Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-94-RC2, 2019

Ocean Science
Discussions
Open Access

Interactive comment on "Internal tides in the Solomon Sea in contrasted ENSO conditions" *by* Michel Tchilibou et al.

William Kessler (Referee)

william.s.kessler@noaa.gov

Received and published: 12 November 2019

Review of "Internal tides in the Solomon Sea in contrasted ENSO conditions" by Tchilibou et al.

Non-anonymous review by Billy Kessler, NOAA/PMEL

This is useful work that will be of interest to the community. The authors use simulations with and without tidal forcing, following and progressing from previous such work. They have advanced the understanding of the role of tides, and the modeling of tides, in the Solomon Sea. The results might have implications for ENSO effects, and also be of interest for other such partly-confined seas.

I recommend MINOR REVISION.

Printer-friendly version

Discussion paper

Major point: The authors consider the periods JFM 1998 (El Nino) and AMJ 1999 (La Nina). They should emphasize that these are truly extreme periods, likely to exemplify the very maximum possible effects of ENSO variations.

In that light, the rectified anomalies due to tides are quite small: about 0.08psu in the surface layer, and 0.06psu in the upper thermocline (lines 568-583, and Fig.13). These small signals, given that extreme-opposite situations are compared, are not particularly convincing that ENSO-related tidal mixing is an important part of the observed erosion of thermocline properties that has been shown in several publications. In effect they are an upper limit of tidal effect variation, and the values given really aren't that impressive.

In addition, there may be tidal-effect anomalies due to the study periods being in different phases of the seasonal cycle. These are not brought out, but they should be since we can't tell if the rectified tidal difference signals are due to ENSO or to seasonality.

It would therefore help to expand the work to include a comparison of the phases of the seasonal cycle (say by compositing an annual cycle from the many years of the NEMO simulation). That might explain part of the differences found. (It might even be that the ENSO anomalies are _larger_ in the context of the seasonal cycle).

Given the above, some of the results are over-claimed. Examples would be the last, and 3rd-last, sentences of the abstract, and the paragraph in lines 640-645.

Other comments:

1) The boundaries of the nested region should be stated in the text, and ideally shown on a map (probably Fig.1). The crucial tidal forcing is specified as a boundary condition at these edges, so this is an important point that should be stated precisely (section 2.1).

2) The explanation/definition(s) of barotropic and baroclinic components is explained very nicely in section 2.2a. This was especially clear and helpful to this non-specialist. Bravo!

However, one point remains to be explained, possibly reflecting my ignorance: I am familiar with a Sturm-Liouville mode decomposition, but only by assuming a complete separation of z from (x,y,t). In that case the vertical structure cannot vary in (x,y,t), but here it apparently is computed locally (according to the topography) and therefore does so vary. That means that the vertical mode structures differ by location; presumably the modes would then disperse in hard-to-understand ways, and their propagation becomes unclear.

On a related point, lines 504-506 assert a mode change due to the near-surface T/S ENSO variability. This could be verified by doing the S-L decomposition for these two periods. Do the modes in fact change?

3) (nearly a "Major comment") Lines 336-341 describe large-scale water mass changes during the ENSO cycle. But I think this might be confusing "heaving" of the thermocline (vertical motion of the entire T/S structure) with water mass changes. This is apparently the cause of the anomalies in Fig.4c/d. It is easy to be misled as these might appear to be T/S variations if measured at a particular depth. But if what is really happening is heaving, then no water mass changes are implied. Please clarify this point, perhaps by plotting on isopycnals instead of depth.

This is a crucial point because the paper argues that differences in tidal effects across the ENSO cycle have implications for mixing and downstream impacts.

4) Lines 479-495: This paragraph reads like an incoherent list of information. It jumps topic from sentence to sentence. I found it very difficult to read. Please improve it, perhaps by making an outline of the points to be made, and then probably separating into several paragraphs, each with a clear topic.

5) The analysis of potential effects on SST - and thus on air-sea fluxes - is intriguing but not developed enough (lines 610-624). The idea seems plausible but with the short description here the conclusion is not well established. Since this is mentioned in the abstract and several other places (e.g. L642-645) the conclusion needs to be

clearer and more confident. Otherwise it is could be mentioned in the discussion but not highlighted as much as done here.

Worth noting here (line 621) is that anomalies don't mix, only total properties do. Please rephrase this sentence.

6) Line 641: As noted in the major comment above "greatly" is much too strong for the small rectified changes described, given the extreme forcing differences.

7) I have spent a great deal of time along coasts of the Solomon Sea in several locations on both sides of the sea, and consistently observe a very predominantly diurnal (24-hr) sea level tide. This phenomenon appears in Fig.6a.

Why is this paper almost entirely focused on the M2 tide? Why doesn't the diurnal tide also produce a baroclinic component worth analyzing? If there is a straightforward answer it would be useful to say it.

8) Considering the small rectified changes noted in the major comment above, it might be worth noting that there is a different hypothesis for the mixing that apparently occurs in the Solomon Sea. Kessler et al (2019) show very large changes of velocity structure between their two glider lines, one just outside the sea, the other just inside (e.g their Figs.1 and 6 or 11). The two inflows (SEC and NGCU) merge (and presumably mix) just at the southern entrance to the sea. They speculated that the property differences between entrance and exits noted in several papers could be due to this non-tidal effect.

9) The figures need a good deal of improvement:

a) Many (most) of the figures have information that is too small to see. These include:

- Teeny-tiny axis labels (Fig.4 is the worst, but 3, 6b, 10, 11, 13, 14 are also very hard to read).

- Some fine contours are impossible to read and are thus useless. E.g. phase lines in Fig.7 and EKE contours in Fig.14.

Printer-friendly version

Discussion paper

- I strained to see the "isobathymetric lines" referred to in many figures. I still don't know what this refers to; is it just the reef edge? (In any case I would call these "isobaths").

- Teeny vectors and color blotches in Figs.9 and 10. These figures thus do not convey the information desired. Perhaps use fewer, larger vectors and simplify the color shading?

b) It is hard to see the difference between the green and blue lines in Fig.5. Then these colors change meaning in Fig.6. These should be more distinct, and use consistent colors (for El Nino/La Nina) between these figures.

Minor comments: 1) The English is quite good throughout, with the exception of the Introduction which is sprinkled with distracting small errors. Since one of the authors is a native English speaker (let's accept for the moment that Australian is a close-enough dialect of English ;-), it would be worth going over this section.

2) Line 323: I think this should be "154E" (not 154S).

3) The citation for Kessler et al (2019) is wrong (author list as published is different).

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2019-94, 2019.