REVIEWS OF GEOPHYSICS, VOL. 25, NO. 2, PAGES 183-192, MARCH 1987
U.S. NATIONAL REPORT TO INTERNATIONAL UNION OF GEODESY AND GEOPHYSICS 1983-1986

# SMALL-SCALE PHYSICS OF THE OCEAN

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## Introduction

Four years ago, we noted that, although there was a widespread belief that the key to many ocean processes must be the communication of energy by internal waves, there had been little progress in definitely relating small-scale processes to internal waves (Caldwell, 1983a). For example, although it seemed plausible that the energy that supplies spots of turbulence in the thermocline is delivered to those spots by internal waves, the process had neither been directly observed nor shown theoretically in a verifiable calculation. In the past four years there has been some progress; observations have been made which render such connections more plausible and some theoretical ideas have been advanced, but we've seen no breakthrough in this direction.

Instead, progress has come on other fronts. Our picture of turbulent processes in the upper ocean, especially in the equatorial ocean, is becoming more clear (and more complicated). Strong downdrafts in the mixed layer have been discovered. Dissipation in the convective mixed layer has been found to scale very much as it does in similar layers in the atmosphere. The dynamics of intrusions have drawn attention because they may contribute a good deal of the mixing of the oceans. Although significant laboratory experiments are now available, controversy remains concerning the application of laboratory turbulence concepts to the ocean. Bottom-layer flows have now been carefully defined. One notable development is the increased interest in the effect of mixing conditions on biological activity. Our instrumentation is becoming more routinely deployable, an important step toward obtaining the large amount of sampling required to form a reasonably accurate picture of small-scale processes in the ocean.

It seems to this author that we will see an even more exciting period in the next four years as we employ the ideas and instruments we have been developing. The equatorial regions draw our interest because we expect the small-scale activity taking place there to have global scale effects. We will, I hope, look deeper into the ocean, far enough at least to see the mixing in the ventilated thermocline, perhaps far enough to resume the study of mixing in the deep, unventilated ocean. The diurnal cycle in momentum and energy needs to be well understood. Our ideas about the physics of turbulence in the ocean need to be settled.

In the following, we will take a quick look at progress in the past four years and end with a biased list of projects and problems for the future.



Paper number 6R0761.
8755-1209/87/006R-0761$15.00

## Surface-Layer Physics

Recent advances in our knowledge of the upper-ocean boundary were recently reviewed by Thorpe (1985) and a report on progress in upper ocean dynamics is included in the present collection (Price, Terray and Weller, 1987), so we will not deal with the surface layer in depth (so to speak). Some of the points discussed by Thorpe are: (1) the diurnal modulation (which we will discuss in the equatorial context); (2) the similarity between the atmospheric and oceanic boundary layers in terms of velocity spectra (Jones and Kenney, 1977), kinetic energy dissipation in stable (Dillon, Richman, Hansen and Pearson, 1981) and unstable (Shay and Gregg, 1984 [discussed later]) conditions, and patterns of temperature structure; (3) the continuing lack of progress in discovering the significance of Langmuir circulation, although the discovery of large oceanic downdrafts by Weller, Dean, Marra, Price, Francis and Boardman (1985) is a beginning; (4) the discovery by Kitaigorodskii, Donelan, Lumley and Terray (1983) that wave-generated turbulence extends below the surface as far as ten wave amplitudes; and (5) the lessons that may be learned about near-surface turbulence by the study of bubbles and their generation.

## Equatorial Turbulence

Some of the most interesting turbulence observations of the past four years come from one cruise of the Thompson and Wecoma to the Pacific equator in November/December, 1984, as part of the Tropic Heat project. A number of our ideas about the equatorial region were overturned, and some of the results may not be specialized to the equator at all. One reason so many new results were obtained is that the sampling by both ships was the most systematic turbulence sampling ever. For example the Wecoma stayed on one station for twelve days, microstructure profiling night and day, obtaining 1749 profiles. This volume of sampling needs to be performed at other sites in the ocean before a true picture of the working of the upper ocean will be achieved.

Prior to the 1984 cruise, our thoughts about turbulence in and above the undercurrent were dominated by one fact, one calculation, one controversy and one idea. The fact has been disproven, the calculation has been rendered more complicated, although we suspect still correct, the controversy has been settled, and the idea has been proven wrong:

The fact: It was accepted as fact that on a meridional plot, turbulent activity peaked at the equator, with the peak being no more than a fraction of a degree wide (Crawford, 1982). This "peak" turned out to be caused by inadequate sampling. If there is a peak (open to question), it is as broad as the undercurrent itself (Moum, Caldwell, Paulson, Chereskin and Regier, 1986; Peters, Gregg and Toole, 1986a).

The calculation: As we noted four years ago, the equatorial undercurrent is one ocean current that has its speed controlled by turbulent dissipation (Crawford and Osborn, 1981). This is undoubtedly true, but the time variability, especially on the diurnal timescale and on the scale of the wind motion and the 21-day wave, makes appropriate estimates of the turbulent friction difficult to obtain.

The controversy: At one time there were indications from measurements made from a towed body that extremely large (0.08 cgs) values of turbulent dissipation were present in the core of the undercurrent. When these were not found in later microstructure casts, it was suggested that more sampling than could be performed from profiling instruments might be necessary in order to find the large dissipation spots. On the Tropic Heat cruise approximately 2500 casts through the core were made, and it was found to be exceptionally quiet, with dissipation 1000x less than the towed-body measurements.

The idea: It was thought that because of the high mean vertical shear above the undercurrent the turbulence would draw most of its energy from that shear and not be sensitive to surface conditions or solar heating, and therefore that the lowest significant period of variation would be that of the 21-day wave. This idea proved conspicuously untrue; diurnal variation dominated, and the local wind may be the next most important source of variation (see below).

## Summary of Equatorial Results

1. Temporal variations are dominated by the diurnal. Turbulent dissipation increases after sunset and continues to increase, on average, until morning. It quiets after sunrise except on the windiest days. (Gregg, Peters, Wesson, Oakey and Shay, 1985; Moum and Caldwell, 1985)
2. The increased turbulence propagates somehow (internal waves?) into the stratified water well below the mixed layer (ibid plus Chereskin, Moum, Stabeno, Caldwell, Paulson, Regier and Halpern, 1986). Often the dissipation below is stronger than that in the surface mixed layer (which oscillates diurnally as expected). A hint of this intense nighttime mixing in the thermocline can be seen in nighttime overturning off the California coast (Price, Weller and Pinkel, 1986). In their Figure 6, overturns can be seen at night in the observed temperature profiles, but not in the modelled profiles. Some unmodelled process must have been at work. We have recently seen a similar effect in turbulent dissipation off the Oregon coast, so evidence is mounting that this is not an exclusively equatorial effect (Moum and Caldwell, 1986).
3. Variations of term longer than 1 day appear to be due either to the local wind stress or to the changes in shear caused by the 21-day wave (Moum and Caldwell, 1985).
4. The usefulness of parameterizing equatorial turbulence in terms of the gradient Richardson number is not established. Plots of dissipation and eddy coefficients show a wide spread, although larger dissipations etc. are found chiefly at lower Richardson numbers. (The waters above the core show Richardson numbers consistently below 1.) There is a decrease in the occurrence of large values of dissipation as the Richardson number increases. This decrease is either abrupt or steep in slope. These Richardson numbers were calculated on a depth scale approximating 20m, a scale appropriate for numerical models but probably not for the physics.
5. A transect across the equator with thermistor chain, RSVP and Doppler showed complicated hydrographic structure, coupling between hydrographic structure and turbulence, and no narrow peak in turbulent dissipation at the equator (Moum et al., 1986). If there is a peak, it is at least as broad as the undercurrent; my bet is that there is no peak within the equatorial wind system.
6. On the transect, a remarkable series of breaking internal waves was encountered (Paulson, Moum, Caldwell, Chereskin and Regier, 1986), accompanied by greatly increased dissipation.

## Off-Equator Mixed Layers

There is not a great deal of information to compare with the equatorial results. Two studies in recent years have looked intensively at the turbulent dissipation in the mixed layer; a study of a wind-mixed layer on the coast of Nova Scotia (Oakey and Elliott, 1982) and an experiment on a convectively mixed layer inside a warm-core ring (Shay and Gregg, 1984). In the wind-mixed layer, the vertically-averaged dissipation is related to the power injected by the wind, approximately 1% of the wind power transported downward past the ship's anemometers by the wind being dissipated in the mixed layer (whereas 0.1% is converted to potential energy in deepening the layer). At the equator, the proportion of wind power being dissipated in the water column is approximately the same. (Does the energy dissipated at the equator come directly from the local wind or is it released from the mean shear?). In the convectively-mixed layer the dissipation is related to the buoyancy flux in exactly the same way as it is in the atmospheric boundary layer, the dissipation being 45% of the buoyancy flux through most of the mixed layer. This contrasts with the wind-stirred layer, where the dissipation decreases as the reciprocal of the distance away from the surface except for an increase near the base of the layer.

## The Scaling of Mixing

It would be convenient if there were a way to estimate vertical mixing from some easily-measured property of the water column. The most easily-measured quantity is the stratification - usually expressed as the buoyancy frequency, N. We expect a layer of great stratification (large N) to present a barrier to vertical mixing, whereas in a layer of weak stratification much more mixing is expected. Gargett (1984) summarizes the evidence for an inverse dependence of eddy diffusivity on N in many contexts, from shallow lakes to the deep ocean. (Such a relation is not expected to hold in the vicinity of specific energy sources like the surface or bottom of the ocean.) A model of breaking internal waves indicates this result to be reasonable if the energy is indeed derived from breaking internal waves (Gargett and Holloway, 1984). To date, direct measurements in the ocean have contributed little to this question because of the inadequacy of our sampling.

Measurements of dissipation by the "airfoilprobe" method to more than 2000m in the western North Pacific show agreement with a direct dependence of dissipation rate on N and an inverse dependence of the eddy diffusivity on N, below 400m (Moum and Osborn, 1986), but this dependence is not strictly monotonic, it is not single-valued and it is seen only with a great deal of averaging, as we expect. The dissipation rates are not large (the largest value below 2000m for a 30m patch was 3 x $10^{-5}$ cgs units), but values above $10^{-5}$ dominated the averages. Deep measurements off Vancouver Island show most values of dissipation rate falling below the instrumental noise level of 2 x $10^{-6}$ cgs (Lueck, Crawford and Osborn, 1983).

Even if the vertical eddy diffusivity is related inversely to N in many contexts, the "constant" of proportionality has to be determined for a given situation. Parameterization in terms of the Richardson number, $Ri = N^2/S^2$ (S is the vertical shear), promises a wider application. (Although at present we do not know how widely applicable a Richardson number parameterization might be. Would it also be site-specific?) A disadvantage of the Richardson number, obviously, is that its use requires shear measurements in addition to the density measurements required for N. In ocean current models, this presents no problem, of course, except that the scale of the grid upon which N is calculated tends to be larger than the scale of turbulent eddies, so that even if turbulent mixing is related to Ri on an eddy scale, it is not clear that the relation will hold on the grid scale.

As a case in point consider the flow above the equatorial undercurrent. Turbulent mixing and friction have been recognized as critical to the calculation of the flow for a long time (Veronis, 1960 [constant eddy coefficients], Robinson, 1966 [eddy diffusivity similar to the atmospheric form]). Recently, various forms have been suggested for relating eddy diffusivities, K, to Ri, usually based on the formulations used by atmospheric scientists (Pacanowski and Philander, 1981; Henderson-Sellers, 1982). All forms represent the relationship as a formula in which the diffusivities decrease as Ri increases. The observations of the Tropic Heat cruise show that, at least on the 20m scale, the relationship between Ri and K may be statistical rather than deterministic. The data of Peters, Gregg and Toole (1986b) are interpreted as representing a Ri dependence of K which has a great amount of scatter and which becomes very steep in a transition range in Ri. On the other hand, the data of Chereskin et al. (1986) indicate a transition between two states, one at large Ri where K is always small, and one at small Ri where values of K are distributed from values as low as those at large Ri to very large values (a log-normal distribution). Neither groups' plots resemble any of the formulations in use even remotely.

Observations in the Sargasso Sea are also difficult to interpret in terms of Richardson numbers even though the Richardson numbers were calculated on a somewhat smaller scale (Marmorino, Dugan and Evans, 1986).

## Turbulence Concepts

Several debates over the nature and description of turbulence continue. Do complicated ocean flows obey the same laws as are obeyed in the simpler laboratory flows? We believe that the temperature and velocity spectra do, so the most surprising (to me) result of the past four years came from Gargett (1985), who found in very careful and complete observations from the submersible PISCES in a tidal flow in Knight Inlet, British Columbia, that temperature fluctuation spectra take the Batchelor form when the velocity field is <u>not</u> isotropic, but do not take the Batchelor form when the velocity field <u>is</u> isotropic. This result is of course directly opposite to expectation. Previously Batchelor forms have been found in situations (large Cox number for example) where the flow is expected to be isotropic. Unfortunately, Gargett's measurements are the only ones for which the isotropy is directly measured. The isotropy parameter she defines, $I = k_s / k_b$, can be shown, if the flux Richardson number is constant (as we expect except near boundaries), to be equivalent to the Cox number criterion for classification used by Dillon and Caldwell (1980), when we found Batchelor spectra for Cox numbers so large that isotropy seemed guaranteed. Could there be some peculiar property of the PISCES, or Gargett's instrumentation, or the tidal flow that accounts for this discrepancy?

Using 3-dimensional small-scale shear data from these PISCES experiments, Gargett, Osborn and Nasmyth (1984) examined the question of isotropy in detail. They found that for $I > 3000$, the velocity components indicated isotropy and an inertial subrange in the spectra. For $600 < I < 900$, isotropy was still found, but the inertial subrange was deteriorating. For spectra with lower I, the inertial subrange disappears but isotropy remains in the dissipation range until $I = 50$. In terms of dissipation,

$$I = (\epsilon/\nu N^2)^{3/4}$$

so $I = 50$ corresponds to

$$\epsilon = 200\ \nu N^2.$$

where $\epsilon$ is the kinetic energy dissipation rate in cgs units and $\nu$ is the kinematic viscosity in cgs units. In the laboratory experiments, the minimum dissipation for turbulence was found to be $25\nu N^2$, so the range of dissipation for which isotropy cannot be assumed is rather small, only the lowest decade of turbulence. For the important oceanic measurements, the larger ones that dominate the averages, $\epsilon$ is comfortably above $200\ \nu N^2$, so calculations based on the common assumption of isotropy at dissipation scales are justified. In the deep sea, where strong turbulence may be rare, this may not be true.

Another question that has been debated for years involves the energy supplied to turbulent eddies. Are turbulent eddies in equilibrium with their energy supply? Or are they created in discrete events and observed only in a "fossilized" condition? We cannot consider this point in detail here. Caldwell (1983) advanced the equilibrium hypothesis, whereas Gibson (1982a, b, c; 1986a, b, c), has consistently believed in a "big bangs" view. Dillon's (1984) results, derived from careful study of the energetics of a number of turbulent eddies, indicated that temperature anomalies die by diffusion within a time less than $N^{-1}$ if their energy source is removed. We have not heard the last of this question.

## Laboratory Results

In a stratified continuous-flow channel, some of our ideas about turbulent scales have been confirmed and quantified (Stillinger, Head, Helland and Van Atta, 1983). A comparison of the decay of grid-generated turbulence in stratified and unstratified salt water showed that, as expected (Osmidov, 1965), turbulent eddies are confined in vertical size between the buoyancy scale and a multiple of the Kolmogoroff scale. To be exact, if L is the vertical patch size, overturning exists only for L such that

$$1.4L_R > L > 15.4L_K$$

where $L_R$ is the buoyancy scale, defined as $L_R^2 = \epsilon/N^3$, and $L_K$ is the Kolmogoroff scale, defined as $L_K^4 = \nu^3/\epsilon$ (Stillinger, Helland and Van Atta, 1983). Smaller eddies are stopped by viscous friction before they can overturn, larger eddies lack sufficient kinetic energy available to convert into potential energy as they overturn in the density field.

The lower limit for the size of an overturning eddy may be expressed in terms of $\epsilon$ as

$$\epsilon > 24.5\ \nu N^2$$

(Stillinger, Helland and Van Atta, 1983). The "constant" 24.5 was found to vary somewhat with the grid mesh size in later experiments (Itsweire, Helland and Van Atta, 1986), implying that the scale of forcing has an influence.

In the ocean, it is observed that the vertical extent of turbulent eddies tends to the value $L_R$ unless otherwise limited. The vertical size is expressed more precisely as the Thorpe scale, $L_T$, defined as the rms displacement of water parcels from their position in a water column consisting of the same parcels but stably stratified (Thorpe, 1977; Caldwell, 1983b). For ocean eddies, $L_T$ approximates 1.2 $L_R$ (Dillon, 1982; Crawford, 1986). In the laboratory tunnel, Itsweire (1984) found a similar result. In both, there is considerable scatter, the meaning of which is not clear.

The turbulence in these experiments does work as it increases the potential energy of the stratified fluid. It is interesting that the efficiency of this conversion is never above 20% (Stillinger, Helland and Van Atta, 1983).

## Internal Waves and Mixing

Several exciting observations have been made directly relating increased mixing rates to specific internal waves. Gregg, D'Asaro, Shay and Larson (1986) followed a patch of water for several days, measuring currents and dissipation. They found what seems to be a close association between mixing activity and maxima in near-inertial motion. The sampling was a bit lean - it is bothersome that contours of mixing parameters depend so much on a single burst of eight casts (Gregg et al.'s Figure 9, burst #5). The contours would look quite different without that burst. Is it truly representative? The result is certainly reasonable. This paper opens some interesting ideas, in particular the notion that turbulence events in the thermocline may be of two types, "puffs", short-lived eddies which are consequences of random coincidences of internal waves (Desaubies and Smith, 1982; Jones, 1983, Padman and Jones, 1985, McEwan, 1983), and "persistent patches", which are not explicable in terms of the random internal wave field.

Near the equator, an intense burst of breaking internal waves was found, a burst in which the dissipation rates were far larger than normal (Paulson, Moum, Caldwell, Chereskin and Regier, 1986). In those waves the dissipation was so great that they could have existed for only a few hours. Bursts almost as intense were found near a front in the Sargasso Sea, but no dissipation measurements are available from them (Marmorino, Rosenblum, Dugan and Shen, 1985). The Sargasso Sea bursts were determined to be internal waves, rather than local Kelvin-Helmholz instabilities (Marmorino (1986b).

Also, in the equatorial region where dissipation rates were found to increase so radically at night, the high-wavenumber internal-wave activity was found to increase simultaneously (Paulson, 1986).

## Rings

The application of microstructure techniques to large-scale problems continued with a study of dissipation and mixing in a Gulf Stream ring (Lueck and Osborn, 1986). This study must be considered preliminary because only 21 profiles are available (not too long ago 21 would have been considered a large sample), but their interpretation is interesting. Even though the vertical profiles of temperature and salinity are similar to those in the Sargasso Sea, dissipation rates in the ring were more than 100x larger. The spin-down time for the kinetic energy of the ring is only 150 days, but there is much more energy stored as potential energy, so the ring must be maintained by conversion of potential to kinetic energy. The eddy diffusivity is not large enough to affect the lifetime of the ring.

## The Bottom Layer

The flow at the bottom of the ocean has a special importance because it supplies much of the friction connected with ocean currents, and because it governs the deposition and movement of sediments. The nature of this flow must be expected to change from place to place as the form of the bottom surface varies, as the magnitude and time-dependence of the external flow varies, as the sediment load varies, and as other local factors enter. Even so, we can attempt to describe it in terms of laboratory flows even though the precise terms will vary with location and time. Is it properly described as a "smooth" flow or a "rough" flow? Does universal similarity apply? What are the effects of the various types of surface roughness elements? How do the variance and shape of the spectra compare with laboratory spectra? Over what time periods can the flow be regarded as quasi-stationary? On these questions significant progress was made in the past four years.

Is the flow smooth or rough? There are locations where the flow must be rough; a bottom covered with inches of shells can hardly generate a smooth flow. On the Oregon shelf, however, we found as pretty a viscous sublayer as you can generate in the laboratory, and we found it on every profile we made (Caldwell and Chriss, 1979). So it is possible to find smooth flow. At a depth of 4000m on an abyssal plain, Elliott (1984) found smooth flow. Gross and Nowell (1985) interpret their data from a tidal flow over a bed consisting of small pebbles and shells embedded in cohesive silt as rough, although the case may not be completely clear because they have no measurements within 19 cm of the sediment, and as we shall see, it is easy to be fooled by form-drag-influenced profiles if measurements close to the boundary are not available. In the North Atlantic, at 4990m depth, rough flow was found in an area expected to contain 10-cm longitudinal ripples (Gust and Weatherly, 1985).

Are the smooth profiles similar to laboratory layers? On the Oregon shelf, we found that, although the viscous sublayer was approximately as thick as expected from laboratory measurements, the thickness varied in time and space more than measurement errors can explain. This variability we ascribe to the effect of surface nonsmoothness (Chriss and Caldwell, 1984a). Unfortunately, no other measurements of the sublayer exist for comparison.

Above the sublayer, departures from the universally-similar profile are caused by roughness elements which take many forms. One effect of roughness elements is to alter the thickness of the viscous sublayer, as above. Another effect might be to destroy the viscous sublayer and convert the flow to rough. Also, the wakes of larger, more distributed obstacles can introduce a local form drag, a vertical and horizontal change in the stress. We found a change in stress, a change from one logarithmic slope of the velocity profile to another, well above the seabed (Chriss and Caldwell, 1982). This change was quantitatively similar to the expected effect of a group of sea-urchins a meter or two upstream of the sensors, and photographs show sea urchins to be ubiquitous in the region of this experiment. (One cannot be entirely satisfied with experimental results which require the presence of imagined sea urchins for their interpretation!) Gust and Weatherly (1985) also found that form drag influenced their profiles, the evidence being a difference between skin friction measured with a heat-flow type sensor and the friction determined from the velocity profile.

Variance and spectra: Comparison of turbulence intensities in the logarithmic layer indicates that, for a given stress, the turbulent kinetic energy is less than for laboratory flows, although the spectral shapes are quite similar (Gust and Weatherly, 1985). In the sublayer and buffer layer we found the same result; although the laboratory scaling worked quite well, the intensities were a bit lower in the ocean (Chriss and Caldwell, 1984b).

To be useful, future experiments on the nature of the flow at the seabed will require detailed determination of the microtopography in the vicinity of the measurement site. The sort of mapping required is now available, using stereo cameras and image processing.

## Double-Diffusion and Intrusions

By double diffusion we mean any effect of the difference in diffusivities between salt and heat in the ocean. A 1984 conference covered many aspects of double-diffusivity, oceanic and non-oceanic (Chen and Johnson, 1984). Interest in the past four years has shifted to some extent from the staircases produced by fingers or by diffusive interfaces to the effect of the vertical asymmetry of double diffusive processes on intrusive "interleaving" structures. Suppose we have a horizontal front across which T and S change to smaller values, t and s, but density does not change. Consider an intrusion of T,S water into the t,s water. At the bottom of the intrusion a region of fingers forms, while at the top a diffusive interface forms. The density ratio, R, will be nearly one because the densities were assumed to be nearly equal. Does the intrusion gain or lose buoyancy, thereby moving up? or down? The answer depends on the transport properties of the two kinds of interfaces, expressed in terms of the flux ratio, $R_f$, which in general depends on R, and possibly on the magnitude of the step in S. It appears that for R near 1, $R_f$ approaches 1 for the diffusive interface, but remains less for the fingering interface (McDougall and Taylor, 1984). That is, the fingering interface transports density as salt more efficiently than as heat. Our hypothetical interface loses weight in salt faster than its density is increased by cooling, and it will rise, thereby transporting its properties across isopycnals. The opposite case, an intrusion from the t,s region into the T,S region, will gain density and fall (Schmitt and Georgi, 1982).

The intrusions seen by van Aken (1982) in the North Rockall Trough had this character: the warm, salty intrusions are light, the cold, fresh intrusions are heavy. Earlier Gregg and McKenzie (1979) observed an intrusion of this nature.

The relative significance of shear and buoyancy forces in intrusions was addressed by Larson and Gregg (1983), principally for some intrusions in the Bahamas. They compared kinetic-energy dissipation rate with buoyancy flux, and found that on the diffusive surface of the intrusions the dissipation was less than the buoyancy flux, whereas on the fingering surface the dissipation rate was larger. This result is dependent on the laboratory-derived flux relations, which are uncertain by a factor of 2. In observations in the Gulf Stream accompanied by electromagnetic shear measurements they found sufficient shear in the interfaces to lower the Richardson number to the neighborhood of 0.25, suggesting that shear production of turbulence could be significant.

How does the Coriolis force affect the growth of intrusions? Chereskin and Linden (1986) found in lab experiments that it inhibited their formation and lateral spread. A larger Rayleigh number is required for the onset of double-diffusive layering.

Are intrusions produced by double-diffusive processes? Although double-diffusive processes may be significant in the evolution of an intrusion, we expect that other forces are responsible for their generation. The fact that an intrusion crosses isopycnals has been taken as an indication that double-diffusive processes are significant in its evolution, but this may not necessarily be so.

Woods, Onken and Fischer (1986) observed an intrusion that was traveling in the wrong direction for the double-diffusive process to be dominating its dynamics, although it was crossing isopycnals. They produced model results showing that double-diffusive effects are not a necessary condition for the crossing of isopycnals.

Are double-diffusively driven fluxes important in the ocean? Posmentier and Kirwan (1985) suggested that it may "refresh" mesoscale rings. Garrett (1982) calculated vertical (diapycnal) fluxes due to this effect, in a computation based on a combination of uncertain formulas concerning the properties of such intrusions, and found that the vertical fluxes might be important in regions with weak vertical stratification and strong horizontal gradients. McDougall's (1983b) calculation with a steady-state finite-amplitude model yields, on a basin-wide basis, negative diapycnal diffusivities, for salt, heat and buoyancy, of 0.1 cgs units ($10^{-5}$ $m^2$/sec).

An interesting study of the evolution of a single intrusion between constant-property layers showed that, depending on the initial conditions, such a layer can 1) overturn upward, 2) overturn downward, 3) have both interfaces become diffusive, 4) both become fingering, or 5) simply slowly change properties (Ruddick, 1984). Like all such studies, it depends on the flux laws for the two types of interfaces, which are not well-established, especially in the crucial regime in which R approaches 1. In the only flux-law experiment, Newell (1984), in diffusive-interface experiments for large R, found a transition in interface character which occurred for R between 6 and 10. In that range the interface changes from a boundary layer character to a solidly-established diffusive core. The heat flux as a function of R, for R>4, lay between the curves estimated by Huppert (1971) and by Marmorino and Caldwell (1976). For use in many calculations it would be reassuring to have more careful experiments; it's not clear however how to set up an experiment that is more representative of oceanic conditions than the past experiments.

Another calculation derived from the "flux laws" uses an empirically-derived formula for the layer step height in oceanic diffusive staircases in conjunction with the flux laws for diffusive interfaces to derive flux-gradient relations for a diffusive staircase (Kelley, 1984). A house of cards, indeed.

## Salt Fingers

In 1983 it appeared likely that the mixing properties of large sections of the oceans were determined by salt fingering (Schmitt, 1981). Since then, we have seen no reason to doubt it, but no further proof either. The rather sparse observational work on salt fingers in the meantime has focused on direct observations. Osborn and Lueck's (1985a) observation of salt fingers from a submarine is interesting because those fingers were found fairly near the sea surface and because their stability ratios were as high as 4, surprising because fingers have a low growth rate at such a high value of the stability ratio and therefore are vulnerable to competing processes like the turbulence expected so near the surface.

Observations by Marmorino (1986a), who used towed conductivity sensors in the seasonal thermocline of the Sargasso Sea, are in agreement with Gargett and Schmitt's (1982) observations in the central waters of the eastern Pacific and Mack's (1985) data from the Sargasso Sea. All observed salt fingers in 1-m thick layers several hundred meters in width. The depth limitation was provided by the variability in the salinity profile rather than by some properties of the fingers themselves, such as collective instabilities.

One physical effect that is interesting, though probably not important in the bulk of the ocean, is fingering made possible by the Soret effect, the transport of salt caused by a gradient of temperature. According to McDougall (1983a), fingers can occur under certain circumstances even when the gradients of both salt and temperature are stable! A recent measurement of the fractionation of seawater-like solutions by the Soret effect might be mentioned in this connection (Caldwell and Eide, 1985).

An interesting experiment is the observation of fingers in a Hele-Shaw cell (Taylor and Veronis, 1986). In the Hele-Shaw cell it is easy to watch individual fingers, their development and their adjustment to changing circumstances.

## Biological Interactions

One area of research just in its infancy is the interaction among turbulent mixing (and various other water motions) and the distribution of biota (Tett and Edwards, 1984). Interactions between mixing and life in the ocean have usually been considered on the larger scales: vertically in terms of mixed layers vs thermocline, horizontally in terms of patchiness on scales of kilometers (Denman and Platt, 1976). Now, much smaller scales are drawing interest. For example, millimeter-scale patchiness has been found to influence competition among algae (Lehman and Scavia, 1982). On the other hand, there is evidence that an effect we thought might be very important, the direct effect of mixing on photosynthesis by the mechanism of cycling organisms in the light field, may not be significant after all in terms of productivity; apparently the organisms adjust as quickly as required to take advantage of the light levels to which they are carried by the mixing motions (Gallegos and Platt, 1982).

The importance of phytoplankton distribution to the mixing in the water column was emphasized by Lewis, Cullen and Platt (1983), who found that the distribution can vary so much that differential absorption of solar radiation can cause heating rates uneven enough to affect stability significantly.

The influence of mixing on diversity was pointed up by Longhurst (1985) when he found much greater diversity among large zooplankton in the thermocline than in the mixed layer.

Still lacking are truly-coherent measurements of mixing and bio/chemical parameters. If we could reduce the scales of in-situ bio/chemical measurements to 10 cm and if we could make the bio/chemical measurements from the same instruments as physical mixing measurements, I believe that a whole new illumination of biological processes would result.

## Instrumentation

In the past four years, improvements have come more in vehicle development and deployment techniques than in the development of new sensors. Emphasis has been on submersibles and quickly-sampling profilers, for good reason: these vehicles make possible the volume of sampling required to form a complete image of the intermittent processes in the ocean. In the upper ocean, at least, we have learned 1) that the turbulent processes are so intermittent that approximately 20 casts are required to accumulate sufficient statistics to define a given situation, and 2) that the time over which upper ocean conditions change is a few hours. A few years ago, we thought that diurnal changes were significant only in the surface mixing layer; now it has become apparent that the diurnal signal penetrates the thermocline strongly (Moum and Caldwell, 1985; Gregg, Peters, Wesson, Oakey and Shay, 1985). Therefore in the mixed layer and thermocline we can expect stationarity in time only for a few hours. As we become capable of determining horizontal variations continuously, we are beginning to see that conditions can change drastically in only a few kilometers (Moum, Stabeno and Caldwell, 1986: Paulson et al., 1986). Therefore we need either 10 casts per hour, which such instruments as the AMP(Advanced Microstructure Profiler; Gregg, Nodland, Aagaard and Hirt, 1982), or the RSVP (Rapid Sampling Vertical Profiler; Caldwell, Dillon and Moum, 1985) can just barely perform in the upper 100 meters, or the measurements must be made continuously in the horizontal. Continuous horizontal measurements are made either by submersible (Gargett, 1982; Osborn and Lueck, 1985b) or by towed body (Osborn and Lueck, 1985a; Tang, Bennett and Lawrence, 1985).

Fast sampling profilers: In order to sample at the repetition rate required, a number of groups are now using "slack-line" instruments. Although these instruments are connected to the ship by a datalink, which also serves as a retrieval line, they are constructed so that the basic force balance is between the weight-in-water of the instrument and its hydrodynamic drag. Sufficient drag and weight is present in the instruments themselves that the drag and weight of the line is not important by comparison. If this is accomplished, the instrument will fall with a constant speed, and the motion of the ship will not be transmitted to it through the line. The line must either be powered out as in the AMP, or payed out from a low-drag winch as in the RSVP. In either case, the scheme seems to work quite well, yielding surprisingly low noise levels in the shear signals. (The noise level in the kinetic-energy dissipation rate measurement is most recently only $10^{-5}$ cgs units, not much higher than the noise level of much larger, truly freely-falling instruments)

The AMP and the RSVP represent an interesting contrast in solutions to a similar technical problem. The design objectives are somewhat different, the RSVP being designed for deployment from a ship under way in conjunction with the thermistor chain whereas the AMP is designed for deployment from a "stationary" ship. The RSVP is lighter (5 kg vs 18 kg) and thinner (5 cm vs 17 cm), although the lengths are similar (1.9 m). The drag for the AMP is supplied by stationary brushes whereas the brushes on the RSVP fold on retrieval to lessen the strain on the line (critical because of the additional drag caused by the superposition of the ship's speed on the retrieval speed).

Placing recording equipment inside these instruments would make them too large to handle easily and also would make the dumping of data necessary between casts, slowing the sampling. Therefore each has a datalink to allow recording on shipboard computers. The AMP's datalink is an optical fiber, which has the advantage of a thin line and a wide bandwidth. The multiconductor cable of the RSVP conducts power to the instrument, so batteries are unnecessary and the circuitry in the instrument is simpler.

The sensors on these instruments are nearly identical, the exception being the use of a platinum film on the AMP to get high-frequency temperature information, as opposed to the microconductivity sensor on the RSVP which obtains high-frequency conductivity data. The other sensors on both are thermistors, shear probes, and NBIS conductivity probes.

Upwardly profiling with such an instrument, profiles extend to the surface. The Wave Zone Profiler has produced some interesting results discussed elsewhere in this report (Dillon, Richman, Hansen and Pearson, 1981).

Deeper Profilers: In order to obtain absolute velocity information from a passive electromagnetic system, Sanford has added an acoustic Doppler to his freely-falling profiler (Sanford, Drever and Dunlap, 1985). The Doppler measurements, available when the instrument is in the range of 60 to 300 meters from the bottom, reference the electromagnetic shear profile in absolute terms to within 1 cm/sec. This means that absolute velocity profiles are obtained without setting navigational transponders.

Several new studies of older instruments were published. TOPS is an acoustically-tracked freely-falling package with an acoustic current meter and a CTD aboard. A study of its response to shear flows has general application (Hayes, Milburn and Ford, 1984). With the measured transfer function, TOPS can determine the shear on scales of 0.2 m to the depth of the ocean

From noise studies of the CAMEL and the fact that the turbulent signal in most of the ocean is beneath its noise level ($3.0 \times 10^{-6}$ cgs units), Moum and Lueck (1985) conclude that the background internal wave dissipation must be less, in agreement with the estimate of Olbers (1983).

Ship's motion can be disconnected from a descending instrument either by free-falling it or by lowering it by a winch that compensates for the ship motion. The necessity of disconnecting CTDs from the ship's motion has been shown in detail by Trump (1983) in terms of the degradation of data quality and by Berteaux and Walden (1984) in terms of the cable loading and possible instrument loss. It will be interesting to see whether the best approach to this problem for routine deployments lies in servo-controlled winches or in free-fall packages. My prejudice is toward light freefall packages; servo-controlling a big winch to handle a heavy package rather than cutting down the package seems like a complex solution to a rather simple problem.

Horizontal sampling has the advantage over vertical casts that more water can be sampled in a given amount of time, and it can be sampled continuously. Towed bodies, small submersibles, full-scale submarines, and vertical chains have all been reported on in the past four years.

A small submersible has been used successfully by Gargett (1982). The advantages claimed are low vibration levels, slow mean speed, and flexible operation. Such a platform would seem useful for specialized applications, especially in such situations as hydrothermal vents, but perhaps not the best in the open sea.

A submarine can operate for extended periods in open water without a surface support ship. By careful mounting of transducers, the noise level in dissipation can be reduced to that of a free-fall vehicle ($10^{-6}$ cgs). On the Dolphin (Osborn and Lueck, 1985a) the transducers are mounted atop a tower which extends 16 feet above the forward part of the hull. Instrumentation includes a CTD and an acoustic current meter for measurement of mean conditions and ship speed, together with a turbulence package, which includes airfoil probes and a fast thermistor. Because a given flow environment extends farther in the horizontal than in the vertical (often by 100:1), the submarine can obtain long samples under statistically stationary conditions. Salt fingers near the surface were seen that would not have been seen by a vertical profiler.

Even though they recorded some successes in the past, towed instrument packages have not been used extensively for turbulence measurements recently, probably because of the fear of trouble with vibrational noise. Osborn and Lueck (1985b) have developed a towed body that shows great promise. By using a streamlined body with little drag and little hydrodynamic depression, and by decoupling elastically at both ends of the tow line, they have managed to limit the noise level in dissipation to $2 \times 10^{-5}$ cgs, which is 20x worse than the better free-fall profilers, but which is quite adequate for measuring turbulence in near-surface regions where the dissipation can average $10^{-3}$ cgs. This system seems to have great potential, especially if used in conjunction with a vertical profiler.

Conductivity sensor response has been measured for the Sea-Bird sensor (Gregg and Hess, 1985) and has been predicted theoretically for tube-type sensors by Topham and Perkin (1984), whose calculations agree with the measurements of Gregg, Schedvin, Hess and Meagher (1982) on the NBIS transducer.

Dissolved oxygen sensing may become more practical in situ because of several recent developments: 1) Pulsing an ordinary membrane-type sensor seems to improve its reproducibility greatly (Langdon, 1984), but produces measurements no closer than several minutes in time, and 2) Tiny, faster-responding (0.2 seconds) probes have now been used in the ocean (sediments) and would seem to have the potential for increasing the speed of the measurement (Reimers, Fischer, Merewether, Smith and Jahnke, 1986)

Acoustic Doppler Current Profilers have been used for a few years by specialists (Crocker, 1983; Joyce, Bitterman and Prada, 1982; Regier, 1982; Trump, Okawa and Hill, 1985), but their routine use as a ship-operator-supplied service has only arrived in the past year or so. A better picture of the upper ocean, particularly in non-specialist experiments, should emerge. In the next several years we might hope that moored units can replace current-meter strings in the upper ocean. If only their vertical resolution could be made 1m instead of 10m, the data would be much more useful in, for example, calculating truly relevant Richardson numbers. Unfortunately, because of beam separation in combination with internal waves, and because of the averaging required to suppress ship motion, it appears that another method will be required to reduce the vertical scale of measurements to the size of typical turbulent eddies. Acoustic correlation techniques may have potential (Farmer and Crawford, 1983).

## Directions

The study of turbulence and mixing in the ocean has barely begun. My own prejudices as to the most exciting possible projects are:

1. Follow-ups of the discovery of plunging sheets of water in the mixed layer (Weller, Dean, Marra, Price, Francis and Boardman, 1985): Do these sheets occur often? What are their dynamics? Is their transport an important part of the total vertical transport? We must imagine that even more intense effects are found in really big winter storms.
2. Measurements of the 3-dimensional shear in ocean conditions to compare with the submersible observations in the Knight Inlet tidal flow (Gargett, Osborn and Nasmyth, 1984; Gargett, 1985): Would we really find no Batchelor spectra for intense isotropic turbulence, but yet spectra resembling the Batchelor spectrum for weak, non-isotropic turbulence? Is the criterion for isotropy the same as in the tidal flow?
3. Extension of the measurements of turbulent eddies to 2 and 3 dimensions: What does an eddy look like viewed from the side? A rule of thumb is that features tend to be 100x wider than they are tall. Is this really so? Does it mean that eddies are short and fat?
4. Studies of the Richardson number parameterization: We owe it to the modelers, who have been proceeding without any real information for so many years. To examine the parameterization properly, the shear is needed on vertical scales down to a meter or less, and yet a large amount of sampling is required - an as-yet unsolved instrumental problem.
5. Determination of the extent of the effects found at the equator: the nighttime thermocline mixing, the diurnal internal-wave cycle, the intense, highly-dissipative internal wave bursts. Do they persist throughout the year at the equator? Do they extend through more of the ocean than just the equatorial region?
6. A careful laboratory study determining the flux laws for fingering and diffusive interfaces, especially for stability ratios near unity: What is the dependence of the flux laws on mean shear? On internal wave motions?
7. Deeper: We do not even know the sampling requirements yet for the study of mixing below the seasonal thermocline. How much mixing is there in the ventilated thermocline? How do deep eddies affect the turbulence and vertical transport properties?
8. The bottom layer flow is important, but a useful experiment in any but the simplest of

situations requires measurements of the microtopography in the vicinity of the measurement site and measurement of the velocity profiles with appropriate resolution to within a centimeter of the sea floor. Otherwise the basic state of the flow is unknown.

9. Instruments need to be built that can measure the full spectrum of shear, especially the meter-scale vertical shear, and yet can sample often enough to form a description of the oceanic processes. I suggest that the repetition period of sampling needs to be shorter than half the local buoyancy period, that is, that the Nyquist frequency of the sampling should be greater than the buoyancy period. A spatial requirement is harder to guess at, possibly half the wavelength of the shortest energetic internal waves.

10. Instruments need to be built that measure the bio/chemical fields in a manner truly coherent with the physical measurements. Perhaps oxygen microprobes and laser-fiber-optics fluorometry are practical directions.

Acknowledgements: Our work has been supported by the Physical Oceanography Section of the National Science Foundation, by the Environmental Sciences Branch of the Office of Naval Research, and by the Ocean Sciences Branch of NASA.

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(Received October 9, 1986;
accepted December 10, 1986.)