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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.

S. Michel et al.

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General comments

We would like to thank all 4 referees for their positively critical comments. This is very helpful to improve our article, of which we acknowledge many aspects require a careful revision.
We provide below detailed replies to each of the main comments expressed by the referees. The minor comments and technical corrections will be treated directly in the revised version of the article.

After replying to all the referees’ comments, we concluded that the main issues to be considered before submitting the final version of our work can be sorted in 5 categories:
1. Format of the paper:

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

- length: The text is too long a makes the reading difficult. We will focus on exposing clearly the assumptions behind in the model, presenting a short synthesis of the simulation results and drawing well-argued conclusions.
- lack of references: The previous studies on which we build many arguments will be quoted more systematically.
2. Model formulation and input data
- uncertainty of the input data: A list of the known uncertainties for each input variable will be provided, together with estimates of their impact in terms of MLD and SSS errors.
3. MLD estimate:
- inversion technique: The method we have developed to infer MLD from observed SST will be described more clearly and concisely, whilst explaining the possible implications for the applicability of the model.
- domain of validity: The areas where the MLD estimate is diagnosed to be unreliable will be excluded from the subsequent SSS budget and will not be discussed.
4. Mixed layer salinity balance: We will shorten the descriptions of the geographical patterns of each process. We will focus on general explanations in terms of the model physics.
5. Variability of the simulated SSS: We will explain the differences we expect in the SSS variability when comparing our simulation with a daily forcing to the monthly climatology.

We believe that after addressing each of these topics in the final version of the article, it will fulfill all the referees' requirements. Thus we will soon submit a revised version to be published in Ocean Dynamics.

Specific comments

1. Format of the paper

1.a. Length of the paper

Referee 1: *The paper is rather long, much too long to be easily readable and grasped in an easy way by the reader.*

Referee 2: *However, I agree with the comments of the first referee that the paper is rather long. In particular there is much discussion of qualitative comparisons, which although valuable, should not be over interpreted.*

Referee 4: *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 importance 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.*

We acknowledge that this paper is too long and should be reduced to highlight the main conclusions. The paper length is partly due to describing with too many details the geographical distribution of the results. Also, two different issues are addressed: first, the MLD estimate then the SSS budget. One of the referees suggests these two issues could be discussed in two separated paper. However, as the budget of salinity (and any other property) in the mixed layer is intrinsically dependent on the estimation of the layer depth, both issues should be examined on the same ground. Consequently, we will do our best to shorten the text by eliminating unnecessary descriptions and summarising the conclusions. We will also increase the number of references to previous works, which would provide this paper with a more critical view on the model assumptions and results.
Note that the model and the MLD inversion technique have already been addressed in a companion paper (cited on page 60, line 4), focusing on the surface heat bal-

ance in the North-East Atlantic. However this paper requires an extensive revision and has not been re-submitted yet. Some results from this work are described in another paragraph below. The draft of this paper can be downloaded from this FTP site: ftp://ftp.nbi.ac.uk/pub/general/smic/Thesis/article_OceanDynamics.pdf

1.b. Size of the figures

Referee 1: *The figures are potentially interesting, but very hard to read in their small format.*

We agree that some figures presenting global maps of the results are difficult to read, especially when there are 6 maps on the same page. The on-line version of the paper solves this problem, as the figures appear on a whole landscape page. For the final paper version, we will enlarge the problematic figures as much as possible, so that they cover the full page width.

1.c. Equation of salinity balance

Referee 2: *The description of the salinity balance at the start of section 4.1 does not apply to equation 2 as it is written (Eq. 2 has only 4 terms) but seems to apply to equation 2 when equations 3 and 4 are substituted.*

A step was skipped in the development converting the SSS evolution equation (Eq. 2) into the salinity balance equation. This equation will be added in the final version of the manuscript to help the reader in understanding the 6 processes contributing to the salinity budget.
In Eq. 2, the horizontal advection can be separated in 2 terms from Eq. 3, namely Ekman transport and geostrophic advection. Similarly, the vertical entrainment term can be separated using Eq. 4 in Ekman vertical advection and diapycnal mixing. Thus Eq. 2 can be written:
$dS/dt = (E-P-R)S/h - U_E.\nabla S/h - u_g.\nabla S - \Gamma(w_e)w_E(S-S_d)/h - \Gamma(w_e)(dh/dt)(S-S_d)/h + \kappa\nabla^2 S$

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This equation describes the mixed layer salinity balance in the model, which consists of 6 processes: surface freshwater flux, Ekman transport, geostrophic advection, Ekman vertical advection, diapycnal mixing and horizontal diffusion.
Note that the salt budget at the sea surface (in $kg.s^{-1}$) can be obtained from this equation (in $psu.s^{-1}$ or $kg.m^{-3}.s^{-1}$), simply by multiplying all the terms by the layer depth (*h*) and the surface of a grid cell (*dx dy*):
$F_S = dS/dthdxdy$
where *dS/dt* is the mixed layer salinity balance obtained from Eq. 2.

1.d. SSS balance and salt budget

Referee 2: *Page 68: The global SSS balance seems very sensitive to what happens in the poorly represented, ice covered regions and I am unclear what point the second paragraph on this page is making.*

We agree the second paragraph on page 68 in not clearly enough formulated. As the referee is pointing, this paragraph deals with the model sensitivity in seasonally ice covered areas. In these regions, the model results are less reliable than in any other regions, due to the lower accuracy of the input data and the lack of a representation of sea ice impact. Remarkably, the global salinity budget is negative when integrated over the ice-free ocean ($-1 \times 10^{-3} psu.day^{-1}$), but positive when the ice-covered areas are included. The global salt budget (F_S expressed in the previous paragraph) may be a more relevant diagnostic. As this quantity depends on the MLD as well as on the SSS evolution, its sign can be different from the sign of the salinity balance. That's why we can obtain a positive salt budget over the ice-free ocean, while the salinity balance is negative. This implies that salinity tends to increase in regions with a large *h* and decrease in regions with a lower *h*.

1.e. Units for salinity and salinity variation

Referee 4: *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*

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would urge the authors to go even further and state that all values are on the Practical Salinity Scale and multiplied by 103 for all sections where this is relevant. The authors do this for some sections, but not others.

We do not want to enter the debate on using PSU, PSS or no unit at all. We decided to use PSU in this article, as this unit is used in most of the studies by European authors. We believe each of this convention is equally valuable, as long as it is kept from the beginning to the end of an article to ensure consistency.
We agree that the unit used in the discussion on the salinity budget (10^{-3} psu.day^{-1}) makes the reading uneasy, as it has to be repeated in almost every phrases. We already suppressed it in Sect 4.3 and we will do the same in Sect 4.2.

1.f. Discontinuity in model using assimilation

Referee 4: *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.*

We agree a reference is missing when addressing the problems implied by data assimilation for estimating surface budgets. We would like to cite Giordani, Caniaux And Prieur, 2005a:
“The majority of models use sequential assimilation systems, which are efficient in reducing drift but induce mass-circulation imbalances at each reinitialization procedure. Such imbalances produce shocks and the radiation of spurious gravity waves that are inappropriate for estimating physical diagnoses and time-integrated mixed layer budgets (Gavart 1996; Gavart and De Mey 1997).”

2. Formulation of the model

2.a. Clarity of the assumptions

Referee 1: *There are very long discussions through out the paper, both on the results and on the different assumptions made, but at the end, it is hard to figure out what are*

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the important ones.

Some long discussions on the results will be suppressed, or at least summarised, in the final version of the paper. We will also try and make the assumptions clearer in Sect. 2.2, where they are described along 2 pages. We believe the most important hypotheses are:
- T, S properties are vertically homogenous within the “slab” ML
- the totality of the surface heat flux is absorbed within the ML
- the totality of the Ekman transport is contained in the ML
- the surface geostrophic velocities inferred from the Absolute Dynamic Topography represent accurately the vertically averaged current in the ML
- lateral diffusion is not dominant on these large scales (resolution 100 km, 1 day) and can be satisfactorily parameterised using a simple Laplacian operator with a constant diffusion coefficient (κ)
- T, S below the ML vary on much longer time-scales, thus they can be approximated from the monthly climatology
- vertical entrainment can be diagnosed from its effect on SST, using our inversion method which provides the MLD estimate

2.b. Surface properties vs mixed layer properties

Referee 4: *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.*

We are aware that the link between sea surface properties and the model properties is confusing. This should be explained more carefully, as both SST and SSS can be significantly different from the quantities averaged over the mixed layer thickness.
However, we would like to make some remarks about the referee's comment of this issue:
1) On page 46, the concept of the slab mixed layer model is explained. The mixed layer is treated as a slab of water, thus its characteristics are strictly constant from the surface ($z = 0$) to its base ($z = h$). In particular, the mixed layer salinity (MLS) is equal to the SSS and the mixed layer temperature (MLT) is equal to the SST. In reality, the quantities which control the processes intensity (such as S in the air-sea freshwater flux $(E - P - R)S/h$) are the properties averaged over the mixed layer thickness. On page 49, the simplifications behind the model formulation are described and their possible consequences are investigated. To validate the model results, the simulated temperature and salinity must be compared to some observations. Estimating the MLS and MLT from in situ data is dubious, as we can not decide on the "real" MLD. Should it be the MLD from the model, which is intrinsically related to its formulation? Or an MLD from in situ data, which depends on the choice of a criterion for its definition? As we cannot answer this question, we believe it is more reasonable to compare the model temperature and salinity to observed SSS and SST.
2) In addition, we believe that there is no such thing as an observed SSS or SST. In the case of SST, the measurement depth ranges between a few micrometers for microwaves remote sensing, to a few meters for in situ sampling. In the case of SSS, the in situ measurements are performed between 1 and 10 meters deep. Thus they can not be strictly considered as surface properties.
3) In general, using the mixed layer averages or the surface properties does not make a large difference. If the mixed layer is totally "well mixed", this should even make no difference at all. The example given by the referee does not appear to be relevant: in case of a stratification controlled by salinity, the MLD estimated from the model is

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the depth of the salinity change, not the depth of the temperature change as it is suggested. The model MLD represents the penetration of the surface flux, which is fixed by the density gradient. Only in case of a density-compensated layer, the simulated MLD is expected to be higher than the depths of temperature and salinity changes.

2.c. Altimetry input data

Referee 4: *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.*

The geostrophic current data which are used in the simulation are obtained from the Mean Absolute Dynamic Topography of the SSALTO-DUACS analysis. This dataset combines Sea Level Anomalies measured by 5 satellite altimeters (TOPEX/Poseidon, Jason, ERS-2, GFO and ENVISAT) and the Mean Sea Surface Height from the GOCE gravimeter. T/P and Jason share the same orbit with an inclination of 66 degrees. Thus their observation is indeed limited to latitudes ranging from 66S to 66N. GFO has an inclination of 108 degrees, allowing SLA measurement at latitudes as high as 82N/S. ERS-2 and ENVISAT both have a near-polar orbit with a 98.5 degrees inclination, thus their coverage is almost global. Further details can be read on the AVISO website:
http://www.jason.oceanobs.com/html/donnees/duacs/welcome_uk.html
The accuracy of each individual altimeter is estimated to be around 2-3 cm, but the collocated product is accurate to 1 cm. Moreover, as we use a climatology computed from 4 years of data, the climatological currents probably benefit from a SSH accuracy

better than 1 cm, at least in weakly turbulent areas.
As a reference for the accuracy of the SSALTO-DUACS data, we cite Rio and Hernandez, 2004, on page 83. We acknowledge this reference should be also cited on page 50.
The impact of this error on the simulated SSS can be roughly estimated using orders of magnitude. From Eq. 3, the current error associated with a 1 cm SSH error over a distance of grid cell ($\sim$ 100 km) is:
$\delta u = g/fd(\eta)/dx \sim 1.3cm.s^{-1}$
Then from Eq. 2 the impact on SSS evolution, with a 1 psu horizontal gradient over a grid cell, would be :
$\delta(dS/dt) = \delta udS/dx \sim 1.3x10^{-5}psu.s^{-1} = 1.2psu.day^{-1}$
This is a considerable error in terms of salinity. But this value is probably a high estimate of the error and would decrease linearly with the current velocity error and the salinity gradient. Thus we believe the mean error on SSS evolution is at least one order of magnitude lower (0.1 psu.day-1), which seems acceptable. Indeed, the recommendation from the GODAE program (and the aim of the SMOS mission) is an error 0.1 psu with a 100 km resolution and a frequency of 10 days.

2.d. Validation of the current data

Referee 5: *But one of the processes affecting the most the variability of SSS is advection. There is no validation at all about the quality of the velocity fields used in this study. Without validation of the currents, the results lack of robustness.*

We agree on the point that using validated velocity data is a key requirement. Before interpreting the results from any model which needs input of current velocity, one has to make sure these data are in agreement with in situ observations and GCMs.
We have conducted a qualitative comparison of our current estimates against velocities measured at TAO and PIRATA buoy arrays, as well as circulation patterns from the OPA 3D model. However, we think an accurate validation of the current velocities in the mixed layer, over the global ocean and during a seasonal cycle, would require a

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paper on its own. Several referees argue this paper is already too long and we believe such a validation is beyond the scope of this study.

Moreover, the data sources used in our simulation have already been exploited by a large scientific community. Concerning Ekman currents, there is no doubt the ECMWF model provides one of the most reliable wind stress dataset. In particular, the ERA40 reanalysis is used to force a wide range of ocean models. The formulation of Ekman velocity from wind stress is universally accepted. The only unknown variable is this formulation is the MLD *h*, as expressed from Eq. 3:

$u = u_E + u_g$

$u_E = 1/(\rho f h)\tau \times k$

Thus we have to validate our MLD estimate, which has never been used elsewhere, but not the wind stress data from ERA40.

For what concerns geostrophic currents, the data we are using are certainly less commonly used as forcing data for an ocean model. However, the SSALTO-DUACS data have already been used in numerous studies of the ocean circulation. Thus we think we can rely on previous studies to investigate their accuracy. In this respect, several validation reports are available on the AVISO website:

http://www.jason.oceanobs.com/html/calval/welcome_uk.html

As quoted in one of the replies to referee 4, the error on SSH in this analysis is considered to be less than 1 cm. The corresponding error on current velocity is of order of 1 cm.s$_{-1}$, at the resolution of our model. We have argued this is an acceptable error, at least for our aim of quantifying the global surface salinity budget.

Thus the main question regarding these geostrophic velocities data would be: do they represent the surface current or the circulation averaged over the mixed layer? As the definition of the mixed layer is variable, this question can not be answered directly. Estimating the temperature or salinity budget in the mixed layer is a way of tackling this issue, as we intend to do in this article.

2.e. Inclusion of equatorial regions

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Referee 5: *On the other hand, the equatorial regions are not considered here. A global study should really consider these regions. The authors recognize the possibility of extending their work to these regions (see for example Durand and Lagerloef, JPO, 2002). Including the equatorial region will clearly differentiate this work from the previous works that have used the same model as, for example Mignot and Frankignoul (2003).*

We acknowledge equatorial regions surely play a significant role in the global budgets of heat and salt. Their removal is only due to a technical reason, as we did not know how to compute Ekman currents in the equatorial band when we performed the simulation on which we based our study. Thus we simply ignored the 3S-3N latitude band, just as in the 3D simulations analysed by Mignot and Frankignoul (2003, 2004). Please note that these authors used the slab mixed layer formulation with a constant MLD (50 m) over their domain (the North Atlantic). Our MLD estimate is variable both in space and time, which already differentiates significantly our work from theirs.
In the discussion of our article (page 80, line 5), we note that Ekman currents can be estimated at low latitudes by using a method proposed by Lagerloef et al. (1999). From a β-plane approximation, Ekman velocity can be computed with a complex friction coefficient:
$(ifh + r)u_E = (\tau_x + i\tau_y)/\rho$
This method has been subsequently implemented in our model, but not in the simulation described in this paper. Indeed, we needed more time for the validation of resulting currents in equatorial regions. At first sight, the total current (geostrophic + Ekman) seems to be in reasonable agreement with observations from the TAO array in the tropical Pacific.
Therefore, the equatorial regions will certainly be included in the next version of the model. The results from an improved simulation will be presented and analysed in a future article.
Similarly, polar regions have not been accounted for properly, as the model lacks of an estimate of the freshwater flux induced by sea-ice. When a reliable estimate for this

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flux will be available, we will include it in the model to infer a salinity balance over a fully global ocean.

2.f. Representation of light penetration

Referee 5: *Moreover, when applying the model on the equatorial region, the model has to take into account the penetration of light, as the equator is one of the regions where the correct heat balance requires the explicit consideration of the attenuation of solar radiation (Murtugudde et al., JCLI, 2002).*

Taking into account the penetration of the solar heat flux is important in the shallowest mixed layers. The smallest MLDs are found mainly in equatorial regions, so it could make a significant difference if these regions were fully included in the simulation (i.e. after extending the Ekman advection term). The light penetration model by Paulson and Simpson (1977) is one of the most commonly used. It is based on a double exponential formulation of the irradiance *I*:

$$I(z) = Rexp(-z/D_1) + (1 - R)exp(-z/D_2)$$

The coefficient R = 0.74 and the 2 penetration depths are set to D_1 = 1.3 m and $D_2 \sim$ 20 m. Thus *I* is significantly lower than 1 only if the MLD is shallower than 20 m. This is rarely the case in our simulation, except at near-equatorial latitude (Figs. 3, 4). Therefore, we can assume that almost all the solar radiation is absorbed within the mixed layer, as stated in the model description (page 47, line 4) and in the appendix about forcing data (page 82, line 21).
Adding a parameterisation of light penetration to the model will be proposed within the perspectives of the final article. This model development will contribute to a better representation of the equatorial ocean, together with the improved formulation of Ekman currents.

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