Comment

Anand Gnanadesikan

It is always easy to find flaws in a review paper which attempts to cover the whole of one's field. It is a persistent feature of such papers that they will inevitably omit what someone considers the central issue of the field. While the present report does contain some such omissions, it also raises a great many important issues. The application of a number of these issues to the field of satellite remote sensing is well presented in the report and is, in my opinion, important to the development of the field as a whole. In this discussion, I would like to amplify some of the issues raised, by looking at some different examples than those presented in the text. The use of these examples should not be taken as denigrating the importance of satellite-based remote sensing for understanding oceanic dynamics. Rather, I would like to show how some of these issues raised are of broad interest to a range of oceanographers.

In making comparisons between theoretical models of the ocean and real data, a number of problems may arise. In this paper I will focus on four such problems, illustrating each with a separate example.

1. Are the fundamental assumptions of the theory statistically valid? If not, does this explain the difference between theory and data or differences between different measurements?
2. Can we find a theoretical quantity that means something?
3. Can we extract this quantity from the data?
4. What are the errors involved in making the measurement and do they explain any discrepancies between theory and data?

One example where the statistical validity of a theory has been the subject of much discussion in the oceanographic literature is the question of microstructure and eddy diffusivity. Section 1 of the report noted that mixing in the equations of motion is often parameterized using an eddy viscosity (in the case of momentum) or diffusivity (in the case density). One of the standard ways of estimating the deffusivity is by looking at the velocity shear on scales of a few centimeters. The turbulent dissipation ε (representing the conversion of kinetic energy to heat) may be estimated from the small-scale shear

$$\varepsilon = 7.5\nu \left\langle \left(\frac{\partial u}{\partial z} \right)^2 \right\rangle, \tag{1}$$

where ν is the molecular viscosity, u is the horizontal velocity along some axis, z is the vertical direction and $\langle\ \rangle$ denotes averaging of the shear variance over a wavelength range from 100 cm to the order of 1 cm. Within the stratified interior, the turbulent dissipation is forced by instabilities with scales of order 100 cm associated with the field of internal gravity waves. The assumption is made that the turbulence over some portion of the water column is in a statistical steady state and that some fixed fraction of the energy f (of order 0.2) goes to transporting density. Then if the density flux is given by $-K_v \partial\rho/\partial z$, where K_v is the vertical eddy diffusivity and $\partial\rho/\partial z$ the density gradient, then

$$K_v N^2 = -K_v \frac{g}{\rho}\frac{\partial\rho}{\partial z}$$

is the energy flux required to support this density flux and

$$K_v = \frac{f}{1-f}\frac{\varepsilon}{N^2} \tag{2}$$

(N is the buoyancy frequency–of order 0.001 to 0.01 Hz—which is the natural frequency of oscillation of a fluid parcel in a stably stratified water column). These measurements yield eddy diffusivities of order 0.1–0.5 cm^2/s (Gregg, 1987; Polzin, 1992).

If one looks at closed basins where ocean water enters at one temperature, is warmed by diffusion and upwells throughout the basin at the warmer temperature, it is possible to estimate the required eddy diffusion coefficient (Munk, 1966; Hogg et al., 1982; Johnson, 1990). Using these methods, the required eddy diffusivities are of order 1–5 cm^2/s, a difference of an order of magnitude. Explaining this discrepancy is one of the more interesting problems in physical oceanography today.

In a series of papers in the 1980's, Gibson (1986, 1987) proposed that the reason turbulence measurements underestimate the diffusivity is that the actual mixing events are themselves very rare. He

Anand Gnanadesikan is a graduate research assistant at Woods Hole Oceanographic Institution, Woods Hole, Massachusetts 02543.

noted that the underlying distribution for ε is approximately lognormal and intermittent. By estimating $\sigma_{\ln \varepsilon}$ from a number of data sets Baker and Gibson (1987) argued that profilers were missing most of the energetic mixing events and grossly underestimating the real diffusivity. He also argued that far larger samples (hundreds or thousands rather than dozens) of profiles were actually necessary to estimate the true diffusivity.

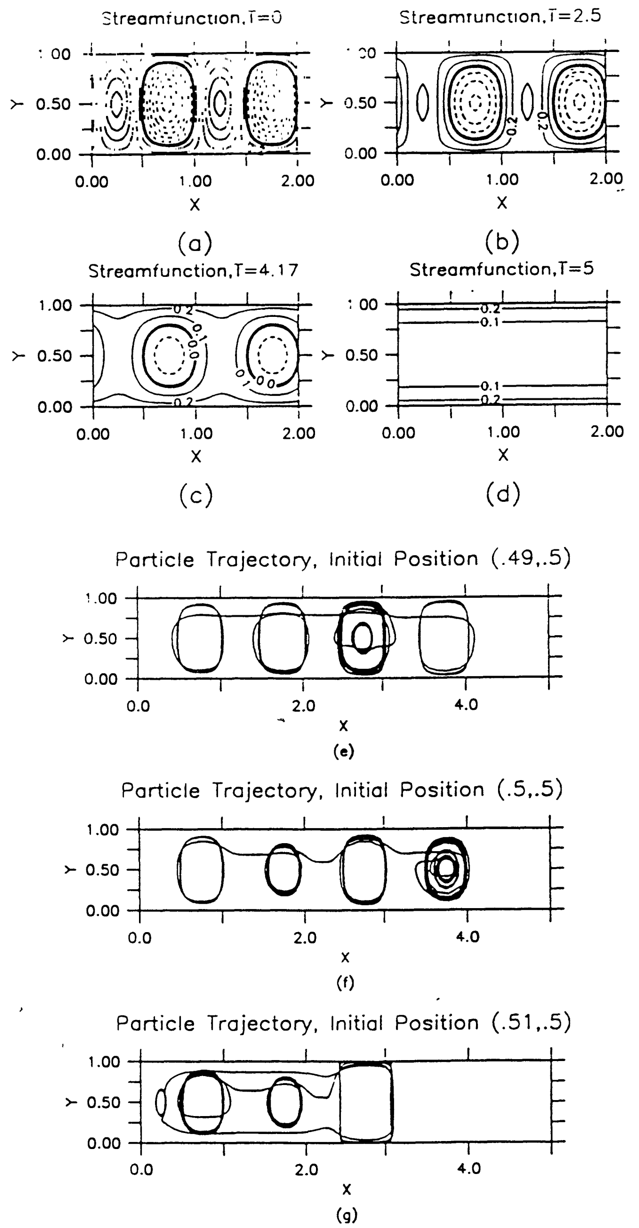

FIG. 1. *Illustration of chaotic advection:* (a)–(d) *stream function of the underlying flow field (given by equation* (3) *for* $L_x = L_y = 1$, $\omega = 2\pi/10$) *at various times illustrating a half-cycle of a blinking vortex; contour interval is* 0.1; (a) $T = 0$; (b) $T = 3.33$; (c) $T = 4.17$; (d) $T = 5$; (e)–(g) *particle trajectories over five cycles* ($T = 0$–50) *for three points initially close together*; (e) *trajectories starting at* $X = 0.5$, $Y = 0.5$; (f) *trajectories starting at* $X = 0.49$, $Y = 0.5$; (g) *trajectories starting at* $X = 0.51$, $Y = 0.5$.

Very few oceanographers accept this argument. There are good reasons for their stance. Profiler observations have been found to be relatively consistent and repeatable. A recent paper (Gregg, Seim and Percival, 1993) showed that for a number of data sets the spread in ε could be understood by normalizing the rate with amplitude of the internal wave field. When a single data set representing a homogeneous wave field was considered, much of the spread vanished. They argued that the large values of $\sigma_{\ln \varepsilon}$ were the result of mixing data from different data sets with different levels of energy in the wave field and thus differnt mean values of ε. Additionally, new measurement techniques which use very tiny amounts of passive tracers to measure diffusivity seem to be yielding results consistent with the turbulence profilers (Ledwell, Watson and Law, 1993). Nonetheless, the discrepancy between basin-averaged diffusivities and point-averaged diffusivities remains a nagging one. While the recent paper by Gregg, Seim and Percival is a major step in the right direction, informed statistical comment on this issue might provide useful insight.

We have seen how uncertainty about the fundamental statistics governing oceanic mixing can affect our estimate of the vertical eddy diffusivity. Turning now to the horizontal diffusivity, we will consider a process where the theory may be well defined, but it is unclear how to convert the theoretical results into something that might be meaningfully tested. The process is known as chaotic advection (see Ottino, 1989, for a review). Its basic premise is that one can take a flow field which is perfectly deterministic in the Eulerian (fixed point) sense, and end up with particle trajectories which are chaotic. It is an excellent illustration of the problem, summarized well in Section 3 of the report, of going between a Lagrangian and Eulerian representation of flow fields.

A simple example of chaotic advection may be gained by considering the model flow field

(3a)
$$\frac{dx}{dt} = u = 2\left(y - 1/2\right) - \sin\left(\frac{2\pi x}{L_x}\right) \cdot \cos\left(\frac{\pi y}{L_y}\right)\left(1 + \cos\left(\omega t\right)\right),$$

(3b)
$$\frac{dy}{dt} = v = 2\frac{L_x}{L_y}\cos\left(\frac{2\pi x}{L_x}\right) \cdot \sin\left(\frac{\pi y}{L_y}\right)\left(1 + \cos\left(\omega t\right)\right),$$

where u is the east–west velocity and v is the north–south velocity. The flow field is shown in Figure 1 a–d for $L_x = L_y = 1$, $\omega t = 0$, $\pi/2$, $5\pi/6$ and π. The flow corresponds to a linear shear with superimposed vortices which "blink" on and off. Figure 1e, 1f and 1g shows trajectories of three particles in the above flow field which start off close to each other and diverge over time. In a remarkable paper, Ridderinkhof and Zimmerman (1992) showed that chaotic advection similar to that described above may actually occur in the field as a result of tidally driven flows. In such cases, the question of what the actual "horizontal diffusivity" should be a difficult question and would depend critically on the scale over which one was working. For spatial scales long compared with L_x and temporal scales long compared with T, one might treat chaotic advection as a random-walk problem and use ωL_x^2 as a reasonable approximation for the diffusivity. On spatial scales small compared with L_x and temporal scales small compared with $1/\omega$ it is not clear at all that this would be the right scale to use. (Another question of interest in connection with this flow field is how many Lagrangian floats would be needed to characterize such a flow accurately.)

We have seen that non-Gaussianity and nonlinearity may make for difficulty in matching a theoretical prediction with field data. The fact that the data itself is nonlinear may also cause problems. To see this, we turn to yet another example from the field of oceanic turbulence.

In *Walden*, Henry David Thoreau noted that when the wind blew "I see where it dashes across the water by the streaks" (Thoreau, 1854). Langmuir (1938) showed that these streaks were due to helical vortices aligned with the wind which created slicks in their convergence zones. In honor of his pioneering investigations, the vortices have come to be known as Langmuir cells. Figure 2a shows contours of stream function from a numerical model of Langmuir cells which I have developed as part of my dissertation work. The arrows show the direction of the crosscell velocity. Figure 2b shows contours of velocity in the alongcell direction. There are strong plumes of alongcell velocity (denoted by the "+" marks) associated with downwelling zones. These plumes are strongly nonlinear. More than one scale of cells may be present at one time. In between the plumes there are regions of low alongcell velocity (denoted by the "−" marks) which are associated with upwelling zones. The velocities associated with these plumes are of order 5–10 cm/s. When attempts are made to measure the velocities of Langmuir cells with current meters, they are made in an environment with surface gravity waves (with frequencies of 0.1–1 Hz and velocities of order 100 cm/s) and low-frequency tidal and inertial motions (with frequencies of 10^{-4} Hz and velocities of order 50 cm/s). Langmuir cells, as noted in the report in Figure 2.1, fall in an intermediate band in frequency and have much smaller velocities.

Our group at Woods Hole has attempted to isolate structures similar to those seen in these model runs by looking at data from strings of current meters suspended off of stable platforms and off of buoys. Our basic hope was that the low-frequency currents would sweep cells past the current meter array, so that the time series from a vertical array of current meters would correspond to a two-dimensional spatial slice. While we have had very occasional success with reconstructing cells from the current meters (Weller et al. 1985) and have been able to relate the overall variance to dynamical quantities (Weller et al., 1993; Gnanadesikan, 1994), it has been hard to extract consistently features which we could identify in all confidence as Langmuir cells. Part of the problem is that the velocity structures associated with the plumes are highly nonlinear. As a result, the energy associated with the plumes may have a large spread in frequency space, making simple band-passing difficult. Additionally, velocities in the alongcell and crosscell directions are out of phase. As a result, empirical orthogonal function analysis will tend to split a single wavelength of cells into alongcell jets and crosscell vortices. This is illustrated in Figure 2c and 2d, where the empirical orthogonal functions are computed from the model velocity fields in Figure 2a and 2b. The solid lines show the horizontal velocity in the crosscell direction, the dashed lines the horizontal velocity in the alongcell direction and the "+" marks the vertical velocity. The first mode (containing 39% of the variance) isolates the horizontal velocities associated with the vortices, while the second mode (containing 15% of the variance) isolates strong vertical and alongcell velocities associated with the downwelling zones. Simple time domain EOF's are clearly not the right method for isolating structures of this sort.

The problem of isolating signal from noise is closely related to the fourth issue mentioned earlier, that of characterizing errors associated with measurement techniques. While space does not permit me to explain the problem fully, I feel it would be remiss not to point out one context in which considerable room for interaction between statisticians and oceanographers exists, that of acoustic tomography. The reader will doubtless be familiar with the concept of the CAT scan, in which an array of x-rays is used to create three-dimensional images. This problem is one of the outstanding examples of inverse theory. Recently, similar techniques have been proposed to look at oceanic circulation. A schematic of how this works is shown in Figure 3. In much of the ocean, the profile of sound speed with depth has a minimum at about 1000–1500 meters depth. This low-sound speed layer acts as a waveguide along which sound can propagate for distances of thousands of kilometers. Because there a pulse of transmitted sound will be received

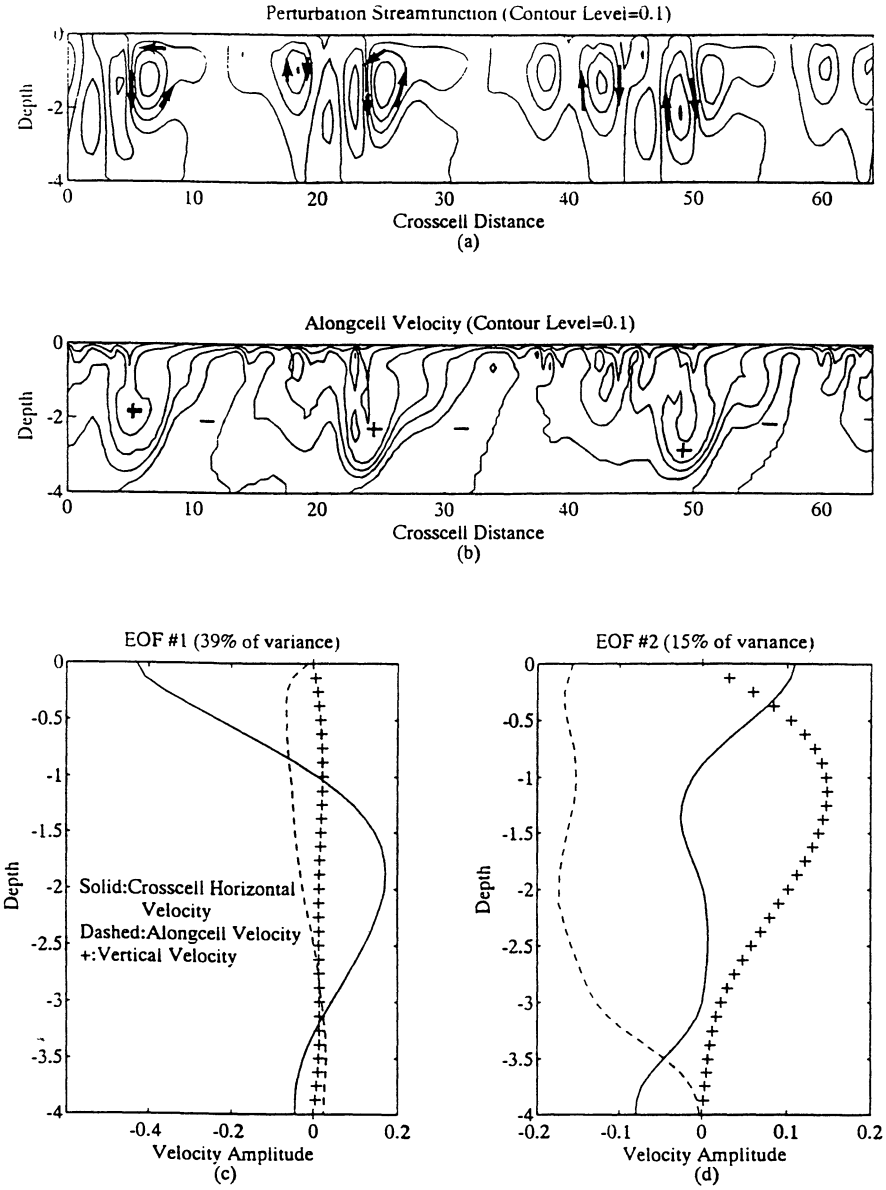

FIG. 2. *Isolating Langmuir cells from model data:* (a) *crosscell stream function field associated with Langmuir circulation from a numerical model; arrows show the direction of flow;* (b) *alongcell velocity field associated with Langmuir circulation from a numerical model, showing plumes of high alongcell velocity* (+) *and regions of low alongcell velocity* (−) (c) *velocity structure versus depth for first EOF of the velocity field shown in* (a) *and* (b); (*solid line*) *crosscell horizontal velocity,* (*dashed line*) *alongcell velocity,* (+) *vertical velocity;* (d) *same as* (c) *for second EOF.*

as a series of pulses at a receiver, each corresponding to a different path, each received pulse will sample a slightly different portion of the water column. Since the sound speed is a strong function of temperature, one can use changes in the travel time of various pulses to infer changes in the temperature structure. For more details, the reader is advised to refer to Munk and Wunsch (1979), which introduces the concept of acoustic tomography, and Chester (1993), which discusses results from a field experiment.

One question which arises in regard to measurements of this type is their "response function" to oceanic phenomena. What kinds of signals will tomographic measurements pick up and what phenomena will they miss? Can the measurements be tuned so as to better pick up certain features? It is clear that insight from statisticians would be of help in this area.

To summarize, I would like to reemphasize the points made in the NRC report that oceanic flows are frequently nonlinear, often nonstationary and sometimes non-Gaussian. These facts can lead to problems in comparing models of oceanic processes with field observations. The problems may arise because the underlying statistical nature of the phe-

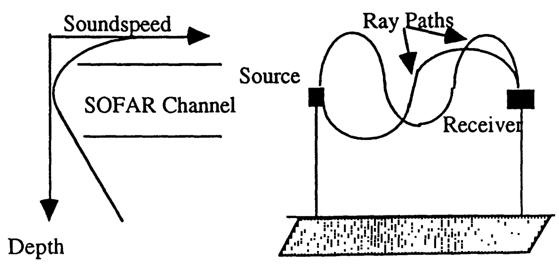

FIG. 3. *Cartoon illustrating the concept of acoustic tomography.*

nomenon is not well understood, because it is difficult to decide how to go from theory to data, because extracting nonlinear structures from data is difficult or because the sensitivity of measurement techniques to various phenomena is not well understood. Informed statistical expertise is essential to alleviate these difficulties.

ACKNOWLEDGMENTS

Ramanathan Gnanadesikan commented on an early version of the paper. Kurt Polzin made a number of useful sugestions about a later draft. This work was supported by the Office of Naval Research under contracts N0014-91-J-1495 (Surface Waves Processes Program). Computing resources were provided under contract N0014-91-J-1891. This work is contribution no. 8595 of the Woods Hole Oceanographic Institution.

Comment

Greg Holloway

The panel has done a commendable job of collecting material of such diverse nature into a concise, readable overview which recounts a brief history of the subject, present state of the art and also some research outlook. Here I only mention a research thread which is not included in the report—not included for good reason: the topic is sufficiently controversial that it may well be set outside of more "mainstream" directions. The question is to what extent methods from statistical mechanics may help clarify what we suppose are the "equations of motion" for oceans.

First reaction to this question is often dismay. Although ability to observe the ocean is limited, and ability to model the ocean numerically is limited, at least we have the equations of motion. They come from textbooks after all. Yet, when we think of some of the very reasons that move us to statistics (viz., limited ability to observe a noisy system), we may reconsider the confidence with which we know the equations of motion. When a numerical model computes temperature or velocity or elevation at some grid point at some time, do we really mean that is supposed to be the temperature at that point at that time? Or do we have in mind some expectation for some space-time "lumped" temperature? Conceptually we might pose the problem as follows: Given a probability distribution for possible states of an ocean at time t_0, and given a probability distribution for forcing functions (possibly also probabilistic boundary geometries), what is the probability distribution for states of the ocean at later time t_1? In principle one might imagine solving a prognostic equation for evolution of probability. In practice this is too ambitious. However, if one had probability at t_1, then it would make sense to ask for temperature, velocity, kinetic energy, etc. as moments of probability. If we cannot realistically hope to solve for time-dependent probability, perhaps we can write equations of motion only for moments of probability. Here our dilemma becomes clear: which textbooks give us equations of motion for moments of probability of ocean states?

It is in this sense that we may not have the right equations of motion. Is this only fancy talk that makes a hard problem harder? It is possible—but here is the controversy—that we might start making skillful ocean modelling easier. At least we may identify systematic corruptions in the presently assumed equations of motion that can be improved upon. There are theoretical hurdles. The few problems that can be dealt with carefully from statistical mechanics are so idealized (such as unforced, inviscid, finite degrees of freedom, quasi-geostrophic) that they are too far from oceanic reality to be deemed meaningful. More meaningful applications including forcing and dissipation can be approached from disequilibrium statistical mechanics but the effort is

Greg Holloway is a Ph.D. at the Institute of Ocean Sciences, which is a part of the Department of Fisheries and Oceans, P.O. Box 6000, 9860 West Saanich Road, Sidney, British Columbia, Canada V8L 4B2.