From classical to statistical ocean dynamics

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1. Ocean, lakes and (most) duck ponds are too big

Advances in computing capacity along with advances in numerical methods provide ever greater power to solve for flows in oceans, lakes and duck ponds. In principle we suppose we know a good approximation to equations of motion on some scale, e.g. the Navier-Stokes equations coupled with heat and salt balances under gravity and rotation. In practice we cannot solve for oceans, lakes or most duck ponds on the scales for which these equations apply. For example, the length scales over which ocean salinity varies are often shorter than 1 mm. We try to solve for fields represented on grids (or other bases) that are far larger than the scales to which 'known' equations apply. Then we are compelled to guess equations of motion.

Guessing equations is uncomfortable, often causing us to assume without question equations used by some previous author. When we are brave, we realize this too is uncomfortable. It is natural to wish that, as computers grow ever more powerful, we guess less and less. What would be needed from the computer? In the oceans there are about 1.36×10^{18} m^3 of water. If we felt that variability was unimportant within volumes of $O(10^{-9}\ m^3)$ then the computer should track $O(10^{27})$ volumes each described by several degrees of freedom. Clearly one can fiddle these numbers. Today's 'big' computer models, e.g. for weather forecasting or turbulence research, may advance 10^7, 10^8 or 10^9 variables. Over time we are assured the computers will become bigger yet. Even if we imagine computer models advancing 10^{12} variables (not on my desk in my lifetime!), we still face the situation that, for each one variable we track, we must guess how that variable interacts with 10^{15} variables about which we are uninformed. Limiting ourselves to coastal oceans or lakes, the mismatch in degrees of freedom might reduce to 10^8 or less. A suitably modest duck pond could -- someday -- prove computable? Maybe.

This enterprise is like seeking to reinvent the steam engine from molecular dynamics simulation of water vapour. What a brave but bizarre thing to attempt. For oceans, lakes and ponds the circumstance is even worse than the dismal numbers above suggest. As computing power opens more explicit degrees of freedom, solutions exhibit chaos in the sense that arbitrarily nearby solutions at some time become entirely uncorrelated at later time. This invites ensemble forecasting to allow 'meaningful' results such as means, variances, most probable results, etc. We are led to ask what is an appropriate size of ensemble. 3? No. Perhaps 10^3? Maybe.

The point of scary big numbers is: for the numerical ocean, lake or duck pond to be workable, huge numbers of degrees of freedom must be somehow 'slaved' to a far fewer number. Indeed we suppose this is true. But the keyword is 'somehow', and that is the motivation for this paper.

2. GFD acquires an 'arrow of time'

Our goal is to represent subgridscale variability, often seen in eddy viscosity or mixing parameterizations. A wealth of experience in such matters has shown, arguably, greater and lesser success. Numerical ocean models 'run', and outputs are not conspicuously absurd (sometimes). Successes have depended in parts on choices of eddy viscosities and mixing schemes, drawn from history of trial-and-error tuning. Our purpose here will not be to re-tune traditional eddy viscosities but rather to focus upon underlying fundamentals which can lead to results strikingly different from traditional schemes.

Ocean modeling rests upon geophysical fluid dynamics (GFD), a branch of classical mechanics applied to continua under gravity and rotation. Apart from the role of viscosity or mixing (which are 'added on' to GFD without derivation), GFD inherits from classical mechanics a marvelous geometric quality about space and time. 'Past' and 'future' in t are not fundamentally different from 'left' or 'right' in x. An evolution from past to future is no more usual than from future to past (bearing in mind that Earth's rotation reverses for a time reversal experiment). But real flows do not evolve 'backwards' from future to past. This is true in global oceans, lakes, duck ponds and tea cups. (Milk stirred into tea turns brown. Reversing the stirring does not re-separate the milk from the tea.)

How should GFD learn about time's arrow from past to future? The question is at the heart of moving from classical to statistical mechanics, e.g. as Coveney and Highfield, 1992. Two key ideas come to bear: probability and entropy.

Symbolically, let **y** denote a vector in phase space (of dimension 10^{27} or whatever), governed by d**y**/dt = **F**(**y**) + **G**. **y** is simply the collection of all variables needed to fully describe the ocean (or whatever) while **F** and **G** express terms in the forced equations of motion. Depending upon one's interest, **y** may include also biological and chemical components. Computation of the highly erratic (presumably chaotic) **y**(t) in 10^{27} is not feasible or even desirable. We consider instead probability p(**y**)dy for actual "**y**" within phase volume dy of any **y**. Then we would seek an equation for dp(**y**)/dt = ... But this is even less feasible than attempting to solve d**y**/dt.

Aside: one sometimes solves ‘toy’ stochastic models, such as a Fokker-Planck equation $\partial_t p=-\partial_y J=-\partial_y A(y,t)p+{}^1/_2\partial_y\partial_y B(y,t)p$ where J is a flux of probability and A and B are modeled after some properties estimated from F and G. For realistic ocean modeling, actual derivation of A and B from F and G are not feasible (to my knowledge), and Fokker-Planck equations are explored as intuition-building models. Practical progress results from reducing the problem of p(**y**) to that of seeking a chosen set of moments **Y** = **y**dp or, generally, **K** = **k**(**y**)dp for functions **k**(**y**). An advantage is that one may choose as few **Y** as one likes, perhaps 10^8 or perhaps only 10, 'integrating out' as much of **y** as one wishes. The outstanding problem is that we do not have equations for d**Y**/dt = ... Progress is made by carrying out probability averaging (expectation) over the components of **y** retained in **Y**, yielding

$$d\mathbf{Y}/dt=\mathbf{F'}(\mathbf{Y})+\mathbf{G'} + \ldots \qquad 1$$

where **F'**(**Y**) includes linear dynamics and couples explicitly the retained components of **Y** while discarding the myriad couplings among **Y** and 10^{27} **y**. **G'** are the expectations of **G** for the retained **Y**. Eq. 1 expresses traditional GFD symbolically with '…' supplied from ad hoc mixing. The manifest successes of GFD are due largely to circumstances near forced linear dynamics that are fully captured by **F'** and **G'**.

Comment: Suggesting that 'textbook' equations of GFD are wrong often meets resistance. The books have been in use for so long, and the work of GFD turns to applications and properties of these equations, not to questioning the equations. How could such well-used equations be 'wrong'? The problem, I suggest, has been too little attention to the dependent variables we mean to describe. Viewing output of numerical models, it is easy to see maps of 'flows' or 'temperatures' or whatever. Pausing for a moment, we retreat to something like 'grid-cell and time-step averaged flow', etc. Yet if we try to define space-time filtering operators to pass over underlying equations, it is deeply problematic to obtain traditional GFD. The enterprise is far more troubled if we advance to modern computing with eddy-active, presumably chaotic, output. Then what meaning do we attach to 'temperature', say, in some cell over some step in some specific (chance!) realization? Perhaps what we should mean is 'temperature moment over probabilities of possible states'. That's a lot to say. However, if we then asked 'what is the equation of motion of the temperature moment over probabilities of possible states?', we would be far more ready to suppose that a textbook equation for 'temperature' might not be right.

Apart from '…', eq 1 expresses the marvelous symmetry between past and future inherited from classical mechanics. Time is without its arrow, bringing us to our second key idea: entropy. Entropy is most familiar in thermodynamic context, often dH = dQ/T where dH is the increment of entropy resulting from supply of an increment of heat dQ to a system with constant volume at temperature T. Entropy has also entered common language as an allusion to disorder. We should be careful to be precise. For a processe defined by probability function p, entropy is

$$H = - \quad dp \log(p) \qquad 2$$

to within an arbitrary multiplicative constant and within an arbitrary additive constant. Describing probabilistically the field of molecular chaos, H from eq 2 is the thermodynamic entropy $dH = dQ/T$. In another context, describing probabilistically the likelihoods of transmitted messages, H from eq 2 is the entropy of information theory.

Aside: while 'log' in eq 2 is considered natural $\log_e$, this is sometimes $\log_2$ for information application, rendering units of H in bits. Equivalence of thermodynamic H and information theoretical H is discussed in texts such as Katz (1967). However, the duality of entropy under statistical physics and information theory has not been exploited in the context of oceans, e.g. relating dynamics and state estimation.

Finally, the arrow of time arrives with the 2nd Law of Thermodynamics. For 'internal' or 'free' interactions (interactions not subject to external influence), p evolves such that for $t_2 > t_1$, $H_2{}^3\ H_1$. We are left to instruct GFD that '…' in eq 1 should provide this property -- appropriately!

3. Equations of motion for moments of probable flows

The alert reader already asks: do not traditional eddy viscosity or mixing as '…' already break time reversal symmetry? They do. The challenge is that traditional schemes, while irreversible, may not satisfy $H_2 H_1$ for $t_2 > t_1$. In many cases, traditional schemes, guided by intuition and experience, are ok. Milk stirred into tea turns brown and eddy diffusion accomplishes such irreversible browning. H for the milk-tea mix is increased after browning. If intuition and experience worked this reliably, the present paper would be unnecessary and we could instead invest effort refining estimates of eddy diffusion coefficients. But intuition, and GFD, go way wrong as two illustrations (eddy-topography interaction and stably stratified mixing) will show. For now we continue developing the equations of motion.

There have two approaches which we consider briefly. In these approaches the aim is not to complete eq 1 itself but rather to characterize solutions of eq 1.

An approach by Paltridge (1975, 1978) hypothesized that the mean state (**Y**) of the atmosphere is such as to maximize the production of (thermodynamic) entropy. This has been further advanced by Ozawa and Ohmura (1997) in context of Earth's atmosphere and shown to be a plausible descriptor for other planetary atmospheres by Lorenz et al.(2001). Of particular note for ourselves are (1) the role of thermodynamic entropy, absent in other work below, and (2) a principle of maximum entropy production (MEP) to which we will return.

The second approach to solutions of eq 1 resulting from '…' is seen in Salmon et al. (1976) and in a host of papers since, referenced in parts by Holloway (1986) and Salmon (1998). In these cases the mean states of idealized oceans are described by the maximizing entropy due to uncertain macroscale eddies. These studies have focused upon expectations for potential vorticity q=(_ + __**u**)b__, where _ is Earth's rotation, __**u** is vorticity, and __ is density gradient. Calculations assume no external forcing and no internal dissipation. Thus the macroscale flow field does not communicate with the field of molecular chaos and thermodynamic entropy is not included. For such unforced, non-dissipative, ideal dynamics, H is maximized subject to conserved integrals of the motion. In early studies, conservation constraints included domain-integrated energy, potential vorticity (q) and enstrophy (q^2). Subsequent studies by Miller (1990), Robert and Sommeria (1991) or Robert and Rosier (1997) have considered the roles of further invariants derived from advection of potential vorticity. Despite such idealizations, many aspects of these solutions seemed to capture realistic features. However, construction of such solutions does not yet provide the missing '…' in eq 1. For later reference, we denote these maximum entropy (ME) solutions as **Y***.

In many cases, our concern is not only to characterize stationary or equilibrium solutions to eq 1, such as addressed by MEP or ME, but rather to ask how solutions to eq 1 evolve from assigned initial conditions and under assigned external forcing. For this we need to complete the equations of motion, i.e. to represent '…' in eq 1.

Two paths to '…' have been opened. One path is seen in recent research by Chavanis and Sommeria (1997), Kazantsev et al. (1998) and Polyakov (2001) based upon MEP for subgridscale potential vorticity. The promise and challenges along this path are not yet clear. For the present I would refer the interested reader to the cited sources while we here turn to another path for which there is simply more experience to date. It will be seen that the two approaches have conceptually much in common and indeed recent work of Polyakov (2001) finds also quantitative results with much in common.

The other path, sketched below, follows 'generalized thermodynamic force' (GTF) after Onsager (1931a,b) or Onsager and Machlup (1953).

If entropy depends upon some set of macroscopic parameters, **X** (e.g., the expectations **Y**), then a forcing acts upon **X** due to gradient of H with respect to **X** , i.e., **C**b_**X**H where **C** is a coefficient tensor projecting _**X**H onto d**X**/dt. As an illustration consider a gas in two concentrations in two chambers separated by a membrane with X, say, the fraction of total gas in one chamber. Derivatives of H with respect to available volume (per molecule) give thermodynamic pressures in the two chamber, yielding nonzero dH/dX when pressures are not equal. dH/dX can force dX/dt depending upon the extent of perforations in the membrane, etc., described by C. Importantly, there are two parts to GTF: $\__{\mathbf{X}}H$ and **C**, which, together provide '…'

$$d\mathbf{Y}/dt = \mathbf{F'}(\mathbf{Y}) + \mathbf{G'} + \mathbf{C}b\__{\mathbf{Y}}H \qquad 3$$

Clearly GTF and MEP have very much in common. Each seeks to drive **Y** as rapidly as possible toward higher H, the physics challenge focussed upon 'as rapidly as possible', considering availability and skill of different methods of estimation.

We return to eq 3 in section 6. Meanwhile there is an intermission.

1. Illustration: eddies and topography, the wrong way

Consider interaction among eddies and mean flows in basins with complex topography. First let's get a wrong answer, coincidentally the answer provided by nearly every major ocean model at ever major institution on Earth. Because it can be difficult to assess 'wrong' in realistic circumstances of many complicated, poorly known inputs while outputs are compared with limited data, let us instead pose a thought experiment. Suppose we only 'see' the ocean on larger scales amenable to numerical modeling, perhaps some 10s to 100 km. Suppose on these scales we find our ocean to be at rest, motionless with flat density surfaces. We cannot see but are aware that on smaller scales the ocean is filled with ubiquitous eddies. Maybe the eddies are from previous episodes of forcing, or maybe they are driven by smaller scale forcing we cannot see, perhaps by enthusiastic goldish. Suppose on the large scale we observe there is no imposed forcing. We wish to predict the future ocean on the scales we can see. We poll the major ocean models worldwide, asking: if the ocean is stably at rest and no forcing acts, what is the future? With extraordinary unanimity across different models, the answer is: nothing. That unanimous answer is dead wrong. Let's see.

We can test the answer within the same models. For a given model, we get a bigger computer allowing resolution on the eddy-active scales not previously seen. From our awareness that small scales eddies exist, we randomly excite the newly realized small scales. The test remains the same as before: on larger scales the ocean is at rest and no large scale forcing acts. We run our newer, higher resolution model to compare the previous prediction that no large scale flow will occur. We find instead (as the reader with computer access -- or a friend -- can check) that large scale flows emerge and these flows have a definite sense (shallow to right in northern hemisphere).

How did (nearly) all the models at all the major institutions (including those advising governments about climate, etc.) fail this test? It was all in '…' What the models did, based on intuition, experience and simply getting the models to run, was to replace '…' with some manner of eddy viscosity, perhaps even of fancier iterated-Laplacian sorts. When those eddy viscosities saw fields of no motion, they took no action. (If there was some slight motion, it would be damped anyway.) That was wrong. While eddy viscosity did serve to break time symmetry in eq 1, it broke time symmetry the wrong way, driving H the wrong way (to be shown below).

Traditional GFD with traditional eddy viscosities violates 2nd Law, assuring wrong answers.

Aside: Apparently a 'solution' is to use enough computing power to resolve eddies. The challenge is: how much is enough? While modern models are often termed 'eddy-resolving' the more apt term is 'eddy-admitting'. That is, the resolution is sufficiently fine, allowing explicit damping terms sufficiently weak, that model dynamics support internal instabilities, admitting eddies. But … eddies are only dynamically 'resolved' when further increase in resolution does not lead to systematic changes to statistics of the eddies. Importantly it is the feedback of eddies upon larger scale mean flow which (I speculate) is most difficult to achieve from refined resolution. Ultimately -- in principle -- we may suppose computers approaching the molecular dynamics simulation of steam engines or even duck ponds. Our aim in this paper is to seek another way.

1. Illustration: eddies and topography, the hard way

That ocean models would fail the eddy-topography test (above) was known theoretically for nearly three decades after a comprehensive theory set out by Herring (1977) following spectral-based statistical closure after Kraichnan (1959). A simpler spectral closure theory by Holloway (1978), after Kraichnan (1971), was consistent with the results from Herring (1977). Despite effort to simplify, these closure theory calculations are hugely difficult and a brave reader is referred to the cited references.

Briefly we recall only relevant aspects from the simpler calculations by Holloway (1978). To render the problem tractable, barotropic quasigeostrophic (QG) dynamics were considered. Potential vorticity q=(2_ + __**u**)b__ is approximated q=_+h where _ is the vertical component of QG vorticity__**u** and h=f_h/h_o represents variation _h of total depth, h_o is a constant reference depth, and f is a constant vertical component of 2_. The fluid is considered of uniform _ with __ replaced by the inverse of total depth.

Supposing fluctuations of _ and h are spatially statistically homogenous, evolution of

<_ _> and <_h> in spectral domain are predicted for assumed statistics of <hh> where < > denote probability expectation. Compared with direct numerical simulations, such closure theories showed reasonable skill. Moreover, considering an ensemble of realizations of _ for given realization of h, theory easily showed that <_ _> confined initially to small scales would readily force nonzero <_>=<_h>/h on all scales for which h_0. The thought experiment posed in sect 4 was sure to fail.

Eddy-topography closure was taken a little farther in Holloway (1987) to include nonzero spatially uniform flow U and admit simple $f=f_O+\_y$. This allowed calculation of pressure-topography 'form drag' with dynamically responding dU/dt. If, in addition to external forcing applied to U, there were assumed sources of eddy energy (e.g., stochastic wind forcing), then the pressure-topography forces could systematically propel U. Use of the term 'form drag' has been largely replaced by 'form stress' or 'topographic stress' to recognize that this force may not simply retard mean flow but may force mean flow. Numerical experiments confirmed that, with mean wind forcing applied to U of sign U>0, the response tended toward U<0. This counter-intuitive result gave rise to the label 'neptune effect' (whimsically suggesting the phenomenon was otherwise inexplicable).

Although some of the above-mentioned results are satisfying theoretically, they are very limited from practical perspective. A large amount of tedious calculation is needed to obtain results restricted to statistically homogenous, barotropic QG flow. So what? Clues to a way forward were buried in Holloway (1978) then made powerfully clear in a key paper by Carnevale et al. (1981) who showed that for entire classes of closure theory after Kraichnan (1959, 1971) the theories strictly assured nonlinear interaction terms yielding dH/dt_0, driving the system monotonically toward the ME solution **Y***. (Other terms due to external forcing or dissipation could decrease H, preventing **Y***.) Thus the outcome of closure theories were to show in detail how dynamics drove systems from any **Y** toward **Y***. Can we use this property to motivate highly simplified approximations to closure theory?

1. Illustration: eddies and topography, the easy way

Apparently the roles of external forces and internal dissipation are to drive realistic **Y** away from **Y***, inducing entropy gradient $__{\mathbf{Y}}$H which, if unchecked, would force **Y** toward **Y***. In terms of GTF this can be viewed as a Taylor expansion **C**b$__{\mathbf{Y}}$H _ **C**b$__{\mathbf{Y}}__{\mathbf{Y}}$Hb(**Y**-**Y***) about **Y**=**Y*** where $__{\mathbf{Y}}$H=0. More carefully we would admit we do not know if typical **Y** are sufficiently close to **Y*** and we would be daunted seeking to estimate **C**b$__{\mathbf{Y}}__{\mathbf{Y}}$H. But a simple scheme emerges. Denoting

K_**C**b$__{\mathbf{Y}}__{\mathbf{Y}}$H, eq 3 becomes d**Y**/dt = **F'**(**Y**) + **G'** + **K**b(**Y**-**Y***) 4

For application we need **K** and **Y***. **Y*** can be inferred after Salmon et al. (1976) who find at ME a relation between QG streamfunction, _, defined by _^{2}_= _ and topography, h, viz. _=L^2 (_^{2}_+h) where L^2, occurring as a ratio of Lagrange multipliers in maximization of H, has units of length^2 related to coherence scales in the eddy vorticity field. If our scales of interest, perhaps as model resolved, are significantly larger than L then the relation for _ simplifies further to __ L^{2}h. However this is still based upon QG for which h=f_h/h$_o$ requires |_h/h$_o$ | << 1 contrary to the actual ocean whose depth varies by the full depth itself.

For implementation into realistic ocean models, ambiguities involve both h$_o$ and the interpretation of _ as velocity or transport (integral of **u** over ocean depth D). These questions were considered in Holloway (1992) then implemented in Alvarez et al. (1994) by taking a ME transport streamfunction to be _*=-f L^{2}D, from which the barotropic component of ME velocity **u*** is **u***D= z   *. The remaining question is how to represent **K**, presumed to be a scale-dependent operator governing the rate at which eddy interactions can force **Y** toward **Y***. Simple choices (with view to practicality) include –K (a damping constant) or A_^{2} (Laplacian diffusion). In practice the choice has been to assign momentum tendency as A_^{2}(**u**-**u***) with constant coefficient A.

It must be clear that choices in the previous paragraph are not strictly derived. They were available, practical choices at a time (c. 1992) offering advances over the common practice of eddy viscosity, c.f. A_^{2}**u**. Substituting A_^{2}(**u**-**u***) with **u*** from **u***D= z   * where _*=-f L^{2}D became known as the ‘neptune parameterization’, with L a length scale presumed to take values from some few to several km.

A number of papers have explored applications of neptune for cases ranging from estuarine to global ocean (Alvarez et al.1994; Eby and Holloway,1994; Fyfe and Marinone, 1995; Holloway et al. 1995; Pal and Holloway, 1996; Sou et al. 1996; England and Holloway, 1998; Marinone, 1998; Nazarenko et al. 1998). Two items in particular are (1) a study of neptune impact upon global skill (Holloway and Sou, 1996) measured against current meter records, and (2) the Arctic Ocean study by Nazarenko et al. (1998) compared with MEP calculation by Polyakov (2001).

The point of simple (easy!) schemes like nepune is to allow present-day ocean models to produce more skillful results from physics closer to statistical dynamics. Such simple schemes are not ‘right’; they are only ‘less wrong’ than traditional models. This should spur further effort. . Among such efforts, Holloway (1997) considered baroclinic extension from neptune, including ‘thickness’ transports in layer models. Merryfield (1998) considered ME QG with continuous stratification. Frederiksen (1999) carefully re-examined to closure theory to see how to evaluate terms such as **K** that are only guessed in neptune applications. Merryfield et al. (2001) extended ME without assuming QG, helping overcome ambiguity stemming from QG forms of h and _, proposing □ *=f $L^2D_o^2/D$ with reference depth D_o . Such efforts, both to establish fundamentals and to devise practical parameterizations will continue, presumably conjoined by newer work such as from MEP.

1. Illustration: mixing heat and salt in bi-stably stratified flow

Before closing we turn to a very different phenomenon on an entirely different scale. In part we seek to test how robust are statistical mechanical approaches. As well we could ask: if the statistical mechanical apparatus were brought to bear only to aid the eddy topography problem, maybe that’s not so worthwhile. For other circumstances perhaps simpler intuitions suffice? Let’s see.

Stratification of sea water is due to heat and salt scaled by corresponding density coefficients. At the level of ‘molecular’ conductivity, sea water is about 100 times as diffusive for heat as for salt. This leads to interesting effects. If the water column is stably stratified with respect to temperature, T, say, but unstably with salinity, S, while the overall density stratification remains stable, one may encounter spontaneous instabilities called ‘salt fingers’. Contrariwise, if stable with respect to S but unstable with respect to T, there are instabilities called ‘layering’. Much of the ocean interior is however stably stratified with respect both to T and S. Then it is assumed that heat and salt behave similarly and that ambient turbulence, perhaps on account of internal wave breaking, mix the two with a single apparent diffusivity, _. It has also been known for some time that this is not true.

In early experiments, Turner (1968) mechanically agitated a fluid stratified with respect to T and, separately, a fluid stratified with respect to S, taking care that the two stratifications were initially the same. Under the same mechanical agitation, it was found that T was mixed more efficiently than S by an amount greater than could attributed to molecular conduction. Later, Altman and Gargett (1980) performed similar experiments in a tank was stably stratified with respect to both T and S, arranged so that both made the same initial contribution to stratification. Again it was seen that T mixed more readily than S by amounts exceeding molecular conduction. This is termed ‘differential diffusion’. Observation of differential diffusion in the ocean is technically difficult, with results reported by Nash and Moum (2002). While the phenomenon is now observed, why does it happen?

Let’s try intuition and experience. Omitting at first gravity, turbulent stirring of a (passive) tracer is like the milk-into-tea example. Heat and salt would be stirred down their respective background gradients, with the fluxes in vertical (w’T’ and w’S’) dominated by scales of turbulent energy with lesser contributions at smaller scales (subject to viscous and diffusive cutoffs). Because ‘molecular’ diffusion of T is faster than S, the short scales of w’T’ are suppressed more strongly than for w’S’. Hence we might expect the total turbulent salt transport to be greater than heat transport, the opposite of what is observed.

Including gravity, what changes? Mainly the turbulence is suppressed, exhausting its energy by working against gravity (in traditional thinking). But that is only to say we expect a weaker version of stirring-milk-into-tea and the previous (wrong) result that salt transport should exceed heat transport is still expected. Where did we go wrong?

Numerical simulations at first in 2D (motion only in a vertical plane) by Merryfield et al. (1998) then fully in 3D by Gargett et al. (2002) reveal what happens. Contributions to w’T’ and w’S’ reverse sign to counter-gradient in the shorter scales. When larger ‘molecular’ diffusion of T preferentially cuts off w’T’, the surviving w’S’ fluxes are of counter-gradient sense and hence subtract from the overall down-gradient S flux. The result is S flux weaker than T flux, as observed.

While numerical simulations have showed what happens, we are left asking: why? Especially why the prevalence of counter-gradient (backwards!) fluxes at shorter scales? I think such counter-gradient transports are generic to stably stratified turbulence. Closure theory (Holloway, 1988) in 2D (vertical plane) anticipated very well (quantitatively) the 2D numerical simulations and (qualitatively) the 3D simulations. Importantly, that closure theory is of the broad class which, per Carnevale et al. (1981), strictly satisfies dH/dt _ 0 (apart from external forcing and dissipation). Then entropy gradient forcing, or GTF, explains each aspect of what happens.

At larger scales (relative to ‘molecular’ diffusive cutoff scales) a source of turbulent kinetic energy (KE) is assumed. Under gravity, two things happen. In part KE is redistributed to smaller scales due to entropy gain from such redistribution (a GTF view of ‘turbulent cascade’!) In part KE is converted to potential energy (PE) stored in T'^2 and S'^2 (as density variances times gravity), driving the ratio KE:PE toward higher entropy (per GTF). Large scale conversion of KE to PE is by down-gradient w’T’ and w’S’. Large scale T'^2 and S'^2 are also redistributed to shorter scales by another GTF ‘cascade’. Things get a little complicated though. Recall GTF works as **C**b**_Y**H involving **C** as well as **_Y**H. The C for redistribution of KE is weaker than for redistribution of PE due to the role of pressure forces maintaining incompressibility **_**b**u**=0 for vector field **u** whereas scalar fields T and S are unrestricted. A consequence is that PE is more rapidly transferred from large to small scales causing the ratio KE:PE on small scales to favor entropy production by converting PE to KE, forced by _H/_(KE:PE). Conversions PE _ KE are by counter-gradient w'T' and w'S' whence stronger diffusion of T' leads to overall stronger w'T'.

8. Outlook

Broadly, entropy calculus helps clarify and organize the work that must be done so that traditional ocean models, based in classical mechanics plus ad hoc mixing, may acquire a consistent arrow of time. In some cases, traditional mixings and eddy viscosities happen (willy nilly?) to point the right way. Too often such guessed-at schemes point oppositely to time's arrow and then go quite wrong. No surprise! Illustrations above are drawn from two extremes: (1) the characteristics of ocean currents of scales of 10s and 100s of km, and (2) the nature of mixing on scales of cm to mm and smaller. In each case approaches from entropy calculus are sketched. It must be admitted though these are barely sketches. Very little intellectual resource has yet been invested on the statistical dynamical side compared with investment on the classical (traditional) ocean dynamics side. Among tasks ahead are (1) ongoing effort to build confident fundamentals and (2) brave efforts to bring statistical dynamics into practical ocean modeling even while further efforts build and refine fundamentals. Practical steps will be understood as steps along the way, in place only until they can be superceded. But the sweep of what may be done, with prospects for improvement both in understanding and in practical skill, are powerful motivations for the tasks ahead.

Acknowledgement. I am first grateful to Joel Sommeria and Peter Davies for organizing this Grand Combin school and for inviting the present lecture. The character of the work, and thoughts expressed above, were inspired from interactions with students and faculty throughout the school period. Research upon which this lecture was based has been supported by the Office of Naval Research.

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