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Climate of the Past Discussions

Interactive comment on "Holocene hydroclimate reconstruction based on pollen, XRF, and grain-size analysis and its implications for past societies of the Korean Peninsula" *by* Jinheum Park et al.

Anonymous Referee #1

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The authors Park et al. used a sedimentary record from Miryang in the Korean Peninsula to describe climate induced hydrological changes for the Holocene period ca. 8.3-2.3 ka BP and indicate shifts in the human population with changing intensity of the East Asian Summer Monsoon (EASM). They mainly used a pollen record in combination with high-resolution titanium XRF scanning data and grain size variations to decipher the influence of the Kuroshio Current in the Pacific Ocean and El Nino Southern Oscillation (ENSO) in connection with solar forcing that influenced the development of EASM-regulated hydrologic variations. A summed probability distribution indicates

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correspondence with changes in the local population in response to climate variations. This manuscript is well written, logically structured and provides reasonable explanations for the influences of different signals on hydrological variations and EASM impact. However, there are several aspects which need to be considered:

1. The chronological frame is not well explained. For example, there is no information how calendar ages related to OSL dating were connected with radiocarbon ages (uncalibrated) to develop a Bayesian age model (Blaauw and Christen, 2011). Furthermore, any explanation about potential reservoir errors is lacking. 2. With respect to the table1, it remains open, how the authors calibrated the 14C ages. Did they use 1 or 2 sigma uncertainties, and did they report mean, median or weighted mean values? How do these values differ from the Bacon age model? A column should be added to show this. The uncertainty values (in table 1) for the calibrated values are somewhat strange. They should check and correct it, while mentioning this in the results part under 4.1 Chronology. By the way, the age-depth model in figure 2b needs a readable age axis. It is impossible to read it because the axis description is far too small. Furthermore the authors should explain that the reported ages later in the discussion part refer to calibrated (cal.) ages BP or not. How did they deal with OSL calendar ages in this respect?

2. The explanation in the methods part is not sufficiently provided. How did they drill and how long were the core sequences? Furthermore how did they deal with potential sediment loss/overlapping at the boundary between the core segments? Finally how did they splice the different core sequences towards a composite one?

Furthermore, how was the instrumental setting for detecting titanium signals by XRF scans?

3. In chapter 4.2 the authors described the selected zones based on the provided data. This part is partly mixed with interpretation of data variations. The authors should perhaps change the title of chapter 4 (Results) to Results and interpretation. Furthermore,

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I wonder why the authors did not provide graphs for the clay and silt fractions in addition to the sand fractions. In line 149 they mention that the sediments mainly consist of clay. This would we worthy to demonstrate this by the clay and silt fraction graphs. They could be attached to figure 2a.

4. Lines 229-234: The authors refer to a cooling trend around 6.4-6.0 ka BP and mention that this is not seen in other records. They used an example (lines 232-234), but to my understanding this explanation supports their finding. So, what are the differences? This part of the discussion remains not very clear and shall be considered for revision.

5. All parts of the discussion strongly rely on their provided chronology. Hence it is important to explain in more detail whether this chronology is reliable (see comment no.1).

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