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Interactive Comment

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

Anonymous Referee #1

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Summary: This paper presents a sensitivity analysis of one widely used method of borehole climate reconstruction, the pre-observational mean or POM method. Using a synthetically constructed borehole temperature profile, the author tests the sensitivity of the POM method to the choice of thermal diffusivity, uncertainties in the surface air temperature (SAT), and the reducing parameter, i.e. the estimate of the background geothermal gradient. The author finds the method relatively insensitive to the thermal diffusivity and SAT uncertainties, but points out that the sensitivity to the reducing parameter is very large. He concludes by suggesting that dependence on the reducing parameter in the global borehole database may be quite large, and further investigations into the role of the reducing parameter uncertainties in borehole climate reconstructions are required.

General Remarks: Investigations into the robustness of geothermal climate reconstruction techniques are important. The POM-SAT method has been widely used and it is nice to see this manuscript further assessing the sensitivity of the method to various uncertainties. The manuscript needs a lot of work, however, to improve the presentation of the material and to more fully vet the results that are presented. I list below several major issues that must be addressed before the work can be considered for publication. Multiple minor revisions are also included to improve the manuscript, the presentation of which suffers from small mistakes and inconsistencies.

Major Comments

1. One of the largest shortcomings of the paper is the construction and discussion of the synthetic borehole temperature profile. All of the subsequent analyses are fundamentally tied to the assumptions that go into the construction of the synthetic profile. As such, it is surprising that no more than five lines are devoted its description and discussion. Perhaps one of the biggest assumptions that underlies the construction is the representation of past climatic history as a POM, or initialized temperature profile, and subsequent forward modeling of an SAT time series into subsurface (which we are also never shown). This construction is a best case scenario for the POM model because the forward model matches the synthetic reality. It would be a far more realistic test of the POM model to use a "representative" temperature history over the last millennium. Why not use an idealized MCA-LIA scenario or another multiproxy reconstruction to establish the baseline historical climate history? Both Beltrami et al. (2011) and Majorowicz et al. (2002) discuss this construction in more detail and the author should not gloss over the implications of the profile construction in the uncertainty analysis. It also should be noted that while the author does find a strong dependence of the reducing parameter on the POM estimate, the influence is limited by the synthetic construction. A short SAT series (how many years does it span?) driven into the ground will not penetrate much below the 160 m depth at which point the author estimates the background

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gradient (it is never mentioned how deep below 160 m depth the gradient is estimated, i.e. the depth range). Different GST histories that do not so closely match the author's forward modeling assumptions will increase the uncertainties that he defines. None of this is discussed but it should be.

2. The author barely mentions a very important uncertainty of borehole reconstruction, namely the nature of long-term coupling between SAT and GST signals. It is assumed that these two signals are coupled on decades to centuries and borehole inversions of GST therefore have been interpreted as representative of SAT changes. There nevertheless has been a lot of work to assess such an assumption (some of which could be cited, including the author's own work on snow cover) and it is possible that long term decoupling between SAT and GST may complicate interpretations of borehole inversions. The author's estimate of SAT uncertainties (I have no idea where 10 percent comes from on Pg 2507, Ln 4) is therefore a likely underestimate of true uncertainties imposed by the coupling between SAT and GST. Moreover, the coupling features are undoubtedly frequency dependent, making the uncertainty range in annual SAT values as the author has applied it a likely simplification of the true uncertainties in borehole reconstructions.

3. I would prefer a more complete discussion of the depth dependencies on the estimates of the reduction parameter. The impact of depth on the estimate is a nuanced one and a one-size-fits-all treatment is not appropriate. The impact of a borehole depth on reduction parameter estimates depends on the real GST history and the thermal diffusivity. The former is particularly important and plays a role in both the reduction parameter estimate at a given location and the role that the depth of boreholes play in large-scale averages. I think the author can expand this discussion to be more detailed, given that it plays a central role in a principal conclusion of his work.

Minor Comments

1. GST is used throughout the abstract, but not consistently in the manuscript. SGT is

used once (Pg 2505, Ln 26), while more vague uses of terms such as "climatic signals" or "surface temperature-time fields" are used elsewhere. It would be useful to be more precise and consistent with this terminology.

2. Abstract, Ln 2: I prefer "reconstruction of past climate" or "past temperature change" to "climate reconstruction." In general, the author could be more specific about what borehole temperatures can actually provide, namely reconstructions of past temperature or GST histories.

3. Abstract, Ln 24: There is little context for what "reduced" means at this point. I am not sure the word is necessary anyway.

4. Pg 2505, Ln 3: I do not like the use of climate field reconstruction here. This term is typically used to represent gridded reconstructions of climate variables, as opposed to large-scale average climatic indices (such as hemispheric or global temperature averages). There are some gridded products from boreholes, but the vast majority of borehole work targets large-scale averages. The borehole community has even argued for the importance of large-scale averaging to reduce noise in borehole reconstructions (e.g. Pollack and Smerdon, 2004). I therefore would prefer more restrictive terminology here.

5. Pg. 2505, Ln 5: How do they provide a broader geographic context? While a borehole can in principal be drilled anywhere (albeit many places would present difficult factors for interpretation), the geographic distribution of available boreholes for climate reconstruction does not look significantly different from other terrestrial proxies or multiproxy terrestrial networks.

6. Pg. 2505, Ln 9: I disagree that diffusion is the only disadvantage of borehole reconstructions. See my more general comments above, but the method is also limited by the fact that the downwelling signal of temperature change must be filtered through multiple land-surface processes. This obscures to some extent the interpretability of any GST reconstruction in the context of past SAT changes (the climatic variable of

most relevance in discussions of past temperature change).

7. Pg 2505, Ln 12: "Temperature-time fields" should be "temperature time series," but it would be better to use temperature histories or GST histories here.

8. Pg 2506, Ln 1: It is misleading to describe the POM in this way, although I admit that this has been the prevailing way of describing it in the POM literature. The POM is no more than the temperature initialization of the temperature-depth profile prior to initiating the 1D conductive forward model using the SAT as a time-dependent upper boundary condition. I think it would be much clearer to non-specialists to describe it in terms of an initialization than as a time step as the author does here.

9. Pg 2506, Ln 5: Again I think field is used incorrectly here. I would use GST histories instead of "the climate field." There is also the issue of the cited studies being 1000-yr reconstructions. First, I don't think these studies ever suggested their results were representative of the entire last millennium. Secondly, this gets to the deeper problem of calling the initialization temperature the pre-observational mean in that it implies a time component to the value. But it is no more appropriate to call a POM reconstruction a 500-yr reconstruction or a 10,000-yr reconstruction. There simply is no time component to the initialization profile, except that the initialization assumes a constant previous temperature ad infinitum into the past.

10. Pg 2506, Ln 2: Again, this should be SAT time series, not field. Also see my longer comments on the construction of the synthetic profile above. It also makes sense to introduce Figure 2 at this point in the manuscript.

11. Pg 2507, Ln 2: Provide a citation for the accepted range of diffusivity variations in common crustal rocks (e.g. Carslaw and Jaeger).

12. Pg 2507, Ln 4: Provide a citation for the range of SAT uncertainties. Clarify if you mean station SAT data or the grid center of a gridded SAT product (e.g. CRU or GISS data). Also, uncertainties in many products are time dependent (larger uncertainties

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in the earlier part of the records), making the uncertainty estimate of the SAT trends provided by the author conservative (if he assumes equally distributed uncertainties in each year).

13. Pg 2507, Ln 5-10: The variability of the reducing parameter comes from two sources: 1) uncertainties in estimating the parameter at a given location, and 2) the variability of the geothermal heat flow spatially. The author's estimate is more strongly influenced by the latter than the former. What is not clear is how variable the estimate might be over different depth ranges, in regions with different climatic histories (which would influence the depth of climate perturbations, in different geologic settings, etc.). The author should at least expand this section to discuss these limitations to the way he has estimated the variability in the reducing parameter.

14. Pg 2507, Ln 23: Figure 1 is misidentified here as Figure 2. It also is not clear from Figure 1 what the true POM value in the synthetic profile actually was. Is the median estimated POM close to the synthetically adopted value? It would be good to include the actual POM as a dotted line in the figure. This brings up a larger point of the paper regarding the mean and variance of the estimated POM. It should be noted (and preferably after using a more realistic past temperature history to construct the synthetic profile) how well the estimated POM actually compares to the known imposed value. It is one thing to talk about the variability of the POM distribution, but how well does the method provide a useful and interpretable estimate temperature prior to the SAT record? Also, what values of RMS misfit are determined for these results? Is it comparable to the $\sim$10-20 mK that are often shown for minimums in the POM literature?

15. Pg 2508: Why not show the distributions for the other variables, similar to Figure 1?

16. Pg 2509, Ln 13: I believe the author means "depth correlation" as opposed to spatial.

17. Pg 2510: The discussion about the impact of borehole depth is limited. It should point out that the impact of borehole depth is also dependent on the surface temperature history, namely the depth of perturbation may not be large if there have not been long and sustained surface changes.

18. Pg 2510, Ln 19-21: The author should mention that the averaging is also dependent on warming vs. cooling. The impact of a short borehole on reducing parameter estimates is to mute the amount of estimated warming or cooling. It therefore is difficult to say how this will work for a large-scale average without some knowledge of the number of warming and cooling boreholes and how many shallow vs. deep holes each of the subpopulations contains.

19. Pg 2511, Ln 3-10: This argument is very hard to follow.

20. Pg 2511, Ln 20-22: I am not sure that Beltrami et al. ever suggested that limiting the DOF would reduce the impact of uncertainties in the reducing parameter, nor am I sure that this has ever been suggested. The author should clarify this statement about how he sees his work fitting into the established literature.

21. Pg 2512, Ln 2: "a priori piece to the inversion:"

22. Pg 2512, Ln 27: "climate field" should be "GST history"

23. Pg 2513, Ln 1: "major borehole reconstruction"

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