

Interactive comment on “SST phases in the open-ocean and margins of the tropical Pacific; implication on tropical climate dynamics” by L.-J. Shiau et al.

Anonymous Referee #2

Received and published: 21 July 2014

The manuscript discusses some interesting new SST and d18O data from a Coral Sea record and compare them with several, published SST records from the tropical Pacific to try to reveal underlying mechanisms driving surface temperature changes. In general, I found the manuscript too long and unclear over some parts. It could definitively be shortened and more focused.

In order to discuss convincingly leads and lags between various forcings and paleorecords (ie. insolation curves, ice core GHG records, ice volume, and SST variations from sedimentary cores) the KEY issue is the proper set up and discussion of age models. This aspect being central requires an in-depth discussion in the manuscript, which is clearly lacking in the submitted version. I do not think that the final "adjust-

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ment" to the 1984 SPECMAP record can be considered satisfactory enough. Not only because this stacked, planktonic d18O record is not the best reference record available nowadays as far as global (ie. ice volume) signal is concerned. But also because one really needs a proper, in-depth evaluation of how age models were developed (tuning target? Selection and density of tie points?...), the impact of their temporal resolutions on phase estimates (resolution varies apparently from ~ 200-300 yrs for MD05-2928 to about 6 ka for ODP Site 806) and their respective uncertainties. The discussion about uncertainties should take into consideration the potential diachronisms that may exist between ice volume (global) signal recorded in planktonic and benthic records or between benthic records (ie. see the work by Skinner and Shackleton, Waelbroeck et al,...). If 14C dates are available for some of the records used in this manuscript, they should be discussed and the coherency between "the orbital tuning" approach and the "radiocarbon" approach should be addressed over the last 40ka and, in particular, across the last deglaciation. (other remarks are available below).

The English needs to be improved significantly. There are numerous grammar problems and many cases of language misuses. I doubt that Steve Clemens - second author of this manuscript and a native english speaker - had the chance to read and correct the text before it was submitted.

In conclusion, there is a need for major improvements before this work can be published in CP and, at present, I consider that this manuscript should be rejected and resubmitted once properly modified.

***** Introduction The introduction is rather fuzzy. It lacks (i) well-identified questions that the manuscript will try to address as well as (ii) a thorough explanation of the strategy that will be used to answer those specific questions. The description (location, type of sediment, modern oceanographic settings) of the new site MD05-2928 does not bring much to the introduction and is rather misplaced here (should be part of a "material and method"). In the introduction, one would rather want to know why this new site - in conjunction with other, published sites - can potentially

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help answering some of the key questions raised about SST variability in the tropics.

page 1859- Lines 2-5 : "Solar radiation incident on the large area of the equatorial Pacific triggers atmosphere and ocean circulation that result in the transport of heat and moisture from low to high latitudes"... As indicated later in the manuscript, latitudinal insolation gradients are important in driving atmospheric and oceanic heat exchanges. Thus, low latitude insolation is one important component, but not the only one and insolation and temperature changes at mid and high latitudes are key players as well.

Age model

As said above, since the main aspect of this paper is comparing SST records and dealing with lead/lag issues, age models are absolutely CENTRAL to the whole discussion. Thus, the age model should be described thoroughly and all the pieces of information should be given in this manuscript (and compared carefully with age models from the other records used for comparison). Ideally, to remove any ambiguity, the same strategy should be followed for developing age models of all the records; ie. same tuning target (LR04), same strategy for the localization of tie-points, ..

Page 1861, line 10: Rephrase (and develop) "we compared oxygen isotopes with LR04" in "we synchronized the oxygen record to LR04 using X tie points.. etc ..".

Uk paleothermometer

Page 1861, line 18 : "modern sediment trap observation on western equatorial Pacific indicates the C37 alkenone flux is with three maxima at February, June and November". Where are located the sediment traps? Is this observation valid for the Coral Sea site?

What is the depth interval used for the estimation of SST from the oceanographic atlas (figure 1, a & b). Is it only the surface (0m), or does it take into account a mixed layer (0 to XXm ?).

Conte et al. (2006) showed that the calibration using surface water Uk37 and alkenone production temperature differed significantly from the calibration based on core top ma-

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terial and using annual mean temperature. The sediment calibration was best fit with a simple linear model, whereas they used a non-linear model for their water calibration data. These authors also indicate that "Uk37 in surface sediments is consistently HIGHER than that predicted from Annual temperature and the surface production temperature calibration, the magnitude of the offset increasing as surface annual SST decreases" (Conte et al 2006), the offset being likely associated to differential degradation of 37 :3 and 37:2 , seasonality and/or thermocline production... I believe that all these elements should be indicated in the text as they may also help the interpretation of Uk37-SST data and the potential differences with other temperature proxies (ie. Mg/Ca), together with seasonality and depth problems.

Are there data available from the Coral Sea that make it possible to decipher what is the depth of maximum Uk37 production?

Because Mg/Ca data are also used in the manuscript, there should be a thorough presentation of this method and a rapid discussion of potential drawbacks and differences between Mg/Ca and Uk37 temperatures (ie. seasonality, depth, preservation issues).

Results

1862 – line 14 : Temperatures "truncated" ? I do not understand. Do the authors mean that several Uk37-temperatures reach the asymptotic temperature maximum of the "non-linear" Conte et al's calibration equation ... or do they mean that the Uk37 values exceed the values measured in seawater by Conte et al, and that they decided to cut those values out?

1862 – line 15 and 16 : MIS 5.1, MIS 5.3, etc.. should be labeled on figure 2. As far as isotopic stratigraphy is concerned, 5.1, 5.3, 7.1... are not "interglacial periods" they correspond to interglacial maxima (peaks). Isotopic interglacial periods (=intervals) are labeled with letters (5a, 5c, 5e,...).

In Figure 2, the differences between planktonic d18O records, on the one hand, and

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benthic d18O records, on the other hand (especially across isotopic stage 5) does readily point out that these records do not uniquely convey a global, ice volume signal but also contain local/regional effects (temperature, salinity). This somehow limits the use of those d18O records as strict proxies of ice volume changes. Thus, this raises some questions about leads/lags estimated between SST data and those d18O records. This issue definitively requires a careful discussion.

Page 1863-line 9 : "these modern seasonal amplitude carry through the paleo record". Long-term (orbital) amplitude and seasonal amplitude (which is smeared out in paleo-records due to the combination of low sedimentation rates and bioturbation) are not necessarily linked. Thus, the choice of the word "carry" is quite misleading in this sentence.

Page 1863, lines 11 to 21 : Because the whole discussion is based upon leads/lags between the different SST records and with other records (insolation, GHG), age model is THE major key issue of this work. There is a need for a more detailed discussion about the different age models, their coherence, and their uncertainties. Saying "we have assessed the various age models in order to compare them with confidence" is far from being enough. Key pieces of information are missing, here : - What about the time resolution of the different records ? Are they all equivalent ? (clearly not, from what I can see from figure 3 where Site ODP 806, for instance, has a much poorer resolution than MD05-2928). What are the implications in terms of uncertainties when estimating lead/lag at the different Milankovitch frequencies ? - what are the records that have 14C ages for the chronology of the last 40 ka and the last deglaciation ? - have all the benthic records been tied to the same "target", reference record than the one used for MD05-2928 (ie. LR04) ? If not, are those target records "coherent" with LR04 and the age model developed by Lisiecki and Raymo? (what about, for instance, the age model of the ODP Site 806 developed by Bickert et al. in 1993 or that from ODP 846 developed by Liu and Herbert in 2004 ?). - Following the work by Shackleton, Skinner and others, there have been serious questions these last years regarding the

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synchronism of benthic records in the world ocean. On intervals properly dated with good 14C dates, these authors have shown that there exist phase differences (diachronisms) between remote benthic records, located in different basins or at different water depths. As far as planktonic records are concerned, the question is even more acute due to the relative importance of local (temperature, salinity) effects, which can largely exceed the amplitude of the global, ice-volume effect. These questions of synchronism and local versus global effects on d18O are key to the discussion. They should be discussed in the manuscript. - In records where BOTH isotopic records - planktonic and benthic - are available, the authors should test the coherency and phase of these two signals at the different Milankovitch bands through cross-spectral analyses.

Page 1864 – lines 11-18. I did not really understand why the d18O records have been "adjusted" to have them in phase with the SPECMAP record. Apparently, the authors refer to the SPECMAP planktonic record (Imbrie et al , 1984), which may not be the best reference material for global ice volume changes. If a SPECMAP record need to be used as a reference, why not using instead the benthic record published by Martinson et al. or why not using directly LR04 ?? (which is the new reference target used for developing age models, including that of MD95-2928).

Page 1863 lines 25 to 28 and page 1864 lines 0-2 : The authors may be correct in interpreting lower temperatures in ODP1146 and MD97-2142 as reflecting colder winter monsoon seasons... But it is always dangerous somehow to strictly interpret long-term paleo-temperature changes from seasonal variations without a careful study of production seasonality. Paleo- SST records are smoothed by bioturbation. This smears out seasonal, inter-annual and long-term (centenal to millennial) variability. How can the authors be sure that a long-term decrease in temperature is indeed related to more severe winter monsoons, especially if alkenone production shows several peaks during the year? The long-term changes could also be related to less intense summer seasons, increased production of alkenones during the cold seasons/years without necessarily implying a change in SST (-> more relative weight to the winter production),

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etc. . .

1864 line 11-13 : I did not really understand the sentence (also problem in wording: reactive -> relative).

1864 lines 5-10 : Cross correlation with ETP is clearly not the best argument to conclude about the strong link between equatorial SST and ice volume since ice volume changes are not strictly (linearly) related to ETP variations, particularly in the eccentricity band. Why not performing cross-spectral analyses between SST records and records of global ice volume changes (ie. Waelbroeck et al's work ? Rohling et al's work?).

Page 1865 – lines 18-25 : the authors describe the differences in GHG as if they were somewhat surprised about those differences. Isn't it obvious that those GHG should show differences since they are driven by different mechanisms? (CO₂ -> marine circulation, brine formation, export carbon production ; CH₄ -> wet land expansion, high latitude permafrost extension, clathrates destabilization).

Page 1866 – lines 18-20 ("Thus, MD052928 is ideally located to monitor. . . WPWP on orbital time scale"). This argument should come earlier in the manuscript (even in the introduction?). This helps to explain the interest of this new SST record.

1866 – lines 19-24. There should be a figure showing (zoom) the difference between terminations II and III (SST leads d18O by 4 kyr) and termination I (signals almost in phase).

Page 1867-lines 3 to 6. I'm not quite sure why the authors have chosen to emphasize the June insolation, estimated at the latitudes of the different sites (north or south to the equator). In particular, I don't understand on which basis did the authors conclude that the MD05-2928 SST is strongly correlated to June (winter) insolation in the southern hemisphere. Is it only on the basis of phase relationship (ie. minimum phase relationship between SST and June 10°S insolation)? Don't the authors think that the fact that

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MD05-2928 SST oscillations vary in phase with June (boreal summer) insolation appears to contradict somehow modern observations, which show that SST variations in this area are coherent with southern hemisphere seasons rather than northern forcing ? (cf. page 1866, lines 12-14: the authors indicate that maximum SST at site MD05-2928 occur during austral summer (-> boreal winter), and minimum SST are measured during austral winter (-> boreal summer), when the WPWP contracts northward).

Page 1867, lines 19-23 : The authors should be extremely cautious when comparing amplitude of SST changes based on different proxies (Mg/Ca for Site 806 and MD97-2140) and Uk37 (for MD05-2928). I think they are correct in assuming that their observations of sites 806 and MD97-21490 showing large, long-term changes in SST amplitude can be largely explained by a proxy issue.

Page 1868, lines 7-9 : If temperature changes in the SCS, in particular in the northern part, are more strongly related to winter monsoon dynamics than to summer monsoon dynamics, why did the authors have decided to focus their comparative study on June insolation (ie. fig 6) ? Again, the rationale behind the insolation control is not clear to me.

Page 1869 and following pages : discussion about leads/lags (eccentricity and obliquity). I got confused regarding the "early group" and "late group" when reading the text and looking at the figures 5 and 6. The authors should make it more clear that the grouping in "early" and "late" groups is frequency-dependent and that the early/late groups in the eccentricity/obliquity bands are not the same than the groups for the precession band. Maybe different wording should be used in the text to avoid confusion. In general, the whole discussion is a bit confusing, with redundancies. It is a bit difficult to read. . . also due to the bad English. The entire discussion chapter needs to be rewritten and clarified.

Page 1870, line 6 : This is actually an anti-phase relationship, with maxima in SST in the obliquity band being correlated to minima in the equator-65°N insolation gradient.

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Page 1870, line 7 : The fact that insolation gradient is maximum when obliquity is minimum is not based on "Raymo and Nisancioglu's hypothesis", this is a result of calculations made with insolation curves. . .

Page 1870, line 8-11 : Why do the authors conclude that the warming (cooling) associated to reduced (increased) equator-65°N insolation gradient is associated to the strength of the equatorial upwelling system. Can't a large part of heat excess stored in the low latitude during periods of low meridional gradient be also explained by a reduced latitudinal export of latent and sensitive heat by atmospheric and oceanographic circulation ?

Page 1872 lines 12-15 :As far as precession is concerned, the late group comprises one marginal site (ODP 1146) and two sites for which one has planktonic d18O records (MD97-2140 and MD97-2142). Can the apparent "late" SST relative to "the ice volume signal" be associated to the fact that the d18O record does contain some local, early hydrological responses ?

Page 1873, lines 25-28: "Its phase also indicates slightly lead than sites in the SCS on the precession band that may suggest. . .". This is just an example of a quite unclear sentence (English problem..), that makes it difficult for the reader to properly grasp the author's idea. Other examples like this are numerous (eg. page 1874, lines 0-5; 1874: lines 20-24; etc.. etc. . . etc..). Sometimes, probably due to English problem and bad sentences, it even seems that the authors reach opposite conclusions in different parts of the manuscript. Example: Page 1866, the authors indicate that SST of MD05-2928 show high glacial-interglacial amplitude, larger than in the central WPWP area, making this site particularly suitable for studying the warm pool variations . . . whereas in the conclusion (page 1880, line 20-21), they conclude apparently the opposite: "however, the lower SST variation than in the central WPWP that may be attributed to the local character of the Coral Sea". This is quite confusing. . .

In general, the whole discussion chapter needs some careful rewriting to shorten it

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(much too long and wordy other some intervals) and clarify it .. a lot...

Interactive comment on Clim. Past Discuss., 10, 1857, 2014.

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