

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 #1

Received and published: 15 December 2009

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.

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 the mixed layer salinity ? 2/ page 2392, line 15. Looking at

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DH91, the maximum standard deviations they found is actually 0.4 (and not 0.2). 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). 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. 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 period covered by each data set. 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. 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 ? 8/ page 2402, lines 13–14. Results from Boyer and Levitus (2002), in the reference list, must be cited as well here. 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. 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 ? 11/ page 2406, lines 26–28. “Imbalance in the ...”. That sentence is correct but why is it needed here ? 12/ Figure 8. ‘ dS/dt ’ should be partial derivatives 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.

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