REV_01

Reviewer 1

1.1 This study investigates the origin of western equatorial Pacific sea level anomalies that seem to be related to the strong El Niño 1982/83 via modulating the North Equatorial Counter Current. This is motivated by a previous study about the importance of these sea level anomalies in driving the strong El Niño 1982/83. Using a global ocean general circulation model forced by different atmospheric wind fields, the author rules out different possibilities for causing the anomalously low sea level in the western equatorial Pacific, such as the annually occurring Rossby wave or a remote wind field, coming to the conclusion that it is the local wind anomaly field that drives the sea level anomalies driven by Ekman divergence.

The study is interesting as it sheds light on further processes that seem to be relevant in driving overly strong El Niño events beyond the conventional processes that are typically addressed in this context.

No response

1.2 The manuscript has rather the form of a report-type publication rather than a classical research article. However, this does not degrade the fact that it is informative. The methodology, the analytical reasoning and the writing are adequate.

I do not understand the 'report-type publication' unless it means that a review is not included at the beginning of the paper. The introduction has been rewritten and expanded to cover this possibility.

1.3 The overall quality of the current stage of the manuscript is however such that it is partly built too complicated, especially regarding the way the figures are presented (see specific comments).

The figures have been combined to reduce the number of figures and to make their relationship clearer.

NOTE: Because of the figure changes, the text had to be reorganised. This is most noticeable in section 3 (Validation).

1.4 There are some typos and some phrases that are difficult to understand (see technical comments)

The revised manuscript includes a large number of minor improvements to the text.

1.5 I suggest to rephrase the title of the manuscript to "On the origin of western equatorialPacific sea level anomalies prior to the 1982/83 El Niño"

Change made.

1.6 I strongly encourage the author to provide a few more references about driving mechanisms of the strong El Ninos and the role of anomalous wind fields in the introductory section, especially more recent ones.

See 1.2 : New introduction.

1.7 *A lot of figures can be merged to reduce the total number of figures and also ease the comparison among them. For example, one could merge figures 1, 2, 7 and 8; figures 3, 4, 9 and 10; figures 5, 6, 11 and 12; figures 13 and 16; figures 14, 15, 17 and 18; figures 19 and 20; figures 25 and 26. The author may find a more reasonable order of merging some figure, but I think some merging should be done in a way that it reduces the total amount of figures and such that it puts those figures side-by-side that shouldbe compared to one another.*

See 1.3 : Revised figures

1.8 *The author may add the observed sea level and temperature field for that time period to the supplement to complement the model validation.*

Suitable data for 1981 and 1982 is not available. Webb et al (2020) showed that the Nemo model successfully reproduced the SSH and SST fields in the period 1995-2000. The paper assumes that it did equally well for the period 1980-1985.

1.9 *There are some typos such that double-occuring words: "from from", "that that" –please search for these occurrences and correct.*

See 1.4: Many minor errors corrected.

1.10 *I also suggest to add the information of the physical units to the figure captions.*

Units added.

1.10 *I also think that is called "Hovmoeller" rather than "Hovmuller" diagrams.*

Spelling corrected.

1.11 *147-148: Please check structure and meaning of the text - unclear*

Section revised..

Reviewer 02

2.1 *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.*

The revised paper contains text designed to answer this point. The introduction refers to the poor representation of the NECC in many ocean-only models - which Yu et al (2000) showed was due to problems in representing the curl of the wind stress near the latitude of the ITCZ

and overestimating the wind stress on the Equator. Their work showed that the ECMWF reanalysis had the smallest error.

The Nemo model (and the Occam model here) used the Drakkar dataset which is based on the ECMWF with improvements in the equatorial Pacific (see the Drakkar reference for details). The agreement between Nemo and observations reported in Webb et al (2020) indicate that any remaining error in the wind stresses must be small.

The paper reports on the good behaviour of the ECMWF analysis, the further improvement of the Drakkar dataset and the and the good agreement with observations of the Nemo results. Other datasets may have problems but I hope this is enough to cover the forcing used here.

2.2 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.

I am not sure what the reviewer had in mind but to this end in section 6.1 I report on a t-test comparing the response of the different runs to 1981 and 1982 winds. The result is fairly conclusive.

2.3 *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.*

See 1.5

2.4 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 do not really understand the reviewers problem here and take issue with what they are expecting. The abstract does contain the provision "If Webb (2018) is correct .." and given the results of that paper the rest of the paragraph follows.

2.5 *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.*

See 1.3 : Revised figures.

2.6 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

See 1.2 : Revised introduction

2.7 Specific Comments

The paper contains a large number of minor improvements including those referred to here.

* *Paragraph beginning Line 117 and associated Figures. The key, near-equatorial features are difficult to see with latitudes +/- 30 and surface current vectors shown. The author may wish to consider reducing the Y-axis range to facilitate visual inspection of the most import model results.*

On the question of latitude range, the use of 30S to 30N arises partly from the requirement to include the whole of the longitude range of the Pacific without much distortion. It has the advantage of including the tropics and sub tropics.

Once on-line the paper will most often be read using a pdf viewer. In the original manuscript the individual figures contained the underlying postscript at full resolution allowing readers to magnify the figures to see every detail.

Unfortunately I could not find software which could combine the figures at full resolution without entering some infinite loop. Instead for the revised manuscript the longitude-latitude figures use a high resolution jpeg images. These are not so good but still allow a large amount of magnification.

* *Paragraph beginning on Line 252. Integration smooths any field regardless of whether its variability is characterized by a "long term systematic change", or a more event-like abundance of, for example, equatorial westerly anomalies. Results therefore may not imply what the text claims they do*

I would agree that the results do not ‘prove’ that a systematic change in the wind field is involved but I think that the word ‘imply’ is valid - especially given how poorly MJOs and similar events are understood.

* *Line 300. The observed variability of coupled tropical Pacific system does not support a one-to-one correspondence between equatorial ocean current variability and the observed variables most closely related to deep atmospheric convection activity, such as is implied here. The statement about convection should be sufficiently supported or withdrawn.*

Text changed to “would have extended the region over which deep atmospheric convection could occur.”

Reviewer 3

3.1 26 figures for a paper with a relatively short story seem way too many. E.g., there is no need for 16 figures only for model validation. Why not validate the model with one or two timeseries averaged over selected regions and only show one map for one date and one year as an example?

In general, throughout the manuscript it would be very helpful for the reader to have all related panels closer together (e.g., in one figure) so they can be seen and compared at once without flipping pages. Also, difference plots would be very helpful for the model validation as well as in the results section. A lot of the figures can be merged together to one figure with more panels.

Another comment regarding the appearance of the figures. It should be made sure that all figures/panels are consistent among each other (e.g., same axis labels). Also, many (all?) axis labels and axis tick labels are very tiny and could be made larger.

The figures have been reorganised and reduced in number to 12 with key figures close together and with increased typeface size for many of the labels. The validation might have been done in a different way but there then could be arguments about whether the regions selected were sufficient.

I have not added the difference plots as (a) It would have involved more plots and (b) I do not see how they would have contributed to the final results.

3.2 Why is the author only focusing on the 1982/83 event and not also the 1997/98 event? It would be very interesting to know if the presented mechanism also applies to other strong El Nino events.

The original reason was that this is the event which I had studied most. I've added the curves for 1996, 1997, 2014 and 2015 to Fig. 11 of the revised manuscript and commented on the differences.

3.3 Section 1.2 seems unnecessarily long as it already goes into the details of the methods and even results. It could be condensed by leaving out all the details (shifting them to the Methods section) that are partly mentioned in the following sections and merged to the end of section 1.1.

For many readers skimming through the paper, I think this is worthwhile as it outlines the main arguments of the paper and allows them to focus on the aspects that they think are most important.

3.4 There are multiple small spelling and grammatical mistakes (mostly missing words) throughout the manuscript. The manuscript should be carefully checked against such mistakes as it makes it harder for the reader to follow. I have started with a few in the specific comments below but they became too many to all list them here.

Typos have been corrected. The manuscript also contains many other small improvements.

3.5 Specific Comments

* *line 113: Why specifically these dates, 4th June and 2nd September? Probably related to the build-up and major phase of NECC transport. The author should explain/justify this.*

Explanation added.

* *Figures 3-6: In general, Occam seems to overestimate sea level variations as compared to Nemo. This should be mentioned in the discussion of the Figures. What are possible reasons for this? What impact on the results does this have?*

  lines 133-136: It should also be concluded that due to the lack of heat and freshwater fluxes Occam does not well capture the SST features (amplitude) of Nemo? How does this affect the usefulness of the model for this study?

  With the re-organisation of the figures, the text comparing Occam and Nemo has also been revised. I include a lot on the temperature problem as well as sea level but in section 3.3 conclude with what I think is a fair summary:

  "However by bearing these strengths and weaknesses into account, there is no reason why it cannot be used to study the effect of the winds and the initial state of the ocean on the development of the 1982-1983 El Niño, as is done here."

  Of course in the end the results from Occam are only used to provide hints as to why sea level dropped in the Nemo runs - the relationship between the wind curl in the forcing dataset and the sea level drop in Nemo being the key result of the paper.

  lines 133-136
  This was a typo error.

* *lines 198-203: Is this experiment necessary, after all, as the author has already shown in Figure 13 that the 1981 winds cannot produce a sea level low in the western Pacific?*

  See text: "One possibility that has not been discounted is that winds early in the 1982 generated a Rossby wave, or similar, which was later responsible for the drop sea level drop in the western Pacific."

* *213-214: It is unclear to me how this statement contributes to the overall story presented in the manuscript. Please clarify.*

  First, although I do not go into detail within the paper, an observant reader might notice that the wind field anomaly changes between the far western Pacific, where westerly wind bursts and the MJO are most likely involved, and more central longitudes, where the ITCZ is most likely to be dominant.

  Secondly it reminds the reader that both the scale of MJOs and the latitude of the ITCZ - both poorly understood atmospheric features - are close to the atmospheric equatorial Rossby radius. In any decent physics course natural scales would be one of the first topics discussed.

* *Figure 22: Is panel a (left) really necessary as it was already shown before that the winds before 30th April do not play a role here.*

  This is one of the key results of the paper. A second run starting from an independent initial condition provides some additional support to the result.

* *Figures 25-26: Why is the Nemo output shown here instead of the Occam output?*

  Nemo was calibrated against observations in Webb et al 2020. Nemo was forced by a full surface flux data set. Nemo used a much higher resolution,

line 256: I would say the Ekman divergence causes sea level changes, rather than Ekman pumping which causes isopycnal changes.

It is a moot point, divergence lowers sea level by a small amount, which generates a baroclinic response, pumping, which changes the density field, which determines sea level once the pumping has stopped. My focus is on the final sea level.

lines 262-264: What is the reason for this? Is it because of an increase in temperature as the El Nino develops?

I suspect it is because of increased stratification near the surface but Rossby waves may be involved - anyway I, thought that this was something for another time.