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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Equatorial insolation: from precession harmonics to eccentricity frequencies" *by* A. Berger et al.

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The manuscript "Equatorial inslolation: from precession harmonics to eccentricity frequencies", by Berger, Loutre and Mélice, submitted for publication to Climate of the Past (CP), is reviewed here and recommended for publication. The authors propose that tropical insolation variability (at the equator) may be at the origin of (Late) Pleistocene (and future) climates at periodicities of 100 kyr (related to eccentricity), 11 kyr and 5.5. kyr (related to precession). The authors have submitted a manuscript that is well written, concise and deals with an important issue in palaeoclimatology and palaeo-oceanography (i.e. the role of the tropical insolation variability). The study presented here builds further on earlier work of these and others authors, on what is commonly referred to as "the Milankovitch theory" as a driver of long term changes in the

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Earth's climate. In this paper they present the results of calculations on the evolution of the maximum of the 24-hour mean irradiance at the equator based on the maxima and minima in irradiance related to Summer and Winter Soltices (SS and WS respectively) as well as the Spring and Fall Equinoxes (SE and FE respectively). From these calculations they derive a time record of the maximum of the amplitude of the 24-hour mean irradiance at the equator, max(SE,FE)-min(SS,WS), which is found to contain significant variability in the 100, 11 and 5.5 kyr periodicities. The authors suggest that these findings stress the imporance of tropical/equatorial region in long-term climate change (i.e. the 100 kyr cycle) as well as on shorter, half and quarter precessional, periods.

The manuscript by Berger and colleagues is well written, is clearly focussed on the issue of equatorial insolation and contains a sufficient number of comprehensible Figures that evidently illustrate the main points made (although I do have some small recommendations at the end of this review). In this respect, the manuscript is exactly what the title is suggesting: i.e. a study focussed on solar irradiance (differences) at the equator. When considering seasonality in the tropical realm, however, I presume that there is more to consider than what is presented here (i.e. considering variations at the equator). Despite being very supportive I would like to ask the authors to clarify some points.

My main concern with the present manuscript relates to the fact that the evolution of the maximum amplitude of the 24-hour mean irradiance at the equatoror is, in this manuscript, considered representative for the entire tropical realm. I doubt if this "simplification" can be made, and it seems to me that, given the unequal distribution of land and ocean on the northern and southern hemisphere (especially Indian Ocean), tropical seasonality is more complex than just considering the changes AT the equator. I will try to explain this in more detail below.

Already in the first sentence of the introduction, the authors mention : " the tropics have been long negelected by paleoclimatologistsĚ..", clearly indicating that they here

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consider the entire intertropical belt (i.e. the regions above and below the equator, in between the north tropic (Cancer) and the South tropic (Capricorn)). Also in the first sentence of paragraph 3, the authors mention: “In contrast with the extra-tropical latitudes,Ě.”, again strengthening the importance of the tropical realm as opposed to variations at the equator only. As far as I have understood their approach correctly, their “orbital proxy” for seasonality is given by the Delta index (?): max (SE,FE)-min(SS,WS) (see Figure 4 or Figure 5 upper panel for example). Although this index clearly captures the maximum in the amplitude of the 24-hour mean irradiance at the equator, it does not provide information on the seasonal origin of the maxima of the 24 hour mean amplitude that are considered throughout this study. In Figure 1 it is clearly shown that, for different time slices, maxima in insolation occur in March (SE) or in September (FE), while the minima are found in June (SS) and December (WS). It also can be seen in Figure 1 that, for t=0 kyr, the largest maximum is found in March and the lowest minimum is found in June, while for t=10 kyr the largest maximum has shifted to September and the lowest minumum is found in December. Although the seasonal origin of the evolution of the 24-hour mean irradiance at the equator can be tracked in Figure 4, it also can be seen in this Figure that a maximum 24-hour irradiance difference of approximately 60 Wm-2 near 14 kyr BP results from SE-WS while the same difference near 68 Kyr results from the maximum in the difference in SE-SS! From a palaeo-climate point of view, I envisage that the seasonal origin of these maxima in amplitude is important to further discuss, or at least further clarify, in the context of tropical climate systems such as the low-latitude monsoons that are known to be sensitive to the seasonal variability in insolation and coupled transequatorial pressure differences (e.g. Clemens, 1998; Leuschner and Sirocko, 2003; Rossignol-Strick et al., 1998).

Recommendations: I fully support publication of this manuscript in CP, and consider their work and results as very important for scientists working in the field of Palaeoclimate studies. I am convinced this work will give rise to new insights in the role of the tropics in climate-change. Nevertheless, the manuscript would benefit from clarification on two aspects of their findings and I encourage the authors to further discuss/clarify the follwing two aspects related to their findings: 1.To further discuss and clarify if their “oribital seasonality proxy” (Delta) is indeed repesenting the maximum seasonal insolation contrast for the entire tropical realm (and not only at the equator) and 2. To discuss in more detail how the “seasonal origin” of their Delta parameter may relate to the low latitude African and Asian monsoon systems, which are known to primarily respond to the difference in the cross-eqatorial summer insolation gradient.

Some comments/suggestions related to the text (grammar, style, typo’s) are given below here: [1] In the Introduction section, the authors give an example on the difference of insolation energy received at 70?N for 114 and 126 kyr ago. It may be shortly explained why these time slices were chosen for the example. [2] In the last paragraph of the Introduction, consider to change the word “believe” into “emphasize”. [3] Check numbering of the equations! Note that the equation rm2 = a2 (1-e2)0.5 is not numbered. [4] In the text below equation 2 consider changing: “In the astronomical theory of paleoclimatesĚĚ”, into “In the astronomical theory of insolation variationĚ”. [5] In the same textblock: consider making reference to justify the use of the Solar constant value used (i.e.1368 Wm-2). [6] Consider changing: “Because of precession, the insolation at SE will alternatively be larger and lower than at FE (see Fig. 1)” into “Because of precession, the insolation at SE will alternatively be higher and lower than at FE (see Fig. 1)”. [7] Consider changing: “let us recall that the perihelionĚ” into “Let us recall that the perihelionĚ” (capitalize “let”). [8] On page five, the authors write: “Ě.if the climate system is supposed to respond automatically to the largerst value of the two”. I consider the word “automatically” a little “strange”. I guess, the sentence reads even better when “automatically” is taken out. [9] When discussing mechanisms that may transmit low-latitude sigals to higher latitutes in the Conlcusions, I emphasize that the Mozambique and Agulhas Currents in the western Indian Ocean should be mentioned as being efficient carriers of such signals as suggested earlier (see Peeters et al., 2004). [10] In the last paragraph of the Conclusions, consider changing: “In this paper we have demonstrated that the spectrum of the insolation forcing at the equator is as informative than in the high polar latitudesĚ” into “In this paper we have demonstrated that the spectrum of the insolation forcing at the equator is as informative as in the high polar latitudesĚ”.

Some comments/suggestions related to the text (grammar, style, typo’s) are given below here: [1] The authors have chosen not to label the vertical and horizontal axes in their Figures. I personally prefer to see labelled axes here. [2] As the evolution of the 24-hour mean irradiance amplitude is considered of the last 100 kyr, the negative numbers on the horizontal axis in Figure 5 should be changed into positive numbers as the axis is labelled Time (kyr BP). [3] In Figure 5 lower panel it is clear that the 100 kyr cycle is composed of a 95 and 123 cycle. This may be confusing to the reader and needs some clarification (in this Figure or in the ms. text).

References used in this review: Clemens, S., 1998. Dust response to seasonal atmospheric forcing: Proxy evaluation and calibration. Paleoceanography, 13: 471-490. Leuschner, D.C. and Sirocko, F., 2003. Orbital insolation forcing of the Indian Monsoon - a motor for global climate changes? Palaeogeography, Palaeoclimatology, Palaeoecology, 197(1-2): 83-95. Peeters, F.J.C. et al., 2004. Vigorous exchange between the Indian and Atlantic ocean at the end of the past five glacial periods. Nature, 430(7000): 661-665. Rossignol-Strick, M., Paterne, M., Bassinot, F.C., Emeis, K.-C. and de Lange, G.J., 1998. An unusual mid-Pleistocene monsoon period over Africa and Asia. Nature, 392: 269-272.

Interactive comment on Clim. Past Discuss., 2, 519, 2006.