

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

Anonymous Referee #2

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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. Indeed

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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 modelling 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.

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 $p\text{CO}_2$ 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 $p\text{CO}_2$ and ice volume. Thus I encourage the authors to provide a more faithful title.

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 $p\text{CO}_2$ driving MAT changes) and results that are only mentioned (lapse rate, local hydrology).

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.

4. Material and methods.

It would be nice to know whether the two cores are close or far away from each other. Some information about the lake, e.g. about sedimentation, would also be welcome.

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

P2124 – I 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?

P2124 – I19: The word 'offset' appears twice in the sentence. To be checked.

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

P 2126 – I4: I would write 'first-order autoregressive process' instead of 'first-order autoregressive progress'

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). 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?

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

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5. Mean annual temperature reconstructions.

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

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. Then 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). They call it 'sea surface temperature'. It is in fact the air temperature reduced to sea level. 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? The authors discuss a rapid temperature change of 10C but forget mentioning when it occurs and whether it is an exceptional or usual behaviour. 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.

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

6. Modelling experiments.

Although the experimental settings are largely described some additional information would be worthwhile. The reference is missing for La04. I assume that only CO₂ and CH₄ are taken into account (no other greenhouse gases). The authors carefully choose

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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? 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? 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? Second, I do not understand the role of ICE-5G here. How is it taken into account? I understand that the ice sheet characteristics (volume, extent) are obtained from the 3D ice sheet model from Bintanja et al. 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? 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 than change in atmospheric circulation. Could the authors give more details? 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?). Moreover, they did not indicate the grid point to which it refers and its characteristics, such as altitude. I am sorry but I do not see that H2 and H6 are affected by the lowest 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

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discussed nor mentioned for the orbital time scale.

8. References.

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

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 Carriaco basin does not seem to fit with the point on the map

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 other captions should be checked accordingly.

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