Clim. Past Discuss., 10, C1074–C1081, 2014
www.clim-past-discuss.net/10/C1074/2014/

Climate of the Past Discussions Open Access

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.

T. Ajioka et al.

ajioka@ees.hokudai.ac.jp

Received and published: 18 July 2014

We thank Referee #2 for his/her review. The comments help us to improve our manuscript.

Comments by Reviewer #2 and reply

Ajioka et al present a combined record of temperature and precipitation in Japan over the past 282 kyr based on GDGT distributions in a sediment core from Lake Biwa. With GDGT-based paleothermometry they have used a state-of-the-art method with a lot of potential. However, they do not meet the required level of discussion and interpretation that their record is worth. In my opinion, main flaws of this manuscript are 1) the very large uncertainties of the age model, which are in fact a lot larger than the trends and

cycles that are being discussed, 2) the unpublished local MBT/CBT calibration that is used to derive the presented temperature and pH records and other people than the ones on the manuscript have no idea of, and 3) the sources of the GDGTs in the lake sediments are not well constrained. All other major comments are a result of these main points of criticism. I suggest that the authors wait until the local calibration is published, improve the age model, and extend the discussion/interpretation of the record before this paper should actually be considered for publication in Climate of the Past (or publication in general). I thus suggest to reject the paper in this state, with the encouragement to resubmit once the age model has been updated, the used calibration has been published, and the records are more thoroughly discussed.

Reply: The referee pointed two critical things. First, the paper of local calibration was 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 uncertainity 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 the 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 discuss the variation in this interval. From 50 ka to the present, the age uncertainity is small enough to discuss the variation on orbital timescales. From 143 to 50 ka, the age uncertainity (2σ values [95% confidence level]) of core BIW08B ranges from 5 to 11 ky because of large error of radiogenic ages of tephras. However, the pollen composition (Tp and Chyptomeria abundance) in core BIW95-4/BT nearby the study core 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 MD01-2421 and Lake Biwa cores 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:

Introduction (page and line numbers are based on the printer friendly version) -p1155, line 11-20: add references to proxy examples in line 11-13. Move the lines on GDGTs in lakes until after they have been introduced. Summarize the findings based on the mentioned proxy records from Lake Biwa so far and identify the open question. What is the aim of this research?

Reply: We will add references and reorganize the introduction in the revised manuscript according to this comment. The purpose of this study is to add new paleoclimate records based on new proxies to reveal a new aspect of the East Asian monsoon variability.

- p1155, line 23: brGDGTs can not (yet) be attributed to Acidobacteria. So far, only one type (i.e. brGDGT-Ia) has been found in a few Acidobacteria cultures (Sinninghe Damste et al 2011, AEM). All other types are still orphan.

Reply: We will change Acidobacteria to Bacteria.

-p1156, line 10-18: This calibration first need to be published before it can be used to actually reconstruct a paleotemperature record. Furthermore, I do not understand how different GDGT distributions in soils and lake sediments can have similar relations with environmental parameters? What is the influence of in situ GDGT production in the lake? And where in the lake are they being produced?

Reply: Ajioka et al. (2014) was recently published in Organic Geochemistry (vol. 73, page 70-82). For readers' convenience, 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. Ajioka et al. (2014) suggests that branched GDGTs are mostly produced in the lake.

-The different monsoon systems that influence Lake Biwa need more introduction, as does the actual research question. What hypothesis are you actually testing with the generated GDGT records?

Reply: We probably do not understand this comment. We try to test the hypothesis of Kutzbach (1981), Wang et al. (2001), Yamamoto (2009), Clemens et al. (2010), etc.

Materials and methods - p1157, line 5: 118 rivers flow into the lake. Recent studies indicate that brGDGTs are also produced in rivers (e.g. Kim et al., 2012, GCA; Zell et al., 2013 L&O; De Jonge et al., 2014, GCA), and can influence the brGDGT signature stored in river fan sediments. I guess this potential fluvial contribution is/should be discussed in the Ajioka paper with the local calibration used here. . ..?

Reply: Ajioka et al. (2014) showed that coarse-grained lake sediments and all of river sediments have an intermediate composition between typical lake sediments and soils. Because the sediments are fine in the study core, we assume that terrestrial contribution is negligible in the study core samples.

- p1157, line 20: It seems like the age model as published by Takemura et al. has been adjusted based on personal comments of Kitagawa. If so, please mention the changes made to the original age model, and how this influences the interpretation of associated records.

Reply: The age model was improved by adding radiocarbon dates. After the submission of the original manuscript, we further improved the age-depth model by adding the age controls of tephras and paleomagenetic events. We will describe the improved age-depth model in the revised manuscript.

-p1158, line 5: The age model of this core is not well enough constrained to perform the spectral analysis and subsequently draw conclusions from these analysis. The uncertainties are simply unacceptably large.

Reply: Because the period before 143 ka is not well dated, we will not discuss the variation in this interval. From 50 ka to the present, the age uncertainity is small enough to discuss the variation on orbital timescales. From 143 to 50 ka, we will discuss the variation with caution of this limitation of age-depth model in the revised manuscript.

-p1159, line 21-25: I find it unacceptable to use a calibration that has not yet been published. For anyone not on the paper it is now unclear what the calibration is based on and how these equations have been derived. Also, what are the calibration errors of MAT and pH?

Reply: Ajioka et al. (2014) was recently published. The average root mean square errors of MAT and pH are 3.5°C and 0.5, respectively. We will add sentences in the revised manuscript.

-p1159, line 26: what is the analytical accuracy based on?

Reply: The analytical accuracy is an averaged standard deviation of repeated measurements.

Results - What is the pH from the soils in the lake catchment? These data are needed to constrain the sources of the brGDGTs in the lakes later on.

Reply: The pH in the surface soils ranged from 3.3 to 8.0 with an average of 5.0.

-The BIT index and MI index are presented in this section, but are not further interpreted in the discussion section. If these data are not used in the paleoclimate discussion then exclude them from the paper.

Reply: BIT will be discussed in the revised manuscript. MI will be removed.

Discussion -section 4.1: In this section I expected to find out what the CBT-derived pH signal would represent. Is it catchment soil pH, water pH, or sediment pH? Where in the lake (catchment) are they produced, and how do they influence brGDGT signal stored in the sedimentary record? Instead, the section is about controls on lake water pH, and except for in the title, GDGTs are nowhere mentioned in the section.

Reply: In this section, we discuss the factors affecting lake water pH. We will change the title of section. The CBT-based pH record is discussed in the next section.

- section 4.2: In the previous section the authors conclude that photosynthesis is the major factor controlling water pH in Lake Biwa. In this section, pH is used as a proxy for precipitation, whereas the link between photosynthesis and precipitation has nowhere been made.

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

- p1162, line 6: replace . . .delayed behind. . .. By . . .lags. . .

Reply: O.K. It will be revised.

- p1162, line 6-7: I don't think it is fair to do such kind of spectral analysis with an age model uncertainty that is larger than the cycles in the spectra that are being extracted from the record.

Reply: Indeed. The discussion of spectral analysis will be removed.

-P1162, line 8-9: I do not share the observation that the CBT record varies similar in timing with the Tp record. For example, I see a clear offset between the records during the MIS5-MIS4 transition (>20kyr), and also the LGM appears to be later (∼10kyr) in the CBT record than in the Tp record. As a consequence, the subsequent statement that East Asian summer temperature and summer precipitation varied in concert is not convincingly supported. The offset between temperature and precipitation is observed in practically all other records from East Asia, and can potentially be reassessed by directly comparing the CBT-derived pH (if proven that this truly represents precipitation) with the MBT/CBT-based temperature record. Since both records are based on the same set of molecules, lags and leads can be determined unrelated to age model (e.g. cf. Peterse et al., 2014, QSR).

Reply: We agree with this comment. Our interpretation will be revised thoroughly. Variation pattern in CBT-based pH was consistent with that of Chryptomeria pollen abundance in core BIW95-4/BT from 130 ka to 55 ka (Fig. 8; Hayashi et al., 2010a, b) and delayed behind that of the Tp value in cores BIW95-4 and Takashima-oki BT in Lake Biwa (Figs. 7 and 8; Igarashi and Oba, 2006). On the other hands, variation pattern in CBT-based pH was not consistent with that of Chryptomeria pollen abundance but with Tp value in core BIW95-4/BT from 55 ka to the present (Fig. 8; Hayashi et al., 2010a, b).

-p1163, line 3-4: what is the reason that the MBT/CBT proxy generates winter temper-

atures? Has this specific equation been calibrated on winter temperature? Or do they just happen to underestimate MAT?

Reply: By applying global and Lake Biwa soil calibrations, the estimated pH values in lake surface sediments by CBT fit the winter pH values of lake water, but the application of lake calibration (Tierney et al., 2010) was not successful. By applying global and Lake Biwa soil calibrations, the temperature estimated by MBT'/CBT fit winter temperature of lake water. Ajioka et al. (2014) thus conclude that the branched GDGTs in Lake Biwa sediments are produced in winter, CBT and MBT reflect the winter pH and temperature of lake water, and soil calibration is applicable to Lake Biwa sediments.

-p1163, line 13-14: eccentricity cycle of East Asian winter monsoon climate: please extend the discussion and support this statement with data from the literature.

Reply: This discussion will be removed because of reorganization.

-p1163, line 17-18: please discuss the differences and similarities between the record of Kuwae et al and your record. Which one is more reliable?

Reply: Discussion on Kuwae et al. (2004)'s record will be removed because of reorganization.

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