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Climate of the Past Discussions

Interactive comment on "Defining the Little Ice Age" *by* Ø. Paasche and J. Bakke

Anonymous Referee #3

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In this paper, the authors aim to provide a "better definition of the Little Ice Age, and hence a better dynamical understanding". It is indeed true that there is a lot of confusion about the temporal and spatial characterization of the LIA, and an improved definition is therefore welcome. The authors first show that glacier records are not very suitable to make a generally valid definition of the LIA. Instead, they base their analysis on six previously published climate reconstructions that reflect changes in atmospheric circulation.

Unfortunately, as I explain in detail below, I am not convinced by their analysis of the LIA. In my view a much more comprehensive global analysis is required to see if a definition of the LIA can be established. I do not see how this can be resolved by a revision of the present manuscript, and I therefore I cannot support publication of this paper in Climate of the Past.

Major comments A) In my opinion, the 6 records in Figure 3 do not convincingly show that there was a synchronous shift in atmospheric patterns at 1400 AD and again at 1800 AD. - Fig 3a: the Greenland ion record decreases clearly before 1800 AD. - Fig. 3c: the Lake Bosumtwi record shows shifts clearly after 1400 AD and before 1800 AD, so the anomaly associated to the LIA according to the authors is shorter. - Fig. 3d: the anomaly in the Lake Edwards record starts later than 1400 AD - Fig. 3c: the anomaly in the Galapagos record starts clearly earlier than 1400 AD and ends later than 1800 AD. - The LIA-anomaly in the Norwegian glacier reconstruction starts earlier than 1400 AD and ends clearly before 1800 AD. On the one hand, the differences in timing between the records could (at least partly) be related to the uncertainty of the individual chronologies. On the other hand, it is very well possible that the climate anomaly was time-transgressive, meaning that it started in one region and then extended to other regions, leading to leads and lags. In fact, the authors make an a priori assumption that the LIA is a synchronous event across the globe, while this is highly uncertain and even unlikely. In my view, this assumption hampers our search for a better dynamical understanding of the LIA.

B) Six records are not sufficient to provide a definition of the LIA. In my view, a global compilation of high-quality records is required. This compilation should make clear if the LIA was truly a global event. And the uncertainty in the chronologies of these records should be taken into account, because this sheds light on the temporal relations between anomalies at different locations and would clarify if a clearly defined time-window is associated with the LIA. Timing at different locations is essential if we want to improve our dynamical understanding of the LIA.

Minor comments 1) Modelling studies should be included in the discussion, as these provide relevant insights for this topic. For instance, Goosse et al. (2005, QSR 24, 1345-1360) show that, in simulations of the last millennium forced by relevant external forcings, the noise of the climate system at local and regional scales often overwhelms the forced variability, making it unlikely that a climate anomaly such as the LIA is syn-

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chronous at different locations.

2) The authors claim that the role of the atmosphere is dominant. But how do the authors know that the atmosphere is not slave to oceanic variations?

3) No explanation for the supposed co-varying of atmospheric phenomena is provided. This paper should at least discuss various options.

4) The record of Meeker and Mayewski (Fig. 3a) is a proxy for the Siberian High, not the Northern Annular Mode.

Interactive comment on Clim. Past Discuss., 6, 2159, 2010.