Clim. Past Discuss., 8, C2355–C2361, 2012
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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "The Medieval Climate Anomaly and the Little Ice Age in the Eastern Ecuadorian Andes" *by* M.-P. Ledru et al.

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Received and published: 14 November 2012

Reply to Referee 2

1. The presentation and discussion of the tephra geochemistry (Section 3, 1st para Methodology; Section 3.1 Sediment description, trace elements) is erratic in the context of this manuscript and is not further used in the Interpretation/Discussion or Conclusion. Should be removed.

The presentation and discussion of the 3 tephras observed in the sediment core PA 1-08 are part of the sediment description and have been used to improve our chronology. We think it is important to show why we are sure about their origin in the context of the published studies on volcanism in our study area and how we got the age that we

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mention in table 2. Moreover these tephra enabled us to improve the accuracy of our chronology. They are not used in the discussion because they were used to assess our age model.

2. The chronology: This part is largely confusing and its presentation is below standard (text, Table 1 & 2 and Fig. 3). 14C dates seem to be ok although the sample at 22-24 cm sediment depth seems enigmatic (or its presentation). Does "0 14C yr BP" correspond to 100 pmC (i.e. AD 1950; so why AD 2000?) or is there evidence for bomb 14C? If not it seems to me that a sedimentation rate of 22 cm during the last 50 is very high. A comment regarding sediment compaction (?) or bulk density (?) should be made.

We agree that this part was confusing and made changes in the text and tables 1 and 2. Sample 22-24 cm gave a pMC of 102.62 $\pm$0.26 equivalent to 1950 AD; We made the correction on table 1. We showed the radiocarbon ages used by the volcanologists on table 2.

3. The description of how the chronology was made (linear interpolation) is very confusing. It needs a detective's work to combine the information in the Fig captions and tables and text to find out what you actually did. The approach you have chosen is quite outdated; there are much better models (e.g. Blaauw 2010, spline models and Mixed Effect Models, Heegaard), where tephra can be included. Judging from the naked eye I would guess that your linear interpolation (regressions in caption of Fig 3) is even outside the +- 1 sigma range of the 14C ages. This is significant. Fig. 3: The final age model should be shown (line); also the age errors (!), distinguish between tephra ages and 14C ages (symbols), show the tephra layers; there is likely a typo : the uppermost segment for interpolation ranges likely from 115 – 0 cm (and not 15 – 0; line 3 of the caption).

A new age model plotted with BACON (Blaauw and Christen 2011) shows the age errors, the tephra layers, the calibrated distributions of the individual dates and the

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95% confidence intervals. The age model is based on 5 radiocarbon dates obtained from bulk and 20 published radiocarbon dates for the tephras (see below). M1 and M2 eruptions are well identified and dated and only the age adopted by the volcanologists is reported in table 2. The eruption that corresponds to tephra M3 lasted 200 years with several eruptive events and we were not able to define which event did produce the tephra M3 at Papallacta. Therefore we decided to use all the published radiocarbon dates obtained by the volcanologists around the volcano Guagua Pichincha and to plot them on our age model. The resulted age of 1520 AD is obtained from our age model and represents the best compatibility with the other dates. Table 2 presents the radiocarbon dates of the tephras used in our age model.

Lab Material 14C age Calibrated age*

Beta analytic Carbonized wood 840±50 1147-1274 AD (676-803 BP)

USGS-M Carbonized wood 900±150 854-1320 AD (630-1096 BP)

USGS-M Carbonized wood 1150±300 322-1405 AD (545-1628 BP)

Teledyne isotopic Paleosol 820±80 1030-1291 AD (659-920 BP)

Beta analytic Charcoal 770±40 1205-1289 AD (661-745 BP)

Beta analytic Charcoal 780±40 1182-1284 AD (666-768 BP)

Radiocarbon ages obtained for the last eruption of the Quilotoa (from Hall and Mothes 2008) * 2 sigma >75%

Lab Material 14C age Calibrated age*

GRN30187 Charcoal 930±60 1012-1221 AD (729-938 BP)

GRN30189 Charcoal 1020±25 974-1040 AD (910-976 BP)

GRN25513 Charcoal 1100±40 866-1021 AD (929-1084 BP)

GRN25515 Charcoal 1100±90 762-1052 AD (989-1188 BP)

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GRN24776 Charcoal 1120±30 862-994 AD (956-1088 BP)

GRN25809 Charcoal 1180±30 772-900 AD (1050-1178 BP)

GRN26206 Charcoal 1260±70 649-897 AD (1053-1301 BP)

Radiocarbon ages obtained for the 10th century eruption of the Guagua Pichincha (from Robin et al 2008) * 2 sigma >75%

Lab Material 14C age Calibrated age*

GRN25522 Charcoal 240±20 1642-1797 (153-308 BP)

GRN 25521 Charcoal 290±20 1520-1653 (297-430 BP)

GRN 25517 Charcoal 320±20 1491-1643 (307-459 BP)

GRN 32951 Charcoal 330±30 1477-1642 (308-473 BP)

GRN 32953 Charcoal 330±30 1477-1642 (308-473 BP)

GRN 25508 Charcoal 335±20 1483-1637 (313-467 BP)

GRN 25519 Charcoal 450±40 1407-1513 (437-543 BP)

Radiocarbon ages obtained for the 17th century eruption of the Guagua Pichincha (from Robin et al 2008) * 2 sigma

4. Also the question arises whether there is a sediment hiatus between 120 and 140 cm sediment depth. The apparently very low sedimentation rate in this segment of the core is striking. I think the chronology can be improved and fixed.

We have 5 AMS dates and 3 tephra horizons to cover 200 cm and the past 1000 years. The lower sedimentation rate is reflecting change in sediment deposition and the drier climate. The 130-115 cm interval, 15 cm, 300 years, is well constrained by 3 different dates: one radiocarbon date within the interval (530±30 BP), bracketed by 2 dated tephras. In addition the 7 pollen spectra of the interval are homogeneous in

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pollen content. Therefore we did not consider the hypothesis of a hiatus for this zone. Nevertheless this is an important remark and we will add a comment in the text.

5. Discussion: I am not at all convinced about the discussion about the climatic changes and their dynamic links (causes?) with Atlantic, Pacific SSTs and ENSO. The sentence on Page 4316 line 25 brings it to the point: ": : : teleconnection mechanisms are still poorly understood." One can claim just about everything depending on the ENSO record(s) one selects and chooses to make a case. E.g. D'Arrigo et al 2005 don't see hardly any change for the past 600 years, there is ample evidence for non-stationarity in the teleconnections (Timm et al 2005) and so on. Rein et al 2005 (Paleoceanography) report the most prominent regime shift around AD 1250 (coincides with your data), but nothing is found in the 18th or 19th century. Also Gergis et al 2006 (and Gergis & Fowler 2009) do not find any change change since 1700 AD. I agree with the 'remarkable coincidence' (Fig 6) for the 13th century, but I don't see this for the rest of the time series (Fig 6 A and C). The entire Discussion related to ENSO, and long-distance teleconnections in the Atlantic / Pacific SSTs (Section 6) seems to me much too speculative and should be substantially lowered in the profile (or even removed). One could take other ENSO records for comparison and would likely draw different conclusions.

We understand the arguments given by referee 2 and we reinforced the discussion in our paper to better explain our results. Referee 2's last sentence (above) is very important and shows exactly how little we know about ENSO. This issue was clearly observed in modern data analyses by Garreaud and Battisti 1999. ENSO at interannual scale represents what we observe today when for example extreme events along the South American coast are observed. However when dealing with sediment cores we rather refer to interdecadal variability which shows different patterns of climate variability (please see fig.1 in Garreaud and Battisti 1999). In this study we are commenting the effect of an interdecadal effect of ENSO variability (or ENSO-like) on andean ecosystems. Therefore for this study we selected the El Junco record to discuss changes

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in equatorial Pacific SST because it is a sediment core located at the same latitude than our pollen record. We showed here that regional effects of global change must be considered at smaller spatial scales. None of the cited papers by referee 2 are located in the eastern ecuadorian Andes. Vuille et al 2012 showed that records located on the eastern cordillera similarly responded to changes in the intensity of the SASM. However the Papallacta record showed different patterns with a dominant forcing of the Pacific SSTs before 1500 AD and a dominant forcing of the Atlantic SSTs after 1500 AD which impacted significantly the dynamic of the glaciers of the last millenium. The time scale covered by the different proxies is important to consider when dealing with ENSO. For instance D'Arrigo et al 2005; Gergis et al 2006 2009 approaches are all based on tree-ring analysis at an interannual scale which is definitely not the time scale covered by our pollen record. This is the difference between ENSO and ENSO-like or ENSO-variability. The last GCM IPSL model for the past 1000 years showed that the dry episod that started during the 13th century was induced by large scale volcanism at the equator able to maintain cooler SST on the equatorial Pacific during 300 years (Myriam Khodri personal communication, paper in preparation). This is why the study by Rein et al based on marine sediment strongly recorded this effect. However the end of the dry episod is coinciding with stronger SASM when the Pacific SST became progressively warmer and a different climate forcing not observed by Rein et al must be inferred. The following paragraph was added to discuss these different interpretations and concepts around interannual ENSO variability, interdecadal ENSO-like and ENSO variability.

"The terms La Niña-like or El-Niño-like have been broadly used to refer to higher La Niña or El Niño frequencies (e.g. Sachs et al., 2009) and ENSO variability to the occurrence of a number of ENSO events within a time interval (e.g. Cobb et al., 2003). However we know for instance that during the LIA the glaciers expanded in the tropical Andes which is not coherent with an increase of El Niño events which according to modern observations would favour a regression of the glaciers (Jomelli et al., 2009). Garreaud and Battisti (1999) associated the term El Niño-like to longer period of variability with major effects on the patterns of seasonality. Consequently in this study to avoid misunderstanding with the interannual ENSO variability and the paleo El Niño/La Niña-like of the equatorial Pacific we will rather refer to "ENSO variability" to discuss changes in equatorial Pacific SST at interdecadal scales in the sense of Garreaud and Battisti (1999). "

6. Others What exactly do you understand with ‘high/low ENSO variability’ (amplitude, frequency, phase lock)?

Please see the above discussion

Page 4298, Line 12-13: why restricted to a particular season? Corals e.g., are all year round period, epoch?

We changed for " short particular intervals"

P4299 L 8 : seasonality of what?

the interannual seasonal climate

P4304 L 24: check the logic of this sentence. Isn’t it the other way round?

This paragraph was entirely rewritten

P4312 L 19:reference needed

Jomelli et al 2009

Interactive comment on Clim. Past Discuss., 8, 4295, 2012.