Representing Topographic Stress for Large-Scale Ocean Models

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ABSTRACT

Interaction of eddies with seafloor topography can exert enormous, systematic forces on the ocean circulation. This interaction has been considered previously under idealized circumstances. Theoretical results are here simplified and extended toward practical application in large-scale ocean circulation models. Among the suggestions is that coarse resolution models can "correct" a depth-independent part of the velocity field toward a velocity given by $-\mathbf{z} \times \nabla fLH$, where $\mathbf{z}$ is the vertical unit vector, f is Coriolis force, L is a length scale O (10 km), and H is the total depth. Absence of this tendency may be implicated in a number of systematic defects that appear in present ocean models.

1. Topographic form drag

Exchange of momentum between the ocean and the underlying earth may be one of the strongest, yet least well understood, forces acting on the ocean. Misrepresentation of this force (or omission thereof) can be implicated in a number of systematic defects that appear in large-scale ocean models. Purposes are a) to obtain a theoretical account of the force and b) to suggest simple ways to correct ocean models.

Vertical transfer of horizontal momentum is readily effected by differences of pressure acting on sloping bottoms. Expressed in spherical geometry, this leads to "mountain torque," which has been most studied in the context of atmospheric circulations, coupling the angular momenta of the atmosphere and the solid earth. Thus, it has been identified that atmospheric circulation models may suffer certain systematic defects due to omission of a component of topographic drag due to gravity wave excitation. The subject has not received as much attention in oceanography, perhaps because most oceans are interrupted by continental barriers. An exception is the Antarctic Circumpolar Current, for which Munk and Palmen (1951) already drew attention to the inferred role of topographic form drag. Even for an enclosed ocean basin, a corresponding question arises with respect to the relative angular momentum due to circulation within the basin. Both in the ocean and in the atmosphere, the effect of topography upon circulation may be very significant—perhaps more so in the ocean, where it is also less well understood (Holloway and Muller 1989).

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The topographic force may be separated into two parts: that is, gravity wave drag and vortex drag.

Gravity wave drag is due to resonant excitation of gravity waves within a wavenumber band from f/U to N/U, where f is Coriolis parameter, N is a characteristic stability frequency, and U is a free-stream velocity characterizing a "mean" flow above some boundary layer (Gill 1982, section 8.7–10). Because this excitation of gravity waves is not explicitly resolved in general circulation models, it has been parameterized for atmospheric models (Palmer et al. 1986; McFarlane 1987). It appears, however, that the corresponding gravity wave drag in the abyssal ocean may not be so significant because N/f is not large and U is small. Thus, only a narrow band of topographic variance at relatively high wavenumber is able to contribute to the gravity wave drag. It is interesting to speculate that gravity wave drag may be more important to coastal oceans where N/f is larger, U is larger, and there may be strong topographic variability, especially near the shelf break. However, in the present study, we turn to the second contribution: the vortex drag, which (as will be shown) turns out not to be a "drag" at all but rather may be one of the stronger forces *driving* ocean circulations.

At the outset, let us make a quick estimate of the possible amplitude of the vortex drag. Here and in what follows, we use Cartesian geometry for simplicity. Final expressions will be given in coordinate-free notation for application, for example, in spherical geometry. The horizontal force that topography exerts on an overlying fluid due to the pressure term (ignoring friction) is

$$\int dAp\nabla H. \tag{1}$$

To estimate the possible amplitude of stress (average force per unit area), we can simply multiply $p'(1/L')H'$ where p' is a pressure deviation (on geopotential surface), L' is a length scale for either pressure or depth fluctuation, and H' is a variation of total depth. Here p'/L' can be estimated from geostrophy as $\rho f v'$, where ρ is density, f is Coriolis, and v' is a characteristic velocity. If, for example, we choose $v' = 0.03$ m s^{-1} and $H' = 300$ m, then

$$\rho f v' H' \sim (10^3)(10^{-4})(0.03)(300) \sim 1 \text{ Pa}, \quad (2)$$

which is an enormous stress (should it occur over large areas on average). Typical wind stress is smaller by about an order of magnitude. Moreover, we might readily choose H' greater than 300 m, while $v' = 0.03$ m s^{-1} is only a moderate velocity. Yet, for many large-scale ocean models, H' of 300 m may not even be resolved while v' of 0.03 m s^{-1} may be omitted altogether (especially as these velocities are likely to be associated with mesoscale eddies that may be lost to subgrid scales or only marginally resolved). Thus, effects that may be entirely absent for ocean models can—in principle—exert forces on the actual ocean that may be an order of magnitude larger than any "usual" forces. In reality, this does not happen. (There would be nothing to balance the topographic force!) Evidently what does happen is that p and ∇H are not very well correlated. Yet, if that correlation is small (of order 0.1), the resulting force is still fully as large as any other mean forcing. Clearly, whatever physical dynamics governs the small correlation between p and ∇H, the same dynamics must be considered to be of order unity with respect to other forcings. Outright omission or haphazard relegation to some manner of "drag" parameterization threatens the fidelity of ocean models. Figuratively, one might say that a sort of "wind" blows on the bottom of the ocean—a wind that may be as strong as the surface wind but is neglected or grossly corrupted by large-scale ocean models. Efforts to correct this defect are clearly a priority. This paper will offer one suggestion along the way.

What to do? A natural and most common answer is higher resolution. How much higher? Although the length scales that dominate $p\nabla H$ may well occur in the larger scales of mesoscale eddies (Treguier 1989; Treguier and McWilliams 1990), a question remains as to how many free eddy interactions across a range of scales are necessary to realize the dynamically faithful correlation. Marginally resolved eddies that barely survive in the dissipative "tail" of an ocean model cannot be expected to perform faithfully. Moreover, in the case of climate research where the domain is global and the time scales of oceanic concern are decades and longer, adequate eddy resolution (whatever adequate might mean) poses an enormous computational cost—at direct debit to other climate questions one may wish to address.

Alternatives to higher resolution include 1) process-oriented numerical experiments (limited domains at high resolution), from which one might seek to discover an empirical parameterization, or 2) theoretical effort. Here we pursue the theoretical effort.

2. Idealized (quasigeostrophic) theory

Our goal is to address the urgent practical question of how to make realistic large-scale ocean models work better. In the approach to this goal, we shall be obliged to take leaps for which there is little careful guidance. We commence from a more idealized problem that we may examine with greater dynamical confidence. Then we take the leaps.

Consider a barotropic, quasigeostrophic ocean, either in the geometry of a reentrant zonal channel or with periodicity both in x (east) and y (north). Motion is defined by the vorticity equation

$$\partial t\zeta + \mathbf{z}\cdot\nabla\psi\times\nabla(\zeta + h + \beta y) = F_\zeta - D_\zeta, \quad (3)$$

where $\zeta = \mathbf{z}\cdot\nabla\times\mathbf{u} = \nabla^2\psi$ is the vertical component of relative vorticity, $\mathbf{z}$ is the vertical unit vector, ∇ is the horizontal gradient operator, $\nabla^2 = \nabla\cdot\nabla$, and ψ is the velocity streamfunction. A total depth of fluid $H(x, y)$ has been taken as $H = H_0(1 - (h(x, y) + \beta y)/f)$, where $h(x, y)$ may describe a roughness of the seafloor while a larger-scale bottom slope is included with βy, the β-plane representation of Coriolis parameter f (in this barotropic formulation). Here F_ζ and D_ζ represent external torques (wind curl, say) and any explicit dissipation operator that one might include.

Complete prescription requires also an irrotational component of flow, U, such that $\psi = \nabla^{-2}\zeta - Uy$. Evolution of U is given by

$$\partial_t U + \{\psi\partial_x h\} = F_U - D_U, \quad (4)$$

where braces denote area average over the flow domain, F_U represents external forcing of x-directed momentum, and D_U represents any explicit dissipation or drag assumed. Because ψ is proportional to pressure, one sees that $\{\psi\partial_x h\}$ is the flux of x-directed momentum due to pressure–slope correlations at the seafloor. From the view of subgrid-scale (SGS) modeling, we might seek prognostic equations for large-scale variables such as U, while parameterizing those contributions to $\{\psi\partial_x h\}$ from interactions of smaller-scale eddies with smaller-scale topography. The scale that will separate large from small will depend upon each modeler's interests and computing resource. Our goal here is to try to *permit* "large" to be as large as possible.

Interest to parameterize $\{\psi\partial_x h\}$ is at least twofold. Theoretical methods exist that allow one to make a direct assault on this term. And importantly, a seemingly strange outcome emerges. The specific analysis yielding $\{\psi\partial_x h\}$ has already been given (although incompletely) in Holloway (1987, hereafter H87), and will not be repeated here. That result is

$$-\int dA\psi\partial_x h$$
$$= -\mathrm{Im}\sum_{\mathbf{k}}\frac{k_x}{k^2}C_{\mathbf{k}} = \sum_{\mathbf{k}}\left[-\frac{\eta_{\mathbf{k}}+v_{\mathbf{k}}}{\omega_{\mathbf{k}}^2+(\eta_{\mathbf{k}}+v_{\mathbf{k}})^2}\frac{k_x^2}{k^2}UH_{\mathbf{k}} + \frac{\omega_{\mathbf{k}}k_x}{\omega_{\mathbf{k}}^2+(\eta_{\mathbf{k}}+v_{\mathbf{k}})^2}\frac{\gamma_{\mathbf{k}}}{k^2}H_{\mathbf{k}}\right], \quad (5)$$

where we have supposed that variables h and ζ have been Fourier transformed onto wave vectors $\mathbf{k} = (k_x, k_y)$. The net force is given by the sum over $\mathbf{k}$ of the imaginary part of the correlation $C_{\mathbf{k}} = \zeta_{\mathbf{k}}h_{-\mathbf{k}}$. Quantities within the brackets include $v_{\mathbf{k}}$, the Fourier representation of D_ζ, and $\omega_{\mathbf{k}} = k_x(U - \beta/k^2)$, the frequency of propagation of a linear wave at wave vector $\mathbf{k}$, including Doppler shift by the mean flow U. Here $\eta_{\mathbf{k}}$ and $\gamma_{\mathbf{k}}$ are given by expressions (see H87) involving sums of Z_p and H_p over all wave vectors $\mathbf{p}$, where $Z_{\mathbf{k}} = \zeta_{\mathbf{k}}\zeta_{-\mathbf{k}}$ and $H_{\mathbf{k}} = h_{\mathbf{k}}h_{-\mathbf{k}}$.

The aforementioned incompleteness of H87 involved inconsistently truncating certain terms to obtain an explicit expression for $C_{\mathbf{k}}$ at (5). A more thorough calculation has been carried out with supporting numerical experiments as reported by Zou and Holloway (1992). Although differences are found, for example, in energetics of $Z_{\mathbf{k}}$, corrections to (5) appear to be slight and do not affect the point to be made here.

In part, the result at (5) may be understood in a phenomenological way. One observes that the first term within the brackets is a drag (opposed in sign to U), given only that $\eta_{\mathbf{k}}$ and $\gamma_{\mathbf{k}}$ tend to be positive expressions (see H87). The drag is unsymmetric with respect to the sign of U because of the role of U in $\omega_{\mathbf{k}}$. When $U < 0$, the mean flow is in the same sense as intrinsic wave propagation, $U - \beta/k^2$ is large and drag is small. When $U > 0$, there will be k for which $U - \beta/k^2$ is small and the drag contribution will be relatively large. Thus, we recover a view of unsymmetric drag (as Haidvogel and Brink 1986). That view is extended at (5) to recognize that some drag still occurs for $U < 0$.

The unexpected part of (5) is the second term in square brackets. This is a contribution that takes the sign of $\omega_{\mathbf{k}}$ and hence may not oppose U at all. Indeed, for sufficiently large U, $\omega_{\mathbf{k}}$ will itself tend to take the sign of U, so that the term is an antidrag: *propelling* the mean flow. This seems strange, and clearly contradicts modeling practice!

A greater challenge arises if we ask how we could "correct" modeling practice following (5). The spectral information in (5) would itself need to be somehow parameterized, while the idealizations behind (5) may be so far from the physics of practical large-scale ocean models that the result is irrelevant. Nonetheless, the result (5) should not be ignored. Quite the contrary, I suggest that large-scale ocean models exhibit strong, systematic infidelities *because* of misrepresentation of (5). Infidelity is seen, for example, in underdevelopment of poleward eastern boundary current systems, underdevelopment of equatorward western boundary undercurrents, latitudinal overshoot of the Gulf Stream or Kuroshio, overdevelopment of transport by the Antarctic Circumpolar Current, lack of westward tendency along the Antarctic continental margin, and lack of eastward tendency along continental margins on the Arctic Ocean. Among other failures, it remains that (5) does not lend itself readily to practical incorporation into large-scale models. However, (5) does point toward a more fundamental view that may lend itself to modeling use.

3. Unprejudiced ocean circulation

We are stuck. With a good deal of effort we can obtain results such as (5) that can be shown to have modest skill when compared with direct simulations (including such influences as forcing and dissipation). However, the circumstances for which such solutions are tractable entail drastic idealizations: statistical homogeneity, quasigeostrophy, barotropy. If the goal is to contribute practically toward realistic ocean models, we appear to have a very long way to go. Nor is the direction clear. Let us instead take a detour that, at first, may seem very strange. The reader will be asked to suspend disbelief for just a little while. Then we will rejoin our earlier path strengthened with a clearer vision where this is leading.

Recalling the preceding section, we pause to ask: Why is the topographic stress so determined to *drive* the ocean in a certain direction? Consider a thought experiment. Take an ocean basin, hence $h(x, y)$. Randomly (as blindfolded) toss eddies into the basin. Assume we know nothing about any mean forcing applied to the basin. Then the objective is to predict what *mean* circulation will arise. It might appear that, since we do not know what the initial conditions are nor do we know what forcing (if any) is applied, we cannot predict any mean circulation at all. (Each realization of the experiment will have a circulation, of course; but we have no information about that circulation.) So is the answer none? That turns out to be mistaken, as we will see. We surely know something about eddies, random as they may be. An amount of eddy energy has been tossed into the basin, for example. We may have some statistical information, say about the length scales of eddies. Although we do not have specific information about phase relations among the eddies, it turns out that we can make a much better estimate of the mean state resulting from interactions among the random eddies.

The answer is nearly as simple as the case of a box of marbles, initially segregated with red marbles to left, blue to the right, then subjected to agitation. When one observes a rightward transport (on average) of red marbles, this result seems quite unremarkable. The distribution of marbles in the box is only becoming more uniform, that is, more "random," naturally. However, the ocean problem seems more difficult because of a conflicting intuition. We might suppose that an ocean that is full of eddies but lacks any mean motion is already about as random as possible. Having random eddies spontaneously organize definite, predictable flows (giving up eddy energy to those mean flows) may seem like generating order from chaos, contrary to the marbles experiment. It is the second intuition about random eddies that is wrong, as the following calculation illustrates.

Consider the discussion of quasigeostrophic flow but simplified further to omit external forcing and dissipation, including large-scale flow U within ψ and large-scale βy as part of h. Thus,

$$\partial t\zeta + \mathbf{z}\cdot\nabla\psi \times \nabla(\zeta + h) = 0. \qquad (6)$$

Let ψ and h be projected onto some set of basis functions chosen to suit geometry and boundary conditions. With computability in mind, suppose we retain some finite set of basis functions, possibly a large number (depending on the size of one's computer).

Now let us take into account that we do know something about the eddies. We may suppose we know the total energy

$$E = \frac{1}{2}\int dA|\nabla\psi|^2 \qquad (7)$$

of the eddy field at $t = 0$. We may have information about eddy length scales or spectra. This might be expressed in terms of total enstrophy

$$\Omega = \frac{1}{2}\int dA(\zeta + h)^2. \qquad (8)$$

[We suppose at $t = 0$ that ζ and h are uncorrelated (in ensemble mean) due to our blindfold in the thought experiment. Thus, at $t = 0$, $2\Omega = \{\zeta^2\} + \{h^2\}$. Length-scale information might be given, for example, as $L^2 = \{|\nabla\psi|^2\}/\{\zeta^2\}$.]

Special attention to E and Ω is motivated for two reasons. First, these quantities are invariants of the advective motion (6) within closed or reentrant domains. Second, these invariants are respected by numerical advection algorithms in many ocean models. Thus, with dynamics simplified to contain only advection, as (6), we can say that what knowledge we have is given only by E, Ω, and h. See *Note added in proof.*

Returning to the thought experiment, what shall we predict for the ensemble (over many random trials) mean flow $\langle\psi\rangle$? The problem is probabilistic. Defining a phase space spanned by the expansion coefficients of ψ on a set of basis functions, say, or by discrete values of ψ at points on a finite-difference mesh, the state of the model ocean is given by a state vector $\mathbf{Y}$ (simply the collection of dependent variables). Presumably, $\mathbf{Y}(t)$ describes some complicated trajectory for each realization of the experiment. Our interest, however, is in the probability distribution for possible $\mathbf{Y}$. Let $dp = p(\mathbf{Y})dY$ be the probability for finding the state of a model within phase element dY of state $\mathbf{Y}$. Then the entropy (or negative of "information") describing $p(\mathbf{Y})$ is

$$S = -\int dYp \log p. \qquad (9)$$

Given E and Ω and $h(x, y)$, maximization of (9) subject to (7) and (8) yields

$$(\alpha_1/\alpha_2 - \nabla^2)\langle\psi\rangle = h, \qquad (10)$$

where α_1 and α_2 are Lagrangian multipliers determined from E and Ω for given $h(x, y)$ and specification of the set of basis functions (or finite-difference grid). The result (10) was already obtained by Salmon et al. (1976) using a more complicated but more encompassing derivation. A brief argument for (10) is included in the Appendix. As well, (10) may be seen as an extension from Fofonoff (1954) with this difference: Whereas Fofonoff observed that (10) provides a solution among arbitrarily many possible solutions, entropy consideration indicates that this is the solution to be most expected if only eddies tend to conserve energy and enstrophy.

The most important remark is that we do not predict $\langle\psi\rangle = 0$. On the contrary, even when we commence from $\langle\psi\rangle = 0$ as in our thought experiment, eddy-topography interactions "spontaneously" organize $\langle\psi\rangle$ according to (10). The maximum entropy solution (10) describes a mean flow that expresses the minimal information content consistent with what we do know—E and Ω in this case. Any other solution for $\langle\psi\rangle$, in particular $\langle\psi\rangle = 0$, would purport to describe more information than is actually given. The "extra" information for which there is no actual basis is appropriately called "prejudice"; hence, (10) describes the "unprejudiced ocean circulations."

What does this contrived thought experiment with its information theoretic "stuff" have to do with the calculation of topographic stress in the previous section? And how is this to help with the practical problem of correcting actual ocean models?

First, the present discussion and that in the previous section are brought back together. The theory leading to (5) describes a part of the statistical dynamical tendency toward the state given by (10). It is shown (as in Carnevale et al. 1981) that statistical closure theories such as underlie (5) have the property that nonlinear interaction terms satisfy $dS/dt \geq 0$, driving the system toward (10). The topographic stress in (5) and much of the complicated derivation leading to (5) centers on estimating how *rapidly* the system is driven toward (10). The key is entropy, not just as an information theoretical idea but as the guiding principle for physical evolution of complex systems (Levine and Tribus 1979, and references therein).

Second, we see that this problem really is nearly as simple as the box of marbles. The more complicated variational calculation leading to (10) may be regarded as a technical detail due to the specific form of constraints (7) and (8). We are not surprised in the case of the marbles to observe a rightward transport of redness after commencing "rattling." Likewise, the occurrence of strong topographic stress, tending toward (10), ought not surprise us in the ocean. As an exercise in computational virtuosity, one might undertake a supercomputing simulation of the rattling of very many marbles in a very big box. Our goal instead is to apply insight from statistical dynamics to reduce the computational burden in ocean modeling, avoiding a supercomputer effort that would serve mainly to rattle a big box of marbles (or ocean eddies).

Third is a remark. Our nominal subject is the topographic stress (1). Solution (10) clearly depends upon $h(x, y)$. However, it is not the case that approach toward (10) *requires* topographic stress. So long as eddy advection tends to conserve E and Ω, the evolution will be toward (10) by any mechanism at all—such as lateral eddy transport, for example. Tendency toward (10) is what determines the sense of (5). However, the *primary* result is (10), which we will use in the following section to parameterize not only topographic stress but also more general eddy–eddy interactions.

Fourth, compare the practice of SGS parameterization in ocean models with the thought experiment previously described. In each case we know that unseen eddies are present. In the thought experiment, eddies generate a predictable mean flow. In the model SGS, we try to anticipate how eddies affect the larger-scale resolved flow. In practice, nearly every SGS has the tendency (if left to itself) to bring a model toward rest. In particular, if one imagined a model whose resolved flow was at rest, then the SGS leaves that resolved flow at rest. That is, in absence of nonzero mean flow, our guess at what eddies should do in the mean is: nothing! This is the very same wrong guess that we might have made in the thought experiment. It is the wrong guess that we do make in actual ocean models!

The following section discusses (speculatively) suggestions for SGS parameterization that surely will turn out not to be "right" but are not so conspicuously wrong as the more familiar tendency toward rest in usual SGS practice.

4. Guessing a better SGS

Our goal here is to proceed so far as possible without prejudice; that is, to attempt to proceed consistently with an entropy maximizing tendency. The difficulty is that the circumstances for which maximum entropy solutions are obtainable are far from oceanic reality. Reality is characterized by forcing and dissipation. The sun shines, wind blows, it rains, etc. However, the analogy with the box of marbles is apt. Although a segregation (red marbles to left, blue to the right) is far from maximum entropy (due to initial conditions), the ensuing tendency for rightward transport of redness appears "naturally." If we only observed (perhaps from numerical experiments) that red marbles tend to go to the right, then began to try to think of a strange force that somehow detects the color of marbles, applying a rightward push to red marbles, we would be quite misled. The mechanism here is a *statistical mechanism,* driven by the *difference* between initial conditions and maximum entropy.

Extending the marbles analogy further, we might imagine installing a gate at the right side of the box that would be color sensitive. Each time a red marble arrived at the gate, it would be removed and reinserted at the left. Now the box of marbles never does come to uniform color (in the mean), while the mean gradient of color depends on details such as how big is the gate, how vigorously we rattle the box, and so on. In this case there would be an enduring rightward flux of redness. Careful theoretical estimation of that rightward flux could be very demanding. Effectively, that is just the calculation mentioned in section 2, where the topographic stress arises because the system is not at maximum entropy. However, that careful calculation is 1) difficult and 2) quite restrictive in the idealized conditions for which it applies. What we now seek are shortcuts that are 1) easier and 2) more widely applicable.

Let us consider a number of apparent restrictions along with suggested ways to relax those restrictions.

First, the foregoing calculations are *barotropic;* the ocean is not. In fact, even in the original work of Salmon et al. (1976), a baroclinic (two-layer) derivation was given. Summarized simply, the result was that the maximum entropy state for motions on scales larger than the first deformation radius is barotropic. Large-scale baroclinicity of the actual ocean is a direct consequence of the nature of wind and buoyancy forcings applied at the surface of the ocean. Thus, if we seek to model the global ocean on grids that are coarser than the first deformation radius, then the SGS eddy *tendency* should be toward barotropy. For large-scale, long-time issues such as global climate, such coarse resolution is valuable. On the other hand, if one is motivated to model smaller scales, an SGS tendency toward increasing entropy should still be felt. The generalization in the case of a continuously stratified fluid is that flow structures should tend toward aspect ratio (vertical/horizontal extent) approximating f/N, where N is stability frequency due to stratification. In particular, topographic influences tend to be increasingly bottom trapped for shorter length scales.

Second, the calculations are quasigeostrophic. Comments: To the extent that we seek to represent the aggregate effects of SGS eddy advection, this may be a reasonable "approximation" over much of the extratropical ocean. Later we will see that the mean flow resulting from (10) should satisfy geostrophic balance rather well. The right side of (10), given by $h = f\delta H/H_0$, becomes small as $f \to 0$ approaching the equator. Also, the mean flow from (10) tends to follow isobaths so that finite amplitude topographic effects are not too disturbing. Of course one does recognize that higher-order dynamics occurs, ultimately leading to diabatic mixing. Ocean models, particularly those that address the time scales of climate change, must finally parameterize such higher-order interactions. For the present paper, we simply set aside such questions under the category "other."

Third, *finite amplitude topography* is a concern. As remarked, the flow tends to be along isobaths, so that quasigeostrophy is not too badly disturbed. However, quasigeostrophy presents us with certain ambiguities that affect practical application. There is a reference depth, H_0, whereas the depth of the ocean ranges from $H = 0$ at the shoreline to great depth in the abyss. There is a further ambiguity concerning the "stuff" called streamfunction. Does ψ describe a velocity streamfunction or a transport streamfunction for depth-integrated motion? To the order that quasigeostrophy obtains, interpretation of ψ is arbitrary. When applied to actual circumstances of finite variation of H, the consequences can be enormous. We will return to this point. For the moment, we observe only that if we took ψ to describe a *transport* streamfunction, then made application in the real ocean, we should encounter velocity magnitudes $|u| = |\nabla\psi|/H$ that diverge as $H \to 0$. With practicality in mind, we are motivated to read ψ as *velocity* streamfunction. See *Note added in proof.*

Fourth, there are *Lagrange multipliers* α_1 and α_2 to estimate in (10). This would appear to require that we have region-specific estimates of E and Ω as well as some idea about a dynamical truncation scale. Here we have really run afoul of the unphysical idealizations behind (10). However, we are also a little bit lucky. Actually we do not need α_1 and α_2 separately; only the ratio $\alpha_1/\alpha_2 \equiv 1/\lambda^2$ is needed, where λ has the dimension of length.

Does λ turn out to have a value of length that makes sense? From (10) we can estimate a mean flow speed $|u| \approx \lambda^2|\nabla h| \approx \lambda^2 fS/H_0$, where S is a bottom slope. With $f = 10^{-4}\ \mathrm{s}^{-1}$, $S = 10^{-3}$, and $H_0 = 3 \times 10^3$ m, say, values of λ like 3×10^4 m (30 km) yield equilibrium mean flow speeds like $3 \times 10^{-2}\ \mathrm{m\ s}^{-1}$. By no means does this make $\lambda = 30$ km "right." The observation is that choices of λ from several kilometers to a few tens of kilometers yield equilibrium flows that are not "crazy." We do not need λ of subatomic or supragalactic scales. However, of the various research topics that are certainly going to need more work (in part experience at application), foremost may be the question: What is λ? Plausibly, λ may reflect the shorter length-scale range of the mesoscale eddies. These are scales at which dissipative processes (energy-enstrophy cascade) are forcing the energy spectrum away from a more random equipartition form (Kraichnan 1967). It is encouraging that the spatial variation of λ will be much less than the separate variations of E or Ω, say. A suggestion for the present is to treat λ as a "fudge factor" (of just the sort that afflict eddy viscosity), for which a nominal value, say $\lambda = 10$ km, might make a first guess. Clearly this invites "refinement." One supposes that λ may have some dependence on latitude, perhaps tending to follow deformation radius scaling $\sqrt{g'H}/f$ although without such strong divergence toward $f \to 0$. On the other hand, models already suffer a plethora of fudge factors before admitting spatial variation of λ.

Fifth, a simplification appears. If the appropriate value for λ is indeed only as large as O (10 km) while our interest may be at larger scales O (100 km) or greater, as for global climate interest, then we can also drop ∇^2 in (10). We need not invert the elliptic operator on the left side of (10), and our theory of ocean circulation reduces to $\langle\psi\rangle = \lambda^2 h$. This might not be "right," but it may well be the "simplest-ever" theory of ocean circulation! Besides, it might be right—in part. (Clearly efforts to look at $\langle\psi\rangle$ at smaller scales—such as individual seamounts, near the shelf break, or near other topographic features (as in Cannon et al. 1991)—may bring back ∇^2. However, at such smaller scales, baroclinicity also should be considered. Let us here defer these questions to future research.)

Sixth, we may try to simplify further. As observed, λ is a fudge factor. As well, the choice of reference depth H_0 will be arbitrary to some extent—especially when one considers large domains. However, it is only the ratio λ^2/H_0 that appears. Since both numerator and denominator contain elements of arbitrariness, we might replace the ratio with a single length scale L', say, where L' is evidently of O (10 km) or larger. Then, since it is only $\nabla\psi$ that is physically significant, and observing that depth varies over length scales smaller than planetary radius, we can approximate the occurrence of h in (10) by $-fH/H_0$. The result is that our maximally simplified theory of the equilibrium *velocity* streamfunction is

$$\psi^* = -fL'H. \qquad (11)$$

Seventh, suppose that our object of interest is the depth-integrated transport streamfunction Φ, such that $H\mathbf{u} = \mathbf{z} \times \nabla\Phi$ rather than a velocity streamfunction ψ. Again we get lucky. Variation of ψ^* will tend to be dominated by variation of H, with much weaker dependence on f. In this case we can approximate the equilibrium *transport* streamfunction by

$$\Phi^* = -fLH^2, \qquad (12)$$

where $L \approx L'/2$ remains an adjustable length scale of O (10 km).

Having simplified the expressions for maximum entropy equilibria to (11) or (12), the idea will be that SGS should occur in ocean models in ways that move the model solution toward (11) or (12). Most important, SGS should not move ocean models toward *rest*. The SGS is just the analog of the rightward redness flux in the box of marbles. If we can make reasonable representations of this "redness flux" for ocean models, we can spare ourselves the enormous computational burden of explicitly "rattling the box."

5. Implementation

There are a wide variety of ocean models. This section offers illustrative suggestions for some of the more familiar model forms.

Most familiar may be prognostic models in which the velocity field $\mathbf{u}$ is obtained among dependent variables. Except for depth-integrated formulations, $\mathbf{u}$ will vary in all three spatial dimensions. Defining maximum entropy $\mathbf{u}^* = \mathbf{z} \times \nabla\psi^*$ from (11), SGS is expected to occur on account of the difference field $\mathbf{u}^* - \mathbf{u}$. If model resolution is coarse relative to first deformation radius, then $\mathbf{u}^*$ is independent of depth; however, the SGS following $\mathbf{u}^* - \mathbf{u}$ will be depth dependent. How shall SGS depend upon $\mathbf{u}^* - \mathbf{u}$? Without clearer guidance, one is disposed toward simplicity. Perhaps the most immediate idea is to append a relaxation term $(\mathbf{u}^* - \mathbf{u})/\tau$ to the right side of momentum tendency equations. We are then obliged to specify τ, which could have spatial and temporal variation. If done in this way, then the choice of τ should plausibly be guided by eddy advection time scales of perhaps a few tens of days for the entropy adjustment process, perhaps longer.

In fact, more detailed theories such as leading to (5) would suggest that the tendency to relax $\mathbf{u}^* - \mathbf{u}$ should be more rapid at shorter length scales. Moreover, the prognostic model may already include an explicit representation of eddy viscosity, say $A\nabla^2\mathbf{u}$. (If instead the eddy viscosity is buried within a nonconservative advection scheme or otherwise results from some filtering algorithm, then great care should be given to the effects of such "numerical diffusion" with respect to physical motivation.) When the explicit eddy viscosity is available, we see straightaway why the *viscosity* is "wrong": it tends toward *rest*. The immediate "fix" is to replace eddy viscosity by a form centered on $\mathbf{u}^*$, that is, $A\nabla^2(\mathbf{u} - \mathbf{u}^*)$. This is more scale selective than in the preceding suggestion, and also obviates the need to specify some τ. Of course there is still Austauch A to be specified, but that will be required either explicitly or implicitly in any case. Hence, this second approach has the further advantage of requiring fewer ad hoc specifications.

Other operators that occur in prognostic models may include a "hyperviscosity" ∇^4. The preceding remarks apply if only ∇^4 is caused not to act upon $\mathbf{u}$ but rather to act upon $\mathbf{u} - \mathbf{u}^*$. This operator is simply more scale selective than ∇^2. Choice between the two forms often hinges upon whether the model intends to be eddy resolving or not, with ∇^4 preferred in eddy-resolving models since it is less dissipative for those eddy structures rather larger than the grid scale. (Let me comment that the term "eddy resolving" can be misleading. A model with sufficient resolution, hence reduced damping, may permit eddies to exist—often only on the ragged edge of viscous extinction. By no means is one assured that such eddies are performing dynamically faithfully; they are just there. Of course, for some sufficiently high resolution, we suppose that numerical models approach actual ocean dynamics, only we do not know how high that resolution must be. Ultimately, at enormous supercomputing cost, we can numerically rattle the box of marbles.) For the present, let us suppose that modelers have already selected their operators. The important remark is that the operators should apply to $\mathbf{u} - \mathbf{u}^*$ rather than $\mathbf{u}$.

Models might not be formulated in momentum variables, but rather in vorticity/divergence, say. A maximum entropy vorticity $\zeta^* = \nabla^2\psi^*$ can be defined from (11), whereafter the preceding remarks in connection with momenta apply to vorticity. However, the "theory" that is based in quasigeostrophy remains mute with respect to horizontal velocity divergence.

A simple approach to model SGS is seen when a model formulation already separates velocity variables into baroclinic and barotropic parts, the latter given by solving a prognostic equation for evolution of a transport streamfunction Φ. In this case (at resolution coarser than first deformation radius) the baroclinic momentum field is unaffected and we revise the SGS only insofar as it affects Φ. This might take the form of an appended relaxation $(\Phi^* - \Phi)/\tau$ or might be more scale selective under ∇^2, say.

Certain questions will occur. One might observe, for example, that an operator upon $\mathbf{u} - \mathbf{u}^*$ will likely exert the strongest tendency in the upper water column where $\mathbf{u}$ may be largest and least guided by H. On the other hand, insofar as the nominal topic is topographic stress and $\mathbf{u}^*$ is just proportional to $\mathbf{z} \times \nabla fH$, it may be distressing that the effect is not strongest near the bottom. What needs be emphasized here is that the influence of topography ("Neptune effect"[1]) is being felt by the entire ocean as a statistical dynamical system. Circulations (11) or (12) approximate a maximum entropy system given a) eddies and b) topography.

One might inquire also about SGS insofar as a model may predict temperature (T) or salinity (S) fields. Pre-

[1] "Neptune effect" has been adopted as shorthand for referring to the statistical dynamical tendency of eddy–topography interaction to induce mean circulation. The term appeared in a cartoon (Holloway 1986b) that addressed why coastal currents might flow against wind, pressure head, or other apparent driving.

viously we have revised eddy viscosity to be centered on $\mathbf{u} - \mathbf{u}^*$. Is something similar implied for T and S? So long as our concern is at scales larger than first deformation radius, the answer is simple: T and S should relax toward horizontal uniformity, consistent with the barotropic character of $\mathbf{u}^*$. Old-fashioned eddy diffusion of T and S appears to have been a good idea after all. At smaller scales for which maximum entropy flows should exhibit baroclinic structure, density fields will require geostrophic adjustment.

Whereas the preceding discussion has addressed prognostic modeling, most of the comments carry over to inverse modeling and to mixed prognostic–diagnostic modeling. Indeed, our whole approach is based upon information theoretical constructs. If we knew nothing about the oceans except the shapes of ocean basins and some idea that eddies exist, then our best guess at ocean circulation should be (11) or (12). If we know anything else, like approximately how the wind blows, where the sun shines more brightly, where the air is cold, or where rivers discharge, then we can apply this knowledge—as in the forcing of prognostic ocean models. The result pulls our theoretical ocean away from (11) or (12), hence to some lower entropy state—which is just what the application of knowledge should do. Moreover, if we have made various direct observations of aspects of the ocean circulation, then we apply this knowledge in the inverse or diagnostic modeling. Application of direct knowledge about the ocean pulls our solution to lower entropy as is appropriate. The problem is that current *inverse-diagnostic* models do not commence from (11) or (12) but rather from rest (before insertion of any data at all). In this sense the models are strongly *prejudiced.*

The field least constrained by inverse calculations is the barotropic component of flow. Initial guesses at this component may be based upon ad hoc principles such as integrating Sverdrup balance along f/H contours from the eastern boundary (setting aside such nuisance issues as f/H contours actually commencing where the equator runs into the eastern shoreline, or the tendency for most f/H to close within the ocean basin). What is suggested here, depending upon the formulation of the inverse model, is either that the initial guess be given by (11) or (12) or that a penalty be assigned to the "distance" between the inverse solution and (11) or (12). In the case where distance from (11) or (12) is penalized, a question will arise as to what weight is assigned to this penalty. Whereas penalties on data mismatches may be estimated objectively (based on observed variability about mean data), the penalty weight for departure from statistical mechanical equilibrium must be more subjectively assigned. Although the maximum entropy method (unforced, nondissipative case) can provide information on higher moments, hence fluctuations about the mean, the reality is that those higher moments are so influenced by nonconservative processes that they appear to provide no useful guidance with respect to penalizing departure from (11) or (12).

An interesting opportunity occurs in assimilative models following the adjoint method, as in Tziperman and Thacker (1989). While a goal of such assimilation is to fit optimally a model solution to observations that are distributed throughout space and over time, the method also permits optimal adjustment of internal "control" parameters of the model. Whereas we have seen already that length scales L or L' are left as fudge factors, the possibility appears under adjoint assimilation to evaluate such fudge factors from direct observation, much as one makes adjoint evaluation of eddy viscosity. Or one may use this approach to answer whether the Neptune effect is a good idea at all. If there is a coefficient such as $1/\tau$ where the Neptune tendency has been appended to the dynamical model, then adjoint evaluation of $1/\tau$ tells us if the model is improved. If we find very small $1/\tau$, the effect should be left out.

6. Does it "work"?

This question will not be answered in this paper. Only experience in large-scale ocean models will make clear the value or lack thereof from the preceding suggestions. Idealized test cases certainly can be, and have been, set up in which one determines that a theory leading to (10) "works." Moreover, extended to forced-dissipative motions in model oceans with only planetary β, the tendency toward maximum-entropy solutions is exhibited, for example, by Griffa and Salmon (1989). However, *the* question is how relevant such tendency is with respect to correcting "realistic" ocean models. Because of the great deal of higher physics included in more realistic ocean models, it is hard to foresee what effects any change in parameterization will have. In fact, a number of colleagues who are involved in executing large ocean models have expressed interest to test the effects of including a Neptune effect parameterization. Some steps have been taken toward specific implementations. Outcomes are not yet in hand, though, and it should not be the place here to presage those colleagues' results.

Let us, however, indulge in some brief speculation in advance of the tests. Although it is difficult to foresee how complicated models will respond to any revised *tendency* terms in the models, what we can easily do is simply look at the equilibrium flows. Figure 1 shows $\mathbf{u}^*$ for the case of the extensively observed, extensively modeled North Atlantic. It should be borne in mind that Fig. 1 takes no account of such influences as wind, sun, or rain. Rather, Fig. 1 shows the state to which the SGS acting *alone* would draw an ocean model. The practical question is whether Fig. 1 is more or less plausible than the state of rest in terms of SGS ten-

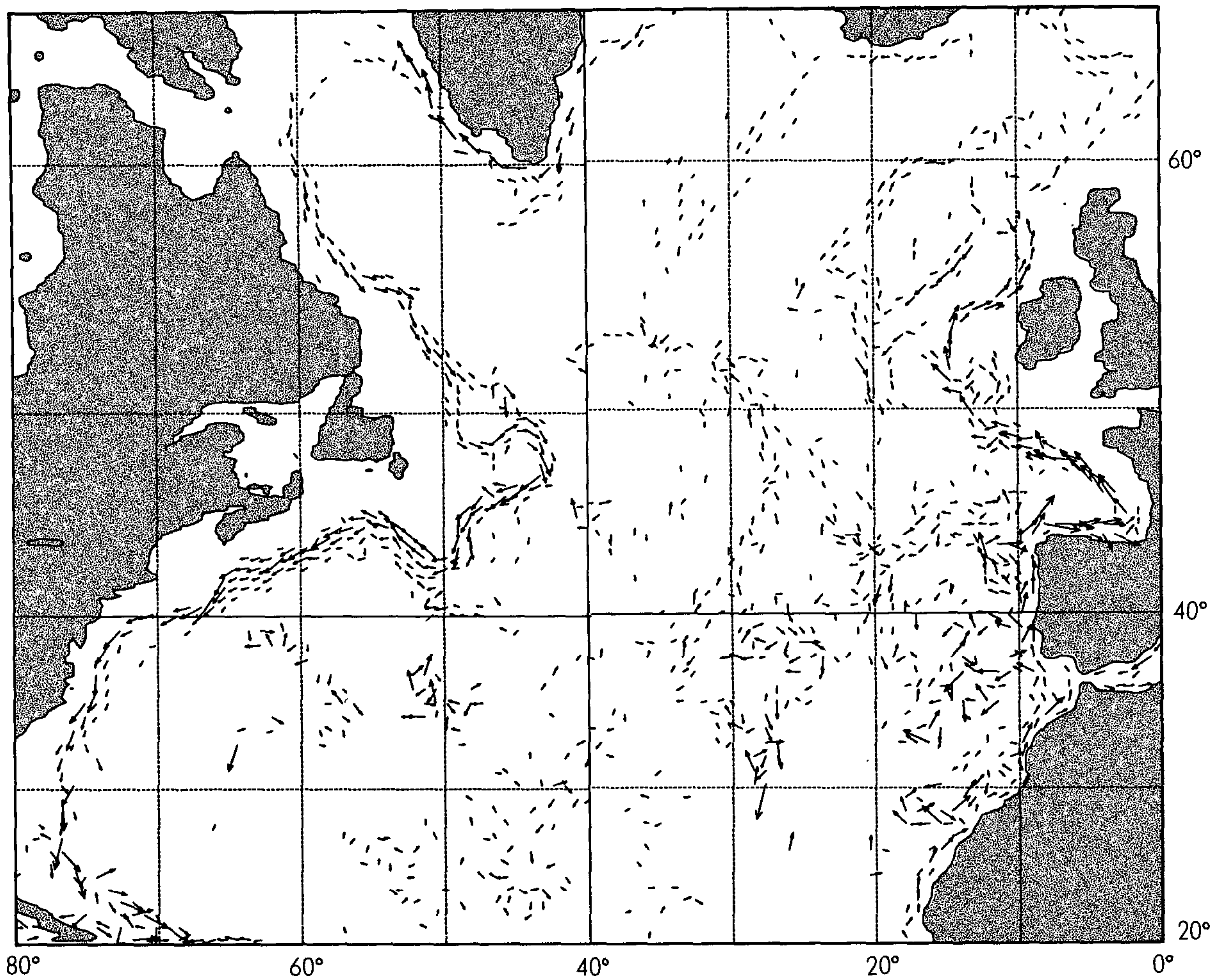

FIG. 1. Unprejudiced circulation is shown for the North Atlantic. Although overall magnitude of the flow depends upon uncertain length scales λ [cf. (10)] or L' [cf. (11)], the relative strengths of flows may be seen.

dency. That is, would appending a tendency toward Fig. 1 help ocean models work better?

The motion field shown in Fig. 1 should be regarded as nearly depth independent. The calculation from gridded 5′ topography (dataset "ETOPO5") has been subsampled to produce this figure, and the vector field is not otherwise smoothed. Thus, apparent "noise" in the vector field should be ignored. An impression at larger scales is all that is intended. If one sought to interpret smaller scales, (i) more care would be needed in data presentation, and (ii) depth trapping due to stratification would need to be considered. For the present we ask only: Does the big picture "make sense"? If yes, then successive refinements are invited.

The lengths of vectors in Fig. 1 indicate relative flow strengths. Because of dependence upon length scale L, absolute magnitudes are not assigned. Speeds from a few to a few tens of centimeters per second would result from plausible L. It should be borne in mind that Fig. 1 depicts a part of a tendency term rather than the prediction of flows per se.

What about Fig. 1? At once we notice that the Gulf Stream is going the wrong way. However, the Gulf Stream is usually understood—with some skill—to be an aspect of the directly forced, wind-driven ocean circulation. More quantitative details have depended upon numerical modeling. Without identifying particular model calculations that get particular details "more right" or "more wrong," I believe it is the case that realistically forced primitive equation ocean models systematically corrupt the Gulf Stream, insofar as the stream tends to hug the western margin to high latitudes around the Grand Banks and Flemish Cap before separating. From the view of climate interactions, consequences are bad indeed as the ocean models expose very warm water in regions where an atmospheric model should not expect such conditions. So, what is wrong? Any number of things can be "fiddled." Wind fields or surface buoyancy flux fields may need to be adjusted. Maybe eddy viscosities are not right. Inevitably, higher resolution is suggested. Yet it is difficult to see that great progress in the "separation problem" has resulted to date. (Ultimately we should imagine that sufficient resolution will get things right, but we cannot say how much more resolution will be sufficient. It can be remarked that in limited-area models with *prescribed* inflow/outflow conditions, the situation can be corrected, for example, by *imposing* a sufficiently strong southward undercurrent.) Figure 1 suggests another approach: forcing the model by the difference between modeled flow and Fig. 1. Will that "work"? It should just have to be tried to see. Plausibly a swift, narrow force "in the face" of the Gulf Stream may likely "trip" the stream.

A further manifestation of the Gulf Stream problem is that models appear not to develop adequate slope water penetration. This is the colder, fresher water that advances southward along the shelf break/upper slope region of the New England margin. Again it seems likely that the missing Neptune effect may account for this weakness of penetration. Corresponding defects are seen in Pacific Ocean models for which the northward Kuroshio penetrates too far against a weak Oyashio, whereas the Neptune effect should strengthen the Oyashio. From the view of ocean–atmosphere climate modeling, even minor infidelities affecting the Kuroshio–Oyashio confluence can have major impacts on heat exchange.

At greater depth in the North Atlantic, one observes equatorward flow along the western boundary (Hogg 1983). This may be due to high-latitude sinking contributing to global thermohaline circulation in the manner expected by Stommel and Aarons (1960). The problem again is to get the numbers "right" from large numerical models. Even when deep waters are being formed in the higher North Atlantic/Greenland–Norwegian Sea, the pathway for that water to enter the global ocean circulation appears to be ineffective in the models. Consequences of this defect can be seen in weak penetrations of transient tracers (C^{14}, freons) and global errors in middle-to-deep oxygen/phosphate ratios due to poor development of North Atlantic Deep Water. This raises a further concern. Although we understand that deep sinking at high latitude may drive equatorward flow along western boundaries, how much is such sinking the *cause* of the western boundary flows and how much is sinking only a way to "paint" property signatures onto a flow that would be there anyway? How much are the observed flows actually *falling* under a gravity head versus how much are high-latitude properties being *scavenged?*

At the eastern margin of the North Atlantic we observe poleward flow, a feature ubiquitous in eastern boundary undercurrents (Neshyba et al. 1989). A host of explanations have been offered concerning these ubiquitous features. To that host we add the Neptune effect. Models, within their bounds of resolution, may tend to get the poleward flow in the right direction at higher latitudes, but often mistake the sense of subsurface flow on the eastern limb of subtropical gyres. This happens in the North Atlantic off northwest Africa. Moreover, where model flows do obtain the right sense, transports often are too weak. In part this surely depends upon resolution. However, with respect to basin-scale geochemical budgets, results can be seriously afflicted. It may be encouraging that, by "correcting" the streamfunctions at (11) or (12), models could more nearly get eastern boundary transports "right" even when resolution is much too coarse to detect the topography of the continental margin. Of course this speculation, like those previously mentioned, must remain just as speculation awaiting actual modeling experience.

Two further remarks might be made from Fig. 1. Although the figure tends to be dominated (realistically?) by strong flows on continental margins while the abyssal flows are less clear (in part from discrete sampling), there is a tendency in each abyssal basin for flow of a cyclonic sense. Second, a poleward tendency over the western flank of the Mid-Atlantic Ridge should play some role in guiding property transports associated with Antarctic Bottom Water penetration.

When one's theory of ocean circulation is so simple—and cheap!—as at (11) or (12), one can readily go anywhere in the World Ocean to prepare figures such as Fig. 1 at computational cost no more than the cost of rendering the graphics. Consideration for page space dissuades here printing the *Whole World Ocean according to Max* (entropy). Let us only append some brief comments. Among eastern boundary undercurrents that sometimes prove troublesome for large-scale ocean models are (i) the California Undercurrent, (ii) poleward flow off southwest Africa, and (iii) the poleward Chilean Undercurrent.

With regard to western boundary currents, attention can be given to the western rim of the Argentine Basin insofar as one observes the northward entrance of Antarctic Bottom Water, whereas numerical models may suggest a less effective spreading of this water mass.

Two regions that bear special remark are a) the Antarctic, where it may yet be an observational question of how much flow over the continental margin tends westward, and b) the Arctic. The high Arctic tends to be dominated by polar high pressure as exhibited also in anticyclonic rotation of the ice pack. Model studies tend to carry this anticyclonic forcing into a general tendency toward anticyclonic circulation, particularly within the Canada Basin. Observations are sparse. However, a weight of indirect inference plus some direct observations (Aagaard 1981, 1989) point toward cyclonic circulation around the peripheries of both the Canadian and Eurasian basins, with a transpolar flow along the Lomonosov Ridge in just the manner one anticipates from the Neptune effect.

A final remark about circulations concerns lakes. There are extensive observations, lore, and explanatory literature that address the tendency for larger lakes in the Northern Hemisphere to exhibit cyclonic circulation (Emery and Csanady 1973; Wunsch 1973). Of course, smaller and shallower lakes will respond more directly to applied forcing. However, to the extent that a background cyclonic tendency is exhibited, one naturally anticipates the Neptune effect just as soon as any dynamical system exhibits more than a few degrees of freedom that are not simply "slaves" to direct forcing/dissipation.

7. Secondary circulation, upwelling

There is an aside, mentioned by H87, recalled here. Theoretical underpinnings for the Neptune effect have been obtained under quasigeostrophy, extended in ad hoc fashion to circumstances that do not meet the quasigeostrophic idealization. To the extent that one might incur small "corrections" to quasigeostrophic results, this may not be so dangerous. However, one aspect of special consequence as regards climate or marine productivity issues is a tendency toward up/downwelling at continental margins. This ageostrophic flow is linked to the larger-scale, quasigeostrophic flow. What we have seen is that the ocean "tries" to organize itself with a sense of cyclonic circulation around the basin peripheries. Indeed, among the historical "rules" of ocean circulation is a dictum that in the Northern Hemisphere flow is parallel to the coast with land on the right side of an observer facing downstream (Bigelow 1927; Huntsman 1924; Iselin 1955). What are the consequences of such flow in terms of vertical recirculation?

To whatever extent approximate geostrophy holds, this will tend to inhibit mean on/offshore motion in the middepth water column, insofar as mean on/offshore flows imply significant longshore pressure gradient. As well, longshore wind stress, driving on/offshore surface Ekman transport, plays its role in coastal up/downwelling. Both the longshore pressure gradient and the wind stress can be of either sign, without apparently favoring either onshore or offshore flow.

What about Ekman transport near the benthic boundary? If large-scale flows exhibit cyclonic (Neptune) sense around basin perimeters, benthic Ekman transport is offshore. A preferred sense of transport is established. As the deeper water column usually exhibits the highest nutrient burden, this transport is of the sign to tend to exhaust the coastal ocean of nutrients. (In the Arctic, this also affects the shelf-zone ice budgets that depend upon heat and salt from Atlantic layer water that occurs below shelf-break depth.) If upwelling favorable winds drive surface Ekman transport offshore, this aggravates the problem of setting up a compensating onshore mean flow at middepth. The Neptune effect appears to get us in trouble here, flying in the face of the manifest productivity of coastal oceans (and of Arctic ice climatology).

On the contrary, it may be just the Neptune effect that resolves the seeming dilemma. The topographic stress (1) is given by $p\nabla H$. Integrating by parts, the stress is reexpressed as $-H\nabla p$ while the total force includes contributions from pH evaluated along the boundaries of the domain of integration. Defining longshore coordinate x and offshore coordinate y with velocity components u and v, a tendency toward geostrophic balance will *tend* to yield $fv = \partial_x p$. The remark is that we *expect* (averaged over a long time) a positive correlation between v and $\partial_x p$. With H increasing in the positive y (offshore) direction, and $f > 0$, we expect the stress (1) to accelerate flow in the positive x direction, hence $H\partial_x p < 0$, hence $Hv < 0$. That is, there would appear to be a net *onshore* volume transport. Whereas frictional effects in benthic Ekman transport suggested a preferred offshore sense, here we find a preferred onshore sense. Moreover, the onshore volume transport is not supported by motions at middepth, since mean v at middepths should, by geostrophy, imply mean longshore pressure gradient. Whether there is a longshore pressure gradient (and of what sign) will be determined by a myriad of forcings. However, deeper in the water column, pressure gradients may be supported against topographic features so that little or no overall longshore pressure gradient accumulates over larger longshore distance. In this way it is the deeper water column that is forced onshore by Neptune, tending to compensate offshore benthic Ekman transport. While the sign of wind stress and of longshore pressure gradient will vary for different coastlines at different times, it appears that the topographic stress contribution will be *systematically* of the sense to force *onshore* flow in the *deeper* water column, upwelling the nutrients (and Arctic heat and salt) to close the dilemma set out previously.

The preceding argument has involved further leaps, extending a theory obtained under quasigeostrophic dynamics to address the ageostrophic secondary circulation in coastal zone dynamics with large-amplitude topography. These leaps are, I hope, plausibly motivated. It may be that high-resolution 3D primitive equation modeling and/or process-oriented field observations can be brought to bear in some more thorough account. Certainly it is the case that observations of nutrient-rich, oxygen-depleted waters near canyon heads are at least consistent with the *sense* of Neptune forcing as discussed previously. More thorough testing, isolating different mechanisms, remains to be done.

8. Comments

A host of objections can, and should, be raised. It is outside the mainstream of oceanography to bring in an idea like maximizing the system entropy. Casting about for ad hoc variational principles, we could come up with any number of hypotheses. However, we have repeatedly considered the illustration of the box of marbles in order to emphasize that maximizing entropy is not some "foreign" idea. Tendency toward increasing entropy is all around us, as our own existence is living evidence. This tendency is powerful, so much so that one might not be surprised when more conventional (deterministic) ocean dynamics are simply overwhelmed. A number of specific geophysical examples are reviewed by Salmon (1982) or Holloway (1986a).

Nonetheless, the question arises if one cannot see the results of previous sections from more familiar points of view. For example, it was remarked previously that asymmetric form drag in the presence of oscillatory forcing can lead to rectified flow (Brink 1986; Haidvogel and Brink 1986; Samelson and Allen 1987).

What about potential vorticity mixing? At middepths, maps of fN^2 show regions of weak gradients as well as regions with steep gradients (Keffer 1985). Potential vorticity mixing can generate mean flow (Rhines and Holland 1979) that may compensate changes in bottom topography. However, this cannot be done by altering just the stratification field because, if one weakens potential vorticity gradients in some depth range, they will usually be strengthened in another depth range. To remove the topographic "signal" one is obliged to resort to the relative vorticity contribution. Can that do the job? Depth changes $\delta H/H$ are order unity, implying mean-flow Rossby numbers U/fL of order unity. Although such mean-flow Rossby numbers can occur in very small and special areas, the O(1) depth changes $\delta H/H$ cover scales from tens of kilometers to basin scale. To obtain mean Rossby numbers of order unity over such broad areas would require mean flows of order 10 m s^{-1} or more over large regions. The ocean is not remotely so energetic. One may remark, however, that a *tendency* toward potential vorticity mixing has the same *sense* as the unprejudiced circulation.

A related hypothesis, advanced by Bretherton and Haidvogel (1976), is based upon selective decay of potential enstrophy $\frac{1}{2}(\zeta + h)^2$ relative to decay of energy $\frac{1}{2}|\nabla\psi|^2$. If dissipative processes remove enstrophy more effectively than energy, the tendency is toward a state of minimum enstrophy constrained by $h(x, y)$ and prescribed energy. The result is a unique, steady flow given by $(\alpha_3 - \nabla^2)\psi = h$. This is (10) except that just the one Lagrange multiplier α_3 appears due to the energy constraint. In fact, α_3 is the limiting case of α_1/α_2 when enstrophy takes its minimum attainable value. The minimum enstrophy solution is a special case of the family of maximum entropy solutions. From a utilitarian point of view, with either α_3 or α_1/α_2 treated as fudge factor, the motivation behind (10) could be called "academic." On the side of fundamentals though, one could address the rapidity of approach to maximum entropy relative to the time scale for selective decay, or that the maximum entropy mean flow coexists with a vigorous transient eddy field whereas the minimum enstrophy flow is the complete, steady flow. However, since one ultimately appeals only to *tendency* toward such conditions, this distinction may not have much practical significance.

There are other mechanisms. In particular, rectified tidal flows around banks are a consideration (Zimmerman 1978; Loder 1980). A question occurs: How much would those rectified flows be present even if the tides were not? Benthic mixing on sloping boundaries in a stratified, rotating fluid can force mean along-isobath flow (Garrett 1990). This catalog of mechanisms can be elaborated further. Sorting among the mechanisms in terms of their relative efficacy and parameter dependences will be an ongoing effort.

Quite another line of objection often arises. It can be said, and rightly so, that statistical dynamics describes isolated systems, whereas the role of direct forcing in ocean circulations is manifestly important. A companion objection is raised on the side of information theory: that the derivation of (10) is based upon presumption of no knowledge about ocean circulation apart from inviscid invariants of the motion. Of course, the oceans *are forced* and we do *know something* about that forcing. However, the present paper should not be read as a proposal to ignore what we know about the forcing of the oceans. Surely we should apply that knowledge as best we can, for example, in the forcing of ocean models. What the present paper says is that we do not know what the subgrid-scale eddies are doing. Haphazardly, we can sweep up all the eddies and call them eddy viscosity. Then we unwittingly *conjecture* that eddies try to drag flows toward a state of rest. Demonstrably, this is just not so. On the other hand, a complete account of what the eddies are doing is not available to our present understanding. Attempting to overcome in part this limitation, we have turned to statistical dynamics/information theory to ask in what "direction" eddies should tend to drag the flows. The answer we get is certainly "fuzzy"; clearly we are taking one step to be succeeded by steps to come as we refine our sense of direction. However, lacking courage to explore these steps is tantamount to a positive assertion on the side of eddy viscosity, that is, asserting that eddies move flows toward rest.

9. Summary

It is clear that eddy–topography interaction can, in principle, cause enormous, systematic forces to act in the ocean. The reality is not so clear. However, these forces (of whatever strength) are represented haphazardly if at all by large-scale ocean models. Finer eddy resolution is expected to help, though how fine the resolution and at what computational burden remain open questions. The statistical dynamical problem has been considered under idealized circumstances, while the extension from idealized theory to practical application involves a number of dangerous leaps. It does appear that a number of systematic defects in large-scale model output are of a sense such that correction for eddy–topography interaction may significantly improve the output. But large-scale ocean models are complicated, and guessing the response to any change invites speculation. It is hoped that the material set out here will provide motivation to experiment with these Neptune parameterizations.

Acknowledgments. Ideas in this paper have been stimulated in discussions over many years with Rick Salmon. This paper represents the author's effort to respond to the receipt (on two occasions) of boxes of marbles (from Myrl Hendershott and Hank Stommel) and to the receipt of one "red herring" award (from Ken Brink). I am grateful to Ken Brink, Denis Gilbert, Bill Boicourt, Josef Cherniawsky, and Josef Oberhuber for helpful comments and to Michael Eby, Keith Dixon, and Frank Bryan, who have shared preliminary model results. Research has been supported in parts by the Office of Naval Research (N00014-87-J-1262) and by the National Science Foundation (OCE-88-16366).

APPENDIX

Equation (10) was obtained by Salmon et al. (1976). That original derivation can be seen as complicated (but also more comprehensive than the calculation that follows). It has been suggested that the following brief calculation may provide a useful view of (10). See also Salmon (1982).

Consider (6) within a closed domain (or other simple domain such as a reentrant channel). Define eigenfunctions $\nabla^2\phi_n + q_n^2\phi_n = 0$ *with* ϕ_n satisfying the boundary conditions. Expand $\psi = \sum\psi_n(t)\phi_n$ and $h = \sum h_n\phi_n$. Conserved quadratics (7) and (8) are given by $E = \frac{1}{2}\sum q_n^2|\psi_n|^2$ and $\Omega = \frac{1}{2}\sum|-q_n^2\psi_n + h_n|^2$. Motion in the phase space defined by $\{\psi_n\}$ satisfies the Liouville property $\sum\partial(\partial_t\psi_n)/\partial\psi_n = 0$ where $\partial_t\psi_n$ is obtained from (6). It remains to seek the maximum of entropy $S = -\int d\mathbf{Y} p\log p$ subject to $\langle E\rangle = E_0$, $\langle\Omega\rangle = \Omega_0$ and $\langle 1\rangle = 1$ where angle braces denote expectation and the third condition assures normalization of the probability p. Introducing Lagrange multipliers α_1, α_2, and μ to meet the three constraints, set

$$\delta\int d\mathbf{Y}(p\log p + \alpha_1 Ep + \alpha_2\Omega p + \mu p) = 0,$$

hence

$$\log p + 1 + \alpha_1 E + \alpha_2\Omega + \mu = 0$$

or

$$\begin{aligned}p &= \exp\{-1-\mu\}\exp\{-\alpha_1 E - \alpha_2\Omega\}\\ &= \exp\{-1-\mu\}\exp\{-\textstyle\sum\{q_n^2(\alpha_1+\alpha_2 q_n^2)|\psi_n|^2 \\ &\qquad - 2\alpha_2 q_n^2\,\mathrm{Re}\psi_n h_n + \alpha_2|h_n|^2\}\}\\ &= \{\exp\{-1-\mu\}\exp\{-\textstyle\sum\{\alpha_2 - \alpha_2^2 q_n^2/\\ &\qquad (\alpha_1+\alpha_2 q_n^2)\}|h_n|^2\}\\ &\quad\times\exp\{-\textstyle\sum\{q_n^2(\alpha_1+\alpha_2 q_n^2)|\psi_n - \overline{\psi_n}|^2\}\\ &= \Gamma\exp\{-\textstyle\sum\{q_n^2(\alpha_1+\alpha_2 q_n^2)|\psi_n-\overline{\psi_n}|^2\},\end{aligned}$$

where $\overline{\psi_n} \equiv \alpha_2 h_n/(\alpha_1 + \alpha_2 q_n^2)$ and Lagrange multiplier μ has been absorbed into the normalization coefficient Γ. Then $\langle\psi_n\rangle = \overline{\psi_n}$. From $\overline{\psi_n}$ and the definitions of eigenfunctions ϕ_n, one has $(\alpha_1/\alpha_2 - \nabla^2)\langle\psi\rangle = h$, that is, (10).

Note added in proof: Since this paper was accepted for publication, on-going discussions with colleagues have continued to raise important issues. I'm grateful to Paola Cessi and Bill Young for their concern that entropy maximization should be constrained by more than just energy (7) and enstrophy (8) preservation. In fact the ideal dynamics (6) should preserve any function of $\zeta + h$, integrated over the domain, although discrete representation destroys most of these invariants. Nevertheless, circulation $\int dA\zeta$ is an invariant of particular interest which is violated by the Neptune parameterization suggested here. It is not clear what to do about this, apart from flagging the question as caution to reader. My speculation is that circulation will be much influenced by processes near, and inshore of, the shelf break region, and not of principle concern for the present paper. Based only on simplicity, I have been inclined to overlook this issue for the present purpose; it should remain a concern though.

Secondly, Michael Eby has further explored tests (to be reported) of the proposed parameterization. In the course of these tests, it has become clear that adopting a *velocity* streamfunction interpretation of ψ is not compelled (in actual numerical models) on the basis of avoiding singular $|u| = |\nabla\psi|/H$ for vanishing H. Adopting a transport streamfunction interpretation, replacing (12) by $\Phi^* = -fL^2H$, may yield superior model results.

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