

Interactive comment on “A 150-year record of phytoplankton community succession controlled by hydroclimatic variability in a tropical lake” by K. A. Yamoah et al.

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General comments: The manuscript by K. A. Yamoah et al. presents phytoplankton community succession and geochemical variations over the past 150 years in a tropical lake in southern Thailand. Overall, the authors comprehensively collected DNA, lipid, elemental data, and drew relevant conclusions. But, there are specific and technical problems that should be resolved, so I think that the manuscript doesn't meet the requirements for publication on GB.

Answer We sincerely thank the reviewer for taking the time to review our manuscript. The comments and suggestions made have been taken into account in the revised manuscript. We respectfully disagree with the particular comment that our manuscript

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does not meet the requirements for publication in Biogeosciences based on technical problems. Below we provide justifications to enable the right editorial decision to be made.

Specific comments: The authors concluded that hydroclimate change and anthropogenic activities played an important role in phytoplankton succession. However, the authors just mentioned “hydroclimate” in the title, but I suggest “anthropogenic activities” should be also included in the title.

Answer Although anthropogenic activity does play a role over the last 40 years with increased phosphorus levels, this is not the main focus of the paper. Instead, we highlight the influence of natural hydroclimate variability on phytoplankton community change over a ~150 year period. The main factor why hydroclimate variability is highlighted in the title is because, without the changes in hydroclimate conditions, new resources entering the lake will be limited, including Si and P input, regardless of the source. Particularly, the atmospheric component of P is weak and continental weathering and runoff transportation often promote supply to aquatic systems. That is to say that the degree of drainage into the lake controlled the phytoplankton shifts, not anthropogenic activity. We have identified two key changes in nutrient input sources that are directly coupled to hydroclimatic dynamics: (1) changes in Si input originating from weathering of local rocks and (2) phosphorus originating from anthropogenic sources, which have been brought into the lake by runoff intensity. Therefore, the title captures the main pathway to nutrient input into the lake, not the processes that generate the nutrients, namely weathering and anthropogenic sources. Otherwise we will also have to change the title to include weathering.

Some more specific comments as follows: Page 3, line 18-19: Please specify “external” and “internal” ecosystem regulators.

We have changed the text to elaborate more on the external and internal factors we allude to. By external factors, we mean processes such as rainfall and anthropogenic

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activities that affect the lake, including weathering and runoff intensity. Internal factors include the cycling of various nutrients within the lake, including nutrient regeneration, rates of primary productivity and organic carbon and nitrogen cycling, etc. These are factors, which we subsequently address by the amount of data provided.

Page 4, line 19: it may be better to change “chemical environment” to “chemical and physical environment”.

We have changed this in the text.

Page 10, line 9-13, authors show wet/dry conditions in parenthesis. Pls explain how the results “wet/dry condition” were inferred, and include appropriate inferences.

δD_{wax} is commonly used as a paleoclimate proxy to reconstruct moisture availability in monsoon regions, as supported by references in the manuscript and the supplementary (see page 4 line 23-25). We do not deem it necessary to discuss how the proxy works in the main text and have therefore provided citations to this effect. Rather, a comprehensive summary about the δD_{wax} as a proxy for hydroclimate is provided in the supplementary, which we referred readers to. In addition, we provided rainfall data for much of the studied interval and compared it with the δD_{wax} data. Therefore we use wet and dry to clearly define more and less rainfall, respectively as alluded to in the text.

Page 11, line 3, change “Eukarya” to “eukaryotic”. Page 11, line 16-17, “as observed in many other systems (Emerson and Hedges, 2003)”, two or more references should be cited here.

We have changed the text and added the following references: Ostrom et al 1997; Altabet, 1998; Sachs and Repeta, 1999).

Page 12, line 2-3: references cited here suggested that ^{13}C enriched is in residual organics. However, in Unit III, $\delta^{13}C$ was more negative, while *mcrA* abundance was relatively high. Please explain the inconsistency.

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We agree partly with the reviewer that this appears inconsistent and have made changes in the text to address this point better. We like to note that *mcrA* abundance was used sparingly to connect lake productivity to anaerobic methane cyclers. However, the *mcrA* gene is a proxy for both anaerobic methanotrophy and methanogenesis. High rates of anaerobic methanotrophy and methanogenesis tend to produce extremely negative $\delta^{13}\text{C}_{\text{org}}$ that can even reach -60‰. Therefore, the coincidental transition to more negative $\delta^{13}\text{C}_{\text{org}}$ values coupled to increasing abundance of the *mcrA* gene, do indeed suggest an increase in either anaerobic methane oxidation of methane or methanogenesis. Therefore we have now discussed the data accordingly in the manuscript since it appears that specifically in unit III anaerobic methane cycling might have contributed significantly to the $\delta^{13}\text{C}_{\text{org}}$ signature of residual organic carbon.

Page 12, line 4-5, the sentence is obscure, i.e. “eukarya” doesn’t represent all “primary productivity”, which includes both cyanobacteria and eukaryotic algae. Please clarify it.

This has been clarified in the revised text. Now it reads “Eukaryotes contribute significantly to primary production in lake systems, thus a strong correlation between *mcrA* gene abundance and Eukarya (Fig. 6; $r^2 = 0.85$) could indicate that the depth profiles reflect a concurrency of primary productivity and methane cycling in the anoxic lake bottom waters.”

Page 12, line 20, does “microbial activity by anaerobic microbial methane cyclers” mean “methanogenesis”?

No, because *mcrA* genes are for anaerobic methane cycler’s i.e. methanogenesis + anaerobic methane oxidation, as explain in a comment above.

Page 13, line 7-8: Cartapanis et al. 2014 used opal other than total Si elemental concentration to infer nutrient dynamics. I’m not sure if it is appropriate to use Si concentration in this study.

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We respectfully disagree here with the reviewer. Cartapanis et al. (2014) used elemental ratios of Si/Ti as a proxy for opal, which is a hydrated amorphous form of silica. Indeed, we also used Si/Ti as a “proxy for nutrient dynamics linked to hydrological changes (Cartapanis et al., 2014) and as an indicator for enhanced diatom production in lakes (Wennrich et al., 2014)” (refer to page 13 line 7). What is of importance in our data is not the specific Si mineral in the sediments, since what form of Si remaining in the sediments reflects diagenetic and recrystallization processes. What we are interested in is mapping changes in the Si budget as a function of the detrital input. Ti is a common detrital input signal. Essentially, the supply of dissolved Si by runoff should vary accordingly with the Si/Ti ratio since Ti is broadly immobile. An increase in the Si/Ti ratio implies more input whereas decreasing Si/Ti implies the reduction in runoff supply since the authigenic Si content of the basin is not amplified by an external source. Our approach is consistent with that used in many Paleo-environmental studies and the mineral form of Si in the sedimentary basin is inconsequential. In fact, most mobile elements are often normalized to Ti to show changes in sedimentary inputs, from lakes to marine systems. See for example:

1. Konhauser KO, et al. (2011) Aerobic bacterial pyrite oxidation and acid rock drainage during the Great Oxidation Event. *Nature* 478:369–373. 2. Mathur R, et al., (2004) Cu isotopes and concentrations during weathering of black shale of the Marcellus Formation, Huntingdon County, Pennsylvania (USA). *Chem Geol* 304-305: 175–184. 3. Demory F, Oberhänsli H, Nowaczyk NR, Gottschalk M, Wirth R, Naumann R (2005) Detrital input and early diagenesis in sediments from Lake Baikal revealed by rock magnetism. *Global Plan Change*, 461:145-166.

Page 13, line 25, “which confirms that the C17 n-alkanes were produced mainly by Cyanobacteria” seems too arbitrary. I suggest to change it to “which suggested that the C17 n-alkanes may be produced mainly by cyanobacteria”

We agree with the reviewer. This has been changed in the revised manuscript.

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Page 14, line 2, it's better to replace "race" with "lineage" or "subgroup". We have changed the text according to the suggestion.

Page 14, line 8, "likely results" should be "is likely resulted". Page 14, line 10, replace "within" with "in".

We have changed the text as suggested

Page 14, line 13, replace "in" with "during". Paragraphs within "Summary and conclusion" from page 14, line 14 to page 15, line 16 are just a repeat from the last section. I suggest that these sentences should be deleted.

We have revised the text and have incorporated the reviewer's concerns.

Technical corrections: A lot of terms should not be italic or capitalized. For examples Bacteria, Cyanobacteria, Eukarya, sp. Pls check. Page 3, line 22-25, the sentence is confusing. Please revise it. Change "factors that" to "which". Page 12, line 22-23, the sentence is hard to understand. Pls rewrite it. Page 13, line 23, "photosynthetic bacteria such as Cyanobacteria" can be changed into "cyanobacteria". Page 11, line 3, change "Eukarya" to "eukaryotic". Page 11, line 5, "and" should not be italic

We have revised the text and have incorporated the reviewer's concerns.

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