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Climate of the Past Discussions

Interactive comment on "Can we predict the duration of an interglacial?" *by* P. C. Tzedakis et al.

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Response to Reviewers' comments

We are grateful to the two reviewers for the their insightful comments and supportive remarks. Below we provide a point-by-point response to comments.

Anonymous Referee 1

Point 1. Referee 1 raises an important point that a stronger motivation for this undertaking should be provided at the outset. Clarifying the natural length of the current interglacial in the absence of anthropogenic interference, is indeed one such motivation and this has been considered in a separate publication (Tzedakis et al. 2012). A more general underlying motivation arises from the fact that past interglacials can

be thought of as a series of natural experiments in which boundary conditions varied considerably, with consequent effects on the character of climate change (Tzedakis et al., 2009). Estimating interglacial durations can therefore provide insights into climate processes and feedbacks that are relevant to glacial inception and by extension contribute to our understanding of the sensitivity of the Earth System to different forcings. We propose to modify the text to include these points.

Referee 1 is also correct in pointing out that our sentence "It is also important to clarify that this is a definition of interglacial duration rather than a definition of what is an interglacial per se" is perhaps too defensive. The underlying reason for inserting the sentence in our MS was the difficulty in providing a satisfactory distinction between interglacials and interstadials, but in the context of the present paper we agree that this is superfluous and we will remove the sentence.

Point 2. The reviewer is raising the valid point that the MOC response may be modulated by different boundary conditions and therefore different intervals may have different propensities to bipolar-seesaw variability. This indeed appears to be the case with respect to the character of millennial-scale variability during times of intermediate ice volume (e.g. MIS 3 vs early MIS 6) when the most prominent expression of such variability occurs (e.g. Margari et al., 2006). With respect to the terminal oscillation of the bipolar-seesaw, this appears to be a characteristic feature of deglaciation. Differences in geographical distribution and volume of ice sheets may be more important factors in modulating the magnitude of such terminal Heinrich events, but we would still expect them to be sufficiently prominent to be identified at every termination. With respect to the reactivation of the bipolar-seesaw, differences in ocean state may be important in determining the sensitivity to freshwater fluxes, and therefore the extent to which bipolar-seesaw events occur (in other words, iceberg discharges may occur, but do not lead to the reactivation of the bipolar-seesaw, either because of the magnitude of the discharge or because of the ocean state). This is why examination of ice-rafted detritus in marine records is also important, as we have stated in the text. We propose

to insert a short section to reflect this discussion in the text. With respect to the use of the GLsyn record of Barker et al. (2011), the reviewer is correct in pointing out a certain amount of circularity in its reconstruction. However, comparison of the GLsyn record with highly-resolved δ18O planktonic and pollen records of millennial-scale variability from the Portuguese margin that extend beyond the Last Interglacial (Margari et al. 2006) has underlined their striking similarity. We return to this point later on, in our reply to a similar comment by Referee 2.

Point 3. The reviewer is correct; we will modify the statement on the occurrence of inceptions in relation to summer insolation minima and also the last sentence in the caption of Fig. 3.

Referee 2: Michel Crucifix

Points 1 and 2. Michel Crucifix has distilled the main message of our work much better than we seem to have managed to (and we may therefore ask permission to plagiarize his phrase on "obeying some simple rules"). A short comment on the occurrence of the MIS 15c inception at the time of the summer insolation maximum: we are more inclined to think that this is an artefact of the EDC3 timescale rather than a real climate conundrum.

Point 3. We are grateful to the reviewer for setting us straight on the correct attribution of references to earlier works and we will incorporate his suggestions. We will include caloric summer insolation in our figures.

Point 4. The reviewer is correct in that the CH4 record could have been used instead of the GLsyn record (as we state in the text). One potential drawback is that the CH4 may sometimes not record all events as clearly as North Atlantic records (e.g. subdued CH4 variations during Greenland interstadials GI 19 and 20). However, the main reason for using the GLsyn record is its absolute timescale through alignment with the Chinese speleothem records. While at present this is only available for the interval 0-400 ka, we still think that it represents a sufficient advance that is worth incorporating in our

approach.

Point 5. We are grateful to the reviewer for pointing out the issues arising from the use of the term "relaxation time". As we are not able to establish whether this is a manifestation of a limit cycle or an aspect of the forcing, we agree that the term "relaxation time" is not appropriate. We propose to use the following phrase instead: "a characteristic timescale of interglacial decline, which may be seen as an aspect of internal dynamics (e.g. limit cycle) or an aspect of the forcing". We will amend the text accordingly throughout.

References cited

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