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Ocean Science Discussions

Interactive comment on "Sea surface salinity variability from a simplified mixed layer model of the global ocean" *by* S. Michel et al.

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This paper presents results on an important study on the seasonal cycle, both on mixed layer depth diagnosed from SST variability and on sea surface salinity variability.

The paper is rather long, much too long to be easily readable and grasped in an easy way by the reader. The figures are potentially interesting, but very hard to read in their small format. There are very long discussions through out the paper, both on the results and on the the different assumptions made, but at the end, it is hard to figure out what are the important ones. Also a comment is made at the beginning of 4. to use criteria to select areas where the salinity budgets are relevant. However, these areas are not clearly selected on the following figures.

A deliberate choice was made to use a mixed layer depth in the model that is estimated

in a rather complicated, non-linear way from the observed SST variability and fluxes. It is really hard to figure out whether these estimates are reasonable or not (and I am more concerned with their variability, because of the assumption of what happens to SSS when there is entrainment, see below). This could have been tested a little further with sensitivity studies. For instance, one can fear that the errors on the heat fluxes (roughly to give a range on the order of 40 to 80 W/M2) would have in some situations huge impact on the estimated mixed layer depth (mld). It would be relevant to have a few time series of what these smooth climatological mld seasonal variability looks like in different areas (and not just the two March and September maps. Is the varaibility rather smooth, or does it present large sea-saws.

Then, in integrating the SSS equation to have the seasonal SSS fields, a fairly large assumption was made on the vertical advection (both for T and S). It is barely described, but acts as a strong negative feedback on actual variability towards the climatology in case of detrainment. Other assumptions could have been made, which would have a less drastic effect. Based on that, it is not surprising that the "winter hemisphere" SSS looks rather close to the observed one. The actual S which is entrained is taken from a climatological monthly profile at the actual depth of entrainment. This comes from a climatology which is known to have some serious month-to-month noise. What is the impact of that on the results. Again, this is a function of the amplitude of changes in MLD, and when entrainment actually occurs, and how often this happens.

I have also a general question on the strategy retained to keep in the average year some residual of the day-to-day variability (see table 1, if I understand it correctly). This introduces high frequencies which confuse the issues and might have resulted in some strong variability in MLD and from that in the frequency of entrainment. I feel that we need to have some guarantee that this is not a major issue. I am mentioning that, because the variability modeled appears rather large in some areas compared to what is expected (but Fig. 13 indicates small day-to-day increments; actually, is this figure really relevant?)

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On the other hand, I suspect that some of the results presented are likely to be reelvant and to stand the criticsms stated above.

Interactive comment on Ocean Sci. Discuss., 4, 41, 2007.