

Interactive comment on “Seasonal cycles of mixed layer salinity and evaporation minus precipitation in the Pacific Ocean” by F. M. Bingham et al.

F. M. Bingham et al.

binghamf@uncw.edu

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Reviewer 1

Main comments. The manuscript aims at analyzing seasonal changes in mixed layer salinity (MLS) within 20°S–60°N in the Pacific Ocean, using complementary sources of salinity data. Seasonal changes in E-P are also examined, together with estimates of salt advection, so that to assess the role to the main factors assumed to be responsible for MLS changes. The ms. is relatively clear, easy to read, and does not include misinterpretations and deficiencies. It is however a poor contribution to the field since most, if not all, results were previously derived and discussed in the published and cited literature.

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First, we appreciate the reviewer's careful reading and thoughtful comments. They have greatly improved the manuscript. We do believe that what is presented here is new and novel. We know of nowhere in the literature where the seasonal cycle amplitudes and phases are displayed in such detail including the statistical significance. There are similar calculations of mixed-layer salinity balance published, but not in this part of the ocean.

Specific comments.

1/ The terminology of 'Mixed Layer Salinity (MLS)' is unclear. What is actually analyzed is salinity data collected in the 0-10 m layer. Why does this should be representative of the mixed layer salinity ?

To satisfy the reviewer's concerns (and those of reviewer 2), we have changed the term from MLS to the more generic UOS, upper ocean salinity.

2/ page 2392, line 15. Looking at DH91, the maximum standard deviations they found is actually 0.4 (and not 0.2).

Fixed

3/ page 2393, lines 12-14. P is indeed not the only process controlling MLS. It would be fair here to cite Gouriou and Delcroix (2002) and Johnson et al. (2002).

OK

4/ page 2395, line 4. I'm not comfortable with the word 'random'. What do the authors mean here ? The locations are not random as ARGO deployments were based on scientific programs, and the ARGO drifts are controlled by the mean circulation. May be change that word.

We get the reviewer's point here. How about making it "quasi-random"?

5/ Section 2.1. What is the rationale to stop at 20°S latitude ? There are apparently some data further south, at least to 30°S. Also, it would be informative to detail the

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period covered by each data set.

The period covered by each dataset is shown in Figure 2. Is the reviewer looking for more detail than that? In the revised manuscript, we have extended the analysis to 40S.

6/ Section 2.2. MLS time series are made available on a 2.5° latitude x 2.5° longitude grid. Given data availability, the length of the times series are variable depending on where you are. Still, based on the cited references, we know that MLS in some regions are strongly affected by ENSO. Then, how can you compare amplitudes and phases of the annual harmonics when computed from different time periods, and how does the ENSO signal impact estimates of those quantities ? Also, were the time series detrended before performing the Fourier analysis ? This should be discussed in more details here.

Harmonic (not Fourier) analysis is a very standard method in the field, and is discussed in the reference (Emery and Thomson) to which the reader can refer for more detail. No, the data were not detrended before fitting. We have added a sentence stating this. At the suggestion of the reviewer, we tried “pre-detrending” but the results were not substantially different.

The question of ENSO influence is important, since ENSO is a multi-year signal, but phase-locked to the seasons. We ran the harmonic analysis first excluding ENSO years, and found little difference in the results. Then we ran the analysis including only ENSO years. There was some minor differences, but mainly we believe due to a smaller amount of data.

7/ Section 3.1. The lack of strong seasonal cycles in MLS for the SPCZ is surprising, given past results. Any idea of why this does not appear in the present analysis ?

Part of the SPCZ is over the area of large seasonal cycles in UOS, particularly, around (10S, 170W). Some discussion of this has been added to section 3.1.

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8/ page 2402, lines 13-14. Results from Boyer and Levitus (2002), in the reference list, must be cited as well here.

Done

9/ Section 3.2. Equation (2), I suspect that u refers to the velocity vector (u,v) and not to the zonal velocity (u) only. Please provide some details here.

We added a vector symbol over the u , and a statement that it refers to the vector velocity.

10/ page 2406, lines 5-7 and 16-17. The conclusion that seasonal advection play a minimal role in the salinity balance is inconsistent with the results of Johnson et al. (2002) for the tropical area, noting that these last authors did use the OSCAR currents having a marked seasonal variability. Any explanation for the different interpretation ?

Johnson et al are not very specific with regards to variability in the tropical Pacific. They do show maps of salinity divergence in four different seasons, and it is clear there is some variation between them. However, they do not explicitly calculate the seasonal cycle or determine its significance. They state that seasonal variability of salinity divergence is 53% of the annual mean, but are not clear about what area they are talking about. Thus it is difficult to compare their results with ours directly. Also, we consider only the $u' \langle \text{grad-S} \rangle$ term and part of the seasonality shown in Johnson et al. (2002) may come from the $\langle u \rangle S'$ term.

11/ page 2406, lines 26-28. “Imbalance in the . . .”. That sentence is correct but why is it needed here ?

This paragraph contains some speculation regarding the implications of a seasonal cycle that may not be closed, and the relationship between it and ENSO. We have added another sentence that may make the point clearer.

12/ Figure 8. ‘ dS/dt ’ should be partial derivatives

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Fixed

13/ Figure 3. What are the differences, if any, between panel A (left ?) and panel B (right ?). This is unclear to me, and probably to other readers. Please explain.

Panel B was inserted by mistake in the production process and should not have been there. It has been removed.

Interactive comment on Ocean Sci. Discuss., 6, 2389, 2009.

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