

## ***Interactive comment on “Cumulated insolation: a simple explanation of Milankovitch’s forcing on climate changes” by F. Marra***

**Anonymous Referee #2**

Received and published: 9 December 2013

In this manuscript, the author is attempting to link the timing of terminations during the last 800 kyr with the sum of summer insolation forcing at successive minima and maxima. The results are actually not very surprising. Indeed, the summer insolation is modulated in amplitude mostly by eccentricity. 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. 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.

C2674

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

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.

---

Interactive comment on Clim. Past Discuss., 9, 5553, 2013.

C2675