Review of

"Putting the rise of the Inca Empire within a climatic and land management context"
by A.J. Chepstow-Lusty, M.R. Frogley, B.S. Bauer, M.J. Leng, K.P. Boessenkool, C. Carcaillet, A.A. Ali, A. Gioda
for Climate of the Past

Topics:

This article intends to show that rapid expansion of the Inca from the Cuzco area would not have been possible without establishment of favorable climatic conditions. It is based on anthracological and palynological studies previously published (Chepstow-Lusty et al., 1998, Mountain Research Development; Chepstow-Lusty et al., 1996, Antiquity; Chepstow-Lusty et al., 2003, Journal of Quaternary Sciences) to which some geochemical indices are here added.

General comments:

The paper is well-written and relatively easy to read, even if I would have preferred to gather « results » and « discussion » to save some repetitions.

Interpretations based on palynological and charcoals appear to me as robust but I'm not a specialist. In contrary interpretation of geochemical records ($\delta^{13}C$ on bulk organic matter and C:N ratios) is very weak and superficial. Surely authors are not specialist of organic geochemistry and had to interpret by themselves an (too) intricate signal. Some errors with "high" and "low" (by the way, geochemists rather talk about "heavy" and "light") tend to illustrate that authors are neophyte in isotopic geochemistry. $\delta^{13}C$ on bulk organic matter are regularly used in palaeoenvironmental reconstructions along with C:N ratios. However in lake sediments, organic matter is derived from multiple sources, land plants, aquatic organisms, microbes; by using $\delta^{13}C$ TOC values alone it is impossible to say whether algae, macrophytes or terrestrial plants were driving the isotopic signal. In lakes, the C:N ratio must also be used with caution as cells of the very common green alga Botryococcus braunii have very high C:N ratios and can be misinterpreted as originating from terrestrial plant. Likewise specific degradation that would preserve carbon with respect to nitrogen (e.g. carbohydrate sulfurization, …) or "half-completed" early degradation can lead for misinterpretation. All of these limitations should be at least mentioned and undoubtedly discussed in the context of the study. With so high amount of organic carbon that reaches 30%, specific degradation, anoxia

and so on have to be both considered and discussed. In this manuscript, the presentation of organic geochemical indices greatly questioned their interest within the context of the study.
I was not able to get previous papers published in Antiquity and Mt. Res. Dev., I only read this one published in JQS. The present paper submitted to Climate of the Past is a detailed interpretation of part of "Zone m-3" and "Zone m-4" mentioned in the JQS paper. JQS paper effectively covered the last 4,000 yrs.
Based on this solely lecture, I'm not sure that the present manuscript contributes to important advances to the here considered scientific issue. Nevertheless a detailed study is always a added value if this is supported by coherent indices. In this case, everything but geochemical appears as having a stand-alone value whereas d13C and more exactly interpretation you attempt to do is definitively a weak point of this paper. Briefly, two possibilities: remove geochemistry from this paper or discuss it with a geochemist.
=> major revisions.

Detail:

- **Title:** The title is well-chosen. It's highly more attractive that would be a simple "Paleoenvironmental changes over the last millennium in the Cuzco area"

- **Abstract:** effectively reflect the title and the paper

- **Introduction:**
  - Historical and geographical contexts of the study are well exposed.
  - I would like to find some points on the strategy. "New proxy" (p. 773, l. 30) is definitively too short to present the paper.
  - And on the added-value of this paper to the previously published.

- **Site selection**
  - a little bit too long.

- **Methods**
  - P. 776, l. 22: is the 0.1% relative or absolute precision? Is it for C:N or %TOC or %N?
  - Add a subtitle for "charcoal analysis". It should not be included in "organic matter geochemistry" paragraph. Please refer to Figure 5.

- **Results**
  - As already expressed in "General comments", I would prefer to have results and discussion gathered. The presentation you choose to divide your interpretation according to the predefined zones constrains the reader to constantly jump from results to discussion and from discussion to results.
  - I'd be pleased to find data in a table (supplementary data?) … figures are so small and intricate that it's really difficult to see exactly what happened and to define any threshold. I'm thinking about C:N ratio that always seem to be higher than 15 but I'm totally unable to constrain its variability. C:N ratio >15 would indicate at the order 0 that you have a majority of terrestrial input in your lake.
  - P. 778, l. 10: where do you see a decrease of d13C? I only see a rapid decrease at ca 880-920 but no way along the whole AD 1100-1400 period. If the zones you defined fit relatively well with floral records, that's not the case for d13C nor C:N and according to me, nor for micro-charcoals abundance, nor for charophytes… I'm not sure you choose the most efficient way to define the zones in such a multi-approaches study.
  - P. 778, l. 13: which kind of biological indicator do you use to assert "a critical threshold for biological activity appears…"

- **Interpretation and discussion**
  - P. 779, l. 16: why do you argue that lower %TOC suggests greater erosion? How can you rule out that is not linked to a lower organic production? What about the age-model, does it effectively show an increasing sedimentation rate associated to the lower %TOC that would act in favor of inorganic input and thus higher erosion?
  - P. 780, AD 880-1100: C/N doesn't at all mirror d13C. I guess that when you're talking about "high C/N, low d13C", you're focusing on the early beginning of the period: AD 880-920 during which you have a spike in both C:N and d13C. What about the end of the period?

    Can you here consider two subzones: 880-960AD and 960-1100 AD? 880-960 AD: increase of d13C, lot of cheno; 960-1100 AD: less cheno, lot of myriophyllum, decrease of d13C, increase of N.
  - P. 781, l. 10: what is the link you invoke to tie decrease in TOC and erosion increase?
  - P. 781, l. 13: do you wish to mention soil OM maturation when you associate high C:N and increase of soil input?
  - P. 782, l. 4: you mention both "decline in d13C" and "increased agriculture (including maize)" BUT maize is C4 plants and increase of maize should result in increase of d13C… not the reverse!
  - ….. and so on.

- **Conclusion:**

- **Bibliography:**

- **Table and Figures**

They are all very small and not easy to read. This should be checked for a future potential publication.

  - Table 1: please respect 14C conventions and present 14C ages as they should be. 14C ages are effectively under xxx ± yy form with the precision of 1 sigma. Calibrated ages can't be shown under the same format since the calibrated interval doesn't show a Gaussian distribution with the most likely value in the middle. Provide the 2 sigmas calibrated interval with the associated probability. As last column, you might show the "median" of 14C age that is the most probable value.
  - Figure 1: I'm not able to situate the bottom panel in the right upper one.
  - Figure 2: nice view… is it really useful?
  - Figure 3: no interest
  - Figure 4: please gather indices according to what they represent.
  - Figure 5: I would like an extended legend… What are the red and black lines in panel d? What are red lines in panel b? what is the meaning of N_{FRI}? … and so on!

    I'm not sure that all of these panels are useful … as a proof, you don't refer to all of them in the manuscript.