

Interactive comment on “Late Holocene environmental reconstructions and the implications on flood events, typhoon patterns, and agriculture activities in NE Taiwan” by L.-C. Wang et al.

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Both reviewers recognized that this paper contains interesting results. The authors report the pollen and diatom records from a sediment core retrieved from a small shallow lake in the Ilan plain of NE Taiwan. Based on the results along with some published results (Chen et al., 2012, JQS), the authors discuss the local environmental change, the frequency of flood events, typhoons, and agricultural activities.

The first reviewer recognizes the significance of the data presented in this paper but

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also mention that he/she cannot validate the interpretation because of poor writing.

The second reviewer also recognizes the significance of the data but gave critical comments on how to distinguish normal flood and typhoon events, the validity of age-depth model, and inconsistency of other dry-wet records. I have the same questions. The author should address all of these issues by adding more explanations, or changing the interpretation.

In addition to reviewers' comments, I have the following comments. First, the pollen and diatom records reflect local environmental changes in the study area. According to the data and the author's interpretation, the study site changed from an alluvial plain to a shallow lake environment. Diatom frustules are a good indicator of lake water, indicating that shallow lake was situated at the study site in the late 10th century and after 15th century. The authors should describe the development of depositional setting based on lithology and diatom fossils. Based on limited information in this paper, the shallow lake (2 m deep) environment is just a local expression of the transient phase of the development of the alluvial plain at the study rather than the expression of regional change. Fig. 4 shows that drastic changes in pollen assemblage were associated with the establishment of shallow lake environment. I wonder that pollen assemblages reflect the local depositional environments rather than regional vegetation. I also wonder whether or not the wet-dry information is successfully extracted from pollen and diatom assemblages. I thus suggest the author to interpret what pollen assemblage implies with careful consideration of local depositional environment.

Second, the some results of the study core DH7-B were already reported by Chen et al. (2012, JQS). The authors should cite Chen et al. (2012) in a proper way. All of the radiocarbon dates were reported in Chen et al. (2012). Thus, the description should be located in “results” but in “method” with citation of Chen et al. (2012). The discussions on the grain size, typhoon, and the linkage between typhoon signal and ENSO are the same as those in Chen et al. (2012). The authors should clearly indicate that they borrow the discussion from Chen et al. (2012).

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Third, the discussion on the linkage between pollen-PC2, typhoon signal (sand content) and climate parameters is speculative. I suggest to remove the discussion and focus on agricultural development or to rewrite it thoroughly. 1) The description of modern linkage between the typhoon precipitation in Ilan area and the ENSO is not enough in introduction. The diagrams showing the monthly precipitation in typical El Nino and La Nina years are helpful for readers. 2) Explain why the typhoon record from the study site is different from that from south Japan (Woodruff et al., 2009, QSR, 28, 1774-1785). 3) The SOI index by Yan et al. (2011 Nature Geoscience) is a grain size record in lake sediments from a northern South China Sea island; larger grains were deposited in heavy rainfall events. The record indicates heavier precipitation (larger grain size) in the LIA and drier (smaller grain size) in the MWP. In contrast, the PC-2 suggests wetter climate in the MWP. What caused this difference. 4) The speleothem record from Wanxiang Cave (Zhang et al., 2008 Science, 322, 940-942) rather than the Dongge Cave record may have better correlation with Ilan records. If this is true, it suggests the linkage with the East Asian summer monsoon.

I recommend the authors to thoroughly revise the manuscript by considering reviewers' and my comments. I encourage a submission of the revised manuscript.

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