Interactive comment on “Rapid shifts in South American montane climates driven by pCO_2 and ice volume changes over the last two glacial cycles” by M. H. M. Groot et al.

Anonymous Referee #1

Received and published: 8 December 2010

Rebuttal by Groot et al. 16 January 2010

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This paper presents a first and very interesting attempt of quantitative climate reconstructions of past abrupt shifts in the northern Andes. The authors used different available and up-to-date software, methods and models to reconstruct terrestrial past temperatures of a long sequence. However the conclusions of the paper lacks clarity and do not reflect the title. I suggest to improve the last part of the paper and to discuss the data in the light of the new results brought by this recombination of methods and data.

We have restructured the manuscript into an extended method, results, discussion and conclusion section.

It is sometimes difficult to separate the new datas from old publications e.g. the LGM lapse rates from the new reconstructed ones.

We have clarified the text

The authors show that "the large scale orbital-induced vegetation changes can be explained by the 100 kyr and obliquity (41 kyr) dominated glacial-interglacial global temperature…". However I do not agree with the last phrase of the conclusion "...has revealed that sub alpine climates in the northern Andes and associated ecosystems react very sensitive ..." as the paper did not explain much about the responses of the vegetation itself.

In the section on ‘Mean Annual Temperature Reconstruction’ the vegetation response is shortly explained. In addition, here seems also a misunderstanding present. In the world of climate modelling “climate sensitivity” means the quantitative change in temperature when CO2 pressure is doubled. In paleo-ecology “climate sensitivity” means that the sediment record is located in a site where small

changes in climate cause significant changes in vegetation distribution. In general this means the site is located close to an ecotone and that both biomes are well reflected in the pollen signal.

More particularly questions such as: We know about the vegetation at Cariaco but what is the vegetation composition during the Heinrich events in Lake Fuquene?

The response of the vegetation to climate change is mainly a reorganisation of the altitudinal distribution. This was developed in the classical paper by Van der Hammen & Gonzalez (1960) en since that time supported, and detailed in tens of papers. The most relevant papers have been mentioned here. The present paper deals with the AP% record, the inferred MAT, and the comparisons with marine and ice core records. The full pollen diagram (of unprecedented huge size) and its interpretation in terms of migrating and changing plant associations is the focus of two papers which will appear in a Special Issue of the Rev. Palaeobot. Palynol. These papers have their own focus which does not interfere with the focus of the present paper.

After all these reconstructions can the authors differentiate different types of altitudinal shifts?

See previous.

Are they the same for Ti-Tii , Tiii? For all the Heinrich events 8, 12, 14?

There are no Heinrich events 8, 12 and 14. The reviewer refers here probably to the DO cycles. We have better explained these different climate cycles in section 4.2 Sub-Milankovitch related MAT changes.

A large number of studies suggest that the general mechanisms and global teleconnections associated with at least the last 4 glacial terminations were essentially the same (see e.g. Cheng et al., Science 2009).

Why don't we record the other Heinrich events?

Assuming that the referee refers here to the DO cycles, we only identified the DO events with a robust appearance in the AP% record, namely the pronounced DO interstadials 8, 12 and 14. There is also variability in the AP% record between these events which might correspond to the weaker developed DO cycles, but we are not confident to identify them. Partly this may be due to small hiatuses in the pollen record and/or weaknesses in the AP% response for various reasons.

What shows the vegetation during other abrupt changes?

Altitudinal vegetation changes of various amplitude (see above)

What are the amplitudes of the shifts?

We improved the text: amplitudes of biome shifts are more clearly expressed in the section "Mean Annual Temperature Reconstructions".

Therefore I suggest to rewrite the last part of the discussion and include a more detailed and critic review of the new results obtained.

We have improved the last part of the discussion.

Interactive comment on “Rapid shifts in South American montane climates driven by p**CO**2 **and ice volume changes over the last two glacial cycles” by M. H. M. Groot et al.**

Anonymous Referee #2

Received and published: 10 December 2010

Rebuttal by Groot et al. 16 January 2010

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This paper presents a high-resolution pollen record for the last 284 kyr from Lake Fuquene in Colombia. There are only very few high-resolution long climate-related record for the continent. This new record is therefore very important because it allows comparison between tropical climate change and climate changes recorded in the Polar Regions. The paper first gives a very detailed explanation of the construction of the record. This was done very meticulously in order to get the very best from the cores. Second a chronology is carefully designed. Third the pollen analysis, in particular the percentage of arboreal pollen, is interpreted in term of temperature changes. All this work is done with great care and clearly presented. I would only suggest moving the section 4.1 on ‘Mean annual temperature reconstructions’ to the result section.

We have moved this section to the results.

Indeed the temperature reconstruction is, on my view, the highlight of the paper and the key point for starting the discussion. Some rapid variations are identified in this record. Moreover the temperature changes are compared with result from a modeling experiment and with ice records. I must say that this discussion part (including mainly the comparison) is slightly weaker than the first part of the paper. I suggest the authors to improve it.

We have added power spectra of the reconstructed and modeled MAT in the time domain and explained the results in the section 3.3 Transient climate modeling experiments. Caveats of the modeled MAT are furthermore in more detail explained in the discussion.

Detailed comments

1. Title. I do not fully agree with the title. The authors show indeed rapid shifts in South American montane climate. They also show that pCO2 and ice volume drive the low frequency part of their climate record but I do not see evidence that the high frequency part (rapid shifts) is driven by pCO2 and ice volume. Thus I encourage the authors to provide a more faithful title.

We have changed the title into: *Ultra-high resolution pollen record from the northern Andes reveals rapid shifts in montane climates within the last two glacial cycles*

2. Abstract. The abstract is not much detailed. For example, it gives the conclusions of the comparison between the new record and the modelling work but not the conclusion from the comparison with the ice core. On the other hand the same weight is put on result that are really discussed (ice volume and pCO2 driving MAT changes) and results that are only mentioned (lapse rate, local hydrology).

We have improved the abstract.

3. Introduction. I urge the authors to be more precise on the 'temperature' they are discussing, in the introduction as well as throughout the paper. In the introduction, they give an estimate of the monthly mean temperature. There is only one value. Should we assume that monthly mean value remains the same during all the year? That would mean that their (unique) monthly mean temperature is also an annual mean temperature. Please clarify in the text.

In the introduction we clearly stated that the present-day seasonal temperature cycle is weak with monthly temperatures of 13° to 14°C. This implies that the present-day mean annual temperature (MAT) is in the order of 13.5 ±0.5°C. In the remaining part of the manuscript we only compared the MAT changes relative to the present.

4. Material and methods.

It would be nice to know whether the two cores are close or far away from each other.

The cores are ten meters apart. We have added this in the text.

Some information about the lake, e.g. about sedimentation, would also be welcome.

We have added this information.

P2122 – l 6: the name 'Fq-9C' is introduced but only explained later.

We changed P-2121-14 into: for collecting >5000 samples <u>at 1 cm increments</u> for pollen and grain size analysis. We deleted P-2122-6-7: The Fq-9C ….content.

P2124 – l 9: the authors write that the composite core represents 90% of the sediment infill. Is it then correct to say that there is 10% of missing sediment? If yes, then how are the gaps identified?

A proportion of the 10% reflect small coring gaps, inadequate sediment intervals, and not analyzed parts due to organizational problems.

P2124 – l19: The word 'offset' appears twice in the sentence. To be checked**.**

P2124-19 has been changed into: The escape of methane and subsequent compaction of the peat explains the significant shift in the offset between both cores at 24-26 m (Fig. 3).

Section 3.2 is dealing with spectral analysis. I must admit that I do not understand the rationale behind this part. More precisely, the authors are first performing a spectral analysis in the depth domain. It means a strong hypothesis on the sedimentation rate. Why can they assume a constant sedimentation rate?

In accord with the comment of referee #3, we have moved part of the statistical explanations to the method section. We choose to run the spectral analysis first in the depth domain to find out if we could already identify the impact of the main astronomical cycles of precession (19-23kyr), obliquity (41kyr), and eccentricity (95-125kyr) without considering in advance large changes in sedimentation rates. Since the precession cycle was not expressed, we decided to use only the statistically significant imprint of the obliquity (~9m) cycle for tuning, implying a relatively constant sedimentation rate for the studied sediments.

At the bottom of page 2125, they discuss a Blackman-Tukey spectral analysis. I understand that they want to confirm the previous result by running different kind of spectral analysis and I fully agree with the procedure. However, the description of the parameters for the BT-analysis is far from clear.

The parameters used in the BT method are standard parameters used in the program. We have moved this description also to the method section.

They interpolate the series in time (although there is no chronology on the record yet) but the main frequency/periods are given in depth scale. This would be worth some explanation.

We have interpolated the series in depth, although the parameter name dt suggests that it is in time. This is not the case. In addition, we have changed time at P2126-16 into time (depth), and the mistakenly 200-year interpolated time series (P2125-28) into 5 cm interpolated and detrended depth series.

P 2126 – l4: I would write ‘first-order autoregressive process’ instead of ‘first-order autoregressive progress’

This has been changed.

The authors argue that they used LR04 as tuning target because it is the most commonly used. However, they are only using the obliquity component of this record. Therefore, I wonder why they couldn’t have use the obliquity record itself, taking into account a _7.5 kyr time lag (as mentioned in the paper).

In principle this is correct, but then one should argue why to incorporate a ~7.5 kyr time lag anyway. Therefore we choose the LR04 stack, also because its spectral characteristic, including the 100-kyr cycle is in better agreement with the signature of the AP% spectrum in the depth domain.

Alternatively, they could have used the simple ice sheet model on which LR04 is tuned. Their justification for using LR04 is not fully convincing. Do they think that using obliquity or the ice sheet model would lead to large differences? Should such a difference be considered as the uncertainty on the chronology?

Indeed we have considered these options, but it does not make any difference for the resulting chronology. Instead, we would have to explain the simple ice volume model and show it, which would increase the number of figures and text without adding new insights into the chronology or interpretation of the data.

P2127 (second paragraph): I do not understand why Fq-7C is used.

As Fq-2, Fq-7C is another core which top part is well constraint by 14C dates. By correlating the AP% record of Fq-9C to Fq-7C and Fq-2, using additional palynological bioevents as constraint, we increased the number of age control points for the top of Fq-9C. We have modified the old Table 4 into a new Table 1, including all tie and age control points. Note that Tables 1-3 are moved to the Supplementary.

5. Mean annual temperature reconstructions.

P 2128 – L9: I do not know what is FqBC

Fq-BC means Fq- Basin Composite. It is composed of Fq-9C (lower part) and Fq-2 for uppermost part (27 ka). We have explained this better in the text.

Several points deserve to be clarified in this section. The authors identifies several period in the temperature signal. There is a 41-kyr period, which is totally expected as long as it is the base for the tuning of the record. There is a 113-kyr, in which I wouldn't put too much confidence, as displayed in figure 7. There is an 8-kyr period appearing only at the major termination, which is actually part of a large range of periods/frequencies appearing during the major terminations. In fact, they reflect the rapid change at that time. Thus, I would suggest the authors to discuss briefly the identified frequencies in order to put forward their importance and significance.

We have changed this part of the results following the suggestions made by the reviewer: As expected on basis of the spectral results in the depth domain, wavelet analysis reveals highly significant spectral power at the glacial-bound 41 and 113-kyr periods, while a clear imprint of precession is lacking (Fig. 6). In addition, there

is a distinct 8-kyr period appearing only at the major terminations, which is actually part of a large range of periods, reflecting the rapid change at that time.

Then there comes a long discussion on what can be called the transfer function (from AP% to temperature). The authors discussed the lapse rate at present and at LGM. They come with a temperature of 3 to 5C (that would maybe deserve additional explanation).
We have clarified the text.

They call it ‘sea surface temperature’. It is in fact the air temperature reduced to sea level.
Correct, we have changed this accordingly.

All the section is rather difficult to read. It is not always easy to understand how the authors come with their estimates, in particular for the error estimate. Is it the uncertainty on the AP% measurement? Is it an uncertainty on the transfer function? Is it both? Is it something else?
We have rephrased this part of the manuscript. Error estimates include the estimated uncertainty intervals of the lapse rates and AP%.

The authors discuss a rapid temperature change of 10C but forget mentioning when it occurs and whether it is an exceptional or usual behavior.
We have added here that the 10C change occurs at terminations T_{II} and T_{IIIb} and that this is exceptional for the low latitudes and normally found only in high latitude records (e.g. Antarctic Ice core). We also added that the temperature changes of up to 7C within MIS 3 are also exceptional and are commonly only observed in Greenland ice core records.

The comparison with other records (model and Antarctic) is really too short. There is almost no explanation of what these records are. It is not explained about the validity of the comparison and its limitations.

We have improved the text on our climate modeling experiments and the comparison with the reconstructed MAT, including spectral estimates in the time domain.

P2130 (top): the same information appears in two consecutive sentences.

We have moved the basic information on the climate model and experimental set-up to the Material and Methods section and removed the repetition in wording from the text.

6. Modelling experiments.

Although the experimental settings are largely described some additional information would be worthwhile. The reference is missing for La04**.**

The reference for La04 has been added, and we have improved the text.

I assume that only CO2 and CH4 are taken into account (no other greenhouse gases). The authors carefully choose their CO2 forcing, however they do not provide information about the uncertainty (both on the chronology and on the value). Is it important in the context of the modelling experiments presented here?

As is now clearly explained, only CO2 and CH4 are taken into account. Uncertainties or deviations between different chronologies for the Antarctic ice core for the past 284,000 years are considered to be within error of the purpose of our climate experiments as well as the differences in the measured greenhouse gas concentrations between the different sites (e.g. Epica, Vostok).

It is not very clear how the ice sheet (and their evolution) is taken into account. First, I understand that only the northern hemisphere ice sheets are allowed to change. Is it correct?

This part of the manuscript is thoroughly rephrased (and partly moved) and contains now a clearer description of the glacial boundary conditions. As is now stated in section 2.7, only the Laurentide and Eurasian ice sheets are allowed to change. Greenland, Antarctica and small glaciers are kept fixed during the

simulations. Furthermore, we refer now to Weber and Tuenter (2011) who give a very detailed description of the boundary conditions and the experimental set-up. At the time of the first submission of the this paper Weber and Tuenter (2011) was still in review.

I do not have in mind the design of the grid cells in CLIMBER. Do they contain several surface types (e.g. ice sheet, land, snow)? I assume so, otherwise, how would it be possible to increase the land?

The grid boxes in CLIMBER do contain several surface types and within a grid box more surface types can occur. The land-sea distribution in EXP OIG and EXP OI differ from EXP O, especially at high northern latitudes. However, the modified land-sea distribution alone (so without the effect of ice-sheets) does not significantly influence climate at low latitudes. For that reason we decided not to mention the land-sea distribution in the revised version. The effect of land-sea distribution is in more detail described by Weber and Tuenter (2011).

Second, I do not understand the role of ICE-5G here. How is it taken into account? I understand that the ice sheet chracteristics (volume, extent) are obtained from the 3D ice sheet model from Bintanja et al.

The 3D-model only computes ice sheet volumes. Consequently, we had to translate these volumes into areas and heights. For this translation we used ICE-5G as a reference. This is now more clearly explained in the revised section 2.7 and we now refer to Weber and Tuenter (2011) for more details.

By the way, I assume that the ice-covered area in CLIMBER is set to the ice sheet extent in the 3D-model but I do not see it mentioned in the paper. Moreover the authors write that ‘only the height of the ice sheets changes in time while the areas of the ice-sheets are fixed however the ice sheet extent is (most probably) changing in the 3D model. Why couldn’t these changes be transferred to CLIMBER?

As explained above, the 3D ice sheet model only computes ice-sheet volumes.

The authors underline the importance of the variations of the albedo on the climate. They seem to strongly link ice-sheet and albedo. However the snowfield has a similar albedo. How does the snowfield extent vary during the transient simulation? In any case, I do not see why the albedo of the ice sheet should have a stronger impact on climate that change in atmospheric circulation. Could the authors give more details?

Indeed the snowfield has a similar albedo. The extent of the snowfield on the NH is somewhat larger during glacial periods but the (southward) extension is quite small (some degrees) and only occurs in boreal winter. The glacial signal in the thickness of the snow layer is much larger (variations of several decimeters). However, variations in thickness do not change the albedo of snow. Consequently, the influence of snow on the NH layer on climate at low latitudes is small. For this reason we decided not to describe variations in snow.

In order to separate the effect of albedo changes and changes in the height of the ice-sheets we did some additional experiments in which we only change the heights of the ice sheets (with fixed albedo of vegetation and soil) and in which we only changed the albedo (with fixed heights as for present-day). It turned out that heights of the ice sheets only significantly influence climate over the ice sheets itself and downstream (i.e., eastward of the ice sheets). The influence of height is negligible for climate at latitudes southward of the ice sheets. In contrast, albedo changes influence climate at high as well as at low latitudes. This is now explained in more detail in the model-section (2.7). Although the influence of ice-sheet height at high latitudes was also found in other model-studies (e.g., Yin et al., 2009), a lot of uncertainty remains about complex teleconnections associated with disturbances in planetary waves (e.g., Hoskins and Rodwell, 1995).

References:

Hoskins, B.J. and M.J. Rodwell., 1995. A model of the Asian summer monsoon, Part I: The global scale. J. Atmos. Sci., 52, 1329-1340.

Yin, Q.Z., A. Berger, and M.Crucifix, 2009. Individual and combined effects of ice sheets and precession on mis-13 climate. Climate of the Past, 5(2), 229-243.

Third, the authors display some temperature curves without explaining which temperature it is (annual mean, monthly mean, others? Is it surface temperature? At which altitude?

In Figure 7C it is given (although very small) that the temperature curves are modeled mean annual temperatures, for the grid box 115W-65W;0-10N and for an altitude of 2000 meters. This is now also given in the caption of figure 7.

Moreover, they did not indicate the grid point to which it refers and it characteristics, such as altitude. I am sorry but I do not see that H2 and H6 are affected by the lowest MAT. **This is in the reconstructed MAT, not the modeled MAT**

7 Conclusions.

I disagree with the first sentence of the conclusion. I do not see a clear demonstration of the coupling between tropical and North Atlantic climate variability, although I acknowledge some correlation, at least at the millennial time scale. Nothing is really discussed nor mentioned for the orbital time scale.

We have improved the text of the discussion and conclusions significantly.

8. References.

There is potentially a typing error in the reference Roberts et al (1987).

This has been corrected.

9. Tables and figures.

I urge the authors to improve the caption of their figures, and incidentally of their tables. Here are only some examples.

Figure 1: how is SST defined over the continent? Latitudes and longitude of the Cariaco basin does not seem to fit with the point on the map.

We have placed Cariaco Basin at the right position. Only SST's are given. Colors on land represent the heights.

Figure 4: (B) data are not only detrended but most probably normalised as well. The dashed red curve is not the filter but the filtered series. Strictly speaking, (C) is not showing a correlation but two curves. Moreover they are not clearly identified. The reader can only guess which is which. From the legend it could be guessed that the LR04 series is filtered in the 41-kyr component, which is not the case. What are the different numbers? Each curve must be identified.

The record is not normalized, only detrended. We have mistakenly added the title of the secondary y-axis to the first and vice versa. We have changed this. Numbers indicate tie points.

The other captions should be checked accordingly.

We have checked all other captions and made them clearer when necessary.

Interactive comment on "Rapid shifts in South American montane climates driven by pCO2 and ice volume changes over the last two glacial cycles" by M. H. M. Groot et al.

Anonymous Referee #3

Received and published: 16 December 2010

Rebuttal by Groot et al. 16 January 2010

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.

We have restructured the manuscript into an extended method, results, discussion and conclusion section.

Moreover, most tables are very important for the core itself but can be omitted for the understanding of the results in the paper.

Developing a composite core is an important first step of the study. The reader should be able to understand the procedure and be able to assess the quality. In particular because a suite of forthcoming papers focusing on the interpretation of

the various proxies all rely on the age model presented in this paper. Therefore, we are of the opinion these tables cannot be omitted, and suggest to move three of them to the Supplementary Information. We are of the opinion however to leave the Table showing the age calibration and tie points between cores in the main article, but we leave this decision to the editor.

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.

We have restructured the manuscript into an extended method, results, discussion and conclusion section, and included sub-sections.

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.

We have improved the text where possible.

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'

We have changed the title into: *Ultra-high resolution pollen record from the northern Andes reveals rapid shifts in montane climates 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?).

We have improved the text.

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

We have improved the text.

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.

The reviewer is right that the model cannot simulate local processes because they do not exist in the model. Our major goal using CLIMBER is to separate any influence of orbital forcing, NH ice sheets and greenhouse gases on the climate of northern South America. For that reason we had to perform three experiments (EXP O, EXP OI and EXP OIG). Also taking into account that the record is very long (284 kyr) we decided to use a fast model with a low resolution. The next step would be to use a higher-resolution model to be able to simulate regional climate. This is now more emphasized in the Discussion section. Despite the very low resolution of CLIMBER, the model is capable to capture most large-scale processes, both at high and low latitudes. For the low latitudes, this is better explained in the model section (2.7).

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.

We have improved the text.

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.

A short discussion on other (transient) climate simulations with separated forcings has been added.

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).

We have changed pCO2 into greenhouse gas forcing or atmospheric CO2. The new method section 2.7 contains a description of the model and experimental set-up of how we simulated the influence of CO2 changes on the MAT in the study region.

- 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.

We do not see why the lead lag problem is important here, because it does not matter for the interpretation of the Fuquene record if changing CO2 is the driver of the glacial termination or a positive feedback. What we know for sure is that CO2 changed drastically during glacial terminations and at least according to our model this had a significant impact on the MAT temperatures.

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.

We have changed the text to make clear that the rapid and extreme MAT changes at 2550 m elevation of up to 10 ± 2C within a few hundred of years occurred at the glacial terminations. Rapid changes that are observed during glacial periods might correlate with Dansgaard-Oeschger temperature cycles recorded Greenland ice cores.

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.

We have changed the title in accord with the suggestion of the reviewer. We condensed the introduction and made clear that the main aims of the study were (1) to establish an ultra-high resolution pollen-based record of climate change to resolve the orbital and sub-Milankovitch mean annual temperature variations in the northern Andes over the past 284,000 years, and (2) to carry out three transient climate modelling experiments to explore the significance of the orbitally induced insolation, greenhouse gas forcing and glacial-induced ice albedo feedback mechanisms on the reconstructed temperature variations. We are aware that the

resolution of the CLIMBER-2 model is limited and that the sub-Milankovitch variations are underestimated, due to its statistical-dynamical approach. We have added this short coming in the discussion.

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.

For instance, the reconstructed changes in lapse rate imply that temperature changes at high elevations are much larger on glacial-interglacial time scales than in the low lands.

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

This has been done.

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

This has been done.

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.

There is no specific reason for showing the current SST's, but is gives the reader a reference for understanding the reconstructed variations in SST of core TR163-19.

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.

We have improved the text.

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

We have improved the text.

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.

We moved the description of the model and experimental set-up to chapter 2 and improved the description of the boundary conditions. Furthermore, we refer now to Weber and Tuenter (2011) who give a very detailed description of the boundary conditions and the experimental set-up. At the time of the first submission of the present paper Weber and Tuenter (2011) was still in review.

As described above, in the introduction we also added a short discussion on other (transient) climate simulations with separated forcings.

2 Material and Methods

add “model setup” to chapter 2.

This has been done

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.

We have moved the description of the statistical methods to the method section and placed the spectral results and orbital tuning in the results section.

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.

We have followed this suggestion

2.1 Sediment Cores

Please leave out Table 1**.**

Tables 1-3 have been moved to the Supplementary.

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.

Some more information was added

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.

We have improved the text.

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?

We have clarified this part.

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.

We have followed this suggestion.

3.1 Composite Section

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

Tables 1-3 have been moved to the Supplementary.

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.

Part of the spectral analysis has been moved to the method section.

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

We have changed this into peaks with lower 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 264 kyr. For statements related to statistical significance more 100 kyr cycles would be necessary.

Yes, but taking into consideration that the past 500,000 years or so is dominated by ~100 kyr climate cycles, we may conclude that this peak in the power spectrum most likely indicates this variability.

3.3 Orbital tuning

The method might be better explained in chapter 2 in order to concentrate here on the results.

We have improved the structure of the paper.

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 tuning target, LR04, has been explained, including its orbital-driven forcing function.

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.

We have interpreted the 9m cycle as related to obliquity, because its ratio with the 22.5 m cycle (or ~ 100 kyr climate rhythms) is consistent with other studies. Hence the tuning is a logical next step to transfer the depth record to the time domain.

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.

We have restructured the paper, including the discussion, results and methods of the modeling experiments.

4.1 Mean annual temperature reconstructions

p. 2128, l. 6: Which earlier pollen based records?

We have improved the text. The improvement does not only refer to records from Lake Fuquene but even holds true on a global scale.

Same line: please remove the term 'radically'

OK

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

Text has been clarified and also we have also added a CLEAN spectrum of the AP% time series.

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.

We have reduced the number of references, where possible.

4.2 Comparison with transient modelling experiments

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

Has been done.

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".

We agree (see also below). We removed "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.

See below.

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.

As explained above, the reviewer is right that the model is not suited for simulating regional temperature variations due to the very coarse resolution. In the discussion section we explain that our major goal using CLIMBER is to separate any influence of orbital forcing, NH ice sheets and greenhouse gases on the climate of northern South America, rather than simulating regional climate. For that reason we had to perform three experiments (EXP O, EXP OI and EXP OIG). Also taking into account that the record is very long (284 kyr) we decided to use a fast model with a low resolution. Despite the very low resolution of CLIMBER, the model is capable to capture most large-scale processes, both at high and low latitudes. For the low latitudes, this is better explained in the model section.

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

La04 refers to the Laskar 2004 astronomical solution. (1,1) stands for present-day values for tidal dissipation and dynamical ellipticity. These latter has been moved from the current text.

p. 2131, l. 14: "measurements" – please use the term 'reconstructions'.

The text has been modified.

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

We have included this reference and improved the text.

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

8.

The horizontal scale could be enlarged pending of the size of publication.

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.

Uncertainty in the age model prevents us from ascribing individual events to certain DO cycles in the Greenland record. All we can say is that the overall characteristics and features appear to be very similar.

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 result 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.

There are changes in the Atlantic MOC, especially in EXP OIG. But, as explained in the text, there is no transport of water from the oceans to the ice sheets and vice versa during the waxing and waning of the ice sheets. Of course this transport can strongly influence the strength of the MOC. In addition, the CLIMBER modeling experiment does not prescribe millennial scale freshwater pulses in the North Atlantic and therefore changes in the AMOC are only driven by orbital scale variations. Consequently, we decided not to describe the MOC because the results could not be realistic. Hence, the DO related ITZC shifts cannot be observed either.

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.

We have extended the conclusion section.

Illustrations and Tables:

The figures are in general good but should be explained in greater detail (in the text and/or the figure caption).

Figure captions have been improved.

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.

Tables 1-3 have been moved to Supplementary. Table 4 has been modified and Table 5 has been omitted.

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.

Figure has been improved.

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

We have added the relevant references.

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

The caption has been modified.

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).

There is no difference in temperature scaling since they all refer to degrees Celsius.

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.

See previous.

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

See previous.

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

See previous.

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

See previous.

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

See previous.