

## ***Interactive comment on “Holocene weak summer East Asian monsoon intervals in subtropical Taiwan and their global synchronicity” by K. Selvaraj et al.***

### **Anonymous Referee #1**

Received and published: 2 October 2008

The manuscript attempts to address an important paleoclimate question related to Holocene East Asian monsoon and its global teleconnections using a lake record from Retreat Lake in Taiwan. However, there are several major problems with the manuscript, including the inadequate proxy interpretations and unsupported conclusions, and potential duplicated publication. Here I just list some of these concerns.

1. Proxy interpretations. The authors claim that TOC and C/N ratios from lake sediments can be used as a proxy of EAM intensity, but provide no adequate explanations and evidence in this manuscript and in the previous paper (Selvaraj et al., 2007) on the same data set. In this manuscript, the authors indicate that "The content of TOC and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



C/N ratio in lacustrine sediments are valuable indicators of monsoon changes because climate, nature of lake catchments vegetation (C3 versus C4), and lake morphology largely control these parameters (e.g. Meyers and Lallier-Verg'és, 1999). Since the lake is situated directly on the moisture-carrying southeasterly down-winds of EAM from the WTP, higher contents of TOC and C/N ratio are indicative of vascular plants dominance and, in turn, strong summer monsoon (Selvaraj et al., 2007)." (page 933, lines 10-16). In the Selvaraj et al. (2007) paper, a similar simplistic statement was made "High TOC value and C/N ratio, and associated higher negative carbon isotope ( $\delta^{13}C$ ) values, for example, are interpreted to reflect vascular plant (C3) dominance due to increased monsoon precipitation during periods of warm/wet climate." Why TOC and C/N ratios would indicate EAM change? Are there C4 plants in the catchment? Any reference? Why any contributions of C4 plants to lake organic matter or TOC would result in carbon isotope values from about -31 to -27 per mil from the same core as presented in Fig. 1 of Selvaraj et al. (2007)? Could high TOC and high C/N reflect more inwash of terrestrial organic matter into the lake during heavy storms that may be associated with stronger monsoon? If so, then why didn't the lake produce its own organic matter within the lake in a subtropical setting? I don't think that the authors made their case in their proxy interpretations based on modern-process studies or on sound ecology and geochemistry.

2. Dating and chronology. It is difficult to evaluate and understand the dating and chronology of the core without seeing and discussing sediment lithology. The two dates/ages of 8599+/-189 cal BP and 8640+/-88 cal BP are identical statistically. As such, the derived chronology of about 20-30 cm sediment deposition within apparently 41 years is unjustified. Why the lake has such a rapid sediment-accumulation rate? Could this be simply a single event caused by a landslide or underwater slope failure? Without presenting and discussing lithology, it is very difficult to evaluate this. Therefore, the rapidity around 8600 cal BP as claimed by the authors could just be a dating artifact and not a real pattern. Also, how deep is the lake in causing the desiccation and hiatus at 4-2 ka? I don't find such information from both papers. Did I miss anything?

3. Explanation and conclusions. If the chronology and proxy interpretation were acceptable, the authors fail to discuss why the EAM at their site shows a sudden change in intensity at 8.6 ka, while most other monsoon records in the region (e.g., Dongge as cited in their 2007 paper) show much earlier increase in EAM intensity? What causes this difference? Also, the identification of weak monsoon events in the manuscript is subjective at best, e.g., events at 8.2 ka and 2-1.6 ka. Also, the proposed correlations with other records are weak to non-existent. The arguments presented cannot support the proposed connection and synchronicity. For example, how would C/N ratios have any connection with atmospheric CO<sub>2</sub> concentration as implied in Fig. 3b? Correlation (they look similar) doesn't necessarily mean causation!

4. Potential duplicated publication. This manuscript presents almost identical data as in the paper by the same authors (Selvaraj et al., 2007). Only difference is the slightly higher resolution data for TOC. All other curves for correlation were presented in the previous paper. Also, there are no new interpretations and conclusions supported by these existing and new data.

In summary, I don't think this manuscript should be published in any refereed journal, so I recommend its rejection for publication in *Climate of the Past*.

---

Interactive comment on *Clim. Past Discuss.*, 4, 929, 2008.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)