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Climate of the Past Discussions

Interactive comment on "Impact of maximum borehole depths on inverted temperature histories in borehole paleoclimatology" *by* H. Beltrami et al.

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Response to Reviewers

Impact of maximum borehole depths on inverted temperature histories in borehole paleoclimatology, by Beltrami et al., Clim. Past Discuss., 7, C424-C425, 2011

Reviewer # 1

We are grateful for the time that Dr. Cermak has taken to read and review our paper. We are very pleased that he approves of the manuscript in its present state and that he recognizes the importance of the work that we present. His one requested improvement was to provide a brief discussion of, and citations for, processes that may decouple the surface air temperature and downward propagating ground surface

temperature signals. We previously had included many of these references, but had not specifically noted the processes that are important. These are now more explicitly included at lines 71-79 in the revised manuscript.

Reviewer # 2

We appreciate the time Reviewer #2 has dedicated to our paper and his/her positive comments on the manuscript serving as a good source of information for newcomers to the field of borehole paleoclimatology. A specific criticism of Reviewer 2 is the length of the Introduction and the list of references that it provides. In the revised version of the manuscript, we have attempted to shorten the introductory material. We nevertheless have tried to balance the reduction of material suggested by Reviewer 2 with the comments of Reviewer 1, who has applauded the scope of our introductory material for the context and background that it provides. We therefore believe that we have reached an adequate balance between these two disparate points of view.

Finally, Reviewer 2 also expresses the opinion that our paper is not worthy of a full research article and is in effect "just a calculation." As outlined below, we disagree with this assessment for multiple reasons.

Our paper presents multiple numerical experiments demonstrating the importance of employing boreholes 500 m or deeper for borehole climate reconstructions targeting the last 500-1000 years. While there are some important caveats regarding the impact of maximum borehole depths on geothermal reconstructions (which we list in our manuscript), the consequences that we outline do not define an axiom that simply states "the deeper the better." Much to the contrary, the message of our manuscript is that if boreholes are not deep enough, the resulting reconstructions will simply be wrong. This is a vital message that is given quantitative scope in our realistic synthetic experiments, and those using observational borehole data. Nebulous distinctions between research papers and research notes notwithstanding, we believe this demonstration is worthy of the full and comprehensive vetting that we provide, as well as the

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necessary context that is established through our background and literature discussions.

As we have stated in our manuscript, there also are numerous examples in the borehole paleoclimatology literature in which the maximum depth of employed boreholes has not been properly vetted. In this sense, the literature provides examples in which researchers do no appear aware of the fact that certain shallow boreholes may not be appropriate for their performed analyses. This simple fact contradicts the assessment of Reviewer 2 and implies that the message of our manuscript is not widely appreciated by the community. Furthermore, our conclusions suggest the impact of the identified depth sensitivity on global borehole analyses is not obvious and should be quantified in future investigations. The later statement alone makes this work very important because of the implications it has for borehole paleoclimatology specifically, and for the wider efforts to quantify temperature variability during the late Holocene.

In summary, our manuscript is both a rigorous investigation of a previously unquantified effect, which the literature suggests is a point not widely appreciated. We therefore believe that the message is not only important enough for a full research article, but also significant enough to require a full discussion of our results, their caveats, and the research (literature) context into which our study emerges.

Review from Dr. Jacek Majorowicz

We thank Dr. Majorowicz for his interest in our paper. While he agrees with the conclusions of our study, he is unsatisfied with the degree to which we address his previous paper:

Majorowicz, et al., East to west retardation in the onset of the recent warming across Canada inferred from inversions of temperature logs, J. Geophys. Res., 107, 2227, 12 pp., doi:10.1029/2001JB000519, 2002 (hereinafter MSS2002).

This paper was cited in our original manuscript, but Dr. Majorowicz points out that it

contains experiments similar to our own and that we have not adequately addressed his previous work. He further suggests that our experiments are in fact not new, and effectively performed in his earlier published paper. We address each of these criticisms below. Regarding a more direct discussion of MSS2002 and the maximum depth experiments that they performed, we agree that it was an oversight to not directly address them in our manuscript. We thank Dr. Majorowicz for bringing this to our attention. We now discuss the maximum borehole depth experiments from MSS2002 in the Theoretical Framework section of our revised manuscript (lines 131-141), where we previously reviewed several other studies that have considered the maximum borehole depth.

Despite the commendable effort by MSS2002 to address the borehole-depth issue, we nevertheless respectfully disagree with Dr. Majorowicz that these previous experiments were sufficient for their intended purpose or that our more recent efforts do no represent a new contribution to an assessment of the problem. It is most important to point out that the MSS2002 study used a surface temperature function that was much too short to observe the effect that we quantify in our manuscript. In both MSS2002 and our present study, a specific surface temperature history is driven into the subsurface as the basis for the evaluated synthetic experiments. The resulting subsurface temperature profile is inverted with different maximum depths and the impacts are evaluated. Our experiments employed a 1000-yr temperature history, which represents the full time-scale of interest in most borehole paleoclimatology applications. As we have shown in our manuscript, such a signal can penetrate to significant depths and perturb estimates of the background steady-state signal, which in turn can bias geothermal inversions of surface temperature histories. By contrast, MSS2002 employed a single temperature ramp from 1850 to 2000 CE. Given the realistic subsurface thermophysical properties applied by MSS2002, this 150-yr forcing function simply cannot penetrate below depths of about 150 m – the upshot being that the penetrating signal cannot have any impact on the temperature log below 150 m and therefore no impact on inversions that use a borehole deeper than about 200 m. The conclusions of MSS2002 are therefore inapplicable for any location other than a one at which climate changes

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have only occurred in the last 150 years, prior to which surface temperatures were effectively static. While this may describe certain very special cases, locations with well-established Little Ice Age or Medieval Climate Anomaly periods are well documented and are more likely the norm. The potential impact of these periods on the borehole profile is therefore extremely relevant and necessary to evaluate, as we have done in our manuscript. We also note that the sensitivity of the maximum borehole depth is also demonstrated using observed borehole temperatures, and thus goes beyond the use of synthetic data to characterize the problem that we illustrate. Both of these latter points do not apply to the MSS2002 analysis.

Finally, it is worth noting that the effect that we describe is the result of a downwelling surface temperature signal that impairs the robust identification of the steady-state background signal. To observe this effect, we take the step of adding a realistic background steady-state signal to our synthetic borehole profiles, which then must be removed in the inversion scheme that we apply. Failure to add a background steady-state signal will prevent the observation of the effect that we identify. It is our impression that the addition of a background steady-state signal was not explored by MSS2002, although the authors simply do not discuss this element of the experiment.

In summary, we have added a discussion of the MSS2002 paper to our revised manuscript reflecting the comments that we make above (lines 131- 141). While we agree with Dr. Majorowicz that his previous results should have been better highlighted, we disagree that they are merely earlier versions of our own. To the contrary, the design of his experiments renders them ineffective for observing the effect that we identify. Furthermore, they are not an appropriate assessment of the impacts of maximum borehole depths in eastern or western Canada, or elsewhere.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/7/C722/2011/cpd-7-C722-2011-supplement.pdf

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