OCEANOGRAPHY AND ENGINEERING[1]

By

JOHN D. ISAACS

Scripps Institution of Oceanography
University of California
La Jolla, California

I have been greatly honored by the invitation to this gathering and to the dedication of this newest and most impressive implement to the study of the province of oceanography.

I wish that I could unveil some blinding breakthrough to shine by its own light in this assemblage. However, I present only a few humble rays that interrelate two great fields of human effort, Oceanography and Engineering, and the light engendered thereby is scarcely adequate to view their raveling. It seems happily appropriate to me to discuss these relations, virtually within sight of this laboratory which we might consider as being recognition of the value and implications of oceanography to a great branch of engineering, Naval Science. In this discussion I hope to indicate several ways in which Engineering and Oceanography are related and to discuss what I consider the most important mutual problem of these two provinces, the need for rational realistic co-operative attack on complex problems.

Certainly I do not need to point out the many ways in which the information of the oceanographer is of importance to the engineer who is involved in marine problems. Such lack of exchange of information as does exist is being reduced rapidly as the engineer becomes better acquainted with the reality of the oceans and becomes better able to ask questions. Recent work on west coast sewer outfalls is an example. Engineers, troubled by the stability of a surface layer of light effluent from their great outfalls, have, with the aid of oceanographers, planned to effect mixing of the effluent below the thermocline and thereby avoid surface stability. Many such examples could be enumerated. Recognition of the variability of ocean waves in ship-model studies is another valuable step toward realism.

In the application of engineering information and methods to oceanography, the record is not so commendable. Certainly much of the engineering information on models, evaporation, turbulence, settling, jets, and mixing is amenable to generalizations, with immense implication to studies of oceanographic processes. A type of problem

[1]Contribution from the Scripps Institution of Oceanography, New Series No. 745c.

that springs to mind is that of the eventual history of the downwardly projected mass of water in a deep-sea comber. Can its momentum be balanced short of the thermocline? Is it important in the deepening and steepening of the thermocline?—in mixing?—in friction? Surely there are many engineering investigations on jet mixing that will increase evaluation of the importance of such possible processes. Woods Hole scientists have employed engineering methods in their important investigations of oceanic circulation. Others, including myself, have applied engineering model laws to such matters as the motion of sea animals, with highly suggestive results. For instance, in studying the model laws of a simple prey-predator relationship, I am convinced that the factors which limit the size of predatory pelagic fishes are demonstrable, that many outstanding characteristics of fish form can be explained, and that probably a strong evolutionary requirement for the development of rapid changes in the schooling habits of pelagic fishes can be demonstrated.

I believe that there is great potential value in exchange of information and methods in this direction and that this should be fostered. However, the fact that one should consider it necessary to suggest a mere exchange between oceanography and engineering points up a far more important and extensive problem in the relationship of the "pure" and "applied" fields. I have felt that here a grave gap exists in our methods of progress toward understanding. To enable me to point out this problem, why it has arisen, and why it is seldom recognized, I will follow, for what they are worth, some contemplations which begin with the hierarchy of man's endeavors to discover the truths of the world.

May we not say that all of man's effort to understand the universe stems from a need to feel more secure within it? I believe that this is so and that the sense of security is of two kinds. The first results from an understanding of "what will something do?" and "what will be something's effects?" Underlying this is the necessity to understand "what is this something like now?" and "how did it get that way?" This is the realm of science, and its "purity" depends inversely upon how far it goes into the crass problem of "how can the effects, particularly on mankind, be altered?"

The sense of security that results from the description of something and from an understanding of the processes which explain its state and its probable future course must consist of a feeling of unity with the environment and must be what we speak of as "spiritual security." Is not the great potential benefit of science to mankind such that men might be relieved from the necessity of explaining physical events by supernatural forces? If so, it is the benefit which has no organized group for its harvesting but which is left as an untouched reward for the worker and some interested gleaners.

The further sense of security results from having altered the effects. This is the province of engineering, which is charged with the imperative statement, "alter the effects of something!" The degree of the "applied" nature of engineering depends inversely upon the degree of its investigations into the questions, "what are the effects of something?" and "what is the something like?" Engineering goes far into these areas, especially where science does not see fit to go. And it does so because the rational approach to its problem of control must be founded on understanding.

Nevertheless, basically science is charged with the problem of "what will something do?" while engineering is charged with "do something about it." The sense of security gained from pure science is a security of the spirit and its reapings are poorly disseminated, while the sense of security gained from engineering is physical and its harvesting is highly organized.

These foregoing statements leave a great area untouched. Originally the "somethings" were natural[2] parts or processes of the universe, but today many of them have already been acted upon by man for their alteration. The sullying effects of man, except for those of the scientists involved, must not exist in the "something" under investigation if science is to be the purest.

This curious idea of the uncleanliness of man's effects on nature, or rather, of man's unnaturalness, has resulted in a great group of studies, intermediate between science and engineering, which science calls applied and which engineering calls basic. These studies are fostered more by engineering than by science, but they suffer from the dichotomy. Surely they are as true a study into the truth as any. Consider metallurgy. A "pure" metallurgy could inquire only into the nature of "native" copper, gold or meteoritic iron. Is there not as much truth in steel as in meteoritic iron insofar as the state of matter is concerned? Yet the metallurgy of steel is relegated to the applied category. Or consider the genetics of *Drosophila* as opposed to horse breeding.

I believe that this attitude and the resultant dichotomy would be entirely defensible had science pursued and organized itself for its most important task to man, the generation of spiritual security through understanding. I cannot see that this has been much considered. One may argue, of course, that the revelation of truth will, in fact, not result in spiritual security. To this speculation there is no answer, for its conditions have not been discovered.

[2] For later emphasis I leave this word as I first unconsciously used it.

For completeness I must continue the argument around the last radian of its circle.

Concerning some geometric theorem, one of Euclid's students asked, "What is its use?" whereupon Euclid directed his assistant, "Give the young man a coin, he needs must be paid for his learning." Thus Euclid stated the precept that "usefullness" from inquiry into truth was only of economic value. I believe that this may have been a far reaching theorem and that its corollary implies the benefits of pure science to the spirit.

It is increasingly apparent that no strict or even qualified definition can differentiate physical from spiritual security. In the light of Greek history or of the present state of mankind, spiritual security cannot exist without physical security. Thus the gambit, offered by Euclid, is again challenged. I believe there is no defensible statement in "purity" of science or in the use of the words "applied" or "practical" or "artifactual" or "military" or "natural" or "unnatural" as applied to any basic difference of investigations into truth.

Let us not associate the matters which are called "directed" and "free" inquiry with the commonly used terms "applied" or "pure" research. This association is frequently met. I think that there is no veracity in believing that any un-directed inquiry exists. All workers attack problems that are directed by some compulsion. I believe that the only type of *directing* of research which is highly objectionable is that which insists on answers before understanding can be attained. On the other hand, certainly answers can be requested when understanding which is probably adequate *has* been attained. This understanding can be attained only by investigation on a broad front, for the threads of truth are pervasive and interweave in a fashion that can be understood only in retrospect.

Let me quote from the Fifth Annual Report to the American Cancer Society by the Committee on Growth:

> As scientific advisor to the American Cancer Society, it is the responsibility of the Committee on Growth to formulate and implement a comprehensive cancer research program. At first glance, the problem of what is and what is not cancer research, may appear to be a relatively simple one. It is true that a number of competent scientists have devoted themselves to the study of cancer and laboratories and institutions have been established for the sole purpose of investigating the cancer problem. Many of the most important discoveries related to cancer, such as x-rays and radium, however, were made by people who had no direct interest in cancer. There is no simple way to determine with any degree of certainty whether a particular research project will make important contributions to our knowledge of the nature and control of cancer.

In 1941, to illustrate, Dr. Charles Huggins of the University of Chicago announced his extremely important discovery regarding the treatment of cancer of the prostate in man by administration of female sex hormones. Although the treatment does not constitute a cure, in a matter of days or a few weeks after treatment is begun, all of the symptoms are gone in many instances and the patient feels completely well. The treatment may be effective for a period of several years.

The foundation for Dr. Huggins' discovery was laid in 1889 by Dr. Joseph Griffiths of Cambridge University in England. Dr. Griffiths, studying the seasonal changes in the size of the prostate gland in the hedgehog, noticed the decrease in the size of the prostate gland when an animal was castrated; this indicated a relation of the male sex hormone to the gland. In 1912, the basis for the second aspect of the problem was formed when Drs. Grosser and Husler, working at a children's clinic in Frankfurt, Germany, first discovered the enzyme, phosphatase, in the membrane lining the intestine. Drs. Steinach and Kun in 1926 demonstrated that the injection of female sex hormones into male animals caused a rapid decrease in the size of the prostate gland. In 1934, Dr. D. R. Davis in Wales and Drs. Baaman and Riedell in Stuttgart independently discovered that there were two different phosphatases, one active only in an acid medium and one only in an alkaline medium. A year later Drs. Kutcher and Wolbergs at the University of Heidelberg showed that the secretion from the human prostate gland was very rich in acid phosphatase. In 1938, Drs. A. B. and E. B. Gutman demonstrated that the blood of patients with cancer of the prostate often contained more than the normal amount of acid phosphatase. The basic information was now available to lead Dr. Huggins to undertake his highly successful studies on the treatment of cancer of the prostate with female sex hormones.

How many of the above investigators who made important fundamental contributions to this great advance in the treatment of cancer would have been supported by funds for cancer research had such funds been available? As a comment on the question, the following paragraph is quoted from the book Science and the Planned State, by Dr. John R. Baker of Oxford University in England:

> 'What central planner, interested in the cure of cancer, would have supported Griffiths in his studies of the seasonal cycle of the hedgehog, or Grosser and Husler in their biochemical work on the lining membrane of the intestine? How could anyone have connected phosphatase with cancer, when the existence of phosphatase was unknown? And while it was yet unknown, how could the men in charge of cancer funds know to whom to give the money for research? How lucky it is for sufferers from cancer of the prostate that Griffiths and Grosser and others were not doing cancer research!'

Certainly these men possess the humility to recognize their lack of understanding and the importance of a broad attack. How different from research "by the numbers"!

To victimize you further in your helpless position, but also to clarify my position to a degree adequate for your competent judgment, let me sketch a notion of the hierarchy of physical science and engineering. Central in this hierarchy are the studies of the great physical provinces, namely oceanography, meteorology, astronomy, and geology, drawing from all of the applicable established disciplines and proceeding toward an understanding of the complex processes in the provinces and their interrelations. Engineering is the study which, through its apprentice industry, reaps the results of the hierarchy for the *physical* benefit of man. Thus, whether it be fisheries, marine architecture, agronomy, applied mathematics, textile testing (or if you like, surgery), I will call it engineering in this broad sense.

In this picture, an established discipline, such as chemistry or genetics, is concerned with the study of apparently consanguinous aspects of several physical provinces. It is believed that these aspects are encompassed by some family of rules by which the worker may be able to answer the question: what will something do? In reality, a discipline attempts to set up an analogue of the parts or processes in one of several ways, depending on the complexity and developmental stage of the study. In complex cases, including most phases of biology, the discipline is an attempt to organize words in such a way that they will set up in the consciousness a mental analogue of the part or process, that is, to describe a static taxonomy or dynamic behavior. In the less complex subjects, such as physics, chemistry, hydrodynamics, physical oceanography, etc., an attempt is made to establish symbols and associated rules of counting which will cause numbers to behave like the physical quantities under study. Also, physical analogues are devised on the basis of assumed similarity of the counting laws, or descriptions of the model, to the part or process. That is, the disciplines attempt to establish a neural, symbolic or physical analogue which will re-enact processes in the past with the greatest attainable similitude so that other related future processes can be predicted with the greatest possible reliability. The symbolic approach is most capable of handling a number of rather simple and similar quantities that behave in simple rigorous ways, whereas the descriptive approach does surprisingly well with inductive results derived from description of a large number of highly dissimilar quantities which behave and interact in equally rigorous but highly complex ways. The great biological laws of evolution and biogenetic recapitulation have been induced from masses of description, although the inexpressible uncertainties of our understanding of biological processes are immeasurable compared with those expressed uncertainties of the uncertainty principle of atomic physics.

Frequently, in different disciplines the symbolic and descriptive approaches almost become fetishes. Both extremes certainly are suspect: the group which is content that the gyrations of its symbols truly explain events without the necessity of observation and visualization as well as the group which believes that a description can be improved only by making it more detailed, each without understanding the processes in both quantitative and descriptive realistic terms.

There are many present evidences of relaxation; the rigorous symbolists content themselves with statistical approaches while the documentists also utilize these possibilities. Analogue computors, including models, are increasingly important. Yet there remains a great middle ground which contains a large number of problems in oceanography and engineering whereto there seems to be no adequate approach. These are problems wherein the processes, materials, and interactions are so complex and uncertain as to permit no rigor and too difficult of observation to permit adequate description; *yet quantitative answers are required.* It is not that engineer and oceanographer are reactionary in their approaches—on the contrary, they have employed the best discoverable attack on their many problems but have been stultified to a degree by those groups that have adhered strictly either to symbols or descriptions without searching the middle ground. I believe that here we have a large and neglected area wherein engineering and oceanography together can benefit.

Let me quote a simple example. For many years the refraction of ocean waves was well known. The engineer encountered wave damage to his structures from wave forces that produced failure or erosion. The general descriptive understanding of the refraction process was well known. The fisherman's principle of "the points draw the waves" was adequate for many needs but it was inadequate for the location and design of structures on a headland. At the same time, rigorous expressions for wave refraction existed so that the "distribution of wave energy on a ridge with parabolic contours and exponential contour spacing" could be handled. Between these two extremes were the real world conditions where real waves peaked up and damaged installations on a point that looked like no point on a parabolic ridge yet devised. The simple solution to this problem resulted from the rational use of all that was known of wave refraction. From the description of offshore waves, laborious step by step construction was started, each step being determined by the quantitative laws and modified by the descriptive understanding. Realistic waves were brought in over realistic bottom topography to a realistic shoreline by a process in which a draftsman was taught to move a pencil as a wave moves. Many vexing questions were resolved in this process; for example, the effect of small bottom features was no longer open to argument since it could now be visualized. I am happy to note that this solution in no way froze the attack on wave refraction.

Aids which facilitated construction were devised for the draftsman yet room was left for the exercise of judgment. The documentists reported more observations on wave refraction and the symbolists set about explaining these with excellent results and with an advance of understanding.

This is a simple example. Surely better ones could be quoted. Yet, simple as it is, I believe the implications are important. I am impressed by the dichotomy of approach in many important cases. Let us go beyond the last example to the virtually unsolved problem of the impulsive loading of complex structures. I believe it probable that such impulsive loading of ships, buildings, automobiles and human bodies is rapidly becoming man's most import environmental condition.

Let us say a pier is destroyed by waves, or a ship by explosions. When a pier is destroyed, the engineer determines the probable height of the waves, describes the nature of the original structure and the nature of the damage, perhaps makes some static calculations on the strength of the installation, and proposes an increase in the factor of safety of some of its members from 3 to 5. On the other hand, the symbolist writes the equation for the impulse of a sinusoidal or trochoidal wave, approximates the characteristics of the pier as a cantilever, and then permits the two to interact according to certain rules governing harmonic motion. It is little wonder that opinions differ so widely. Actually the process is far too complex to be handled by either alone, whereas together the workers are capable of describing the events step by step, not losing sight of the realism of particles in either pier or water or of the drag on attached fouling organisms in contemplation of gyrating symbols; thus a visualization, tethered by quantitative calculations, could be carried through at the most fundamental level of process. What are the forces? What do the particles do? No doubt, in the case of the pier or ship, the symbolic approach could be enhanced by drawing from seismology and expressing transient waves (flexural, shear or torsional) which are generated and which move through the body in some idealized way. For these early purposes are these not artificialities? Is it not sufficient to know the forces and particle restraints and to trace through the process as laboriously as necessary in order to create a breakthrough in understanding the complex processes for future realistic, understanding methods and generalizations, as we have seen in the simple case of wave refraction?

To express this more clearly it would be necessary for me to quote more examples. I believe such an attack is applicable in oceanographic problems of mixing, evaporation, wind drag, productivity, sedimentation; in fisheries problems of population dynamics and availability; in naval problems of mine countermeasures and explosion damage; and in a host of other problems now being attacked from two aspects to the neglect of the middle ground.

I am sure that many persons will believe this to be a small point. Many are quite sure that present analytical or descriptive approaches are adequate. I will say that the approaches are slowly becoming so. Careful, thoughtful observations and rational "real world" approaches must sculp idealization. The consideration of fronts, ripples, bursting bubbles, white caps, surface slicks and spray as real conditions of the ocean surface will reveal the manner in which fluxes of matter and energy occur through the ocean-atmosphere interface in a way that never could be realized by the strict analytical approach. The scientists who have initiated this realistic work are to be congratulated.

To emphasize the degree to which our thinking is often circumscribed by the fictions that we have created, I would like to offer the challenge of two deceptively simple problems that I have been perpetrating on friends lately. One is for the oceanographer. How does a group of water particles at a depth in the ocean accumulate the pressure commensurate with that depth? And one for the engineers. How does an immersed vertical oceanographic cable reflect at the surface the total buoyant effect of the water over its length?

I pose these for fun. However, sometime, if you have the opportunity, consider them, not with vectors or free body conventions but with a realistic rational picture of how the water is aware of that lying above and how the cable can accumulate its information at the surface. You will be amazed at how meaningless become the fictions and smooth conventions with which we often limit the veracity of our thinking.

It is, of course, presumptious of me to discuss this matter so incompletely in the presence of so many whose understanding far exceeds mine. However, I believe I owe no apologies for the things for which I sue:

First, an unreserved recognition of the common brotherhood of investigation into truth, that there is no stigma of the "applied," indeed, that there is no defensible difference between the "applied" and the "pure"—in my opinion, any stigmata attach to "pure" science for having in a measure failed of its promise.

Second, the recognition that "free" untrammeled research is the scientist's responsibility rather than his license and that such research is demanded by the necessity to understand the complex weave of the threads of truth, which admits no extensive preconception of what is or is not important in an inquiry.

Third, that no relation needs to exist between the degree of application and freedom; but that serious lack of freedom always obtains when "directing" insists on a narrow attack or proceeds without understanding.

Fourth, a recognition of a great common ground of real world *problems amenable to unreserved rational attack by those of all groups who possess part of the truth and who themselves are morally responsible for their solution.*

Fifth, a free exchange of methods; tolerance and understanding of different approaches when brought to focus on an important problem.

In conclusion, I realize that this new structure and this community tell the tale better than I. For here exemplified is a community, a common brotherhood of scientists and engineers, bringing their energies to bear on the "somethings," many man-made, that we must understand and control for the security of mankind.