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Climate of the Past Discussions

Interactive comment on "Rapid shifts in South American montane climates driven by $p\mathrm{CO}_2$ and ice volume changes over the last two glacial cycles" *by* M. H. M. Groot et al.

Anonymous Referee #3

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The authors present a new lake sediment core taken in the high montane region of the Southern American Andes. Changes of the percentage of arboreal pollen (AP%) are used as a predictor relating changes in the surrounding vegetation to mean annual temperatures (MAT) changes. The AP% is analysed using spectral analysis and orbital tuning to be comparable to other reconstructions and to identify possible drivers of the climate variations on different time scales. The results from the sediment core are compared with three model experiments of a global climate model with intermediate complexity using different external forcings.

Major comments:

The paper presents a highly valuable new sediment core with a noticeably high temporal resolution of the last two glacial cycles. However, there are several serious caveats related to the interpretation of the results and the general structure of the paper.

Considering the scope of "Climate of the Past", the details about the spectral analysis and orbital tuning should be shortened and focussed, because methods are already well elaborated in the scientific literature. Moreover, most tables are very important for the core itself but can be omitted for the understanding of the results in the paper.

The authors should re-structure the chapters about the methods and the discussion as many sections in the discussion should already be part of the methods. This would make the discussion much more focused and understandable. Some more subparagraphs would be helpful.

The language is overall acceptable. However, some sentences are difficult to understand. Also the quotation of many references one behind each other, without referring to their respective content make some parts very difficult to understand.

I suggest the publication in "Climate of the Past" after a major revision of the manuscript.

Minor comments:

Title:

Rapid shifts: should be defined relative to the time scales of glacial cycles in the paper – pCO2 and ice volume changes are the reason for RAPID climate shifts - this would be a hypothesis for the explanation of rapid climate shifts dependent on what is meant by rapid relative to which time scale. Moreover the regional scope of the analysis should be focused a bit more. I would suggest to re-formulate the title without the hypothesis to:

'Climatic variability and forcing mechanisms in northern and central South American montane regions within the last two glacial cycles'

Abstract

l. 4: Please clarify here whether the records are measurements or reconstructions and for which kind of variable they represent (temperature, pollen?).

l. 4-5: Please clarify to which process or variable the statement “their magnitude and rates of change” refers.

l. 12-14: Low spatially resolved global models underestimate a priori the climate variability on non-global scales. The global model cannot underestimate local processes as they are not existent in the chosen model. If the latter should be considered, numerical or statistical downscaling of the global model output has to be applied over the given domain.

1 Introduction

The introduction together with the conclusion is crucial for a good understanding of the paper. Both are too short. A lot of references are given without discussing them. A selection of the given studies should be shortly discussed to better explain the context and improvements of the paper relative to literature already published. For instance, model simulations have already been carried out for the last glacial cycles related to the question of climate change due to orbital changes on regional and global scale which should be shortly discussed here.

Please explain what is meant with pCO2. It should be shortly discussed why and how rapid shifts should be caused by CO2 and ice volume changes and how this might have an impact on the environment of high montane regions in the given region.

- explanation for pCO2 is missing (radiative forcing, magnitude of changes, relation to temperature changes caused by CO2) - literature (1) shows, that CO2 is lagging behind the temperature changes on timescales discussed in this paper. If CO2 is a climate driver or only an amplifier is still unclear. See e.g. Fischer et al. (1999), Monnin et al. (2001), Caillon et al. (2003), Siegenthaler et al. (2005) for lagging CO2 in case

of Antarctica (listed in ref. 1)

(1) Ganopolski, A., Roche, D.M., On the nature of lead–lag relationships during glacial-interglacial climate transitions, Quaternary Science Reviews (2009), doi:10.1016/j.quascirev.2009.09.019

(2) Sigman, D. M. and Boyle, E. A.: Glacial/interglacial variations in atmospheric carbon dioxide, Nature, 407, 859–869, 2000.

It is not obvious how rapid and extreme MAT changes within few 100 years should coincide with 100kyr or 41kyr cycles. This would mean that rapid changes would only occur on this cycles which is not the case (rapid shifts occur much more often, of course dependent on the definition of rapid and extreme). This should be more clarified in this work.

The separation of the driving mechanisms of climate variations in general and rapid shifts is not completely elaborated. From the title of this work follows that changes in CO2 and ice volume are responsible for rapid changes of MAT (this would be a hypothesis?) whereas the general (slower) insolation changes are controlled by changes in orbital configuration.

(3) Broecker, W.S., 2003. Does the trigger for abrupt climate change reside in the ocean or in the atmosphere? Science 300, 1519–1522.

(4) Timmermann, A., Timm, O., Stott, L., Menviel, L., 2009. The roles of CO2 and orbital forcing in driving southern hemispheric temperature variations during the last 21,000 years. Journal of Climate 22, 1626–1640.

(5) Stephens, B.B. and R.F. Keeling (2000), The influence of Antarctic sea ice on glacial/interglacial CO2 variations, Nature, 404, 171–174.

p. 2119, l. 22: Please shortly discuss the main features of the high altitude regions here according to the given reference from 2009. It is not clear why high altitude climates are responding to pCO2 or glacial-induced ice volume changes whereas the surrounding

lowlands don't show this phenomenon.

p. 2120, l. 18: Use a new paragraph here to separate current climate conditions from those of the past.

P 2120, l. 1-3: The "previous investigations" should be shortly discussed here as they form the base of the presented work.

p. 2120, l. 8-9: Figure 1 displays current SST. However, the impact of SST on the climate is not discussed here. Please indicate the relevance of SSTs to the present work. Alternatively you could provide figures displaying precipitation/temperature as motivation for the mean present-day climatic conditions for the different seasons.

p. 2120, l. 8-11: Amount and annual variability of precipitation around the lake might be important when discussing biome migration patterns even though temperature might be the main limiting factor here.

p. 2120, l. 18 ff.: Changes during glacial conditions should be discussed here in more detail.

p. 2120, l. 24: Use a new paragraph here for the modelling part. The given information is much too short and should discuss already existent model studies related to orbital forcing and climate (refs)

(6) Ganopolski, A., Rahmstorf, S., 2001. Rapid changes of glacial climate simulated in a coupled climate model. Nature 409, 153–158.

(7) Yoshimori, M., A.J. Weaver, S.J. Marshall and G.K.C. Clarke (2001), Glacial Terminations: sensitivity to orbital and CO2 forcing in a coupled climate system model, Climate Dynamics, 17, 571–588.

2 Material and Methods

add "model setup" to chapter 2.

In general, this section is quite short because some parts are distributed over chapter 3 (spectral analysis) and 4 (climate model). Methods and description of analysis or climate model should be more separated from results and discussion. I would suggest an addition of two sub-chapters here:

Most parts of chapter 3.2 (spectral analysis) and some of 3.3 (orbital tuning) might be moved to a new sub-paragraph "2.4 Spectral Analysis and Orbital Tuning". This would make chapter 3 more focused on the results.

The model description including the different settings might be explained here in a new sub-paragraph "2.5 Climate model" instead of chapter 4.2 in the discussion.

2.1 Sediment Cores

Please leave out Table 1. A short description of the lake (mean and max. depth, volume, size, lake regime, inflow, sedimentation) would be helpful. The reason why exactly this lake is used should be shortly motivated. GPS coordinates of the lake/cores might be helpful for some readers, if available.

2.2 Analytical Methods

Leave as is.

2.3 Pollen Analysis

This section seems to be a little too short, i.e. concerning biome migration patterns and altitudinal shifts related to temperature changes. This might not be obvious for all readers of the paper.

l. 9-10: This sentence makes no sense. I suppose ". . .with a clear response to. . ." was meant here. At least some of the given references should be explained here. Which pollen or spore taxa are related to which vegetation in terms of climatic/environmental changes?

3 Results

The description of the spectral analysis is too detailed (not clearly motivated) and should be part of chapter 2 instead of 3.2. Many details are already part of literature and should be referred to the given references.

3.1 Composite Section

Table 2 and 3 are not giving any useful information here and could be removed

3.2 Spectral Analysis

Too many technical details make the section difficult for the reader to follow. The method itself should be discussed in chapter 2. Only results should be discussed here, e.g. the explanation and discussion of Fig. 5.

p. 2125, l. 20-21: What means “less significant”? It can only be significant or not at a given level of significance.

p. 2125, l. 21-25: It seems to be questionable to attribute 100kyr cycles with 99% significance here when the respective data basis only comprises of 264kyr. For statements related to statistical significance more 100 kyr cycles would be necessary.

3.3 Orbital tuning

The method might be better explained in chapter 2 in order to concentrate here on the results. It seems that information of Milankovitch cycles is already included in the calibration of the age model? If so, this should be stated more clearly.

The 9m component was already identified as 41kyr cycle in chapter 3.2 and is now “correlated” or fitted to the 41kyr component of LR04 to build up an age model. Even it might be reasonable to do so, this method leads to an a priory inclusion of information of Milankowitch cycles in the calibration of the age model. This leads to a circular reasoning when relating the fitted reconstruction to data it was fitted prior to analysis.

4 Discussion

The description of the climate model should be part of chapter 2 instead of chapter 4.2. This makes the section much more readable and precise.

4.1 Mean annual temperature reconstructions

p. 2128, l. 6: Which earlier pollen based records? Same line: please remove the term ‘radically’

p. 2128, l. 9-12: might the “highly significant power” here be influenced by the underlying age model?

p. 2128: ll. 16-21 and p 2129 ll 12-14: please only refer to a selection of most important publications related to your context of citation.

4.2 Comparison with transient modelling experiments

Model description and setup is in general good but should be part of chapter 2.

p. 2130, l. 3: The model analysed here with a very coarse horizontal resolution is a priori not very well suited for comparisons in the context of “regional vs. globally-induced temperature variations”.

p. 2130, l. 7: Only a “very fast turnaround time” is no reason to use a specific model. The choice of a climate model should better be motivated by the underlying scientific question rather than the availability or computational effectiveness. Even if it is clear that the current computer power does not allow highly resolved GCM simulations over glacial timescales this point should at least be mentioned when comparing results from very coarsely resolved EMICs with local-scale empirical evidence.

p. 2130, l. 11: This resolution might be even too coarse to explain much of regional climate variations or mechanism as 10° lat corresponds to 1111 km and 51° lon corresponds to 5100 km. Because the longitudinal distance of South America at Lake Fúquene is less than 30°, I don’t know how this should work at all. There might be only one grid cell for this grid position and the altitude of this grid cell is most likely very low

and not representing high altitude conditions. The model results should therefore only be used for comparisons at the large scale.

p. 2131, l. 5: What is meant with “La04(1,1)” here?

p. 2131, l. 14: “measurements” – please use the term ‘reconstructions’.

4.3 Correlation between land and ice records of climate change

In general, the regional aspect of Southern/Central American climate in the past is not discussed in the paper. There are several studies in a special issue at least for the period since the last glacial cycle, e.g. (many articles within Vol. 14):

(8) Francoise Vimeux, Florence Sylvestre and Myriam Khodri: Past Climate Variability in South America and Surrounding Regions From the Last Glacial Maximum to the Holocene. Developments in Paleoenvironmental Research, Volume 14, 2009, DOI: 10.1007/978-90-481-2672-9

p. 2134, l. 2: The millennial time-scale is hardly visible on a 160kyr time scale of Fig. 8.

p. 2134, l. 6: DO number 8 is shifted by 4000 years on Fig. 8 and not synchronous. In addition, DO number 24 is existent also in both records. DO 26 of Lake Fúquene does not match DO 26. However, DO 27 of Lake Fúquene is synchronous to DO 26.

p. 2135, l. 1: Is the reduced Atlantic meridional overturning circulation also visible in the CLIMBER experiments? Despite the coarse spatial resolution of the model, the mean shift of the ITCZ might be also visible in the model results. This could show (dis-)agreement with results from the empirical pollen analysis of the present study. Also this results could be helpful to give an indication of general climate “shifts” in the tropics explaining at least qualitatively the environmental/climatic history of Lake Fúquene.

5 Conclusion

Compared to the overall length of the manuscript the conclusion is very short. In the

conclusion the results of the analysis should be summarized and also put into perspective and discussed with results already published in the context of the study.

Illustrations and Tables:

The figures are in general good but should be explained in greater detail (in the text and/or the figure caption). The tables are not important for the understanding of the paper and most of them should be removed. If Tables are used, a short explanation must be given about the table.

Minor:

Fig. 1: The full or a higher range of the colour bar should be used for the SST. Characters in the figure should be larger and/or in bold. In general, it is not obvious why SSTs are shown at all neither from the title nor the text of the paper. This should be shortly explained or the Figure should be changed to be consistent with explanations given in the description of the overall climatic characteristics of the region

Fig. 2: A reference of the classification/altitude should be added below the figure.

Fig. 4: In (B) and (C) use “(blue)” and “(red)” to avoid confusion. “Correlation” might be replaced by “comparison” here.

Fig. 6: The different temperature scaling factor of (E) should be indicated. The chosen scaling factor for (A) is not clear as it gives the isotope ratio instead of the related temperature. The latter might show a much lower magnitude in temperature compared to (B) and (C) as it is the case for (E).

Table 1: The information in the Table might be important for the core analysis but does not add for the understanding of the paper. I suggest removing Table 1.

Table 2: see Table 1. The relevant information is already included in fig. 3.

Table 3: The given information is only useful for the practical use but not for the paper.

Table 4: Might be removed. The matching of the maxima in AP should shortly be explained in the text.

Table 5: Without explanation above, the Table is not helpful.

Interactive comment on Clim. Past Discuss., 6, 2117, 2010.