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Climate of the Past
Discussions

Interactive comment on "Obliquity forcing of low-latitude climate" *by* J. H. C. Bosmans et al.

Anonymous Referee #1

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This paper analyses climate simulation results from a complex Earth system model to understand the influence of obliquity on low-latitude (thus tropical) climate. In detail, it is a more extensive (global) analysis of simulation results which are already described elsewhere, where previously the focus was more local (Africa, Mediterranean).

I found the described idea interesting. Thus, in principle this paper should be published in Climate of the Past. However, I believe some more details need to be shown and discussed before the idea is really convincing and supported by the paper. Thus, I have concerns about three major issues, (a) on the originality of the proposed idea, (b) on what the experiment is really telling us and (c) on a missing wider discussion.

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(a) Originality of the proposed idea:

Right now the paper cites as original source of the Summer Inter Tropical Insolation Gradient (SITIG) hypothesis a paper by Lourens and Reichart (1996). It turns out that this is one chapter out of the PhD thesis of G.J. Reichart, which never made it in the peer-reviewed literature. While L Lourens is coauthor of the present paper here, G.J Reichart is not. Furthermore, from the given citation it was not possible to figure out who the overall author of the PhD thesis was.

I found this proceeding questionable. Since this chapter by Lourens and Reichart (1996) was never published in peer-reviewed journals (for reasons we do not know), it needs to be considered unpublished. It is similar to a paper submitted to Climate of the Past Discussion, that is rejected: The paper is available online in submitted, but not in peer-reviewed form. I urgently suggest, this is not a reference, that should be used here. If any it might be mentioned in the discussion that some ideas on SITIG were proposed in the whole PhD thesis of GJ Reichart (then citing Reichart 1996, PhD thesis, not a single chapter).

So one needs to acknowledge that the origin for any kind of low-latitude index on climate was the monsoon index proposed first by *Rossignol-Strick* (1983) (not cited by the authors, the authors cite another, later paper of this author). It was later changed to something similar as proposed here by Leuschner and Sirocko 2003. It might be worth investigating, which of the ideas is better, e.g. by revising Fig 6 and also including the other 2 indices here.

(b) What is the experiment really telling us:

Right now it is argued that with eccentricity switch off, and analysing the effects of changes in Earths tilt from minimum to maximum with an Earth system model without land ice it is analysed what the response of the low latitudes climate to these respective insolation changes is. I think this approach needs more evidence and information for the following reasons: The effect of land ice sheets on climate is (at least) twofold: First, higher surface albedo changes the radiative balance, second the height of the ice sheet changes in the atmospheric circulation and thus climate. The later was recently shown to be potentially the reason for abrupt climate change (*Zhang et al.*, 2014), and this surely played no role here (no ice sheets in the model). The first however, still needs to be shown. Thus, the authors should analyse the difference in surface albedo in high latitudes. Maybe winter snow is not melting anymore in summer (probably not building an ice sheet, because of the short simulation time, and because the model might not be able to do so), thus having a similar effect on the radiative balance than an ice sheet. So in a first step, at least the albedo changes need to be described. If it turns out that albedo changes were still high (which I would suggest they are) it might be necessary to think about an additional simulation experiment, in which high latitude surface albedo is fixed in order to stick to the bold statements that high-latitude changes are not important for tropical climate change on obliquity time scales. It is up to the authors to decide here how to deal with sea ice.

Furthermore, to be able to assess the impact of the chosen simulation scenarios a figure showing incoming radiation (insolation) as function of latitude and season (month) for both scenarios Tmax and Tmin and the difference of both is needed.

All analysis (figures) are restricted to the latitudinal band between 50°N and 50°S. Thus, it is impossible to judge, if changes in the high latitudes occur and potentially have an effect on tropical climate. I therefore suggest to change the figures to show results from 90°N to 90°S.

(c) Missing wider discussion:

So far, it is proposed that the SITIG hypothesis is for the tropical regions an alternative to the Milankovitch hypothesis, which proposed that summer insolation changes at 65°N is responsible for climate change on orbital time scales. They furthermore

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mention that a similar idea to that of SITIG was proposed by Leuschner and Sirocko (2003) and some other ideas about monsoon strength. However, what I believe is missing here is how this idea is related to / or different from that of *Laepple and Lohmann* (2009), in which the local seasonal cycle in insolation and temperature are related. *Laepple and Lohmann* (2009) nicely show, that local insolation might be more important to local climate change on orbital time scales than the concept of explaining everything with incoming light in summer at 65° N. This idea seemed at least to be applicable to Antarctica (*Laepple et al.*, 2011), but for the tropics the analysis are more diverse and less significant in *Laepple and Lohmann* (2009). So some comparisons and discussions are necessary here.

There are other hypothesis on the obliquity forcing mainly on the idea how and why high-latitude ice sheets change, (e.g. integrated summer insolation by *Huybers and Tziperman* (2008)), so they might not be important here.

Minors:

- First cited figure is figure 6, so this should become figure 1.
- I found the acronym *T*min and *T*max for scenarios with low and high obliquity rather confusing. Tyically in climate science *T* is the temperature. So I would suggest to use a different variable for obliquity. However, I also realized that in other papers of the same authors analysing the same simulations these acronyms were already introduced, so I have little hope here.
- Citation Bosmans et al 2014 in Climate Dynamics was published in 2015.
- Citation Leuschner and Siroko 2003 has an incomplete journal name.

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- Please give a link to PhD thesis of GJ Reichart 1996 (available online at Utrecht University) and correct the citation Lourens and Reichart 1996 to Reichart 1996 (PhD thesis), if still cited in the discussion.

- Fig 6: Please use the same scaling for both left and right y-axis (both give insolation or insolation change in W m^{-2}) in subfigure 6a. Now left shows 140, right 100 W m^{-2}, exaggerating the influence of SITIG. Please show curves in different colors in both subfigures. Include results for the other two hypothesis (monsoon index, index calculated by Leuschner and Sirocko 2003).

References

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Interactive comment on Clim. Past Discuss., 11, 221, 2015.