

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

Anonymous Referee #1

Received and published: 31 May 2012

This is a valuable and thoughtful contribution to an important debate, and it's a paper that I will cite and will certainly find useful in the context of my own work. I suspect that others will cite it widely too.

I have the following main points:

1. Why is it important to define/predict the length of an interglacial? I think there should be a stronger motivation for the work at the outset. I suspect this hinges on the debate stimulated by the Ruddiman Hypothesis and which enters the fray only on p.1070 line 22 (“A corollary...”). The authors should be more upfront about this, and the implications of being able to predict the length of our current interglacial in the absence of anthropogenic influences, because there are important philosophical and policy issues that arise from this. The authors are somehow a little shy about this and

C493

almost seem to want to avoid the issues. The final statement in the recent “sister” paper in Nature Geosciences by Tzedakis et al. (2012) is similarly oblique: “Although verification of an imminent glacial inception will elude us at current CO₂ concentrations, it is important to reiterate that the current insolation forcing and lack of new ice growth mean that orbital-scale variability will not be moderating the effects of anthropogenically induced global warming.”

I would argue whether the sentence starting “It is also important to clarify...” on p.1062 line 29 is necessary. A semantic point I accept, but unless you can define something satisfactorily you cannot define any of its attributes (you can't define the mass of an elephant until you know what an elephant is). You satisfactorily define an interglacial in terms of ice sheet minima and therefore sea-level highstand, so I don't see why you require this slightly defensive statement about “what an interglacial is per se”.

2. Bipolar see-saw: the bipolar see-saw is central to the argument since the authors use the millennial-scale variability inherent in this mode of climate variability as the defining characteristic of “glacial” stages when ice sheets deliver freshwater fluxes to marine margins sufficient to impact the MOC. Thus, interglacials are defined as “the interval between the terminal oscillation of the bipolar seesaw and three thousand years before its major reactivation” characterized by minimal global terrestrial ice volume hence high eustatic sea level. I think it is really important to bear in mind here that there are controls on MOC stability/variability other than freshwater flux and therefore to use the lack of bipolar seesaw as the defining criterion for interglacials might be dangerous. For instance, the deep-water formation at high latitudes is balanced by vertical mixing driven mechanically by the wind field and tides (Green et al., 2009) and there are indications that the North Atlantic responded very differently to freshwater fluxes between MIS 2 and MIS6 (Green et al., 2010) probably as a result of differences in ice sheet extent, atmospheric pCO₂ and orbital configuration. Thus the MOC response to freshwater fluxes might be modulated by other forcings. It is the MOC itself and not the contributory freshwater fluxes deriving from ice-ocean interaction that drive the bipolar

C494

seesaw. So, whilst an active bipolar seesaw indicates glacial conditions a non-active bipolar seesaw might not necessarily indicate interglacial conditions. I think a qualifying statement to this effect might be helpful.

Is there a circularity with respect to the GLsyn record? This record is a synthetic construction of Greenland millennial-scale variability based on demonstrated bipolar seesaw behaviour for the time period when proxy records for Greenland and Antarctica overlap. By definition, then, it is going to demonstrate millennial-scale variability because it is derived using the bipolar seesaw model. I don't see why it is necessary to use this synthetic derivation when you could simply use EPICA instead and make the assumption that during phases of millennial-scale variability the bipolar seesaw was in operation.

3. The evidence presented in Fig. 5 indicates that glacial inceptions are spread throughout the curve of northern summer insolation intensity, so the statement starting "The empirical evidence.." on p.1066 line 20 is a bit strong.

A minor point: in the caption to Fig. 3 should the final sentence "...but its onset on the EDC3 timescale" read "...but interglacial onset on the EDC3 timescale"?

REFERENCES

Green JAM, Green CL, Bigg GR, Rippeth TP, Scourse JD, Uehara K. 2009. Tidal mixing and the meridional overturning circulation from the Last Glacial Maximum., *Geophysical Research Letters* 36: L15, 603, doi: 10.1029/2009GL039, 309 Green CL, Bigg GR, Green JAM. 2010. Deep draft icebergs from the Barents ice sheet during MIS 6 are consistent with erosional evidence from the Lomonosov ridge, central Arctic. *Geophysical Research Letters* 37: L23606, doi:10.1029/2010GL045299.

Interactive comment on *Clim. Past Discuss.*, 8, 1057, 2012.