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Interactive comment on "Changing climatic response: a conceptual model for glacial cycles and the Mid-Pleistocene Transition" *by* I. Daruka and P. D. Ditlevsen

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1 General comments

The renaissance of Milankovitch's theory in the 1970s was in fact a deep mutation of the original paradigm: while Milankovitch theorised about the relationship between insolation and climate, the problem that has been expressed for 40 years is one a dynamical system identification: which physical laws effectively govern the slow evolution of climate, subject to astronomical forcing? Solutions to this problem have been proposed by reasoning on the physics directly, specifically on ice physics (these are the pioneering contributions of e.g. Weertman, Oerlemans), or following a more heuristic approach speculating on the possible role and non-linearities associated with different components of the Earth's system, and specifically the carbon cycle (Saltzman, 2002 and ref therein), possibly in association with non-linear ocean circulation responses (Paillard and Parrenin 2004). The many models proposed so far justify an even more abstract modelling level, the objective of which being to identify the mathematical elements that determine the dynamical properties of climate, and specifically those that can be attached to the broad concept of predictability. This is the sense given to a line of works focusing on the Van der pol oscillator, the delayed oscillator and—here—the Duffing oscillator, as generic representations of the slow dynamics of climate.

Specifically, Daruka and Ditlevsen write:

- still it is an open problem to which extent the global stack marine isotope record itself is sufficient to discriminate between [different models] (p. 1105),
- we must ask in which sense we must be able to reproduce the past, by reproducing the evolution or by reproducing the past in some statistical sense (p. 1117)

Both sentences are valid, keeping in mind though, that deliberately ignoring the CO_2 record is putting oneself in the weakest possible position for model selection.

The Daruka-Ditlevsen model is a variant of the Duffing oscillator. It differs from the tradition of oscillatiors initiated by Saltzman and co-workers. The latter are essentially build around oscillators, and Crucifix (2013) and Mitsui and Aihara (2013) have recently identified the emergence of strange non-chaotic attractors as a source of unpredictability in these models. In that case unpredictability is linked to the quasi-periodic character of the forcing.

Here, we do not have a free-standing oscillation and much importance is given to multiplicative forcing, which presumably plays in important role in the emergence of 100-ka dynamics through generation of combination of tones. From this point of view, the Daruka-Ditlevsen model is more akin of Paillard (1998), Huybers (2009) and Ditlevsen (2009) [1]. In particular, as in Huybers (2009), the Daruka-Ditlevsen displays parameters regimes yielding chaos, not very far from those yielding optimal fit. This might suggest that chaos is a not-so-implausible scenario for the slow dynamics of climate, even though the authors do not go as far as explicitly suggesting this.

A number of questions then emerge, some of which at least the authors should consider in a revised version of the manuscript:

1. What are the specific roles of multiplicative forcing vs non-linear climate potential in the dynamical properties of this model (100-ka oscillation, and greatest Lyapunov exponent)?
2. What could be the physical interpretation given to variations of the damping factor κ?
3. What is the fundamental dynamical difference between Ditlevsen (2009), Huybers (2009), and the present Daruka-Ditlevsen model? How would their signature on the climate record be different, and thus distinguishable? (MPT, amplitude and frequency modulation patterns, spectral signature, phasing with eccentricity . . .).
4. Same question as above, but with respect to oscillators ?
5. If the signatures are in fact similar, could the authors think of a decisive physical argument?
6. What is the specific effect of the quasi-periodic nature of the forcing in equation (5) in what the authors have identified as a 'butterfly effect'? What is then the physical implication?

[1] Note that Le Treut and Ghil (1981) specifically discussed the relevance of non-linear resonance and the effect of multiplicative forcing.

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2 Other comments

p. 1103, l. 4 : 'more-or-less synonymous' is too informal in this context. p. 1103, l. 14: In Milankovitch compared two solutions: those of Pilgrim, based on Stockwell's integrals, and those of his colleague Miskovic, based on the Leverrier integrals with corrections on the masses. He did not compute astronomical elements himself [see Milankovitch's Canon of Insolation, English edition by the Serbian Academy of Sciences and Arts (1998), p. 371]. Milankovitch's contribution lies essentially in (a) the elimination of other effects, such as polar wandering, as explanation of ice ages and (b) the modelling of the climate response to insolation changes, with an explicit account of radiative feedbacks.

p. 1104 , lines 1-5 : how would this discussion accommodate the observations by Lisiecki, Nature Geosciences (2010)?

p. 1105, l. 3 : it needs to be clarified whether the point being discussed is the forcing or the internal dynamics.

p 1106, l. 24 : Admittedly, Paillard and Parrenin (2004) does a pretty good job in simulating the MPT. The model features both additive and multiplicative forcing terms, but additive forcing alone may be enough to explain the MPT. This is achieved by gradually increasing the length and amplitude of the limit cycle, causing different mode locking regimes to be scanned (Crucifix et al. 2011)

p. 1115. l. 8 : The term 'butterfly' effect is very generic and informal. It could be confused with the more restrictive meaning of sensitive dependence to initial conditions. Here the authors describe a sensitive dependence to the parameter κ. This is a distinction that we are only beginning to realise in climate science and time is adequate to chose words carefully.

p. 1113 and figures : **IMPORTANT** : all numerical values of parameters must be checked since they are generally inconsistent between text and figures or accross figures ($\lambda = 10$ or 0.087, $\omega = 10$ or $2\pi/10$ etc.). This has hampered verification and result replication during this review.

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Dr. T. Mitsui drew my attention on the similarity between Daruka and Ditlevsen's model and the Duffing oscillator.

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