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Discussion

Reply to the comment by T.J. McDougall, P.R. Gent and S. Drijfhout

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Abstract

McDougall, Gent, and Drijfhout's [McDougall, T.J., Gent, P., Drijfhout, S.S., in press. Comment on "Dynamical model of mesoscales in z-coordinates" and "The effect of mesoscales on the tracer equation in z-coordinates OGCMs" by V.M. Canuto and M.S. Dubovikov. Ocean Modell. doi:10.1016/j.ocemod.2006.11.004, cited as MGD] opening sentence is that we have suggested that OGCMs have ignored a large diapycnal flux Σ in the mean buoyancy equation. As everybody knows, they have. The fact that in the Eulerian framework Σ exists and is large is uncontroversial. The real question is whether one should include Σ in OGCMs or whether there exists a formalism in which Σ does not show up in the mean buoyancy equation while at the same time the momentum equations are the same as the ones solved by OGCMs.

MGD present a defense of the TRM formalism which they claim does exactly that. The belief in TRM, they write, is widely spread among oceanographers and thus they cast our work as going against popular wisdom. Since we are not dealing with a popular context but with a scientific issue that is either correct or incorrect, the MGD claim about TRM must be scrutinized in detail. We have done so and our result, which we present in detail so that it can be checked, tested and verified, is that a key assumption concerning the TRM momentum equation is badly violated.

Specifically, when compared with the OGCMs mean momentum equations, the TRM version of the Eulerian mean momentum equations contain an additional term which MGD claim is smaller than other terms and thus negligible. By contrast, we show it is much larger than the non-linear terms. Our conclusion is that MGD's contention that TRM exonerates OGCMs from having to include Σ is true but incomplete. The question concerning the presence or not of Σ in the mean buoyancy equation remains unresolved.

A final comment may be of interest. MGD's lengthy and fervent defense of TRM is at odds with the fact that in our papers [Canuto, V.M., Dubovikov, M.S., 2006. Dynamical model of mesoscales in z-coordinates. Ocean Modell. 11, 123–166; Canuto, V.M., Dubovikov, M.S., 2007. The effect of mesoscales on the tracer equation in z coordinates OGCMs. Ocean Modell. 16, 17–27] we never criticized it since it had little overlap with our mesoscale model. On the other hand, the Comments by MGD forced us to dwell into TRM and what we found is that it does not do what the authors promised

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and the community, including us, took to be true. At the same time, none of the criticisms of MGD has any effect on any results we have derived in our work.

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1. MGD's assert that TRM makes modeling Σ unnecessary

The aim of this section is to prove that MGD's (McDougall et al., in press) assertion that TRM (McDougall and McIntosh, 2001, cited as MM1) makes the modeling of Σ unnecessary, is not the full story. Consider the TRM mean buoyancy equation:

$$\tilde{b}_t + (\overline{\mathbf{U}} + \mathbf{U}^+_{\mathrm{TRM}}) \cdot \nabla \tilde{b} = Q \tag{1a}$$

which does not contain Σ which appears in the Eulerian mean buoyancy equation (3a) below. Due to the similarity of (1a) with the OGCM equation,

$$\bar{b}_t + (\overline{\mathbf{U}} + \mathbf{U}^+) \cdot \nabla \bar{b} = Q \tag{1b}$$

it has been assumed that, if properly re-interpreted within the TRM formalism, the results of (1b) are correct. However, there is a constraint: the solution of the OGCMs horizontal momentum equations in which one has used the buoyancy field from the TRM buoyancy equation (1a), must be the Eulerian mean velocity. *Is this correct?* While MGD assume it is, we show it is not so by a wide margin. Consider the Eulerian OGCMs mean momentum equations:

$$\partial_t \bar{\mathbf{u}} + \bar{\mathbf{u}} \cdot \nabla_{\mathrm{H}} \bar{\mathbf{u}} = -\rho_0^{-1} \nabla_{\mathrm{H}} \bar{p} + \cdots \tag{1c}$$

where

$$\bar{p} = \rho_0 \int \bar{b}(z) \mathrm{d}z \tag{1d}$$

However, the TRM mean buoyancy equation provides:

$$\tilde{p} = \rho_0 \int \tilde{b}(z) \mathrm{d}z \tag{1e}$$

which does not coincide with (1d) needed in (1c). The difference of the two pressure terms causes the appearance of an *additional term* which changes (1c) to:

$$\partial_t \bar{\mathbf{u}} + \bar{\mathbf{u}} \cdot \nabla_{\mathrm{H}} \bar{\mathbf{u}} + \cdots = -\rho_0^{-1} \nabla_{\mathrm{H}} \tilde{p} - \underbrace{\nabla_{\mathrm{H}} W}_{\text{additional term}} \tag{1f}$$

where $W = -\frac{1}{2} g \rho_0^{-1} (\overline{\rho'^2} / \overline{\rho_z})$ is the eddy potential energy. If OGCMs included the additional term in (1f), the result would be the Eulerian mean velocity and the MGD's claim would be correct. However, when the last term is neglected as done in all OGCMs, the resulting mean momentum *is no longer Eulerian*. MGD try to solve the difficulty by claiming that the additional term is small and does not affect (1f). In particular, they claim it can be neglected since it is smaller than terms due to cabelling which are usually neglected. We find the opposite result (see below). However, there is a more general argument that reveals the importance of the additional term. If we take the ratio of the additional term to the non-linear term in the lhs of (1f), we obtain

$$\frac{\nabla_{\mathrm{H}} W}{\nabla_{\mathrm{H}} \cdot \bar{\mathbf{u}}\bar{\mathbf{u}}} \cong \frac{1}{2} \frac{W}{\mathrm{MKE}} \tag{1g}$$

where MKE is the mean kinetic energy. To compute (1g), we use three sources:

(a) the work of Boning and Budich (1992) tells that the maximum value of W/K_t (where K_t is the surface eddy kinetic energy given by Schmitz, 1996) is between 2.7 and 3,
(b) the work of Wunsch (1997) shows that W/K (K is the eddy kinetic energy) is about 4, and finally,
(c) the work of Stammer (1997, Fig. 4), based on the Topex/Poseidon data, shows that $K/\mathrm{MKE} \gg 1$.

On this basis, Eq. (1g) gives

$$\frac{\nabla_{\mathrm{H}} W}{\nabla_{\mathrm{H}} \cdot \bar{\mathbf{u}}\bar{\mathbf{u}}} \sim 2\frac{K}{\mathrm{MKE}} \gg 1 \tag{1h}$$

Therefore, the additional term in the mean momentum equations is much larger than the non-linear terms that are usually accounted for. We may further notice that in (1f) there should also be a term representing the mesoscale-induced Reynolds stresses which in their simplest and model independent form, give rise to a term of the form:

$$D_t\bar{\mathbf{u}} = \cdots - \underbrace{\nabla_{\mathrm{H}} W}_{\text{additional term}} - \underbrace{\nabla_{\mathrm{H}} K}_{\text{Reynolds Stresses}} = \cdots - \nabla_{\mathrm{H}}\mathrm{TEE} \tag{1i}$$

where $\mathrm{TEE} = K + W$ is the total eddy energy whose dynamic equation is

$$\frac{\mathrm{DTEE}}{\mathrm{D}t} = \mathbf{F}_{\mathrm{H}} \cdot \mathbf{L} + \cdots = F_{\mathrm{v}} - \Sigma + \cdots \tag{1j}$$

where F_{v} is the vertical buoyancy flux $F_{\mathrm{v}} = \overline{w'b'}$.

Let us now consider the MM1-MGD argument about cabelling. Another way of looking at the additional term in (1f) is by recalling that the difference in the pressure terms corresponds to a difference in the densities which, as shown in MM1, amounts to

$$\delta\rho_{\mathrm{TRM}} = -\frac{1}{2}(\overline{\rho'^2}/\bar{\rho}_z)_z \tag{2a}$$

MM1 argue that since there is a term of the form:

$$\delta\rho_{\mathrm{cab}} = \alpha_{\Theta}\overline{\rho'^2} \tag{2b}$$

due to cabelling (caused by the non-linearity in the equation of state of seawater) which is usually neglected in all OGCMs and which is larger than (2a), there is no reason to worry about the latter. Since we were unable to find an explicit proof of such a statement, in either McDougall and McIntosh (1996) or MM1, we have done so. As shown in Appendix A, we have obtained the following results (corresponding to different depths):

$$\frac{\delta\rho_{\mathrm{cab}}}{\delta\rho_{\mathrm{TRM}}} < 0.13, \quad \frac{\delta\rho_{\mathrm{cab}}}{\delta\rho_{\mathrm{TRM}}} < 0.25 \tag{2c}$$

Thus, contrary to what MM1 claim, $\delta\rho_{\mathrm{TRM}}$ exceeds $\delta\rho_{\mathrm{cab}}$ and the neglect of $\delta\rho_{\mathrm{cab}}$ cannot justify the neglect of $\delta\rho_{\mathrm{TRM}}$.

MGD also briefly argue that the additional term "spatially average to zero". Since it enters the momentum equations under ∇_{H}, when one averages it, the result tends asymptotically to zero as ℓ^{-1}, where ℓ is the scale of the averaging process. However, the asymptotic regime begins at $\ell \gg \ell_0$ where $\ell_0 \sim 10^3$ km is the characteristic scale of variation of the mean fields. However, at scales ℓ, all the terms in the mean momentum equations also vanish. In fact, the pressure term which also appears under ∇_{H}, as well as the Reynolds stresses and the mean non-linear terms, have the same asymptotic behavior; the same is true of the Coriolis term which is generally balanced by the pressure term. Thus, the asymptotic behavior of the additional term is irrelevant to the problem of whether such a term is negligible or not.

The previous arguments show that the TRM assumption that the solution of (1f) without the additional term is still the Eulerian mean velocity, is incorrect. It follows that TRM cannot be used to justify the OGCMs neglect of Σ whose inclusion or not in the mean buoyancy equation remains an open problem.

2. MGD assert that Σ is a purely diabatic term

The aim of this section is to disprove MGD's assertion that Σ represents only diabatic processes, a problem that entails basic considerations of physics. We begin with the model independent, Eulerian mean buoyancy equation (Treguier et al., 1997):

$$\bar{b}_t + (\overline{\mathbf{U}} + \mathbf{U}^+) \cdot \nabla \bar{b} + \frac{\partial \Sigma}{\partial z} = Q \tag{3a}$$

where the eddy-induced velocity $\mathbf{u}^+$ and the diapycnal flux Σ are defined as

$$\mathbf{u}^+ \equiv -\partial_z(\overline{\mathbf{u}'b'}/\bar{b}_z), \quad \Sigma = \bar{b}_z^{-1}\overline{\mathbf{u}'b'} \cdot \nabla_{\mathrm{H}}\bar{b} + \overline{w'b'} \tag{3b}$$

For numerical estimates of Σ, see Gille and Davis (1999, Figs. 8 and 10) and Henning and Vallis (2005, Fig. 6). MGD basic assumption is that Σ represents only diabatic processes, an assumption that we represents as follows:

$$\Sigma \iff \text{Diabaticity} \tag{3c}$$

In fact, MGD write that: "*The mean density* Eq. (3a) *has a diabatic term* $-\Sigma_z$" and furthermore, "*because of the presence of the* $-\Sigma_z$ *term*, Eq. (3a) *will not preserve the adiabatic property*". Later, they criticize us for the "*diabatic nature of the approach*" (CD6: Canuto and Dubovikov, 2006; CD7: Canuto and Dubovikov, 2007) and add that we have built a model that must be "*diabatic even though the same flow in density coordinates is adiabatic*".

We have done nothing of the sort. The incorrect characterization of our work is rooted in (3c), namely in MGD's belief that the presence of Σ violates adiabaticity. Below (see Proof) we show that exactly the opposite is true. Specifically, we prove that any flux across any surface consists of two components: the first is the flux across the surface taken at a given moment in time, i.e., across "frozen" surfaces, while the second component is due to the time variation of the surface itself. Adiabaticity requires that when we consider isopycnal surfaces, the total flux must be zero.

In *isopycnal coordinates,* isopycnal surfaces are fixed planes (although they vary in physical space) and therefore any flux across such planes has only the first (diapycnal) component. Thus, in isopycnal coordinates the diapycnal flux in an adiabatic regime must be zero. However, *in z-coordinates*, both contributions exist and to ensure adiabaticity, their sum must vanish. Since by definition Σ is the component across frozen isopycnals, it *must* be non-zero in order to cancel the second component so as to ensure adiabaticity. Therefore, we reach exactly the opposite conclusion of MGD: *Σ is instrumental to ensure adiabaticity while MGD claim that its presence violates adiabaticity*.

Proof. To show that Σ is compatible with an adiabatic regime, consider the velocity w_{s} across a moving surface:

$$w_{\mathrm{s}} = (\mathbf{U} - \mathbf{U}_{\mathrm{s}}) \cdot \mathbf{n} \tag{4a}$$

where $\mathbf{U}$ is the fluid velocity, $\mathbf{U}_{\mathrm{s}}$ is the velocity of the surface and $\mathbf{n}$ is the unit vector normal to the surface. For an arbitrary scalar φ, the flux $F(\varphi)$ across this surface is given by

$$F(\varphi) = \overline{w_{\mathrm{s}}\varphi} = \overline{(\mathbf{U} - \mathbf{U}_{\mathrm{s}}) \cdot \mathbf{n}\varphi} \tag{4b}$$

where the overbar represents an average over a sub-grid cell. From (4b) it follows that the flux is the sum of two fluxes:

$$F(\varphi) = \overline{\mathbf{U} \cdot \mathbf{n}\varphi} - \overline{\mathbf{U}_{\mathrm{s}} \cdot \mathbf{n}\varphi} = \underbrace{F(\varphi)}_{\text{froz. surf.}} + \underbrace{F(\varphi)}_{\text{var. surf.}} \tag{4c}$$

The first term represents the flux across *frozen surfaces* while the second represents the flux due to the *variation in time* of the surfaces themselves. Clearly, the above relations are valid in any system of coordinates. Next, we specialize them to the case of isopycnal surfaces which are characterized by the following variables:

$$\mathbf{n} = \mathbf{k} + b_z^{-1}\nabla_{\mathrm{H}} b \quad \mathbf{U}_{\mathrm{s}} = -\mathbf{k} b_z^{-1} b_t \tag{5a}$$

It follows that

$$\mathbf{U}_{\mathrm{s}} \cdot \mathbf{n} = -b_z^{-1} b_t, \quad \mathbf{U} \cdot \mathbf{n} = w + b_z^{-1}\mathbf{u} \cdot \nabla_{\mathrm{H}} b \tag{5b}$$

Next, we choose $\varphi = N^{-2} b_z b'$ ($N^2 = \bar{b}_z$). Substituting the results in (4c), the two fluxes become

$$F_b(\text{froz. surf.}) = \Sigma + N^{-2}\left(\overline{\mathbf{U}} \cdot \nabla \frac{1}{2}\overline{b'^2} + \frac{1}{2}\overline{\mathbf{U}' \cdot \nabla b'^2}\right)$$
$$F_b(\text{var. surf.}) = N^{-2}\partial_t \frac{1}{2}\overline{b'^2} \tag{5c}$$

where the diapycnal flux Σ was defined in (3b). Since in an diabatic regime fluxes across isopycnal surfaces vanish, we have $F(\varphi) = 0$. Using (4c) and (5c), we obtain:

$$\text{Adiabaticity}: F(\varphi) = 0 \rightarrow \partial_t \frac{1}{2}\overline{b'^2} + \left(\overline{\mathbf{U}} \cdot \nabla \frac{1}{2}\overline{b'^2} + \frac{1}{2}\overline{\mathbf{U}' \cdot \nabla b'^2}\right) = -N^2 \Sigma \tag{5d}$$

which is the well-known dynamic equation for the *buoyancy variance* (Treguier et al., 1997). This is an interesting result in and by itself.

The terms in brackets represent advection and diffusion of the buoyancy variance whereas the right-hand side represents its production. MM1 have argued that the diffusion term is negligible in comparison with advection since the former is a third order term in fluctuating fields. Thus, Eq. (5d) becomes

$$\text{Adiabaticity}: F(\varphi) = 0 \;\rightarrow\; \partial_t \frac{1}{2}\overline{b'^2} + \overline{\mathbf{U}} \cdot \nabla \frac{1}{2}\overline{b'^2} = -N^2 \Sigma \tag{5e}$$

Next, we discuss the balance among the three terms in (5e) and here is where we strongly disagree with MGD. We do so by presenting both numerical and physical considerations which begin with Eq. (6a) below. The result is that the second term in the lhs of (5e) is smaller the rhs and thus, going back to (5c), Eq. (5e) can be rewritten as

$$F_b(\text{var. surf.}) + F_b(\text{froz. surf.}) = 0, \quad \Sigma = F(\text{froz. surf.}) \tag{5f}$$

Eq. (5f) says that the adiabaticity requirement of zero flux can be achieved only if Σ is not zero.

Of course one can claim that the first of (5f) is also satisfied when both terms are zero. This would be mathematically correct but physically incorrect. In a strictly adiabatic regime, dissipation of energy is absent while production is not, being always positive. This implies that the eddy energy grows and the tendency term in (5e) is always positive. In turn, this implies that the first term in (5f) is positive (no stationarity). To satisfy the first of (5f), one therefore needs a non-zero and negative Σ. Thus, Σ is not only compatible with an adiabatic regime but is instrumental to satisfy the zero flux condition.

The balance in (5e). Let us consider (5e) rewritten as

$$\partial_t W + \overline{\mathbf{U}} \cdot \nabla W = -\Sigma \tag{6a}$$

Eq. (6a) shows that $-\Sigma > 0$ acts as the source of potential energy. Let us compare the various term in (6a). First, we have

$$\overline{\mathbf{U}} \cdot \nabla W \sim 10^{-10}\ \text{m}^2\ \text{s}^{-3} \tag{6b}$$

where we have used $\overline{U} \sim 10^{-2}$ m s^{-1}, $\nabla \sim 10^{-6}$ m, $W \sim 10^{-2}$ m^2 s^{-2} (if we took W ten times larger the rhs side would be 10^{-9}, see below). Next, since we know that the rate of production of total eddy energy (kinetic + potential) given by ($\Sigma = F_v - \mathbf{F}_H \cdot \mathbf{L}$ where $\mathbf{L}$ is the slope of the isopycnals, 10^{-3}):

$$P_T(\text{m}^3\text{s}^{-3}) = \int \text{d}z \mathbf{F}_H \cdot \mathbf{L} \sim H\Sigma \tag{6c}$$

must be such that (Wunsch, 1998):

$$\rho A P_T \sim 1TW \tag{6d}$$

where A is ocean's surface area, it follows that

$$\Sigma \sim \frac{1TW}{\rho A H} \sim 10^{-8}\ \text{m}^2\ \text{s}^{-3} \tag{6e}$$

Thus, (6e) is larger than (6b) even in the largest W case and it does not seem possible to balance the rhs of (6a) but with the tendency term, that is, via the relation:

$$\omega W = \Sigma \tag{6f}$$

where ω represents the growth rate. It is interesting to note that if one uses $\omega = 1/3LN$ derived from the Eady model, Eq. (6f) yields:

$$\Sigma \approx 10^{-8}\mathrm{m}^2\,\mathrm{s}^{-3} \tag{6g}$$

which is consistent with (6e). Furthermore, to get a feeling about the magnitude of Σ, consider Eq. (3a) and compare $\mathbf{U}^+ \cdot \nabla\bar{b}$ vs. $\partial\Sigma/\partial z$. We obtain

$$\frac{\mathbf{U}^+ \cdot \nabla\bar{b}}{\Sigma_z} \sim \frac{U^+ N^2 L}{H^{-1}\Sigma} \sim \frac{U^+}{10^5\Sigma} \sim 10^3 U^+ \tag{6h}$$

This ratio is O(1) for $U^+ \sim 10^{-3}$ m s^{-1}, which is of the right order. Thus, the eddy-induced velocity and the Σ_z term with Σ given by (6g), are of the same order. Therefore,

(1) MGD's assertion that the balance is between Σ and the advection term $\overline{\mathbf{U}} \cdot \nabla W$, is not borne out by the previous calculation.
(2) Gille and Davis (1999) showed that in the Eady case, which is physically close to the picture we had in mind (no dissipation), there is no stationarity and their Fig. 10 show that the tendency term is balanced by Σ, and thus (6f) is indeed satisfied.
(3) The model for the GM diffusivity (Visbeck et al., 1997) that achieved the largest "skill" of about 40% (which is much better than the 10% of the horizontal diffusivity models) is based on the Eady model with the growth rate modeled based on the work of previous authors (e.g., Green and Stone). Actually, it was precisely those Eady-based computations that steered us toward our model (6f).
(4) The stationary case advocated by MGD can be valid only when sinks are included in (6a). In fact, Gille and Davis (1999) showed that in the shear driven case, which is no longer representative of an adiabatic regime, stationarity sets in, but this is to be expected since having changed the physical conditions of the problem, the balance of the various terms in Eq. (6a) also changes.

In conclusion, the key difference between our interpretation and the one by MGD hinges on different physical interpretations of the implications of adiabaticity vs. stationarity: *MGD assume that stationarity is possible within an adiabatic regime whereas we believe that is physically incorrect*. Clearly, our interpretation of Σ as given by (6f) is reflected in the model that was developed in CD6. To assess our model of Σ, we ask the reader to compare Fig. 4 of CD6 with Fig. 10 of Gille and Davis (1999) and to assess the predicted behavior of Σ_z of Fig. 6b of CD6 to consult Fig. 6 of Henning and Vallis (2005). The overall signs are in accord with those of our model. It must be pointed out that in the real ocean there is dissipation and exact adiabaticity is not satisfied. The model of Σ on the basis of (6f) presented in CD6 assumed that the violation of adiabaticity is sufficiently small not to invalidate the results we obtained. □

3. Tapering and baroclinicity

MGD criticize us for relating the tapering for $\mathbf{\Psi}$ to baroclinicity of the eddy-induced velocity and write that "the near surface tapering of the quasi-Stokes stream function $\mathbf{\Psi}$ is not a device to ensure that the eddy-induced velocity has no depth integrated transport". Actually, it is trivial to show that the tapering of $\mathbf{\Psi}$ results in the baroclinicity of the eddy induced velocity. In fact, since

$$\mathbf{u}^+ = \partial_z \mathbf{\Psi} \tag{7a}$$

due to the boundary conditions $\mathbf{\Psi}(0, -H) = 0$, it follows that the baroclinicity condition:

$$\int_{-H}^{0} \mathbf{u}^+(z)\mathrm{d}z = 0 \tag{7b}$$

must be satisfied by any model of $\mathbf{u}^+$. If one adopts Eq. (12) of MGD, that is, the GM model $\mathbf{\Psi} = -\kappa_M \mathbf{L}$, it follows that the tapering of $\mathbf{\Psi}$ requires the tapering of $\kappa_M(z)$, that is, $\kappa_M(0) = 0$ which contradicts numerous studies cited in CD6, as well with the most recent work by Marshall et al. (2006).

As an aside, we recall that the derivation of the eddy-induced velocity presented in CD6 Eqs. (4) automatically satisfies (7b) without having to impose it and that comes about because the form of $\mathbf{u}^+$ is no longer a simple GM form but it contains three additional terms. The latter not only do not require $\kappa_M(0) = 0$ but imply that the maximum of $\kappa_M(z)$ occurs at the surface, in agreement with the conclusions of several studies (e.g., Ferreira et al., 2005; Marshall et al., 2006).

As independent proof for the need of additional terms to GM, it must also be noted that using an eddy resolving code, Bryan et al. (1999) concluded that the GM form is incomplete and that the missing terms "are not related to the gradient of thickness", which is what our model predicts it should be the case.

4. Advection–diffusion balance

In this context, MGD raise a non-issue since the multiple terms in the diapycnal upwelling in z-coordinates were discussed by us *only* in connection with the work done by previously authors (Radko and Marshall, 2004a,b) who did not have a model for such terms and were forced to use a heuristic expression. Our CD6 model provided a closure for each of those terms. If anything, MGD should criticize the authors just cited rather than us.

5. Mixed layer, bottom boundary layer and TRM

In the mixed layer (ML) and in the TL (transition layer), TRM cannot be used and like the isopycnal models, it must be substituted with an Eulerian, z-coordinate model. In addition, since evidence is accumulating about the importance of tidal dissipation at the ocean's bottom and since the shear produced by tidal currents is much larger than the one due to the mean flow, the shear will be correspondingly larger, implying a smaller Ri to the point that a tide-generated BBL (bottom boundary layer) will have to be constructed, something that isopycnals models are unsuitable to handle. The GISS vertical mixing scheme has recently been extended to account for these processes (Canuto et al., submitted for publication-a).

Thus, both top/bottom of the ocean, where these driving processes operate, will have to be treated with an Eulerian, z-coordinates model since TRM cannot be used. Eqs. (3a) and (3b) will have to be solved and a model for Σ will be needed, that being the aim of CD6,7.

Concerning the mixed layer, no mesoscale model exists today to treat this regime whose importance stems from the fact that it is where the air–ocean interaction occurs and whose proper description is instrumental to achieve a credible coupled ocean–atmospheric model. Recently, we have analyzed two heuristic models and found they have severe internal problems. In particular, we found that the heuristic model that yields re-stratification is based on a stream function that violates parity conservation and it is unphysical. In that respect, MGD's assertion that "we are a long way from achieving a satisfactory parameterization of near surface mixing processes", is a correct reflection of today's state of the art in ML mesoscale modeling.

However, the CD6,7 model has been recently used to construct a ML mesoscale model. The results were successfully checked against data from an eddy resolving code (Canuto et al., submitted for publication-b).

6. The mesoscale model CD6,7

It must be stressed that the present discussion does not alter in any way any of the results of the mesoscale model presented in CD6,7 whose key results we reiterate for completeness.

(1) to the best of our knowledge, CD6,7 represent the only non-heuristic mesoscale model presently available,
(2) all relations are derived from the solution of the vertical eigenvalue problem to which the primitive mesoscale equations reduce,
(3) a key result is the derivation of the expression for the eddy induced velocity $\mathbf{u}^+$, CD6, Eqs. (4)(a–f),

(4) the model predicts that $\mathbf{u}^+$ contains four terms, the first of which has the structure of the GM model,
(5) while the GM model does not satisfy the baroclinicity relation (7b) for an arbitrary mesoscale diffusivity, the new model does it automatically, CD6, Eq. (4i),
(6) the expression for $\mathbf{u}^+$ contains non-local terms, namely integrals that involve the value of various fields over the entire water column,
(7) the model predicts a specific form for the mesoscale diffusivity that is in accord with a host of heuristic models, CD6, Eq. (4b),
(8) the model predicts the profiles of $K(z)$ and $W(z)$, kinetic and potential energies, in agreement with the data, CD6, Eqs. (5) and (7a),
(9) the model makes an explicit prediction for the eddy drift velocity and predicts that it is not the same as the mean velocity field, CD7, Eq. (6d),
(10) the model predicts the explicit form of the Reynolds stresses to be employed in the mean momentum equations, CD6, Eq. (12c). To the best of our knowledge, no other model has done so.

7. Conclusions

Contrary to the impression of our work that MGD try to give the reader, we have no reasons whatsoever to defend the presence of Σ in the mean tracer equation. It is an undisputed fact that in Eulerian z-coordinates, Σ exists, is large and must be modeled; in CD6,7 we offered and tested a model for it. Whether there is a way to rewrite the mean buoyancy equation so as to make Σ superfluous is an altogether different matter, one of great importance for OGCMs that ought to be clarified beyond any reasonable doubt. TRM was presented as the formalism that dispenses with Σ since the TRM mean buoyancy equation no longer contains Σ. That is true but it is only half of the story. The other part concerns the mean momentum equations in which there appears an "additional term" that must be negligible for the TRM claim with respect to Σ to be valid. The only fleeting discussion of such a topic in MGD is a repetition of what was written in MM1, namely that the "additional term" amounts to no more than a few % since it is smaller than a cabelling term that is ordinarily neglected. Since we were unable to find any explicit proof of such a statement, we checked it ourselves and found it to be the other way around, cabelling terms are smaller than the correction terms and the neglect of the former has no bearing in the discussion of the additional terms. That alone calls into question MGD's claim about the TRM formalism with regard to Σ. However, we have presented a new and much more general argument to show that the additional terms are even larger than the non-linear terms, a conclusion that in our opinion undermines the claim that TRM is the formalism that exonerates OGCMs from having to account for a diapycnal flux. If MGD's claim were correct, it would be greatly advantageous for OGCMs, but even then, only in the adiabatic ocean since in the Mixed Layer TRM does not apply to begin with. However, we were not able to confirm MGD's claim about the smallness of the additional terms which we have found instead to be very large.

We can only conclude that TRM does not do what MGD say it does and as of today, the question concerning the presence or not of Σ in the mean buoyancy equation remains unresolved.

Appendix A

The mesoscale induced correction to the density due the non-linearity of equation of state (EOS) is given by

$$\left(\frac{\delta\rho}{\rho_0}\right)_{\text{cab}} = -\frac{1}{2}\alpha_\theta\overline{\theta'^2} + \frac{1}{2}\beta_s\overline{s'^2} - \alpha_s\overline{\theta' s'} \tag{A.1}$$

where $\alpha_\theta \equiv \partial\alpha/\partial\theta$, $\alpha_s \equiv \partial\alpha/\partial s$, $\beta_s \equiv \partial\beta/\partial s$ and α, β are the thermal expansion and haline contraction coefficients. The variables θ', s' are due to the z-fluctuations of isopycnal surfaces (fluctuations within isopycnals are orders of magnitude smaller). Thus, to the leading order in the fluctuating fields, we have

$$\theta' = \bar{\theta}_z z', \quad s' = \bar{s}_z z' = \frac{\partial\bar{s}}{\partial\bar{\theta}}\theta' \tag{A.2}$$

where $\partial\bar{s}/\partial\bar{\theta}$ is computed along the vertical direction. To compare (A.1) with the TRM correction given in MM1:

$$\delta\rho_{\mathrm{TRM}} = -\frac{1}{2}\left(\overline{\rho'^2}/\bar{\rho}_z\right)_z \tag{A.3}$$

it is convenient to express (A.1) in terms of $\overline{\rho'^2}$ making use of the relation:

$$\frac{\rho'}{\rho_0} = -\alpha\theta' + \beta s' = -\left(\alpha - \beta\frac{\partial\bar{s}}{\partial\bar{\theta}}\right)\theta' \tag{A.4}$$

Substituting (A.2) and (A.4) into (A.1), we obtain

$$\left(\frac{\delta\rho}{\rho_0}\right)_{\mathrm{cab}} = \left(\alpha - \beta\frac{\partial\bar{s}}{\partial\bar{\theta}}\right)^{-2}\left(-\frac{1}{2}\alpha_\theta + \frac{1}{2}\beta_{\mathrm{s}}\left(\frac{\partial\bar{s}}{\partial\bar{\theta}}\right)^2 - \alpha_{\mathrm{s}}\frac{\partial\bar{s}}{\partial\bar{\theta}}\right)\frac{\rho'^2}{\rho_0^2} \tag{A.5}$$

both $\alpha_\theta, \beta_{\mathrm{s}}$ are positive and $\frac{\partial\bar{s}}{\partial\bar{\theta}} < 0$ in the largest majority of places in the ocean, one concludes from (A.5) that

$$\left|\left(\frac{\delta\rho}{\rho_0}\right)_{\mathrm{cab}}\right| < \frac{1}{2}\frac{\alpha_\theta}{\alpha^2}\frac{\rho'^2}{\rho_0^2} \tag{A.6}$$

To compare (A.6) with (A.3), we use $\partial_z \sim H^{-1}$, $H = 1$ km. Then

$$\frac{\delta\rho_{\mathrm{TRM}}}{\rho_0} \sim \frac{1}{2}gH^{-1}N^{-2}\rho_0^{-2}\overline{\rho'^2} \tag{A.7}$$

which implies that

$$\frac{\delta\rho_{\mathrm{cab}}}{\delta\rho_{\mathrm{TRM}}} < g^{-1}\alpha^{-2}\alpha_\theta HN^2 \tag{A.8}$$

For example, using the Levitus data, at a latitude 42.50S, the temperature of 1 °C considered in MM1 occurs at a depth of 3.7 km where $N^2 = 0.8 \times 10^{-6}\ \mathrm{s}^{-2}$. Using the UNESCO equation of state, we computed $\alpha = 0.88 \times 10^{-4}\ \mathrm{C}^{-1}$, $\alpha_\theta = 1.25 \times 10^{-5}\ \mathrm{C}^{-2}$ ($S = 35$ psu). Substituting these values into (A.8), we obtain:

$$\frac{\delta\rho_{\mathrm{cab}}}{\delta\rho_{\mathrm{TRM}}} < 0.13 \tag{A.9}$$

In addition, we also considered a depth of 250 m (same latitude), where $T = 8.5$ °C and $N^2 = 0.7 \times 10^{-5}\ \mathrm{s}^{-2}$. It follows that $\alpha = 1.83 \times 10^{-4}\ \mathrm{C}^{-1}$, $\alpha_\theta = 1.25 \times 10^{-5}\ \mathrm{C}^{-2}$ which implies that

$$\frac{\delta\rho_{\mathrm{cab}}}{\delta\rho_{\mathrm{TRM}}} < 0.25 \tag{A.10}$$

References

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

Bryan, K., Dukowicz, J.K., Smith, R.D., 1999. On the mixing coefficient in the parameterization of bolus velocity. J. Phys. Oceanogr. 29, 2442–2456.

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

Canuto, V.M., Dubovikov, M.S., 2007. The effect of mesoscales on the tracer equation in *z* coordinates OGCMs. Ocean Modell. 16, 17–27, cited as CD7.

Canuto, V.M., Howard, A.M., Muller, C.J., Jayne, S.R., submitted for publication-a. GISS mixing model: inclusion of tides and bottom boundary layer. J. Phys. Oceanogr.

Canuto, V.M., Dubovikov, M.S., Clayson, A.C., Luneva, M., submitted for publication-b. A mesoscale model for the mixed layer. Ocean Modell.

Ferreira, D., Marshall, J., Heimbach, P., 2005. Estimating eddy stresses by fitting dynamics to observations using a residual-mean ocean circulation model. J. Phys. Oceanogr. 35, 1891–1910.

Gille, S.T., Davis, R.E., 1999. The influence of mesoscale eddies on coarsely resolved density: an examination of sub-grid parameterization. J. Phys. Oceanogr. 28, 1109–1123.

Henning, C.C., Vallis, G.K., 2005. The effects of mesoscale eddies on the stratification and transport of an ocean with a circumpolar current. J. Phys. Oceanogr. 35, 880–896.

Marshall, Shuckburgh, J.E., Jones, H., Hill, C., 2006. Estimates and implications of surface eddy diffusivity in the Southern Ocean derived from tracer experiment. J. Phys. Oceanogr. 36, 1806–1821.

McDougall, T.J., McIntosh, P.C., 1996. The temporal-residual-mean velocity, part I: derivation and scalar conservation equations. J. Phys. Oceanogr. 26, 2653–2665.

McDougall, T.J., McIntosh, P.C., 2001. The temporal-residual-mean velocity. Part II: Isopycnal interpretation and tracer and momentum equations. J. Phys. Oceanogr. 31, 1222–1246, cited as MM1.

McDougall, T.J., Gent, P., Drijfhout, S.S., in press. Comment on "Dynamical model of mesoscales in *z*-coordinates" and "The effect of mesoscales on the tracer equation in *z*-coordinates OGCMs" by V.M. Canuto and M.S. Dubovikov. Ocean Modell. doi:10.1016/j.ocemod.2006.11.004, cited as MGD.

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

Radko, T., Marshall, J., 2004b. The leaky thermocline. J. Phys. Oceanogr. 43, 1648–1662.

Schmitz, Jr., 1996. On the World Ocean Circulation, vol. I–II, Woods Hole Oceanographic Institution, Tech, Rep., WHOI-96-03.

Stammer, D., 1997. Global characteristics of ocean variability from regional TOPEX POSEIDON altimeter measurements. J. Phys. Oceanogr. 27, 743–1769.

Treguier, A.M., Held, I.M., Larichev, V.D., 1997. Parameterization of quasi-geostrophic eddies in primitive equation ocean models. J. Phys. Oceanogr. 27, 567–580.

Visbeck, M.J., Marshall, J., Haine, T., Spall, M., 1997. On the specification of eddy transfer coefficients in coarse resolution ocean circulation models. J. Phys. Oceanogr. 27, 381–402.

Wunsch, C., 1997. The vertical partition of oceanic horizontal kinetic energy. J. Phys. Oceanogr. 27, 1770–1794.

Wunsch, C., 1998. The work done by the wind on the ocean general circulation. J. Phys. Oceanogr. 28, 2332–2340.