Ocean Science Discussions

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Reply to review by D. Wang

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., https://doi.org/10.5194/os-2021-5-AC1, 2021

Many thanks for your comments on my paper. In response to your main points:

1. I am intrigued by your comment that it is more like a report than a research paper and am not sure whether, given the extensive length of many research papers, I should take it as a compliment. I always think of a report as a short cold dispassionate thing, which is also good to find in a research paper.

But maybe it is a response to the small number of citations. I know these are important for researchers and that I am bad at the game.

I also have a problem in that the model validation and the run of model tests was a hard slog and inevitably means trying to get a large amount of data across - which is difficult to do without getting repetitive. However I hope that the reader finds the final conclusion worth the effort.

2. On the question of figures, in the two column version of the manuscript (see supplement), many of the key figures lie side by side. Unfortunately this is not possible in the single column version used by Ocean Science for reviews. However the suggestions for reducing the number of figures are helpful and I will investigate the possibilities.

3. On the question of discussing mechanisms, I did this a lot in the previous two papers, so reduced the amount of discussion here. I could say a bit more about the hypotheses which were disproved. However for the final conclusions where Ekman divergence appears to be operating over a period of many months I think a lot more work is needed.

The problem here arises because of my idea that on oceanic time scales, the atmosphere acts as a source of random noise (possibly red, i.e. with an excess of low frequencies) plus some systematic biases (like the westerlies or the trades). To generate anomalous Ekman suction over a period of order 6 months, something has to be organising the atmosphere, and I suspect this has to be some unusual ocean feature.

The simplest might be that it was just a warmer than normal NECC in the western Pacific. But it also could be say a weaker than normal convective regime in the Coral Sea, the resulting weak cross equatorial flows in the atmosphere allowing a stronger than normal ITCZ in the western pacific.

Anyway more hypotheses are needed and more tests.

4. Thanks for the references. I will read (and reread) them and consider.

5. As far as I know the only sea level measurements available for 1982-83 would be from tide gauges on island stations. This is really a task for another paper. For the 1997-98 El Nino, comparisons with satellite data were made in the Webb, Coward and Snaith (2020) paper - which showed very good agreement between the NEMO model and observations. For that reason I think that for the purposes of this paper comparison with the NEMO sea levels is a good start. However there is nothing to stop someone else looking at the tide gauge records.

6. My meteorological colleagues claim that MJOs are associated with westerly wind bursts and so cause El Ninos. I'll find some references.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2021-5/os-2021-5-AC1-supplement.pdf