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Response to Reviewer #2

David Webb

Author comment on "On the low western Pacific sea levels observed prior to strong East Pacific El Niños" by David J. Webb, Ocean Sci. Discuss.,
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Response to Reviewer #2

Again many thanks for reviewing this paper. I suspect that you were not all that happy with premise, content and style (I've been there myself), but thanks for continuing.

1. Background

"The pairing of the two [NECC and wind stress] in the previous paper Webb (2018) does not represent a widely held belief. In that sense, the context for the present paper can be considered somewhat novel. However, the present paper is focused on explaining modelled NECC variability."

Yes. I assume that the reader knows the background to the work. The pairing only comes up when the other mechanisms investigated have been discounted. It is in the abstract but I should probably add a bit more in the introduction to prime the reader for what comes later.

2. Model validation

"There is no observational evidence offered in this manuscript to validate the model that is used in the experiments presented."

This is probably best answered by adding more references to the use of OCCAM elsewhere. OCCAM has been around for nearly thirty years, being one of the first high-resolution global ocean models. A quick search on google advanced scholar search for 'occam ocean model' produced 58 papers using OCCAM results out of the first 70 references listed. I do not have a full count but it probably runs into many hundreds and includes many intercomparisons with the models and observations. There will be even more if you add David Stevens' SEA and Kristofer Doos' Baltic Sea models which are related.

As far as I know none of the papers have shown any major issues with the model. In the Pacific I have used it previously to study the zonal jets south of the Equator and the Indonesian Throughflow. In neither case were there any significant problems.

"The degree to which the forced model can accurately hindcast the observed space-time

variability of SST, SSH, and surface currents in 1981 and 1982, along with northwest Pacific isopycnal depth (as in Fig. 25), is fundamental to the conclusions reached in this manuscript. These aspects have not been adequately evaluated but should be."

I would argue that the track record of the NEMO model and the comparison paper with Andrew Coward and Helen Snaith (which covers SST and SSH) give grounds to suggest, not only that the model is as good as (or better than) any other present day ocean model but, more importantly, that its performance is more than sufficient for the present research project. I would accept that the OCCAM model, being older and less developed, is not so good. However there is nothing in previous research and publications using the model to indicate that it should not be used for the present study.

The changing depth of the near surface iso-pycnals depends on the Ekman divergence. This depends on the surface wind stress and, although you may query the quality of the wind stress field, it would be difficult to construct an ocean model which generated the wrong divergence without also showing other major flaws.

Of course if anyone has doubts, it is always open for them to prove that something is inadequate or wrong and always essentially impossible prove that it is right.

3. Observational Uncertainty

"The role of observational uncertainty has not been but also should be considered in this context. Previously, for example, Harrison et al. (1990) reported on forced ocean model hindcasts of the 1982-83 El Nino event and found that the answers to questions like those being asked presently, for example, concerning the relative importance of local and remote wind forcing to anomalous currents and SST, depended very much on which wind data set was used to force the model. The present manuscript appears to report results based on only one wind data set, which is not described in the text. Given the previous Harrison et al. demonstration of the importance/limitations of observational wind uncertainty in this context, the impact of this wind uncertainty needs to be examined before the reliability of the results presented can be understood."

This is a valid point, the results do depend on the wind field. However, as reported in Webb (2018), the NEMO model was forced with the Drakker DFS5.2 atmospheric fields which are themselves based on the ECMWF reanalyses. I should probably include a similar statement in the present paper.

The forcing dataset has been widely used and has a good reputation. The comparison carried out with Coward and Snaith is essentially a test of both the model and the forcing dataset, the agreement with observations in the equatorial Pacific indicating that neither has significant errors.

I accept your point about the differences in the Harrison forcing datasets but modern reanalyses, like that from the ECMWF, appear to have overcome many of the earlier problems. The work could be repeated with another reanalysis dataset but this would take a considerable amount of work and computer time and it seems unlikely that it could both show agreement with the satellite data and show that different mechanisms dominated the physics.

4. Metrics

"Notwithstanding the issues raised in the comments above, more precise description of the experiment results would improve their presentation (and facilitate comparison to observations). This manuscript relies mainly on visual inspection of snapshot-maps and time-longitude plots of SST, SSH and currents to support its conclusions about the relative

importance of different ocean initial conditions and components of wind variability for causing changes in NECC-related SSH. I suggest defining metrics that quantify the salient model experiment results in relation to the control-hindcast to thereby offer a more streamlined and precise presentation of results."

Good point. I am not sure what metrics you had in mind but one possibility would be one or more t-tests using pairs of the height changes seen in figure 23. This could supplant many of the other figures by summarising the changes in height in the west Pacific. However I am unwilling to lose the other figures (they are packed better in the 2-column version of the m/s included in my response to the first reviewer) because they give the reader a chance to think about how different parts of the ocean are connected physically - something you do not get with plots of NINO 3.5 anomalies or a table of statistics.

5. Title

"I suggest that "Westerly Wind Events" be removed from the title. This manuscript does not identify or directly discuss westerly wind events. Something like "On the development of low North Equatorial Pacific sea level pressure during 1982-1983" would be more appropriate."

Maybe. Certainly westerly wind anomalies are involved - but even your Harrison et al reference shows that they are not just anomalies.

6. Relationships

"The last paragraph of the abstract attempts to describe the relationship between westerly wind events, the Madden Julian Oscillation, North Equatorial Counter Current (NECC) and the observed development of the 1982-83 El Nino event based on the NECC-related model experiments presented herein. However, what is presented herein does not sufficiently support conclusions about these relationships because three of these four phenomena (wind events, MJO, observed El Nino development) are not substantially addressed by the results presented in the manuscript. The abstract should be modified to better reflect what has and has not been done here."

I take your point but I think that the connections need to be made - to stimulate discussion and further research. When I have talked to an expert from the UK Met Office they were convinced, on the basis of their own work and others, that El Ninos were connected with Madden Julian events and would accept no other hypothesis or theory. There is evidence that such oscillations were involved in early 1982 so I think I need to work up this aspect the manuscript.

A related reason is that I think the apparent link with the Madden Julian oscillation and the associated westerly wind bursts has helped the theoreticians believe they are on the right track by concentrating on the equatorial wave guide and equatorial Kelvin waves when developing theories of the El Nino. I accept that I may be biased but the result here helps show that other solutions are possible.

7. Figures

"Many of the model comparison figures, for example Figs. 3&4, 5&6 etc. can be combined to the benefit of the reader's ability to make the intended visual comparison. Reducing the total number of figures may also improve the presentation; 24 is perhaps an over-abundance of figures for the scope of this paper."

The figures were designed to work with the 2-column version of the manuscript - where most of the figures do lie adjacent to their mate. Unfortunately Ocean Science requires

the single column version for reviewers.

8. Progress

"There has been considerable progress made in understanding El Nino development since Wyrtki (1973, 1974) offered hypotheses about the role of enhanced NECC. This manuscript would benefit from taking into account what has been learned and described in many of the relevant publications since then."

You may guess that I have a problem with current theories of the El Nino and for that reason it is probably best that they are not raised in the manuscript (because the m/s is not concerned with the general problem). That said if you know of any work on thermocline movement or SSH changes between 140E and 170E and around 6N I would be pleased to include references.

After reading your comment I read through the review by Wang et al 2016. OK its not right up to date (I do not have a copy of the recent AGU book) and for some reason there is at least one theory involving thermocline displacement it does not include but otherwise it is reasonably detailed.

As I understand it, any physics that has been learnt is usually summarised in a decent theory. If you go to the section on ENSO mechanisms in Wang et al there is no proper discussion of the NECC. Instead it is full of the unphysical assumption that equatorial Kelvin waves can transport heat horizontally over significant portions of the Pacific. The associated Stokes drift can move water particles and heat but not over significant distances.

For the detailed theory, seven separate ENSO mechanisms are put forward dating from 1969 (Bjerknes), 1988 (Suarez and Schoph), 1987 (Jin), 1997 (Weisberg and Wang), 1997 (Picaut et al), 2001 (Wang), 1985 (Lau) as well as models like Zebiac and Cane (1987). They may be "conceptual models capable of producing ENSO-like oscillations" (I like the capable) but as far as I can tell over a period of 50 years none of these contain enough hard physics for them to have been tested and and if necessary discarded - using modern computers or the huge amount of data now available. They cannot all be right and if they cannot be rigorously tested or used for realistic prediction, what are they?

And if there is no decent theory, what really has been learnt? - except that the ECMWF model with data assimilation can predict El Ninos a few months in advance.

Anyway, sorry to go on but I think that mine is a valid point. At the same time I appreciate that many people have worked hard in the subject and I should include more references.

9. Finally

Many thanks for your specific comments which I'll make full use of.

Regards,

David Webb.