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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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We greatly appreciate the reviewer's careful reading of our manuscript and thoughtful comments. The paper is much improved as a result of his/her input.

"the idea of extending. . ." For the record, the Yu paper was published 1 week after ours was submitted. We did not know anything about it at the time our paper was submitted to Ocean Science. However, now that it has been published it is appropriate to compare our results with those of Yu.

There are a number of differences between our paper and Yu's. Though we look at a similar problem, we have used somewhat different methods. One difference is that Yu

just includes h-bar, i.e. there is no seasonal variation of mixed-layer depth. She does a more complete SLS budget than we do, something that we feel is not within the scope of our paper. We assess the statistical significance of the fits and she does not. We have added some discussion of these differences into the introduction.

"the authors spend a great deal of time. . ." We have calculated the advection and upwelling terms as suggested and include them in the revised version.

"the authors state in a number of places. . ." We have redone the calculation of the annual cycle of S0(E-P)/h as the reviewer suggests, with constant E and constant P, and also with constant h to test the dependence on mixed-layer depth (see reviewer 2). The maps with constant E and constant P look very much like those of (former) Figure 11 C-F. In other words, E and P are dominant in different areas of the ocean, which are pretty well separated. With constant h, the map looks like that of (former) Fig. 11a-b. This is important to note though and we put some text into the paper to state this.

We have removed (former) Figures 9 and 10. We felt that figure 8 was of interest in that it shows the meridional distribution of phase.

"the statement. . ." Looking at the distribution of (former) Fig. 13, the SLS amplitude has a large number of high outliers. Because of this, we felt that the median would be more representative of most ocean areas than the mean. What we are trying to do here is globalize the SLS seasonal cycle. Given the fact that the SLS only has a significant seasonal cycle in 37% of the ocean area, what is the global impact on the ocean's mass and sea level height? This is a rough calculation and we were looking for order of magnitude agreement with previous results, which we did find. Using the mean, which is 0.26, instead of the median gives a result of 0.1 instead of 0.06. We will mention that the result we get using the mean instead of the median in the text.

". . .it should be investigated. . ." The calculation the reviewer is requesting was done by Boyer and Levitus (2002) without regards to significance levels. Their maps look like the ones we have produced (Figs. 3-5) with the blank areas filled in with light

blue. In our calculations we have used a 95% significance level, which is a typical value used for such purposes. We think it adds important information to show where the seasonal cycle does not exist in a statistically significant sense. Doing the same calculation without any significance test, one gets a median (mean) amplitude of 0.10 (0.16) rather than 0.19 (0.26). The standard deviation in this case is 0.22.

"the authors need to clearly delineate. . ." The reviewer is absolutely correct. The terminology was varied mostly to keep the text from being too monotonous at a cost of imprecision. We have revised the text to be consistent in describing the significance of the fit.

"It is a little disturbing. . ." The 0.1 offset for the LEGOS bucket data was done following Delcroix et al. The reasons for this offset are discussed at length in this reference. The 0.1 is a constant offset and should not affect the calculated seasonal cycle. The LEGOS are largely bucket data which is the reason for the offset. The GOSUD data make up a very small fraction of the total dataset (Fig. 2). QC procedures for the GOSUD data are documented on the GOSUD website - as described in the paper. We believe that the data are of sufficient quality for use in this calculation and they are the highest quality available. We would note that in our previous paper we did an independent calculation using SLS from the TAO array, and found similar results.

"Hosoda et al. . ." The reviewer has a good point, in that the seasonal cycle of SLS may be changing due to a warming climate and intensifying hydrologic cycle. This could introduce some hemispheric bias because the northern hemisphere is much more populated with SLS measurements in the pre-Argo era than the southern. In order to investigate this, one must first determine if the seasonal cycle is in fact changing and if so where and by how much. Except in a few limited areas, determining interannual changes in the seasonal cycle is not possible with current data. It may be possible to investigate this question, for instance, in the areas heavily sampled by the LEGOS bucket data, i.e. in some parts of the tropical Pacific, or using a long-term tropical mooring. While this is a great set of questions to ponder and a potential topic

for future research, we feel it is a second order question that would require more sophisticated statistical techniques that go beyond the scope of this paper. We will add some discussion of the Hosoda et al paper to the conclusion to reflect this.

Interactive comment on Ocean Sci. Discuss., 8, 2377, 2011.