate.

The author team presents four experienced authors, each of them proved the ability to understand the problem by contributing to various problems of borehole climatology. They compiled a huge material which they arranged into systematic flow of information. The unquestionable valuable input of the work is the extensive list of references and existing data sources. Another most valuable input is the discussion of the climate modeling, its advances at the interface of climate reconstructions and General Circulation Model simulation. The discussions on using the external forcing in climate modeling and the role the snow cover and soil content effect on the global scale belong to the most interesting part of the paper.

AUTHORS' COMMENT:
We appreciate the postive perspective on the manuscript and are, in fact, honored by the comments of Dr. Cermák. We are thankful for the comments and suggestions that follow.

REVIEWER'S COMMENT:
There are several, rather trifle comments (technical) For labeling of models sometimes is used CON, CTRL, and Control, similarly FOR 1 x For 1, see text, figures and figure captions Ad Figure 5, I guess the panels C and E are to be exchanged with panels D and F ? Several figures could be improved; for better illustration by suitably adapt the size of the vertical axis

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(see graphs 6, 11, 12)
Some general comments (aimed for certain discussion and considering the future work)

AUTHORS' COMMENT:
Thank you for the corrections. We have revised the text changing these inconsistencies and trying to improve the figures.

REVIEWER'S COMMENT:
Due to the downward smoothing of the climate signal from the surface, any effect of climate events older than 500 years is difficult to recognize; majority of visited holes are less than 400 or 500 m deep. Therefore any reliable detection of the Little Climate Optimum/Medieval Warmer Period is problematic, especially for cutoff values of 0.15 and higher (see e.g. Figure 6). The limited depth range of borehole data together with the nature of inversion techniques can hardly contribute to substantial progress. For the future studies it may be recommended to select a borehole data set of holes of minimum 500 m depth, drilled in consolidates (hard rock) basement with no or little potential disturbances (such as long-term vegetation changes, soil moist, ...) and individually approach each borehole log and location features. As the likely area for such study is North-East America (Canadian Shield). Inversion procedure can be completed with other statistical methods (Monte Carlo evaluation?).

AUTHORS' COMMENT:
We agree with this comment. The present manuscript also suggests that noise in boreholes can have an important impact complicating the identification of the MWP to LIA transition. Some nice exceptions exist though (see Demezhko and Golovanova, 2007, in this issue) in which the MWP to LIA is evident.

The last part of the comment, as well as the one following below are well received and very telling of the reviewer's vast experience.

REVIEWER'S COMMENT:
For a more detailed insight, the assessment of the massive deforestation on the ground temperature history can help. In Europe the deforestation took its climax much earlier than in America, in America itself there may be some east to west trend. The Prairies may be a good area to test. There is a certain remarkable difference in the shallow temperature-vs-depth profiles on both continents.

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AUTHORS' COMMENT:
The deforestation issue and its representation in GCM is commented in Sec. 3 but the text does not get into these details. These comments are welcomed and kept in mind for future work.

REVIEWER'S COMMENT:
The major communication of the present work is definitely formulated in the first three chapters. The Fargo and Cape Hatteras story is focused on a rather specific problem (geographically as well as substantially). If the paper is subdivided in two parts, the first part can be completed with several practical cases showing the effect of snow cover or soil moisture effects in detail and better illustrate the theoretical consideration.

AUTHORS' COMMENT:
We understand the reviewers perspective and appreciate the value given to the first part of the manuscript. We still regard the Fargo and Cape Hatteras story could be useful one, in spite of the local character in its presentation. The reason for this is that we think that it illustrates in a very simple way the behavior of the model in its representation of reality for these two different cases of soil regime. This simple example helps to describe the heat conduction regime around Fig 9 and 10 as in similar examples in the literature dealing with the annual cycle in observations (e.g. Smerdon et al., 2004). Fig. 12 offers a somewhat new perspective that illustrates the damping and phase shift of all the frequency range in the spectrum rather than focusing just in the annual cycle and it depicts also the perturbation produced by the zero flux bottom boundary condition placement (BBCP) with respect to the pure heat conduction regime. We think this section provides a somewhat different perspective on the BBCP that complements that of the BBCP impacts on heat accumulation described in Section 5. We hope that, after the changes done to the text this is more clear now.

Reviewer 1 valued positively this part of the text. Reviewer 3 recommended some shortening of Section 4. Some slight reduction in the section has been done, but the Fargo and Cape Hatteras story still keeps its lay out. We hope we have been able to convey the motivations for this.

Anonymous Referee 3

General comments:

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REVIEWER'S COMMENT:
This is a very important and thorough review on the potential and applications of boreholes in paleoclimtology. Such a contribution, to my knowledge, is lacking in the current literature and matches very well in CP and will be a very relevant publication for future applications in the field, research opportunity, etc.. The review also describes very nicely the combination of borehole climatology and paleoclimate modeling and the important contribution to understand the low frequency climate evolution covering the past centuries. It also shows also results which point to the potential for simulating the adequate energy balance within climate change scenario experiments. The paper is well written, the different parts are logically developed. I have just a few minor comments which I think the authors can address easily. I first start with some particular questions, followed by a few minor comments on parts of the manuscript. I look forward seeing this publication published soon within the special issue of CP Interpreting subsurface temperature signals of climate change.

AUTHORS' COMMENT:
The authors appreciate the positive perspective of the reviewer. We thank the reviewer for time and effort devoted to review this manuscript. We have considered all comments/suggestions of the reviewer and made changes in the text to accommodate most of them. We hope the reviewer finds them satisfactory.

Specific comments:

1) *Improved understanding and knowledge of long-term natural climate variability and large-scale climate changes on different spatio-temporal timescales is of great importance to place recent anomalous, climate change in a longer-term context. To what extent can boreholes contribute to the current discussion on the spatial extend,intensity, trends within the so-called Medieval Warm Period/ Medieval Climatic Anomaly and the turn into the Little Ice Age?*

   Answer: The present issue in CP exemplifies a nice case Demezhko and Golovanova (2007) in which the MWP to LIA is evident. Also see Safanda et al. (2007)

   Having said this, we should state that in general the signal of the MWP and its transition to the LIA is a limitation in most boreholes of the database. The time span covered by a given borehole temperature log depends on the length of the profile. For example some borehoes are deep enough that the signal from the ice retreat is present at 2000 m. Indeed some of these boreholes used for classical heat flow studies were often cleansed of this ’unwanted’ signal by forward modeling an ice-melt event some thousand of years ago. However, although a large signal could potentially be recorded in a detectable fashion, its resolution would decrease rapidly at depth and only long term trends would be preserved. Additionally, the resolution of the borehole method to climate reconstruction worsens with noise Beltrami and Mareschal (1995) as illustrated in the present manuscript (Fig. 6). This resolution also decreases in time such that for an event to be detected, as a single event, it would have (at best) to have lasted 60% of the time since its occurrence. Therefore, for an event to be detected about 500 yrs before present, it would have to have lasted for 300 years. So caution would have to be exercised when interpreting signals such as those from the MWP and the LIA. The signals of the LIA have been detected at several locations already and they seem well established, although the occurrence of the LIA is not spatially homogeneous as the work of Mareschal has shown in Canada. The MWP has been shown to be potentially detectable in model simulations González-Rouco et al. (2006). We do not think that there is a definite evidence of a clear signature of the MWP in the borehole dataset, although it could be retrievable in principle.

As indicated also in the 3rd comment of reviewer 2, and suggested by the results of the present manuscript, the noise in the profiles can complicate the identification of such signals.

2) *The authors use ECHO-g for the model/borehole comparison. Can the authors state anything of other models such as HadCM3 or CCSM that also cover the last 500 years? It would be nice if the authors could incorporate some kind of comparison or discussion on the use of the other models as well. Do they show similar/different features?*

Answer: As for the temporal evolution of the temperature response described in Figure 1, the text indicates (Section 2.1, 3 paragraphs before the end) that the FOR2 simulation compares well with the other existing millennial simulation with a high complexity GCM (Zorita et al., 2007). Additionally, it would certainly be interesting to incorporate other simulations in the methodological and in the comparison assessment between borehole information and model output. This is particularly so in some cases that incorporate interesting types of the external forcing, for example concerning land use changes

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(Tett et al., 2007) as indicated now in Section 3. The necessary data of these simulations were not available during the course of this study, but it will be certainly interesting to analyze them in the future.

3) *The authors show the example of America (which is also discussed in more detail in a paper by Stevens et al. 2008, JGR. To what extent are the results applicable/ comparable/relevant to other continents, say Europe?*

Answer: The manuscript focused on North America in Section 3 as a case example to illustrate some methodological features of the borehole approach to climate reconstruction, given the fact that the population of borehole logs offers a good coverage over the area as well as good distribution of depths and logging dates. This made of the selected area a good framework for discussion of available literature and consideration of the effects of several factors in our exercise. The results of the tests are not expected to be dependent on the area selection.

As for the borehole/model comparison described in Section 4, it will be certainly interesting to extend this studies to other regions like Europe. This assessment is currently under way.

4) *Relate to the point above, to what extent can boreholes tell us anything about southern hemisphere, tropical climate change?*

Answer: Hemispherical reconstructions have been the target of some studies. This is the case for instance in Huang et al. (2000), where global and hemispherical scale reconstructions from borehole information have been provided. Therefore, the reader has here and in other provided publications specific information also about the SH. Concerning the latitudinal distribution, most of the boreholes in the SH are located north of 30 S (see Pollack and Smerdon, 2004) so this potential exists, and in fact has been exploited for some subregions (Pollack et al., 2006; Hamza et al., 2007). Some listing of citations to aid the reader in finding information about several examples of reconstructions at site and regional scale is provided at the beginning of Sec. 1.2 (2nd paragraph)

5) *I was wondering about the uncertainties in the borehole data and the derived temperature estimate. Can the authors go a bit in more detail on this issue?*

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Answer: This can be interpreted at least from two different perspectives. One is that of the limitations and related uncertainties that the borehole dataset is subjected to. This is further commented in point 7). The other one is how to translate all these uncertainties into error bars. In the present study we have preferred not to enter this still open issue. As in the case of many other proxies, the development of fairly realistic uncertainty intervals that embrace the most important unknowns is a complicated issue that needs to be addressed in the future. The inversion methods used in this manuscript offer the possibility of including methodological uncertainty intervals. This is detailed in the literature dealing with such methodological issues. For the case of the inversion method considered herein some estimation of the uncertainty band related to noise is possible (see Beltrami et al., 1992; Beltrami and Bourlon, 2004). Since we are using simulated profiles without noise we decided not to include such uncertainty estimates here. We also think that including them would obscure the main messages around Fig. 6 and unnecessarily lengthen the section in order to incorporate an issue that, even if relevant, relates more to a pure methodological thread than rather model-proxy interaction.

A brief comment in this direction has been included placing the results in Fig. 6 in the perspective of such methodological uncertainties.

6) *Is there a way to get cold/and or warm seasons climate information out from boreholes?*

Answer: Regrettably not. As described in the text, the filtering nature of heat conduction damps the high frequency variability making it impossible to recover seasonal climate evolutions as other proxy records offer. Typically, borehole records start to be taken at a few meters below the surface downwards, the reason for this being that temperature measurements are taken from the beginning of the water table in the borehole. The annual cycle signal reaches in typical crustal rocks a depth of the order of 10 to 20 m (e.g. Smerdon et al., 2004). This means that at the depths where the borehole log starts, there is barely any influence of the annual cycle.

From a different perspective, the fact that seasonal information cannot be recovered does not mean that seasonal changes cannot have an impact on borehole records. We can consider, for instance, a location in which the presence of the winter season snow cover has changed systematically with time, either diminishing or increasing. This would produce a decoupling on the long term between SAT and GST leading to a progressive

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isolation of the ground in winter. This effect can have an imprint on the long term trends recovered from boreholes, such that a warming trend could be interpreted instead of a progressive isolation and apparent warming of the the ground, relative to surface air temperature, in winter.

7) *I think it would be of importance to detail also the limitations of boreholes in our understanding of paleoclimate variability, etc. The manuscript mostly addresses the potential of this proxy.*

Answer: Some changes have been made on this direction. The text states for instance in section 1.2 the limitations of the borehole climatology to recover low frequency variability (1st para.). A number of problems and related literature are enumerated in Sec. 1.2:
"The geothermal approach, as any other method of inferring past climate, is not free from unknowns and limitations such as..."
In addition: Section 3 states that even if the changes in snow cover have not been found to be critical within the model simulations, this could have an effect in the more complex real world, and particularly at the local/regional scale; Section 3.2 describes te potential of the model simulations to explore questions related to the borehole method of climate reconstruction and indicates that assumptions are made with respect to other critical aspects of the method like the discrimination of the geothermal gradient in shallow boreholes or the existence of noise. Also, uncertainties related to the potential impacts of glacial-interglacial changes are briefly mentioned in the text and in the conclusions.

16) *page 28, line 21, can you say anything for the pre 1700 period? See also fig 7, is there a possibility to show also a trend over the full period or for the 1500 to 1700 period?*

Answer: Figure 7 shows trends since approximately the minimum of the LIA. The purpose of this is to qualitatively compare the spatial pattern of trends in both simulations, both to report on their mutual consistency and mainly to compare with results of other papers addressing the behavior of long term trends in North American boreholes. We think that showing trends for the pre-1700 period, though interesting, would somewhat deviate the focus of the section. Other publications can be informative for such purposes (e.g. Zorita et al., 2005). Having this in mind and the view of not enlarging the section too much, we avoided to include further plots.

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