

Interactive comment on “Evidence for a widespread climatic anomaly at around 7.5–7.0 cal ka BP” by Mei Hou et al.

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Hou et al. compiled 47 paleoclimatic records of temperature, precipitation, and sea level from around the world to search for evidence for a global climatic event around 7.5–7.0 ka BP. The authors discuss the evidence for the event in continental-scale regions around the world, and discuss various possible forcings (e.g. changes in solar irradiance triggering feedback loops or changes in deep water formation, a volcanic eruption, and meltwater pulses influencing AMOC). We find the overall ambition to be well placed, and the paper is well structured and generally well written. However, we strongly agree with referee #2: the design of the study is flawed. The paper begins with the conclusion that an event exists at 7.5–7.0 ka BP; the authors then select records that they contend show an event around that time. However, in many cases, there is

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little or no convincing evidence for an “event” within their chosen datasets. We have four primary concerns with the study.

The authors need to define what is meant by an “event” and then apply an objective test for its presence or absence. Ideally, the test would be quantitative (significantly different than random, e.g. the 8.2 ka study of Morrill et al., 2013), but a rigorous qualitative test would also work. To our eyes, only 11 out of the 47 records presented show a convincing event near 7.5-7.0 ka (i.e., Qinghai lake, Guliya ice core, Yellow Sea, Dajiuhu peat, Nordan’s pond bog, Kilimanjaro ice core, Padul Lake, Eastern Alps, mid-European high level scores, Laurentide ice sheet sea-level contribution, SE Sweden relative sea level). The others show fluctuations at 7.5-7.0 ka that are indistinguishable from any other 500-year-long period in the time series. After an objective test is implemented, a map showing the spatial distribution of the results of the test would help both the authors and the readers in interpreting the underlying climate dynamics. The selection criteria for the records appear subjective, “We exclude those records that do not provide convincing evidence of an event across this interval [7.5 – 7.0 kBP]” (line 113). It is unclear how 47 records were selected out of the thousands that exist globally. Critically, the authors must discuss the implications of why so few studies show an event during this interval. If only a small subset of global paleoclimate data show an event in this interval, what does that imply about the global nature of this event? Moreover, the current stated screening criteria do not appear to have been universally applied in the study. Some of the selected records do not appear to meet the criterion of “sampling resolution of better than 200 years” (line 119). Finally, the regional treatment of records was variable. For example, only three records for North America were included. One potential avenue for strengthening the study would be to focus on one region or one climate feature instead of reaching for a global conclusion on a shaky foundation. The sentence “Therefore, in some cases, we have followed the authors’ original interpretations of paleoclimate records and have not made any corrections” (line 137) is concerning. It is unclear whether some interpretations should be considered suspect or whether the authors did make any corrections. Moreover,

any corrections made should be clearly articulated and changes to the original interpretation need a solid scientific justification. Finally, the study is framed as providing insight into future abrupt climate change, but other motivations may be more convincing. Understanding Holocene climate variability and the climate dynamics associated with certain forcings are important, and certainly motivate this study and others like it. Additionally, the language used throughout the paper is often strong (e.g., lines: 532, 630, 738) and inconsistent with the uncertainty apparent throughout the manuscript. Finally, we suggest standardizing the figures' visual style (at present there are many varied styles for presenting each time series), providing more details in the captions, and checking the data. For example, the eastern Alps temperature anomaly shows a curious horizontal bump around 7.6 ka BP that would suggest an age reversal.

For these reasons, we feel like the paper is not suitable for publication until the shortcomings described above are addressed, which requires extensive changes. Regardless, we wish the authors well in their future research.

Morrill, C., et al. "Proxy benchmarks for intercomparison of 8.2 ka simulations." *Climate of the Past* 9.1 (2013): 423-432.

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