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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.

Anonymous Referee #2

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Review of "Seasonal cycles of mixed layer salinity and evaporation minus precipitation in the Pacific 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 Pacific Ocean 20S to 60N. Areas of seasonal cycle of amplitude 0.1 to > 0.5 are identified. Many areas of small or no seasonal cycle are also identified. Comparison 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 profiling float data along with other historical datasets (CTD, thermosalinograph, bucket) and makes an attempt to calculate the importance of E-P in salinity variability for different areas of the Pacific. The paper has scientific merit, but lacks focus. Many subjects are touched upon, but none are explored in detail. For instance, the discussion in-

dicates that large amplitude changes in mixed layer salinity (MLS) play a significant role in mode water formation in the Northwest Pacific. 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 varying salinity changes vs. E-P and advection, diffusion, entrainment factors. But to interpret this table, the calculations and reasoning need to be better developed. I would recommend significant changes before final acceptance. Below are more specific comments:

Much of the value of the paper lies in the use of recent Argo data. For the first time relatively uniform spatial and temporal coverage are available for salinity data for the Pacific 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 paper never really defines how much data is sufficient to get a robust seasonal cycle. There is no clear indication in the text or figure 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 fill in holes in the analysis. How much data is necessary in each season, month? Please define a criteria.

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

The paper notes that Delcroix et al. (2005) adjusts bucket and thermosalinograph data.

Why does the present paper not do this?

Why is MLS used instead of sea surface salinity (SSS)? I see little difference between MLS as it is defined here and SSS. Mixed layer is defined here as a change of 0.2degC in temperature. So salinity over the entire mixed layer and SSS (or MLS as defined in this paper) are not necessarily the same thing, given barrier layers and other phenomena. 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.

The TAO/TRITON array is used as an independent dataset with regards to the harmonic analysis of the seasonal cycle. However, very 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 very 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 very useful.

A little more detail is needed on figure 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.

The areas used in figure 10 and table 1 appear completely arbitrary. 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 identified, but only loosely defined. To clarify the paper and maintain some kind of continuity, it would be nice if these areas were defined graphically either right on figure 3 or on a separate figure. Figure 10 could do this job if there were some coherence between the areas defined in section 3.1 and those defined on figure 10/table 1.

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.

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?

How is the phase of dS/dt shifted back 3 months from the phase of MLS? First, it is not really possible to compare figures 4 and 8 for a 3 month shift. Maybe if the color bar for figure 8 were shifted by 3 months, the figures could be compared. Secondly, the way the calculation is set up and executed and described, dS/dt is simply proportional to E-P, with no significant advective component except close to the California Current, and either no significant 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 very generally. Are any of them significant? If not, then, yes dS/dt should be simply proportional to E-P/h, and if h is in phase with E-P or does not have a significant seasonal cycle, dS/dt would simply be proportional to E-P.

There is no mention of the South Pacific 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 figure 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.

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

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 insignificant. Where then is all the variance? Is it in higher harmonics, basically noise, longer time scales?

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