The paper has strongly improved in the review process and I now recommend publication in Climate of the past after minor revisions.

My remaining comment concerns the still limited discussion about insolation and the insolation imprint in paleorecords as pointed out in my first review (third comment) and the comment (c) of the other reviewer.

Many aspects of the insolation forcing have essentially identical variability, and thus from just looking at the spectra of a record (especially just the amplitude ratio of precession and obliquity), many possible explanations remain (for an extreme example, see Huybers, Science 2009 which discusses an ice-core record but the same would apply to any geological record)

Classical 65N summer insolation + ice-sheet response + effect on low latitudes is one hypothesis, and using forward modeling, the presented study offers important insight in a more realistic alternative (as there was obliquity variability without ice-sheets). However, the wider discussion of local hypotheses in the literature (e.g. Short et al., 1991, Laepple and Lohmann, 2009 who explicitly claim that one would expect obliquity from local forcing in the tropics) is not presented to the reader.

The authors argue that the Laepple and Lohmann, 2009, hypothesis can't explain the obliquity signal as the amplitude would be too weak, but exactly this point would be the needed discussion for the reader to put the presented study in the context of the existing literature.

Huybers, Peter. „Antarctica's Orbital Beat." Science 325, Nr. 5944 (28. August 2009): 1085–86. doi:10.1126/science.1176186.
Short, David A., John G. Mengel, Thomas J. Crowley, William T. Hyde, und Gerald R. North. „Filtering of Milankovitch cycles by Earth's geography." Quaternary Research 35, Nr. 2 (1991): 157–73.
Laepple, Thomas, und Gerrit Lohmann. „Seasonal Cycle as Template for Climate Variability on Astronomical Timescales." Paleoceanography 24, Nr. 4 (1. Dezember 2009): PA4201. doi:10.1029/2008PA001674.

Author response:

We agree that the discussion can be extended. We initially did not include the local mechanism (e.g. Short et al, Laepple and Lohmann), as it is a different and in our view not a correct explanation for obliquity signals at low latitudes. However, the reviewer is correct that it should be included in the discussion, placing our current work in a wider context. The discussion will be extended along the lines of the comments given on this point in the previous rebuttal:

The study of Laepple and Lohmann (2009) applies the relationship between present-day temperature and insolation to the past, which gives overall a good indication of temperature changes in the past. Their conclusion that local insolation might be more important to local climate than 65N insolation is similar to ours, but our study focusses on more than temperature and local insolation. We focus on changes in for instance precipitation or moisture transport through the interhemispheric insolation gradient that can help explain obliquity signals in for instance the sapropels. Furthermore, applying the relationship between present-day temperature and insolation to the Milankovitch cycles would result in a very weak obliquity signal in temperature over the tropics, as insolation over the tropics has very little obliquity variance (see Figure 6 of submitted paper, Figure 1 below). It can therefore not explain the obliquity patterns in low-latitude proxy records.

Short et al 1991: this paper hardly discusses the effect of obliquity at low latitudes and only focusses on temperature, not precipitation and circulation (not possible with an EBM). It is therefore not referenced.

September 10, 2015
Journal: CP
Title: Obliquity forcing of low-latitude climate
Author(s): J.H.C. Bosmans et al.
MS No.: cp-2015-9
MS Type: Research Article
I am reviewer 1, I can agree on the repsonses to my earlier review, but I still see some things that need to be clarified:
1. lines 60-61: Show somewhere that SITIG fits better to the sapropel record than other indices. This conclusion cannot be drawn from Figure 2 right now. Maybe this was in detail already shown somewhere else, then give the reference to this at the end of the sentence.
Author: In lines 60-61 a reference to section 3.1 will be added, where Figure 2 is discussed in more detail. See also point 6.
2. line 95 "obliquity signal on the tropics" change to "obliquity on tropical climate".
Author: this will be changed as suggested.
3. lines 93-96: Hazeleger et al 2011 is the reference to EC-Earth, and should be given, when EC-Earth is mentioned as tool first (line 94). You then mention, this was used in the IPCC AR5, so then please give a full reference of the relevant IPCC chapter, in which it was used, or of the whole IPCC AR5.
Author: References to AR5, WG1 chapters 9 and 12 are added (Flato et al, Collins et al)
4. line 98: What do you take from Tuenter et al 2003? the details of the simulation scenario, please say so.
Author: Instead of "Following Tuenter et al..." it will now say "We performed two idealized obliquity experiments, using the same experimental set-up as Tuenter et al: one with a low obliquity..."
5. line 99-100: When you introduce Tmax and Tmin, please mention once, that "T" is for "tilt".
Author: this will be mentioned as suggested
6. line 112: Match to the sapropel records. Again, I can from Figure 2 not see any curve that fits better than the other to the sapropels. Some improvement is necessary here, maybe another record, maybe some frequency analysis.
Author: 65N 21June insolation and SITIG have a very similar pattern and a very similar (normalized) power spectrum (Figures 2, 3b). Both match the sapropel record in terms of both sapropel occurrence (during precession minima) as well as the thick-thin alternation (relatively strong - weak insolation maxima). The latter is related to the additional influence of obliquity, or more correctly, precession-obliquity interference (Figure 4 in Lourens 1996 shows this interference). A frequency analysis on the sapropel record can in principle be carried out by assigning different values to different lithologies (for instance 0's for marls and 1's for sapropels), or by measuring color or any other proxy related to sapropel cycles for time series analysis. However, such patterns have been described and statistically analyzed in many papers about Mediterranean Neogene sapropels (e.g. Ti/Al in the Plio-Pleistocene record of Lourens 2001, color patterns in Zeeden 2014). Hence, we refrained from repeating this in the present paper, as it is clear from the literature. Nevertheless, we have added additional lines in Figure 2 connecting sapropels with precession minima and insolation maxima - making comparison of individual peaks / sapropels easier. We will also mention the existing literature more elaborately in the new manuscript's section 3.1.
7. line 132: Say BY HOW MUCH the insolation gradient varies, I get 20 W/m2 from Fig 1.
Author: the difference between the minimum value of SITIG and the maximum value for this time period is ~50W/m2, so maximum amplitude is ~25W/m2. In line 132 we'll add that the gradient can differ up to 50W/m2 (at times of high eccentricity, i.e. strong precession

variability).
8. line 143: paleoclimate recordS
Author: this will be changed to recordS
9. line 155, 159: Precipitation over Sahara and S America and monsoon winds (not shown). Please focus your text on what is shown, so describe the figures you plot here.
Author: we chose to add the line on the relative changes in precipitation, next to discussing and showing the changes in absolute sense (Fig 4) in order to make clear that obliquity-induced changes in precipitation are large (can be >100%) despite small (local) obliquity-induced insolation changes.
10. line 160: include "wind speed change IN BOERAL SUMMER are small".
Author: this will be added as suggested
11. line 163: change "net precipitation DURING AUSTRAL SUMMER increases".
Author: The beginning of this sentence (161) already states "During austral summer"
12. line 167: Why should that be in agreement with the SITIG meachnism? Was this already written somewhere, if so cite, if not explain.
Author: this refers back to the introduction and section 3.1, where we explained that cross-equatorial insolation gradients such as SITIG acknowledge the role of cross-equatorial moisture transport, related to changes in the (winter hemisphere) Hadley cell and hence stronger surface winds towards the summer hemisphere. We will add a "see Section 3.1" reference to this sentence.
13. line 173, fig 7 is referred to BEFORE Fig 6, so change order of figures.
Author: Figure 6 is referenced in the caption of Figure 4. We will add a reference to it in the text in the first to paragraphs of section 3.2 (so we will switch figures 5 and 6 around).
14. line 208: The long form if the acronym SITIG is again explained, not necessary.
Author: Long form will be removed as suggested.
15. line 222: "Winter (intrahemispheric) insolation gradient does not vary with obliquity". According to your Figure 1, this statement is wrong. Maybe you should include which points you want to compare here in Figure 1 to guide the reader. Please check and correct, if necessary.
Author: Figure 1 does not show a winter intra-hemispheric insolation gradient, only a summer intra-hemispheric gradient (M, Rossignol-Strick) or cross-equatorial gradients (SITIG, ISMI). The statement that the winter intra-hemispheric gradient does not vary with obliquity is taken from the Davis and Brewer 2009 paper, and used here to show that previous explanations for obliquity signals at low latitudes (as in Rossignol-Strick, 1985, Larrasoana, 2003) are incorrect.
16. line 235: "P" and "T" are never explained, also not what is meant with "P-1/2T" in detail. Does it refer to annual mean or any specific dates?
Author: P stands for precession and T for tilt (obliquity). The P-1/2T curve is the sum of the (normalized) orbital parameters of precession and obliquity; the latter multiplied by -1/2 to acknowledge the stronger influence of precession on (tropical) insolation. This curve does not show insolation change but purely change in orbital and therefore can be used without making any direct assumptions on climatic mechanisms. Since P and T refer to the orbit, the curve does not refer to any specific dates / seasons. This will be explained in more detail.
17. line 242: SITIG is NOT a BOREAL signal, something wrong here.
Author: here we do not say that SITIG is a boreal signal, "boreal" here means we use I_{23N}-I_{23S} at June 21st instead of I_{23S}- I_{23N} at December 21st. Instead of "(boreal) SITIG" we will say "SITIG (I_{23N}-I_{23S} at June 21st)"
18. line 244: change "reconsidered" into "rejected"
Author: we choose to keep "reconsidered", as even though we find strong support for SITIG explaining low-latitude obliquity signals without ice sheet changes, more work is needed to

investigate the relative roles of SITIG, equator-to-pole insolation gradients and ice sheets.
19. line 246: change "model results" into "analysis presented here"
Author: this will be changed as suggested
20. line 287, 289: Do not refer to NOT SHOWN results, so include new figures or reduce discussion.
Author: instead of NOT SHOWN we now refer to Figure 5 (6 in new version). We initially said NOT SHOWN because our figures focus on 50S:50N so do not show all of the areas outside the tropics, but some enhanced moisture transport outside the tropics during Tmin can still be seen in these figures.
21. line 308: change "reconsidered" into "rejected"
Author: See comment 18
22. Figure 1: Explain what Tmax and Tmin mean in the caption, obliquity is not even mentioned here.
Author: T stands for tilt, we will also add that Tmax is maximum obliquity and Tmin is minimum obliquity.
23. Figure 2: The lower part (sapropels) are discussed in the text to be more in line with SITIG, actually, I do not see any of that. It was said that the weakness of the sapropel signal in S4 and S7 with respect to the others is a key component of the obliquity imprint in the data, and the relation to minima in the insolation indices. If this is really one key messsage, then please introduce vertical lines on these 2 intervals to guide the eye, but also think about other sapropel records to be shown here.
Author: vertical lines are now added, which should make it more clear for the reader that the weaker sapropels S4 and S7 are related to weaker precession minima (which are related to precession-obliquity interference, reference Lourens et al 1996). References given in the caption show that other (older) sapropel records show the same obliquity interference.
24. Figure 3: consider a frequencey analysis of the sapropels to be included here, maybe also on a longer time series to have a robust signal.
Author: As mentioned before (see comment 6), such patterns have been described and statistically analyzed in the literature. This will be elaborated on more in the new manuscript.
25. Figure 8: Changes in the Hadley cell. Please mention the interpretation of changes in the Hadley cell as given in the discussion also in the caption of the figure. I am note sure the annual mean changes in the Hadley cell (Fig 8c) are important. From the line of argument given I had the feeling, all is said with the description of the changes in subfigures 8a and 8b (JJA vs DJF).
Author: we include figure 8c to show that despite strengthening of the Hadley cell in the seasonal mean, the annual mean circulation is weaker. This fits with stronger SITIG and at the same time weaker annual mean equator-to-pole-gradient (lines 192-198). An interpretation of Figure 8 will be added to the caption.