Ocean Sci. Discuss., 4, S13–S18, 2007
www.ocean-sci-discuss.net/4/S13/2007/

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.

Anonymous Referee #4

Received and published: 9 February 2007

Review of "Sea surface salinity variability from a simplified mixed layer model of the global ocean" by S. Michel, B. Chapron, J. Tournadre, and N Reul

The paper is actually two papers, one exploring a formulation of mixed layer depth in the slab model, the other investigating salinity variations in the mixed layer and their causes. I think both papers are worthy of publication, but they should be published separately. This would give the authors more time to explore further the investigation implied in the title, global mixed layer salinity variations and their causes. I think this is a very worthy goal and the model is well suited to looking at the global scale variability and attributing the relative imporance of different factors. I think this paper needs more extensive and careful analysis, and more references to work which is alluded to but not named. Once this is done, it will make a significant contribution to our understanding of mixed layer salinity. Below are elaborations on specific points in the paper.

I think the paper needs to be consistent as to the use of Sea surface salinity (SSS) vs. average mixed layer depth salinity. The authors state (p. 46) "The temperature T and salinity S of this layer are supposed to be equal to the SST and SSS, respectively. Alternatively, T and S can be considered as vertically averaged quantities over the layer thickness." They alternate back and forth between using SSS and mixed layer salinity as the quantity being investigated (starting with the title). The authors state (p 49) "In this model, T and S stand for vertically averaged quantities over the mixed layer, not for surface quantities". It is the mixed layer salinity rather than SSS which is being investigated. While this quantity can be compared to SSS, it is not SSS, and can be quite different in areas, such as the North Pacific, where salinity can be the defining factor in the depth of the mixed layer. This needs to be clear.

I think the formulation of mixed layer depth should be introduced and expanded upon in a separate paper. A good portion of the present paper is devoted to validating the mixed layer depth formulation. This distracts from the main purpose of the paper, discussion of the factors involved in mixed layer salinity evolution. It would be nice to have a more expansive treatment of the mixed layer depth formulation elsewhere. I am not convinced by the authors arguments for the need for the mixed layer depth formulation as opposed to using a monthly climatology. Stating that in situ mixed layer depth product is based on subjective criteria (p 54) or arbitrary criteria (p 77) does not do justice to in situ mixed layer climatologies. In the in situ calculations of deBoyer-Montegut et al., a definition of mixed layer is put forth, and then calculations performed. I would not term this arbitrary or subjective, even if the definition, of necessity has some subjectivity. Similarly the present papers presents a definition of mixed layer depth and then performs the calculation. The definition includes terms partial derivatitve of h with respects to input variable (dTh) and partial derivative of h with respects to time. I would need to be convinced that these parts of the definition are not also arrived at with some degree of subjectively. And is there an initial mixed layer depth? If so, how is this calculated? The authors state (p. 53) that SST data from satellites have high resolution and accuracy. It is hard to dispute the high spatial coverage, but accuracy is another matter. How accurate are satellite SST and how do SST errors affect the mixed layer depth formulation? Further, would the errors inherent in the present papers formulation of mixed layer depth be small enough to justify using the formulation? A change of 10 meters over the course of the model run is potentially a large error (or is it real?) in areas where the mixed layer depth is shallow. Finally, do the daily variations of the mixed layer depth (vs. error) justify the need for mixed layer depth calculation at less than the monthly resolution of the climatogies? All of these questions could probably be easily addressed in a separate paper.

There is no unit psu. The authors should note at the beginning of the paper that all values are on the Practical Salinity Scale and then leave the values unitless. I would urge the authors to go even further and state that all values are on the Practical Salinity Scale and multiplied by 10ˆ3 for all sections where this is relevant. The authors do this for some sections, but not others.

This would help the reader understand the magnitude of the equivalent annual salinity variation of -.38 (page 68). Is this an average annual variation? This is a huge variation. The authors state that the ECMWF reanalysis precipitation is in excess of evaporation over a given year. The authors could calculate how much of the salinity variation is due to this P-E imbalance. It is perfectly acceptable to have a freshening over the global ocean for a year. But I dont think the magnitude of the change is realistic. In 100 years the mixed layer would be pure freshwater if that annual rate of change continued. And adding the ice covered areas would reverse this fresheninng? What does this say about the model results? Are errors so large as to be unrealistic? I dont think so, but I need to be convinced of this.

This extends to daily variations. Are the errors in daily salinity calculations acceptable? The daily changes in salinity due to the different factors are very small. Are they significant compared to errors? This is not a trivial question, and I dont know how the authors can answer the question. But some answer beyond simple comparison with in situ climatologies is important. The authors mention in the perspectives section that daily salinity variations can be compared to PIRATA and TAO salinity measurements if the model is run with one years worth of forcing. But I think the comparison can and should be made with the run of the model already completed, if only to validate the magnitudes of the daily variations.

The authors state (p. 51) that the model domain "extends between 80N and 80S, because no altimetry data are available at higher latitudes." I have not yet encountered reliable altimetry data at latitudes higher than 66. I am not familiar with the SSALTO-DUACS data. Does it extend higher than 66N or 66S? If so how? A mention of this fact and a reference would be helpful. The authors also mention (p 50,84) that the geostrophic currents are calculated from altimeter data because of unprecedented resolution and accuracy. Is the accuracy of 1 cm good enough for these purposes? Again, this may be covered in a reference (Rio and Hernandez?). Make this clear, put the reference in, or mention the reference in the introduction as the source for information on altimeter data comparisons, accuracy evaluation, etc.

I see figures 9,10,11 as the heart of the paper, along with table 2. On page 60 the authors state that figures 6,7, and 8 together show those areas for which the model results do not meet stated criteria. However, it is hard to relate the information from these 3 different figures to the interpretation of figures 9,10,11. Either an additional figure showing all areas where the criteria are not met, or a masking of values in figures 9,10,11 in these areas would help with the interpretation of these figures.

Figure 11 shows diapycnal mixing as the major factor in salinity balance in the mixed layer for much of the North Pacific. Yet, as the authors state, significant convection in the North Pacific occurs only on the margin (Sea of Ohkotsk). So, how to explain the dominance of diapycnal mixing in the North Pacific? Could it have to do with the formulation of the mixed layer depth calculation neglecting the salinity barrier layer which can be important in this area?

The authors state (p. 71) that simulated variability is higher than in situ climatology variability because of the daily forcing used. But the simulated values in this comparison are monthly means, just like the in situ climatology. Even if there are large daily variations, the mean monthly value should average out to be similar to the in situ climatology. So, I dont think the difference in variability of the monthly means can be explained by the daily forcing.

The authors state (p67) "Moreover precipitations produce shallow mixed layers, while evaporation induces thicker mixed layers, thus the impact of P on SSS is generally stronger than the impact of E." This needs more explanation. Why does precipitation produce shallower mixed layers? Why do shallower mixed layers have a stronger impact on mixed layer salinity or SSS?

The authors state (p75) "The local maximum is due to the advection of salty water from the Bay of Bengal" when talking about the Gulf of Oman. The Gulf of Oman has an outlet to the Arabian Sea, not the Bay of Bengal.

The authors state (p. 43) that "only global climatologies can be generated, with a resolution limited to 1degree in space". In the references to this paper is a reference to Boyer et al. of a 1/4degree global climatology.

The authors state (p. 44) "assimilation methods generally produce shocks at the beginning of each cycle and no precise long-term budgets can be inferred from this kind of model." This needs a reference.

Grammar mistakes (mostly small, but worth correcting): p. 45 " - How does daily variations in salinity compare to its seasonal cycle?" Either needs to start "How do" or "How does daily variation"

p. 48 equation 4 needs cross product (X), not ˆ

p. 50 "The latter solution has been chose for our simulations, to take advantage of an accurate analysis of satellite altimetric data with high space and time resolution is now available" Either remove the "is" or replace with "which is"

p. 54 "AntArctic" should be "Antarctic" The reader will understand where AACC comes from.

p. 59 "can cumulate" should be "can accumulate"

p. 62 "associated to Westerlies" should be "associated with Westerlies"

p. 62 "Good Hope Cape" is usually referred to as "Cape of Good Hope"

p. 63 "vertical salinity gradient is particularly." This sentence needs to be finished.

p. 68 "Ekman pumping is affected with the smallest impact" should probably be "Ekman pumping has the smallest impact"

p. 68 "global amplitude of 8x0ˆ-3" should be "global amplitude of 8x10ˆ-3"

p. 69 "The salinity variability our mixed layer" might be "The salinity variability of our mixed layer"

p. 73 "The model enables to investigate" might be "The model enables us to investigate"

p. 74 "salinity stability is due low variabilty" should be "salinity stability is due to low variability"

p. 74 "exhibits a higher value (36.75 instead of 36.75 psu)" one of these 36.75 needs to be replaced with the correct value.

p 79. "the combination of these processes is confronted" might be "the combination of these processes is compared"

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

OSD

4, S13–S18, 2007

Interactive Comment