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Interactive comment on "A Comparison of Ocean Model Results with Satellite Observations during the Development of the strong 1997–98 El Niño" *by* David J. Webb et al.

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Again many thanks for your review. In the following, the reviewer's comments are in **bold text** and my response in normal text.

Although the mechanism may have been explained in depth in the lead author's previous papers, to make this paper self-contained, more convincing arguments should be provided in this paper too.

I accept the referee's point and will try to, briefly, explain the importance of high sea surface temperatures and the roles of the annual Rossby wave, tropical instability waves (eddies?) and sea level near the Equator in the mechanisms discussed in the original paper.

The mechanism for the development of an El Niño proposed by the authors is an intriguing one, but in the strongly coupled series of events underlying the ENSO phenomenon, it is unclear how to identify causes and effects. During an ENSO event, the tropical Pacific undergoes a profound adjustment process, involving wind anomalies and wave propagation. In particular, anomalous warming in the equatorial Pacific causes a southward shift of the ITCZ with likely changes in the wind stress curl at the latitude of the mean ITCZ position. Changes in the NECC can be expected as a result of this adjustment, but it is unclear whether the NECC changes are actually the drivers of the El Niño development.

This is really a comment on the original paper. From what I can tell, since Wyrtki's time people have been trying to separate causes and effects in the El Niño system without a lot of success. The previous paper had the advantage of access to the results from an ocean model which has much higher resolution than normal in both the horizontal and vertical directions. Thus it is possible that it would emphasise connections which were hidden by resolution problems in earlier studies.

However it might have also introduced new errors - and that is the reason for the present paper - to check key oceanic features which were central to the arguments in the previous paper. I think the previous paper gives reasonable causal arguments that changes in the the annual Rossby wave in the western Pacific and changes in the strength of the tropical instability waves are enough to allow the NECC to transport warm water (>28°C) from the western to eastern Pacific at latitudes near the ITCZ.

Such water is known to be warm enough to trigger deep atmospheric convection (see the paper by Evans and Webster).

It is possible that SST values near 26°C on the Equator are more important than 28°C near the ITCZ but that is for someone else to check.

The authors mention several times the annual Rossby wave. What causes this wave, why do they think that it is the main driver of the NECC changes, and why was the wave particularly strong in 1997? As mentioned before, Rossby waves in the tropical Pacific are the agent that allows the tropical ocean to adjust. Why so much emphasis on the annual Rossby wave?

The emphasis on the annual Rossby wave arises because in both 1982 and 1997 it propagated further than normal into the western Pacific. This resulted in a greater sea level difference across the latitudes of the NECC, which because of geostrophy increased the transport of the NECC. This meant that it carried eastwards a greater amount of warm pool water than normal - starting the process which eventually resulted in water with temperatures greater than 28°C reaching the eastern Pacific.

The wave is thought be be generated in the eastern Pacific, partly by winds blowing across the Isthmus of Panama. As it propagates across the Pacific it my be modified by local winds. However I have not been able to find any modern authoritative paper or review of the wave, its generation and propagation.

The comparison between model and observations is very qualitative, except for the estimate of mixing in section 5.

Yes, this is a weakness of the paper. However the eye is a pretty good data processor and the figures have been chosen and constructed such that if there are significant differences they should show up - as they do in the plots which aim to measure the strength of the topical instability waves. An analysis which gives a single number can look fine but hide a wealth of errors.

The review makes a number of suggestions :

Much more could be done, including: 1) Support the interpretation of propagating anomalies as Rossby or Kelvin waves with an estimate of their phase speed; 2) Examine whether the changes in the NECC velocity, as estimated from the model, are consistent with the meridional gradient in sea level; 3) Compare SST and SSH Hovmoeller diagrams to show that the warming seen along the equator is concurrent with equatorial Kelvin wave propagation, and 4) Estimate whether the changes in the strength of the NECC are indeed large enough to make an impact at 5N and along the equator.

These are really comments on the physics discussed in the original paper (which was long enough) - whereas the present paper is focusing on the equally important problem of model accuracy. If the comparisons with observations show that the model is wrong in describing key features then the arguments of the previous paper are irrelevant anyway.

For (1) I did some rough comparisons and was confident that these were Rossby and Kelvin waves but further analysis is always possible. For (2) a lot of effort had been spent improving the NEMO physical model. The model is also widely used so I believe that if there was any problem with geostrophy in the model it would have shown up elsewhere by now. (3) I do not think the warming seen along the Equator is consistent with equatorial Kelvin wave propagation. This can be seen by comparing figure 6 and 19 of the original paper and figure 1 and 5 of the present paper. The Kelvin waves are the fast waves seen in figure 5 of the present paper. (4) The particle tracking plots of the original paper showed that the NECC was able to transport warm pool water into the eastern Pacific. Some of this will have displaced water towards the Galapagos but the main reason for warming on the Equator there is probably a result of reduced upwelling due to a reduction in wind generated Ekman divergence.

Reviewer's Detailed comments:

1. Why the deepening of the thermocline cannot produce surface warming? This should be briefly explained.

Not really a question for the present paper. At a fixed depth within the thermocline, deepening of the thermocline will produce warming due to the descending warmer water. However at the surface there is nothing to descend. The only way the surface layer can warm is through horizontal advection or by local processes, such as increased heat flux into the ocean. The Webb (2016) paper indicated that in the Nino regions studied, the temperature gain during the development of the El Niño was not due to local processes.

3. How did the Authors assess that it was the "annual" Rossby wave to produce changes in the NECC?

Not really for the present paper, but see the SSH figures.

4. The high sea level in the equatorial central Pacific discussed by Kug et al. (2009) only occurs during Central Pacific El Niño events.

The observational evidence (Fig. 5) shows that this is not always true.

4.1 During Eastern Pacific events, like the extreme event consider here, the equatorial thermocline exhibits a very strong zonal dipole with deeper thermocline in the eastern Pacific and shallower thermocline in the western Pacific.

If we assume that sea level is a measure of thermocline depth, then Fig 5 shows that there is a deeper thermocline in the eastern Pacific and shallower thermocline in the western Pacific - however that is not the full story.

5. SSH is important for its dynamical meaning, as it can be viewed as a proxy for thermocline depth and upper-ocean heat content.

I agree.

6. Why was the annual Rossby wave unusually strong that year?

I do not know why it was strong that year.

7. In what way the chaotic nature of the waves is emphasized in difference plots?

Let the variances in the two plots, measured relative to the same time and place, be V1 and V2. If the processes are random then the variance of their difference is expected to be V1+V2, and the r.m.s. value is the square root of V1+V2. This is larger than the figures for the individual plots.

8. It is important to note that the two different stages of development of the 1997-98 El Niño have been related to different phases of Westerly Wind Burst (WWB) activity by several Authors (McPhaden 1999; Menkes et al. 2014; Capotondi et al. 2018, among others). How do the Authors reconcile the view they present in this paper with those previous studies?

Not really for the present paper. However Fig. 32 of the original paper is a plot of the easterly wind stress along the Equator for the period 1995 to 2000. The data comes from the ECMWF reanalysis and during 1997 it shows a number of westerly wind bursts.

Figure 5 of the present paper shows the satellite observations of sea level along the equator and also the model response. Both show the Kelvin waves generated by the westerly wind bursts.

Figure 1 of the present paper shows the satellite observations of sea surface temperature along the equator and also the model response. Both plots show that the temperature anomalies propagate much more slowly than the equatorial Kelvin waves. They also show little or no evidence of Kelvin wave activity.

9. The NECC can affect the ITCZ, but how is the perturbed ITCZ going to influence the warming in the eastern equatorial Pacific?

Not really for the present paper. However I expect it is through a reduction in the winds along the equator and the resulting reduction in wind driven upwelling along the Equator (see earlier comment).

10. How the "annual signal" was identified needs to be explained.

Not really for the present paper but see the plots. Some of the plots were made for the whole period 1957-2009. Something that usually occurred at the same time every year during this period was assumed to be an annual signal.

11. The increasing sea level in the west is typical of a developing La Niña, as it happened in 1998.

In 1984 the model behavior is similar.

12. p.7 line 40. I don't think that we are looking here at a model prediction, but at a model simulation.

I agree.

In conclusion thanks for the comments. Most of them however are related to the content of the previous paper - which I accept remains an area of debate and which I am happy to continue elsewhere. Therefore, and unless the editor advises me differently, in the revised paper I shall concentrate on a better summary of the earlier results and a better explanation of the reasons for the present paper.

Regards,

David Webb.

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