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Interactive Comment

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

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1 Summary

The study is a survey of the latest 9 interglacial periods. The question being addressed is whether their durations obey simple rules involving, for example, specific measures of insolation such as summer solstice insolation or a quantity termed 'Summer Energy' (commented on later in this review). The authors observe that the timing of the beginning of an interglacial is related to the phase of the obliquity signal. By contrast, the timing of the end of an interglacial cannot be predicted empirically. The authors, however, observe a few rules. Opposite trends in precession and obliquity tend to cause long interglacials, spanning two precession cycles; early peaks in CO_2 and Antarctic

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temperature (inferred from Deuterium) tend to be associated with short interglacials, ending about 10,000 years after the peak has been reached. Marine isotopic substage 15c is presented as a ‘conundrum’, because the relative phasing of precession and obliquity are typical of short interglacials, while this interglacial lasted about 20,000 years, possibly because insolation stayed at a too high level. This situation reminds us of the Holocene, showing once more how difficult it is to estimate the natural duration of the present interglacial.

Beyond the survey character of the study, its originality resides in using glacial variability (more specifically: the absence of) as a criteria to estimate the duration of interglacial periods. The authors rely on the Barker et al. 2011 synthetic record, controlled with additional evidence from marine records.

2 General commentary

This is a nicely written article, with a clear message: interglacial durations are difficult to predict, but there are a few rules which they seem to obey. Compared to the earlier tradition, the authors downplay the role of eccentricity and emphasise the relative phasing of obliquity and precession as a better predictor. They also introduce the concept of interglacial relaxation, commented on in the final section of this review.

Beyond a number of technical and editorial shortcomings, which can probably easily be addressed, the paper appears as a useful, authoritative synthesis of evidence about interglacial durations. I therefore recommend its publication.

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3 Note on referencing earlier works

The present article comes as one more piece of evidence outlining the importance of obliquity in controlling ice age dynamics. This might be the opportunity of paying tribute to Milutin Milankovitch's introduction of insolation over the caloric summer (see sect. 127 of Milankovitch's book, 1998 edition). This insolation quantity has properties very close to the 'Summer Energy' introduced by Huybers and referred here to by the authors: caloric summer insolation and 'Summer Energy' have almost identical power and phase spectra. Caloric summer insolation was computed by Berger as early as 1978 and it was used recently in a discussion about interglacial dynamics by Ruddiman (2011). Crucifix (2011) compares the two quantities over the last 800,000 years, and also notes the ambiguous character of the present-day insolation as a possible indicator for glacial inception, which may provide some useful background to the nice discussion p. 1071 of the present article. [1].

A number of reference choices are questionable. For example, Paillard and Parrenin (2004) is cited to support the affirmation "isostatic effects and rates of deglaciation vary between terminations, depending on the location and size of ice sheets and insolation forcing". With all respect due for their excellent article, Paillard and Parrenin do not discuss isostatic effects in this study. Early references on isostatic effects on climate include Oerlemans (1980), Birchfield (1981), Peltier (1981), Pollard (1983), to quote only but the pioneering ones (Peltier (1981) is particularly remarkable for its breadth and scope).

The reference to Ghil et al. 1987 for the ice-sheet-precipitation feedback is also arguably not the most appropriate. Ghil et al. propose a remarkable and original use of boolean delay equations (an idea that might not have been led to its full potential) and, indeed, use as a starting point an hypothesis about an ice-sheet precipitation feedback.

[1] All these insolations can now be computed easily with the R software using a package called INSOL, available from perso.uclouvain.be/michel.crucifix/software

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Ghil et al. have been advocating an ice-precipitation feedback since the early contribution of Källén et al. 1978 (see Ghil and Le Treut 1981 and references therein); in the present context, reference to general circulation modelling might be more appropriate and, in particular, to the contributions of Vettoretti and Peltier (quoted in the article) or, for a focus on the North Atlantic salinity budget, Khodri et al. 2001.

The authors also quote Archer and Ganopolski, 2005 to support the sentence “reducing CO_2 concentrations shifts the inception threshold to higher [...] insolation values". It is correct that Archer and Ganopolski provide a very neat illustration of the phenomenon (their Figure 2), but the message was already present in Berger et al. 1998.

Finally, “Bintanja and van de Wal (2005) depends on assumptions about deep-water temperatures". That is correct. Note also that this reconstruction presents the shortcoming of relying exclusively on the LR04 benthic stack record.

4 Identification of ice age variability

The authors have adopted Barker et al. 2011 as the reference for identifying glacial variability. This is a defensible choice, but not entirely free of problems. Quoting Barker et al., they “used a thresholding approach for predicting the occurrence of abrupt Greenland warming events based on minima in the second time differential of [Antarctic Temperature]". Estimating the second-order time-derivative of an observed process is particularly difficult, especially if the record is non-evenly sampled such as here. Furthermore, thresholding may create artefacts. I understand from the manuscript that the decisive advantage of Barker et al. is the synchronisation on the Chinese speleotheme record, because the latter has a solid chronology. Though, “Before 400 kyr BP, the [Barker] record uses the EDC3 chronology". So, this might not be such a big advantage after all. In conclusion, the reader is left wondering why not use the methane record straight away. The latter is supposedly giving a direct access to glacial variabil-

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ity, free of the possible artefacts possibly introduced in the processing leading to the Barker et al. record.

5 On the relaxation concept and how it may conflict with the prevailing modelling paradigm

The reference to a “relaxation" effect is intriguing. Relaxation is generally used to refer to internal dynamics following a forced event, like a spring relaxing to rest state. Here, the authors observe a consistent 10,000 year lag between peak-interglacial conditions and glacial inception. The concept implies that glacial inception is somehow decided after the interglacial peak, through an interplay involving the slow components of the climate system (supposedly: carbonate balance and isostasy). Therefore, the paper contributes to the paradigm interpreting glacial-interglacial cycles as the manifestation of a limit cycle synchronised on the astronomical forcing. There is literature supporting this view, but one has to realise that it somehow conflicts with the experimental design of most interglacial simulations (early examples with models of “intermediate complexity" include Crucifix and Loutre 2002, Khodri et al. 2003, Kageyama et al. 2004, Calov et al. 2005, the latter two modelling ice build up explicitly). These experiments are initialised from a steady state, and the glacial inception is continuously forced into the glacial inception by the astronomical forcing. Initialisation from steady state is incompatible with the relaxation and the limit cycle interpretations.

I believe that the authors have to clarify the distinction to be had between these two approaches to glacial inception, in order to give full meaning to their closing statement: “ This in turn suggests a key role for long transient simulations in exploring further the timing of different glacial inceptions."

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