Ocean Sci. Discuss., 6, C1171–C1178, 2010
www.ocean-sci-discuss.net/6/C1171/2010/

Ocean Science Discussions

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

Received and published: 5 April 2010

Reviewer 2

Review of "Seasonal cycles of mixed layer salinity and evaporation minus precipitation in the PaciïňĄc Ocean" by F. M. Bingham, G. R. Foltz, M. J. McPhaden, and T. Suga. This paper explores the seasonal cycle of mixed layer salinity using harmonic analysis for the PaciïňĄc Ocean 20S to 60N. Areas of seasonal cycle of amplitude 0.1 to > 0.5 are identiïňĄed. Many areas of small or no seasonal cycle are also identiïňĄed. Compari- son is made to evaporation minus precipitation seasonal cycle. This paper is similar in method to previous work, but does have added value in that it uses the Argo proïňĄling ïňĆoat data along with other historical datasets (CTD, thermosalinograph, bucket) and makes an attempt to calculate the impor tance of E-P in salinity variability

for different areas of the PaciïňĄc. The paper has scientiïňĄc merit, but lacks focus. Many subjects are touched upon, but none are explored in detail. For instance, the discussion indicates that large amplitude changes in mixed layer salinity (MLS) play a signiïňĄcant role in mode water formation in the Nor thwest PaciïňĄc. But this is never shown or even addressed in detail in the body of the paper. The most interesting results are found in Table 1, comparing time var ying salinity changes vs. E-P and advection, diffusion, entrainment factors. But to inter pret this table, the calculations and reasoning need to be better developed. I would recommend signiïňĄcant changes before ïňĄnal acceptance.

We thank the reviewer for his/her careful reading and thoughtful comments. They have improved the manuscript considerably. The comment in the introduction about mode water was put there simply as one motivation for doing the study. This study is not specifically about mode water formation but we hope can help understand it.

Below are more speciïňĄc comments:

1. Much of the value of the paper lies in the use of recent Argo data. For the ïňĄrst time relatively uniform spatial and temporal coverage are available for salinity data for the PaciïňĄc Ocean. But there are only a few years of such coverage available and so the data are simply mixed with salinity data from other sources. Much more exploration of the effect of the Argo data is necessary. First, how do the Argo data affect coverage for different seasons? I expect, especially for the higher latitudes that the Argo data dominate the winter season, while the coverage is more balanced in other seasons. In this case, would the winter months MLS be skewed to the last few years of Argo data, while the other months have a more uniform temporal (yearly) distribution. What are the implications for the resulting amplitude of the seasonal cycle? It would be interesting to see the results of the harmonic analysis with and without the Argo data to see how much of an effect the Argo data may have.

The harmonic analysis was attempted using only Argo data and without any Argo data, without much difference to the overall results. Some areas, especially the northeastern North Pacific and eastern South Pacific, were sparsely sampled pre-Argo, and thus the Argo data helped fill in some gaps.

2. The paper never really deïňĄnes how much data is sufïňĄcient to get a robust seasonal cy- cle. There is no clear indication in the text or ïňĄgure 3 whether the areas of low seasonal cycle are due to lack of data or simply lack of a seasonal cycle in the area. The authors also state in the discussion that more Argo data may ïňĄll in holes in the analysis. How much data is necessar y in each season, month? Please deïňĄne a criteria.

Amplitudes and phases were determined using a least squares harmonic fit as stated in section 2.2. We required at least 10 data points in each box. A standard f-test was used to determine whether the fit was significant at the 95% level. Blank areas in the figures were places where enough data existed, but no significant fit was found. All of this is stated in the text or figure captions, though clarified somewhat in the revised version.

The question remains as to whether the blank areas are blank because of a lack of data, or because there really is no seasonal cycle there. There did tend to be fewer data in the blank squares. Blank squares had a median of 189 observations vs. 322 for non-blank. Perhaps as Argo fills in some of the data gaps, seasonal variability will emerge. However, our guess is that the picture will not change that much.

3. Figure 6, the E-P cycle also has white space which is undeïňĄned. Is this also lack of data or does the white space designate something else?

White or blank areas indicate that enough data existed, but no significant seasonal cycle was found. This was not clear before, but has been stated in the figure caption in the revised version. Similar statements were added to some of the subsequent figure captions where needed.

4. The paper notes that Delcroix et al. (2005) adjusts bucket and thermosalinograph data. Why does the present paper not do this?

This does make a small difference in the results, so we decided to add this to the data processing steps.

5. Why is MLS used instead of sea surface salinity (SSS)? I see little difference between MLS as it is deïňĄned here and SSS. Mixed layer is deïňĄned here as a change of 0.2degC in temperature. So salinity over the entire mixed layer and SSS (or MLS as deïňĄned in this paper) are not necessarily the same thing, given barrier layers and other phenom- ena. What is really being examined in this paper is SSS, then the assumption is made that SSS is representative of the entire mixed layer for the calculations which involve the full mixed layer. Please be more careful with the terminology and also justify why SSS can be used to represent the full mixed layer when that mixed layer is temperature dependent.

See response to reviewer 1. In general we used the shallowest observation available as long as it was 10 m or less. For Argo, that is usually 5 m. For bucket and TSG measurements that may be less. Salinity at 10 m cannot really be described as SSS, so we changed terms to the more generic UOS instead.

6. The TAO/TRITON array is used as an independent dataset with regards to the harmonic analysis of the seasonal cycle. However, ver y little attempt is made to compare the independent datasets. I would expect, the TAO/TRITON SSS to have a robust seasonal cycle given the high frequency of measurements and the long, consistently measured time period over which SSS measurements are available. This would make the TAO/TRITON seasonal cycle a ver y good check of the same calculation using the CTD/thermosalinograph/Argo/bucket dataset. More qualitative comparison along with a quantitative (correlation coeff?) comparison would be ver y useful.

This is a great idea. We calculated the correlation between the amplitudes derived from bucket/Argo/CTD data and amplitudes from the TAO moorings in the same area. They correlated very nicely with each other with correlation coefficients of 0.68 (amplitude) and 0.73 (phase), and slopes of about 1. We included a discussion of this in the revised MS.

7. A little more detail is needed on ïňĄgure 11. I assume the dark black line is the mean for each month, but this needs to be explicitly stated if this is the case.

This is stated in the caption, “Solid curves are monthly averages.”

8. The areas used in ïňĄgure 10 and table 1 appear completely arbitrar y. Some explanation of why these areas were chosen for the salinity variation calculations is in order. In the results section 3.1, areas NWP, SP, TP, and HI are identiïňĄed, but only loosely deïňĄned. To clarify the paper and maintain some kind of continuity, it would be nice if these areas were deïňĄned graphically either right on ïňĄgure 3 or on a separate ïňĄgure. Figure 10 could do this job if there were some coherence between the areas deïňĄned in section 3.1 and those deïňĄned on ïňĄgure 10/table 1.

The areas in Fig. 10 were chosen (somewhat arbitrarily as the reviewer points out) to correspond to regions where seasonal variability is important, and appears to be coherent across the region. Fig. 10 has been aligned more closely with the text in Section 3.1 by defining a new area in the northern North Pacific.

9. On page 2403, line 8, the authors state that "The phase of E-P (Fig. 6b) indicates the month of maximum E/minimum P". This is not necessarily the case. It indicates the maximum difference between E and P only, not the max or min of either variable separately.

This is true where the seasonal cycles of E and P are of similar size, but throughout most of the domain, one or the other dominates. Nevertheless, the reviewer is correct and we have changed the wording to be more precise.

10. On page 2403, lines 9-11, the authors state "In a regime where MLS variability is dominated by E-P, we would expect approximately a three month time lag between the

maximum of E-P and the maximum of MLS". Why is this to be expected? Why does the seasonal cycle of the S0(E-P)/h term largely follow E-P? Is the cycle of mixed layer depth simply in phase with the cycle of E-P, or is mixed layer depth seasonal cycle not large enough to affect the S0(E-P)/h term?

According to equation (2), (E-P) is proportional to the derivative of S, dS/dt, not S itself. If E-P is sinusoidal, then dS/dt peaks at the same time, or S peaks one quarter cycle later. DH91and Hires and Montgomery discuss this at some length, and a reference to these papers was added. Also, the word “seasonal” was inserted before MLS (UOS) to be clearer.

As far as the second point, the seasonal variation of the mixed-layer thickness does not seem to be enough to overcome that of E-P. At the reviewer’s prompting, we have gone over the calculation again carefully and believe the result to be correct. We have added a sentence stating this.

11. How is the phase of dS/dt shifted back 3 months from the phase of MLS? First, it is not really possible to compare ïňĄgures 4 and 8 for a 3 month shift. Maybe if the color bar for ïňĄgure 8 were shifted by 3 months, the ïňĄgures could be compared. Secondly, the way the calculation is set up and executed and described, dS/dt is simply propor tional to E-P, with no signiïňĄcant advective component except close to the California Current, and either no signiïňĄcant cycle in mixed layer depth or mixed layer depth in phase with E-P, so the phase of dS/dt should look just like the phase of E-P. Is this saying anything valid, or is it a preordained outcome of the setup of the problem? I believe the reality is a little more complicated. Some attempt should be made to estimate the entrainment and diffusion terms, as well as the other 2 advective terms, even if ver y generally. Are any of them signiïňĄcant? If not, then, yes dS/dt should be simply propor tional to E-P/h, and if h is in phase with E-P or does not have a signiïňĄcant seasonal cycle, dS/dt would simply be propor tional to E-P.

See response to point 10. As stated in the caption for Fig. 8, it presents exactly the same information as Fig. 4, but shifted backward in time by 3 months. For this reason we hesitated to include the figure as it is somewhat redundant. Fig. 8 indicates the phase of dS/dt, as opposed to the phase of S, which is shown in Fig. 4. In our calculation, we compare the phases and amplitudes of the terms of eq. (2) to see if they are similar.

The calculation of advection includes both u and v components. A simple scaling argument can be made to indicate that the seasonal variability of entrainment is small – though this is an important process in the mean budget of the surface salinity. As far as diffusion, calculation of that requires gradients and their seasonal variabilities and vertical or horizontal diffusion coefficients and their seasonal variabilities, none of which I feel able to estimate with any accuracy from the current data.

12. There is no mention of the South PaciïňĄc Convergence Zone. This is within the study area. Is there no effect on salinity of this meteorological feature? Figure 3a and 4a are repeated twice. A ïňĄgure for the amplitude of dS/dt should be provided, even if it is the same as 3a, with different units, since it was calculated independently.

The revised version covers the SPCZ. The repeated figures have been deleted. The figure depicting dS/dt is exactly the same as the one depicting S. It is only the color scale that changes. We included the appropriate color scale, but did not feel the need to repeat the figure.

13. I am not that familiar with Ocean Science Discussions, but I have never seen color bars given their own ïňĄgure designation (such as ïňĄgure 1E). It caused me confusion, especially with ïňĄgure 7. Can the color bars simply be par t of the ïňĄgure without a special designation?

Since the color scales are referred to by other figures, we would prefer to leave them the way they are. However, if the editor feels differently...

14. Figure 5 shows the percent variance due to the seasonal cycle. In most instances it

appears to be below 25% of the variance. The authors state that the second harmonic is insigniïňĄcant. Where then is all the variance? Is it in higher harmonics, basically noise, longer time scales?

This is an excellent question, one probably worth exploring using time series of SSS, perhaps even using the TAO data. However, we do not feel it is within the scope of this paper. From Fig. 12, one would guess that much of the missing variance is in frequencies higher than 1 cycle / year.

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