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Climate of the Past Discussions

Interactive comment on "Interglacial and glacial variability from the last 800 ka in marine, ice and terrestrial archives" *by* N. Lang and E. W. Wolff

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We thank the reviewer for this positive review and for his useful suggestions. Comments by the referee are labelled (R) and followed by the authors' response (A).

R: This is an extremely useful compilation and insightful discussion of ice, marine and terrestrial records of the last 800 kyr. Although only few records satisfy the criteria of completeness and detail over the entire 800 kyr, they nonetheless begin to provide a sense of some emerging general patterns (pre-MBE vs post-MBE; strong interglacials following strong glacials), but also of the lack of an underlying pattern (e.g. lack of an archetypal interglacial). This review also highlights the dearth of information from terrestrial records. Given that specific regions respond differently from any global integration, regional records will be extremely important in establishing the robustness

of some conclusions, as the authors point out. The MS is extremely well written and argued. I have only some minor observations.

R: Discussion Paper According to the authors, the procedure followed for indexing interglacial and glacial minima was: "The minima and maxima identified were those nearest to the transition, and therefore do not necessarily correspond to the absolute maximum or minimum within a particular stage. These values were identified (a) in the raw data, i.e. single point maxima and minima were included; (b) in records averaged to 1 ka to ensure we were considering sustained values in the records with highest resolution, and (c) using the statistical RAMPFIT programme to determine the values of maxima and minima: : :The differences in the strength of interglacials (glacials) found using each of these three methods were not significant, and all results shown in this paper are from the raw data." This approach would seem to provide the same answer if the maximum (minimum) were achieved near the terminations, but I wonder to what extent the picture is different for records where the peak values occur later (earlier) in the interglacial (glacial).

A: This is a good point, and recognising this the later peaks are discussed in sections 4.4 and 4.5 of the manuscript for some key MIS such as 7, 13 and 15. The values identified in all stages are broadly synchronous within e.g. marine records, but the ages of the identified peaks vary more widely between e.g. marine and terrestrial records due to differences in the response and signal recorded. Because of the variability, it is specified in the paper that we chose the peaks nearest to the termination. If interested, the reader could examine the effect of later (earlier) peaks in the supplemental data table we intend to add.

R: I have also checked some of the peak values provided for one of the sites (Tenaghi Philippon). In Table 3 most values given are correct, except MIS 9 and 15, where higher values have been recorded (95.5% and 95.18%, respectively), although this makes little difference in the classification. In Table 4, the lowest value I could find from MIS 2 was 5.19% (not 2.09%) and 1.33% (not 2.12%) for MIS 16. This is more in line

with our current understanding of the extent of glaciations in decreasing order: MIS 12, followed by MIS 16, then MIS 6 etc.).

A: The selected value of 2.09% is at 30.8ka – the absolute minimum for MIS 2, however on reflection this is a little early for the LGM at this site. The value of 5.19% at 13.16ka (the Younger Dryas?) conversely seems a bit late for the LGM, however if this better reflects the strength of MIS 2 in records of European vegetation then we have updated this in the tables. Thank you for pointing out the error with the other values, which was made when updating the TP data from an older version to the most recent; the values have been updated in the tables and I am confident there are no other errors in the data.

R: In addition, the following points require some attention: p. 2225, l.14-16: "The emergence: : :seen in the interglacials" I understand what the authors are saying, but perhaps this could be rephrased.

A: This has been rephrased.

R: p. 2241-42: The observation that the strength of the interglacial may be partly determined by the strength of the preceding glacial is important. Perhaps the authors could refer to the conceptual model of Parrenin & Paillard (2003, EPSL) in this respect

A: We agree with the reviewer that Parrenin and Paillard (2003) is relevant here. Although they do not really discuss the strength of interglacials, they do propose that the timing of termination is controlled by a combination of ice sheet size and astronomical forcing, which implies that in times of low astronomical forcing, a strong glacial has to be achieved. Because, in their model, the transition out of the interglacial is controlled only by the astronomical forcing, periods of weak forcing tend to give longer interglacials, implying the potential for more loss of land ice and therefore a stronger isotopic signal in the interglacial. Thus it would be the weak astronomical forcing which is (paradoxically) responsible for stronger glacials and interglacials. This is an interesting possibility, and we have adjusted the text to note this.

R: Finally, one comment on nomenclature. While the ice core community has traditionally used a decimal notation when referring to Marine Isotope Substages (e.g. MIS 5.5 etc.), strictly speaking, this refers to Marine Isotopic Events rather than Substages. While in low-resolution records it may be difficult to see the difference between the two, an event refers to a point in time rather than an interval. In a detailed record there will be space between the position of successive events, whereas substages have boundaries and together fill the stratigraphic record and the time represented (see discussion in Shackleton 2006 QSR). Moreover, several sub-events may be recognized within a substage (e.g. 13.11, 13.12 and 13.13 within MIS 13a). Thus while substages have not formally been defined for all Marine Isotope Stages, the usual practice would be to use the corresponding substage notation from Events defined in the low latitude stack (Bassinot et al., 2004 EPSL) (so that Marine Isotopic Substage 19c contains event 19.3).

A: This is a good point; to clarify the text has been edited to refer to e.g. substage 7e or event 7.5, etc, as appropriate.

Interactive comment on Clim. Past Discuss., 6, 2223, 2010.