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Interactive comment on "Evolution and forcing mechanisms of ENSO over the last 300,000 years in CCSM3" *by* Zhengyao Lu et al.

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We wish to sincerely thank both reviewers for careful reading of the manuscript and their thoughtful comments and criticism that have helped us improve its quality. We have addressed all comments raised by the reviewers to the best of our knowledge, and hope the revised version could better guide the readers through the methods we used and the mechanisms we proposed.

Below, we copy all our response in Plain text. They will also be uploaded as the supplement in one pdf file where in the response we copy the reviewers' comments in bold and blue color, followed by the point-to-point reply, together with the revised manuscript and supplementary figures.

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Response to Reviewer#1

1 Scientific Comments Objection#1: A poor model No model is ever perfect, and the authors do acknowledge that. However, I am concerned that the excessive semi-annual cycle, the absence of a combination tone, and the very strong link to the annual cycle (see Objection 2 below) really limit the generalizability and usefulness of these simulations.

We acknowledge that our model may ignore some important mechanisms that is associated with ENSO dynamics as recent studies reveal. However, there is still an improvement of the model from the latest study of this topic (Timmermann et al., 2007), by which we show a possible mechanism of complete difference. Single model study is indeed not enough to fully understand this difficult question (ENSO sensitivity to the orbital modulation). We hope our study can inspire more related studies using other models. The semi-annual cycle is caused by the shift in phase of the annual cycle, a point we did not notice before (and thought it to be a bias!) and failed to demonstrate in the previous manuscript. Actually, the tropical Pacific annual cycle in the accelerated simulation is quite reasonable: it agrees with the non-accelerated TRACE-ORB in amplitude (Fig. 1g), and agrees with GFDL model snapshots (Erb et al., 2015) in phase. The phase modulation by precession has been studied in another paper by us (Lu and Liu, 2017), in which more analysis is presented. We have revised Sec 6.1 to comprehensively discuss the model performance in the tropical Pacific and the acceleration effect. The model performance issue is also noticed by reviewer#2 (comment 4), and a similar reply can be found.

Objection#2: Frequency entrainment

The authors also seem to believe in the frequency entrainment mechanism as an explanation for virtually everything. Although it does seem to explain the orbital response of most PMIP3 models, it does not apply to all GCMs, especially a more realistic one [An, SI. & Choi, J. Clim Dyn (2013) 40: 663. doi:10.1007/s00382- 012-1403-3]. More importantly, it was recently shown to be incompatible with observations over the Holocene [Emile-Geay et al., (2015), doi:10.1038/ngeo2608]. The authors cite the latter paper but seem to completely discount its critical conclusion, and how this conclusion undermines most of their reasoning. Let me, therefore, rephrase it: in a model where the annual cycle runs the show, one will infer relations to forcings that are overly centered on the annual cycle. This would be actively misleading, perhaps worse than no model at all. I urge the authors to seriously consider the implications of frequency entrainment being an unphysical aspect of CCSM3, perhaps by targeted experiments with other community models like GFDL's CM2.1, which does not exhibit this behavior (and presumably reacts differently to orbital forcing). It is no longer good enough to assume that frequency entrainment explains everything.

The Nonlinear mechanisms controlling ENSO variance that we already know are actually not many. For example, there are the frequency entrainment and the combination mode. We have tested frequency entrainment because the orbital modulation of annual cycle in CCSM3 shows robust precession signal (e.g. Fig. 1g), so it is necessary to see the relationship between annual cycle and ENSO. The frequency entrainment mechanism is robust in CCSM3 model, for example, under millennial fresh water discharge (Timmermann, et al., 2007; Liu et al., 2014); it also explains the CO2 and ice-sheet forcing on ENSO (Liu et al., 2014; Lu et al., 2016). The orbital forcing of ENSO, on the contrary, is quite different. We quantitatively show that the nonlinear terms contribute almost negligibly to the SST tendency (see reply to comment 5 of reviewer#2). Emile-Geay et al. (2015) do show inconsistency between high resolution reconstruction (in-phase change of interannual and seasonal variability of the past 10 kyr) and PMIP3 simulations (out-of-phase change of interannual and seasonal variability in mid-Holocene and PI simulations). And our orbital forcing simulation, accelerated and unaccelerated (ORB, TRACE-ORB and TRACE), agrees with the proxy on ENSO-AC relationship. On the other hand, our results suggest that the combination mode does not exit in our model. And we speculate it is due to the biennial ENSO bias.

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Objection#3: Problematic acceleration

Central to the long time span claimed in the title (300,000 years) is the hundred-fold acceleration technique used by the authors. Just because it's been done 15 years ago, doesn't mean it's a good thing to do today. To their credit, the authors do a good job of using the TRACE simulation to evaluate the consequences of the acceleration. However, they fail to adequately emphasize in their conclusions how seriously this alters the model's response compared to the non-accelerated case, which in my view completely undermines the rest of their conclusions. To wit: the response to orbital variations takes place during 200 years, not 20,000. This is barely sufficient for ventilation to takes place in the lower thermocline, and seriously compromises any claim made about the quantitative importance of the thermocline feedback, to take one example. The authors partly concede this, but in my opinion this needs to be the main topic of the paper: acceleration is a bad idea, and completely distorts the physics of the response. There is still value in the results presented in this article, but only from the strict perspective of paleo modeling techniques. The applicability of CCSM3 results to the real world is questionable, but still of interest. The applicability of accelerated CCSM3 results to the real world is non-existent. In summary, major revisions are needed to bring the title and abstract of this work in line with what can be reliably concluded from these simulations. We acknowledge that the acceleration can be problematic, but the result still provides an example to study ENSO sensitivity under 'pseudo' modulation of insolation. The revised manuscript shows more quantified analysis on the effect of acceleration in Sec 6.1. Please see more details in reply to comment 2 of reviewer#2. Âă 2. Editorial Comments i. The English is remarkably poor.

Thanks for the advice and the example from the reviewer. The revised manuscript will be carefully proof-read.

ii. The Thomas et al 2006 reference lists the paper as 'in review'... 10 years ago. What is the current status of this article?

The paper has been published on GRL in 2017. The reference list is updated.

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Response to Reviewer#2

Main comments: 1. The scope of the study is of course important as there is still a large uncertainty about the impact of external forcing (both past and future) on ENSO properties. The authors propose an ambitious modelling study with potentially interesting results. This said the current manuscript has some severe issues that need addressing to fully realise this potential.

We thank all the comments and critics from the reviewer. Please see our reply to the specific issues below.

2. The first major issue the impact of acceleration. The authors do point out this may lead to issue at sub-surface but do not provide any quantification of this effect. The comparison with the un-accelerated TRACE runs remains qualitative and unconvincing. In particular Fig. 1f questions the relevance of this comparison and no proper statistical analysis is provided. This is all the more problematic as the dominant mechanisms invoked for ENSO change involve the sub-surface ocean.

We agree with the reviewer that more statistical analysis is needed to show the impact of acceleration. 1. We add some discussion associated with the the Appendix (Effect of Accelerated Forcing in a One-Dimensional Diffusive Ocean) of Timm and Timmermann (2007), which also applies to this study. It gives us a framework of the magnitude of acceleration effect in the deep ocean using a simple diffusive model. Furthermore, the phase lag estimated from this simple model is consistent with that calculated by comparing ORB and TRACE-ORB (e.g. Fig. 1d). The former estimates a phase lag of 2000 accelerated years at 200m depth, while the latter shows a longer lag of about 5000 accelerated years at the thermocline depth when including oceanic subduction process. 2. We would like to stress that the annual cycle (Fig. 1g), both in

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amplitude and phase, is quantitatively consistent for accelerated and non-accelerated simulations. It provides an evidence for the surface (or the mixed layer) equilibrium of mean climate even in the tropical Pacific using acceleration. In addition, ENSO dynamics is associated with ocean dynamics no deeper than thermocline depth, so it is still interesting to study its change in the accelerated simulation. 3. Even if the acceleration could pose serious problems to understand ENSO change under orbital time scale modulation, to see results from the ENSO response from ‘pseudo’ 200-yr precession-magnitude and 400-yr obliquity-magnitude insolation modulation can still improve our understanding of climate evolution and ENSO sensitivity. We will quantitatively show these points in the revised discussion sections 6.1 and 6.3. We address the concern for robustness of ENSO precession signal (Fig. 1f) in the next comment.

3. The second major issue is the lack of proper quantification and significance testing of the results. In many cases, the analysis is weakened by this lack of quantification. Most prominently, the significance of the ENSO change signal in Fig. 1f is not clear and is not tested against a proper null hypothesis (no forcing) – this issue is briefly touched upon in the discussion but not properly addressed (qualitative analysis of Fig. S6 is not sufficient), putting the rest of the manuscript in jeopardy. Appropriate statistics (error bars/correlations/significance testing, etc.) are needed in all figures to ensure that the anlaysis only concentrates on actual signals and not noise.

We calculate the power spectrum for the time series of ENSO amplitude, and its $\sim$21ka frequency peak passes 95% significance level. It suggests the primary change in ENSO amplitude is due to the precessional forcing. See revised discussion section 6.3. In addition, as we argued in the manuscript, the relative change (about half of +15%) of ENSO amplitude in the mid-Holocene ($\sim$6ka) to pre-industrial is in the within the range of PMIP snapshots as that of TRACE experiment. We believe these two evidences are enough to support our argument against the no forcing null hypothesis. For other issues associated with proper quantification or significance test, please also refer to more details in reply to minor comments (e.g., # 5, 20).

4. The third major issue is the tropical Pacific and ENSO performance in CCSM3 – more details should be given on how well the model is doing (mean annual cycle, seasonal phase locking of ENSO, etc.. . .), including the use of the BJ index to analyse it, as for example discussed in Kim and Jin (2010) (e.g. their Fig. 9). Also the implications of the 2 years pendulum behaviour are not fully explored. Currently there are only a few lines on this key issue.

Thanks for pointing it out. Indeed, more details of the model performance in the tropical Pacific should be added in the manuscript, especially for tropical annual cycle and ENSO variance phase locking (Sec 6.1). In fact, these issues are discussed in another paper (Lu and Liu, 2017). We gain more confidence as our accelerated simulation results are in qualitatively agreement with other studies. For example, the phase of seasonal cycle (Erb et al., 2015) and ENSO variance phase locking (Karamperidou et al., 2015). The discussion about Fig. 9 in Kim and Jin (2010) mainly focuses on the relative contribution of each BJ term on the total BJ index, which we have already done when discussing our Fig. 4b. Please also see reply to the reviewer#1 (comment 1). It seems the quasi-biannual ENSO bias is robust in both accelerated and unaccelerated CCSM3 simulations. This bias can have two potential impacts: first it is speculated that the combination mode vanished because of this bias; it somehow shortens the typical ENSO time scale and can increase the number of ENSO events during a certain period which also increases the sample size to calculate the correlation of evolution of BJ index and ENSO (when analyzing the linear mechanism). There are also minor comments related to this issue (e.g., #16,17 and 47), please also see the reply to them.

5. The fourth major issue has to do with the BJ index itself and its underlying linear assumptions. After some initial success for CMIP3 and a couple of other cases, the BJ index has since not been successful in evaluating model ENSO errors (for instance, not working for CMIP5). Graham et al. (2014) attributed this lack of skill precisely to the linear assumptions made in the deriving the BJ index. Using the full on-line heat budget

in a model they showed that the BJ index misrepresents the true magnitude of the ENSO ocean feedbacks. It seems that as models improve and exhibit a similar degree of non-linearity as observations, linear analysis frameworks as proposed here became no longer reliable guides for model analysis. Whether this applies to CCSM3 or not has to be investigated. A related issue is the impact of acceleration on the nonlinear behaviour of ENSO in this model. For these reasons (and the lack of proper model evaluation – see point above) section 4.1 too quickly dismisses the role of nonlinearities (either in ENSO or in its interactions with the annual cycle).

Thanks for the suggestion. The Graham et al. (2014) paper shows a practical way to estimate the uncertainty in BJ calculation. Following their method, we use heat budget to check the contribution from the linear terms as well as the nonlinear terms. It is found in orbital modulation of CCSM3 simulation the nonlinear behavior of ENSO is negligible (Fig. S3). We also add the error bars to the BJ terms, which estimate the uncertainty from the regression (Fig. S4). The discussion of validity of BJ index is added in P9L23-31. Minor comment #25 also points out this question.

6. Finally there are a number of conjectures (no evidence provided) and vague terms (“slow”, “dominated”, “closely tracks”, “follows more closely”, “less robust”, “can be largely explained”, “weaker”, “resemble closely”, “enhanced”, “tend to be closer”, “almost identical”, “fairly consistent”, “good agreement”, “higher”, “smaller”, “seems quite robust”, “pronounced”, etc.) that weaken the manuscript and should be either removed or properly defined/quantified. Also a few phrases need to proof read as the English is not correct.

We thank the reviewer for careful reading of the manuscript. The conjectures and vague terms are modified to our best knowledge. For example, see reply to comments #12,16,20,21,24,25,27,35,38,39,41,43,45 and 48. The revised manuscript will be carefully proof-read. .

Other comments: 7. The abstract is vague and its language needs tightening.

It is revised.

8. P.2 L. 6: more recent ref needed.

We add IPCC AR5 (Christensen et al., 2013), from which the conclusion is similar.

9. L. 6-10: much too quick of an intro for this important topic. and 10. L. 11, what do the authors mean by “slow” ENSO evolution ? An why focus on this ? What do paleo observations provide us to compare with ?

This topic is explained in more details. See P2L9-18. The ‘slow’ means orbital time scale, or longer. It is interesting to study the ENSO sensitivity to orbital modulation because current proxy data (especially since the mid-Holocene) can constrain the model uncertainty of simulated ENSO variance. Also because the current PMIP 6ka simulations is mainly about to study orbital modulation, which can be compared with our results. We will put this topic and more description about the paleo ENSO observations in the introduction.

11. L. 17-19: please provide references for this statement.

It is a conclusion from Liu et al. (2014).

12. L. 30: I read carefully the Liu et al. study and was not convinced by the BJ analysis (mostly because of the points highlighted above).

The method of BJ is tested. See more details in reply to comment 5.

13. P.3, l. 1-7 : there are many ENSO mechanisms – why focus on these only ?

We were trying to give a review of previous orbital forcing mechanism of ENSO variance, those studies listed were interested to us, which includes both linear and nonlinear mechanisms.

14. L.8 : please detail the “different processes”.

The processes are added. Please see P3L18-20.

15. L. 23-24: please provide reference(s) and mechanism(s).

Yes. It is Chiang et al. (2009).

16. P.4 l. 8-11: much too quick - see issue 4 above. and 17. L. 15-16: please explain.

We move all model performance in accelerated simulation discussion to Sections 6.1 and 6.3. In short, since even the 100-fold accelerated orbital time scales, say, 200-yr for precession and 400-yr for obliquity is far longer than that of typical tropical surface climate processes, e.g., the annual cycle and ENSO. This point is tested quantitatively in the discussion sections 6.1 and 6.3.

18. L. 30-31: how is this done w.r.t the land-sea mask and heat and fresh water local/global conservation at the air-sea and land-sea interfaces?

The sea level is changed only with context of the land-sea distribution (e.g., during the glacial period the Bering Strait is closed which implies the lowering of sea level), so the fresh water conservation is not strictly valid, but such a small amount should not affect low-latitude climate too much.

19. L. 32-33: this is cryptic for non-experts. Please explain.

Yes, they are explained in P5L12-16.

20. P.5 l. 22-25: how significant are these changes w.r.t to a null hypothesis ?

Please see reply to comment 3.

21. L. 31: please explain this conjecture.

we will remove this conjecture.

22. P.6 l. 16: please clarify.

Done (P7L1).

23. L. 20: which non-linear mechanisms are we talking about here ?

We mainly focus on the frequency entrainment mechanism, which is proposed in a previous accelerated orbital forcing simulation (Timmermann et al., 2007). The combination mode is also tested later, but it turns out not a robust feature in our model.

24. P. 7, l.1-26: this is much too quick (see point 4 above). ECHO-G is notorious for having quite degraded climatology and balance of processes in the eastern Pacific cold tongue (due to crude vertical mixing scheme). Proper comparison is required to have confidence in the points (too quickly) made in this section.

Since the ECHO-G simulation is the only previous modelling study on this topic, it is necessary to compare its wavelet spectrum (main feature of frequency entrainment, and the killer figure of their paper) with our results. We would argue that a qualitative comparison of the power spectrum is reassuring that the frequency entrainment cannot explain the change of ENSO variance in our simulation, in addition to the plots of evolution of ENSO and annual cycle (Fig. 1f,g). The significance level of precession signal in ENSO variance, on the other hand, is tested in reply to comment 3.

25. P. 8 l. 24-28: what is the impact of these ? Did you compare to actual tendencies such as in Graham et al. (2014) ? More is needed to go beyond the current "cuisine" feel when reading this. Thanks for pointing it out. The impact is that the absolute value of BJ index is increased (now close to 0), but not its trend on the orbital time scale.

BJ index itself is tested as seen in reply to comment 5.

26. P. 9 l.2 why is the relative BJ index change the right measure?

The absolute value of BJ index can be different due to different methods of estimation, for example, the choice of region (e.g., equatorial eastern Pacific or Nino 3.4), the processing method (e.g. band-pass filter). So its relative change can be more meaningful, and reveals the influence of external forcing.

27. P. 9 l. 4-15: please quantify all qualitative and vague terms. and 28. L. 4-10: is a correlation of 0.4 large enough to infer a causality link? Again here proper significance

testing is missing. Please use the 21k simulation to show that the “acceleration can make the forcing signal less robust”. And what would be the mechanisms? This key section is quite unclear and not convincing.

More details of the quantification of acceleration effect can be found in Sec 6.1. Both the evolution of ENSO variability and its linear growth rate (BJ) are predominately modulated by the precessional forcing, as confirmed by analysis on the frequency domain (e.g., Fig. S1a and Fig.S8a). In accelerated simulation, the correlation between them is 0.4, and is higher when the precession modulation is more pronounced (e.g. 250∼100 ka BP). By comparing the accelerated and unaccelerated simulations, it can be concluded that the acceleration can dampen and delay the precession signal in the ocean. A comprehensive discussion on the acceleration effect can be found in Sec 6.1.

29. L. 24-31: this conjecture is not really convincing.

This argument is now supported by more quantified explanation (P10L23-L30).

30. L. 31 – p.10. l. 2: then what is the point of analysing slow ENSO variations if a basic mechanism affecting the thermocline slope is not correct?

Indeed the precession signal is dampened and delayed in the deeper ocean, but the BJ index still shows pronounced 21 ka cycles (confirmed by the coherence analysis). A more detailed analysis on each feedback of the BJ index suggest the thermocline feedback (wbardzT’) does show a larger uncertainty than the most important Ekman upwelling feedback (w’dzTbar).

31. L. 10-14: yes, I agree with this caveat.

Unfortunately, the reason for this caveat is not clear to us.

32. L. 18: WWBs are not a “remote” forcing.

OK. We change it to ‘stochastic’ forcing.

33. L. 24-25: how reliable is this approach ? Have you tested it for a period when both

frequencies are available in the output ? Otherwise, this section is not convincing.

As the equations after the argument show, the approach should be reliable. Unfortunately, due to the limited computational resources, we did not save the output higher than monthly resolution.

34. P. 11 l. 9: please clarify.

Done

35. P. 12 l. 11-22: please quantify all qualitative and vague terms.

Done. (P13L12-25). Additionally, we also add power spectrum analysis for ENSO in ORB+GHG and ORB+GHG+ICE.

36. L. 28-34 – p. 13 l.4: I am probably missing something as a I thought increased GHGs were enhancing ENSO amplitude?

The CCSM3 model shows increased GHGs could weaken ENSO. We copy the description of the nonlinear mechanism below: "The increased GHGs concentration leads to an asymmetric annual mean warming (a stronger warming north of the equator) in the tropical Pacific (Figure not shown), which enhances the equatorial asymmetry and in turn the annual cycle (Timmermann et al., 2004). The enhanced annual cycle then weakens ENSO through frequency entrainment (Liu, 2002). The ice sheet change, also predominant in the 100-kyr cycle, forces an in-phase change of annual cycle intensity and an out-of-phase change of ENSO intensity."

37. P. 13 l. 8-12: too quick – please explain.

Done (P14L10-18).

38. L. 12-19: conjecture – please show it or remove point.

We have reorganized this part (P14L9-30). First the quantified results are shown: in ORB+GHG, the correlation between ENSO strength and BJ is increased, and BJ

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has pronounced frequency peak at ∼21 ka and a secondary peak at ∼ 100 ka (not significant). It somehow implies the possible relation between the ENSO linear growth rate and the GHG forcing. Second, we speculate that it can be explained by a previous proposed mechanism (Meehl et al., 2006, and they used the same CCSM3 model), by which the CO_2 warming at the sea surface leads to a more diffusive equatorial thermocline and weakened ENSO. The hypothesis is hard to be quantified because we only have available data for the upper ocean (above ∼50m).

39. L. 25-32: because of the lack of proper significance testing and of issue with non linear mechanisms, it is hard to follow this discussion.

The discussion of the effect of different forcing combination can only be quantified if more sensitivity experiments are done, e.g., single forcing 300 ka accelerated simulation. Since our purpose is only to remind the readers of this issue in our simulation, we move it to the discussion Sec 6.2.

40. P.14,l. 6: why?

Please see reply to comment 10. It is previously defined as 'slow' evolution of ENSO variance.

41. L. 6-32: please quantify all qualitative and vague terms. and 42. L. 17-19: by which measure(s) are the accelerated and TRACE simulations "fairly consistent"?

Done. And the 'fairly consistent' amplitude and phase are quantified. See the revised Sec 6.1.

43. Section 6.2: please quantify all qualitative and vague terms.

Done.

44. L. 31 – p. 16 l.2: isn't this a circular argument? If not, please clarify.

It is not, because both tropical mean climate (annual cycle) and climate variability (ENSO) are quantitatively consistent for accelerated and unaccelerated simulations

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during the last 21 ka.

45. Section 6.3: see point 4 above

Power spectrum discussion is added.

46. L. 10-13: a low correlation can also be due to physics! Why should one expect 100% correlation if the sampling is right?

In the revised manuscript we quantify the effect of acceleration below the surface ocean, and we hope these analyses (Secs. 6.1 and 6.2) more evidently support our argument.

47. L. 13-16: indeed and please expand on this important caveat.

Yes. The biennial ENSO could increase the number of ENSO events (sample size). We add more details of ENSO phase locking when replying to comment 4.

48. Conclusion: please quantify all qualitative and vague terms.

Done.

49. P. 17 l. 12-16: this is an unsupported conjecture, not a conclusion

See reply to comment 36, 38 and Lu et al., 2016.

Reference Please see those in the manuscript.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/cp-2016-128/cp-2016-128-AC1-supplement.pdf

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