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Climate of the Past Discussions

Interactive comment on "Variations in air and ground temperature and the POM model: results from the Northern Hemisphere" *by* R. N. Harris

R. N. Harris

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Manuscript cpd-2006-0068, "Variations in air and ground temperature and the POM model: results from the Northern Hemisphere", by R. N. Harris

Response to Referee Comments

Both reviewers provided careful and thoughtful reviews that helped strengthen this study.

Referee 1.

1. This reviewer's main criticism seems to be the use of coarse SAT data (5 x 5° gridded SAT data). The comparison between SAT and temperature-depth profiles follows from Harris and Chapman [2001, 2005], where the averaging of the different data sets are described in more detail. I have revised the manuscript to make clear the way in which

I average the borehole and SAT data. The reviewer is correct in his implication that the boreholes are not evenly distributed. If we were comparing boreholes and grid cells individually the reviewers intuition that SAT for a 5° grid cell compared with one or even a few boreholes would likely be correct. This was exactly the point of Pollack and Smerdon [2004]. Indeed, this study and others recognize that temperature-data is noisy and spatial comparisons must use representative areas and a lot of boreholes to minimize the noise. As the reviewer implies I am using the 5 x 5° grid because these extend back in time the farthest and allow this comparison at the longest temporal scales. If the analysis was a spatial comparison I would agree that the 0.5° data may be better and sacrifice time for space. Nevertheless, I formed an average weighted SAT curve using the 0.5° gridded SAT data. The minimum RMS in this case is 16 mK, whereas for the 5° gridded SAT data it is 18 mK. I consider this difference negligible. The POM does not change. Because the 5° gridded SAT data extends to 1851 whereas the 0.5° SAT only extends to 1901 I argue that the 5° gridded SAT is suitable for this analysis. This analysis is now alluded to in the paper.

2. I agree with the reviewer that the rise in SAT is not linear, and there are variations in both the rise in surface air and surface ground temperatures that are nonlinear. I fit the average SAT time series linearly to provide a comparison later in the paper with a linear fit to the temperature data. I have added text to make this clear. This initial fit builds confidence that a more sophisticated analysis is warranted. If the linear comparison was poor, it might indicate that a poor fit at shorter periods. I think the model and SAT produce similar results because these signals are related. These points are discussed in the revised manuscript.

3. The SAT power spectrum is removed from the paper. This analysis did not impact the study and is distracting.

4. The reviewer questions the notion that the impact of snow cover on ground temperatures is small. This topic is an active area of research. I agree with the reviewer that the impact of snow on ground temperatures is scale dependent. It is also true that pa-

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pers finding a large impact of snow cover on ground temperatures are located in high latitudes, while the global borehole data set is mostly mid-latitude. These points have been included in the discussion, and the impact of snow cover has been removed from the conclusions.

5. The reviewer questions the validity of conclusion 3. This may reflect a poor description of the comparison. In fact the linear trend fit to the SAT data is not used in this comparison as implied by the reviewers comment. While linear trends are an oversimplification of both data sets, they do represent the lowest frequency component of change and the agreement of trends provides independent suggestion that temperature changes represented by these datasets are consistent. This is now included in the text.

Minor comments.

The POM is defined in the first line as suggested.

Line 10, abstract, now changed to make the meaning more clear.

Line 11, abstract, now changed to make the meaning more clear.

Line 8, page 348. This text was correct as originally written, but has been changed to make the meaning more clear.

Referee 2

P341 L15 - I have largely rewritten this section to discuss how proxy data and temperature profiles are complementary, and have removed the discussion of seasonality since it is not germane to the manuscript. My intention was not to question the validity of reconstructions based on tree-rings. I have included a reference to the Hegerl paper which was published after this manuscript was submitted.

P341 L25 Purpose of study. Text clarifying the purpose of this study has been added.

P343 L01 - Thermal length is now explicitly defined. The rationale behind using two

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thermal lengths for the RMS calculation is included.

P343 L21 1. The reviewer suggests another way to compare temperature profiles and SAT curves is in the inverse sense. That is to compare the GST solution from a temperature-depth profile with the SAT curve filtered by the resolution matrix. The reviewer is correct in stating that this is a less direct method and subject to issues such as how the GST solution is parameterized and stability issues associated with inversion. Additionally one goal of the paper is to assess the POM model.

2. In effect the reviewer is making two points. 1) The POM model always has a solution, even if GST and SAT do not track, and 2) how do we assess the magnitude of the misfit? These comments are now addressed. I interpret this comment to mean that a good fit does not prove a causal relationship. This is of course true with all models and a statement to this effect is included. Additionally I now include a discussion of the null SAT change hypothesis that provides a benchmark against which models incorporating SAT data can be compared. In a sense this study suggests that if GST and SAT are not tracking each other, the POM will shift to a significantly fictitious level only if the period of decoupling is longer than the length of the forcing function.

P345-347 Section 3 1. The reason for the array of periods is now explicitly stated.

2. The colored time series are now included which helps clarify the figure.

3. Yes, this statement is now included.

4. Yes, I have incorporated this comment. In effect these synthetic tests consist of diffusing an errant amplitude at some period into the subsurface, adding to the temperature profile, and then computing the POM that minimizes the RMS misfit.

5. This figure and figure caption has now been corrected.

6. See above.

7. The wording has been changed as suggested.

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8. Yes, while the temperature profile is 500 m, only the top 275 m are used in the RMS calculation.

9. Figure 4 and caption have been corrected as suggested. Additionally amplitude has been used for weight.

10. Message. I have reworded the text to highlight the significance of this section. There are two points. The first point is that estimating the POM is an integral part to estimating the total amount of warming. The results indicate that even if air and ground temperatures are not perfectly tracking each other, the POM is a robust parameter. The second point is that the degree of tracking between air and ground temperatures is interesting and this method provides a way of assessing tracking at long time periods.

P349-350 1. I hope that any confusion regarding this comment is now cleared up through the rewrite.

P351 The conclusions have been rewritten to clarify the points brought up by the reviewers.

Interactive comment on Clim. Past Discuss., 3, 337, 2007.