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Climate of the Past Discussions

Interactive comment on "Terminations VI and VIII (~ 530 and ~ 720 kyr BP) tell us the importance of obliquity and precession in the triggering of deglaciations" *by* F. Parrenin and D. Paillard

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First of all, thank you very much for your positive and constructive review.

In this MS, Parrenin and Paillard present an evolved version of their earlier (2003) conceptual model, which does an extremely good job at reproducing ice volume variations over the past million years. The role of obliquity and precession is further explored by removing each one separately and this highlights their specific influence on TVI and TVIII, respectively. The paper presents an excellent set of results, with important implications for understanding the role of orbital parameters on glacial cycles. My main complaint is that the MS is rather too concise and does not fully explore the multi-

tude of important observations emerging from the experiments (Fig. 1), but focuses on Terminations VI and TVIII.

Thank you for your positive comments. The manuscript has been expanded and now cover the following aspects: (1) detailed comparison of the model with the sea level data; (2) timing and duration of terminations; (3) complexity reduction and number of parameters in the model; (4) sensitivity to initial conditions, i.e. the stochastic vs deterministic question; (5) quantitative and qualitative importance of precession and obliquity in the deglaciation threshold.

So the first question is whether the analyses presented here reveal any simple rules regarding the role of precession and obliquity at terminations. In this respect, it might be useful to refer to a recent paper by Tzedakis et al. (Clim. Past, 8, 1473–1485, 2012) on the duration of interglacials. More specifically, figure 5 in that paper shows that the onset of interglacials occurs within 2kyr of the boreal summer insolation maximum/precession maximum (maximum in the present MS notation). This observation is also echoed in the modelling results presented here, in that the Best-wo experiment is able to reproduce all terminations but TVI, where although precession peaks at the right time, the amplitude of the change is very subdued and, unlike Termination V, the ice volume is very low, as pointed out by the authors. The onset of interglacials with respect to the obliquity maximum, however, varies considerably (Tzedakis et al. 2012, Fig. 6). The onset of MIS 17 occurs âĹij15 kyr before the obliquity maximum, and thus TVIII is near the obliquity minimum, which accounts for the overriding importance of precession at that termination. In a similar vein, the onset of MIS 11 occurs 10 kyr before the obliquity maximum, and it is interesting to observe that the model experiment without precession does not perform well in terms of the timing of TV, which occurs later in their model. (A similar situation occurs at the onset of MIS 13a, though this is not a proper termination). Most of the other terminations occur near the obliquity peak, with the exception of TIII, where the interglacial onset is âĹij8kyr after the obliquity peak.

We now include a subsection on the duration and timing of terminations with ap-

propriate reference to Tzedakis et al. (CP, 2012). In summary, we do not find the same things here (not two groups of durations, not a constant phasing with respect to precession) but we are not speaking exactly of the same thing (deglaciations vs interglacials).

Against this background, the question that is raised is what is the relative importance of the two parameters. The authors indeed ask this in the MS (p.3149, l. 20), but it would be helpful to discuss it in some depth and compare it with other results (e.g. Ganopolski Calov, 2011, CP). For example a cursory look at Fig. 1d and 1g, would suggest that precession does a better job at capturing the ice volume changes.

We now discuss the relative importance of obliquity and precession in the deglaciation trigger in more details. Both seem necessary: we do not obtain a statisfactory agreement with the data without influence of both parameters. More precisely, the ratio between κP and κO, which is now displayed in a figure, is always close to 0.9, meaning that both parameters approximately have the same importance.

Beyond terminations, the paper mentions in passing glacial inceptions, but does not really address that. Again, it would be interesting to have a more in-depth discussion of that.

The manuscript is really focused on terminations. Of course, it would be interesting to study the glaciations but one cannot address everything in just one paper.

Thus, even though there is nothing wrong with the present MS, my feeling is that it doesn't quite realize its full potential and would benefit from expanding its scope. I would therefore invite the authors to consider restructuring the MS in such as a way as to discuss all terminations (rather than two special cases) and frame it in terms of general implications emerging with respect to the importance of precession and obliquity. A discussion on inceptions would also be welcome.

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The revised manuscript now addresses your suggestions.

Specific points p. 3144-3145 ". . .only orbital forcing seems important to trigger glaciation (Paillard, 1998; Khodri et al., 2001)" While I fully agree that insolation change is the primary trigger and the present model is obviously able to do this with just orbital forcing, other models require some amplifying feedback (e.g. Khodri et al., 2001).

Sentence has been reformulated.

Fig. 1g and section 3.3. In the experiment without precession, how is obliquity alone able to reproduce changes for both MIS 5a and 5c, MIS 7a and 7c, MIS 9e and 9c, etc., when these occur at precessional frequencies?

This is explained in the manuscript. Eqs (1) and (2) do contain precession frequencies. It is only the threshold equations (4) and (5) which do not.

Interactive comment on Clim. Past Discuss., 8, 3143, 2012.