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Interactive Comment

Interactive comment on "Simulating sub-Milankovitch climate variations associated with vegetation dynamics" *by* E. Tuenter et al.

Anonymous Referee #2

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Referee comment on " Simulating sub-Milankovitch climate variations associated with vegetation dynamics " by Tuenter et al.

The paper discusses sub-Milankovitch frequencies identified in climate simulations performed with CLIMBER-2 model. The authors try to trace back the origin of these short periods, "sub-Milankovitch periods", i.e. around 20 kyr, 10 kyr and 5 kyr . They suggest that they originate from the vegetation dynamics in response to astronomical forcing. Their hypothesis is clearly and interestingly presented. The paper is well written.

This paper certainly tackle a question of interest in CP. There is indeed another paper on the same subject under discussion (Equatorial insolation: from precession harmonics to eccentricity frequencies, A. Berger, M. F. Loutre, J. L. Mélice Page(s) 519-533.

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SRef-ID: 1814-9359/cpd/2006-2-519). By the way, the authors could also reference this paper in their introduction.

The abstract is giving a very clear summary of the paper content, except for the last line. Indeed the paper did not really discuss the ocean salinity and circulation. In the paper conclusion, the authors even states that the coarse resolution of the ocean model does not permit an analysis of the marine signal in the model. Therefore, I suggest dropping this sentence in the abstract, as it might be misleading. In the conclusion, this sentence only illustrates a perspective for future work.

General comment.

I agree with reviewer #1 that the conclusion reached by the paper about the sub-Milankovitch periods is obtained from one single model, and moreover this model is rather simple. As underlined by the reviewer these results might be an ‘artefact’ of the model used. On my point of view, it does not prevent the paper to be published although I would urge the author to insist on the fact that their results present an hypothesis for the origin of sub-Milankovitch frequencies in climate record but this hypothesis must still be confirmed by other models (or even data if possible). For example, in the explanation of the processes at work (pages 753-754) the authors explain the sub-Milankovitch periods by changes in vegetation and the 5-kyr period by the maximum extent of desert over the whole grid cell. Is there any evidence that desert actually covered the grid box under concern here? I have the feeling that the authors present their results too much as ‘being the truth’ rather than ‘a hypothesis to be further discussed and confirmed’.

Specific comments.

1. The introduction is giving a very large overview of records of sub-Milankovitch periods in both marine and terrestrial climatic records. The same information appears twice in the introduction and I therefore suggest eliminating the sentence ‘These periods ... are found’ (page 747, lines 13-18). The authors suggest that “there is no known forcing at [sub-Milankovitch scale]”. However (as already mentioned above) there is a

paper under discussion (CPD) on that matter. A reference might be added here.

2. All the paper is discussing three major components of the climate system and their evolution through time, i.e. vegetation, precipitation and runoff. I am afraid that these components are very poorly documented in the model description. The basis for the vegetation module is described (the model is a continuous function of the annual sum of positive day-temperature and annual precipitation). Positive day-temperature should at least be explained. A more detailed description (also asked by referee #1) would of course be welcome. For example, it would be interesting to explain which is the most constraining factor for the vegetation, with respect also to the latitudes. On the other hand there is nothing about the hydrological cycle. How is it represented? How realistic is it? And it is the same for the model of soil. Later in the paper the author talk about 'water holding capacity'. This is not precisely defined and there is no explanation of either how it is represented in this model or how good is this representation. This is really missing in the paper.

3. I have some problems with expressions (vocabulary) used by the authors. They defined the Africa monsoon "as the grid box located ...". It is not the common definition of monsoon. Usually, monsoon is described as an atmospheric process. However I can 'accept' this new definition. But with this definition, it becomes almost impossible to understand the meaning of ' the strength of the Africa and Asian monsoon'. What is the strength of a grid box? In the same line, I am not sure what the authors mean by ''the runoff from the tree and the runoff from the grass', ''the water holding capacity of the trees, grass'.

4. section 3.2 on page 765 is very concentrated. That makes it difficult to clearly understand the point that the authors want to underline here. Indeed, this short section cover almost as much material as three times the previous section, which is two pages long. I suggest either to eliminate this section (which might be a pity), or to enlarge it and take some time to discuss the results outlined (which is most probably better).

CPD

2, S380–S383, 2006

Interactive Comment

5. I also fully agree with reviewer #1 about the figures. They are not of quality good enough for publication. They are too small. The lines are too thin. It is difficult to recognise some colours (red and dark yellow). They are too much information on one single figure. Reviewer #1 suggests labelling each figure. I also suggest putting the colour code (legend) within the figure.

6. The paper discusses the appearance of sub-Milankovitch variability under both obliquity and precession. However all the figures displayed in the paper are about variability under precession forcing. It seems that there is no limitation neither in length nor in the number of figures in CP. Therefore I suggest to add at least one figure about sub-Milankovitch periods (1) under obliquity forcing and (2) under both obliquity and precession forcings for the African and Asian monsoon.

7. Some comments about the figure caption.

Figure 4 - 5 . The authors are using the CLEAN transformation to compute the power spectra. I do not know about this method. Maybe some more explanation could be provided in an appendix. For example what are the advantages and drawbacks of the method. Why is it chosen in this case? In particular, I am wondering why this method needs an interpolation (100 year) while the data are obtained with a time step (VECODE) of 1 year. Isn't it rather a 'smoothing'?

Figure 7 (bottom right). This is the only figure giving 'anomalous' values. I am wondering what these 'anomalous' values are. I assume that it is anomaly or deviation from a reference state. What is the reference state? Could the authors explain.

Minor comments

Although the paper is very well written, I would like to suggest the authors to check with a native English speaker some of their expressions, such as 'additional periods NEXT TO a weak period'

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CPD
2, S380–S383, 2006