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Climate of the Past Discussions



# *Interactive comment on* "Putting the rise of the Inca Empire within a climatic and land management context" *by* A. J. Chepstow-Lusty et al.

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Reply to first reviewer

We thank the reviewer for a series of useful observations and comments, most of which relate to the organic geochemistry aspect of the paper. Where appropriate, changes will be incorporated into the revised manuscript, as detailed variously below. Reviewer comments are here dealt with in turn (where appropriate, these are repeated in bold).

General comments

We are pleased that the reviewer considers our paper to be well-written and easy to read. We deliberately chose not to mix results and interpretation sections; the concise reporting style adopted reflects a conscious effort to avoid repetition.

“. . . interpretation of geochemical records (ïĄď13C 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.” On the contrary, several (three) of the co-authors are entirely familiar with the interpretation of organic geochemical signals in lacustrine settings, each having published widely on these matters. This paper is not intended to be a specialist isotopic/organic geochemistry paper written with a specialist geochemist audience in mind. Accordingly, whilst always mindful of the complexities that working with such systems involves, we have endeavoured to present this element of the study in as clear a way as possible.

“. . . by the way, geochemists rather talk about ‘heavy’ and ‘light”’ In many cases this statement is, of course, true. However, when discussing palaeo-lacustrine settings, convention in the literature is to refer to higher or lower values (which is undoubtedly easier for the non-specialist reader to understand).

“However in lake sediments, organic matter is derived from multiple sources. . .” The authors are fully aware of the complexity of the sources of organic matter in lake sediments and the likely effect those sources might have on ïĄď13C values. We deliberately discuss the interpretation of ïĄď13C values in conjunction with C/N ratios (from which terrestrial:aquatic sources can be distinguished), as this is a much more powerful way of properly understanding these signals. The site itself has been under investigation by the authors for the past 16 years, so we have a good understanding of environmental controls in the catchment. For example, with regard to the green alga Botryococcus mentioned by the reviewer, this has been only rarely observed and, furthermore, occurred in only a few samples during palynological analysis. However, we take the point that the text as it currently stands perhaps underplays the range of potential sources of organic matter in the lake sediments and we will therefore expand on this in the revised manuscript.



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“Likewise specific degradation that would preserve carbon with respect to nitrogen (e.g. carbohydrate sulfurization. . .) or “half-completed” early degradation can lead for misinterpretation.” This is a permanent lake, one that has remained wet over its 4000-year history; Chepstow-Lusty et al. (2003) discusses the origins of the lake in detail and elaborates on the year-round supply of glacial melt-water to the basin. This factor (in part) accounts for the exceptional preservation of the highly organic sediments and the faunal/floral remains they contain. There is no easy way to assess degradation; however, whilst we acknowledge that there will have been some early degradation of the organic material, the effect on the isotope signal will generally be much smaller than from other effects.

“In this case, everything but geochemical appears as having a stand-alone value whereas ïĄď13C 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.” Whilst we appreciate the recognition by the reviewer of the importance of the other proxy analyses presented as part of this study, we would argue that the geochemical data also make a fundamental contribution to our understanding of the environmental changes occurring in and around this lake. This paper builds on a strong foundation of existing multi-proxy work at this site (including geochemical analyses) and, given that three of the co-authors are, to varying degrees, all specialists in lacustrine-based isotopic systems (including a Professor of Isotope Geoscience who runs the national isotope laboratory in the UK), we feel that the level of discussion and interpretation in this manuscript are robust and appropriate for the study being conducted.

Detailed Comments

“I would like to find some points on the strategy. “New proxy” (p. 773, l. 30) is definitively too short to present the paper.” The specific nature of all the proxies analysed and the reasons for their incorporation into this study is all clearly set out on p.775 (line 23 onwards) and in section 3 of the paper.

“Site selection: a little bit too long.” In view of some of the reviewer’s earlier concerns about sediment degradation, etc. we feel that we should provide a reasonable level of detail for the reader without necessitating extensive reference to previously published work.

Methods “p.776, l. 22: is the 0.1% relative or absolute precision? Is it for C:N or %TOC or %N?” A good point. This value is an absolute measure for both %TOC and %TN; C/N is a ratio derived from these measurements and therefore has no units. We appreciate that this is slightly unclear and we will amend the final manuscript accordingly.

“Add a subtitle for “charcoal analysis”. It should not be included in “organic matter geochemistry” paragraph. Please refer to Figure 5.” Charcoal analysis methodology is clearly discussed under an appropriate sub-heading in section 3.3 (page 777 onwards) and is distinct from section 3.2. Sub-section 3.3 deals with methods only; the results (next section) fully reference figure 5 in the appropriate places. This is similar to the treatment of macrofossils, for example; section 3.1 details analytical methods, and the results section refers to the appropriate figures.

Results “As already expressed in “General Comments”, I would prefer to have results and discussion gathered. . .” Please see earlier comment. We consider it important that the presentation of analytical results is distinct from the interpretation of those results; accordingly, these sections are separated.

“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.” The datasets we are dealing with are large and so are presented here in visual formats. It is difficult to see how even a supplementary data table could be formatted appropriately for dissemination in a concise fashion. However, once this paper is published, data will be made available to interested parties via the usual web-based palaeo-databases and will also be available directly from the co-authors. That said, we appreciate the general point raised by the reviewer and recognise that figure

4 in particular contains a significant amount of data – we shall endeavour to make the final version as clear as possible.

"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." This is correct. Normally, lacustrine C/N ratios of between 10 and 20 represent a mixture of aquatic and terrestrial plant material, whereas C/N ratios >20 indicate terrestrial plant dominance. Hence, at Marcacocha, the C/N ratio >15 shows the importance of terrestrial sources. This factor is referred to several times throughout the text (e.g. p.780, line 4 onwards), but we appreciate that we could be more explicit at times (for example, discussing the actual values from time-to-time). We will make appropriate changes to the final manuscript to reflect this useful suggestion.

"P. 778, l. 10: where do you see a decrease of ïĄď13C? I only see a rapid decrease at ca 880-920 but no way along the whole AD 1100-1400 period." We agree that, as it stands, the text is ambiguous and we will make appropriate changes to the final manuscript.

"If the zones you defined fit relatively well with floral records, that's not the case for ïĄď13C 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." The zones were calculated using a numerical binary splitting technique (see p.777, line 24). Whilst it is possible to set parameters that can effectively produce as many zones as the analyst requires, we used the numerical technique to define relatively few zones (clearly dominated by the palynological signal) and so ensure that subsequent discussion was clear and uncomplicated.

"P. 778, l. 13: which kind of biological indicator do you use to assert "a critical threshold for biological activity appears. . ."" We appreciate that this statement is perhaps confusing as it essentially mixes results with interpretation. We will amend the text accordingly



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in the final version.

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?" Immediately prior to AD 880, a combination of lower organic production and higher inorganic input form the lake sediments during this period of colder temperatures. Although human populations would have been minimal, some anthropogenic disturbance is possible (as noted by the minor charcoal peaks at the top of this interval). More importantly, however, since plant growth would have been slow and the vegetation sparse, any natural or human agents would have enhanced erosion as the topsoil would have been less stable. An examination of the radiocarbon dates shows that the sedimentation rate is lower during this interval than the earlier part of the first millennium AD, so it is not necessarily as simplistic as equating higher erosion and greater inorganic input with a higher sedimentation rate.

"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?" We agree that the text is unintentionally misleading and will re-write this sentence in the final manuscript.

"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." Whilst we appreciate that this appears to be a reasonable suggestion, in the interests of maintaining methodological rigour we would prefer to retain the zones defined by the numerical splitting technique (see comment above) and not use a hybrid approach (i.e. to define zones numerically and then add one or more 'by eye').

“P. 781, l. 10: what is the link you invoke to tie decrease in TOC and erosion increase?” At approximately AD 1170, it is likely that the process of terrace construction briefly destabilized the landscape, causing more inorganic material to be deposited in the lake. It was only after construction was complete that erosion would have been reduced and the landscape stabilized – after which the lake sediments become highly organic, with very little inorganic input for many centuries afterwards.

“P. 781, l. 13: do you wish to mention soil OM maturation when you associate high C:N and increase of soil input?” The sustained increase in C/N ratios for almost 50 years immediately after the period of terrace construction (increased inorganic input to the lake sediments – see above) supporting the notion of more soil-derived organic matter and/or the increased influence of terrestrial vegetation, adequately explains the interpretation of the data. It is likely that vegetation was cleared around the lake and during the process part of this has entered the lake contributing to the terrestrial signal.

“P. 782, l. 4: you mention both “decline in ïĄď13C” and “increased agriculture (including maize)” BUT maize is C4 plants and increase of maize should result in increase of d13C. . . not the reverse!” It is true that C4 plants (including maize) can have a ïĄď13C signature of around –13‰. However, maize is never grown directly next to the lake, as conditions are too wet. Maize is only cultivated on well-drained land, and it is only crops such as potatoes that are grown in the areas liable to inundation – as occurs around the present-day lake. The signal from maize that we see in the record is that of occasional maize pollen grains that are blown/washed in. Although these (unusually large) pollen grains provide indisputable evidence of maize in the immediate proximity, their rareness does not give an accurate record of the continuity or scale of maize production. We can, however, state that maize is not contributing to the organic signature of the lake sediments.

Table and Figures “They are all very small and not easy to read. This should be checked for a future potential publication.” We shall endeavour that these are scaled to be as large and as clear as possible, within the guidelines of Climate of the Past.

“Table 1: please respect 14C conventions and present 14C ages as they should be. . .” We take on board the reviewer’s comment with regard to the reporting of calibrated radiocarbon dates and will therefore make appropriate clarifications to the table in the final version of the manuscript.

“Figure 1: I’m not able to situate the bottom panel in the right upper one.” Good point. In the final version of the manuscript we will highlight the name of the lake (Marcacocha) to better enable geographical orientation between the panels.

“Figure 2: nice view. . . is it really useful?” It puts the lake (in-filled today, shown by the dark circle of Juncaceae vegetation) in the landscape context. The road to the right connects the nearby town of Ollantaytambo to the selva and was important for llama caravans in the past. The Patacancha River and the archaeological site of Juchuy Aya Orqo are directly to the left of the in-filled lake. The setting also allows one to better appreciate the nature of the sediments entering the lake in the past and sheds light on the terrestrial vegetation contributing to the ïĄď13C and C/N signal. Note the heavily terraced landscape, but also the wetland zone close to the in-filled lake that remains still very important for grazing.

“Figure 3: no interest” This diagram provides a longer-term historical and environmental perspective for the site and sets the present study in a temporal context.

“Figure 4: please gather indices according to what they represent.” This is a complicated figure and the proxies have been deliberately ordered so as to be read in a logical fashion that will clearly dovetail with the text. Palynological data are plotted to the left of the diagram; macrofossil data are scaled along axes at the bottom of the diagram, whereas geochemical data are scaled along the top.

“Figure 5: I would like an extended legend. . . 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.” We agree that the legend could be made slightly clearer and the manuscript will be revised accordingly. Reference in the text to each of the panels will be ensured.



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