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Ocean Science Discussions

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

Anonymous Referee #1

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Overall this is very interesting work and without a doubt will eventually be a solid contribution. The large differences in baroclinic energy between ENSO states is striking.

The major area in need of improvement is to provide a quantitative assessment of what has changed the baroclinic energy between ENSO states. There is a change in eddy energy, which is clearly shown. However, the actual path by which baroclinic energy changes is not identified. A model is used and so a definitive answer should be found. See works by Zilberman et al and Rainville et al for some possible methods to evaluate how energy changes during generation and propagation.

Some obvious model-data comparison is missing. Mode 1 and mode 2 energy flux is calculated. The results could be compared to altimetric observations by Zhao et al, although not for different ENSO states. Another point of observational comparison is

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Pinkel et al who observed internal waves propagating northward from Solomon St. A new mixing parameterisation is used, but not compared to existing methods in this area (Alberty et al).

comments by line

55 - Jeffreys 1920 actually first identified marginal seas as likely sites

64 - internal waves originating from this topography is also noted by Pinkel et al (1997, doi:10.1029/97gl01610)

100 - "...first attempt..." I'm not sure this is correct or perhaps it's just a poor choice of words. Robin Robertson has several publications in this area and in general there are mixing parameterizations aplenty.

103-113 - The references are inappropriate in some cases here and elsewhere in the manuscript. I suggest referencing the first work and then the latest or most important work in these areas:

Altimetry - Ray & Mitchum

Regional models around Hawaii from the HOME experiment - Merrifield and Holloway paper(s) or Rudnick 2003

Indonesia - Robertson as noted earlier

111 - East China Sea is irrelevant here. I don't know of any tidal studies in the SW Pacific myself, but it would be better to say "As far as we know, no dedicated studies..."

121 - There are again older references on incoherent tides - Munk and Colosi 1998(?)- and observations showing the deflection of internal tide trajectories - Rainville et al 2003(?).

124 - reference?

199 - Vertical modes are invalid over sloping topography.

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201- Here and elsewhere, subscripts are traditionally used.

221 - Nonlinearity of the internal tide in this area is not necessarily small. Large-amplitude internal waves are generated (Pinkel et al, 1997). In areas with shallow topography, tidal harmonics are often noted elsewhere.

231 - Is this not just C = wp' ? Surely there is an earlier reference.

231 - Also this relation is often linearized. Is that the case here? wp' is evaluated at a constant depth level z = 0 (neglecting any topography and where the surface would be at z = h). z=-h and grad_h is a bit confusing. Maybe the depth could be H.

236 - Please proofread all your equations here and elsewhere and use accepted mathematical notation. Alternate or non-traditional notation distracts unnecessarily. Use nabla \dot vector{F}_{bt}. dz is missing too. Same for tidal components such as M_2 and K_1. Using an overbar for barotropic is unusual and with velocities is taken to mean vector. Are you only considering the u component of velocity or is u intended to be a vector?

250 - Complex demodulation or a wavelet transform would be a better way to determine the incoherent fraction.

291 - cmˆ2/sˆ2 - please use SI convention cmˆ2 sˆ-2 or even better 0.2 mˆ2 sˆ-2 in this case

Fig 3 - Cm is incorrect, cm is correct. Use letter labels to identify panels.

Fig 4 - psu or S (psu) would be better. What are the black contours?

323 - Looks to me more like a NE-SW propagation direction. Perhaps a section in that direction would be better.

326 - CARS is nonstandard climatology. Please explain in methods/data section.

353- Upper ocean Nˆ2 has been mentioned. What about deep Nˆ2? Seems pretty

similar and unlikely to affect generation?

Fig 7 - Bathymetry source could be acknowledged or mentioned in the methods section. I does not have to be included in figures: “Isobathymetric lines are from the NOAA/ETOPO2v2 bathymetric file from the Smith & Sandwell database (doi: 10.7289/V5J1012Q)”

Fig 7 - I’m not sure a comparison to a nested model is that valuable. Validation with tide gauges or some other data source is better.

388 - I don’t understand this point about coherent SSH being used to correct altimtery. (1) Correct what? (2) Altimetry measures total SSH = coherent internal tides SSH + incoherent internal tides SSH + everything else. Or are you talking about correcting the M2 amplitude?

393 - Solomon Strait not Solomon strait. Also elsewhere.

403- What is the surface displacement of a mode 1 tide of 10-m amplitude under the conditions in Fig 5? What do the modes for these conditions look like at the generation site? Is the mode-1 maximum aligned with topographic height in some way?

414 - strong flow is generally associated with high Reynolds number which is generally more turbulent and not more laminar. . .? Perhaps you want to rephrase in terms of mesoscale eddy activity.

Fig 9 - units need a space kW[space]mˆ-1 for example

435 - Do Zhao et al have altimetric fluxes in this area? How do they compare to the model? I believe Zhao now uses modes 1 and 2 in his calculations.

470 - doubles is not accurate

479 - The overall difference in internal tide energy and dissipation between the 2 states is established nicely. The explanation though is not so clear.

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504 - “One explanation for such a difference is theÂăchange in stratification between the two ENSO states, with stratification closer to the surface during El Niño that favors the excitation of higher order modes (Fig. 5).” This explanation is a little vague. You have calculated the various source and sink terms. Which ones does it affect? Once you have determined that, which quantity is affected p’ or u’ or something else? And by what? Eddies, changes instratification, changes in currents, etc? See: Zilberman et al (2011) doi: 10.1175/JPO-D-10-05009.1âĂÍ

670 - This describes energy flux. If you wish to better see the contribution by higher modes, energy density is a helpful scalar. Flux = Energy x c. Fluxes emphasize mode 1 because c_1 is about 2 x c_2. Fluxes near the source regions may be confusing- e.g., 2 oppositely directed fluxes give flux = 0 even though there is plenty of energy.

697 - It’s noted that local dissipation is considerable, while for other topography (Hawaii) very little energy is dissipated. Even in the Solomon Sea area there are some ridges that are dissipative and others that are far less dissipative. Explanation is not really provided as to why.

736 - Kida & Wijffels (2012) doi: 10.1029/2012JC008162 also note surface cooling in the Maritime Continent on a fortnightly cycle.

Fig 11- I’m not sure this makes sense. Flux is of the same sign on both sides of the topography. Either the figure does not go deep enough or the meridional direction is not really suitable to show what the authors intend. Perhaps a more NE orientation? Or deeper coverage?

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