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Climate of the Past Discussions

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Interactive Comment

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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Thank you for your review of our manuscript.

1 Summary The authors use a conceptual climate model of the Quaternary ice ages. It is written as a 1-D dynamical system forced by precession and obliquity, with a scalar condition that determines a climate 'state' (glaciation or deglaciation). In deglaciation stage, the forcing function is supplemented with a relaxation term driving the system to deglaciation. The forcing function itself is a linear combination of precession, a phase-shifted precession (which I propose to term : co-precession) and obliquity. It is indeed known that most classical insolations, be them daily mean or averaged over a period of

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Interactive Discussion

Discussion Paper

the year, may be approximated as a linear combination of these three quantities. The model is, in its conception, pretty similar to many phase-space models that have been published in the litterature over the past 30 years, and to which the two authors have substantially contributed.

Note: In using phase-shifted precession, we follow the wording introduced by Imbrie et al. (EPSL, 2011).

The storyline of the paper can be summarised easily. Once the model is calibrated (in a fashion much like earlier proposed by Hargreaves and Annan, 2002, see more comments on this below), sensitivity studies are carried on on the respective roles of precession and obliquity, and it is found that both are necessary to explain the timing of the deglaciations as obtained in the calibrated model. The authors infer that both precession and obliquity control the timing of terminations; more specifically that obliquity " plays a fondamental role in the triggering of termination VI, and precession plays a fundamental role in the triggering of termination VII". They also argue, based on these results that the character of the climate history of the Pleistocene is more deterministic than stochastic.

We agree with your summary.

2 Commentary about the 'deterministic/stochastic character' Scientists interested in conceptual models of ice ages have learned by experience that the exact timing of terminations is sometimes overly sensitive to model parameters or forcing function choices. Paillard himself admitted that the truncation of the forcing function (eq. 3) was commanded by the such considerations. As nicely outlined by Imbrie et al. (2011), this sensitivity is easily understood is systems featuring explicit threshold functions (they may be little between 'crossing' or 'not crossing' a threshold). More generally, this is a manifestation of a form of dynamical instability, which probably is a necessary ingredient to obtain 100-ka cycles in response to obliquity and precession (see, e.g. De Saedeleer et al., 2012 1). Therefore, at the risk of caricaturing the paper storyline, it

is no surprise that a model originally calibrated on the actual sequence of terminations subsequently shows different sequences when the precession and obliquity factors are modified. The very fact that the model may be tuned to reproduce the sequence of terminations is on its own not a proof of the stability of this sequence of terminations. For example, Crucifix 2011 show a simple model of ice ages successfully tuned on the sequence of terminations of the last 700 ka. Yet, the simulated sequence with this model is highly sensitive to external factors, such as additive noise (but similar effects are found with small parameter changes), which cause a form of phase-slip of the climate history with respect to the unperturbed sequence (De Saedeleer et al. 2012).

The original model has not been more calibrated than the obliquity-removed or precession-removed versions. So our conclusion on the respective role of obliquity and precession in TVI and TVIII is in our opinion robust. Moreover, with our new sensitivity analysis with respect to initial conditions, we now clearly show that the timing of terminations in our model is NOT sensitive to noise. More generally, we do not believe that dynamical instability is a necessary ingredient. In our model, as in previous ones (Paillard 1998, Parrenin et Paillard 2003), there is no strong sensitivity to parameters. We have different well-defined domains (or basins) with sharp boundaries (thresholds), but there is nothing like a chaotic behaviour, as for instance in a Van der Pol oscillator placed in a chaotic regime. Therefore, if a chaotic model is a possible alternative, there is no obvious reason to choose this paradigm a priori, and to disregard what the simplest possible (non-chaotic) model should be. The reviewer is right in pointing out that our model has thresholds and it is even probably structurally very similar to a van der Pol oscillator. But parameters are simply not in the range of chaotic behaviours, and the model is quite simple. It lies clearly out of any possible chaotic regime.

3 Technical commentary on calibration procedure In connexion to the earlier comment some observations may be made about the calibration procedure. The modelling and

algorithmic choices are almost identical to those made by Hargreaves and Annan, 2002 in which the Salzman and Maasch 1990 is calibrated: the dynamical system is deterministic, and the “likelihood" function is a priori assumed to be Gaussian on model states (equation (7)), and the calibration algorithm is Metropolis Hastings.

Hargreaves and Annan is now cited.

Again, starting from a calibrated deterministic model to conclude that the succession of ice ages is deterministic is a tautology, and the fact that the number of degrees of freedom is small is not a fully satisfactory objection.

It is not so obvious to find a deterministic model with a small number of parameters that satisfactorily fits the observation. We are just following the "Occam's razor": we prefer to choose the simplest possible description of the system. But the reviewer is right in pointing that this line of reasoning should be different if the system is intrisically chaotic or random: In this case it might be useless to attribute terminations to specific causes (obliquity or precession) and tuning a model to observations would not be an easy task. But again, we do not believe that it is necessary to assume a chaotic behaviour: Our model is not chaotic and is able to reproduce the observations : - First, the model is NOT sensitive to initial conditions: a large perturbation of this initial condition is 'forgotten' after 2 glacial cycles at maximum (see our revised manuscript). - Second, our model successfully reproduces the ice volume data with only a few tunable parameters (10, if we choose the model without phase-shifted precession).

Indeed, experiments with deterministic models of ice ages such as Saltzman's or the van der Pol oscillator forced by the astronomical forcing reveal extremely complex like lihood functions of parameters (shown, e.g. by R. Wilkinson at Isaac Newton Institute Seminar Series, 09 September 2010). This complexity is a sign of local instability: small parameter changes modify the exact succession of terminations (technically, these may be viewed as bifurcations in a non-autonomous system). However, from a probabilistic approach, a highly sensitive likelihood function cannot reasonably reflect our judgements on the system (it is unreasonable to assert that a parameter, say, α, has 1015 more probability of being, say, 0.6524 than 0.6520). Hence, the distributions resulting from a calibration procedure on a deterministic model could, in these examples at least, hardly be viewed as actual probability distributions. So, why would it be different in the Parrenin/Paillard model than Saltzman's or van der Pol's ? And if it is different, why should the Parrenin/Paillard model tell us a better truth about the real world than those models ?

We do not claim our model is right, but at least this model is in good agreement with the data, which is a necessary condition to be right. The fact that our model is (again) not chaotic appears to us a "simpler" hypothesis than the choice of a chaotic model. We prefer to discuss what can be explained deterministically, even if we are at a risk that observations may be misleading and that reality may be just random... But we feel that our approach is nevertheless sound.

For that reason that Crucifix and Rougier (2009) have argued the need of using stochastic models, where the stochastic terms both account for structural model uncertainty and sub-scale variability ("weather"). The unfortunate consequence is that the calibration procedure is much more involved and much thinking is still to be made about the parameterisation of the structural error term.

Since our model is not chaotic, the results (and the calibration steps) would not change even in presence of (reasonably small) noise. We are therefore in a classical situation where the use of traditional tools is entirely justified.

4 Note on bibliography It is unusual to have as many references in the abstract, and those adopted here appear unduly French-centric. For example, while there is no dispute about the Laskar et al. contribution to the state-of-the-art solution of astronomical parameters, the citation here may let one believe that Laskar et al. have shown that "the main variations of ice volume of the last million years can be explained from orbital

parameters", while this is not what that paper is about. In fact the lack of any reference to Berger, even as a co-author, in a subject like this one is almost a performance. A bit more of acknowledgements to other authors of dynamical system models of ice ages, contemporaneous and historical, wouldn't hurt either.

A. Berger is now cited alongside with J. Laskar for the calculations of orbital parameters. Note that M.-F. Loutre was already cited in the submitted version.

5 Summary and recommendation The article is topical and focused but it lacks elementary tests of robustness. The authors must find a mean to visualise the relationship between timing of individual terminations and the parameter space in a more systematic way.

We now have tests of robustness with respect to initial conditions and a vizualisation of the timing of terminations with respect to orbital parameters.

6 Editorial notes Write '3-state climate model', not 'Three states climate model' (idem for '2-state').

Done.

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