

Interactive comment on “On the Milankovitch sensitivity of the Quaternary deep-sea record” by W. H. Berger

Anonymous Referee #2

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Prof. Berger has produced a short stimulating paper, which examines whether it is possible to assess the climate systems sensitivity to Milankovitch forcing. Like most of Prof. Berger’s paper is has made me think and in many ways raises more questions than it answers. My detail comments and summary are below.

1. Abstract – the author introduces the idea that understanding climate sensitivity is important if we are to understand global warming. But this is slightly misleading as we are concerned about the sensitivity of the future climate system to carbon dioxide levels not orbital forcing which is the main thrust of the paper. Moreover the role of: orbital forcing, future ice ages and global warming have been discussed at length by others including Ruddiman (2010) and Tzedakis et al. (2012).

2. Abstract – the abstract is also slightly misleading when is discusses the sensitivity

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changes through the Tertiary. It correctly suggests that climate variability has increased due to the build up of continental ice sheets. But I worry that others may interpret this to mean that climate sensitive has also increased, as this is not necessarily the case. As there are model runs that suggest an ice free world is more sensitive to increasing carbon dioxide than today.

3. I am concerned about the idea of defining “Milankovitch sensitivity (MS)” because it is clear that knowing the orbital characteristics does not allow one to define the climate conditions. This is because the direction in which the climate system is moving defines how it will respond to orbital changes. Hence the sensitivity to orbital forcing is different going into a glacial period or rebounding out of one. Hence MS is a problematic idea which has no unique solution and is inherently non-linear.

4. The discussion of the “100-kyr problem” is out of date and really needs to add in the discussion of obliquity vs precessional forcing. It is hard to ignore all of Peter Huyber’s obliquity papers (see below) even if he is completely wrong. See Ridgwell et al. (1999) and Maslin and Ridgwell (2005) for the precessional argument.

5. Page 1239 – I am also concerned that MS is being defined by benthic oxygen isotope record which of course combines deep ocean temperature and salinity with a non-linear record of ice volume. This means that the sensitivity of the climate system with regards to global temperature, sea level and real ice volume is not measured.

6. Page 1240 Prof Berger is quiet correct when he stresses that orbital forcing should be seen as a ‘change’ forcing. But I am unsure why it is the proxy record that should be derived. I would suggest the rate of change of the insolation curve would show the forcing ‘power’ and would be a better comparison with the proxy data. I also suggest that both the stack benthic oxygen isotope and Antarctica ice core temperature records could be used.

7. Page 1241 there seems to be no acknowledgement of possible offsets and delay responses within the system, especially if the author is relying primarily on the albedo

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feedback.

8. I am really sorry I do not like the term “Deaf zone”, as it is not very descriptive nor an elegant term. Because what the author is implying is that in this period the influence of orbital forcing has been dampened or other feedbacks are occurring which are influencing the oxygen isotope records.

9. I am not sure what the author means by ‘zero influence’ of MF on the climate record. Because there are clear glacial-interglacial cycles during this period of time and at about 420 ka there is one of the sharpest deglaciations in the last 3 million years.

10. I am also concerned about Figure 7 as a 50 ky window has been used and I would have liked to see smaller windows presented because much of the earlier variability in the record is at the 41 kyr power and hence it may be lost at larger window sizes.

11. Page 1243 – section 2.2. is unclear and there should be a much better description of the fundamental shifts occurring in the climate system over the Plio-Pleistocene. The shift from smooth gentle 41 kyrs glacial-interglacials to the saw-toothed threshold influenced quasi-100 kyrs glacial-interglacial cycles. I would have liked to see a better description and review of the currently literature and ideas on the Mid-Brunhes Event. As this event occurs at the end of the so called ‘deaf zone’.

12. Figure 1 is not required and again is misleading as it seems to imply that this paper has implications for our understanding of future global warming and as far as I can see it does not.

The relationship between the global climate system and orbital forcing is complex and there so is the idea of Milankovitch sensitivity. First it depends on the general state of the climate system and whether obliquity (pre Mid Pleistocene Transition) or eccentricity-modulated precession (post MPT) dominates glacial-interglacial cycles. Second it depends where you are within the glacial-interglacial cycle whether orbital forcing has an influence. The classic example of this is that 18,000 years ago and to-

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day we have very similar orbital characteristics but very different climate. Hence there is no single unique level of Milankovitch sensitivity for any given combination of orbital parameters. Third it is difficult to assess sensitivity in a threshold system, which requires preconditioning i.e., very large continental ice sheets are required for deglaciation. In summary I am not sure what would be gained if we could define Milankovitch sensitivity. I hope I have demonstrated despite Prof. Berger's attempts it is not possible to define Milankovitch sensitivity in any meaningful way due to the complexities outline above. This does not mean we should stop trying to understand the influence of orbital forcing on the global climate system as it is one of the most complex and interesting of all our palaeoclimate problems.

Additional References that should be considered:

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