Clim. Past Discuss., 4, S606–S609, 2008
www.clim-past-discuss.net/4/S606/2008/

Climate of the Past Discussions

Interactive comment on "Were last glacial climate events simultaneous between Greenland and western Europe?" *by* M. Blaauw et al.

M. Sanchez Goñi

mf.sanchezgoni@epoc.u-bordeaux1.fr

Received and published: 3 December 2008

The manuscript by Blaauw et al. has the aim of evaluating uncertainties in age models for terrestrial and marine archives and discussing the implications of these uncertainties for our understanding of climatic mechanisms. The authors use the palaeoclimatic sequence of Les Echets to propose that the uncertainties derived from age determination errors and calibration methods (Hughen06 and Fairbanks05) are so large in paleoclimatic records that we should assume, as the more parsimonious explanation, asynchroneity between sequences in general and between the D-O climatic variability and its regional expression outside Greenland in particular. According to them the peak-to-peak correlation between Greenland ice core archives and Mediterranean and North Atlantic marine and terrestrial records must be considered as a mere working

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

hypothesis and for this reason “avoided, if possible“. Only well-defined tephra layers would significantly reduce the uncertainties in the correlation between archives.

While in principle one can only agree with their position (after all some delay will necessarily occur between a triggering cause and its consequence) one can wonder how long this delay should be in order to be considered relevant for the study of paleoclimate and whether we will be able one day to identify it. In other words, the main question here is whether their stand may help to clarify chronological relations between archives and to understand underlying mechanisms or will just create confusion by promoting the idea that each archive is unique and that, due to asynchroneity, mechanisms are largely out of our grasp.

Blaauw and collaborators warn that “the assumption of synchronicity between regions eliminates any potential for objective evaluation of relative timing in different time scales“. It may well be possible, however, that some synchronicities have been already well established and that their analysis makes possible the understanding of crucial mechanisms, i.e. that the Blaauw et al. stand may consist, if taken at face value, in throwing the baby out with the bath water.

Basically, confusion comes in my view from the fact that all archives are given the same reliability from a chronological point of view. The authors do not seem to be aware that some peak-to-peak correlations proposed in the literature, namely those of the Iberian margin, are based on robust stratigraphical grounds. Shackleton et al., (Paleoceanography, 15: 565-569, 2000) and Vautravers and Shackleton (2006) demonstrated a remarkable coupling between sea-surface-water temperature (SST) changes, $\delta^{18}O$ of planktic foraminifera, in this margin and stadial-interstadial changes in the Greenland ice core (see discussion in Skinner, The Climate of the Past, 4: 791-807, 2008). Both records show very abrupt interstadial events implying that the polar front must have migrated northward extremely rapidly (Shackleton et al., 2000). This rapidity of shifts in the Northern Hemisphere atmospheric circulation has been recently confirmed for Greenland Interstadial (GI) 1 and the Younger Dryas (YD)-Holocene transition where

CPD

4, S606–S609, 2008

changes of 2 to 4 kelvin in Greenland moisture source temperature from one year to the next have been observed (Steffensen et al., Science 321: 680-684, 2008). Therefore, it can be reasonably assumed that rapid warming events in the Iberian margin were largely synchronous with those observed in Greenland. These western Iberian margin sequences also provide robust markers, the Heinrich layers, for identifying without any chronological ambiguity major Greenland Stadial (GS) - GI transitions. In this case synchronicity is indeed the most parsimonious explanation. Blaauw et al. gives the impression that pollen records reported by Sanchez Goñi et al. (2002) in south western Iberia have been tuned to GI events, which is not the case. Cores MD95-2042 and MD95-2043 were dated independently of pollen data on the basis of the close stratigraphical resemblance between $\delta^{18}O$ of planktic foraminifera and $\delta^{18}O$ of Greenland ice (Shackleton et al., 2000) in the first core, and the high parallelism of the SST changes in the Alboran Sea with the GI events (Cacho et al., Paleoceanography 15: 565-569, 1999) in the second core. Additionally, in the later case, a second age model based on oxygen isotopic stratigraphy (three isotopic events within Marine Isotopic Stage 3) and four AMS ^{14}C ages show perfect consistency with the age model based on peak-to-peak correlation.

The authors recommend the use of common time markers such as the well-defined tephra layers to reduce the chronological uncertainties. In an ideal world, cross-correlation of different marine, ice and terrestrial archives sharing the same well-identified tephra layers will be the panacea to test whether the events are synchronous at decadal and multi-centennial time-scales. Unfortunately, our experience in the framework of RESOLuTION, an ESF-EuroCLIMATE project in which some of the authors of this manuscript were involved, has shown how difficult, not to say impossible, it was to find common tephra or microtephra layers between archives located in a relatively small region such as that including western Europe, the North Atlantic mid-latitudes and Greenland. Additionally, when tephra layers do occur, they often do not coincide with climatic transitions, which makes their use as stratigraphic markers less valuable. This implies that we cannot escape isotopic stratigraphy to establish corre-

CPD

4, S606–S609, 2008

lations and must preferentially work on sequences, such as marine cores, in which proxies from various reservoirs are preserved in the same samples.

As far as the Les Echets sequence is concerned, I see two problems. Firstly, I do not clearly see in what way the Les Echets record contributes to the above discussion. A case for asynchroneity could have been made without presenting the results from Les Echets. The uncertainties in the age model of Les Echets in spite of the commendable multiproxy study of this sequence and the admirable work done by the authors to refine its chronology do not imply that synchronicity between sequences cannot be proposed in cases in which we have more reliable ways to tune chronology. It only demonstrates, once more, that terrestrial sequences, even when studied in the best possible way, are difficult to correlate to wider past climatic events. Secondly, it seems to me that stuck in their pessimistic stand, the authors do not put enough effort in comparing their results to other records. I suspect that if they would have done so interesting observations could have arisen concerning both the chronology of their sequence and the impact of climatic events of the last climatic cycle in the hearth of Europe.

In conclusion, since the authors propose no practicable way to quantitatively evaluate time lags between sequences their call for a more “rigorous“ approach becomes an implicit invitation to avoid all type of correlation. I am worried by some obvious and many unexpected consequences that this view can have on the discipline. I suggest the authors take into account these remarks before the paper is accepted for publication by The Climate of the Past.

Interactive comment on Clim. Past Discuss., 4, 1203, 2008.