**Ocean Science
Discussions**

Interactive comment on "Mixed layer mesoscales: a parameterization for OGCMs" *by* V. M. Canuto et al.

Anonymous Referee #1

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This paper proposes a parameterization for eddy fluxes of tracers by mesoscale eddies in the surface mixed layer. It is not publishable in its present form. I have two main criticisms:

Problem 1: The model is an adaptation to the mixed layer of the parameterization of Canuto and Dubovikov (CD05 and CD06, Ocean Modelling). I do not understand the link with these previous papers. In CD05 and CD06, the authors find a formula for the surface eddy kinetic energy Ks (called "Kt" in CD06, Eq. 6e). Why is a new model needed for Ks? Was the previous model wrong? It is not possible for authors to publish a paper building upon their previously published results without actually addressing why the previous model was inadequate (in contradiction with their earlier claims).

Problem 2: The comparison of the theoretical model with the numerical simulations

(section 4) is not convincing, and not up to the standard for a publication in Ocean Sciences. The authors do not indicate how many idealized simulations have been performed, how is a surface mixed layer developed and maintained, nor which simulation is used for the profile in Fig. 4. Regarding the "realistic" model, 70 simulations have been performed but the parameters are not documented. Only 8 simulations appear in table 1, why are they chosen? Simulations 2N and 5R seem to have almost no difference in their parameters, what differs between the two? Same for Fig 6: three simulations are chosen, among them one is not in the table (6R): why those three simulations? Why again choose arbitrarily three other simulations for Fig 5? How can the authors conclude from Fig 5 that "the agreement is satisfactory"? Fig 5 shows a scatter of points, to which a linear fit is difficult to justify statistically.

In order to review this manuscript I had to read the previous papers (CD05 and CD06), and this leads me to question the authors' strategy for publishing their research. At the end of their introduction CD06 say "The model results will be tested against a large set of data to assess their reliability before being used in OGCMs". The CD06 model has been around four years now, has it been used in OGCMs? It seems to me that there would be two logical steps before starting to complicate the picture by adding a different parameterization in the surface mixed layer.

Step 1: validate the eddy fluxes and eddy energies computed by CD06 against eddy resolving model simulations. Unless I am mistaken, this work has not been done (it is not referenced in the present manuscript). Why not use the large number of eddy resolving simulations presented here to validate (or invalidate) CD06, to show whether it works in the interior and/or in the surface mixed layer? If it works well in the interior but does not work in the ML, this would motivate the present manuscript.

Step 2: use CD06 in a coarse resolution model to see if the predicted evolution of the large scale variables agrees with the eddy resolving model. For example, in the case of the idealized experiments, the eddies presumably reduce the initial horizontal buoyancy gradient. Does the parameterization reduce it at the same rate? (This kind

of comparison was made for the GM parameterization by Gille and Davis, 1999, for example). Or else, why not repeat the 70 "realistic" experiments with a coarse resolution model using CD06?

The authors claim that their parameterization is superior to other published parameterizations because it is not "heuristic" and contains no adjustable parameter. However, in order to achieve that, the authors make very strong assumptions that are probably not valid: they assume that an analysis of the equations in Fourier space is adequate, despite the large inhomogeneity and anisotropy of mesoscale fluxes in the ocean (think of the tendency for eddies to organize themselves into zonal jets, with meridional scales that are not large compared with the eddy scale, for example). They assume that the eddy kinetic energy spectrum peaks at the scale of the first Rossby radius. In CD06 they make other strong hypothesis about the vertical structure of the kinetic energy. Between a heuristic model making minimal assumptions about the physics of mesoscale eddies, and a non-heuristic model with very strong underlying hypothesis, it is not obvious which is best for the purpose of running and OGCM. Therefore, after the author's models are validated with data and eddy-resolving models convincingly (which is not the case at present, to my knowledge), it will be interesting to see whether their model does better than parameterizations currently used in OGCMs (the GM parameterization and its variants). These parameterizations have been around for years and seem quite robust (they don't make numerical model solutions blow up, but rather they have a stabilizing effect). In order to convince OGCM users to adopt another (more complex, more costly and probably less robust) parameterization, the authors will need to demonstrate its superiority.

In conclusion, I do not think that the authors should revise the present manuscript, but rather they should use the strategy they propose here (comparison with satellite data and eddy resolving models) to validate or invalidate CD06 in comparison with heuristic models, before proposing a variant of CD06 in the surface mixed layer.

Detailed remarks

Abstract: "the present model renders the tapering schemes unnecessary": the same thing was said already in the abstract of CD06: "... thus avoiding the need for "tapering schemes" employed thus far to amend the failure of models for u_m to satisfy the proper boundary conditions". Why is it necessary to modify the CD06 model and to write a new paper, if CD06 already made the tapering unnecessary?

Section 1: Introduction.

line 19: The classification proposed, "two heuristic models and one non heuristic", is misleading. THL did not really propose a new parameterization ready to use, but advocated building a parameterization based on conservation of potential vorticity, pointing out the difficulties involved in doing so. There are a number of other interesting discussions on parameterizations (like Greatbach 1998, Greatbach and Li 2000, who explore the possibility of entering the parameterization in the momentum equations rather than the tracer equations). A discussion of the different "heuristic" forms proposed for the GM coefficient based on scaling arguments (starting with Visbeck et al 1997 and Treguier et al 1997) would be needed for completeness.

page 876 and 877: the discussion is puzzling. Why is CD06 not mentioned, since this model claimed to satisfy Eq. (1a) and therefore remove the need for ad hoc tapering functions?

Page 877: The discussion line 13-18 is in complete contradiction with section 2.8. The use of one parameterization below the mixed layer and another inside it means that in practice, either (option A) the modeller introduces in the code a statement "if this grid point is in the mixed layer, add parameterization P1; else add parameterization P2"; or better (option B), in order to avoid a strong discontinuity from one grid point to the next, the modeller adds to the equation at every grid point a weighted sum of the two parameterizations P1 and P2: (f(z,h)*P1 + (1-f(z,h)*P2) where f(z,h) is function of the depth z and the mixed layer depth h, equal to one in the mixed layer, zero below, and smoothly going from one to zero in between - in other words, an "arbitrary

tapering function"! The discussion in lines 13-18 demonstrates that in order to use the parameterization proposed here, the user needs a tapering function. Indeed, the authors propose an exponential tapering function in section 2.8: why is it less "arbitrary" than the one of Large et al, 1997?

Page 878: the work about the submesoscales, mentioned somehow "in passing", distracts the reader from the main point of the paper and renders the paper more difficult to understand (it makes it weaker rather than stronger). Indeed, the complex structure of the paper presented page 879 is enough to discourage the reader.

Section 2: Dynamical model.

2.1 Mesoscale tracer equation. Page 880, line 20: the hypothesis that the mesoscale energy spectrum peaks at the Rossby radius scale is not verified (because of the energy cascade towards larger scales, eddies tend to be larger in size than the Rossby Radius, especially in very energetic regions).

2.2 Mesoscale tracer field. How can Eq. (3c) be valid in the mixed layer, since by definition (first line of page 880) there is no vertical tracer gradients, and therefore N (Vaissala frequency) is zero and L (isopycnal slope) is infinite?

2.5 Mesoscale kinetic energy in terms of the large scale field. In CD06, the authors claimed to derive an equation of the surface kinetic energy (their equation 6e). Is the CD06 equation wrong? If not, how is it related to the new equation (10c)? Note that the singularities of Eq. 10c (division by N and f, which are zero in the mixed layer and at the equator, respectively) are not addressed.

2.6 Filtering large scale fields in coarse resolution OGCMs. The authors make a remark on practical implementation of their parameterization in models which is out of place between paragraphs 2.5 and 2.7. Has the filtering proved necessary when the parameterization has been tested, or is it just an idea? If is it just an idea, it is too early to mention it in a paper before it has been demonstrated to be useful. I do not think

that currently used parameterizations in climate models require such filtering.

2.7: effect of submesoscales on mesoscales. See the remark about page 878 (introduction).

2.8: Matching mixed layer with the interior. This proposition for a tapering function is in complete contradiction with the claim made in the abstract and the introduction. How does this new suggestion for the characteristic length scale of the transition layer compare with the proposition of Ferrari et al., 2008? Why consider a scale based on the vertical length scale of the flow, rather than a scale based on the vertical gradient of buoyancy or tracers?

Section 3: Assessment of the mesoscale model

3.2 surface KE vs Topex/Poseidon data (note that Scharffenberg and Stammer use Jason as well as Topex, so it would be better to say "altimeter data" rather than mentioning one satellite).

This comparison is interesting but would need to be explained and validated quantitatively in order to be publishable. Firstly, why is it necessary to use a numerical model to calculate Eq. 10c (Fig 1b)? Estimates of mixed layer depth, Rossby radius and surface velocities exist from data. If the purpose is to validate Eq.10c, a calculation from data would be more convincing than from the NCAR OGCM. The authors need to explain precisely how they calculate 10c using the model results: apparently they use 10-day averages of model fields and then average Ks over 3 years (there is a reference page 893 line 2 to a nonexistent section 7). How is the mixed layer depth calculated, how is the vertical gradient of the isopycnal slope calculated? How is the calculation performed at the equator (there is a division by f in Eq. 10c)? Is the result sensitive to these details?

Secondly, a discussion of the physical mechanisms underlying 10c is necessary and should appear either in section 2.5, or in 3.2. Eq.10c makes a number of predictions

that must be verified or falsified. For example, it predicts that the surface eddy kinetic energy is proportional to the mixed layer depth. The ML depth has a large seasonal cycle (it is much deeper in winter). The seasonal cycle of the eddy kinetic energy from altimetry is documented, seasonal data are available. Is Ks proportional to h? Are there enough data to distinguish between a sensitivity of Ks to h (larger in winter) and a sensitivity of Ks to high frequency winds (also stronger in winter?) The authors need to discuss this. Another feature of Eq 10c is that it predicts a large increase of Ks as one gets close to the equator (with a term proportional to the Rossby radius squared, and another inversely proportional to the Coriolis frequency). How can Ks be so low in the Gulf of Guinea in Fig 1b? is it an error of the calculation, or an error of the NCAR model (say, by having vanishing mixed layer depths there?) In reality the mixed layer depth goes from about 20m in the Gulf of Guinea to larger values (say 60m) in the western equatorial Atlantic, but the satellite observations (Fig 1a) do not show a large west-to-east gradient of Ks, while the model (Fig 1b) has a huge gradient of the estimated Ks. Why is there a lower East-West gradient of Ks in the tropical Pacific, where there is also a longitudinal gradient of the mixed layer depth? Why does the model completely miss the western intensification Ks along Australia, while it gets it in the Kuroshio and the Gulf Stream? This may be an artefact due to the lack of western boundary current in the NCAR model (hence too low horizontal density gradients). Why is there so much eddy energy all along the Antarctic circumpolar current in Fig 1b (much more than observed)? All this points out the necessity to calculate Eq. 10c from observations in order to be able to conclude anything at all. In short, does this comparison tell us something more about eddies than "the eddy kinetic energy is large where there are large horizontal gradients of surface density"? Wouldn't a simple map of the product of the Rossby radius squared and the horizontal buoyancy gradient produce a better fit? The authors need to establish that their model does better than a "heuristic" estimate of Ks. They also need to prove that the new model is better than CD06. This is not obvious at all. For example, the main large scale patterns of Ks appearing in Fig 1a are also seen at depth (from drifting floats, for example) and therefore could be better

represented by the formula in CD06, which does not rely on the mixed layer depth and is based on a depth-integrated measure of the baroclinicity of the large scale flow (as suggested by Killworth, or by Treguier et al, 1997).

Section 4: Assessment with eddy resolving simulations

4.1 Numerical experiments. Why repeat here the discussion about filtering out inertial waves (2.6)? In practice, either the correlations (say, product of velocity and temperature, vT) are calculated "online" at each time step, either they are calculated "offline" from a model archive of values averaged over a time interval (perhaps 10 days here?)

4.1.2 Idealized flows, baroclinic instability only. It would be crucial to understand why there is a mixed layer in these numerical experiments, and how it is maintained. The initial buoyancy profile is exponential: is it modified near the surface to introduce a mixed layer? The vertical mixing differs in the ML and below: what is the criterion to determine the ML depth? Is the horizontal gradient fo buoyancy independent of depth? The authors do not say how many experiments have been run, how the Rossby radius has been varied (changing f or changing the density profile?). Why was it important to use basins of different sizes? The size of the basin does not enter Eq 10c. Rather, for a verification of the model, it would be important to vary the depth of the mixed layer and the Coriolis frequency. Is the buoyancy gradient zonal or meridional? Is the beta effect taken into account? It would be interesting to show how these experiments compare with those of Lapeyre et al (2006).

4.1.3 Realistic flows. What are the 70 experiments exactly? Is it the right strategy to vary the wind and heat fluxes intensities (this strategy seems more appropriate for idealized experiments than for realistic ones)? Couldn't the authors also perform a test in another season, to have a very different ML depth? What is the "coarse grid" mentioned page 897, line 11?

The following text of section 4: The model/data comparison is completely unconvincing (see remarks at the beginning of this review).

Section 6, Conclusions.

This section is extremely weak: aren't there any perspectives for this work? The conclusions are not supported by the paper: for example section 2.8 shows that a tapering function is necessary. Conclusion a) was already claimed by CD06. Regarding conclusion c), Figure 1 is not enough to convince the reader.

Additional references for this review:

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