

## ***Interactive comment on “Water pH and temperature in Lake Biwa from MBT'/CBT indices during the last 282 000 years” by T. Ajioka et al.***

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We thank Referee #1 for his/her review. The comments help us to improve our manuscript.

Reply to the comments by Referee #1

Comments: Ajioka et al. generated a 282 000-year record of water pH and temperature in Lake Biwa. Their interpret water pH as a proxy for summer precipitation in central Japan and propose synchronous variation with air temperature, in contradiction with previous studies. They also propose that East Asian summer monsoon precipitation was governed by Northern Hemisphere summer insolation on orbital timescales, similar to the interpretation proposed with oxygen isotopes in stalagmite, although ex-

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tremely debated. Finally, they suggest that the temperature variation reflected winter monsoon variability.

General comments: The new records (MBT and CBT) generated by the authors are interesting and could potentially bring new insight concerning the timing of Asian monsoon at the orbital scale, a very debated topic. However, the interpretations of the records and conclusions are quite speculative because 1) the calibration used in the manuscript to convert the CBT and MBT records to pH and MAAT is unpublished (Ajioka et al., submitted) whereas previous calibration (Tierney et al., 2010) differed from that of the global soil set and 2) the age models used in this study are not published (Kitagawa, personal communication 2014 and Takemura, personal communication 2014) and contain too large errors to address the timing of the records discussed in details in the manuscript. I am therefore sorry to say that I recommend rejection of the paper. The authors must published their calibration study first (Ajioka et al., submitted) and the age models of the two cores (or furnish all the data used for the age model in this paper) before the manuscript can be considered for publication in *Climate of the Past*. I furnish more explanations in the “specific comments” section below.

Reply: The referee pointed two critical things. First, the paper of local calibration was not published. The referee thus could not judge the validity of the method we use, and recommend us to wait until the calibration paper is published. Second, the age-depth model was also not published. Because there are too large uncertainty of age-depth model to discuss the phase of variation. The referee was not convinced with the interpretation. As to the first point, the paper Ajioka et al. (2014) was recently published in *Organic Geochemistry* (vol. 73, page 70-82). We understand that the readers are not convinced our interpretation without brief introduction of local calibration study, we will describe briefly the results of Ajioka et al. (2014) with two figures (Fig. 7b and Fig. 9 in Ajioka et al., 2014) in the revised manuscript. As to the second point, we totally agree with what the referee commented. We will describe the age-depth model in the revised manuscript. Because the period before 143 ka is not well dated, we will not

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discuss the variation in this interval. From 50 ka to the present, the age uncertainty is small enough to discuss the variation on orbital timescales. From 143 to 50 ka, the age uncertainty ( $2\sigma$  values [95% confidence level]) of core BIW08-B ranges from 5 to 11 ky because of large error of radiogenic ages of tephras. However, the pollen composition ( $T_p$  and *Chyptomeria* abundance) in core BIW95-4/BT nearby the study site have a consistent variation with that in a marine core MD01-2421 from the offshore of central Japan in the western North Pacific. The age-depth model of core MD01-2421 was established by oxygen isotope stratigraphy using benthic foraminifera isotopes (Oba et al., 2006). Assumed synchronous vegetation change in central Japan, the correspondence of pollen assemblages between cores MD01-2421 and Lake Biwa assures that the uncertainty of the age-depth models of Lake Biwa cores in MIS 5 and 6 is smaller than that indicated by the dating error of each tephra, which is precise enough to discuss variation on orbital timescales. We will discuss the variation in this interval with caution of this limitation of age-depth model in the revised manuscript.

Specific comments: Comment 1: Introduction, first paragraph. The debate concerning the timing of the Indo-asian monsoon is not detailed enough. There is a lot of records showing a different timing and with different hypothesis that must be explained and reviewed in details (see for examples Morley and Heusser, 1997; Reichert et al., 1998; Sun et al., 2006; Clemens et al., 2008; Cheng et al., 2009; Ziegler et al., 2010; Caley et al., 2011; An et al., 2011; Caley et al., 2013).

Reply: Indeed. We will include the papers in the introduction.

Comment 2: The calibration used by the authors is unpublished for the MBT and CBT. This is mentioned as Ajioka et al. 2014 in the introduction whereas the reference is Ajioka et al. submitted. As mentioned by the authors, this calibration is in contradiction with previous results: "Tierney et al. (2010) noted that the correlation between MBT/CBT from sediments and MAAT for 46 lakes in East Africa differed from that of the global soil set and proposed a calibration applicable in lake environments. Ajioka et al. (2014) investigated the distribution of GDGTs in soils and river and lake sedi-

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ments in the Lake Biwa drainage basin and showed that the distribution of branched GDGTs in the lake sediments was different from that in the catchment soils, suggesting in situ production of branched GDGTs in the lake. They also found, in contrast to the conclusion of Tierney et al. (2010), that the relationships among soil pH, MAAT, and MBT0/CBT in soils are not different from those of lake water pH, temperature, and MBT0/CBT in lake sediments, implying that the soil calibration is applicable without modification to the study of lake sediments to obtain lake water temperature and pH.” Therefore, the calibration must be accepted before it can be used in this study.

Reply: The paper of Ajioka et al. (2014) is now published. Both calibrations by Tierney et al. (2010) and Ajioka et al. (2014) are based on an empirical relationship between sediment MBT'/CBT and observed lake water temperature. The Tierney et al. (2010)'s calibration implies that the MBT' and CBT of lake water bacteria respond to lake temperature and pH differently from those of soil bacteria. In contrast, the Ajioka et al. (2014)'s calibration implies that the MBT' and CBT of both lake water and soil bacteria respond to pH and temperature in a similar way. There is no direct evidence of temperature dependence of MBT'/CBT, and thus further study is necessary to understand this difference.

Comment 3: The age models used in the study are unpublished (Kitagawa, personal communication 2014 and Takemura, personal communication 2014), and this is not acceptable given the importance of such results for the interpretation of records. Furthermore, the proposed age models contain very large errors. It is clearly visible on Figure 3 that the age model has errors of 25-50 ka!! between 50 and 150ka and errors of around 100 ka!! between 150 and 300ka. These errors are clearly more important than a complete precession cycle (23ka). Therefore the timing and forcing of the records interpreted in the manuscript could be completely wrong. As an example, the comparison between the insolation and the CBT-pH on figure 7 is far from clear. Some of the maximum peaks of the CBT are phase with max insolation (200ka and 100 ka), some are in antiphase with max insolation (270ka, 150ka and 60ka) and others

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are various delayed. This is probably due to the significant uncertainties with the age model.

Reply: We totally agree with what the referee commented. We will describe the age-depth model in the revised manuscript. Because the period before 143 ka is not well dated, we will not discuss the variation in this interval. We will discuss the variation in this interval with caution of the limitation of age-depth model in the revised manuscript.

Comment 4: Discussion 4.1 The authors conclude “that that photosynthesis in the lake water is the major factor controlling water pH in Lake Biwa”. In the abstract they mention that “Because water pH in Lake Biwa is determined by phosphorus input driven by precipitation, the record of water pH should indicate changes in summer precipitation in central Japan.” Does the input of phosphorus is responsible of the photosynthesis changes? Insolation changes (light) could also affect photosynthesis without relationship with monsoon input.

Reply: Yes, it does. We will add the following discussion. In Lake Biwa, photosynthesis is mainly controlled by phosphorus concentration in the water (Ishida et al., 1982; Tezuka, 1985). Actually, Lake Biwa has undergone eutrophication and indicated high primary production since 1960s due to increase in industrial and domestic waste water containing much amount of nutrients, consequently the pH of the lake water increased more than 1 from 1960s to 1970s (Nakayama, 1981). Phosphorus concentration in the lake is determined by the inflow of phosphorus from the catchment soils, which is govern by precipitation in the watershed (Kunimatsu, 1993). At the present day, the East Asian summer monsoon brings most of annual precipitation to the study area, exceptionally at high elevations in the northern part where snowfall brought by the East Asian winter monsoon is relatively important (<http://www.jma.go.jp/jma/index.html>). Therefore, summer precipitation in the watershed is a factor that controls photosynthesis and consequently the water pH of the lake. Phosphorus concentration may also be governed by air temperature because the dissolution of silicate depends on temperature in chemical weathering process. We thus conclude that both higher precipitation and

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higher temperature potentially increase the inflow of phosphorus to the lake, enhancing primary production, and thus increases the lake water pH.

Comment 5: Discussion 4.2 The spectral analyses are not sufficiently explained. What is the coherency and the limit of the 95% c.i for the coherency? Only the spectral density is shown. Similarly, the results of cross correlation analyses are not shown and the error bars are not indicated. What is the record used for the cross correlation (the precession index? The insolation?...). The authors also indicate : “The strong precession signal agrees with that postulated by the hypothesis that the monsoon is regulated by insolation variation at low latitudes (Kutzbach, 1981).” However, based on the Fig. 5, the precession signal is weak in comparison to the obliquity signal. This is different to what is observed in East Asian d18O speleothem records. The authors indicate that: “our new record of CBT-based pH was synchronized with the Tp record, implying synchronous variation of precipitation and temperature”. However they do not furnish any explanation for the different timing observed between the Cryptomeria record and their CBT-record (figure 6).

Reply: The discussion of spectral analysis will be removed in the revised manuscript.

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Interactive comment on Clim. Past Discuss., 10, 1153, 2014.

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