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The Ocean Currents of Statistical Dynamics

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Abstract

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The Ocean Currents of Statistical Dynamics

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Abstract

Much can be learned by rattling a box of red and blue marbles. However, the statistics of ocean currents may be more complicated. On the other hand, maybe the currents are simpler than the marbles.

Forward

This paper employs the abstract used fifteen years ago. Only a third sentence is added.

Contents

1. Guessing at ocean circulations: numerical modeling

Attempts to understand ocean circulation usually take into account the forces that are applied to the ocean, whether from windstress, thermohaline interaction or tidal potential. One seeks to identify cause-and-effect relationships. As more realistic details of forcing are taken into account, utilizing more realistic geometry with more complete ocean physics, the search for cause-and-effect becomes more difficult, turning toward large computer modeling. One attempts to discover the cause of ocean circulations by solving Newton's dynamics for an "ocean" divided into as many "little masses" as one's computer can manipulate.

However, using even the most powerful computers to attempt to calculate the flow of the global ocean, the "little masses" are actually volumes of order 10^4 km^3 each containing more than 10^{41} molecules of water. Even if we supposed that individual molecules could serve as the particles to which F=ma might apply, we evidently await computers at least 41 orders of magnitude more powerful than present machines before we can carry out *this* program. Meanwhile, what to do? Simply: guess.

Guessing can occur on many levels. We can "guess" that on some scale smaller than is available to optical microscopy, molecular chaos leads to mixing of momentum, thermal energy, and ions of Na or Cl, for example. We might then guess that the Navier-Stokes equations describe some aggregate integration over Newton's dynamics. Even here though, we may be puzzled that Navier-Stokes are not symmetrical with respect to time-reversal whereas Newton is. Very seriously, oceanographers (and any other fluid dynamicisits) must bear in mind that the Navier-Stokes equations (which we may take as a starting point) are not derived from some law of physics; rather *these equations are simply a guess* about how collections of large numbers of molecules can be described. In any case, having disregarded Newton and moving a little up in scale, we next discover that our computer can't solve discretized versions of Navier-Stokes either, at least not for any volume of water larger than about 1 m^3 (this estimate based upon length scales associated with names like Kolmogorov and Batchelor). Evidently, more guessing is required. So we begin guessing at "turbulence parameterizations" such as may be applicable over scales smaller than about 1 m. Our skill at these guesses appears to be so poor that we leap instead to larger scales, say 1 km^3. At this scale we have even less reason to think we can guess worth beans, so we hasten to yet larger scale, like 10^4 km^3. Although there's no apparent reason to believe that guessing skill has got better, at least we've got to the scale where our modeled ocean fits into a big enough computer. Then we can shift attention to looking at the model output, leaving apart the sordid business about all the poor guessing along the way.

In making up all the turbulence or subgridscale parameterizations which are employed in numerical ocean models, we effectively make assertions about processes that we don't understand. We have to instruct the computer to do something, so we "make something up". Simply, we assert prejudices about what we guess goes on at subgridscales.

2. Guessing without prejudice (or a little less prejudice maybe)

There are other ways of guessing. One way is expressed through information theory. A key idea is to require that, if one has only limited information about something and is then asked to guess about further properties of that something, the ensuing guesses ought not purport to represent more information than one had at the outset. Claims to any further knowledge can only reflect prejudice on the part of the claimer. In this article, we seek an "unprejudiced" basis for guessing features of ocean circulation.

A formalism which seeks to clarify the basis for guessing only at the level of available information was founded by Shannon. If we ask "what's new?" since Vol. 1, No. 1 of J. Corr. Ocean., it may be this: that we see the theory of guessing (after Shannon) as a natural way to develop quantitatively the statistical dynamical subjects discussed in Vol. 1, No. 1. Indeed, that view is already clear in a paper by Rick Salmon in a volume entitled "Topics in Ocean Physics".

In brief schemata, the information-theoretic point of view can be posed in terms of an ocean state vector, say $\mathbf{\Phi}$. Components of $\mathbf{\Phi}$ might be the collection of dependent variables at all the grid points of some computer model. Or the components of $\mathbf{\Phi}$ might be all the quantum numbers to describe all the particles in the ocean. Now we admit that it is practically (even theoretically) impossible to observe $\mathbf{\Phi}$ (in reality) completely, while our ability to predict the evolution of $\mathbf{\Phi}$ is presumably worse yet. Maybe it doesn't even make sense to talk about $\mathbf{\Phi}$ *per se*. Instead consider some probability "density" $p(\mathbf{\Phi})$ for the occurence of actual $\tilde{\mathbf{\Phi}}$ within a small phase-space volume $\mathbf{d\Phi}$ near $\mathbf{\Phi}$. Then an entropy function for this system is given by $H \equiv -\int \mathbf{d\Phi} p(\mathbf{\Phi}) \ln[p(\mathbf{\Phi})]$. The objective is to apply whatever information one has (or *thinks* one has) about the ocean as constraints on p(Φ) while determining p(Φ) by the condition of maximizing H. That maximum entropy ("minimum information", "least biased" or "unprejudiced") solution is then the fairest guess we can make about $\tilde{\Phi}$. The application of this idea to physical systems, such as classical statistical mechanics, has been developed over decades in papers and books by Jaynes, Katz and others.

Regarding the large scale ocean circulation as an aggegate of many, many interacting components, it is natural to expect a tendency toward maximizing H. Concerned about the role of eddy advection in that aggregate interaction, one may seek to impose constraints that p(Φ) satisfy those properties which are conserved under eddy advection. Thus, we imagine that Φ is quite free to evolve in any fantastically complicated way only so long as that free evolution not violate overall conservation of, say, total energy. In this way, the attitude is no more complicated than if we imagine a lump of some material, initially warm in one part and cold in another. We naturally *guess* that such a lump, left in isolation, will come to uniform temperature throughout (although actual integration of F=ma for all the particles in the lump might quite tax one's super-computing resource). A further analogy is to the box of red and blue marbles, initially segregated red on left, blue on right. Upon rattling, we *guess* the distribution of red-blue will become *on average* more uniform despite our not having followed the exact trajectory of each marble. In what follows, we ask how well this simple analogy carries over to guessing states of ocean circulation without attempting detailed calculations of F=ma throughout the ocean.

3. Application to ocean dynamics

Various applications of this statistical mechanical / information theoretical approach to ocean dynamics have been developed by Rick Salmon, myself and others. Although these have appeared in sundry ordinary journals (with apology here to Correct Readers), the approach has been regarded as esoteric and unrelated to more serious problems in mainstream oceanography. Is it actually the case that the ocean currents of statistical dynamics are unrealistic? Purposes of this article are (1) to provide the Correct Readership with amusing maps and (2) to suggest methods for enhancement of conventional ocean models.

It may not be appropriate here to reproduce lengthy derivations. Neither does Correct Journals policy admit referencing ordinary journals. Let me only say that the following maps do not represent any new research; they are simply the evaluation of a formula published by Salmon *et al.* in 1976 in a journal whose initials are j.f.m.

a. Larger scale, extratropical circulations

If we attend only to extratropical ocean circulation on scales larger than100 km, say, one of the "standard guesses" is that an idealization to quasigeostrophic dynamics may be useful. Then one of the results of Salmon *et al.* is to show that, on scales larger than the first internal Rossby radius, maximum H favors more nearly barotropic motion. For simplicity here adopting barotropic quasigeostrophy, and including bottom topography h as elevation above a mean reference depth D, the solution for maximum H has mean streamfunction ψ given by $(L^2-\nabla^2)\psi=fh/D$, where ∇^2 is the horizontal Laplacian and f is Coriolis parameter. L^2 is a ratio of two Lagrange multipliers, resulting from the two constraints that ideal quasigeostrophy preserve total (domain integrated) energy $^1/_2\langle|\nabla\psi|^2\rangle$ and enstrophy $^1/_2\langle(\nabla^2\psi+fh/D)^2\rangle$. Values of the Lagrange multipliers (analogs of statistical mechanical "temperatures") depend upon total energy, total enstrophy and upon the identification of a scale of motion at which the assumed quasigeostrophic dynamics are conservatively truncated.

Just "to see what happens", let us simply choose an L. One might guess that L has something to do with the high wavenumber end of the eddy spectrum, admittedly a vague statement. In what follows, I've chosen an L that varies slowly with latitude, increasing from near 3 km at the pole to near 11 km at the equator. L is being considered here as a fiddle factor, for which one could as well have more simply taken L=10 km everywhere. Now, if our interest (when drawing global maps) is at scales much larger than L, we next notice that ∇^2 can be dropped from the formula for expected ψ. Then one may care to write $\psi=fh\lambda$ where

$\lambda \equiv (L^2/D)$. This relocates the ambiguity about specifying reference depth D, combining that with the uncertain choice of L, into a single fiddle-factor λ (with dimensions of length).

In a contest for simplest-ever-theory-of-ocean-circulation, I think $\psi = fh\lambda$ must be a candidate. At a few seconds cpu (on a plain computer) for global ocean calculation, the calculation is certainly "cheap". But is it any "good"? Correct Readers are invited to review this issue's special map supplement. A caution is required: It is easy to look at this map in the way one might regard output from a GCM. Owners / operators of GCMs may especially leap to see which is "better". Strictly, that does not make sense. The maps are *not GCM output*, and indeed pay no attention to such obvious matters as what forces the ocean. Rather the maps present the theoretical expression for a tendency term that may be substantially or wholly missed by coarser resolution GCMs.

One may be inclined to dismiss a theory of ocean circulation that is so naive as not even to care which way the wind blows. After all, *everyone knows* the Gulf Stream goes the other way! Just so. As well, almost everyone regards the Gulf Stream as a *surface intensified* manifestation of the *wind-driven* circulation. In reality, the wind blows, the sun shines, it rains sometimes, water freezes or melts sometimes, and the moon goes 'round. Such direct forcings are expected to override (in places and at times, depending upon their strength) the statistical mechanical tendencies. For the most part, direct forcings of the ocean are seen in the direct responses of the upper ocean. At greater depths, the internal statistical tendencies become more evident, as seen in such ubiquitous tendencies as poleward undercurrents along eastern ocean margins, equatorward undercurrents along western margins and a tendency toward cyclonic circulation around the periphery of marginal seas and lakes. In a good many cases, these observed undercurrents are opposed to the sense of apparent forcing -- a sometimes source for consternation. We only remark here that the occurence of such currents is fully as "natural" and as expected as the tendency for red and blue marbles, initially segregated, to tend toward more uniform mixture *on average*.

Strictly, the simple $\psi = fh\lambda$ is an *equilibrium* statistical mechanical result, applicable to a closed, isolated system. The oceans are coupled to the rest of Planet Earth and so on to the Universe. In principle, this requires a much more arduous (and uncertain) effort under the category of *disequilibrium* statistical mechanics. In part, such efforts have been carried out and reported in sundry ordinary journals (which will not be mentioned.) As a practical matter, let us here explore some shortcuts.

b. Practical synthesis with ocean GCMs

In gross, the direct response of the ocean to mean forcing is a tolerably manageable problem, appreciating that a substantial computing effort is still needed. The greater difficulty is to incorporate the role of eddies. Here let us imagine a synthesis: integrating gross effects of direct forcing in a large scale OGCM while including the effects of eddies by a *tendency-to-equilibrium* parameterization. This is especially relevant for questions concerning climate change, for example, in which the domain is global and integrations are required over substantial time intervals. It is essential to avoid the explicit computing of as much "detail" as one can (hopefully) omit. In particular, resolution coarser than the first Rossby radius may be attractive.

Two schemes come to mind. The first is to append a simple relaxation-to-equilibrium to one's momentum equations (or vorticity or other derived equation, depending upon model formulation). Thus one would write

$$\frac{\partial}{\partial t}\begin{Bmatrix}\mathbf{u}\\ T\\ S\\ \vdots\end{Bmatrix}=\text{Model}\begin{Bmatrix}\mathbf{u}\\ T\\ S\\ \vdots\end{Bmatrix}+\begin{Bmatrix}(\mathbf{u}^*(\mathbf{x})-\mathbf{u}(\mathbf{x},z,t))/\tau\\ \\ \\ \\ \end{Bmatrix} \tag{1}$$

where, for illustration, we've imagined model variables {**u**, *T*, *S*, ...}. "Model{.}" here collects whatever expressions one already has in the model. Equilibrium velocity $\mathbf{u}^*$ is given from ψ as $\mathbf{u}^*=-\lambda\hat{\mathbf{z}}\times\nabla fD$ and τ is a relaxation time constant which the user will choose. It is interesting that τ could be assumed to depend upon location, depth, season, ...; however, one is inclined first to simplify by taking a constant τ. We observe that $\mathbf{u}^*$ depends only upon horizontal position $\mathbf{x}$, whereas the model velocity $\mathbf{u}$ depends also upon vertical coordinate z and time t. In the expression for $\mathbf{u}^*$, we take the full depth D of ocean, while λ remains an adjustable length scale. One should note that $\mathbf{u}^*$ is obtained by taking ψ as a *velocity* streamfunction rather than depth integrated *transport* streamfunction. The quasigeostrophic theory from which the expression for ψ was obtained is ambiguous on this point. However, as we mean to use the calculation in a practical way within a numerical model whose D may range from greatest depth to zero, an assignment of ψ as trasnport streamfunction would lead to nearly singular $\mathbf{u}^*$ in shallow water.

Some disadvantages of the first scheme may be apparent. Away from strong topography such as continental margins, $\mathbf{u}^*$ will be weak and (1) will be dissipative of kinetic energy, with spin-down time τ. As well, the formulation in (1) is not so faithful to disequilibrium statistical mechanics in which small scales of motion can be expected to adjust toward equilibrium more rapidly. These two complaints might be met by a second scheme consisting of placing the relaxation process under a Laplacian:

$$\frac{\partial}{\partial t}\begin{Bmatrix}\mathbf{u}\\T\\S\\\vdots\end{Bmatrix}=\text{Model}\begin{Bmatrix}\mathbf{u}\\T\\S\\\vdots\end{Bmatrix}+\begin{Bmatrix}\upsilon\nabla^2(\mathbf{u}(\mathbf{x},z,t)-\mathbf{u}^*(\mathbf{x}))\\ \\ \\ \end{Bmatrix} \tag{2}$$

Many models will already include a lateral eddy viscosity, here υ. In any case it will be necessary by some means to remove velocity variance at near grid scale. All we do in (2) is that, rather than letting eddy viscosity move the flow toward rest, the eddy viscosity draws the flow toward $\mathbf{u}^*$. A caution here is that (2) introduces $\nabla^2\nabla D$ which will directly force at small scales. This may require some spatial smoothing of $\mathbf{u}^*$, whose influence then is felt under ∇^2.

Schemes (1) and (2) needn't be exclusive. They may both be present. The concern is for how many fiddle factors one wants. If one's model already includes υ, then use only of (2) avoids introducing τ.

c. Baroclinicity, and small-scale steep topography

Discussion in the preceding section is limited to large scales such that approximating statistical equilibrium by its barotropic component is justified. It should be very clear though that this does not attempt to approximate computed large scale flow as barotropic. **u**, T, S and any other model fields will exhibit baroclinic large scale structures in response to the nature of large scale forcing. It is only **u*** in the preceding section which is barotropic. [Let me here respond to another sometimes concern. It might be thought that such a wide range of D contradicts geostrophy. However, a nearly isobath-following **u*** of the amplitudes obtained for reasonable λ, does not significantly violate geostrophy.]

While one motivation is to improve the fidelity of large scale ocean models, such as for climate studies, we may wish to consider modeling at smaller scales -- such as the scale of an individual seamount, for example. In a domain of smaller scale, one might suppose that computing resource is sufficient to resolve adequately enough of the range of variability that subgridscale representation is not a major concern. Maybe. However, it is still a matter of economy and perhaps also of elegance to consider if some part of the solution can be supplied on theoretical grounds leaving the computer to deal with "book-keeping". At explicitly resolved scales already much smaller than the first Rossby radius, barotropic **u*** is clearly inappropriate. Neither is it necessary to be so restricted. Even in their first study, Salmon *et al.* already considered baroclinic statistical equilibrium within a two layer idealization. Extending such analyses to arbirary continuous stratification with realistic topography may be exhaustively demanding. Again let us here speculate about plausible short-cuts.

Analysis is simplified by transforming the vertical (pressure) coordinate z to a stretched vertical coordinate ζ given by $d\zeta=(N/f)dz$ where N(z) is stability frequency. The quasi-geostrophic motion field, including perturbation density, is fully defined by a 3D streamfunction $\psi(x,y,\zeta)$. Then the total energy (including available potential) and total enstrophy (including vertical potential) are given by integrals of x-y-ζ of $|\nabla\psi|^2$ and of $(\nabla^2\psi)^2$. For a domain unbounded in x-y-ζ, the constraining integrals of the motion appear to be "isotropic" in the coordinates, hence statistical equilibrium is isotropic in x-y-ζ. This result is entirely conistent with the preceding section where the real ocean is bounded by its top and bottom such that, at scales larger than DN/f, isotropy in x-y-ζ exhibits little variation in z. At smaller scales in the real ocean, a tendency toward maximum entropy (i.e. isotropy in x-y-ζ) suggests that eddy variability may realize approximate N/f aspect ratio in x-y-z.

What about realistic topography? A more serious theoretical effort is required, which I've not yet pursued. Let me only suggest the following *caveat emptor*. For topography of sufficiently small amplitude, we still solve $(L^2-\nabla^2)\psi=fh/D$ for velocity streamfunction ψ at the benthic interface. L^2 remains as before. However, we may now be interested in scales as small as, or smaller than, L; hence we may need to invert the elliptic operator $L^2-\nabla^2$ rather than discarding ∇^2. Having found ψ at the benthic interface, we extend ψ into the 3D (x-y-ζ) domain according to approximate isotropy. For example, if we are able to perform a horizontal Fourier transform of ψ at the benthic interface, then for each wavevector **k**, we may take the vertical penetration to decay as $exp\{-k\zeta\}$. When the results are mapped back to x-y-z coordinates, we observe that a tendency to capture an anticyclonic "Taylor cap" above a seamount is clearly expressed. This procedure is probably OK as long as the seamount is not so tall as to penetrate into the upper pycnocline. The problem is this: Mapped to x-y-z, where is the level reference for z? Practically, it may "work" best to take z=0 as the actual benthic interface, grossly deformed as it is. Only experience at applications may finally sort this out. In any case, we obtain a 3D ψ^* from which we obtain both a 3D $\mathbf{u}^*$ and a 3D density perturbation ρ^*. The suggestion then is that a numerical model of flow in the vicinity of the seamount could incorporate relaxation terms involving both $\mathbf{u}^*$ and ρ^*.

Of great practical concern is the role of statistical equilibrium tendency for coastal zone oceanography. The effect of continental margins has already been seen in this issue's special map supplement. However, the maps are drawn at coarse scale for which $\mathbf{u}^*$ should be barotropic. Examined in closer detail, we would observe that the strongest equilibrium tendency is usually found just seaward of the shelf break. Often a significant stratification exists in waters overlying the shelf break. Therefore, in higher resolution numerical models of the coastal zone, one expects to see the strongest u^* overlying the upper slope, with diminished influence above the pycnocline. Of course the numerical model will include direct forcing by wind or buoyancy. Likewise, the numerical model can impose $u=0$ at the benthic interface although u^* is greatest at the bottom.

d. Secondary circulation and coastal zone productivity

Although most of our attention so far has addressed the mean horizontal circulation, there is an important remark concerning also the induced secondary (vertical planar) circulation. We have seen that statistical equilibrium flows are such as keep the coastline to the right of the flow (in the northern hemisphere). That actual coastal flows tend to do this is so well known that it is a part of "classical" oceanography more than a half century old.

Coastal zones are also observed to support high productivity. Sometimes that productivity may be associated with upwelling-favorable winds. However, the productivity usually characterizes coastal zones even when the climatological mean winds do not especially favor upwelling. One might identify an apparent paradox: If the observed flows agree with the "classical" rule of keeping the coast to their right, then the benthic Ekman transport is preferentially downslope. As this water near the base of the water column is usually highest in nutrient, the result should be to exhaust the coastal zone, limiting productivity. Is there a conflict between the sense of longshore circulation and the nutrient budget (also oxygen, heat and salt budgets in some cases)?

Resolution of this depends upon understanding the disequilbrium processes which are responsible for evolution toward statistical equilibrium. In particular I have investigated a "topographic stress" (reported in 1987 in the same ordinary journal mentioned earlier with initials j.f.m.) In terms of secondary circulation, the topographic stress appears as an upslope "pumping" in the lower portion of the water column. This is manifest near canyons which often are observed as sources for nutrient enriched (oxygen depleted) waters. In cases where the mean winds favor neither upwelling nor downwelling, and no mean longshore pressure gradient acts, the longshore flow is expected to achieve a balance between topographic stress (tendency toward statistical equilibrium) and frictional retardation. Then the upslope pumping by topographic stress just balances the downslope Ekman transport. Although this balance does not, by itself, favor coastal productivity, the balance does prevent the exhaustion of the coastal zone due to Ekman benthic transport.

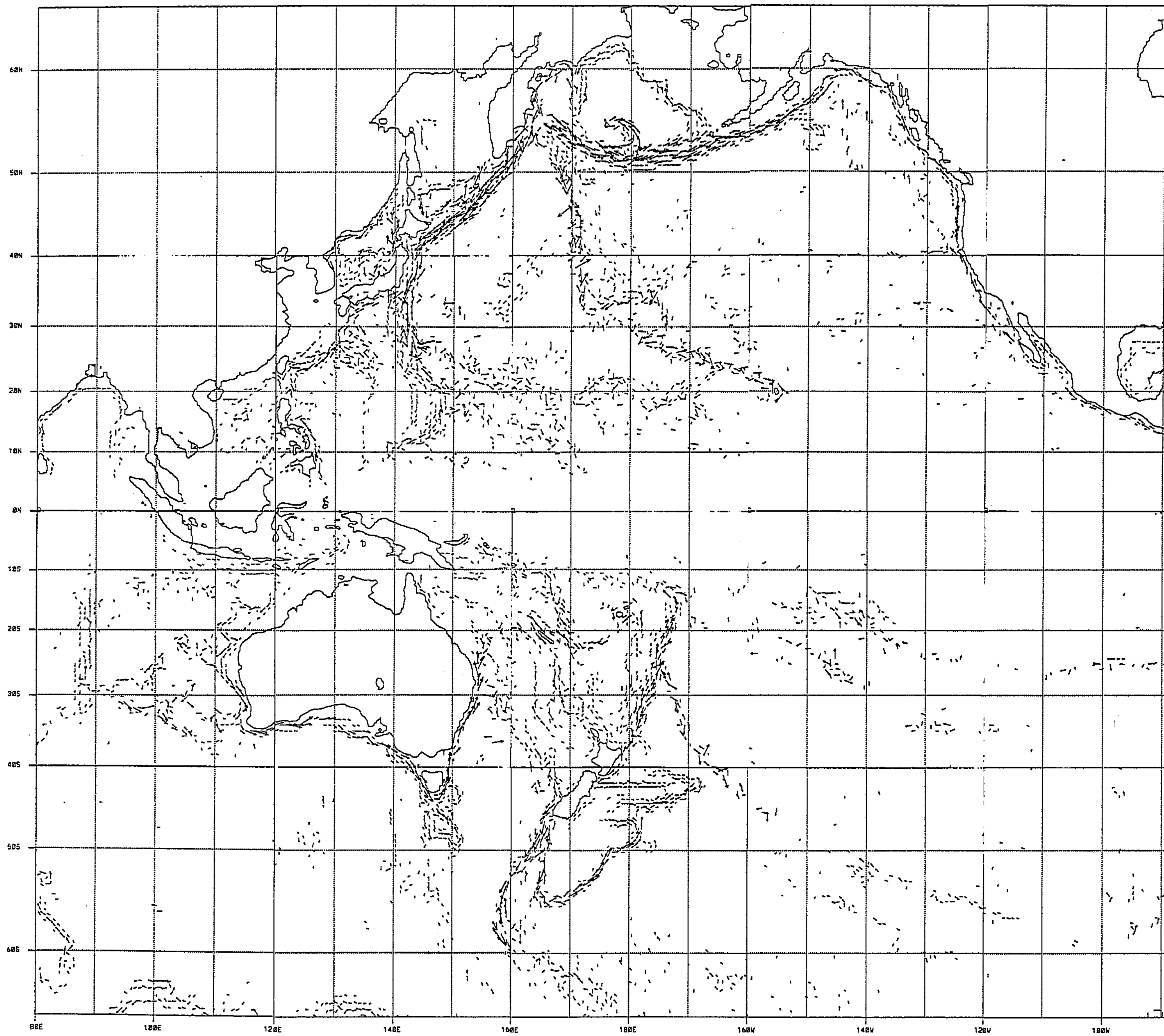
e. Inverse modeling, data assimilation

In the foregoing, we have considered unprejudiced circulation in the context of "forward" ocean models. It makes as much sense, perhaps more, to incorporate unprejudiced circulation within "inverse" models. As a matter of principle, it should appear that any observation of the ocean, *i.e.* an amount of information, reduces our uncertainty (*i.e.* entropy) concerning the ocean state. Thus it is just the formulation of ocean dynamics with respect to system entropy that may naturally include the effect of importing information on account of observations. While further effort will be needed to develop this point of view, a more immediate remark is that the "new" terms which we've added at (1) or (2) can simply be carried forward as dynamic constraints on an inverse calculation. The question which may arise (depending upon one's flavor of inverse modeling) is what weight or confidence to place upon the "new" terms. I don't see any clear way to answer the question. It is rather like asking "Well, how do you feel about eddy viscosity?" On a scale one to ten? I can only say that *I feel* that u* is *at least* as good an idea as eddy viscosity. There is another approach. Statistical dynamics predicts both a mean circulation and the variance of fluctuations about that mean. One could imagine using the inverse variance as the weight for the unprejudiced mean. I think, however, that this is *not* a good idea. As remarked above, statistical dynamics is supplying solutions for closed, non-dissipative idealizations. In applying a relaxation-to-u*, we only assert that eddy interactions should cause real systems to tend toward equilibrium. Especially as larger scale mean flows are predicted, these are expected to better survive against actual dissipation. On the other hand, the statistical equilibrium for fluctuation variances can be shifted toward smaller scales (depending upon two Lagrange multipliers, neither of which is well estimated). My guess *(with prejudice)* is that disequilibrium processes such as energy or enstrophy cascades quickly ruin the statistical equilibrium prediction for fluctuation variances while leaving largely in tact the predictions for mean u*. Thus it remains to assess how good we *feel* about unprejudiced u*, something that may become clearer as we gain more experience.

Secondly (or within the category of "generalized inverse"), there is application under data assimilation. Especially interesting are the "adjoint-method" variational solutions recently published by Thacker, Tziperman and others in ordinary journals (which will not be mentioned). The power of these methods is that they *both* find solution to model dynamics which best fits given data and adjust parameters of the model dynamics in order to optimize that fit. With respect to the novel subgridscale representation set out above, one might particularly wish to evaluate λ while possibly also adjusting a response time τ (if present) and/or eddy viscosity υ. Care is required though that insufficiency of data may lead to lack of convergence (or very slow convergence) of the adjoint method when too many control parameters are adjusted.

Acknowledgments

First I wish to thank the Editor and her staff, the groundskeepers, and management services at Correct Journals for their tolerance concerning alleged publication in ordinary journals. I wish to acknowledge a secret agent (whose initials stand for Meerl Hovershoot) who attempted to thwart these investigations by supplying a booby-trapped box of marbles, filled and sealed to prevent ergodic evolution even upon vigorous rattling. Frustration at these non-ergodic marbles led me to realize how much simpler are ocean currents. Effort was further stimulated in response to the award by Ken Brink of one "Red Herring", presumably in anticipation of completion of this work. I'm grateful to acknowledge the help of Trish Kimber, Wing Quon and Dave Ramsden. This "work" has been supported in parts by one or more agencies which may prefer to remain anonymous, and by my boss who remains anonymous in the interest of our institution.



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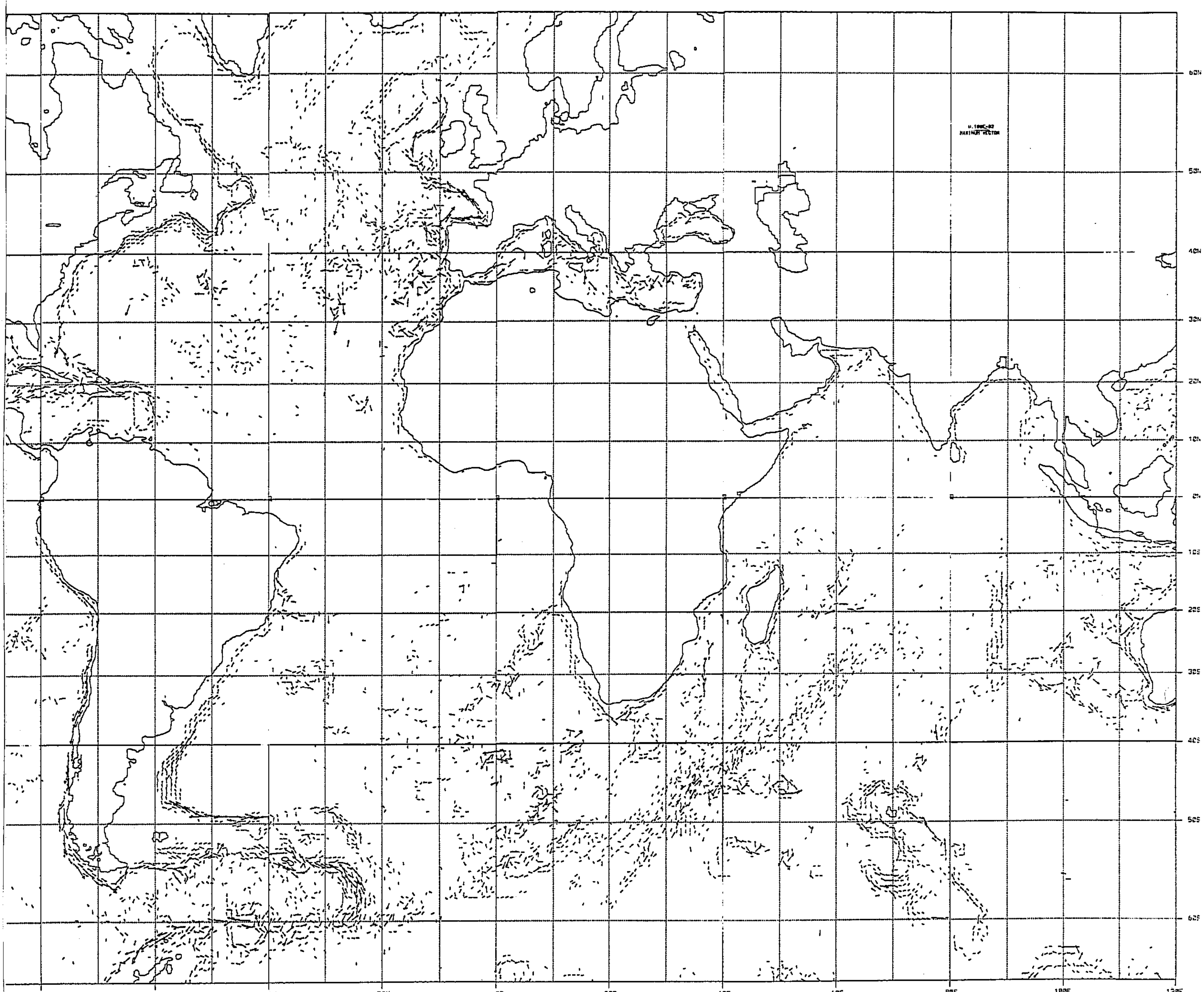

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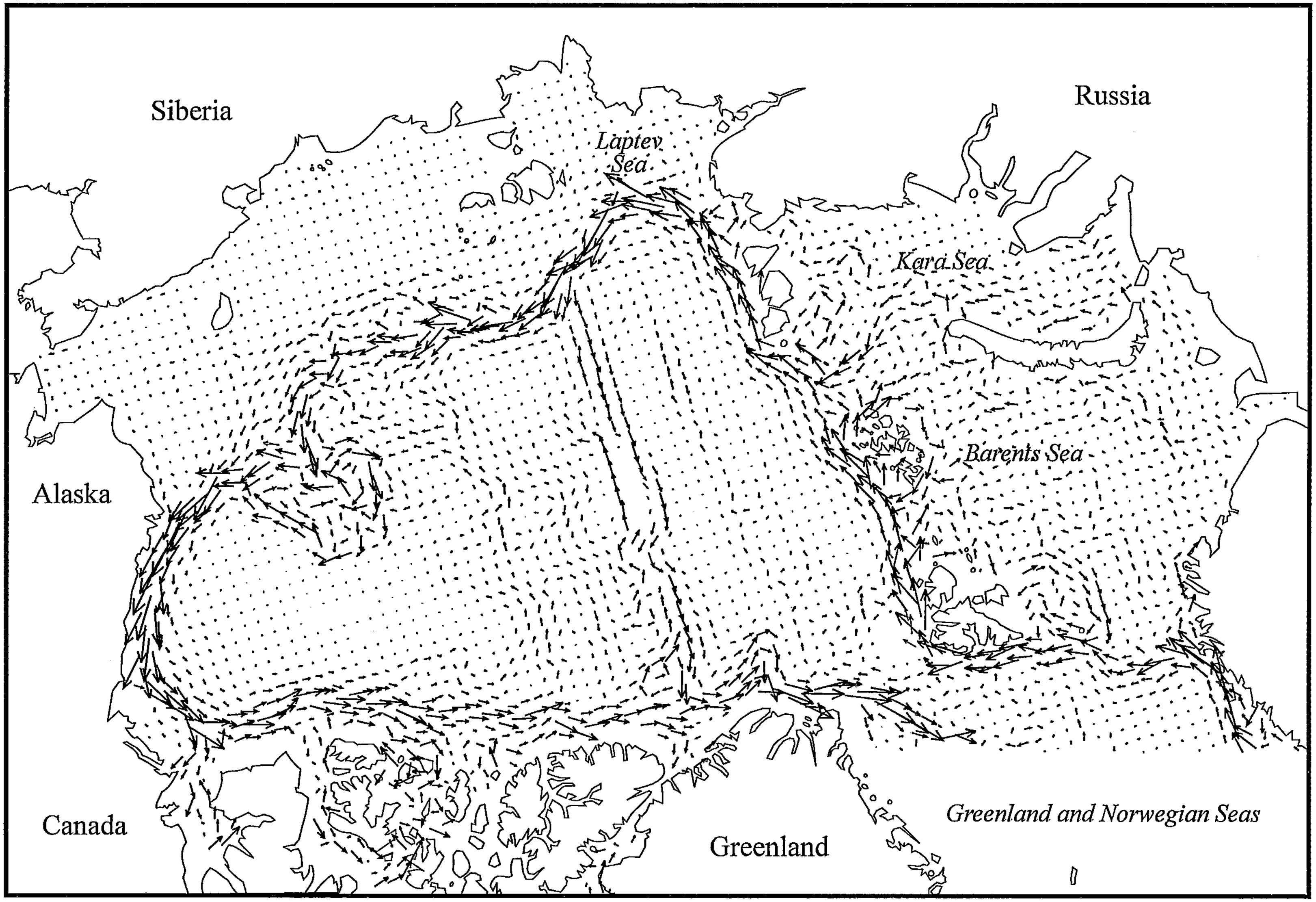
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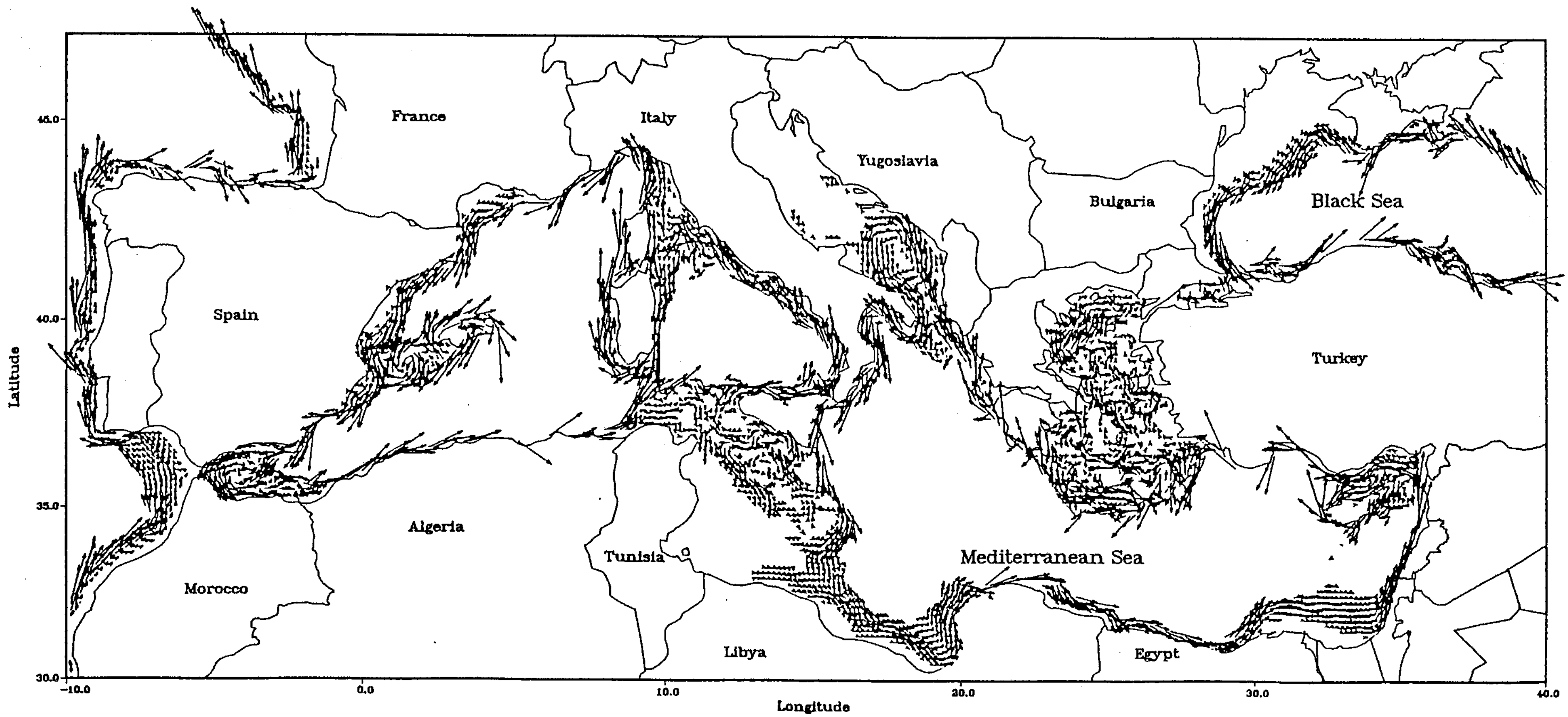

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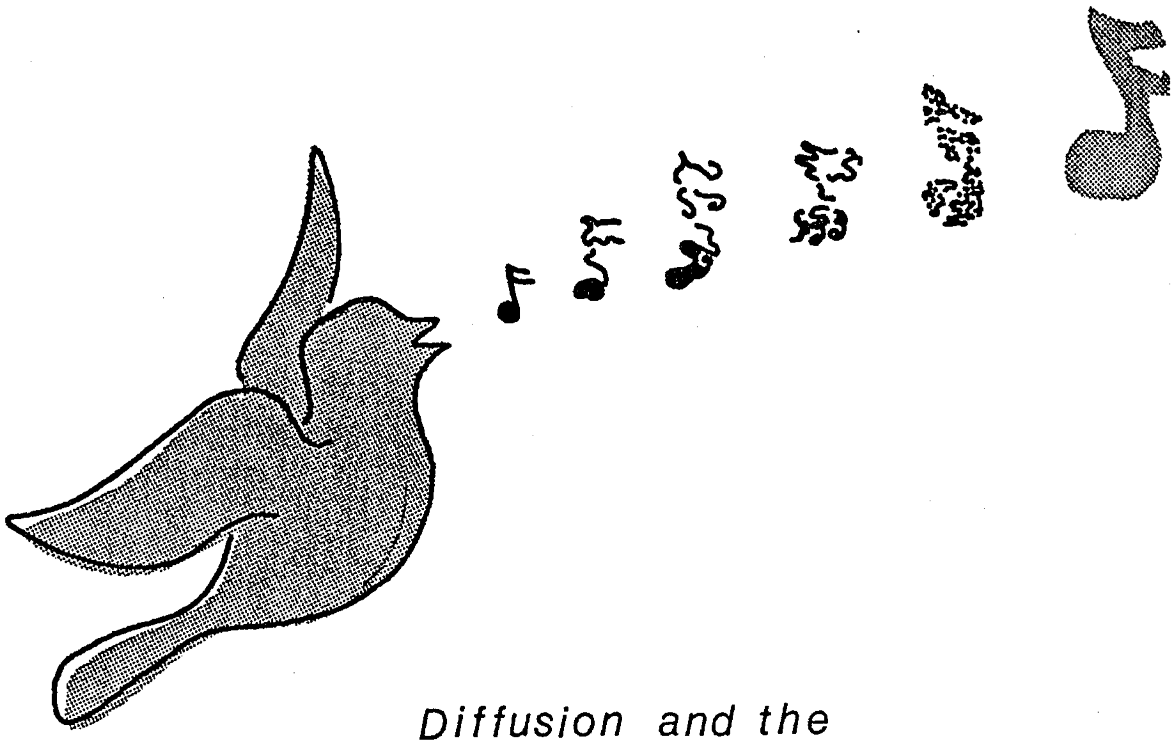
Correct Journals introduces the ***Correct Cartoons Department***. The following has been contributed by Dave Mackas as a "comment on streakiness":



Diffusion and the little bird