

# Interactive comment on “Rapid millennial-scale vegetation changes in the tropical Andes” by D. H. Urrego et al.

**W.D. Gosling** (Referee)

## OVERVIEW

The manuscript presented by Urrego et al. presents a reanalysis of eight fossil pollen records from the tropical Andes. The manuscript is significant because, for the first time, an attempt is made to reconcile two different approaches to interpreting fossil pollen data. The manuscript standardises, as far as is possible, the datasets from each record to examine them from the perspective of the abundance of expertly categorised pollen groups (e.g. arboreal pollen) and multivariate statistical analysis. The strength of the manuscript is in the collation of interesting and important pollen data sets from the tropical Andean region, and the standardisation of analytical approaches. The standardisation undertaken allows key features of the different analytical approaches and the palaeo-climate to be drawn out.

## MAJOR COMMENTS

1) Choice of data set for Lake Fuquene. The data set analysed from Lake Fuquene is the “Fq-2” study that covers the last c. 46,000 years at an average resolution of c. 430 years (Table 2). However, a higher resolution record does exist from this lake “Fq-9c” (Bogotá et al., 2011). Although the Fq-9c record does not reach the modern, given that this manuscript is trying to detect rapid climate change back to 60,000 years ago (Fig. 3) I wonder why this higher resolution record has not been used at least for the older part of the analysis. It would be good if the authors could clarify why the Fq-2 data have been used, or update the analysis to include the Fq-9c data (i.e. explain P. 1721, line 15-16).

>> We originally intended to concentrate on the past 60 ka, but the main focus of the paper ended up being the past 30 ka because the records available span this period or less. We have revised the manuscript to make clear that this period is the focus of the study.

The Fuquene-9C record provides high-resolution data for the internal beyond 27 ka, but is not yet available in the public domain. We realize that there was some confusion with the citation of the Fuquene dataset and have now corrected it in the manuscript. The reference should be Hooghiemstra & van der Hammen (2003) instead of Bogotá et al (2011) (Fq-9c).

2) Geographic relevance. The Figure 1 caption refers to “Northern and Central Andes” while the paper title is related to the “tropical Andes” (and elsewhere). The geographic region considered should be defined early in the paper and the manuscript would be improved if this were consistently used in the figures and text.

>> One of the goals of the paper is to test whether the signature of millennial-scale variability in the tropical Andes is consistent among northern and southern sites ( this is defined in lines 215-216). We have also included a definition of the geographic focus of the paper in the first paragraph of the methods, lines 360-362. This definition of northern and southern Andean sites is now used consistently in the text, Figures and the Table.

3) Focus on millennial scale variability. The title of the paper focuses on the millennial scale variability of tropical Andean climate. However, much of the paper discussion is related to the comparison of techniques, and the role of orbital forcing. The title could be altered to indicate the technique comparison element of the manuscript that would, I suspect, broaden the readership.

>> We have modified the title to broaden the focus of the paper: “Millennial-scale vegetation changes in the tropical Andes using ecological grouping and ordination methods”.

4) Figure 1. The concept of the 3D map figure is very nice and helps to show the altitudinal relationship between the study sites. However, the Lake Pacucha star appears to be in the wrong location and the orange circle representing the South American Summer Monsoon is unclear, maybe indicate that this is the extent of SASM for some months?

>> We have redrawn Figure 1 and now use precipitation data to depict the position of the ITCZ and SASM during January and July. The new projection shows the correct position of Lake Pacucha.

5) Can the authors get agreement from the contributing authors to deposit all data in the Latin American Pollen Database (LAPD)? If so this would be a good step forward. Even if not the case the authors should include DOI information, or LAPD data set ID numbers, so that it is clear to the reader which data have been used and which authors to contact to obtain data.

>> We will make available all the plot data developed for this paper, including ecological grouping to calculate AP% for sites where this has been done for the first time. Some datasets have already been submitted to the Neotoma Latin-American representative and await processing and numbering. We will also encourage authors of the data used in this paper to make data public.

## MINOR COMMENTS

### 1) General

a. Check for consistency of capitalisation of names and acronyms, e.g. SASM South American Summer Monsoon (Figure 1 caption)

>> Capitalization is now consistent

b. Check references, e.g. Bogota et al. (2012) in Table 1, should read Bogota et al. (2011).

>> Corrected

c. Use “-“ after “sub” consistently, e.g. section 4.3 line 8.

>> We have replaced subAndean with sub-Andean everywhere in the paper for consistency

d. I think it would be correct to capitalise names of vegetation types, e.g. Paramo (section 4.3, line 13).

>> Páramo and Puna are now capitalized throughout the paper

e. The start of each site section within Results and Interpretation would be clearer if it started with the site name, rather than “This. . .”, i.e. replace “This site. . .” with “The Llano Grande site. . .” (P. 1711, line 12).

>> Corrected

### 2) Abstract

a. P. 1702. Line 8. Indicate what the millennial scale variability is in, i.e. “climate”

>> Corrected, now it reads: millennial-scale climate variability.

### 3) Introduction

a. P. 1702. Line 13-14. The use of the word “adequate” is unclear. Do you mean that models are likely to predict future climate within a given uncertainty?

>> After revision this sentence is no longer in the paper

b. P. 1702. Line 22-23. In what sense are “paleoenvironmental reconstructions” challenged? Are you talking about the challenge of sufficient resolution to identify past climate change events?

>> After revision this sentence is no longer in the paper

c. P. 1703. Line 10. The introduction of millennial scale variability should include a clear temporal definition of the events that are being discussed (e.g. a table to mirror the illustration on the figures and references). This would help the reader to judge the ability of the records to detect these events.

>> Millennial-scale climate events are defined in the first paragraph of the introduction (lines 77-82). The timing of individual events are defined in the discussion (section 5.3) and corresponds to the bars in the Figures.

d. P. 1705. Line 8-10. I agree that vegetation change is an “internal response of the climate system”; interesting additional discussion on this theme could relate to the different sensitivities of different vegetation types (at different locations) to climate change.

>> This is a good point but we feel we can discuss only the sensitivity of different vegetation communities in the tropical Andes (included in section 5.3).

### 4) Environmental setting

a. P. 1705. Line 22. Replace “unmarked set by Dunia” with a reference (or clarify what is meant).

>> Corrected

### 5) Methods

a. P. 1707. Line 26-27. Define what is meant by “mid- and high- elevations”.

>> We have provided an altitudinal interval for mid and high elevation in line 355, methods section.

b. P. 1708. Line 18. Define what is meant by “relatively short”.

>> This sentence has been rephrased

c. P. 1708. Line 23. I am not sure what you mean by “operating mechanisms”? Please clarify.

>> After revision this sentence is no longer in the paper

### 6) Results and Interpretation

a. P. 1711. Line 4. Changes could be synchronous and opposing.

>> After revision this sentence is no longer in the paper

b. P. 1711. Line 16. The arbitrary direction of DCA z-scores should be explained so that the ability to reverse values is clear.

>> This aspect is discussed in section 5.1 and lines 809-829.

### 7) Discussion

a. P. 1716. Line 17. The start of the discussion picks out an issue with the use of RoC analysis linked to a supplementary figure. As these results have not been presented then this is a bit of a surprise for the reader to suddenly come across a statement saying that they have not been used. I suggest that either this figure is moved into the main document if perceived to be important, or the discussion is moved to a less high profile section of the manuscript.

>> We have moved the discussion of the RoC to another part of the introduction

b. P. 1720. Line 3. Discussion of timings should include discussion of the robustness of chronologies. Especially where distinctions are made on relatively short events, e.g. cooling 13-11 ka (line 7) and short lived cooling 13-11.5 ka (line 10).

>> We have included several caveats on the temporal resolution of individual records and chronological uncertainties in the methods (lines 396-404) and in the last paragraph of the discussion.

## **8) Figures**

a. Figure 2. Plot on 60 ka timescale like Fig. 3 and 4.

>> We have amended the focus of the paper to the last 30 ka, and have replotted the figures accordingly.

# Interactive comment on “Rapid millennial-scale vegetation changes in the tropical Andes” by D. H. Urrego et al.

S. Metcalfe (Referee)

This paper by Urrego and co-authors goes back to eight published pollen records from the tropical Andes (between 6o29’N and 16o20’S) and reinterprets them using both ecological groupings (with a particular focus on AP%) and ordination scores from DCA. The DCA axis scores are also used to estimate rates of change (RoCs). Except for the case of Llano Grande in Columbia, the authors have used the published age models. The paper is set out to look at millennial scale variability, specifically Heinrich stadials and what are called Greenland interstadials (D/O interstadials). In practice, however, the paper says more about the stadials (including the YD) than the interstadials, pre- sumably because only three of the records actually extend back beyond 25 ka.

I think that this is an interesting paper, but have concerns about the emphasis in the text on moisture sources when in fact most of the responses seem to be driven by temperature.

>> We have reshaped the manuscript to emphasize the air temperature signal extracted from the records. The paper now focuses on the last 30 ka and discusses both the signature of HS and GI as far as the chronologies allow.

## MAJOR COMMENTS

Environmental setting. I found this quite confusing in places in terms of a) modern climatology (linking to Fig. 1) and b) setting. a) The description of this in section 2 and Fig. 1 need to be linked together more effectively.

>> The environmental setting section now includes a part on vegetation, and we have revised it to link more effectively different aspects of climate and environmental variability. We have also modified Figure 1, including two panels to illustrate the seasonal variability of atmospheric systems.

At a basic level Fig.1 has no latitudes or longitudes marked on it.

>> The Figure now includes coordinates.

It isn’t clear whether the LLJ that is marked on the figure is the South American Low level jet referred to on p. 1707 (presumably it is).

>> We have replaced the term Low Level Jet with South American Low Level Jet (SALLJ) in the text.

The Choco Jet referred to on p. 1706 isn’t shown on the figure at all. The ITCZ is shown in only one location, with no indication of its seasonal shift and the label for the SASM seems to be located a long way to the east.

>> We have modified the Figure and now include average monthly precipitation data for the period 1998 to 2007. The Figure also includes one panel for the boreal summer and one for the boreal winter. We think this solves potential issues with the incorrect location of SASM and ITCZ. The approximate locations of the Choco Jet, SALLJ and ENSO are depicted in the Figure.

The text on cold front outbreaks (p. 1705) is rather confusing as it refers to the impact of these fronts from both north and south, but then reports that cold fronts (from

which direction?) can make a significant contribution to summertime (SH or NH?) precipitation in western Amazonia. It would be helpful to clarify this.

>> This refers to southern-hemisphere cold fronts and it is now clarified in the text, line 248.

P. 1706 line 9 – based on Fig. 1 it isn't clear how the Choco Jet affects the western flank of the Andes, this feature really needs to be added to Fig. 1. It will then make sense.

>> We have added the approximate location of the Choco Jet to the Figure.

In the text on Millennial scale variability, trends and climatic mechanisms it would be good to clarify the relationship between the ITCZ and the SASM (see top of p. 1704).

>> The potential link between ITCZ and SASM is mentioned lines 305 and 307.

The signature of stadials in Cariaco and the northern sites in this transect is dry. b) The text contrasts sites in the northern and central/southern Andes (p. 1706) but this categorisation isn't shown in Table 1 or Fig. 1. This could also be picked up in the first paragraph of 3 Methods. In this paragraph I would help the reader by inserting a bit more explanation in line 8 e.g. 'Lakes Chochos, Pacucha and Consuelo also lie on the eastern flank of the Andes, but further south'.

>> Table 1 now includes a categorisation of northern, central and southern Andean sites. We have also added a sentence to the first paragraph of the methods, lines 357-359: "For the purpose of this paper, sites are classified according to their latitudinal position into northern (latitude north), central (latitude < 10°S) and southern (latitude > 10°S) Andean sites (Table 1)"

What is meant by mid- to high-elevation in this context? (p. 1704 last line, 1705 top line).

>> We have clarified this aspect in line 355

Methods. I'm afraid that I am not convinced by the use of a ratio between aquatic pollen types classified as deep water taxa and shoreline/shallow water taxa, to reconstruct lake level change. The authors record that the Holocene D/SS is nearly zero for Lake Titicaca when it has been shown that there was a major drop in lake level in the early Holocene followed by recovery (e.g. Baker et al., 2001). Given their own caveats about the D/SS ratio at the top of p. 1719 and the fact that there are often more reliable lake level indicators for the sites discussed in this paper, I suggest that the authors think carefully about removing this element.

>> We have retained the D/SS ratio because we consider that it provides meaningful environmental information despite the caveats. We have added a more clear explanation for the lack of change in Lake Titicaca in lines 739-743.

Although lake level/moisture balance is referred to in Section 5.2 (Orbital scale environmental changes), it isn't referred to very much in relation to millennial scale change. If the authors do want to retain this, then they need a clearer definition of what is meant by shallow and deep (p. 1710, line 6) and make more use of other water balance indicators.

>> Unfortunately water balance indicators like diatoms are not available for all the records, as a result the aquatic vegetation is the best alternative to compare potential moisture availability changes among sites. We have added more information on the

classification of shoreline and aquatic taxa in lines 452-455.

Discussion. The anti-phasing of responses (in relation to moisture balance) to both Holocene orbital forcing and to millennial events between the northern part of tropical South America and the southern part is quite well established. The possible east – west variation suggested by Cheng et al. (2013) based on their speleothem work is a newer idea. If the authors wish to continue to make this a focus of their paper, then these complex patterns warrant more discussion and could be linked much more closely to moisture sources (see above).

>> Our north-to-south network of sites does not allow conclusions about east-west gradients of moisture/precipitation. However, we have included some inferences of north-south anti-phasing of moisture availability during the Pleistocene-Holocene transition (lines 946-951).

Conclusions. These say nothing about warm events (your Greenland Interstadials). I'm not sure there is that much to say given the records available, but they seem less consistent. See previous comments about moisture sources.

>> We have added more discussion on the Greenland interstadials, in particular GI1 (lines 993-1006). The conclusions and abstract also include information on GI1.

#### MINOR COMMENTS

Abstract. See comment above re moisture sources. I do not see this as the emphasis of the paper.

>> The revised manuscript now emphasizes the role of temperature rather than moisture source.

p. 1703 lines 13 and 14 add some non-ice core references

>> We have added non-ice core references

p. 1707 line 7 Do you really mean eastward transport of Amazonian moisture?

>> We have rephrased

p. 1708 The text on age models isn't very elegant. Can you re-phrase?

>> We have rephrased

p. 1709 line 1, insert 'the' before 'original authors'; line 7, insert 'a' before 'pollen taxon' p. 1710, line 13 aimed

>> Corrections incorporated.

p. 1713, line 2, study based on what?

>> Based on inverse modelling. This has been included in lines 550-551

p. 1714, line 3 What do you mean by high resolution in relation to the Chochos record?

>> We have rephrased

p. 1716, line 19 difference; line 20 insert 'the' before La Cocha

>> Corrections incorporated.

p. 1718, line 25 can you suggest some specific proxies? Leaf waxes? GDSTs?

>> Examples included in Line 1238

p. 1721 line 9, please check whether the YD has been mentioned previously; line 25 give the date for G1

>> [Corrections incorporated. Timings of GI and HS have been added to the discussion.](#)

p. 1722 line 17 either 'directions differ' or 'direction differs'; lines 19-21 contrast with speleothem records from the east.

>> [Rephrased](#)

Interactive comment on *Clim. Past Discuss.*, 11, 1701, 2015.



# Interactive comment on “Rapid millennial-scale vegetation changes in the tropical Andes” by D. H. Urrego et al.

**Anonymous Referee #2**

## OVERVIEW

The present manuscript reports a reanalysis of some of the most important records for the tropical Andes, in West of South America, and focus on the vegetation changes detected at sub-millennial scale to study environmental variability. Given the uncertainty of the research question debated and the novel approach, the authors have nicely explained the objectives of the present work and the advantages compared to previous attempts, as have discussed the potential problems of the techniques used. Regarding the objectives proposed however, the paper ends a bit shallow in its present form, lacking further discussion about the meaning of the results found, i.e., the potential drivers that have caused synchronicities/asynchronies between the records.  
>> [Discussion.](#)

## MAJOR COMMENTS

Abstract. The inference of precipitation changes (line 7) based on aquatic and shoreline vegetation is a bit risky as shifts in these taxa provide very local scale information and may be related to different drivers including precipitation, but also temperature through an increase in evaporation. I suggest the use of different terms such as moisture availability, P/E balance or similar throughout the text.

>> [This is indeed a very good point that we had tried to address in the methods and discussion of the D/SS ratio. To avoid overstating the results we have now changed “inferred precipitation changes” from aquatic taxa to “moisture availability” and “lake level changes” throughout the text.](#)

Environmental setting.

Section 2.1 is entitled “Geography, vegetation and climate” but I haven’t found any information about vegetation so far. Although the study area will imply large variation of the taxa occurrence and distribution, some basic information is required that will help the readers not familiarised with the tropics.

>> [We have added a paragraph on Andean vegetation to the Environmental setting section.](#)

Methods.

1) Although the use of AP% as proxy for temperature shifts has been explained, some clarification would be appreciated. This proxy is especially useful in high steep locations (mountain range) that includes a close ecotone between a forested and a non-forested plant community. This would be the case to some extent for the seven Andean records, but please clarify why using AP% in Lake Consuelo should work taken into account that puna is located almost 2000 m upwards and changes in communities promoted by temperature shifts might be unnoticed by AP%.

>> [We clarify that AP% in Lake Consuelo is less sensitive than in high-elevation lakes sitting closer to the ecotone. We have added a section on the discussion on the low sensitivity of AP% in mid-elevation sites \(Lines 967 to 992\).](#)

2) Given the data showed in Table 1, there are some records without a very high resolution. This might be problematic for comparing the level of details that for instance

La Cocha record is going to provide. Please provide further details in how you are avoiding these potential issues.

>> We state that our regional comparison is constrained by the differences in temporal resolution among records (Lines 391 and 397). We consider that this issue is unavoidable and therefore refrain from drawing conclusions beyond the chronological resolution of individual records. The discussion also includes caveats on this, e.g. lines 1197-1198.

Table 1. Please add the number of radiocarbon dates obtained in each record for building the age-depth model.

>> The number of radiocarbon dates have been added to Table1.

Figure 1. Please check the right location of Lake Pacucha and re-draw SASM (maybe as a shade or with bars?) to clarify the real extent of the atmospheric pattern and include the season for the ITCZ etc.. Some coordinates would be much appreciated.

>> We have redrawn this figure and it now includes two panels of average precipitation during January and July depicting the position of the ITCZ and the development of SASM. The Figure includes coordinates.

Figure 2. Where are the lowland taxa? Were not important at all, including in Lake Consuelo?

>> We chose to show the sub-Andean and puna taxa for Lake Consuelo to allow comparisons with other sites. The Puna taxa in Lake Consuelo are also more sensitive to temperature change as they relate to the position of the upper forest line. This is now clarified in section 4.7.

#### SPECIFIC COMMENTS

Page 1704, lines 25-26: I would rephrase this including some potential mechanisms responsible for the lack of consistent signature found to date in time and/or space (differences in analysis resolution, proxy sensitivity, climate system operating. . .).

>> After the revision of the introduction this sentence is no longer in the text.

Page 1705, line 22: Please change “unmarked set by Dunia” for a proper reference.

>> Removed

Page 1712, lines 7-10: Please include the reference for the elevation of subAndean forests in the interglacials (or how they obtained the information).

>> This information was obtained from the original publication of the pollen record from Lake Fuquene. We have added a citation to support the statement.

Page 1713, lines 3-4: This sentence is saying just the opposite of the previous one (page 1712, lines 26-27). Please, clarify.

>> This information was indeed conflicting. The idea has been clarified.

Page 1713, line 11: There is a typo in Surucucho.

>> Corrected.

Page 1714, line 3: Please define “relatively high resolution” taken into account the record data (almost 300 years of sampling resolution).

>> We have removed this idea as it was an overstatement.

Page 1718, line 20: Please see comment on Methods above.

>> Clarified above.

Page 1721, line 11: Please avoid the term “precipitation changes” based on aquatic taxa, it will promote misunderstandings (despite you might be right in some cases, but this proxy cannot provide this type of evidence).

>> We have replaced ‘precipitation changes’ with moisture availability.

Interactive comment on Clim. Past Discuss., 11, 1701, 2015.