

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

**Anonymous Referee #1**

Received and published: 1 February 2012

Bingham et al. (2010), performed harmonic analysis of surface-layer salinity (SLS) to compare the annual cycle of salinity in 2.5-degree gridboxes with  $E-P$  ( $S_{sub0}(E-P)/h$ ) for the Pacific Ocean 40S to 60N. The present work extends the earlier study to the global ocean 60S to 60N.

The idea of extending the earlier work to the entire global ocean (excepting high latitudes) is a good one. The paper would be helped by a more rigorous examination of factors other than Evaporation (E) and Precipitation (P) in the annual cycle of SLS. It would also be helped by a more rigorous treatment of the relative impact of annual cycles of E and P on the SLS cycle. There have been a number of recent works looking at the global hydrological cycle through the lens of near-surface salinity, most notably 'A global relationship between the ocean water cycle and near-surface salinity' by Lisan

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Yu (JGR 116, 2011). This work uses a different technique, but essentially covers the same subject as the present paper. The authors need to emphasize where their work is different and unique from Yu's work. Alternatively, the authors need to show how, using a different technique, they either reaffirm or contradict Yu's results.

- the authors spend a great deal of time trying to explain why some areas of the ocean do not fit the simple model of 3-month lag between cycles of SLS and  $S_{sub0}(E-P)/h$ . The authors are forced into this lengthy explicative discourse because they follow a simplified model, expressed in Equation 1 where SLS is not dependent on advection, entrainment or mixing. In the earlier work on the Pacific (Bingham et al., 2010), advection and entrainment were found to be small over much of the Pacific Ocean. This same examination of the roles of advection and entrainment over the larger area covered in the present work should be done to replace much of the explanation with analysis. This would greatly strengthen the authors work.

- the authors state in a number of places in the paper that the amplitude of the annual salinity cycle is governed mainly by precipitation annual variability, since evaporation does not have a large annual cycle. It would be nice if the authors could verify and quantify this relationship. If the authors calculated  $S_{sub0}(E-P)/h$  amplitude with a constant E (annual mean) and variable P and compare with the same quantity using variable E, the authors could verify their conjecture and present a percentage of change accounted for by evaporation.

- Figures 8,9, and 10 do not add significantly to the paper.

- The statement 'The median value in areas that have statistically significant seasonal cycles of SLS is 0.19' is not, in and of itself very useful information. This value is used in a rough calculation giving a globalized mean value of 0.06 annual salinity cycle. This calculation does not make much sense. Why multiply the median global value by the percent of ocean from which that median was calculated? What is the significance of the 0.06 value? Why not simply calculate a mean value (and standard deviation) for all

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2.5 degree squares with statistically significant annual salinity cycles? If a median can be calculated, certainly a mean can be calculated. Further, it should be investigated whether the significance test used might be underestimating the significance of salinity annual cycles in regions where the amplitude of the cycle is close to zero.

- the authors need to clearly delineate between 'statistically significant', 'significant', and 'identifiable'. The authors use all 3 terms in the paper, I believe interchangeably. But each term is distinct. 'significant' by itself could mean change above a certain threshold of note, as for instance for comparison with Aquarius change would need to be above a 0.1 threshold. 'Statistically significant' is self explanatory given the description of the statistical test done. Identifiable is ambiguous.

- It is a little disturbing that part of the LEGOS dataset needs a systematic adjustment of 0.1, which is roughly the same magnitude as the annual cycle of salinity. How many of the LEGOS data are bucket vs. thermosalinograph? Some mention of the quality of the thermosalinograph data should be mentioned as compared to CTD salinity. Is there some way to independently verify the LEGOS data are suitable for the present study? Looking at the annual cycle with and without the LEGOS dataset where there are sufficient numbers of other data would give an idea of any differences in datasets. What about the GOSUD data. Are these of sufficient quality for the present study? In short, some more information on the relative quality of salinity measurements from the different instruments included in the present study should be added.

- Hosada et al (2009) describe some validation of the idea of intensification of the global water cycle using 5 years of Argo surface layer salinity data compared to long-term means. If, as they show, the SLS distribution in the Argo era is different from the long-term mean, there may be some hemispherical bias in the results of the present work. Salinity data pre-Argo were very sparse in the southern hemisphere, less-so in the northern hemisphere. So the analysis of the annual cycle in the southern hemisphere may be biased to the Argo era while being more representative of the long-term in the northern hemisphere. This should be examined.

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

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