

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

### **Anonymous Referee #2**

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The Ledru et al manuscript presents a millennial long relatively highly resolved (average resolution 14 yrs) pollen-inferred humidity and convection record for the Eastern Ecuadorian Andes.

The topic of the manuscript is very suitable for CP and covers an area where such information is largely missing up to now. The topic is also very timely in the light of the IGBP-PAGES 2 k initiative. I find this very interesting.

The strength of the manuscript is in the pollen data set (modern pollen rain along an altitudinal gradient with surface samples, downcore measurements of fossil assemblages) and its interpretation. This is state of the art and the interpretation of the results seems scientifically sound and consistent. The results are important.

The manuscript has three weaknesses: 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.

2. The chronology: This part is largely confusing and its presentation is below standard (text, Table 1 & 2 and Fig. 3).  $^{14}\text{C}$  dates seem to be ok although the sample at 22–24 cm sediment depth seems enigmatic (or its presentation). Does “0  $^{14}\text{C}$  yr BP” correspond to 100 pmC (i.e. AD 1950; so why AD 2000?) or is there evidence for bomb  $^{14}\text{C}$ ? 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. 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 The approach you have chosen is quite outdated; there are much better models (e.g. Blaauw 2010, spline models and Mixed Effect Models, Heegaard), where tephra can be included.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  $\pm 1$  sigma range of the  $^{14}\text{C}$  ages. This is significant. Fig. 3: The final age model should be shown (line); also the age errors (!), distinguish between tephra ages and  $^{14}\text{C}$  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). 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.

3. 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

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GRL 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. I am absolutely happy (and this is the most valuable scientific contribution of this work here) is a solid, very well made and robust reconstruction for the Ecuadorian Andes. Speculations should be removed.

Major revisions.

Detailed comments (a selection): What exactly do you understand with 'high/low ENSO variability' (amplitude, frequency, phase lock)? Page 4298, Line 12-13: why restricted to a particular season? Corals e.g., are all year round P4299 L 8 : seasonality of what? P4304 L 24: check the logic of this sentence. Isn't it the other way round? P4312 L 19: reference needed

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