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Ocean Science
Discussions

Interactive comment on "Derivation and assessment of a mixed layer sub-mesoscale model" *by* V. M. Canuto and M. S. Dubovikov

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While this paper is substantially improved from previous versions, especially as regards self-contained derivations and argument, it continues to be misleading in a number of regards. While I do not agree with the model presented in this paper, I do think that there is something to be learned from the derivations herein, especially as regards the role of eddy kinetic energy. However, this result cannot be claimed to be new as it is a standard practice in turbulence scaling (see, for example Pope, 2000). The other aspects of the paper do not seem new and are reproductions of the earlier works of Canuto & Dubovikov on the mesoscale adapted to the submesoscale (1997, 2005, 2006).

My primary disagreement with this work is the authors' assertion that this is a 'solu-

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tion' of the submesoscale problem, while certain assumptions used cannot be reconciled with nonlinear simulations, especially the assumption of deformation scale eddies. Furthermore, I do not think that it is fair to state that "In both cases, the model results reproduce the simulation data satisfactorily." Indeed, a statistical analysis of this model has proven exactly the opposite to be true regarding the FFH simulations (as will be shown below).

Canuto & Dubovikov are well aware of this statistical analysis, and they neglect to mention it here. It was performed in a reply to a previous unpublished paper of Canuto & Dubovikov closely related to this paper that was submitted to the Journal of Physical Oceanography as a comment paper on FFH. The reply was deemed to satisfactorily refute the arguments of Canuto and Dubovikov without introducing any new material to FFH, and therefore the two papers were rejected.

As for the comparisons to the Capet et al. simulations, they are encouraging, but agreement with a few vertical profiles does not validate a parameterization. Indeed similar profiles would result from the vertical structure for the FFH parameterization.

Canuto & Dubovikov correctly assert that the FFH simulations are limited in that they do not have wind stress. However, if the Canuto & Dubovikov model does not work in the wind-free case, then there why should it be applied? Furthermore, new results indicate that the FFH model does work as expected in the presence of winds, so long as those winds are of typical magnitude (Capet, X., E. J. Campos, and A. M. Paiva, 2008; Mahadevan, A., A. Tandon, and R. Ferrari, 2008; see http://tinyurl.com/ylckbpo for full bibliography).

I will structure my comments into two sections. The first will address what I think is the crucial issue of submesoscale eddy lengthscale. The second will address the degree to which a satisfactory fit of the Canuto & Dubovikov model is found with the FFH data. It should be noted that while I have many less substantive disagreements with the text, I will constrain this review to address the issues with my, and my colleagues, own work.

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1 The Lengthscale of Submesoscale Eddies

Eqtn 3a is effectively a Reynolds-stress (see Pope, 2000, Chp 11) scaling for ξ. The difficulty with such closures is that it is quite easy to assess the dependence on kinetic energy, but rather hard to find a good scaling for the length, time, or dissipation rate scale. The ease of modeling the kinetic energy is noted by many turbulence closure authors, see Pope (2000) for a summary. It is suggested here by Canuto & Dubovikov that the Rossby radius of the mixed layer is "...the characteristic submesoscale length scale. As it is stressed in the literature on this subject (e.g., review by Thomas et al., 2008; Fox-Kemper and Ferrari, 2008; Boccaletti et al., 2007), l is closely related to the deformation radius in the mixed layer..." This lengthscale is then used throughout the paper, both as the typical lengthscale in 3a, and also as the typical lengthscale of their eddy viscosity closures (Appendix A, B). It is thus one of the most crucial assumptions made in this theory, and I believe it has already been demonstrated to be incorrect.

This statement about the lengthscales found in those works is a misrepresentation of the literature. Indeed, FFH spent months of research to determine and then demonstrate that while the deformation radius is a useful scale for the submesoscale linear instabilities, it is a poor estimate for the finite amplitude eddies that do the restratification and mixing to be parameterized. Furthermore, Thomas et al. were well aware of this research when they wrote the review. Some examples from the articles cited by Canuto and Dubovikov are specifically mentioned below:

1–Thomas et al. only use the Rossby scale to roughly establish the scales of the submesoscale. Fronts and other features are mentioned to occur near this scale, but there is no mention of this lengthscale regarding the submesoscale eddies that form in their figure 2.

2–There is a lot of discussion in these papers that the deformation radius is an appropriate scale for the linear instabilities, to wit:

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2a–From Fox-Kemper Ferrari 08: "The baroclinic instabilities that lead to finite-amplitude mixed layer eddies (MLEs) occur on length scales near the deformation radius of the ML..."

2b–From Boccaletti et al. 07: "For baroclinic instabilities (QG or not) the disturbances grow at a scale near the local deformation radius (Stone 1966)..."

2c–From FFH: "The smaller MLEs result from ageostrophic baroclinic instabilities that develop along fronts within the ML. Their scales begin near the linear instability scale based on ML depth and stratification, O(1 to 5 km), and enlarge as a result of an inverse cascade, as discussed in [Boccaletti et al., 2007]."

3–However, once the finite amplitude eddies have formed and begun to flux materials in earnest, the length scale is quite a bit larger:

3a–From FFH: "The mixing length, however, is not fixed in time in spindown problems such as this one nor is it readily estimated from other horizontal scales: the most energetic eddies enlarge beyond the most unstable scale (e.g., Cehelsky and Tung 1991) and beyond the initial frontal width (Fig. 2c)."

3b–From FFH: "However the nonlinear spectrum departs the linear prediction as the instabilities reach finite amplitude. EKE is transferred to scales larger than the most unstable mode through a vigorous inverse cascade (Fig. 4)." [Figure reproduced here in Figure 1].

3c–From FFH: "In linear theory, the length scale at which the vertical velocity and the buoyancy perturbations are correlated specifies the vertical structure of $\overline{w'b'}$. Figure 9 shows the dominant length scales contributing to the correlations between w', v', and b'. While the correlations and autocorrelations of v' and b' are dominated by features larger than the most unstable length scale, the typical horizontal scale at which w' and b' correlate remains close to Ls. The difference in correlation scales is con- sistent with a vertical mode saturation and a horizontal mode inverse cascade. Thus, the vertical structure of $\overline{w'b'}$ from linear theory persists at finite amplitude (per Branscome 1983a,b)." [Figure reproduced here in Figure 1].

3d–From FFH: "Once the instability becomes fully nonlinear the horizontal scales of the most energetic eddies are larger than that of the most unstable mode as a result of a turbulent inverse energy cascade."

4–Finally, two figures from FFH (included here as Figure 1) show that indeed the deformation radius is a poor estimate of finite eddy length scale.

The scaling proposed by FFH is crucially different from the scaling proposed here, and other scalings derived from linear instability (see Section 3d of FFH). The Canuto & Dubovikov scaling asymptotes to the same result as that found by Stone (1972) based on an analysis of linear instabilities. It is my belief that the reason why this asymptotic agreement with Stone occurs is the deformation radius scaling used by Canuto & Dubovikov. FFH demonstrate conclusively in their figure 14f that this scaling is inappropriate for the simulation data (which is the same data reproduced here). The next section explains further.

2 Goodness of Fit to FFH data

In the Reply to the comment by Canuto & Dubovikov (http://tinyurl.com/ylckbpo), the authors of FFH state: "CD state that their model appears superior to the FFH parameterization based on the fact that a few data points appear low in Figure 14e of FFH and claim that their model explains better the small Ri, small K/KM (i.e., linear regime) behavior. Once again, the linear regime was not the foucs of FFH because restratification is negligible during this phase–eddy effects are small during the linear stages and do not modify the stratification at leading order. However in response to the criticism raised by Canuto and Dubovikov, the analysis of the FFH simulation data was extended to test quantitatively the scaling over a wider range of Ri including the linear regimes.

Notice that this required analysis of Fox-Kemper, Ferrari, Hallberg unpublished data, because contrary to the statements of Canuto and Dubovikov (2009b), the data points presented in FFH were not sufficient to dicuss the low Ri behavior. So, a reanalysis of the FFH simulation data was performed to test quantitatively whether these claims are true generally."

The reanalysis of the FFH simulation data is described in the Reply:

> Recall that Figure 14e of FFH plots the time-mean Richardson number over the complete simulation (which is steadily increasing with time) versus the scaled magnitude of the eddy-induced streamfunction (which is noisy, but not increasing with time). In this figure, the time-mean over each simulation was used to reduce the noise, and since little or no depen- dence on Ri was noted it was inconsequential which Ri was used. FFH chose the time-mean of Ri for the figure and independence from the initial value of Ri is also discussed.
>
> To test the Richardson number close to Ri = 1 as Canuto and Dubovikov (2009b) insist, the time averaging must be made over shorter windows– here two times the fastest growing mode timescale is used as an averaging window [...]. Thus, during the linear stage K will grow by an order of magnitude over each time window. Furthermore, only simulations that are geostrophically balanced are useful, as Rossby adjustment yields Ri = 1 immediately .1 FFH chose to neglect times when $K/K_M < 0.1$ to eliminate the linear stage of evolving instabilities. To fully refute the Canuto and Dubovikov (2009b) scaling, all times where $K/K_M > 0.01$ are now used, ensuring that any bias is in favor of the CD parameterization over FFH. In sum, a subset of the 241 runs Fox-Kemper et al. (2008b) are averaged over windows where mixed layer depth and frontal strength were unambiguous, resulting in 603 time windows without overlapping from 68 simulations used.
>
> The results from these time-windows are binned by Richardson number

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from 1 to 9000 and shown in Fig. 1.

Figure 1 from the reply is reproduced as Figure 2 here. It is clear that the Canuto & Dubovikov scaling agrees with the simulation data only where it also agrees with the FFH scaling (a value of 1 in this figure).

The Table 1 from the Reply (also reproduced in Figure 2 here) shows the results of a student t-test. This test is explained in the reply:

> From the dataset of windowed time-mean values, a student t-test provides a statistical assessment for rejection of a theory over a particular range of Ri. The null hypothesis $\overline{w'b'} = \overline{w'b'}_{theory}$ may be rejected if the deviations between data and theory are statistically significant given the scatter of the data. The results of this analysis are given in Table 1. Apparently, CD are correct that their theory cannot be rejected by the simulation data in the range $Ri < 5$ [...]. In fact, the more careful test here of the simulation data reveals too much noise to reject the FFH parameterization with standard statistical confidence limits. Only the FFH parameterization cannot be rejected over all Ri ranges considered.

Thus, the assertion here that the Canuto & Dubovikov scaling agrees ’satisfactorily’ with FFH data is unsupported.

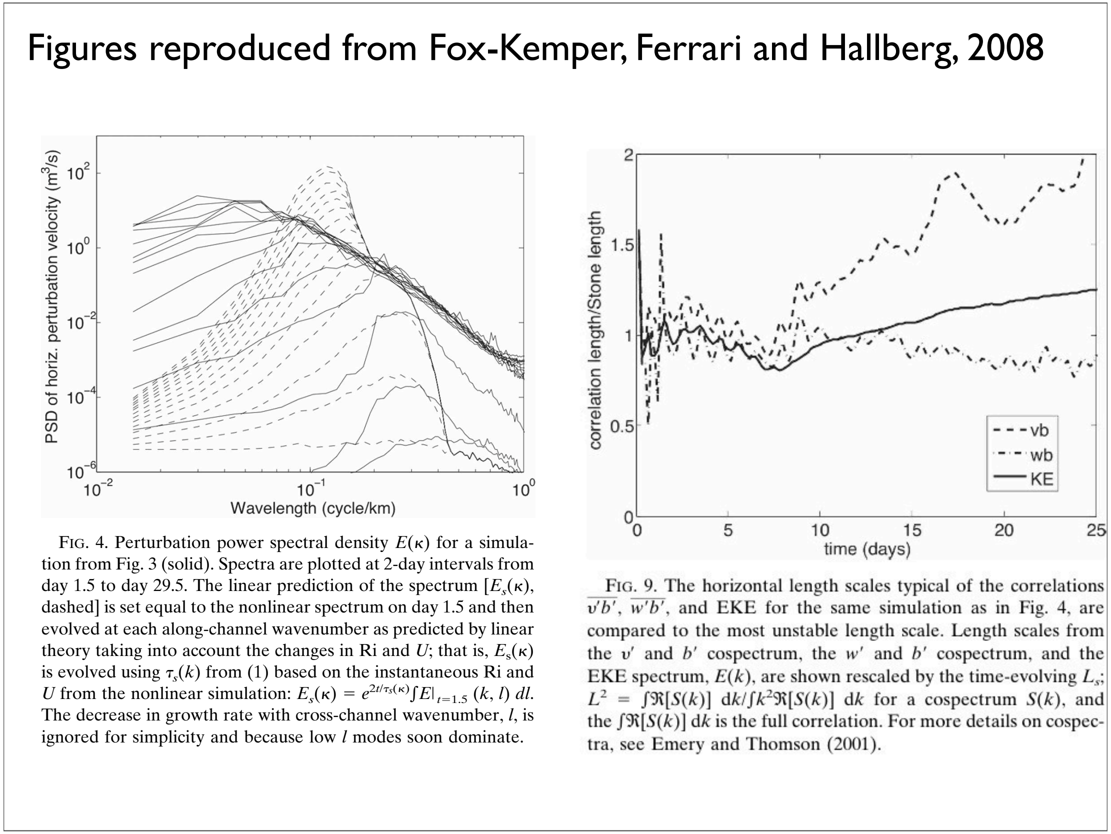
More disturbing to me than the lack of mention of the statistical test above is the fact that the Figure 4 of this paper shows only a limited range of the data where the fit is ’satisfactory’. Nonetheless, Canuto & Dubovikov claim to compare to simulations ’available in the literature’ without mentioned this reduced comparison. Figure 3 of this review shows the full range of Ri, as calculated by Canuto & Dubovikov for their earlier comment paper. Note that the points shaved off at the high Ri end are those in least agreement with the Canuto & Dubovikov theory.

Figures 1-3 of this Canuto & Dubovikov paper do not explore a parameter space of Ri, and show only agreement with vertical columns on F_v. Thus, agreement among these figures is inconclusive as to whether the parameterization works over a large range of predicted eddy lengthscales (per preceding section). The vertical structure of F_v here differs little from that in FFH, and is also the same as occurs in linear instabilities (see also Branscome, 1983).

3 Conclusion

It is agreed that a comparison to simulation data is a necessary, not sufficient, proof of a particular parameterization. Even so, the 'satisfactory' agreement with Figure 4 here is specious. Since all one needs to disprove a theory is a single counterexample, there are counterexamples aplenty shown in Figure 3 of this review that occur outside of the range deemed 'satisfactory' in Figure 4 of the paper by Canuto & Dubovikov.

Interactive comment on Ocean Sci. Discuss., 6, 2157, 2009.



Figures reproduced from Fox-Kemper, Ferrari and Hallberg, 2008

FIG. 4. Perturbation power spectral density $E(\kappa)$ for a simulation from Fig. 3 (solid). Spectra are plotted at 2-day intervals from day 1.5 to day 29.5. The linear prediction of the spectrum [$E_s(\kappa)$, dashed] is set equal to the nonlinear spectrum on day 1.5 and then evolved at each along-channel wavenumber as predicted by linear theory taking into account the changes in Ri and U; that is, $E_s(\kappa)$ is evolved using $\tau_s(k)$ from (1) based on the instantaneous Ri and U from the nonlinear simulation: $E_s(\kappa) = e^{2t/\tau_s(\kappa)} \int E|_{t=1.5}\,(k, l)\, dl$. The decrease in growth rate with cross-channel wavenumber, l, is ignored for simplicity and because low l modes soon dominate.

FIG. 9. The horizontal length scales typical of the correlations $\overline{v'b'}$, $\overline{w'b'}$, and EKE for the same simulation as in Fig. 4, are compared to the most unstable length scale. Length scales from the v' and b' cospectrum, the w' and b' cospectrum, and the EKE spectrum, $E(k)$, are shown rescaled by the time-evolving L_s; $L^2 = \int \Re[S(k)]\, dk / \int k^2 \Re[S(k)]\, dk$ for a cospectrum $S(k)$, and the $\int \Re[S(k)]\, dk$ is the full correlation. For more details on cospectra, see Emery and Thomson (2001).

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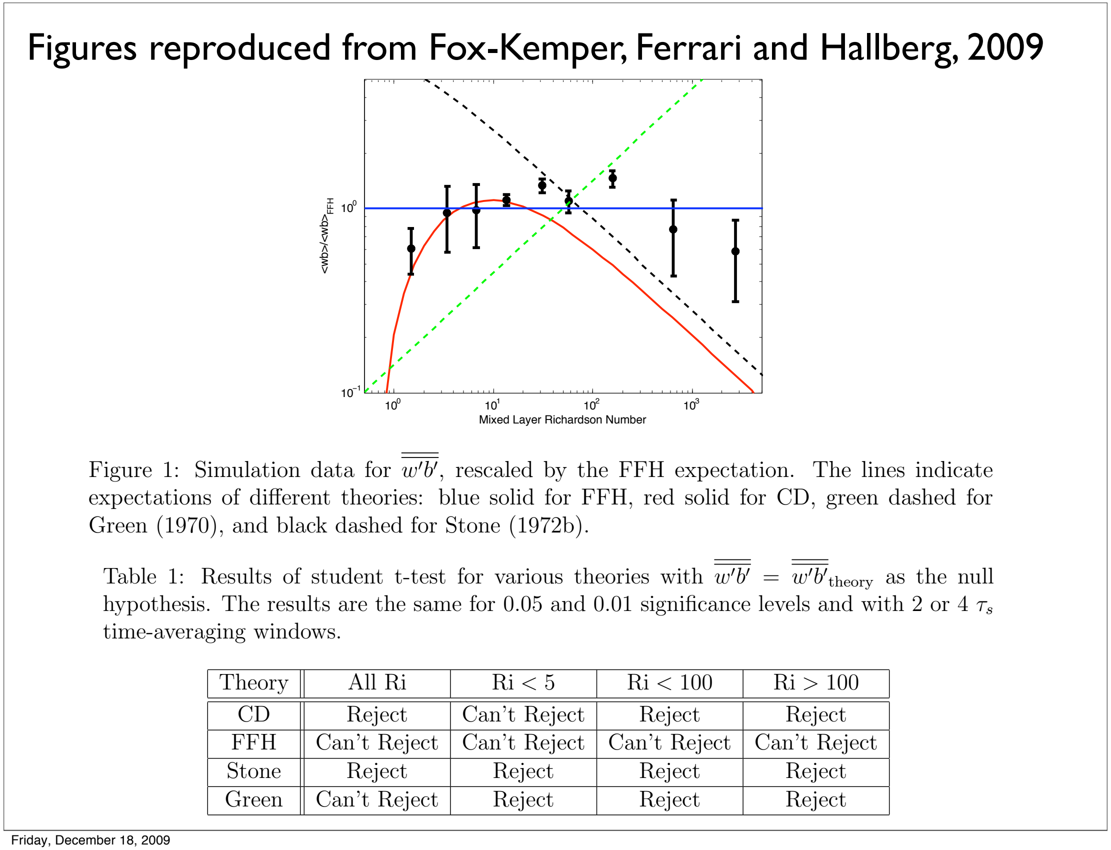
Fig. 1. Inverse Cascade Figures Reproduced from FFH



Figures reproduced from Fox-Kemper, Ferrari and Hallberg, 2009

Figure 1: Simulation data for $\overline{\overline{w'b'}}$, rescaled by the FFH expectation. The lines indicate expectations of different theories: blue solid for FFH, red solid for CD, green dashed for Green (1970), and black dashed for Stone (1972b).

Table 1: Results of student t-test for various theories with $\overline{\overline{w'b'}} = \overline{\overline{w'b'}}_{\text{theory}}$ as the null hypothesis. The results are the same for 0.05 and 0.01 significance levels and with 2 or 4 τ_s time-averaging windows.

Theory	All Ri	Ri $<$ 5	Ri $<$ 100	Ri $>$ 100
CD	Reject	Can't Reject	Reject	Reject
FFH	Can't Reject	Can't Reject	Can't Reject	Can't Reject
Stone	Reject	Reject	Reject	Reject
Green	Can't Reject	Reject	Reject	Reject

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Fig. 2. Figure and Table from Reply to Canuto \& Dubovikov.

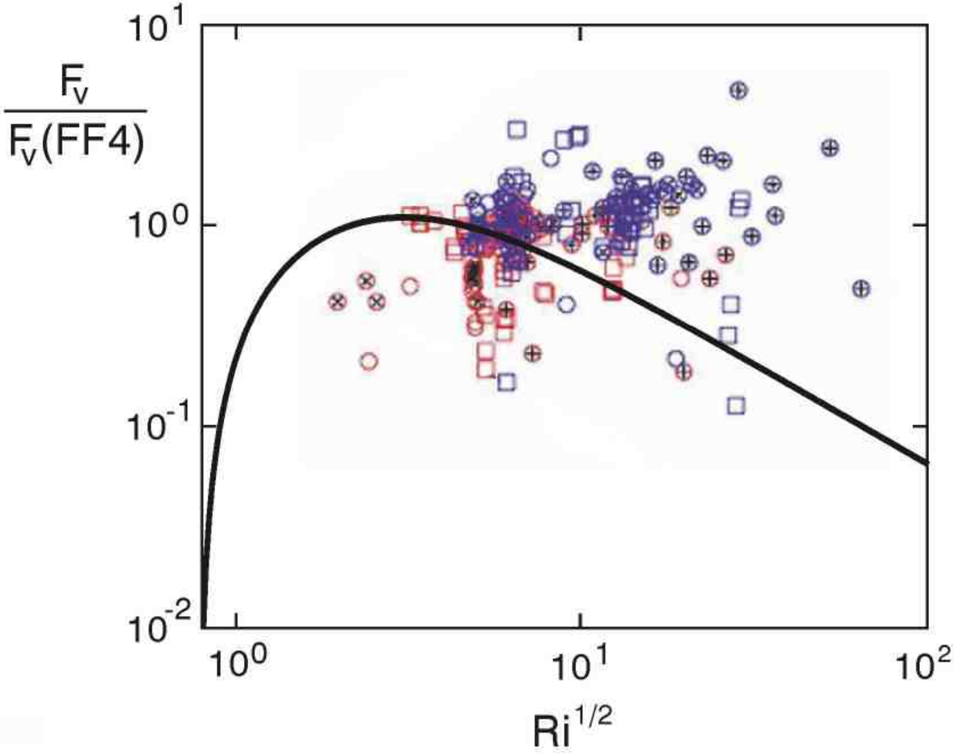

Fig.1. Ratio F_v/F_v(FFH) vs. Ri. The symbols correspond to Fig.(14e) of FFH, while the full curve corresponds to F_v given by (3b,c).

Fig. 3. A previous version of Figure 4 Canuto \& Dubovikov use here, also by those authors, showing the full range of Ri in the FFH simulations and profound disagreement with this theory at large Ri.

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