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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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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 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.

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.

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).

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.

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.

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-

[1] to be supplemented by an article more focused on this specific issue, to be submitted soon

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 10^{15} 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 ?

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.

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.

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.

6 Editorial notes

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

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