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Climate of the Past Discussions

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

Anonymous Referee #2

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The manuscript presents a new conceptual model for glacial-interglacial dynamics. The authors are discussing thoroughly the structure of their model's solutions in comparison with some known qualitative features of glacial-interglacial cycles, like the MPT. This might be of interest for specialists in dynamical systems or chaos theory, but I don't think this is relevant to climate sciences, at least it its current state. Indeed, the model's physics is not explained and appears to be very unrealistic. Furthermore, the results are quite far from what is known from paleoclimatology. I am not sure that the model can be modified in order to answer my comments below.

Main points.

The authors are apparently not distinguishing "temperature" and "ice volume" in their text (see comments below) and they often refer to the results as "climate". From a physical point of view, these two variables have little in common, and in order to build a physically relevant model, it is important to decide which physics is involved. More precisely, I do not understand the background assumptions. x(t) is defined as a "global ice mass anomaly", and equation (1) says that it increases with temperature. Do the authors assume that the ice sheet is growing when it is warmer ? Or may be a minus sign is missing somewhere ? Or may be tau(t) is the ice volume (see comments below) and x(t) is something else representing the "effective climatic memory" of the system ? But which physical component could it be, if not the ice volume ? Later in the text, the corresponding time scale (1/lambda) is taken to be 10 kyr. Do the authors believe that ice volume is lagging temperature by such a long time ? Actually, the standard Milankovitch theory acts mostly the other way around (the "global mean Earth temperature", if this means anything, is a consequence of ice volume changes, not a cause). Things are even more strange in equation (2). Clearly, x(t) can have both positive and negative values (eg. see figures). The forcing term A(t) is always positive (eg. see equation (5)). The multiplicative coupling term - x(t)A(t) in equation (2) is therefore sometimes negative and sometimes positive. In other words, a large insolation forcing does sometimes warm the climate (or melts the ice, or whatever the interpretation of tau) but at other times does the exact opposite (ie. cools the climate or grows the ice sheet). This point appears very crucial for the results, since it explains quite directly the locking to 2x or 3x the forcing periodicity (see eg. figure 5). But I cannot think of a (simple) physical mechanism that could act this way, with the insolation forcing having an opposite effect on "climate", depending on the state of the system. This only looks like a mathematical trick. I personnaly believe that conceptual models are very useful, but they should be based on simple and reasonnable physical assumptions. In this paper, these assumptions are not stated, and they appear quite unrealistic. But more generally, I don't think the authors are raising the right questions. Please read Hays et al (1976). It has NEVER been suggested that eccentricity was "directly" driving the climatic 100-kyr cycles, but only that it was acting through the modulation of precession. This idea goes back to Croll's theory in the 19th century. This is probably THE main reason why precessional forcing is assumed to be important besides obliquity: it can easily generate 100-kyr oscillations through, for instance, signal rectification or any other simple (or less simple) non-linear process. This is strongly suggested by the phasing in the data : glacial maxima correspond systematically to eccentricity minima. This cannot be pure chance (and this also appears to be true in pre-Quaternary glaciation contexts). Prior to the discovery of these 100-kyr cycles (locked to eccentricity) in the 1970's, it was clear (eg. see Milankovitch) that obliquity is indeed the dominant signal at high latitude. The key question behind the "100-kyr cycle" is not so much its origin, but much more its mechanisms: how can we get a phase locking of the ice volume 100-kyr cycles to eccentricity ? (again, please read Hays et al 1976). This is unlikely to be achieved by "removing" precession altogether.

Detailed comments.

p1103 line 6: benthic foram: "single core deviations .. can be interpreted as local climate". NO, please look at textbooks on paleoceanography. In benthic foraminifera, the isotopic signal is dominated by the (global) change in the ocean volume, linked to the size of the ice sheet. This cannot be described as a "local" climatic effect (though there is also a local temperature contribution to the signal which is often small for deep-dwelling animals). This is precisely the reason why benthic forams are used: the signal is mainly a global one.

p1103 line 10: "strongly correlates with Antarctic ice core... which thus provides limited additional information" I STRONGLY disagree. The two records are not similar at all. They are furthermore phase-shifted during deglaciations. The 18O stack is linked to global ice volume, the Antarctic isotopes are linked to Antarctic temperatures. These two physical variables have nothing in common. The leads, lags and other differences between them carries extremely relevant dynamical information. Atmospheric CO2 is obviously also another extremely relevant variable for climate over this time period that

should be used.

p1104 line 1: "The 100-kyr problem". see main comments above: I believe the authors are mis-understanding the "100-kyr problem".

p1105 line 1: "it is unlikely that a single time series...". We need only three time series (eccentricity, obliquity and precessional angle) to get the full richness of the spatio-temporal astronomical forcing, ie. the insolation at different latitudes and months. This is not such a large number... Reducing these three time-series to just one (usually the summer insolation at 65°N) may possibly be a problem, but it is not such a "drastic" loss of information (idem p1110 line 15)

p1105 line 6: "to which extent the global stack marine isotope... is sufficient to discriminate between the suggested ... conceptual models". Indeed !! This is precisely why I strongly disagree with the over-simplification (see comments above) of looking at only one paleoclimatic data series, while thousands of them are available. When looking for "globally relevant" variables, a natural choice would be to represent of course ice volume (benthic 18O data) but also atmospheric CO2, which is available over almost the last million years. Many other dynamically relevant records are also available, for instance concerning the deep ocean state (temperature, ventilation, etc...). Again, the variations of these different variables, are most of the time, very different in terms of timing, amplitude, frequency content, etc... Looking at only one variable is indeed not sufficient. But a first step is to differentiate them (... and not mixing "temperature" and "ice volume" into a single vague concept called "climate").

p1105 line 22: "energy balance models ... more realistically to describe the Snowball Earth" It is not clear what the authors mean by "energy balance model". If the question is about ice-sheets, obviously the model needs an ice-sheet (which is not the case in EBMs). The ice-albedo-temperature feedback involved in the Snowball or the simple EBMs mentionned is mainly the snow or the sea-ice albedo (which depends mostly on temperature). When considering ice-sheets, the situation is very different, since ice sheets do not depend solely on temperature, but also ice dynamics, continental topography, etc... It is more than likely (eg. Calov and Ganopolski., Geophys. Res. Lett., 2005) that multiple equilibria exist in the coupled atmosphere - ice sheet problem, mainly because of the albedo feedback (ie. a coupled EBM - ice sheet model, could probably be sufficient). Again glacial-interglacial cycles are about ice sheets, not about snow or sea ice, and even less about temperature (idem p1109 line 25).

p1106 line 22: "dessert" should be desert ?

p1108 line 20: choice of model variables. I am surprised by the choice of "temperature" tau(t) as a model variable. This should require a bit more physical discussion of what the authors are actually meaning by this term. Atmospheric temperature has no memory (on the time scale of interest here). Ocean temperature may have some long memory in its deeper levels, but I am not sure that this is what the authors are meaning here. Anyway, there is nothing like a paleoclimatic record of "global temperature", only local ones, which are not discussed here. The discussion afterwards in the paper is based on a comparison between this "temperature" and the record of ice volume. But the variable x(t) is presented as the one corresponding to the "global ice mass anomaly". I would therefore expect a comparison between x(t) and ice volume records. But clearly, on Figure 5, the x(t) variable does not have the same shape and does not show the "asymmetry" of tau(t), which might therefore completely change the conclusions of the paper... More probably the authors should change their physical interpretations and call tau(t) the ice volume, and x(t) something else...

p1109 line 7: "The paleoclimatic record strongly indicates that the climate can be in one or more possible states" Well, this needs a bit of substantiation. Which records ? For Dansgaard-Oeschger events, I agree that it might be obvious. But for glacial-interglacial cycles, this does not appear so "clear" from the record of Figure 1 for instance... This assumption may indeed help to produce the "right results", but this is then a (strong) assumption of a model (which one?), not an observation from the record.

p1112 line 13: "of of chaos"

p1114 line 1: "maximum temperature lagged 9 kyr to.." Do you mean temperature or ice volume ? The data is about ice volume (and Huybers' paper also).

p1114 line : "rapid warnings" -> warmings ? though again, I don't think this is appropriate. The real world physics is about ice sheet growth and decay. Warmings and coolings are based on a different dynamics.

Interactive comment on Clim. Past Discuss., 10, 1101, 2014.