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Climate of the Past Discussions

Interactive comment on "Cumulated insolation: a simple explanation of Milankovitch's forcing on climate changes" *by* F. Marra

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Completely opposite to Reviewer 1, who doesn't see any correlation between the proposed maxima of cumulated insolation and the glacial terminations, Reviewer 2 says not only that this correlation is evident, but that it is not surprising. Well, this is actually surprising. Who is right? Another surprising thing to me is learning that a direct causative mechanism between eccentricity and glacial terminations has been demonstrated and is universally accepted by the paleoclimate community. What about the obliquity pacing (for example)? According to Huybers (2007), it matches 33 out of 36 major deglaciations. On the other hand, if the model that I propose is the exact equivalent of that based on eccentricity minima, it would be a good supporting contribution to this hypothesis, since it provides 16 out 17 (or 17 out of 17, if we assume also a second

threshold mechanism on the maxima of insolation) matches for the glacial terminations during the last 900 ka. Better than obliquity. I have the feeling that reviewer 2 has not understood the idea of cumulated insolation, at all. This is very likely my fault, as I have not been much clear in explaining the very simple principle (which is a physical one, in any case) on which it is based. I'll try to explain it better, answering point by point to his/her comments.

«Minima in eccentricity have been linked to terminations at least since the famous Hays et al (1976) paper. More precisely, minima in eccentricity are preceding terminations. By doing the simple arithmetic exposed in the manuscript, the author is defining another equivalent way to detect this amplitude modulation, by taking the sum of successive insolation minima and maxima: this sum will automatically be maximal just after an eccentricity minima.»

Does any maximum of the mean summer insolation at 65°N coincides with one minimum in eccentricity? It seems an odd statement to me. I'm not sure whether each one of the 17 maxima characterized by a cumulated insolation value larger than 742 W/m2 I have identified corresponds to one minimum of eccentricity. Does the reviewer understood that these are simply 17 maxima of the 65°N mean summer insolation curve for which the potentiality to trigger a deglaciation is assessed based on the value of insolation occurring at the initial point of the deglaciation process?

«I think there are probably thousands of arithmetic rules that could provide the same answer. I do not see any scientific advance in such ad-hoc numerology. I therefore recommend rejection of this paper.»

It is hard to understand what the reviewer is stating here. I'm not using any arithmetic rule. I'm only indicating a way to understand the potentiality of one maximum of insolation to trigger a glacial termination. This is perfectly in line with the general Milankovitch assumptions. I only state that in order to estimate this potentiality it is necessary to consider not only the actual intensity of insolation at the maximum, but

also the conditions from which the deglaciation process starts. These conditions are expressed by how much low was the intensity of insolation during the glacial maximum (i.e. the insolation value at the preceding minimum of insolation).

«More precisely, taking the sum at to specific time (minima and maxima) on the insolation curve is extremely remote from any physical process. I do not see any value in such a rule, and do not understand any added value in doing such an arithmetic. The current manuscript has no physically relevant content : physics tends to be continuous in time. Physical laws are not selecting insolated points (here extremal points) to decide what to do next. I do not see any value in this approach.»

The reviewer has repeated this concept twice, still it seems that he/she really doesn't understand the point of what the concept of "cumulated" insolation means. As written in the paper, the cumulated insolation is an intuitive concept that accounts for both the starting and the final condition of a continuous physical process: the insolation history encompassing a minimum and the successive maximum. Is it physically correct to assume that the volume of the ice sheets covering the earth surface at a glacial maximum is the greater the more sever the minimum of insolation is? Is it physically correct to assume that the initial volume of ice plays a role in the following deglaciation process? Is it physically correct to assume that two maxima of insolation characterized by the same intensity may produce or not produce a glacial termination depending on the total amount of ice covering the earth surface at the moment in which earth's temperature inverts its trend (i.e. at the starting point of the process)? Is it physically correct to hypothesize that the deglaciation process may be a "threshold" process and from an initial linear behavior it assumes an exponential character after trespassing the threshold? I think so for all the abovementioned points.

«The author states in the introduction that "no satisfactory answer has been provided so far to the question why one specific maximum... has the potentiality to trigger a deglaciation". I believe the Hays et al. (1976) paper clear shows the link with eccentricity (and therefore amplitude modulation). The current manuscript only obscures this

simple connection.»

If the proposed model were actually an alternative way to represent a statistically significant selection of minima of eccentricity that matches all the glacial terminations in the last 900 ky, then this paper would highlight the connection rather than obscure it.

«Of course, a physical explanation for this link might still be elusive, but several numerical predictive models have been proposed.»

Yes, I agree. Mine is an alternative, very simple model providing a satisfactory match. I believe that it deserves to be brought to the attention of the paleoclimate community and discussed, even though the physical explanation for the link it evidences might still be elusive.

«The author seems to mix up to very different kind of "models": physical (or conceptual) models on the one hand and tuning targets or tuning procedure on the other hand (line 18, page 5555: "most of these models are based on the astronomical tuning"). Obviously, the long list of cited models (Saltzman, Tziperman, Ghil, ...) are aiming at understanding the mechanisms behind glacial cycles, and have no "tuning procedure". Besides, these models have never been used as tuning targets.»

Is this observation relevant with respect to the topics of the paper or is it only aimed at evidencing my scarce familiarity with some complex modelistic approach to the study of the climate processes? I have no problem in admitting I'm not a specialist in this field. This is the reason why I do not attempt to give an explanation after which the proposed triggering mechanism leads to the glacial termination, except the intuitive basic concept that it must rely also on the initial condition of the process.

«Finally (lines 20-25, page 5560) the author suggests that when his numerological rule fails, this must be due to some dating problems in the reference curve Lisiecki & Raymo (LR04). The difference are not small at all (20 to 30 kyr discrepancies) and I would certainly not agree with the author who jumps to the conclusion that LR04 (and

the traditional way of tuning benthic isotopic curves) is wrong.»

21 kyr is one precession cycle. This is the "quantic" base on which the astronomical tuning works. In theory, a mismatch in the tuning of one isotopic peak couldn't be smaller than 21 kyr, if the tuning principle of coupling isotope peaks and insolation maxima is applied. It is not a case, I think, that the cumulated insolation model is in perfect agreement with the astronomical tuning up to 900 ka, since a good age controls is achieved on the isotopic record in this time span. Are you sure that the tuning in the time span 900-1800 ka is perfect? Maybe it is so. However, in this case it means that the proposed model doesn't work 4 times out of 82, and all these 4 times in the time span older than 900 ka. A period when the climatic processes were somewhat different ones with respect to those characterizing the last 900 ka, during which "true" glacial terminations occurred. Maybe it would be interesting and profitable to investigate the possible reasons of the different effectiveness of the model during this obliquity-dominated time span, by diffusing this paper to a broader audience.

In conclusion, the contrasting points of view of the two reviewers about the match between the hot maxima and the glacial terminations, and more in general, the completely different perspectives from which the criticize the paper, show how wide is the range of opinions about the triggering mechanism to glacial terminations, and how far from an unanimous point of view is the paleoclimate community. In this context, it seems to me that the contribution to the discussion provided by the paper is far from representing "nothing significant". The paper provides supporting evidence to the following hypotheses: 1) The deglaciation process during the last 900 ka is principally lead by the intensity of the solar radiation during the summer months on the northern hemisphere (i.e. the full astronomical hypothesis that consider the mean summer insolation at 65°N); 2) For a correct estimation of the intensity, and therefore the potentiality to trigger a glacial termination, it is necessary to consider the entire time window encompassing one minimum and the following maximum of insolation.

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