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Climate of the Past Discussions

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

Anonymous Referee #2

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Review of "Evolution and forcing mechanisms of ENSO over the last 300,000 years in CCSM3" by Z. Lu et al.

This study explores ENSO variations in response to forcing over the last 300 kyrs in a model that uses an acceleration technique. The impact of different external forcings (orbital, GHG, ice sheets) on ENSO properties is analysed using a linear stability analysis framework. The authors argue that the orbital forcing dominates the slow variations of ENSO while the other two compensate each other. They further single out a few of mechanisms to explain this results. A discussion on the impact of the acceleration technique is also provided with a comparison with a 21 kyrs non-accelerated simulation.

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.

2. The first major issue the impact of acceleration. The author 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.

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.

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.

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 non-linear 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 non linearities (either in ENSO or in its interactions with the annual cycle).

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.

Other comments:

7. The abstract is vague and its language needs tightening.

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

9. L. 6-10: much too quick of an intro for this important topic.

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 ?

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

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).

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

14. L.8 : please detail the "different processes".

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

16. P.4 l. 8-11: much too quick - see issue 4 above.

17. L. 15-16: please explain.

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?

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

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

21. L. 31: please explain this conjecture.

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

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

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.

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.

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

27. P. 0 l. 4-15: please quantify all qualitative and vague terms.

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.

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

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 ?

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

32. L. 18: WWBs are not a “remote” 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.

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

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

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

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

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

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.

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

41. L. 6-32: please quantify all qualitative and vague terms.

42. L. 17-19: by which measure(s) are the accelerated an TRACE simulations “fairly consistent” ?

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

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

45. Section 6.3: see point 4 above

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

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

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

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

References:

Graham, FS et al. (2014) Effectiveness of the Bjerknes Stability Index in representing ocean dynamics, 43, 2399-2414, doi: 10.1007/s00382-014-2062-3.

Kim ST, Jin FF (2010) An ENSO stability analysis. Part II: results from the twentieth and twenty-first century simulations of the CMIP3 models. Clim Dyn 36(7-8):1609–1627. doi:10.1007/s00382-010-0872-5

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