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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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I would like to thank the reviewer for taking on this paper and for the comments and references.

As I understand it the comments are of two types. The first concerns just the present paper and mainly concerns how difficult it is for the reader to understand the background and reasons for the paper's publication. The second is a criticism, valid for both papers, of a general lack of external references.

I should say now that the lack of references results from a decision made when I started this study. As the reader might know, in the past I have been involved in a number of studies of the Pacific Ocean, mainly modeling but also experimental, including jointly leading a study of the far western equatorial Pacific. None of this has actively involved the El Niño but of course I have been very aware of El Niño research and ideas.

For the present project I decided to get involved in studying a model run that my ex-group was involved with and to use it to understand more about the El Niño. I am of course aware that the process has been studied at great expense for over 50 years and has produced hundreds if not thousands of papers. But, at the same time I am concerned by the lack of a comparable improvement in physical understanding during this period and the fact that serious researchers can still describe the El Niño, in a new-age way, as a subtle interaction of ocean and atmosphere, with little hard physics and little embarrassment.

I therefore tried to make progress by being different. I did this by focusing on the one model ocean and by trying to make sense of the physics without the constraints of any particular theoretical model or line of research. If it had failed you would have heard no more - but it had some success, one result of which is that in Webb (2018) and this paper there are few references - except ones that help better explain the model's behavior.

Now you may be unhappy about this but it is done to keep the papers focused. There will be time later to discuss how and why other approaches agree or disagree.

In the following, the reviewer's comments are in **bold text** and my response in normal text.

This short paper presents a brief comparison between a model and data (sea level and SST) focusing on equatorial waves and tropical instability waves (TIW) during the 1997/98 El Niño event. The paper is a follow on of a previous recent paper by the lead author that also analyzes the same model and the same event. While I acknowledge the interest of investigating off-equatorial variability for understanding the build-up of heat content and the discharge process during strong El Niño events, it is not clear to me what is the specific motivations and objectives of the paper.

As with the first reviewer I accept that the introduction needs to be made clearer.

The diagnostics are rather rudimentary and do not convey a clear message.

I am surprised by this comment. The eye can be a good data processor and in this paper the figures usually show remarkably good agreement between the model and observations. See for example the equatorial Kelvin waves triggered by westerly wind bursts during the development of an El Niño.

The authors seem also to ignore the existing literature on this event that has been extensively documented and investigated.

Point discussed above.

They should clarify what is their specific contribution compared to previous studies and resolve some methodological issues (see specific comments).

The aim of this paper is to test key areas of the model's response against observations. This needs to be made clearer in the revised paper. The comparison has been restricted by the limited amount of satellite data available for the 1997-1998 period.

The paper does not try and prove that the mechanisms discussed in Webb (2018) are correct, rather it is a test to see if there are major faults in the model which means that their justification is wrong. Thus if anything it is trying to disprove the previous results and conclusions. The fact that no major problems are found in the key areas studied gives some additional confidence that the Webb (2018) conclusions are not based on a poor version of the numerical model.

Specific comments:

Abstract "The results provide additional confidence in the oceanic mechanisms which model analysis implicated as being responsible for the development of both the 1982- 83 and the 1997-98 El Niño". This is quite a vague statement. The abstract should provide some hints of what are the results.

This will to be improved.

Introduction It does not convey a clear motivation and there is no references to relevant works (McPhaden (1999), Boulanger and Menkes, (1999), Vialard et al. (2001) amongst many others, see also all the literature on the ENSO-TIW interaction (see introduction of Holmes et al. (2019) for instance).

The motivation needs to be made clearer and I will try and do this. The references

included in the review are of interest in any discussion of the ocean's role in the El Niño and for that reason I include a few comments below. However the only role I see for such references in a model-data comparison paper is to emphasise the differences in the conclusions reached and to emphasise the importance of checking that the differences are not due to errors in the present model.

Holmes R. M., S. McGregor, A. Santoso and M.H. England (2019) Contribution of Tropical Instability Waves to ENSO Irregularity, Climate Dynamics, 52, 1837-1855.

Interesting, but as shown in Fig.6 of the present paper and Fig. 14 of Webb (2018), TIW amplitudes are generally very low during the year in which strong El Ninõs develop.

Boulanger, J.-P., and C. Menkes, Long equatorial wave reflection in the Pacific Ocean during the 1992-1998 TOPEX/POSEIDON period, Clim. Dyn. 15, 205-225, 1999.

Thank you for bringing this paper to my attention. As I discuss below I am not completely happy with a wave guide expansion. Maybe the strong coupling of the first and third Rossby wave modes reflects the weakness of this approach.

McPhaden, M. J., Genesis and evolution of the 1997-1998 El Niño, Science, 283, 950- 954, 1999.

An 'authorative' paper which now appears to contain many misleading statements. To quote from just the second paragraph:

A weakening and reversal of the trade winds in the western and central equatorial Pacific led to the rapid development of unusually warm sea-surface temperatures (SSTs) east of the international date line in early 1997 (Figs. 1 and 2).

The phase 'led to' implies causes. The work reported in Webb (2018) concluded that both changes were a result of increased advection of warm water by the NECC.

The western Pacific warm pool (surface waters greater than about 29°C) migrated eastward with the collapse of the trade winds,

This will be taken by some to imply that the collapse of the trade winds was the cause of the migration. Webb(2018) finds that it was the other way around - the migration of the warm pool, due to the NECC, led and caused the collapse of the trade winds in the western Pacific.

and the equatorial cold tongue — the strip of cool water indicative of equatorial upwelling that normally occupies the eastern and central Pacific between the coast of South America to the international date line — failed to develop in boreal summer and fall 1997.

This I would agree with if it had also pointed out the reduced winds in the central and western Pacific (McPhaden, Fig. 1a) and that this reduction would have resulted in reduced upwelling.

Vialard, J., C. Menkes, J.-P. Boulanger, P. Delecluse, E. Guilyardi, M. J. McPhaden and G. Madec, Oceanic mechanisms driving the SST during the 1997-1998 El

Niño, J. Phys. Oceanogr., 31, 1649-1675, 2001.

I like this paper especially the care taken to validate the model before use. The model is similar to that used in Webb (2018) but uses lower resolution both horizontally and vertically. Despite this, many of the results appear similar, an example being the reduction in TIW strength during the development of an El Niño.

Where I would disagree is in the interpretation of the results. As an example, the paper says that "some of the ocean processes in early 1997 are associated with a strong Madden-Julian Oscillation", but similar events are seen in the model results for 1982-83 when the oscillation was very weak. The importance of meridional advection is mentioned but not the currents involved. The interpretation focuses on equatorial Kelvin waves and when, for example, discussing the deeper thermocline in the east in early 1997 it gives the same weight to the downwelling Kelvin waves as it gives to the "weaker winds" in the east.

Kelvin waves, like other waves, cannot generate a net transport sufficient to give a step change in the thermocline depth in the eastern Pacific - the Stoke's drift due to the waves being insufficient. In contrast weaker trade winds in the eastern equatorial Pacific will result in reduced upwelling there and so allow a new thermal balance to develop and/or allow the advective inflow of warmer near-surface waters from the surrounding ocean.

The statement "The study concentrated on the strong El Niños of 1982-83 and 1997-97 and found that equatorial Kelvin waves had no significant effect on the surface temperature of the eastern Pacific." is surprising. It is recognized that the Kelvin wave during El Niño produce vertical advection of anomalous temper-

ature, a process refers as the thermocline feedback and shown to be dominant in the eastern equatorial Pacific in previous ENSO studies.

This statement relates to the conclusions of Webb (2018) and should not concern the present comparison of model results with observations. The point is discussed above. In addition Fig. 5 shows equatorial Kelvin waves, triggered by westerly wind bursts, which arrive in the eastern Pacific in the second quarter of 1997. These are the same wind bursts as discussed by Villard et al (2001) and discussed above. Figure 1 of the present paper shows a slight warming in the same region before and at the start of this period. If, despite what was said above, I am wrong and the two processes are connected then I would argue that the warming is not significant in the sense that the temperatures are not high enough to trigger deep atmospheric convection.

These figures and the wind stress figure in the original paper show that the Kelvin waves are generated by westerly wind bursts within the warm region of ocean but they appear to be unconnected to the rate at which the warm region progresses across the ocean.

Model data comparison: The model has no assimilation of data so it is difficult to compare model and observations in terms of TIW, the model simulating eddies that are not necessarily collocated with observations owing to their chaotic nature. So the comparison should be based on statistics rather than the visual inspection of Hovmöller diagrams (e.g. Figure 2). See for instance An (2008) for relevant diagnostics for TIW activity.

I'll quantify the variance in some of the key regions and add the values to the text. On the point of data assimilation I would have thought that if data was assimilated then

usually (a) any comparison between model and observations would be suspect and (b) any inferences concerning cause and effect would be suspect.

Figure 4: It is not really possible to see an equatorial Kelvin wave at 6°N; its amplitude would be very weak.

In the study of the 1982-1983 El Niño, the model showed a large number of significant changes which occur all across the ocean during a very short period at the end of 1982. Similar changes occur at the end of 1997. These occur not only at the Equator (Webb (2018) Figs. 7, 15, 19, 28, 31) but also at 6°N (Webb (2018), Figs. 7, 11, 20, 21, 28, 29, 31). Because of the speed at which the change occurs I have concluded that this is the effect of a Kelvin wave.

The latitudinal extent of a Kelvin wave does depend on stratification, but in the example given by Boulanger and Menkes (1995), which you refer to, the Kelvin wave does affect 6°N.

In the revised paper I will add a note referring to the original paper, saying that the results imply, but don't prove, that the feature is due to a Kelvin wave.

Also the difference between model and observation is not relevant here unless you focus on the low frequencies (periods $>\sim$60 days), which would require filtering the data.

I do not understand the filtering argument but I agree that the figure does not add a lot. However as Figs. 1 and 2 include differences there seemed no reason not to include

it here. One advantage of including it is that then all three figures will be printed at a similar size in the final paper.

Comparison should be done on anomalies relative to the mean climatology, otherwise this is just emphasizing the differences in seasonal cycle. If the authors want to discuss Kelvin and Rossby wave contribution to sea level anomalies, I suggest that they project sea level on the theoretical equatorial wave structures (see Boulanger and Menkes (1995) for the method).

I disagree with the use of anomalies, unless nothing else is available, because they are often misleading. The web contains many pictures of El Niño temperature anomalies (and associated sea level anomalies) in the cold pool upwelling region, where maybe the temperature has increased from 19C to 26C. They look very dramatic and are sometime used to 'explain' why the atmospheric circulation changes. However the effect of the temperature change on deep atmospheric convection and the El Niño is probably insignificant compared with a temperature increase from 27C to 29C near the latitudes of the ITCZ.

In the case of Fig. 4 the plot of absolute values emphasizes the fact that the changes in western Pacific sea levels during an El Niño are coordinated with (not necessarily caused by) the timing of the annual Rossby wave. If an anomaly was used this would not be so clear.

On the question of using modes of the equatorial waveguide, waveguide modes higher than the base mode are often represented as pairs of simple waves which cross each other at a small angle at the center of the waveguide, in this case the Equator. They then get reflected or refracted at two critical boundaries (latitudes) decaying exponen-

tially outside the boundaries and combining to generate standing waves between the boundaries (as seen in the Rossby wave solutions).

For the ocean this implies that energy crosses the Equator at some point. I do not know of any examples of this. For very long east-west wavelengths this may be because the intersection angle is very shallow, the different modes having the same limiting phase and group velocities - so a description in terms of purely westward traveling features is equally valid. The fact that equatorial crossing is not seen at shorter east-west length scales is puzzling, unless stratification and currents near the equator are acting as barriers to the flux of energy.

In conclusion I agree that the introduction and abstract need improving and that a number of other points need attention. Thank you for your comments and the chance to highlight some of the differences between the conclusions of Webb (2018) and previous analyses of the 1997-1998 event.

Regards,

David Webb.

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