

Interactive comment on “Mapping uncertainties through the POM-SAT model of climate reconstruction from borehole data” by M. G. Bartlett

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It was gratifying to have each of the anonymous referees recognize the value of a sensitivity study of the POM-SAT method. While both clearly understood the general results of the work (i.e. the importance of the magnification of the uncertainty in the reducing parameter into the reconstructed pre-observational mean), both authors took issue with some aspects of what was, and what was not, presented. I would like to consider their major and minor criticisms in order.

Anonymous Referee Number 1:

Major Comment #1 – The construction of the synthetic borehole temperature profile.
C3022

The referee's concern really has two parts: (1) that, though discussed in detail in the methods section, I did not “show” the construction methodology in the paper, and (2) s/he is concerned that in constructing the synthetic borehole profiles for analysis of the propagation of errors, I diffused a transient SAT record with a pre-observational mean temperature into the ground – a “best case” scenario for the method under consideration. The methodology of how the synthetic borehole temperature profiles are constructed is discussed in the original paper on page 2506, lines 19-23. This section could be expanded, repeating the presentation Harris and Chapman (2001) and/or Harris (2007). Since these authors' works were referenced in the preceding paragraph, I felt that the summary explanation was sufficient to guide the reader in understanding what was being done in the synthetic construction. Certainly, more details can be supplied on the construction methodology, including the step-wise solution of the diffusion equation employed. The referee's second criticism is more problematic since it fails to recognize that the “best case” scenario (i.e. a synthetic borehole temperature profile drawn from the solution space of the POM-SAT method) is exactly what is desired when one wishes to consider the propagation of errors in the a priori parameters of the model into the solution. In this paper, I am less concerned with the ability of the method to accurately predict the pre-observational mean of “real world” climate scenarios (such as the MCA-LIA suggested by the referee) than I am in the internal expansion or contraction of uncertainty the method produces. The referee would like an answer to the question, “Does the method capture the ‘true’ pre-observational mean? Is it accurate?” To the extent that this question can be answered, it has been dealt with in the literature cited in the introduction (a statement to this effect should, perhaps, be added there). What I am asking in this paper is not “How accurate is the POM produced,” but, rather, “How large should the error bar on the produced POM be?” To answer this question requires us to examine perturbations of the model about synthetic profiles where we have prior knowledge of exactly which values in the solution space of the model should be produced.

Major Comment #2 – The nature of the long-term coupling between the ground and
C3023

the atmosphere. The referee correctly points out that one of the major issues with the use of borehole temperature profiles for the reconstruction of past climatic conditions is the nature and extent of the coupling between ground and air temperatures over multiple time-scales. This coupling has been, and continues to be, an area of active research. If ground surface and air temperatures are tightly coupled to one another (even if the coupling involves time-independent offsets), the reconstruction of the POM can be interpreted as containing information on the pre-observational mean air temperature. If the nature of the coupling is time-dependent and has long-term trends, then no useful information on the pre-observational mean air temperature can be derived from the borehole. While these are important points to keep in mind when working with the POM-SAT method, they are independent of the central question of the paper: How does uncertainty in the internal, a prior assumptions of the method translate into uncertainty in the solution? This point can, and probably should be highlighted in the introduction to the work in order to more clearly define what the paper is trying to accomplish (and what it is not).

Major Comment #3 – The reviewer would like to see more discussion at understanding how the depth dependence of the reduction parameter estimation influences the final solution. My original thought was to include the depth-cut off of the reduction parameter as part of the paper; however, as the reviewer notes, the relationship is highly nuanced and subtle – its full development is likely worthy of a paper all its own. In the end, I felt that the analysis of the three parameters discussed (thermal diffusivity, SAT, and reduction parameter) provided sufficient sustenance for the current paper. In the end, I chose to stick with a common cutoff at 160 m (consistent with all of the previous hemispheric reconstructions that have been done using the methodology) and leave the issue of the depth-reduction parameter uncertainty unaddressed. This point can be made in the introduction and in at the end of the paper where future work is discussed.

Minor comments:

1. Only one instance of SGT occurs in the paper (page 2505, line 26). This can be C3024

changed to GST (which is more commonly used these days).

2. I would propose that “past climate estimate” be used, since an estimate is, in effect, what the methodology reconstructs.

3. I agree.

4. The term “climate field” was borrowed from the literature on the mathematics of inverse problems, in which the solution (a unique realization of a set of parameters from a parameter space) is often referred to as a “field”. I agree that the wording is not well chosen for the audience of the paper, considering the use of the word in the climate community as a specific spatial 2-D reconstruction of a climatic parameter. I think the term “climatic parameter estimate” could be substitute throughout for greater clarity.

5. The reviewer’s suggestion that the distribution of global borehole sites does not look significantly different than those for proxies is incorrect. Boreholes do not suffer from the elevation bias of many proxies and (though still showing some clustering effects) are significantly more evenly distributed than the major terrestrial proxies. Perhaps more importantly, they are located at different locations than the major terrestrial proxies.

6. This same comment could be applied to any of the proxy methods used to reconstruct past climate in which the physics of the temperature imprint on the proxy are poorly constrained. In fact, it could be argued that the boreholes are the best understood in terms of how air temperatures translate into the “proxy” (in this case, ground temperatures). In this sense, the borehole is not significantly different than any other climate proxy. Its central disadvantage compared to other reconstruction methods is the diffusion of the signal with depth.

7. See my comment to minor comment #4, above.

8. The reviewer is correct in his assessment of the interpretation of the POM. However,

it is also true that in the inversion process, what is being sought is the value of a step change in temperature prior to the onset of the SAT record. Functionally, both interpretations amount to the same thing. Perhaps this is worthy of a comment in this location.

9. The reviewer's point is well taken. See comment 4, above.

10. Again, comment 4, above.

11. A reference here can be added.

12. The 10% value was derived from looking at the de-trended inter-annual variability of the CRU gridded product. Discussion of this is warranted. It might also improve the paper if multiple estimates of the uncertainty value were propagated through the model. This can certainly be done.

13. I am not certain that I understand the reviewer's comment here. Estimates of the reducing parameter are based entirely on the data at a given location. The "variability of the geothermal heat flow spatially" suggests either a non-linear geotherm (boreholes used in reconstructions are pre-filtered for these problems, which are unlikely anyways over a few hundred meters) or the lateral transport of heat (again, boreholes suffering from these effects are inappropriate for use in reconstruction).

14. The reviewer's point is well taken, and the figure (and discussion in the paper) can be adjusted to reflect how the distribution of POM's centers on the value imposed in the synthetic construction. The model is, in effect, linear in this respect – a symmetric distribution of the a priori parameter produces a symmetric distribution about the "true" POM value. The distribution of ROM misfits for the results can also be shown. They do, indeed, conform to the range of 10-20 mK for most values of the parameters – the method will find a good fit, even with poor parameter estimates.

15. These can be shown as well. I just thought a bunch of Gaussian curves go a bit boring!

C3026

16. Correct.

17. The reviewer's point is well taken. It is true that there is a "sweet spot" for borehole reconstructions in which the depth of borehole and the length of the reconstruction give meaningful information. Too deep (and long) and all climate information is essentially diffused below the noise level of the hole. Too shallow (and short) and there is no point in using the borehole (since SAT records are likely available).

18. The "muting" discussed is an artefact of the fact that the POM estimate includes a more recent period of time. Trends should remain the same.

19. This section can be reworded. The idea is that averaging borehole reconstructions together (as was done by Harris and Chapman [2001]) to arrive at a valid hemispheric average is preconditioned on the assumption that residuals in the profiles after application of reduction parameters represent Gaussian noise and are uncorrelated between holes. If this assumption is incorrect, then the hemispheric average will retain a greater degree of error than it would if the boreholes were not spatially correlated.

20. The statement is not meant to imply that Beltrami et al. suggested that more degrees of freedom would eliminate this problem. However, my work does show that this problem persists even in the most limited (in terms of DOF) borehole reconstruction method. Correctly arriving at an estimate for the reducing parameter is an inherent problem of the nature of the data, not the method used.

21. Will update to match reviewer's comment.

22. Will update to match reviewer's comment.

23. Will update to match reviewer's comment. Č

Anonymous Referee #2:

Major Comments: More details of the "construction method of the temperature model used," and "the main shortcoming of the manuscript is related with the synthetic tem-

C3027

perature log used to perform the study." To some extent, I think my comments to the major comment #1 of the 1st reviewer addresses the issue the 2nd reviewer raised. The logic behind constructing the synthetic temperature logs in the way outlined flows fundamentally from the type of work being done in the paper, i.e. and internal assessment of the propagation of errors in the POM-SAT method. The reviewer also has concerns about why the 160 m cut-off is used. This point was addressed in my response to major comment #3 of the 1st reviewer. The reviewer also writes, "The range of the surface air temperature time series implies different depths of penetration of the climatic signal into the ground. Therefore, the construction of the surface air temperature time series is also important and should be detailed and shown." I am not certain that I understand the comment; if the reviewer is suggesting that different lengths of the SAT time series are important, this is certainly true. However, the model simply returns an estimate for the pre-observational mean temperature for any SAT profile, where "pre-observational" simply means the time before the onset of your SAT time series. The reviewer also notes: "Finally, and, to a certain extent, related with the previous comment is the fact that the frequency content of surface air temperature time series depends on the time size of the series. And, therefore, the penetration of the temperature wave will depend on the size of the air temperature time series." By "size of the air temperature time series", I infer the reviewer to be making reference to the overall slope of the time series (or, equivalently, the dominant period). Interpreted the way, the statement is true. This is part of the reason Harris and Chapman [2001] and others have chosen the 160 m depth cutoff – for any reasonable estimate of recent SAT trends, it ensures that the lower portion of the borehole is responding to "pre-observational" conditions.

The reviewer also caught the use of SGT in the paper; this can readily be corrected to GST for consistency. S/He also points out the inconsistent use of the word "field", as did the 1st reviewer. See my comments in response to reviewer 1, above.

The reviewer also caught two grammatical/spelling errors that somehow slipped through. I am indebted to such a careful reading.

C3028

Two of the reviewer's final suggestions (#1 and #3) echo points made by reviewer #1 and are addressed above. The suggestion to give more detail on the SAT time series can be readily incorporated and was partially touched on by reviewer 1, above.

Interactive comment on Clim. Past Discuss., 8, 2503, 2012.

C3029