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Ocean Science Discussions

Interactive comment on "Characteristics of the seasonal cycle of surface layer salinity in the global ocean" *by* F. M. Bingham et al.

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The manuscript is a sensible extension to a previous work on "Seasonal cycles of surface layer salinity in the Pacific Ocean" by the same authors. The study examined the amplitudes and phases of the surface layer salinity (SLS) in all areas of the open ocean between 60°S and 60°N, and studied the relationship of SLS with E-P forcing. There have been a lot of interests recently in better understanding seasonal variability of SLS spurred by the SMOS and Aquarius salinity missions. The results are well organized. The study would be a useful addition to the literature after revision. My comments are given below. (1) Distinction between E-P and So(E-P)/h: Strictly speaking, E-P is surface freshwater flux, while So(E-P)/h is surface freshwater forcing. The two expressions have different meanings because of h in the latter. Seasonal change in h is governed by seasonal heating and wind stress, and the phase of h could be different from E-P. Thus, it is better not to refer So(E-P)/h as freshwater flux in the manuscript.

(2) Role of h (Abstract, lines 16-18, Figures 11-12): Is the bimodal distribution of So(E-P)/h controlled by E-P or h? In other words, which variable, E-P or h, plays a more dominant role in determining the peaks in January and July? It seems that the thickness of the mixed layer plays an important role in modulating the oceanic response to E-P flux. If the amplitude and phase of h are added to Figure 11, would it be easier to delineate the role of h?

(3) The scatter plot in Figure 13: There seems no clear relationship in amplitude between SLS and So(E-P/h, which is expected as many studies have shown that, even in the tropical oceans where SLS is most responsive to freshwater forcing, only 40-70% of SLS variance can be attributed to freshwater input (e.g.Delcroix and Henin, 1991; Reverdin et al. 2007). Advection is responsible for about 25% of local salinity budget near strong currents. A covariance contribution of freshwater forcing to SLS was mapped by Yu (2011). Figures 9& 11 in that paper could help to explain Figure 13 here. It would also be beneficial to include the findings of previous studies in the discussion.

(4) The scatter plot in Figure 14: The scatter plot comparing the phases of SLS and So(E-P)/h is broadly consistently with the cross-correlation in Yu (2011, see Figure 12), in which the correlations between the SLS tendency and freshwater forcing peak at 2-4 months time lag, equivalent to a 1-3 month time lag for SLS. Fast processes such as mixing, which may be important locally and regionally, and slow processes such as advection, which bring in remotely generated signals to the local, can all break down the expected 3-month time lag. The peak correlation at 1-2 month lag has been reported both in observational analysis (e.g. Delcroix et al. 1996) and modeling study (e.g. Mignot and Frankignoul 2003).

(5) Amplitude difference in Figure 16: Errors in h and E-P would impose considerable impact on this plot. Wijffels (2001) showed the global differences between the 13 products in her study are about 250mm/yr and regional differences are of 500 mm/yr. Given the large uncertainty in estimates of So(E-P)/h, are green areas statistically significant?

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