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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

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1 Brief description and general comments

The author analyses the sensitivity of the POM model to alterations in the surface air temperature and ground surface temperature coupling through a series of synthetic tests in which specific frequencies are muted and the impact on the POM, and least squares fit between the SAT diffused transients and reduced temperatures is assessed.

I like the approach posed in the text and find interesting its application to analyze the sensitivity of the POM to alterations in various time scales. After reading the manuscript I find however a fundamental disagreement with the author in the interpretation of results in Section 3 and their application to Section 5. I am aware that the POM model

has been used in a number of studies and that there is firm experience behind its use, but I think this can be a good opportunity to discuss in the text these interpretation discrepancies and clarify them.

I think the manuscript should clearly state in the introduction and section 2 the potential limitations of the method as stated in the comments below. As a reader, I have the impression that results in Section 3 support the idea that the POM model can fit artificial non-climate trends present in the observations that decouple the SAT-GST relation and produce a false impression of a good fit. I would support publication of the manuscript only if this issue is discussed and clarified in the text. I think a clear argumentation on the limitations of the POM model or the inadequacy of an alternative perspective like the one provided below would be beneficial for the manuscript and the community.

Other than that, the text is clear in explanations and well structured. There are a number of confusing details in the synthetic experiments which hamper consistency with the danger of confusing the reader which I report below.

I will be looking forward to reading a constructive discussion on the advantages and limitations of the POM model published in CP.

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2 Specific comments

P341 L15 *These multi-proxy networks rely to a large [...] in general agreement with borehole temperature profiles (Esper et al., 2002; Moberg et al. 2005)*
In the introduction the author indicates that the motivation or one of the fundamental motivations for questioning surf. air temperature and ground temperatures tracking is the different amplitude of warming along the last centuries estimated with borehole or with other proxy (tree-ring) records. The author argues that such differences can be reconciled if we consider that tree-rings are mostly sensitive to warm season conditions and since in the last 150 years most of the warming has taken place in the cold season, a warm season indicator will contribute to underestimate such trend. Then the text continues to indicate that *'Tree-ring networks processed to retain low frequency information are in general agreement with borehole temperature profiles'*. While I can agree with some aspects of these statements, I find some of them can be commented:

[1] A recent publication by Hegerl et al. (2007) offers a reconstruction which seems compatible with borehole records also using tree-ring reconstructions and might be worth mentioning.

[2] The argument about seasonality finds also a basis in other observational (Jones et al, 2003; Luterbacher et al, 2004) and modeling (Bauer et al. 2006) studies. The warm season focus and detrending in tree-ring analysis are indeed problematic features. I find the way it is emphasyzed however rather inadequate. The text says 'tree-rings cannot record long term annual trends because they register warm season temperatures' and afterwards 'tree-ring networks processed to retain low freq. info. are in general agreement with borehole ...'. If the ultimate limitation of tree-rings to capture long term annual trends would be their warm season focus, the second statement would not be possible. If, treated appropriately, tree-rings can capture long term annual trends, then the first statement has

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no absolute validity, i. e. , the problem would be the filtering techniques. While both issues are problematic in tree-ring reconstructions the text seems to try to push the argument of discrediting the validity of tree-ring reconstructions somehow in favor of borehole reconstructions, when this is not necessary since both approaches have their advantages and limitations.

[3] As a follow up of last comment, the text would be perhaps better balanced if it would state also some limitations which borehole reconstructions have to deal with in the context of the discussion. For instance systematic long term trends in snow or vegetation cover would ultimately introduce also a bias that would emphasize ground temperature warming in comparison to surface air temperature warming and this would potentially also reconciliate both approaches. This is slightly suggested in the questions posed after L25 but the author might consider clearly stating them in this context for the purpose of clarity and balance in the text; after all the real causes for the discrepancy have not been ruled out so far.

P341 L25 *Purpose of the study*

I would suggest some clarification of the purpose of the study, in particular justifying from the beginning the structure of the manuscript with the synthetic tests in section 3 and how this relates to the real world application in Section 5. I think that explaining this in advance would help the reader.

P343 L01 *Thermal length*

The meaning of *thermal length, tl,* should be specified here. I understand that it is reasonable to place the emphasis on the top part of the borehole profiles where there can be substantial differences between observed and diffused values. However, can you provide any basis or criteria for explicitly choosing 2tl? What are typical values of tl in the text?

P343 L21 *Advantages of the POM model are multi-fold [...]*

This paragraph states some positive aspects of using the POM concept. I can

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agree with the appropriate frame of comparing data in the same 'spectral domain', the borehole profiles and the forward transient signal which has lost part of the high frequency variability in the diffusion process. Some comments:

[1] Another possibility would be to compare GST inverse solutions with inverted histories of SAT diffused signals. I understand this option may be less direct, however, the inverted history of a SAT transient profile should recover the long term trend in the SAT record along its interval of duration. This long term trend should be compatible with the diffusion process and comparable to the GST inverted history in terms of frequencies and errors could be calculated within this time frame without making any assumptions about POM and using for the statistics only the temporal interval of availability of observations.

[2] P343 L21 to P344 L02 highlights the advantages of using the POM model. I think it would be good to report the reader with a discussion on potential shortcomings. As it stands this reader gets the impression that the POM is just an innocuous tool to account for the average climate history previous to the observational period when comparing the transient climate profile with the actual borehole profile. However it would not be difficult to consider situations in which if GST and SAT would not track each other (snow, vegetation, instrumental data inhomogeneities (!), advection due to complex topography, etc) the POM model would be able to find an appropriate pre-observational fictitious average level that would tilt the transient forward profile enough as to optimally fit the borehole profile in a least squares sense. I understand that using the POM concept can be useful and, as fairly mentioned in the text, it rests on the idea that the borehole temperature profile is the integrated heat-conduction history of GST evolution, which is assumed to faithfully track SAT through time. But, if this was not the case, could we be in the situation of delivering an apparent good quality fit? I think this should be discussed, and actually the following sections can be interpreted in this direction.

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P345-347 *Section 3*

I liked the approach proposed of checking the impact of muting frequencies in the forcing series as a general means of representing from a general perspective processes which might contribute to decoupling SAT and GST. Some comments follow:

[1] *... having a duration of 144 years and considering 72, 144 and 500 years...* Why was this array of values chosen? Seems arbitrary but this should be simply stated. Perhaps selected with some purpose of replication of the situation in Section 5?

[2]*Figure 2a: missing series?*
Seems like some of the time series (colored) indicating the muted forcings are missing.

[3]*...Because a particular frequency is muted for each transient the subsurface heat content is less than that constructed using all periods*
Is this also because the time series were phase shifted to build up the 'recent' warming trend? If they would have negative trends muting them should contribute to warming, thus in the opposite direction. The same would potentially apply for the statement *In each case ... toward a positive value*.

[4]*Searching POM for muted series vs. original profile*
I understand from the text that in each exercise the original profile is altered with the POM to match the time series with muted frequencies, otherwise the statement 'The shift in the POM decreases ... heat content of the muted transients' would not make sense. Since the original time series is the 'purported truth' wouldn't it be better to shift the muted series accordingly with the POM to match the 'realistic' profile?. I would assume similar numbers would arise though just with different sign, but the posing of the problem would reflect (I think) better the rationale behind. In terms of energy, since the time series show positive trends at the end of the synthetic record, the complete series stores maximum

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heat in the subsurface and the implication of using the POM is to provide a compensation for that heat in the frequency domain which partially balances the muted frequency.

[5] *In the second set ... periods of 18, 36, 72 ... the forcing function has a duration of 144 years ...*
Is it possible that the time series in Fig. 3a is wrong? Its length is of 100 years. Also there is a timescale of about 10 years which is apparent in Fig. 3a and does not seem to match the statement, though this could perhaps be the result of mixing periods which are multiples of a base period with appropriate amplitude and phase? 18, 18*2, 18*4, 18*8... Also, why this arrangement?. The selection of frequencies is (seems) arbitrary in itself and it should be argued, but this arrangement of harmonics, is there a reason for it?

[6]*Figure 3*
Please, revise the figure caption. The '144 year period' statement does not seem to match. In panel a) a 'blue' line is expected but it is not visible. b) also mentions red and blue lines, but no blue line is apparent. Substitute '(b)' by '(d)' in 2nd line before end.
Is it meaningful to speak of 'negative' amplitudes? If I interpret correctly plots c) and d) would be well served with positive amplitudes since negative values just duplicate positive with a shift of π rad.

[7]*Sensitivity to the amplitude is lost ...* $\pi/2$ *out of phase* Should it say 'reduced'? there is still a range of values between aprox 30 and almost 70 mK?
Any comment about the POM range at $\pi/4$ and $3\pi/4$?

[8]*... the temperature profile corresponds to depths between 30 and 500 m* I assume that the depth involved in the calculation of the misfit stays fixed at 270m (2tl). Clarify otherwise.

[9]*Figure 4*

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Check that caption states now that forcing series is 100 years long instead of 144, compatible with plot of Fig 3 but not of its caption and text.
The text (P347 L03-L04) states that *noise decreases the sensitivity of the RMS misfit function*: however, the change in Fig4f compared to 4d seems quite weak, just an isoline that desapears?
Does 'weight' stand for 'Amplitude' as in Fig3?

[10]*Message*
All in all what would be the message for this section? Beyond the details that the errant 144 yr period with amplitude 1C produces a 0.05 POM or the more sensitive 500 year period with a 0.8 C POM, the results seem to suggest that relatively large discrepancies between SAT and GST can be balanced with quite small POM values. Of course, this allows to technically argue about the sensitivity of POM, but it also indicates that in reality having small POMs can hide large discrepancies. In the direction of L16-17 in P345, '*the danger of a model fit as evidence of faithful tracking between air and ground temperatures*' should be highlighted and discussed. This links to the comments about making clear statements on the POM advantages and disadvantages at the end of Section 2. The implications of this problem is perhaps the part I miss more discussion about in the manuscript. The author details some of the numerical aspects of the result but I miss a perspective of implications.

P349-350 *Section 5*
Moving into the perspective of Section 5 using observed borehole profiles I have problems in reconciliating the interpretation of the author with the results of Section 3.

[1] The author finds a good fit with 0.58 POM and 18 mK between the transient SAT profile and the reduced temperature profile. One potential perspective behind this results is that this POM value is required to account for the climate history before the starting of observations so that the relatively short

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temporal period of measurements can adapt to the 500 m vertical profile when diffused into the ground. However, in view of the results of the previous section it could also be argued that such POM and RMS values can hide important decoupling events in frequency components of the SAT time series relative to the reduced profile. One could argue that some systematic long term decoupling mechanism could have produced some relatively large deviation between SAT and GST and still this deviation could have been accounted for by a relatively small POM delivering a fictitious impression of good match between both signals.

P351 *Conclusions and abstract*
Conclusions and abstract should be revised in view of the comment above or, at least some discussion and explanation would be appreciated. Also:

[1] *... insensitive to periods shorter than about 25 years...*
Where does this specific quantity come from.?

[2] *... The POM is -0.7 C below the ...*
In page P349 L12, the text reports about 0.58 C below the 1961-1990 mean

I think some overall wrap up discussion addressing the limitations in the use and interpretation of POM would be necessary.

3 Minor comments

P340 L26 *[...] other processes [...] has also [...]*
Should be '...have also...'

P343 L09 *[...] measure of the change in heat [...]*
...heat *content*...

P346 L27 *Figs. 4a and b repeats ...*
Should be '... repeat...'

P350 L04 *... misfit plot (Fig. 6)...* Fig. 8?

P352 L21 *References*
Clauser and Huenges (2005) should be Clauser and Huenges (1995) as cited in the text.

P352 L26 *References*
Crowley and Lowery (2002) should be Crowley and Lowery (2000) as cited in the text.

P353 L05 *References*
Harris and Chapman (1997) is not used in the text

References

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