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Climate of the Past Discussions

Interactive comment on "Influence of radiative forcing factors on ground-air temperature coupling during the last millennium: implications for borehole climatology" *by* Camilo Melo-Aguilar et al.

Anonymous Referee #2

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Summary: This manuscript uses an ensemble of LM simulations from the NCAR CESM1.1 LME to investigate the interannual to centennial coupling between SAT and GST in both time and space. The simulations allow the authors to separate different forcing influences on the coupling and to provide some detailed physical assessments of why the coupling changes over different timescales. The biggest implications of the work relate to inversions of terrestrial borehole temperature measurements as estimates of GST changes (and their interpretation as indicators of SAT changes) over the past 500-1000 years.

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Discussion paper

General Remarks: This is overall a well written and interesting manuscript. It is a logical next step, given the work that some of these authors have done to test SAT-GST coupling and the implications for borehole paleoclimatology in LM simulations. It goes into detail to explain the various coupling behavior that is observed and has some important conclusions. I therefore think that it should ultimately be published, but I have significant concerns about some of the analysis as outlined below. A major revision is therefore necessary before the manuscript should be accepted for publication.

The biggest analysis issue that concerns me is the two-phase regression model. The authors do not give a reasonable justification for selecting this regression model and I think it has several significant impacts on their conclusions. In particular, the break-points influence their results in ways that can be hard to interpret. One way in which this is manifest is in their physical interpretations of the coupling influences. In Figure b, d, and f we are provided time series and regression results for SAT-GST and snow cover. The author's interpretation is that snow cover has influenced the coupling, but it appears that the breakpoint comes earlier in the SAT-GST time series than it does for snow cover. If we are to take the breakpoints as physically meaningful (and I think that should be done very cautiously), why should we conclude that snow cover is driving the decoupling when the major break in its trends comes after the breakpoint in SAT-GST? The answer is that we probably should not take the breakpoints very seriously and it is a shortcoming of the analysis that the regressions are confined to the two-phase model. This is also a problem in the comparison of trends in Figure 9. The upper panels indicate that the break points vary significantly spatially, with some locations yielding breakpoints in the 1000-1200 period and many in the 1700-1800 period. All of these trends of course end at the beginning of the 21st century, meaning that the second-period trends displayed in the middle and bottom panels of Figure 9 are taken over vastly different time periods. It is very difficult to determine how to compare such trends, many of which will be impacted by strong 20th-century changes that are either much stronger because of a later breakpoint or much weaker because of an earlier breakpoint. One might justify this approach if the breakpoints could more reliably be interpreted, but in many cases, indeed as evidenced across the ensemble, there is a lot of variability in the determined timing of the breakpoint (look at Figure 15, for instance, and try to convince yourself that the break points in the upper panel for ALL-F or OZ/AER shouldn't be several centuries in either direction of where the breakpoints were determined to be). I therefore have a very hard time interpreting the magnitude of the trends and what they actually mean. As such, I think the two-phase regression model is both poorly justified and probably a very confusing interpretation of the data.

Related to the above, the overall magnitude of the impact is hard to parse in the context of what the authors have done. They look specifically at the 1850-2005 period and note that the overall difference in SAT and GST trends looks to be about 0.05C. First of all, this should be couched in the context of the 20th-century trend estimate from the global borehole reconstruction of ~0.5C/century for the 20th century. If we take the model estimate at face value, the error introduced is therefore less 10%, given the authors' estimate is over a longer period of time (it should be reported in C/century for better comparison). But the other analyses complicate this number and I am not sure how to interpret them. The trends estimated in Figure 9 for SAT-GST are +/- 0.1C per DECADE, which is a much larger regional impact than the number determined for the global difference (I understand these regional impacts would be muted in a global average, but the numbers are complicated by my subsequent point). Part of the confusion is because of an apples-to-aardvarks comparison, namely the global trend difference is from 1850-2005, while the trends in Figure 9 are for widely different intervals determined by the breakpoint in the regression analysis. In some ways it would be much cleaner if the authors determined trends over set centuries to make sure the comparisons were consistent in time and to make the comparisons to the actual borehole reconstructions simpler. Such an analysis would have its own shortcomings, but as things stand the presentation is ambiguous on the most important point of the paper, namely if and how big a bias might be present in the borehole reconstructions based on the authors' analysis.

Another issue related to the interpretation of the authors' results in the context of the borehole reconstructions is the actual spatial sampling of the borehole database and how it may actually be impacted by the biases the authors are reporting. This is particularly the case regarding snow cover and the other cryogenic effects that the authors report for the high latitudes. While these impacts are reasonably described and there are clear differences between GST and SAT particularly in the high northern latitudes, there simply aren't that many boreholes used for the global reconstructions above about 55-60 degrees N latitude. While the biases are certainly a concern and relevant to any regional studies of high-latitude boreholes, it is not clear that the regionally very large impacts play much of a role in the existing NH and global borehole reconstructions. The authors could do a lot to quantify the effect by performing an analysis that restricts the assessment to locations where boreholes exist in the reconstruction database. I would advocate for that, but it would also add to what is already a long and detailed paper. It would suffice to simply mention this issue and *perhaps* provide at least some numbers on the impact of the hemispheric or global estimates if, for instance, latitudes north of 60 degrees were excluded.

Specific Comments:

The paper is generally well written, but there are language issues scattered throughout. I have noted some of those below, but the paper would be improved by a thorough evaluation of typos and language choices.

Pg. 3, Ln 9: has been supported by observations...

Pg. 4, Ln. 2: PPEs experiments is redundant

Pg. 6, Ln 1: It composes a total...

Pg. 6, Ln. 12: covariance during...

Pg. 6, Ln. 16: may help characterize a potential...

Pg. 6, Ln. 28: associations

Pg. 7, Ln 4: I don't think that the OZ/AER (ozone/aerosol) abbreviation has been defined in text.

Pg. 7, Ln 5: allows identification

Pg. 7, Ln 7: through industrial times

Pg. 7, Ln 3: I don't think the subscript notation for the ensemble member or the layer depth has been explained anywhere. It should be defined for completeness.

Pg. 7, Ln 20: While the correlations with GST and the lower depths are perhaps useful to explore, it should be explained that because of the phase shift correlations will diminish even if the system is purely conductive. All borehole inversions take into account the phase shift and this kind of time series comparison is a bit misleading. The authors should clearly point this out if they are going to leave this analysis in the manuscript. For what it is worth, some of the length and detail of the manuscript would be improved by removing these specific analyses because I do not think they add much.

Pg. 7, Ln 26: very cold air temperatures

Pg. 8, Ln 30: consider "In contrast" instead of "On the contrary"

Pg. 9, Ln 28: relationship at relatively

Pg. 10, Ln 3: during JJA consistent with

Pg. 10 Ln 8: high rates

Pg. 10, Ln 24: such partitioning determines

Pg. 10, Ln 27: because the water, relative to the continental air, is warmer in winter.

Pg. 10, Ln 33: The paragraph that ends here is an example of where things get a little too detailed. I think that this paragraph in particular could be scaled down a bit.

Pg. 11, Ln 14: during these months

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Discussion paper

Pg. 12, Lns 13-14: It is relevant that other GCMs may suggest different levels of impacts, but this seems a little strange in the context of the real world. The real question is how well the GCMs represent the real-world impacts. It would be useful here to make a statement along those lines, in connection to what consistency or disagreements across models might imply for the real world.

Pg. 12, Ln 16: trends of both temperatures independently

Pg. 13, Ln 1: Why not report pattern correlations in this discussion? A quantification of the spatial similarity would seem helpful.

Pg. 15, Ln 15: the driving mechanisms

Pg. 15, Ln 26: The authors say nothing about simulated increases in LAI here. Instead of transitions to different plant functional types, it is also possible that increases in CO_2 during the 20th century drive LAI increases in the CESM that in turn impact evapotranspiration. This has been shown for the model projections and also may play a role in the historical portion of the runs that the authors are analyzing. See the following paper for some background:

https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2018GL077051

Pg 15, Ln 32: stable state

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