

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

**Anonymous Referee #2**

Received and published: 14 September 2012

General Comments:

The manuscript is a dense (sometimes difficult to follow for lack of information) study on the uncertainties of the POM (pre-observational mean) method. It is an exercise where the author, by using a synthetic temperature log from a hypothetical borehole, tries to estimate the influence (uncertainties) of thermal diffusivity, surface air temperature (SAT), and reduced temperature on POM estimates. The title reflects the contents of the paper; the “Abstract” and the “Introduction” are readable and understandable; the same is not so true for the rest of the manuscript. Some paragraphs of the “Discussion and conclusions” are difficult to follow.

C1550

Many papers using the POM-SAT method have been used to reconstruct past climate from temperatures obtained in boreholes. The manuscript “Mapping uncertainties through the POM-SAT model of climate reconstruction from borehole data” by M. G. Bartlett is very welcome because it studies how uncertainties in three main parameters used in the POM-SAT method (thermal diffusivity, surface air temperature, and reduced temperature) influence POM estimates; it is a study of the robustness of the method to uncertainties in the thermal diffusivity, the surface air temperature, and the reduced temperature. The manuscript can be an interesting contribution to the field of climate reconstruction from borehole temperature data but needs some clarification and more detailed information about the construction of the temperature model used. The main shortcoming of the manuscript is related with the construction of the synthetic temperature log used to perform the study. Also no surface air temperature time series is shown. Why is the 160 m depth the maximum depth to estimate the geothermal gradient? 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. Besides diffusivity why not showing for all other variables a graph similar to Fig. 1? 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.

Specific comments:

The abbreviation GST (Ground Surface Temperature) should be consistently used along the manuscript instead of SGT (Surface Ground Temperature) (see Abstract and page 2505, line 25). I suggest the author is more careful with terminology; for instance: (page 2505, line 11) – “Ground surface temperature reconstruction” instead of “surface temperature-time reconstruction”; or (page 2505, line 12) “temperature time series” instead of “temperature-time fields”; or (page 2506, line 5) “reconstructions of the ground

C1551

surface temperature” instead of “reconstructions of the climate field”; or (page 2506, lines 20/21) “SAT time series“ instead of “SAT field”; or (page 2512, line) “ground surface temperature history” instead of “reconstructed climate field”. In section “Results” Figure 2 should be Figure 1 (page 2507, line 23).

Technical corrections:

(page 2505, line 15) – “character” instead of “charcter” (page 2512, line 2) – “piece to the inversion” instead of “piece of inversion to the inversion”.

Final comment:

I would suggest a medium to major revision to improve readability; a detailed explanation of some assumptions is needed as well as a clearer discussion of the main results.

Suggestions to the author:

1. Please give more detail about the construction of the synthetic temperature log.
2. Please give more detail about the construction of the surface air temperature time series.
3. Please make the discussion more readable.

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