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Interactive Comment

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

P. Huybers

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Dear J. Guiot,

Below I include a reply to each of the reviewer's comments and provide a description of how the manuscript was accordingly changed. Additionally, I briefly recapitulate my response to the reviewers in the below paragraph with particular reference to the editorial comments you provided. I am grateful for the detailed and insightful comments regarding this manuscript, and feel that the manuscript has been improved and now addresses each point which was raised.

The model presented in this manuscript does have clear implications for the interpretation of the mid-Pleistocene-Transition, though I would not wish to argue that any conclusion is unquestionable. The instability of the 40 ky cycle in the model is, I believe, an important feature — pointing to how we might understand the instability of the 40 ky period observed during the early Pleistocene. I now attempt to expand upon the scope of the discussion regarding this instability for the interpretation of the model and the climate. Physical realism of a model is difficult to establish, but the model does illustrate relevant and interesting phenomena by which we can come to a deeper appreciation of the potential controls on Pleistocene climate. I discuss that this illustration is not a numerical artifact and is an intrinsic feature of the model equations. Saltzman was a pioneer of simple climate models and does make reference to chaos in his work, but his use of the word chaos is inconsistent with the more strict sense that Lorenz used it, and it is this latter interpretation which is followed here.

For completeness and ease of reference, reviewer comments are included below between double quotations. Text reproduced from the manuscript is between single quotes.

Response to M. Crucifix:

"1. The idea of chaotic transitions between 40 and 100k cycles is not new. In 1992 and 1993 Saltzman and colleagues published two papers where they examine the possibility of chaotic transitions between 40k and 100k oscillatory regimes. They call this "chaotic intermittency". In the 1992 model, the astronomical forcing is necessary to trigger the transition, but in the 1993 one it is not. As far as I can understand it, the transition is the result of an unstable trade-off between calving and carbon cycle instabilities, both coded in the model."

In their 1992 and 1993 papers, Saltzman and Verbitsky use the term chaos differently than that used by Lorenz (1963). Chaos in the sense of Lorenz involves a deterministic process which is bounded, nonperiodic, and exquisitely sensitive to initial conditions. The differing interpretation by Saltzman and Verbitsky (1992, 1993) is demonstrated in their stating (Saltzman and Verbitsky 1992, p10, column 2, para 1), '...here this [Milankovitch] forcing appears to act simply as a 'catalytic agent' that can occasionally flip the system between a near-40 kyr free mode dominated by the bedrock-calving influence and a near-100 kyr free mode relatively independent of this influence. In fact, we note that this chaotic type solution would also prevail [i]f Milankovitch forcing were replaced by stochastic forcing...'.

In Saltzman and Verbitsky's model the amplitude and phase modulation of the Milankovitch forcing flips the model from one mode to another, just as stochastic forcing would. In the case of steady or periodic forcing, the model response would be steady or periodic. That is, the model inherits its chaotic-like behavior from the forcing but is not intrinsically chaotic.

The manuscript now better describes Saltzman a nd Verbitsky's contribution by stating in the introductions that, 'The transition has also been suggested to result from an unstable growing mode, sensitivity to subtle anomalies in the forcing, or as a response to stochastic forcing.', and by including a paragraph in Section 4 stating,

'Saltzman and Verbitsky (1992, 1993) also describe the behavior of their model as chaotic, but use this term to describe mode switching in response to modulations of the orbital forcing or, in other cases, from stochastic forcing. Earth8217;s orbital variations appear chaotic (Laskar, 1989) so that a model driven by the resulting insolation would presumably inherit this behavior. Following Lorenz (1963), however, the term chaos is here used to refer to a deterministic process exhibiting bounded and nonperiodic solutions that are exquisitely sensitive to initial conditions in response to a constant or periodic forcing.'

"2. Unfortunately, the dynamics of these transitions (as in the present paper) look far more abrupt than the data suggest (compare the spectrograms Figs. 1 and 2.). The data rather suggest a frequency doubling route to the transition between the 40k and 100k oscillatory regimes."

The frequency doubling description is an interesting one. Fig. 2a shows that the model presented here does undergo a frequency doubling route to chaos. An alternative

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description of the mid-Pleistocene Transition, which could be supported by this model, is that some parameter of the model or the magnitude of the forcing, caused it to undergo frequency doubling from a single 40 ky period, to an 80 ky period, and then into a chaotic mode characterized by 40, 80, and 120 ky periods. I have opted not to present this interpretation, as it is less novel, requiring a shifting of background or forcing conditions.

This is now noted in section 5,

’Model parameters could be adjusted to force a transition from 40 to 100 ky glacial cycles. For example, an exponent of e = 6 gives purely 40 ky variations, and e = 7 or 8 gives 80 ky variations. Changes to the forcing variance, accumulation, or time constant can be made to give similar results. However, as such forced transitions have been thoroughly analyzed elsewhere, the topic is not further pursued here.’

"3. This is a point I am less sure of, but inappropriate numerical schemes tend to exacerbate chaotic behaviours, and the present case seems at risk to me. See Borrelli and Colleman (1994)"

In describing the model, the original manuscript states "...that Eq. 1 is discrete and is thus technically a map rather than a differential equation." Interpreted as a map, the equations are solved to machine precision and are not in danger of the model integration errors discussed by Borrelli and Colleman (1994). There are several other indicators that the present case is not at risk. (1.) The model displays the hallmarks of chaos for a map associated with the doubling route to chaos (Fig. 2a), a curved Lorenz map (Fig. 2b), and a Lyapunov exponent consistent with an e-folding time-scale consistent with that of a few glacial/interglacial cycles. (2.) Experiments have been run wherein the time-step is changed to be as long as 4ky or as short as 0.1ky, and the same behavior is manifest. Finally (3.), the model is similar to other excitable systems which have been shown to exhibit chaotic variability (as discussed in the paragraph 1 of section 4). Thus, the model does not appear to be at risk of numerical errors inducing

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the chaotic behavior.

Response to P. Ditlevsen:

"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 surprise 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 : 729;V = a, where a is the net accumulation, 729;a = T, increasing through Clausius-Clapeyron with temperature T and cT729; = 8722;V through the ice albedo feedback. Here we get the maximum lag correlation = /2 , where = pc. Thus the apparent 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. "

I expect that the lag dependence does have physical significance. My speculation upon this is now included in section two,

'The relationship between ice volume and its rate of change appears most obvious during deglaciation (Fig. 1c), and raises the question of why, during deglaciation, would the climate system *remember* the amount of ice volume present during the preceding glacial? Isostatic adjustment seems to occur too quickly for a 9 ky lag, but there are other possibilities. One is that a thicker ice sheet will be more prone to basal melt-

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ing because it better isolates an ice sheet's base from the cold overlying surface and has a lower pressure-melting point — and which would possibly destabilize large ice sheets (e.g. ??). The past thickness of the ice may then be remembered through the the ongoing conversion of potential energy into frictional heating and the associated maintenance of melt water at the bed. Another possibility involves the carbon cycle. Perhaps the rate and amount of CO_2 fluxed from the ocean into atmosphere, or from the solid Earth into the ocean/atmosphere system via volcanism (Huybers and Langmuir, submitted, depends upon the depth of the preceding glacial. Other possibilities doubtlessly exist.'

Also, I believe there is more associated with the observed time-lag behavior than is represented in the simple oscillator example given above and used to suggest that the time-scale could be related to heat capacity. To obtain such oscillatory solutions requires assuming that the rate of change of temperature is proportional to the ice-volume and, in particular, that a large ice volume is related to warming, which seems counter-intuitive. It seems more physically sensible for temperature to be proportional to the amount of ice volume (or perhaps ice to the 2/3 power of the volume, in order to related area to albedo) but in any equilibrium scenario, the oscillatory effect is lost. Furthermore, however convenient a simple oscillatory description is, it cannot explain the sawtooth nature of the late Pleistocene deglaciations, and it is the presence of the time-lag during the late Pleistocene which appears most interesting.

"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 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

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illustrate dynamical scenarios which the climate system may be capable of." should be qualified."

The quoted line has now been expanded to the following paragraph,

'Eq. 1 is discrete and is technically a map rather than a differential equation, and is best interpreted as a schematic that serves to illustrate dynamical scenarios which the climate system may be capable of, as is common to all simple models. While there is no assurance that the phenomena manifested by this schematic would be reproduced in more complete models, the model's simplicity facilitates a more thorough analysis of its rich behavior.'

"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."

I did not actually try to compare the model results to the observations. Chaos is notoriously difficult to detect in noisy records. Given that the delta-18O record is not only noisy, but reflects the combined effects of changes in temperature and ice volume, I would have been thoroughly surprised for the simple structure evidenced by this model to be discernible in the delta-18O data. Still, this is a reasonable comparison to make, and I have now also obtained these null results and offer some comment on them in the manuscript.

Section 5 now concludes with the line, '... the simple structure relating successive glacial maxima in the model (Fig. 2b) is not identifiable in the δ^{18}O data, as might be expected given the influence of temperature, noise, and different ice sheets on the marine δ^{18}O signal.'

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The fact that the model tends to only produce strings of four or five 40 ky glacial cycles is now discussed in detail in the text, and as outlined in response to anonymous reviewer 2.

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

The internal response time for the ice sheet dynamics is more represented by the lag term, 10 ky. The 90 ky time scale represents the amount of time needed for the ice sheet to become sufficiently large that it becomes unstable. This is now noted when describing the model.

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

When discussing the i, g, and G states, the original manuscript stated that '... these transitions between glacial states emerge as an intrinsic features of the model, perhaps explaining the analogous three climate states specified in the Paillard model (Paillard, 1998).' This has now been strengthened by replacing 'specified' with 'hardwired'.

"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)."

OK.

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

Agreed.

"8. Figure 2(c) should be plotted upside-down (y ! 8722;y) in order to compare directly

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with figure 1(a)."

Done.

Response to anonymous reviewer 2

"1 - I tried to reproduce the author8217;s results shown on Fig.2, but I found it extremely difficult to choose an initial condition that gave a succession of eleven successive 8217;almost 40-kyr cycles8217; as shown on Fig. 2c. The typical results look more like the second half of Fig. 2c, with a mix of 80-kyr and 120-kyr cycles, while the first half is extremely unlikely. The author mention that "the model intermittently happens to land near the unstable fixed point (at the g-G boundary) and then requires many g-G cycles to escape the fixed point influence". In my rapid implementation of the model, "many g-G cycles" typically means about 5 or 6 cycles, which is quite small compared to the duration of the "41-kyr world", even near the fixed point zn = 1.34 mentioned in Fig. 2b. In other words, the 41-kyr oscillation appears far too unstable to be relevant to the Plio-Pleistocene climate."

Discussion of this subject is now provided in four paragraphs in the manuscript and is reproduced below,

'The model episodically becomes trapped near the unstable fixed point, giving trains of 40 ky glacial cycles, and then transitions into the full g-G and i-g-G cycles, giving larger amplitude and longer-period $\sim$100 ky glacial variability. Given a long enough run of the model, a sequence of glacial variability similar to that observed during the Plio-Pleistocene is inevitable, but it is useful to consider the probability of actually obtaining such a realization.

The requirements for obtaining a long string of 40 ky glacial cycles can be approximated from the nearly linear relationship between successive ice volume maxima in the vicinity of the unstable fixed point. The relationship can be expressed as $z_{n+1} - z_f = a(z_n - z_f)$, where $z_f = 1.34$ is the location of the fixed point and the

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slope is $a = -2.0$. Thus, an initial glacial maximum, $z_{n=0}$, near z_f will have successors that hop to alternate sides of the fixed point, moving progressively further away according to $z_n - z_f = (z_0 - z_f)a^n$. If successive glacial maxima having values between 1.3 and 1.4 are considered to be 40 ky glacial cycles, the distance between z_o and z_f must equal 0.02 in order to obtain a string of five 40 ky glacial cycles, and $z_o - z_f$ must be only 5×10^{-13} to obtain forty 40 ky glacial cycles. Thus, if the early Pleistocene is characterized as undergoing 40 successive glacial cycles, and z_o is considered as being uniformly distributed between 0.85 and 1.35, the odds of the simple model yielding results like the early Pleistocene is exceedingly small at one in 10^{12}, given the parametrizations considered here.

But the situation may not actually be this improbable. The early Pleistocene δ^{18}O stratigraphy leading up to the mid-Pleistocene Transition exhibits irregular glacial variability, with strings of three to eight 40 ky glacial interspersed with longer glacial cycles occuring near 1.8 Ma, 1.6 Ma, and 1.2 Ma (**???**) (Fig. 1a). When run over a few million years, the model tends to regularly produce strings of four to six 40 ky glacial cycles. The suggestion, then, is that during the interval leading up to the mid-Pleistocene transition, the climate was poised to spontaneously alternate between 40 ky and $\sim$100 ky glacial cycles. No specific event or change in forcing is required to explain the transition.

Model parameters could be adjusted to force a transition from 40 to $\sim$100 ky glacial cycles. For example, an exponent of $e = 6$ gives purely 40 ky variations, and $e = 7$ or 8 gives 80 ky variations. Changes to the forcing variance, accumulation, or time constant can be made to give similar results. However, as such forced transitions have been thoroughly analyzed elsewhere, the topic is not further pursued here.'

"2 - The author is justifying the need for such a model in the introduction of the manuscript, by the lack of a well accepted forcing for the MPT. Still, it must be emphasized that, though there is no consensus on a long term forcing for the climate system on this time scale, there is a clear climatic trend over the last 15 Myr, since the

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Mid-Miocene. The last events fitting into this trend are the start of significant Northern hemisphere glaciations (about 3 or 4 MyrBP) and the MPT (about 1 MyrBP) which corresponds to a transition towards larger and longer glacial cycles. The MPT appears therefore naturally as part of a climatic trend, even if the underlying forcings are not clear. It is consequently not so obvious to me that a model simulating the MPT as a purely stochatic event has an advantage over other models."

I have partly attempted to address these comments as indicated above. Furthermore, I agree that it is not obvious that the present model has an advantage over the others, but would argue that there is merit to placing all the options on the table. Thus, I would call on the mechanism's uniqueness as being more compelling than its obviously being superior.

The following line is now also included in the last paragraph of the introduction, 'The hypothesis does not, however, address the initiation of glaciation nor the longer term trends in amplitude, duration, and skew of the glacial cycles observed over the course of the Plio-Pleistocene.'

"3 - This lack of external forcing trend may on the contrary become a difficulty, since the ice volume needs to be initialized to zero (or almost zero) at the beginning of glaciations (about 3 or 4 MyrBP ago). When starting from V near zero, the author8217;s model jumps immediately into 100-kyr like oscillations (since this is far away from the unstable limit cycle). Without some external parameter changes, this model therefore cannot reproduce the start of Plio-Pleistocene glaciations."

I agree that the model does not address glacial initiation.

"Other minor comments: - On fig 1a: "glacial cycles lasting more than 60 ky are indicated by vertical bars". And page 239 line 4: "multiple switches observed between short and long-period glacial cycles". Without a clear definition of what a "glacial cycle" is, it is difficult to assign a length to each individual cycle. Please be more precise when suggesting that these lengths switched from 41 to "more than 60", back and forth, sev-

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eral times during the last 2 million years. For instance, i don8217;t understand on Fig. 1a why stage 15 is not grayed out (since I don8217;t see any 41-kyr oscillation in it).

Figure 1 and the relevant discussion have been revised to be more clear. In particular, it is now discussed in the caption to figure 1 that, 'Maxima in obliquity not accompanied by a deglaciation are shaded in yellow. Deglaciations are identified as decreases in δ^{18}O exceeding one standard deviation of the record, the length of which is indicated at right.'.

" - page 239 line 19. "Fig.2c" to be replaced by "Fig.1c". - Units. In equation (1) everything is dimensionless (except time ?). Then if the constant T is measured in kyr, so should also be accumulation and forcing? Clearly, there is a problem with a dimensional T."

It is now stated, 'For convenience, ice volume is treated as being non-dimensional while units of time are retained.' The variables a and θ are unitless representations of volume which are scaled by dimensional time T. Implicit in the right hand side of the equation is multiplication by the time-step, which is one, and which leaves only the unitless representation of volume.

- page 238 line 17. "In the absence of substantial change in the external forcing". I think the author is meaning "astronomical forcing" which is more adequate."

This change was made.

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