05_Comments_01
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Summary

Reviewer 1	:	Excellent	Good	Accepted as is
Reviewer 2	:	Poor	Poor	Rejected
Reviewer 3	:	Good	Fair	Minor revisions

I would like to thank all reviewers for taking time on this paper. I note the 'noise' (Lawton, New Scientist, 19 June 2021, p40-44).

I note that the editor also asked for a major revision.

Major changes
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1 Changes to title and text

One of the main problems comes from the title, which focuses on the 1982-83 El Nino. It is true that the first section of the paper concentrates on this period but this is only to try and understand why the Nemo model generated reduced sea levels at the western end of the North Equatorial Trough in that period. There was no intention to say the Occam model was perfect - just that it reproduced the Nemo results and so, although faster to run, could be used to study why the sea level change occurred.

Once a hypothesis was developed, this was then tested against the observed data, i.e. the reanalysed wind fields, prior to the east Pacific El Ninos of 1997-98 and 2015-16.

What this means that it is not essential for the occam model to be validated against observational data in the early 1980s only that it is validated against the Nemo model.

In the present set of reviews, the focus on the 1980s underlies many of the reviewers criticisms and especially their request for validation of the occam model during this period. I have therefore changed the title and rewritten parts of the paper to emphasise the split between the hypothesis development phase reported in the first part of the paper, where validation against the Nemo model was required, and the testing/validation phase reported in the second part of the paper.

2 Extra figures

To emphasise this point I have also added two figures and their associated text. This increase partly reflects the much greater importance of the second part of the paper.

The first figure illustrates the rise in the density surfaces within the Nemo model during 1997 and 2015, to show the effect of Ekman pumping in these years. The second includes the Nemo forcing estimates of sea level change and the resulting changes in the Nemo density surfaces from 1976 onwards, to put in context the changes that occurred prior to the strong El Ninos.

Detailed comments

1. Review 1

Many thanks.

2. Review 2

2.1 The reviewer is concerned that the paper does not contain a validation against observational data from the period 1981-1983. Reviewer 3 makes a similar point.

In response:

First, the initial part of the paper is concerned with understanding why the Nemo model generated low sea level at the western end of the North Equatorial Trough prior the 1982/83 El Nino. If sufficient computer time had been available the Nemo model itself would have been used for these investigation but here section 3 is used to check that the Occam model is behaving similarly to the Nemo model in key regions during this period and is not affected by its lower resolution (see comments on sub-grid processes below).

Secondly there is a general problem in validating models such as Nemo, Occam or any of the models used in climate research, in that they are so complex that there is no way that they could be properly validated as part of a normal length paper. There is also no guarantee that a validated model does not misrepresent some other key aspect which affects the problem being tackled.

Instead four tests are usually used. The first is that the model is known to be based on good physics. The second is that it has been successfully used in the past without errors which might affect the present study. The third, a trivial one, is that the results are sensible given the known properties of the ocean. The fourth, which is less common, is that the model can make predictions which can be successfully compared against observations.

Concerning the first two tests, both Nemo and Occam have been developed by expert modelling groups and are well documented. They have both also been used successfully in many studies of the ocean in many regions of the world ocean. All ocean (and atmospheric) models have problems with sub-grid processes but here there is no evidence that they are a significant problem.

In Webb (2018), the analysis of Nemo during the development of the strong 1982/83 El Nino was used to predict events that would occur during the development of the strong 1997/98 El Nino. The same paper showed that these events did occur in the Nemo model run itself during the later period. The study of Webb, Coward and Snaith (2020) also confirmed that they occurred in the much better observational data from the later period.

In the present paper the initial study of the 1982/83 El Nino generates the hypothesis that Ekman pumping was responsible for the initial drop of sea level in the North Equatorial Trough. The resulting prediction is that similar strong Ekman pumping over many months would be observed prior to the strong El Ninos of 1987/1998 and 2015/2016.

The prediction did not require further runs of the Occam model as the prediction could be tested directly against the wind stress fields. Because of the increased availability of good scatterometer data, these would would have been much more accurate at the times of the 1997/1998 and

2015/2016 El Ninos than for the 1982/1983 El Nino. The results presented confirm the prediction.

Because of the above points, it appears that there is nothing to be gained by adding a comparison with 1982/1983 observations - although they may be useful in a separate paper which tests the mechanisms discussed in Webb (2018).

2.2 The reviewer believes that I did not take into account his previous comment that "model answers very much depended on which wind data set was used to estimate the surface forcing".

In fact I did take his comments into account and this is the reason why the introduction section contains so much on the wind datasets, their problems and the (possibly lucky) choice of using the ECMWF reanalysis. I thought I had included a reference to the Harrison paper itself but see that I didn't. This omission is now corrected.

2.3 "In my estimation ...".

I do not doubt the reviewer's belief but I hope that after considering the above arguments they will agree that hypothesis development followed by testing of predictions against observational data is equally (or even more) valid.

3. Reviewer 3

3.1 I accept that other people would work in different ways. I did not use climatology because this would have included the contribution from La Nina years. The only other option I considered was the use an average of years with a small El Nino index.

The advantage I saw for using the 1981 ocean was that the background stratification was as close as possible to that at the start of 1982. Similarly the 1981 winds, which must in some way reflect the global 1981 ocean, should be close to what the 1982 winds would have been if the El Nino had not started developing.

If the Ekman pumping hypotheses had been found later not to fit the Nemo model results or the 1997/98 observations, then I agree that the winds would need to be investigated in more detail. But once the hypothesis was developed and shown to be not unreasonable this was not needed.

3.2 I agree that the initial state of the ocean is important - obviously the mechanism proposed would not be effective immediately after a strong El Nino when the pool of warm water in the western Pacific is much reduced.

For this reason I have changed the wording referred to.

The next part of the reviewer's comment refers to the ocean's initial condition, discussed above. The remainder is then primarily concerned with the role of westerly wind bursts in different years.

The present paper does discuss the differences in the development of the 1997/98 and 2015/16 El Ninos and includes references to the unusual behaviour in 2014.

On the question of westerly wind bursts and the additional references given by the reviewer I have two problems. First, westerly wind bursts do not really affect the conclusions of the present paper. Secondly they represent an alternative mechanism for the development of strong El Ninos.

In order to properly discuss westerly wind bursts, the manuscript would need an additional section which argues for and against the two mechanisms. Having thought about it I think this is best done by other people who from a distance can dispassionately judge the competing evidence and arguments.

3.3 Comparison with observations. As discussed in the response to reviewer 2, the test of the prediction of enhanced Ekman suction at the start of the 1997-1998 and 2015-2016 El Ninos - using better wind stress data available for those periods - is a comparison with observations. In both cases the prediction was successful.

I thank the reviewer for the references for data from the 1982/83. I have used these in the revised paper.

3.4 My focus on 1982 is covered in my response to reviewer 2.

3.5 Equatorial currents. I agree that changes in the direction of the Equatorial Current in the west Pacific due to the winds is a key part of the normal El Nino/La Nina process. This has the effect of changing the eastward extent of warm water on the equator and so can significantly change deep atmospheric convection and the large scale atmospheric circulation.

However one of the points of the Webb (2018) paper is that during the development of strong El Ninos warm water transported by the NECC carries warm water much further eastwards, so causing an east Pacific El Nino as opposed to a central Pacific El Nino.

In the reviewed version of the manuscript I do mention the role of the Equatorial Current in transporting warm water eastwards. In the revised version the text has been expanded.

Manuscript Revisions

Reviewers 2 and 3 are primarily concerned about the use of the 1982/1983 in the first part of this study, the lack of comparison with observations during this period and the lack of discussion of other mechanisms - especially the accepted theories surrounding the role of westerly wind bursts and currents in the equatorial wave guide.

My (honest) response has been to provide arguments of why major changes to the paper to address these points are not appropriate. However they are intelligent people - so I suspect that the real problem is background, i.e things I take for granted given my background compared with things they take for granted or are concerned about.

As detailed in "Changes to title and text" I have therefore skipped many of the detailed requests and instead have made changes which, I hope, clarifies my argument and, I also hope, means the paper is better suited for publication. I would like to thank the reviewers again for their time and effort (and for the typos) and hope that they agree the final result is worthwhile.