

## ***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 #3**

Received and published: 12 April 2018

#### General Comments:

In this manuscript, Callergaro et al. demonstrate the usefulness of a multi-proxy approach to reconstruct fire and vegetation change throughout the Holocene from a lake sediment record in the Tibetan Plateau. The research objectives and methodology used in this study are within this journal’s scope. This manuscript attempts to reconstruct fire, vegetation change, and human presence nearby the lake using biomarkers, and discuss how the results and other regional analyses of fire and climate compare.

This manuscript presents a unique and novel record for the TP region. However, this paper could be improved with better interpretation of data, as well as better figures

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and presentation of data. The data and the author's interpretation seemed to conflict with each other, particularly with the fire records presented. Adding more background information, discussion, and analysis of some of the records (detailed below) could drastically improve this paper and the discussion of results. I have presented some more specific issues below, listed from "Major Comments" to "Minor Comments" to "Figure Comments". Making these improvements will greatly increase the readability of this paper and strengthen the arguments.

#### Major Comments:

I'm not sure I follow the PAH argument. In the manuscript, you argue that PAH track local and regional fire activity, along with MAs, in the early portion of the record (10.7-8.7), but may switch to having biogenic origins after 8.7, just due to the fact that they correlate slightly with TOM. Looking at Fig 3, it seems as though the PAHs are actually making sense as a fire proxy more-so than MAs- lower values during the times of increased ISM rainfall, higher values during the times of decreased ISM rainfall. Furthermore, the "noise" in the PAH record looks like millennial scale fluctuations in the fire activity, which you aren't capturing in the MA records. I would suggest more discussion on the PAHs as potentially tracking fire activity, instead of just writing them off as being biogenic in nature. There are many different ratios of PAHs that studies have shown to prove useful in determining PAH source (i.e. biomass burning vs fossil fuel burning, biogenic vs burning, etc. . .). Possibly look into some of these ratios as well to see if you can determine a ratio that is suitable for developing your story. Some papers that use ratios include:

Denis et al. (2012). PAHs in lake sediments record historic fire events: Validation using HPLC-fluorescence detection. *Org Geochem*.

Miller et al. (2017). Local and Regional Wildfire Activity in Central Maine (USA) during the past 900 years. *Journal of Paleolimnology*

Yunker et al. (2002). Sources and significance of alkane and PAH hydrocarbons in

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I'd like to note that just through comparing Figures 2 and 6 by eye, it seems like the PAH record tracks the GCD regional composite record fairly well (at least way better than the MA record does). I would advise plotting the PAH curve on figure 6 – that way we can visualize how the PAH record tracks regional fire activity.

Minor Comments:

Page 13, line 23-24: “In general, fire history shows a decreasing trend from 8 cal ky BP to the present” – this isn't apparent based on the figures you show. The MAs decrease, but the PAHs steadily increase. Distinguish between the two instead of saying “the Paru Co fire history”

Page 17, line 7: this should be labeled the MA fire history, not the overall fire history from your record. The PAH fire history shows the opposite of this – with lowest values at 8 cal kyr BP, and then a long term increasing trend.

It could be beneficial to include coring location in lake and a bathymetric profile of the lake. Looking at lake bathymetry could give insight or possibly explain some of the trends seen in the data, and could manipulate the age-to-depth model so that it isn't linear in reality.

Please check target ions for each compound – for example, many studies that look at retene have a target ion of 219 instead of 234 (the compound's molecular weight). The mass spectra can be found here: <https://webbook.nist.gov/cgi/cbook.cgi?ID=C483658&Units=SI&Mask=2380#IR-Spec>. Using 234 may be adequate, but in some cases using non-major ions may “hide” compounds, particularly when running in SIM mode on a GC-MS. In your case, it seems that this may in fact be occurring, since you report retene was undetectable in most/all samples. Given the fact that retene is produced by combustion of coniferous trees, it's surprising that retene is not found, given the fact that you mention a coniferous forest

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near the lake (line 24, page 5).

The spikes seen in all data at the beginning of the record has me skeptical of whether or not it is a true signal of some climate/environmental variability. Adding in some discussion (1-2 paragraphs) on other, more plausible causes of this (i.e. an event in catchment that was preserved in the sediment record, a coring artifact, etc. . .) could give your arguments more validity throughout the manuscript.

Figure Comments:

Fig 1 a) the map seems a more complex than is necessary. The surrounding areas may not be as important to this study as the TP, so one option could be zooming in on the study region. One option could be to make it similar to the map in the supplement – that map is simpler and much easier to read, and having a map similar to that could more easily highlight the study areas in this figure. Also, you might want to confirm with google about publishing google map images in academic journals – I’m unsure if there are any special permissions needed from Google, but it could be a good thing to check.

Fig 2) do not overlap a) and b). This makes it seem as though there is a peak in values occurring at 4 cal kyr BP. There are multiple ways to fix this – you can either separate them so they don’t overlap, or possibly highlight/box the areas in a) that are being zoomed in on in fig b).

Fig 3) try moving a) and c) y axes over to the right side – that way the axes are not overlapping or too close together.

Figs 2 and 5) use the same color between these two plots for similar things. For example, in figure 5 you use a gold line to separate ISM changes, while in figure 2 it is a blue line. Try to stay consistent in color schemes for the reader.

Fig 6) needs to be higher resolution. On figures b and c, you can barely see the lines. Making the lines bolded/bigger, as well as saving a high resolution image, would help

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fix this issue. Furthermore, adding the PAH record, not just the MA record, would be very beneficial, as the PAH and GCD records seem to track each other.

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-19>, 2018.

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