Mixed layer Mesoscales: a parameterization for OGCMs

V.M.Canuto et al. OSD , 7, 873, 2010

REPLY TO REFEREE # 1

V.M. Canuto[1,2] , M.S.Dubovikov[1,3]

[1]NASA, Goddard Institute for Space Studies, 2880 Broadway, New York, NY, 10025

[2]Dept. Applied of Phys. and Math., Columbia University, New York, NY, 10027

[3]Center for Climate Systems Research, Columbia Univ., New York, NY, 10025

The primary goal of this Reply is to prove that the key arguments of Referee # 1 (R1) are rooted in an unfortunate misinterpretation of our past work. This invalidates his key conclusions.

R1 Problem 1

• R1. *The present mesoscale model in the ML is an adaptation of CD6 to the surface mixed layer* (ML). *I don't understand the link with the previous papers.*

Answers:

A) There is no "link" because there cannot be one since the deep ocean is **adiabatic** (CD6 model) while the ML is **diabatic** (present model). These are fundamentally different physical regimes (with positive and negative turbulent viscosities), described by different dynamical equations whose solutions are different.

B) Concerning "**adaptation**", what follows is the unphysical result that would ensue if we had "adapted" the CD6 results, as R1 erroneously says we did. In the adiabatic ocean treated in CD6, flows occur almost entirely along isopycnals: a zero diapycnal flux implies that the vertical flux F_v is given by $F_v = \mathbf{F}_H \cdot \mathbf{s}$, where **s** is the slope of the isopycnals and $\mathbf{F}_H$ is the horizontal flux. Since F_v is the source of the eddy kinetic energy K, one can estimate the latter under the "adaptation" procedure, specifically, we use relations representing the deep adiabatic ocean and "adapt" the slope of the isopycnals to be consistent with the expected values in the ML, say, 10-100 times larger than in the deep ocean. The resulting K=O(10^4-10^5)cm^2s^{-2} contradicts the altimeter data shown in Fig.A[1]. **(**On the other hand, the model we present in this work is in accord with the altimeter data, see Fig.B).

It must be further stressed that Killworth (K5), recognizing the physical difference between deep ocean flows along isopycnals and ML flows along horizontal surfaces, did not employ an "adaptation" procedure either. Quite the opposite, he pointed out that in the ML the most physically appropriate representation is no longer the one used in the deep ocean, but one in cartesian coordinates**.** The difference between ML and deep ocean resulted in different dynamic equations. We have extended K5's linear case to include non-linearities which have a different representation in the ML and the deep ocean.

• R1. *The authors find a formula for surface eddy kinetic energy K_s . Why a new model is needed for K_s?*

Answers.

a) It is needed because the CD6 expression for the surface kinetic energy (called K_t) refers to a fully adiabatic ocean without a ML, while the new one, called K_s, corresponds to a realistic ocean with a ML. That is why they are different**,**

b) R1 suggested an expression for K_s that we have computed and mapped. The results presented in Fig.A show values one thousand times larger than the altimeter data and a geographic distribution unrelated to the data,

c) the maps in Fig.B show intensities and geographic distribution of K_s (our model) in much better accord with the altimeter data,

d) R1's expression for K_s does not contain the Ekman velocity in contrast with results from eddy resolving simulations whereby "a large component of the eddy kinetic energy is due to the Ekman layer response to the winds" (Maltrud et al., 1998, sec.5). Our expression for K_s contains the mean velocity which can be Ekman and/or geostrophic depending on depth,

e) since the Ekman velocity responds directly to the wind stresses which are expected to be larger in future climates, a mesoscale model without an Ekman flow lacks an important ingredient to study future climates. Finally,

f) R1 erroneously questions the basis of the z-profile of the eddy kinetic energy. We invite him to go through the derivation presented in detail in CD6. The results in Fig.C (presented here for the first time) do not require many comments (see Eqs. 1d-f below).

---

[1] Using the relations $\mathbf{F}_H = -\kappa_M \nabla_H \bar{b} = \kappa_M N^2 \mathbf{s}$, $\kappa_M = r_d K^{1/2}$, $K = r_d^{2/3} P^{2/3}$, where the power P can be taken as the ML averaged vertical flux, one has that $K = r_d^2 N^2 s^2$. With r_d=30km, $N^2 = 10^{-6} s^{-2}$, $s = 10^{-1} - 10^{-2}$, one obtains $K = O(10^4\text{-}10^5)\,cm^2 s^{-2}$.

• *R1. It is not possible to publish a paper building upon their previous published results without actually addressing why previous model was inadequate.*

Answer. This present work **is not built** on our previous work CD6.

As far as previous work, it is perfectly adequate for several reasons among which: **first**, the CD6 expression for the bolus velocity $\mathbf{u}^{+}$ contains both the GM and the THL (Treguier et al., 1997) models as special cases (Addendum, Eqs.5), **second,** the CD6 expression for the mesoscale diffusivity is in accord with Zhurbas and Oh (2003, 2004) and Oh et al. (2000) data from Topex/Poseidon and satellite-tracked surface drifters, **third,** the kinetic energy z-profile shown in Fig.C is more than acceptable and **fourth**, the new $\mathbf{u}^{+}$ naturally satisfies the necessary boundary conditions without any tapering functions nor assumptions about the mesoscale diffusivity. Thus, R1's assumption that CD6 model was *inadequate* is factually incorrect. It then follows that R1's conclusion is at best, a non-sequitur.

R1 Problem 2. Eddy Resolving Simulations, ERS.

a) General considerations

• R1 states that we *should carry out an eddy resolving simulation of the CD6 model before we study the ML.*

Answers. R1'assertion is based on the erroneous assumption that the present work in the ML is an adaptation of CD6. However, since such a contention has just been shown to be incorrect, carrying out an ERS for the deep ocean model CD6 would be irrelevant to this work that deals with a totally different mesoscale dynamics. By irrelevant we mean that it would not prove or disprove the new ML mesoscale model. **However, since we believed that ERS *targeted specifically to ML* were necessary and important, we did so and most of the figures refer to the ERS results.**

• R1 claims that we presented an insufficient number of numerical simulations to validate our mesoscale parameterization.

Answer. R1 can arbitrarily assert that any number of simulations is insufficient. However, the objective fact is that in our work both the number of eddy resolving simulations and the number of results considerably exceeds the number of analogous results in other published works including those R1 refers to.

• R1 claims that we "*do not indicate how many idealized simulations have been performed, how is a surface mixed layer developed and maintained, or which simulation is used for the profile in Fig.4*".

Answers.

A) In section 4.1.2 we wrote that simulations were performed in three different basins with a given sizes and with three given initial horizontal buoyancy gradients. Thus, 3x3=9 cases.

B) we stated quite clearly that the vertical diffusivity was taken to be $5*10^{-3}$ in the upper 100m and $5*10^{-5}(m^2s^{-1})$ in the deep ocean. Due to the large vertical diffusivity in the upper 100m, a weak stratification was formed from the initial one. Of course, maintaining such a ML is purely artificial and does not correspond to realistic forces. On the other hand, considering such flows is reasonable in the context and objective of our work which is to validate a mesoscale parameterization expressed entirely in terms of external fields.

C) As for realistic flows, in Figs. 5-7 and Table we present results of 5 and 8 different simulations. R1 considers this presentation unconvincing. ***How many will be convincing? 20? 50?*** In the Comments, R1 refers to Gille and Davis (1999) who performed the grand total of 2 simulations and to Lapeyre et al. (2006) who performed two sets of simulations, A and B and presented the results of only two runs A6 and B6. Why were those sufficient and ours is not?

- R1 refers to several mesoscale parameterizations that have been proposed.

Answers.

A) Since the latter were never tested with ERS, R1's criterion would imply that they should never have been published.

B) as an example, the GM model was not validated by ERS before being published. Nine years after GM90 paper, Gille and Davis (1999) employed ERS and concluded that the GM model explains 40% of the eddy resolved flux. In 1999, Bryan et al. (1999) also performed an ERS and concluded that something is missing in the GM-model that is not related to the flattening of the isopycnals. Thus the question: if GM had been assessed by ERS and found to miss 60% of the flux, would R1 have accepted it? Now that we know about the missing 60%, we dare suggest that the CD6 expression for the eddy induced velocity, see Addendum Eq.(5a), of which GM is a particular case, might alleviate some of the above shortcomings, a fact that we will soon check.

C) in the case of the ML, we joined forces with C.A.Clayson and M.Luneva whose extensive list of publications (most of which are cited in the References) shows their proven expertise with ERS.

D) Conclusions. ERS is an art on its own and it is disingenuous of R1 to suggest that the authors of CD6, without a track record on the subject, could carry out such a test. However, we did contact people with ERS expertise and inquired whether we could borrow their data. One research group had dissolved and the other two (one is a French group) told us that had they known of our need they would have saved the fields that we needed. Since the ERS produce a huge amount of data, it is quite understandable if these authors kept only what was strictly necessary. In the ML case, we had the opportunity to join forces with two ERS experts and we carried out an ERS test of our model results. At present, we have a joint project with a research group with the computer resources and the technical expertise to carry out an ERS of the deep ocean CD6 model, a program that will begin next September.

Eddy Resolving Simulations, ERS.

b) Technical details of our work

In the *idealized simulations*, we set up an initial exponential density profile and a step-like diffusivity. As no other forcing was present, the characteristic time of formation of the ML is $\approx 10h^2/\pi^2 K_v \approx 10\text{days}$. Due to the mixing stratification, the initial exponential profile in the upper 100m became nearly neutral. We did not use realistic mixing forcing because we wanted to deal with a baroclinic instability only. We fixed the initial buoyancy gradient independently of depth. and varied the density profiles. The Coriolis force was determined on a sphere so the beta effects were taken into account. Zonal jets were not observed as they needed longer lived baroclinic/barotropic instabilities. The buoyancy gradients were meridionally and initially zonally homogeneous.

• Since basin sizes and the effects of boundaries are not included in Eq.10c, it was important to examine the effects of boundaries. We found that such effects are no longer felt at a distance of about an eddy size from the closest boundary. As K and r(d) were different in these experiments, from our point of view it does not matter what parameters varied in this case. The 100m ML depth was chosen so as to resolve well the vertical profiles. Tests with variable ML depth are presented under "realistic simulation" sections.

• We showed the results of 30 experiments. Both ML depth and stratification were quite variable with time and depth. At least half of the experiments (out of the total of 70) were rejected as the Rossby radius was too small from the colder side of front and model resolution was not sufficient to properly resolve the eddy field. The ML naturally varied from 50m to 300m during the cold season, so the natural variability of the ML is reproduced in the model. In other seasons, the ML (10-20m) was too shallow due to strong precipitation (in early autumn), solar radiation (starting at March) that require very high resolution near the surface. Thus, we chose a period of time when we could resolve the vertical fluxes properly.

• R1 states that "*Fig.5 shows a scatter of points, to which a linear fit is difficult to justify statistically*"

Answer. A large scatter of eddy fluxes is a common feature of turbulent fields. We refer R1 to the classical figure of the observational validation of the Monin-Obukhov law in the atmosphere. Another example is the vertical flux of sub-mesoscales presented by Fox-Kemper et al. (2008,

Fig.14e) where a large scatter is observed. On the other hand, in all three Figs.5, the correlations between the horizontal diffusivity obtained from the model and simulations, are positive and statistically significant (from 0.4 to 0.7).

Single Items

3.2 Surface EKE and T/P –Jason-1 data

• R1's writes that.. "*the authors ought to explain…*"

Answers. Point well taken. A 3x3 degrees ocean grid with 25 vertical levels version of the 3D NCAR-CSM OGCM was used to run ocean stand-alone simulations and provide the mean fields (velocity, density, N^2 and mixed layer depth) necessary to calculate K_s. The OGCM was integrated to equilibrium for a thousand years using split tracers and momentum time steps, with tracer time step ten times momentum throughout. K_s was calculated diagnostically and averaged over a year in order to be compared with Scharffenberg and Stammer's (2010) data. The mixed layer depth was calculated using a potential density difference criterion of 3 x 10^{-5} gcm^{-3} . Our expression for K_s has an f^{-1} dependence and thus it diverges at the Equator. In the Addendum we explain that the dynamical mesoscale model is valid only under approximation (A.3a) which means outside the Tropics. For this reason, we have excluded the region 10^0 north and south of the equator in the figures below.

• R1's writes that .. "*eq.(10c) predicts that the surface eddy kinetic energy K_s is proportional to the ML depth".*

Answer. Please look at Eq.(1b,c) below which is a rewriting of Eq.(10c).What R1 says would be correct if the mean velocity were only geostrophic. However, that is not so since there is the very important Ekman component, discussed below in detail, that does not depend on the ML depth.

• R1's writes that "*we need to establish that our model for K_s is better than the one in CD6".*

Answer. It is not a matter of "better" but a matter of "different". The CD6 expression for the surface eddy kinetic energy refers to a fully adiabatic ocean **without a ML,** whereas the new one corresponds to a realistic ocean **with a ML** (in CD6 we used the symbol K_t ,"t" for top, so that it cannot be confused for the new one K_s). The CD6 and the present expressions are different because they represents different physics.

• R1 asserts that: *Wouldn't a simple map of the product of the Ross by radius squared and the horizontal buoyancy gradient produce a better fit*? R1's recommendation is as follows:

$$K_s = C_s r_d^2 < \left|\nabla_H b\right| > , \qquad < \left|\nabla_H \bar{b}\right| >= h^{-1}\int_{-h}^{0} dz \left|\nabla_H \bar{b}\right| \tag{1a}$$

where <…> stands for an ML averaged buoyancy gradient.

Answers.

A) R1's model for K_s. A generally accepted principle of heuristic models such as (1a) is that the proportionally coefficient C_s must be of order unity since the underlying assumption is that the functional dependence is fully accounted for by the two terms in (1a). **Fig.A** contains four panels, the altimeter data and (1a) for C_s =1, 0.1 and an extreme value of 0.001. Comparison shows that none of these results is compatible to the data in either intensity and/or geographical distribution. A constant C_s =O(1) as implied by the R1's suggestion, is clearly ruled out.

B) Present model for K_s. We use Esq. (9) and (10) and rewrite (10c) in the following new form that makes the comparison with (1a) more transparent:

$$K_s = C_s r_d^2 < \left|\nabla_H \bar{b}\right| > \quad , \qquad C_s = C\,\frac{\int_{-h}^{0} z dz\, \mathbf{F}\cdot\nabla_H \bar{b}}{\int_{-h}^{0} dz \left|\nabla_H \bar{b}\right|} \tag{1b}$$

where $C=(3Ko/2)^{3/2}$ and $\mathbf{F} = \mathbf{F}_A + \mathbf{F}_B$:

$$fr_d^2 \mathbf{F}_A = (1+\sigma_t^{-1}) < \bar{\mathbf{u}} > \times \mathbf{e}_z - fr_d^2 < \partial_z \mathbf{s} > , \qquad fr_d^2 \mathbf{F}_B(z) = \mathbf{e}_z \times (\sigma_t^{-1}\mathbf{u}^* + \bar{\mathbf{u}}) \tag{1c}$$

Thus, (1b) is of the form (1a) with the following important differences: **a)** the dimensionless C_s is no longer arbitrary but calculable within the model, **b)** C_s is not a universal constant but location dependent, and **c)** C_s brings into the problem the mean velocity which can be either geotropic or Elman's depending on depth. The latter contribution has been discussed in the context of a global ERS by Maltrud et al. (1999, sec. 5). Furthermore, the relevance of the presence of the Ekman velocity for climate studies has already been discussed at the beginning of this Reply.

C) As discussed in CD5,6 and in the Addendum, sec.1, the present model and thus Esq. (1b, c) are *valid only outside the Tropics* (the solution of the mesoscale dynamical model inside the Tropics is a project under way, but no results are yet available). With that proviso, in **Fig. B,** we present the map of C_s from the second of (1b) which exhibits a clear location dependence, and the map of K_s from the first of (1a) which reproduces some important features of the altimeter data in both intensity and geographical distribution.

.

• R1 states that "*we make other strong hypothesis about the vertical structure of the kinetic energy*.

Answer. Contrary to R1's assertion, the facts are as follows. Our result, derived in detail in CD6:

$$K(z) = K_s\Gamma(z)\,,\; \Gamma(z) = (a_0^2+|B_1(z)|^2)(1+a_0^2)^{-1}, \qquad a_0^2 \approx \overline{K}_M / K_s \tag{1d}$$

where $\overline{K}_M$ is the mean kinetic energy averaged over the ML, is in accordance with Wunsch (1997) who showed that in the vicinity of the thermocline, the mesoscale velocity field is contributed mostly by the first baroclinic mode $B_1(z)$ and thus, to first order, $\Gamma(z) \approx |B_1(z)|^2$ while below the thermocline, the contribution of the barotropic modes becomes important. To compute the first baroclinic mode, one must solve the eigenvalue equation:

$$\partial_z(N^{-2}\partial_z B_1) + (r_d f)^{-2}B_1(z)=0 \tag{1e}$$

with the boundary conditions $B_1(-h)=1$, $\partial_z B_1=0$ at $z=-h, -H$ (ML and ocean depths). Results of the ratio $K(z)/K_s$ computed using (1d-e) at different locations are presented in **Fig.C** for the first time (the data are from WOCE).

Additional information. The dynamic equation for the eddy kinetic energy shows that the mesoscale vertical flux F_v is not the only source of K since there is also the term (Ivchenko et al., 1997, Eq.2; Best et al., 1999, page 335):

$$\overline{S} = -R_{ij}\overline{u}_{i,j} \quad , \qquad R_{ij} = \overline{u_i' u_j'}\,(\text{Reynolds Stresses}) \tag{1f}$$

Such a term, denoted by T_4 in Boning and Budich (1992, BB92, their Eq.16), *if positive*, represents a source of K from the mean kinetic energy (interpreted as a *barotropic instability*). In Bryden (1982, first term on the rsh of his Eq.3), $\overline{S}$ is defined as the "transfer of mean kinetic energy to eddy kinetic energy". On the other hand, Eq.(1a) represents the source of K from baroclinic instabilities only (or more precisely, from the ML averaged energy transfer from eddy potential energy). For the Gulf Stream, Bryden's measurements yield $\overline{S} = -(1.5 \pm 0.9)\cdot 10^{-9}\,m^{-2}s^{-3}$ and thus eddies lose energy to the mean flow. On the other hand, in the ACC (Treguier, 1992; Ivchenko et al., 1997, fig.7; Best et al., 1999, Fig.11), $\overline{S}$ is positive and thus eddies gain energy from the mean flow[2]. It must be stressed that in the ACC the barotropic instability term $\overline{S}$

[2] Eden and Greatbach (2008) presented results of an ERS for the North Atlantic. Their Figs.1a,b show both F_v and $\overline{S}$ at a depth of 200m. F_v is largely positive while $\overline{S}$ has a checkered structure with mostly negative values, as shown in Fig.1c. Since these results are for a depth of 200m, they cannot be directly compared with those of Bryden that correspond to 600-800m.

contributes only 8% of the eddy kinetic energy (Ivchenko et al., 1997, sec.5) and that explains why model (1b) alone is able to reproduce the altimeter data fairly well. Since we do not have measurements of $\overline{S}$ for many ocean basins nor a mesoscale model for the Reynolds stresses R_{ij}, a complete comparison of model predictions vs. altimeter data is not yet possible, as it is impossible to answer the questions posed by R1, such as the value of K_s in the Gulf of Guinea, along Australia etc.

Surface eddy kinetic energy. Conclusions. The problems just discussed must be viewed in the proper context. Studies attempting to reproduce the eddy kinetic energy date back many years and the difficulties are well known both at the regional level, the ACC studies with the ERS FRAM (Treguier, 1992; Stevens and Killworth, 1992; Ivchenko et al., 1996, 1997; Best et al., 1999), and with a global ERS (Maltrud et al., 1998). A common feature of these ERS is their inability to reproduce the eddy kinetic energy which is too low compared to the data (a difficulty that all studies suggest requires even higher resolution). In the present framework, even a cursory comparison of Fig.B vs. the altimeter data in Fig.A shows that the model reproduces the right eddy kinetic energy intensities in several locations. Furthermore, since the ERS at the global level report that "in the top layer the eddy kinetic energy has a large component due to the Ekman layer response to the winds" (Maltrud et., 1998), it is reassuring that model (1b,c) is capable of incorporating such a feature.

• R1 asserts that we have assumed that: "*the eddy kinetic energy spectrum peaks at the scale of the first Rossby radius*" .

Answer. There is no such an assumption in our work. We invite R1 to go through the detailed *derivation* in CD5 where we solved the mesoscale dynamic equations and *derived* that result. The position of the maximum of the eddy kinetic energy spectrum at the Rossby radius is confirmed by the observational data of Stammer (1998). The transition from r_d to L_R (Rhines radius) is not an relevant issue since our model is valid only outside the Tropics.

• R1 asserts that *there is a contradiction between CD6 and the present model since in both cases the tapering schemes have become unnecessary*.

Answer: there is misunderstanding of what we did, not a contradiction between CD6 and the present model. In CD6, where the flows were assumed to be adiabatic top to bottom, the streamfunction and bolus velocity automatically satisfied the boundary conditions (baroclinicity condition) without calling upon any tapering scheme, see Addendum section 3. In the present model for the diabatic mixed layer, the vertical flux automatically vanishes at the surface. Thus, in either case our statement was and is correct.

• line 19: All what we needed in this line and below, was to recall that most of the mesoscale parameterizations were developed for a fully adiabatic ocean and not for a diabatic ML. Thus, R1's suggestion of "a discussion of the different heuristic forms proposed for the GM coefficient based on scaling arguments would be needed for completeness" is hardly relevant. However, sucha forms are discussed in A.2d in the context of the baroclinicity condition.

• **Page 876 and 877**: the CD6 model is not mentioned here because we discuss the features of the diabatic ML which K5 drew attention to while CD6 was designed for an adiabatic ocean.

•**Page 877, lines13-18.** In the new version of the manuscript we have recast our results in the language of the RMT (Residual Mean Theory) and thus we now have the ML stream function which, at the bottom of the ML, is to be matched with one used in the deep ocean, for example the GM form used in many OGCMs. This will make the matching problem considerably more transparent and much easier to handle numerically.

•**Page 878:** in the last few years, there has been as a considerable emphasis on the strong effects of sub-mesoscales on large scale (which include mesoscales) features, especially in the presence of *a strong wind* (Mahadevan et al., 2010). Our work on parameterization of sub-mesoscales (Canuto and Dubovikov, 2010a,b) confirmed this conclusion. To be more precise, our model made predictions that were later corroborated by ERS.

• **Page 880, line20** Mesoscale tracer equation. The point has already been answered above.

• Mesoscale tracer field. In Eq. (3c), the "bracket" averaging is performed over the ocean interior where the problem pointed out by R1, is absent. Regrettably, we missed the misprint in Eq.(3e) in which the upper limit of the integrals must be –h (where h is the depth of ML) rather than 0.

• time filtering (over several days) of both mesoscale and large scale fields is one of the most important steps in processing our simulation data. After filtering, the results improved considerably since mesoscales and large scale fields were no longer contaminated by wave components of the same length scales. Due to filtering, the number of negative horizontal diffusivities decreased sharply. The number of negative diffusivities could arguably be lower in the ERS of Eden and Greatbatch (2008) if they had filtered both mesoscale and large scale fields from inertial gravity waves (which from earlier correspondence we gather they did not do).

• Effect of sub-mesoscales. We have discussed it in our answer to page 878.

•2.8 Already answered above in response to line 13-18.

•4.1 the very question of R1 shows that the problem of filtering is worth repeating. The answer to R1's question is that the correlation functions were diagnosed "offline", i.e., after averaging the fields over 10 days in order to filter out wave fields and to obtain genuine mesoscale fields.

• 4.1.2 R1 repeats several questions from "Problem 2" which we answered above and will not repeat here. The idealized experiments were suggested by Clayson and Luneva (CL) whose idea was to perform a preliminary test of a parameterization that has no free parameters with easily performable simulations before tackling the much more difficult and time consuming simulations of realistic flows. We (CD) suggested to report the results of the idealized experiments since the CL idea of testing theoretical models is of a general interest. *The model was tested in 70 realistic simulations*. CL wanted to make sure that the parameterization works in different basins. Since the model results were derived theoretically (rather than being a fit to numerical experiments), there is no need to test the dependences on each parameter separately. The answers to other questions are that the buoyancy gradient is, of course, meridional. The beta effect was accounted for, as it can be seen from Eqs.(3c,d), (5d), (6a-c), (7a), but in the final formulae it cancels out.

•Lapeyre et al. (2006) ***did not*** test mesoscale parameterizations and thus we don't see how to compare our results with theirs.

•4.1.3 R1 repeats questions from the section "Problem 2". The answer to his last question is that in the context of a mesoscale parameterization designed for coarse resolution OGCMs and the content of section 4.1.3, the term "*coarse grid*" means the grid size over which mesoscale fluxes, as well as velocity and buoyancy fields, are averaged. At the end of his comments of section 4, R1 concludes that our validation is "completely unconvincing". *We have presented more results of ERS than in any publication on the validation of a mesoscale parameterization not only in the ML but also those few dealing with the ocean interior.*

ADDENDUM

Heuristic Models: brief summary

a) *adiabaticity* (flow along isopycnals) implies that the vertical and horizontal buoyancy fluxes are related by (**s** is the slope of the isopycnals):

$$F_v = \mathbf{F}_H \cdot \mathbf{s} \tag{1a}$$

Thus, in the adiabatic deep ocean flow, it is sufficient to model the horizontal flux $\mathbf{F}_H$ and the vertical flux F_v follows.

b) in the ML, flows do not occurr along isopycnals but on horizontal planes, a point first made by Killworth (2005, K5). Relation (1a) is no longer valid and one must parameterize $\mathbf{F}_H$ and F_v separately.

c) if one were to extrapolate (1a) to the ML all the way to z=0, where by definition $F_v(0)=0$, Eq. (1a) implies that:

$$\mathbf{F}_H(0) = 0 \qquad s \neq 0 \tag{1b}$$

which contradicts the surface results derived by Zhurbas and Oh (2003, 2004) and Oh et al. (2000) using data from Topex/Poseidon and satellite-tracked surface drifters.

d) if one extrapolates deep ocean properties to the ML, a further problem arises. For an adiabatic flow, McDougall and McIntosh (2001) derived the following boundary condition for the streamfunction $\mathbf{\Psi}$:

$$\mathbf{\Psi}(0,-H)=0 \tag{2a}$$

and since the eddy induced, bolus, velocity is defined as $\mathbf{u}^{+}=-\partial_z\mathbf{\Psi}$, the following *baroclinicity condition* follows:

$$\int_{-H}^{0}\mathbf{u}^{+}(z)dz=0 \tag{2b}$$

If we employ the GM model $\mathbf{\Psi}=\kappa_M\mathbf{s}$, Eq.(2a,b) are satisfied if the mesoscale diffusivity κ_M satisfies the condition:

$$\kappa_M(0)=0 \tag{2c}$$

which is another way of rewriting (1b) since in the GM model, $\mathbf{F}_H=-\kappa_M\nabla_H\overline{b}$. Eq.(2c) is not satisfied by any of the heuristic expressions for κ_M that have been proposed[3]:

$$\kappa_M=\text{const.},\quad \kappa_M\sim N^2,\quad \kappa_M\sim r_d^2 fRi^{-1/2}\quad ,1\le\kappa_M(0)<3,\ \kappa_M=c\ell^2\sigma \tag{2d}$$

e) Conclusion. Even if one were to “assume” that the ocean is adiabatic top to bottom, the GM model, as a prototype of an adiabatic parameterization, leads to the above inconsistencies. On the other hand, the adiabatic CD6 model does not lead to any inconsistencies since *the baroclinicity condition (2b) is satisfied for any form of the diffusivity* κ_M (z), see sec 3.

While the avoidance of internal inconsistencies is a welcome feature of the CD6 over the GM model, that does not mean that CD6 can be extrapolated to the ML which is what R1 erroneously believes we did. CD6 applies only to the adiabatic regime of the ocean and not to the ML.

---

[3] the *first* is the original GM suggestion; the *second* is due to Danabasoglu and Marshall (2007, DM); the *third* is due to Bryan et al. (1999) and is similar to the one by Visbeck et al. (1997); the *fourth*, where the surface diffusivity is in units of $10^3 m^2 s^{-1}$s, is due to Karsten and Marshall (2002), who used the models of Holloway (1986) and Keffer and Holloway (1998), and TOPEX/Poseidon data for the surface sea height; the *fifth* (ℓ is the smallest scale between the Rossby defor mation radius and the Rhines scale and $\sigma=N|\mathbf{s}|$ is the local Eady growth rate) has been used by Eden et al.(2009). Danabasoglu and McWilliams (1995) expression has a finite surface value.

Dynamical Models : CD5,6

1) Structure, results and regime of validity

In CD5 we presented a mesoscale model that begins with the mesoscale dynamic equations in isopycnals coordinates for the 2D velocity and thickness fields $\mathbf{u}', h'$. Eqs.(4) of CD5 are a generalization of Eqs (6)-9) of Killworth (1997, K97) since they include the non-linear terms neglected in K97. The closure for the non-linear terms was taken from a turbulence model previously derived and assessed on a large variety of turbulence fields as discussed in CD5. The treatment the mesoscale dynamic equations proceeds in the same fashion as in K97, that is, such equations are shown to reduce to a single eigenvalue problem (CD5, Eq.11) for the mesoscale Bernoulli function B' where B=p+gρz. Due to the presence of non-linear interactions, the new eigenvalue equation satisfied by B' differs significantly from that of K97.

To obtain analytical solutions of the eigenvalue problem, the following inequalities were assumed:

$$c_R \le \overline{u} \le K^{1/2} \tag{3a}$$

Here, $\mathbf{c}_R = r_d^2 \mathbf{k} \mathrm{x} \boldsymbol{\beta}$ is the velocity of the barotropic Rossby waves, r_d is the Rossby deformation radius, **k** is the unit vector in the z-direction, $\boldsymbol{\beta} = \nabla f$, f is the Coriolis parameter, $\overline{u}$ is the velocity of the mean flow and K is the eddy kinetic energy. The second inequality in (3a) is based on the results of Fig.4 of Stammer (1997) showing that $K/K_M \ge O(10)$, where $K_M \equiv 1/2\overline{\mathbf{u}}^2$ is mean flow kinetic energy.

The first inequality in (3a) is valid only *outside the tropics.* In fact, studies of mesoscale variability (Chelton et al., 2007), using high resolution sea-surface height fields constructed from the merged altimeter datasets of Topex/Poseidon (T/P) and ERS-1/2, revealed that while at mid latitudes mesoscales can be described as "water-mass anomalies that have nearly circular flow around their centers" whose nature is strongly influenced by the non-linear interactions (Richardson, 1993), in the sub-tropical zone (STZ), mesoscales occur as *Rossby waves* with a *weaker degree of non-linearity*. Mesoscales in the two regimes exhibit also different characteristic length scales: the first indication came from Stammer (1997, Fig.25) who showed a transition from the Rossby deformation radius r_d to the Rhines scale L_R at about 25-30^0 latitude, where $r_d \approx L_R$. The same conclusion was confirmed by Eden (2007) using an eddy resolving simulation. In such a region, the wavenumber at which the eddy energy is maximum is related to the characteristic size of the eddies ℓ by the following relation:

$$k_{max}^{-1} \sim \ell \sim r_d \tag{3b}$$

The different dynamics in the STZ means that in that region one must solve the CD5,6 eigenvalue problem under the conditions opposite to (3a):

$$c_R > K^{1/2}, \overline{u} \tag{3c}$$

The CD5 model has not yet been solved in the regime (3c) and we are not aware of any mesoscale model valid in that region. It must be further stressed that CD5,6 dealt exclusively with an adiabatic ocean which means without a diabatic mixed layer. This is the reason why, in order to complete the model, we worked out the paper under review. Under these conditions, the CD5,6 models lead to the following results.

2) Bolus Velocity $\mathbf{u}^+$

We begin with the definition:

$$\mathbf{u}^+ = -\partial_z \mathbf{\Psi} \tag{4a}$$

where $\mathbf{\Psi}$ is the stream function. Two heuristic models have been proposed:

$$\textbf{GM:} \quad \mathbf{\Psi} = \kappa_M \mathbf{s} \quad , \qquad \textbf{THL:} \quad \mathbf{u}^+ = \kappa_M(-\partial_z \mathbf{s} + f^{-1}\boldsymbol{\beta}) = \kappa_M \nabla_h \overline{q} \tag{4b}$$

where $\mathbf{s} = -N^{-2}\nabla_h \overline{b}$ is the slope of the isopycnals. The first corresponds to the Gent and McWilliams (1990) model and the second is the THL model (Treguier et al., 1997) in which $\overline{q}$ is the QG potential vorticity; finally, κ_M is the *mesoscale diffusivity* which we discuss below. Though models (4b) seem quite different, CD6 offers a unified solution, in fact it predicts that:

$$\textbf{CD6:} \qquad \mathbf{u}^+ = -\kappa_M(\partial_z \mathbf{L} + \mathbf{\Psi}_{new}) \qquad \mathbf{\Psi}_{new} = 2(f r_d^2)^{-1}\mathbf{k}\mathrm{x}(\overline{\mathbf{u}} - \mathbf{u}_d) - f^{-1}\boldsymbol{\beta} \tag{5a}$$

where $\mathbf{u}_d$ is the *drift velocity* (see below). **First,** at the locations where $\mathbf{u}_d$ coincides with the mean flow, the form of the eddy induced velocity acquires the form:

$$\overline{\mathbf{u}}_d = \overline{\mathbf{u}}: \qquad \mathbf{u}^+ = \kappa_M(f^{-1}\boldsymbol{\beta} - \partial_z \mathbf{L}) \equiv \kappa_M f^{-1}\nabla_h \overline{q} \quad \rightarrow \quad \mathbf{u}^+(\mathrm{THL}) \tag{5b}$$

which coincides with the THL model. **Second**, in the limit:

$$\mathbf{\Psi}_{new} = 0, \quad \mathbf{u}^+ = -\kappa_M \partial_z \mathbf{L} \tag{5c}$$

the model reproduces the GM model. Thus, the GM and THL models are special cases of the CD5,6 model and both can actually take place in different situations. **Third**, the presence of a *drift velocity* $\mathbf{u}_d$, called propagation speed by Henson and Thomas (2008) is an expected feature since observations show that in general mesoscales "move through the background water at speed and direction inconsistent with the background flow" (Richardson, 1983, 1993). Therefore, a non-zero difference $\bar{\mathbf{u}}(z) - \mathbf{u}_d$ represents the mismatch between the eddy drift velocity and the background mean velocity field in which mesoscale eddies move. **Fourth**, since eddies are coherent, quasi axi-symmetric structures, $\mathbf{u}_d$ is expected to be the same throughout the water column, that is, $\mathbf{u}_d$ must be barotropic, as indeed Eq.(18) of CD6 shows it to be:

$$\mathbf{u}_d = \frac{1}{2}\mathbf{c}_R + <\bar{\mathbf{u}}> - \frac{1}{2} f r_d^2 \mathbf{k} x < \partial_z \mathbf{s} > \tag{5d}$$

To help visualize (5d), we note that the first term is very similar to the low frequency Rossby waves with frequency $\omega \sim u_d \ell^{-1} \sim c_R \ell^{-1} \sim \ell\beta$, while the last two terms are caused by the non-linear interactions. Finally, the $<\bullet>$ averages in (5d) are defined as follows (h,H are the mixed layer and ocean's depths respectively):

$$< \bullet > = \int_{-H}^{-h} (\bullet) K^{1/2}(z) dz \, [\int_{-H}^{-h} K^{1/2}(z) dz]^{-1} \tag{5e}$$

3) Baroclinicity Condition

The baroclinity condition (2b) can be satisfied by the GM model if Eq.(2c) is satisfied which contradicts all relations (2d). On the other hand, it is a matter of algebra to show that the *CD6 model (5a) identically satisfies the baroclinicity condition (2b)* for *any* κ_M.

4) Mesoscale diffusivity

The solution of the CD6 models yields the following expression for the mesoscale diffusivity:

$$\kappa_M(z) = r_d K(z)^{1/2} \tag{6a}$$

which is to be compared with the result at the surface derived by Zhurbas and Oh (2003, 2004) and Oh et al. (2000) using data from Topex/Poseidon and satellite-tracked surface drifters:

$$\kappa_M(0) = (1.02 \pm 0.13) r_d K_s^{1/2} \tag{6b}$$

where the subscript s in K stands for the surface value of K. The model prediction (6a) is quite close to (6b).

5) Eddy kinetic energy profile

Eq.(6a) requires the z-profile K(z). In CD5,6 the eddy kinetic energy K was derived to have the following form:

$$K(z) = K_s\Gamma(z) \qquad \Gamma(0)=1 \tag{7a}$$

where $\Gamma(z)$ is a dimensionless function whose form is (CD5, Eq. 24a)

$$\Gamma(z) = (a_0^2+|B_1(z)|^2)(1+a_0^2)^{-1} \qquad a_0^2 \approx \overline{K}_M / K_s \tag{7b}$$

where $\overline{K}_M$ is the mean kinetic energy averaged over the ML. To compute the first baroclinic mode, one must solve the eigenvalue equation:

$$\partial_z(N^{-2}\partial_z B_1) + (r_d f)^{-2}B_1(z)=0 \tag{7c}$$

with the boundary conditions $B_1(-h)=1$, $\partial_z B_1=0$ at $z=-h, -H$ **(**defined earlier**).** The CD5 result (7b) is in accordance with Wunsch (1997) who showed that in the vicinity of the thermocline, the mesoscale velocity field is contributed mostly by the first baroclinic mode $B_1(z)$ and thus, to first order, $\Gamma(z) \approx |B_1(z)|^2$ while below the thermocline, the contribution of the barotropic modes becomes important. Results of the ratio $K(z)/K_s$ computed using (7a-c) at different locations are presented here for the first time in **Fig.C**. The data are from WOCE current meter data (WOCE Data Products Committee. 2002. WOCE Global Data, Version 3.0, WOCE International Project Office, WOCE Report No. 180/02, Southampton, UK) computing for each location and at each depth, half the variance of the time series of the velocity field. The same data also provide the mean of the time series used to compute $\overline{K}_M$ in Eq.(7b).

6) Surface eddy kinetic energy profile

As for the surface kinetic energy, K_s in (7a), one has three choices: a) assume that the whole ocean is adiabatic, in which case in CD6 we presented an expression for the surface K which we called K_t (t stands for top of such an idealized ocean without a ML), b) K_s can be obtained from the observational Topex-Poseidon-Jason-1data (Scharffenberg and Stammer, 2010), c) if the OGCM is used for climate studies, neither of the above two choices is advisable since K_s depends on large scale variables that may be different in future climates. The equation for the eddy kinetic energy shows that a source of K_s is the *mixed layer mesoscale vertical flux* $F_v = \overline{w'b'}$ which CD6 was unable to compute since it dealt only with an adiabatic ocean. The computation of K_s was discussed in detail in 3.2 above.

References

Best, S.E., V.O.Ivchencko, K.J.Richards, R.D.Smith and R.C.Malone, 1999, Eddies in numerical models of the ACC and their influence on the mean flow, *J.Phys. Oceanogr*., **29**, 328 350

Boning, C.W. and Budich, R.G., 1992, Eddy dynamics in a primitive equation model; sensitivity to horizontal resolution and friction, *J. Phys. Oceanogr*., **22**, 361-381

Bryden, H.L., 1982, Sources of eddy energy in the Gulf Stream recirculation region, *J.Mar.Res*., **140**, 1047-1968

Bryan, K., J.K.Dukowicz and R.D.Smith , 1999, On the mixing coefficient in the parameterization of the bolus velocity, *J. Phys. Oceanog*r., **29**, 3442-2456

Canuto, V.M. and M.S.Dubovikov, 2005, Modeling mesoscale eddies, *Ocean Modelling,* **8,**1-30, cited as **CD5**

Canuto, V.M., and M.S.Dubovikov 2006, Dynamical model of mesoscales in z-coordinates, *Ocean Modelling,***11,** 123-166, cited as **CD6**

Canuto, V.M. and M.S.Dubovikov, 2010a, Mixed layer sub-mesoscales. Part I: derivation and assessment, *Ocean Sciences***, 6,** 1-15

Canuto, V.M. and M.S.Dubovikov, 2010b, Mixed layer sub-mesoscales. Part II: results for coarse resolution OGCMs, *Ocean Sciences Discussion***, 7,** 1289-1302

Chelton, D.B., M.S.Schlax, R.M. Samelson and R.A.de Szoeke, 2007, Global observations of large oceanic eddies, *Geophys. Res Lett.,* **34**, L15606,doi:10.1029/2007GL0330812

Danabasoglu, G. and J.C. McWilliams, 1995, Sensitivity of the global ocean circulation to parameterizations of mesoscale tracer transports, *J. Climate*, **8,** 2967-2987

Danasaboglu G., and J. Marshall, 2007, Effects of vertical variations of thickness diffusivity in an ocean general circulation model, *Ocean Modelling*., **18,** 122-141

Eden, C., 2007, Eddy length scales in the North Atlantic, *J.Geophys. Res.,* **112** (C06004) doi:10.1029/2006JC003901

Eden, C. and R.J. Greatbatch, 2008, Toward a mesoscale eddy closure, *Ocean Modelling,* **20,** 223-239

Eden, C., M.Jochum and G.Danabasoglu, 2009, Effects of different closures for thickness diffusivity, *Ocean Modelling*, **26**, 47-59

Fox-Kemper, B. and R.Ferrari, 2008, Parameterization of mixed layer eddies. Part II: prognosis and impact, *J.Phys.Oceanogr.,* **38**, 1166-1179,

Gent, P.R., and McWilliams, J.C., 1990, Isopycnal mixing in ocean circulation models, *J.Phys.Oceanog*r. **20**, 150-155

Gille, S.T. and Davis, R.E., 1999, The influence of mesoscale eddies on coarsely resolved density: An examination of subgrid-scale parameterization, *J.Phys.Oceanogr.,***29,**1109-1123

Henson, S.A and A.C.Thomas, 2008, A census of oceanic anticyclonic eddies in the Gulf of Alaska, *Deep Sea Res.,* I, **55** 163-176

Holloway, G., 1986, Estimation of oceanic eddy transport from satellite altimetry, *Nature,* **323**,243-244

Ivchencko, V.O., K.J.Richards and D.P.Stevens, 1996, The dynamics of the ACC, *J.Phys. Oceanogr.*, **26**, 753-774

Ivchencko, V.O., A.M. Treguier and S.E.Best, 1997, A kinetic energy budget and internal instabilities in fine resolution Antarctic Model, *J.Phys.Oceanogr.,***27, 5-22**

Karsten, R., H. Jones and J.Marshall, 2002, The role of eddy transfer in setting the stratification and transport of a circumpolar current, *J.Phys. Oceanog*r., **32**, 39-54

Keffer, T. and G.Holloway, 1998, Estimating Southern Ocean eddy flux of heat and salt from satellite altimetry, *Nature*, **332,** 624-626

Killworth, P.D., 1997, On parameterization of eddy transport, *J.Marine Res.,* **55**, 1171-1197

Killworth, P.D., 2005, Parameterization of eddy effects on mixed layer tracer transport: a linearized perspective, *J. Phys. Oceanog*r., **35**, 1717-1725

Maltrud, M.E., R.D.Smith, A.J.Semtner and R.C Malone, 1998, Global eddy-resolving simulation driven by 1985-1995 atmospheric winds, *J.Geophys. Res.*, **103**, C13, 30825 30853

McDougall, T.J. and P.C. McIntosh, 2001, The temporal-residual-maen velocity. Part II: isopycnal interpretation and tracer and momentum equations, *J. Phys. Oceanog*r., **31,** 1222-1246

Oh, I. S., V. Zhurbas, and W. S. Park, 2000, Estimating horizontal diffusivity in the East Sea (Sea of Japan) and the northwest Pacific from satellite-tracked drifter data, *J.Geophys. Res*., **105,** 6483-6492

Radko, T. and J.Marshall, 2004, Eddy-induced diapycnal fluxes and their role in the maintenance of the thermocline, *J.Phys. Oceanogr*., **34**, 372-383

Richardson, P.L., 1983, A vertical section of eddy kinetic energy through the Gulf Stream system, *J. Geophys. Res.,* **88,** 2705-2709

Richardson, P.L., 1993, Tracking ocean eddies, *American Scientist,* **81**, 261-271

Scharffenberg, M.G. and D.Stammer, 2010, Seasonal variations of the geostrophic flow field and of eddy kinetic energy inferred from TOPEX/POSEIDON and Jason-1Tandem Mission Data, *J.Geophys. Res.,* **115**, C2, doi.1029/2008JC005242

Stammer, D., 1997, Global characteristics of ocean variability estimated from regional Topex-Poseidon altimeter measurements, *J. Phys. Oceanogr*. **28**, 1743-1769

Stammer, D., 1998, On the eddy characteristics, eddy transports and mean flow properties, *J.Phys. Oceanogr*., **28**, 727-739

Stevens, D.P., and P.D. Killwoth, 1992, The distribution of kinetic energy in the Southern Ocean, *Phil.Trans.Roy.Soc.London,* Ser B, **338**, 251-257

Stone, P., 1972, A simplified radiative-dynamical model for the static stability of the rotating atmosphere, *J. Atmos. Sci*.**,** **29,** 405-418

Treguier, A.M., I.M.Held and V.D.Larichev, 1997, Parameterization of quasi-geostrophic eddies in primitive equations ocean models, *J.Phys. Oceanogr*., **27**, 567-580, cited as **THL**

Treguier, A.M., 1992, Kinetic energ analysis of an eddy resolving primitive equation model of the North Atlantic, *J.Geophys. Res*., **97**, 687-701

Visbeck, M, J.Marshall, T.Haine and M.Spall, 1997, Specification of eddy transfer coefficients in coarse resolution ocean circulation models, *J. Phys. Oceanogr*., **27**,381-402

WOA05. **Temperature:** Locarnini, R. A., A. V. Mishonov, J. I. Antonov, T. P. Boyer, and H. E. Garcia, 2006. World Ocean Atlas 2005, Volume 1: Temperature. S.Levitus, Ed. NOAA Atlas NESDIS 61, U.S. Government Printing Office,Washington, D.C., 182 pp.). **Salinity:** Antonov, J. I., R. A. Locarnini, T. P. Boyer, A.V. Mishonov, and H. E. Garcia, 2006. World Ocean Atlas 2005, Volume 2: Salinity. S.Levitus, Ed. NOAA Atlas NESDIS 62, U.S. Government Printing Office,Washington, D.C., 182 pp.

Wunsch, C., 1997, The vertical partition of oceanic horizontal kinetic energy, *J. Phys.Ocean.,* **27**, 1770-1794

Zhurbas, V., and I. S. Oh, 2003, Lateral diffusivity and Lagrangean scales in the Pacific Ocean as derived from drifter data, *J. Geophys. Res*., **108**(C5), 3141, doi:10.1029/2002JC001596.

Zhurbas, V. and I. S. Oh, 2004, Drifter-derived maps of lateral diffusivity in the Pacific and Atlantic Oceans in relation to surface circulation patterns, *J. Geophys. Res*., **109,**C05015, doi:10.1029/2003JC002241.

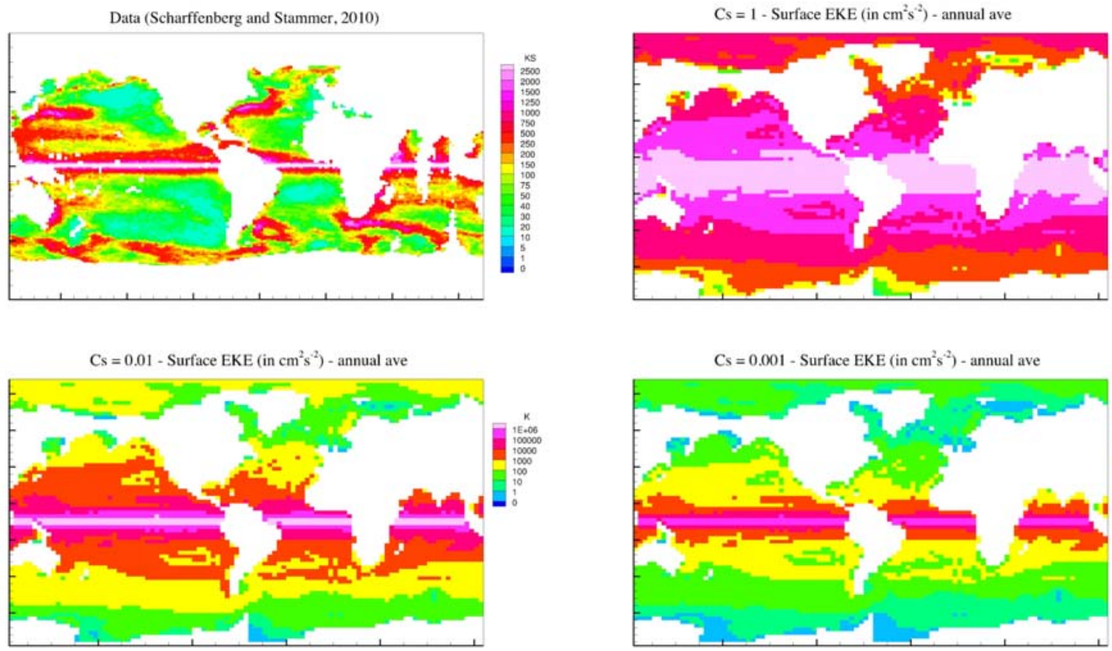
Data (Scharffenberg and Stammer, 2010)
Cs = 1 - Surface EKE (in cm^{2}s^{-2}) - annual ave
Cs = 0.01 - Surface EKE (in cm^{2}s^{-2}) - annual ave
Cs = 0.001 - Surface EKE (in cm^{2}s^{-2}) - annual ave

Fig. A

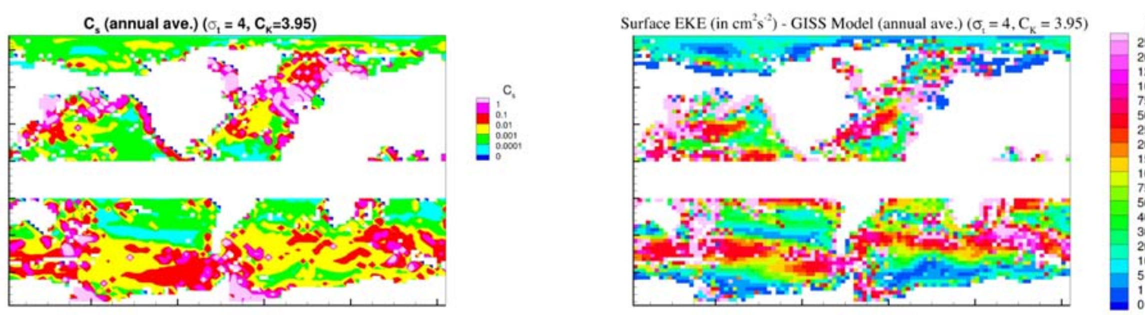
C_s (annual ave.) (σ_t = 4, C_K=3.95)
C_s
1
0.1
0.01
0.001
0.0001
0
Surface EKE (in cm^{2}s^{-2}) - GISS Model (annual ave.) (σ_t = 4, C_K = 3.95)
K
2500
2000
1250
1000
750
500
250
200
150
100
75
50
40
30
20
10
5
1
0

Fig. B

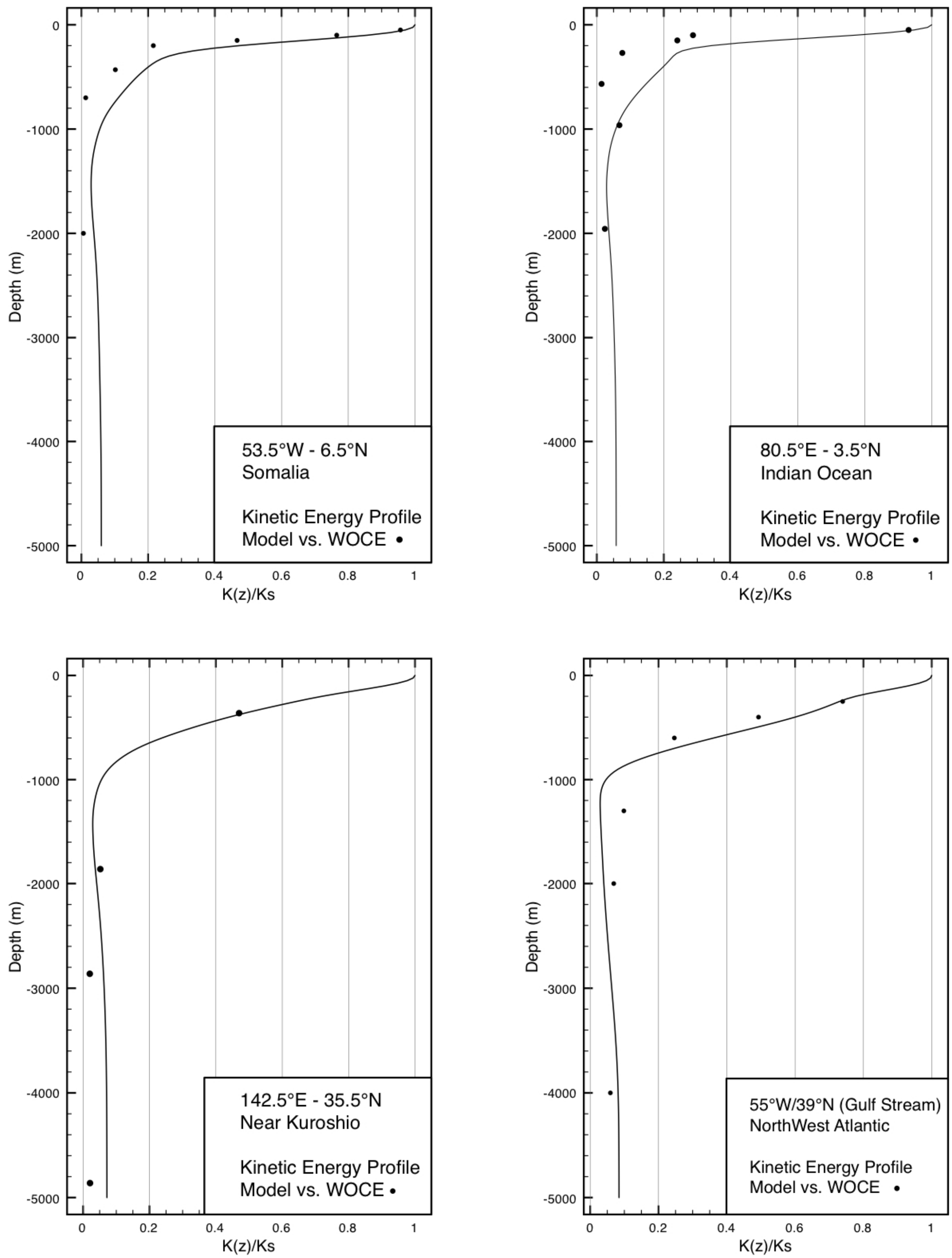
Depth (m)
K(z)/Ks
53.5°W - 6.5°N
Somalia
Kinetic Energy Profile
Model vs. WOCE •
80.5°E - 3.5°N
Indian Ocean
Kinetic Energy Profile
Model vs. WOCE •
142.5°E - 35.5°N
Near Kuroshio
Kinetic Energy Profile
Model vs. WOCE •
55°W/39°N (Gulf Stream)
NorthWest Atlantic
Kinetic Energy Profile
Model vs. WOCE •

Fig. C

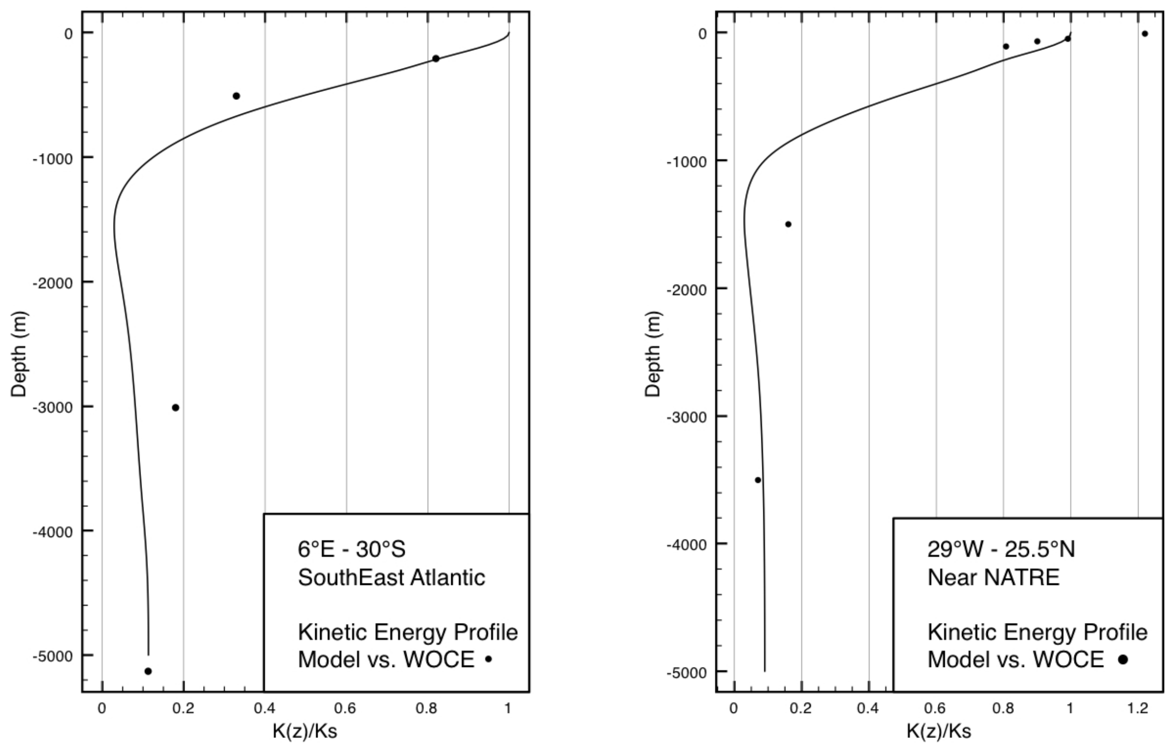
6°E - 30°S
SouthEast Atlantic
Kinetic Energy Profile
Model vs. WOCE •
Depth (m)
K(z)/Ks
29°W - 25.5°N
Near NATRE
Kinetic Energy Profile
Model vs. WOCE •
Depth (m)
K(z)/Ks

Fig. C