

## ***Interactive comment on “Fire, vegetation and Holocene climate in the south-eastern Tibetan Plateau: a multi-biomarker reconstruction from Paru Co” by Alice Callegaro et al.***

### **Anonymous Referee #2**

Received and published: 9 April 2018

General Comments: In this paper Callergaro et al. present results from a biomarker multiproxy reconstruction of fire and vegetation from lake sediments Holocene on the Tibetan Plateau. The methodology used in this paper and the scientific aims of this study are will within the scope of this journal. This paper applies a clever approach where multiple lines of environmental evidence (i.e. fire, vegetation, human/animal habitation) can be reconstructed from the same samples using a relatively streamlined workflow. Additionally, I appreciate the authors' tactic of using data from the GCD to interpret their fire data within a regional framework.

Despite these strengths, this paper could be improved by better presentation (figures)

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and clearer interpretation of the data. I found myself unable to follow the logic at times, and occasionally, the data and interpretations seemed at odds with each other. Adding more complete explanations of proxy interpretations (in both the text and the figures) may clear up some of this confusion. There were also times in the discussion where evidence from other studies was presented without being linked to the new data, and the new conclusions felt buried. Make sure to emphasize the novel contributions of your work and what it adds to the literature framework.

I have outlined some more specific issues below. Making these improvements will greatly increase the readability of this paper and strengthen the arguments.

Specific Comments: Page 8 Line 31: Are you using %BSi as a proxy for monsoon intensity? If so an added sentence explaining why would be helpful. Also did you measure %BSi or %TOM or is it from Bird et al 2014? Please specify.

Page 9 Lines 13: I don't know if concurrent increases between PAHs and TOM implies a specifically biogenic origin for PAHs, just that the total organics in the lake and the PAHs may have a similar source. Especially given that your aquatic/terrestrial indicators show an increase in terrestrial n-alkanes to the lake after 8 cal ky. Perhaps there is more windblown terrestrial material being added to the organic pool and that's why it's increasing? You touch on this in the next paragraph.

Page 9 line 20: MAs are more water-soluble than PAHs, so this argument doesn't make a lot of sense

Page 9 Lines 31-33: You need to explain and cite how you are interpreting these ratios and what is the difference between L/M and L/(M+G).

Page 10 Paragraph on Line 5: ACL and Paq represent indices for differential terrestrial/aquatic inputs I'm not sure how that directly relates to interpretations of fire and vegetation change. Make it clear if you are relating this to lake levels and climate because these proxies don't explicitly address changes in terrestrial vegetation com-

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munity.

Page 10 Line 19: Are you using this to say something about source area changes or vegetation community changes? This distinction is not clear.

Page 10 Line 23: Is there any evidence that sitostanol is correlated with grassy tissues? Citation?

Page 11 Lines 13-32: This paragraph is under "paleofire activity" but doesn't mention fire at all, just gives some climate context. Perhaps having a climate section would be useful?

Page 12 Lines 9-15: Throughout the manuscript, potential reasons for the differences between the MA and PAH records are mentioned, but no evidence is presented to support any of these interpretations over the other. Are there sedimentary changes (i.e. grain size) when these records diverge that indicate changes in transport to the lake? Are there changes in PAH ratios throughout the core that might indicate changes in transport/fire temperature? Perhaps there are inherent differences in the transport of MAs and PAHs due to their size/solubility differences? Why might the charcoal and PAH record correspond better than the charcoal and MA records? If these questions are explicitly addressed it will greatly strengthen interpretations of fire history. If you haven't already, I'd suggest reading Denis et al. 2012 Organic Geochemistry, which has a good discussion considering transportation/degradation/fire temperature differences in lake fire proxy records.

Page 12 Line 26-28: What particular climate effect would Bond events have on fire in Tibet? Link through a mechanism

Page 13 Lines 1-4: Like I said in the results, I don't think you can assume that the PAHs are biogenic. Not without some additional evidence. Have you tried to use some PAH degradation ratios? The two papers you cite here are good resources for these tools. I'd be interested to see if applying the appropriate ratios to your data provide evidence

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for degradation.

Page 13 Line 5-15: I appreciate bringing in this discussion of transport. . .but I feel like how this relates specifically to your data gets lost. Does the peak at 5.6 represent increased wind, fire or both?

Page 13 Lines 14-15: Are there dust layers recorded at Paru Co?

Page 14 Lines 4-13: The times you highlight as having higher vegetation density are not all correlated with your fire records. Make sure you state when this mechanism applies and when it does not, and why that may be.

Page 15: I think that you need to address explicitly how the vegetation changes (i.e. shrubs versus trees) that are observed in the record are (or aren't) tied to your record of fire activity.

Page 15 Lines 2-6: Leaves deposited into the lake are not the only source of terrestrial alkanes, they can also be ablated off leaves and transported by wind.

Figure 3. Instead of color boxes interpreting the ISM instead show the original proxy record you are using for those interpretations. The  $\delta$ Dalkane record from this same core from Bird et al. 2014 would be perfect to show plotted against proxies that you argue are influenced by ISM variations.

Figure 4. A and B could use some interpretive annotations. What do high/low MA ratios mean in terms of vegetation community? This is something that is unclear throughout the entire manuscript. How are these ratios interpreted and why do they differ? Adding some interpretive lines (like in 4C) would be very helpful. 4C. What is meant by arbitrary units? Explain this in the caption.

Figure 5. I think plotting these records with a moving average obscures some important variability. It almost seems that the ACL and Paq have millennial cycles. Looking back at Bird et al. 2014, this seems to vary with reconstructed lake levels, rather than insolation as you argue. I think plotting that data in the same figure as these would

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really strengthen the interpretability of these proxies. Also, ACL and Paq and Norm31 and Sitostanol are really proxies for different things (aquatic v. terrestrial vegetation and changes in terrestrial community). It would be much clearer to separate these proxies into different figures. Perhaps, one figure with lake levels, insolation, ACL and Paq, and then add Norm31 and Sitostanol to Figure 4, which is your terrestrial vegetation figure.

Figure 6: Pg. 10 line 33 it is stated that the charcoal data from the GCD was drawn from a 1000 km radius from the Paru Co site. . .However, after a quick check on Google maps, I found all of the red dots on the Fig 6a map are actually more than 1000 km away from the site. Please address this contradiction and correct how the GCD sites were chosen in the text. Additionally, this large area of integrated charcoal records is potentially problematic because this data is drawn from a large continental area and the assumption that these sites are subject to the same climate conditions as the Paru Co site may no longer hold.

Figure 6 B and C: The shaded regions and arrows are not explained in the caption. And if they are showing correspondence then it almost seems that these records hardly reflect each other (which is discussed a bit in the text). Why is the PAH record not shown? I know it is highly variable, but it actually may correspond better to the charcoal record. . .if this is the case, then what are the implications for your data?

Technical Corrections: Page 2 Line 1: The dependent clause of this sentence is unclear

Page 2 line 13-15: perhaps an i.e. style list of just three or so methods

Page 2 Line : move “in the last Century” to after “biomass burning”

Page 4 Line 6: Perhaps this would be a good place to introduce Neolithic/bronze age societies you talk about in the discussion.

Page 4 Line 13: cite Figure 1a

Page 5 Line 30: cite Figure 1d

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Page 6 Line 18: new paragraph at “Each Sample”

Page 7 Lines 18-19: What company did you obtain your standards from?

Page 8 Line 11-12: This topic sentence does not fit the content of the paragraph. Additionally, I think you actually end up arguing that these records don't agree?

Page 9 Lines 10-13: This sentence is confusing and potentially unnecessary since you elaborate on it in the results.

Page 10-11 Section 4.3: This reads like a methods section rather than results. Move this to methods and instead describe the trends you see in your analysis in the results.

Page 11 Line 29: delete “a” before decreasing

Page 12 Lines 15-19: This is a rambling sentence; perhaps splitting it into two would make it clearer?

Page 15 Line 6: you seem to be missing a word after sedimentological

Page 15 Line 8: delete “seems that and add “also” between “could” and “help”

Page 15 Line 26: I'm confused what you mean with “except when associated to PAHs”

Page 15 Line 28: Citation needed Bush and McInerney (2013) GCA and/or Diefendorf et al (2011) GCA.

Page 15 Line 27: Be consistent with the use of pollens or pollen, don't switch off

Page 16 Line 28: I think this is the first time you mention Bronze Age civilizations. You should elaborate on this earlier in the paper.

Page 16 Line 32: This is an abrupt way to end. Perhaps add a sentence of significance or implications?

Figure 1. Include what the dates in 1D are based on (14C?).

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