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Climate of the Past Discussions

CPD

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Interactive Comment

Interactive comment on "Pleistocene glacial variability as a chaotic response to obliquity forcing" *by* P. Huybers

P. Ditlevsen (Referee)

pditlev@gfy.ku.dk

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The manuscript presents a simple conceptual model to explain the varying lengths of Pleistocene glacial cycles. The main suggestion is that the swiching between 40 kyr cycles and $\approx$ 100 kyr cycles is a result of chaotic response to the obliquity cycles, where the system is sometimes "trapped" near a fixed point resulting in multiple 40 kyr cycles (prior to the Mid-Pleistocene transition) and sometimes "escapes" into a chaotic regime of 80 or 120 kyr cycles (as in the Late-Pleisocene).

The model is a simple forced non-linear map with a lag, which has a behavior similar to a low dimensional chaotic oscillator, such as the Rössler system, or the forced logistic map. In the manuscript some physical realism is argued, referring to the classical Imbrie and Imbrie model.

Similar suggestions for an explanation of the changing ice age periods have been made before from behavior of low order chaotic dynamical models, as pointed out by Michel Crucifix.

I am in favour of simple conceptual models, the manuscript is well written and should certainly be published. However, I am not fully convinced that this model has much realism. I have some concerns, which, if the author can meet them, would make a much stronger case:

1. The use of a lag-dependence between the ice volume and its tendency is argued purely from the observational record. The maximum lag-correlation occurs at 10 kyr, which, as also stated in the manuscript, comes as no surprice since it is due to the dominating 40 kyr periodicity.

The lag correlation could in principle also come (internally) from the response timescales. To make things even simpler than the Imbrie and Imbrie model, assume just the usual harmonic climate oscillator for the ice volume V: $\dot{V} = a$, where a is the net accumulation, $\dot{a} = \alpha T$, increasing through Calausius-Clapeyron with temperature T and $c\dot{T} = -V$ through the ice albedo feedback. Here we get the maximum lag correlation $\tau = \pi/2\Omega$, where $\Omega = \sqrt{\alpha c}$. Thus the apperent lag correlation is just set by the timescale associated with the heat capacity c, and not a lag in the dynamics. Thus a real physical justification for the use of a lag in the model has not been given.

The thorough investigation of the significance of the lag correlation seems a little "over doing". What it really shows is that the autoregressive model (the null-hypothesis) can be rejected as a model of the observations measured by the lag-correlation measure, not that the lag-correlation is highly statistically significant. I would assume that if different periodic models were used (the simplest I could think of is a sinusoidal + noise), the lag-correlation would probably not be significantly different from this model.

2. The main idea of intermittent chaos is interesting. What would be really interesting is to see if this behavior of a simple chaotic low order system or equivalently a map

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is a characteristic of the simplicity of the system or if it "survives" also for models of many degrees of freedom with some realism for the real climate system. Some characteristics are known to survive, such as "jumping between a few quasi-stable states" (cf. multiple states of oceanic overturning, or snowball earth/green planet states), while others, like limit cycles and period-doublings, to my knowledge, are not seen. Thus the sentence p242 line 10: "...Eq(1) should be interpreted as a schematic, and serves to illustrate dynamical scenarios which the climate system may be capable of." should be qualified.

3. An obvious test of the relevance of the model should be the comparison with the observed record. If one tries, and I am pretty sure that the author did, to do the Lorenz trick on the paleo-record (Huybers, 2007) the two scatter plots in the top panels of the attached figure should be compared with the figure 2(b) in the manuscript. That comparison fails! And as regards to the model itself I, as the other reviewer, could not produce more than 5-6 short 40 kyr cycles in a row. This should be commented on.

4. The timescale T = 90 kyr is very long, about an order of magnitude to long, as an internal response time for the icc sheet dynamics.

5. The model is different in behavior from the Paillard 1998 rule based model, where the essense is a set of rules $i \to g \to G \to i$ for the late Pleistocene and $i \to g \to i$ for the early Pleisocene. This should be mentioned.

Minor points:

6. For people not familiar with the Imbrie and Imbrie model it would be helpful with $\theta \to \theta(t)$ in Eq(1).

7. Since the maxima are strictly periodic, the Lorenz trick is equivalent to a Pointcare map when plotting $(V_t \cos \Omega t, V_t \sin \Omega t)$ and intersecting the positive x-axis. See the two lower plots in the attaced figure.

8. Figure 2(c) should be plotted upside-down ($y \to -y$) in order to compare directly

with figure 1(a).

The figures which I refer to are available at: `http://www.gfy.ku.dk/~pditlev/CPD-Huybers.pdf`

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