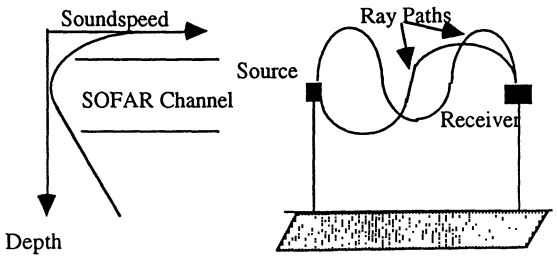

FIG. 3. *Cartoon illustrating the concept of acoustic tomography.*

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

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much greater, new idealizations (such as spatial statistical homogeneity) are needed and so-called theories appear to require adhoc leaps. Researchers may prefer to give up and stick with the "old ways," compensating coarseness of numerical models with ad hoc eddy viscosities. This is mentioned in the panel's report as the "simplest and most commonly used approach." It is not purported to be correct; indeed the panel notes observed eddy fluxes that are countergradient (negative mixing). Regrettably it is profoundly unclear what to do about the "eddy problem." For the most part, modellers await bigger computers which may permit smaller eddy viscosities.

Theoretical studies from statistical mechanics suggest that "old ways" are systematically incorrect. The problem with eddy viscosities, diffusivities and "drags" in general is that they tend to draw mean fields toward a state of no relative motion. From theory, this appears most improbable. More relevant is to consider overall entropy of the ocean as a dynamical system, anticipating generalized forces acting on that system in response to gradients of entropy with respect to coordinates of the ocean state. Are these only big words? In fact one can characterize, if only "roughly," the higher entropy configuration of ocean states; then one can anticipate forces which should arise to drive oceans toward those higher entropy states. Very importantly, such generalized forces are not like eddy viscous drag; they may rather be the propelling force behind aspects of observed ocean circulations.

Despite controversy, a possibility for significant improvement on prognostic ocean models has emerged. The implication also carries over to inverse or data-assimilative models. Given partial observation of some aspects of oceans, one employs dynamics to infer other aspects. But then corrupt dynamics lead to corrupt inferences.

Given the uncertain, preliminary and controversial nature of these comments, it is well that the panel's report omits such matters. They appear here as aside remarks. However, as more attention is given to statistics as descriptors of oceans, the more we are moved to consider what dynamics underlie those descriptors. Tenets of the "old ways," even the presumed equations of motion, come up for fresh review.

Ideas mentioned in these comments are not sufficiently mature to warrant extensive literature. Reviews of the basic ideas, as applied in geophysical fluid mechanics, may be read in Salmon (1982), Holloway (1986) or Lesieur (1990). Examples of more recent investigations can be seen in Griffa and Castellari (1991) or Cummins (1992).

Comment

Andrew R. Solow

By necessity, this report, like the ocean itself, is a good deal broader than it is deep. For this reason, the report is unlikely to stir up much controversy. Let me give it a shot anyhow.

While the report provides an admirably panoramic view of data-rich areas within the field of physical oceanography, it does seem a little short on statistics. This is unfortunate, because there is no reason to believe that it will be harder to teach statisticians what they need to know about physical oceanography than to teach physical oceanographers what they need to know about sound statistical practice. In particular, the need to think carefully about a *statistical* model for data is often lost on oceanographers (and other scientists) in their search for methods. Methods are, of course, a dime a dozen. The trick lies in understanding when and why they work and when and why they do not.

A good example of this is the application of principal component analysis to spatial time series. Briefly, consider a random field $Y(x,t)$ where x is location within some region R and t is time. In a typical oceanographic example, $Y(x,t)$ might represent mean annual sea surface temperature at location x. The field is observed over time at a set of locations $x_1, \dots, x_p$. To reveal spatially coherent temporal variations in the field, it is common practice to extract the first few components from the spatial covariance (or correlation) matrix of the p stations estimated from replications over time and to map the individual station loadings. The oceanographic and meteorological literature is full of this kind of application. One example is given by Jolliffe (1986, page 58).

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