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Scientific Innovation and Creativity: A Zoologist's Point of View[1]

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INTRODUCTION

To undertake to lecture on innovation and creativity to an audience of research scientists requires that one be ignorant, or conceited, or foolhardy, or senile, or some combination thereof. I have given you my credentials, but before I deliver my message I feel constrained to engage in a little appeasement behavior. I am not so completely lacking in perspective that I would undertake to tell any working scientist how to increase his or her creativity. However, most scientists are also teachers of future scientists and these young people need all the help they can get. Present day biology is just too complex a field of endeavor to be entered without some overview of the operation as a whole. During the 40 years that I have been publishing scientific papers I have developed my own overview, some aspects of which I shall share with you this evening. It is my hope that some of the elements of my point of view which, is highly eclectic and probably not very original, may directly or indirectly enhance the creativity of biologists now embarking on their professional careers.

THE MAGNITUDE OF CONTEMPORARY SCIENCE

Scientists usually feel insecure when they talk about the nature of science and I am no exception. Territorial rights to this topic have long since been established by philosophers and historians and they have produced a large and complex literature. It has been said, with at least some justification, that science is too complicated to be analyzed by scientists. However, the practice of science is an art and the point of view of the research scientist should afford some understanding of the operational nature of science. This is particularly applicable to biology which is so complex that an orderly treatment is not only difficult but probably misleading.

Contemporary science is a highly organized social enterprise, but as a generally recognized and accepted profession, science has existed for only about 150 years. Its growth, particularly during the present century, has been one of the most remarkable episodes in the intellectual history of mankind. Its present magnitude strains the credulity even of persons such as ourselves who participate in it.

There are more scientists at work today than the total from all of history prior to World War II. Doty and Zinberg (1972) writing at a time which now appears to have been near the high point in the social support of science in the United States said that half the total of all the world's scientific literature had been published in the previous 12 years, and that half of the total of all the Ph.D.'s ever granted in the United States had been awarded in the preceding 9 years. In 1970 in the United States approximately 11,000 Ph.D.'s were granted in mathematics and the natural sciences. Since then the rate of production has fallen off slightly—in 1980 the comparable figure was 9,000, but in the context of my discussion the critical issue is not the rate of increase or decrease in the size of the scientific enterprise, but its magnitude as an operation, and its continuity of growth.

It is generally accepted that the doubling time for the volume of scientific information is about a decade. This condition, or something approaching it, will presumably continue for many years. Let us reduce this torrent of data to more operational terms. David E. Davis, President of the Board of Trustees of Biological Abstracts, tells me that at the present time approxi-

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mately 400,000 non-clinical biological papers are published annually. How many scientific papers can one person look at, let alone read, in a year? Certainly only a few hundred. Consider the plight of a scientist of my age. I graduated from the University of California at Berkeley in 1940. In the 41 years since then the amount of biological information has increased 16 fold; during these 4 decades my capacity to absorb new information has declined at an accelerating rate and now is at least 50% less than when I was a graduate student. If one defines ignorance as the ratio of what is available to be known to what is known, there seems no alternative to the conclusion that my ignorance is at least 25 times as extensive as it was when I got my bachelor's degree. Although I am sure that my unfortunate condition comes as no surprise to my students and younger colleagues, I personally find it somewhat depressing. My depression is tempered, however, by the fact that all biologists, young or old, developing or senescing, face the same melancholy situation because of an interlocking set of circumstances.

The data base of biology will continue to increase as long as science remains a central part of our social system. The physical universe is infinite. The human mind is finite. Life is short. The span of a scientific career is even shorter, and the span of one's scientific creativity during that career is apt to be still more brief and more fleeting.

Youthfulness and Creativity

Creativity often appears to be some complex function of play, and play is a characteristic of young animals, particularly young mammals. In any event, creativity is a natural characteristic of youth. In some ways it is related to the exuberant behavior of young animals. The most profoundly creative humans of course never lose this exuberant creativity. However, even in these people creativity starts when they are young. But unlike most of us, they stay young in mind. Creativity is something they retain, not something they develop in their middle or late years.

Unfortunately one cannot enhance the creativity of a biologist merely by encouraging him to start doing research when he is young. The magnitude and complexity of contemporary biological research allows few people to launch independent careers before they are in their late twenties, and when they do, the overwhelming flood of publications is apt to obscure their initial scientific papers. This situation leads to an obvious conclusion. If one wishes to contribute in any substantial manner to the scientific understanding of the natural world, one should develop at an early point in one's career clear and exploitable ideas about the nature of the sector of science one wishes to enter. This will help to focus one's finite and fleeting intellectual resources on research that will be productive and scientifically meaningful. In brief, a young scientist should, while still in graduate school, or at latest, during his immediate post-doctoral years, develop an effective scientific orientation—an orientation that most of us never acquire, or acquire too late in life to affect our performance as scientists.

Some Comparisons Between Physical and Biological Science

An essential early step in the acquisition of an effective professional orientation is to develop criteria which will help one in evaluating the quality of research. To do so it is helpful if one can identify some of the important constraints that affect all science—good, routine, or poor. To identify these constraints, it is useful to take a historical view.

Despite its revolutionary and tradition-shattering social consequences science is one of the most deliberately and self consciously historical of all organized human activities. The corpus of science is the published scientific literature, a stylized body of observation, interpretation, and inference. Every research scientist stands on the shoulders of his predecessors, but of course these predecessors are not necessarily members of a preceding biological generation. In fact the effective scientific generation is a social rather than biological unit. Every social generation of researchers in a given area of science has its own starting point and its own preoccupations. These starting points and preoccupations are not measured on a scale closely linked to the human life span or even the length of a professional career, but depend on the rate of development of the field involved. Intellectual aging occurs at different rates in different fields. Obsolescence is a problem everywhere in biology, it just occurs more rapidly in some fields than in others. Ironically one's scientific obsolescence is a direct result of the creativity of his peers.

At the risk of getting ahead of my story, one precept for the scientist-to-be is already obvious. Do not place yourself in an environment where your advisor is already suffering from scientific obsolescence. If one is so unfortunate as to receive his training under a person who is either technically or intellectually obsolescent, one finds himself to be a loser before he starts. It is difficult to move into a position of leadership if one's launching platform is a scientific generation whose time is already past. To avoid such an impasse a historical perspective again can be helpful.

Science is constrained by measurable physical interactions, and rates; in the final analysis it is empirical not logical. It does not have access to ultimate truth, only increasingly realistic approximations and descriptions. Ultimate causality is beyond its purview. It must ask only questions that are answerable, or potentially answerable, by measurements and observations made in the physical world. All of this laboring of the obvious is to allow me to make the following point: Logic alone is an inadequate basis for biological research. A logically derivable conclusion is just a logically derivable conclusion, nothing more. Logic and reasoning alone, and all the rules developed for them, cannot stand as science without empirical testing or the establishment of detailed consonance with a body of systematically organized observations. A sure path to scientific obsolescence for a biologist is to wed himself exclusively to deductive interpretation, or for that matter any single formal pattern of intellectual operation. I single out the problem of excessive dependence on the hypothetico-deductive method because of its current faddishness and because I think it is inappropriate to serve as the primary tool in many areas of biology.

I think that we biologists have been philosophically misled by the historical success of the physical sciences. Classical physics and chemistry are simpler and more regular than biology. Their relative simplicity and regularity linked them closely with mathematics. Mathematics, at least in its formal presentation, is totally deductive. It starts with axioms, makes deductions therefrom and creates an internally consistent and logically coherent structure that is almost independent of the external world.

The philosophers of science have usually been students or observers of the physical sciences. They have based their interpretations almost exclusively on physics and chemistry, which because of their orderliness, allow extensive and effective dependence on logic and particularly deductive reasoning. Biology is a part of science. Ergo the same logical procedures which have worked so well with classical physics should work with biology. This pattern of spurious logic has made biologists captives, usually willing captives, of paradigms based on the physical sciences. Unfortunately these paradigms are applicable only to limited segments of biology and often have little relevance to biological research, particularly on the more complex levels of biological integration.

Heavy dependence on direct observation is essential in biology not only because of the complexity of biological phenomena, but because of the intervention of natural selection with its criterion of adequacy rather than perfection. In a system shaped by natural selection it is inevitable that logic will lose its way. I shall return to this topic later in more detail.

Science is a product of interactions between individuals, or groups of individuals through both space and time. It is a product of the central nervous system and the sensory inputs of a single species, *Homo sapiens.* This presents us with a question that is probably unanswerable. Can the major patterns of a system be understood

TABLE 1. *Some levels of integration in biology.*

molecules
organelles
cells
organs
organ systems
organisms (cellular and multicellular)
populations
communities
ecosystems

by one of its parts? How valid is the view from with inside? The validity of the view of the system from inside acquires particular operational importance to biologists because of our diverse relationships to the many different integrative levels which exist in living systems (Table 1). It is a familiar fact that different biological disciplines deal with different levels of integration. This presents few problems if one is at one end or the other of the series. A molecular biologist is apt to deal only with the first two categories in Table 1. The systems ecologist is similarly restricted to the last two or three. Unfortunately for most of the rest of us, we are somewhere in the middle and deal with systems which, to be adequately understood, should be examined at many levels of integration.

This situation leads almost inescapably to the necessity for defining what we mean by a scientific explanation. Without doing great violence to anyone's sensibilities we can say that a scientific explanation describes the events observed at one level of integration in terms of the events accompanying it at another level of integration, usually one step downward in the hierarchy of complexity. Another, and probably more familiar way to say this, is to suggest that when one describes a phenomenon in terms of the events at a lower level of integration one is using a reductionist approach and is dealing with mechanism. When one describes a phenomenon in terms of events accompanying it at a higher level of integration he is attempting to evaluate its significance. We are of course now looking at a familiar dichotomy—reduction *versus* synthesis.

GOOD SCIENCE *VERSUS* ROUTINE SCIENCE

To ask what qualities distinguish good from routine scientific research is to address a question that should be of central concern to every scientist. We can make the question more tractable by rephrasing it, "What attributes are shared by the scientific works which have contributed importantly to our understanding of the physical world—in this case the world of living things?" Two of the most widely accepted characteristics of good scientific work are generality of application and originality of conception. One may wish to assign additional characteristics of excellence, but it is difficult to quarrel with the pair that I have just proposed. No one could hesitate to say that a new and innovative perception that was applicable to an extensive series of biological phenomena represented good science.

These qualities are easy to point out in the works of others and, of course extremely difficult to achieve in one's own research. At first hearing novelty and generality appear to be mutually exclusive, but they really are not. They just have different frames of reference. Novelty has a human frame of reference; generality has biological frame of reference. Consider, for example, Darwinian Natural Selection. It offers a mechanism so widely applicable as to be almost coexistent with reproduction, so universal as to be almost axiomatic, and so innovative that it shook, and continues to shake, man's perception of causality.

I shall discuss "generality of application" first because I have little to say that is not either familiar or obvious. All scientists must focus closely on limited targets. Whether or not one's findings on a limited subject will have wide applicability depends to some extent on chance, but biologists of superior ability repeatedly focus on questions the answers to which either have wide ramifications or lead to new areas of investigation. One procedure that can be effective is to attempt both reduction and synthesis; that is, direct a question at a phenomenon on one integrative level, identify its mechanism at a simpler level, then extrapolate its consequences to a more complex level of integration. A similar effect can be obtained if one looks at his findings as an analog of a relationship that has been identified at some other level of integration and then extrapolates his findings to either simpler systems or to more complex systems. This coupling of integrative levels will be facilitated if one can formulate his findings in their most general form—as an equation or a model. I shall labor this point no further because I wish to devote the rest of my discussion to scientific creativity.

INNOVATION AND CREATIVITY

When one addresses the problem of scientific innovation he addresses one of the most elusive and baffling of all intellectual activities, namely *creativity*. In an ultimate sense it is not justifiable to separate scientific creativity, from artisitc creativity, or literary creativity. However, I am hopelessly unequipped to address such a broad topic and shall confine my remarks to an area in which I have some competence, biological research.

In the context of biological research one can reasonably identify creativity with the capacity (1) to ask new and incisive questions, (2) to form new hypotheses, (3) to examine old questions in new ways or with new techniques, and (4) to perceive previously unnoticed relationships. I shall subsume all of these aspects of biological creativity under a single rubric "originality of conception."

The capacity for originality of conception is profoundly subject to chance events. It is certainly influenced by genetics as well as by uncontrolled environmental and social factors. Obviously, there is no simple formula that can assure its attainment. However, I think there are ways by which the creativity of biologists such as ourselves can be enhanced and by which the likelihood of our developing original concepts or new insights can be increased. Unfortunately, I cannot assure you that the overt inclusion of these relatively unorthodox methods in a research proposal will enhance the chance of its being funded (see for example, Muller, 1980; Leopold, 1981).

First of all, one should remind oneself that the formal pattern in which scientific results appear in the literature rarely reflects the way in which they were obtained. Rather it represents a format of presentation by which the result of one's creative efforts can be communicated to other scientists in a socially acceptable and psychologically convincing manner. Currently it is fashionable (and effective) to present one's work in the hypothetico-deductive format, although not all of us are true believers (see for example, Willson, 1981). In a previous generation the inductive format was popular. "Darwin consistently portrayed himself as adhering to the accepted scientific pieties of his day, namely the Baconian Method, in which one first marshals the facts and then sees what conclusions emerge" (May, 1981). In fact Darwin's notebooks reveal that he, like most other biologists, worked in a conspicuously nonlinear melange of intuition, deduction, theorizing, observation, conversation, reading, induction, guessing, and chance. Although the product of scientific effort appears orderly, the process by which it is achieved is usually not. Fortunately, a scientist's worth is judged on the basis of his accomplishments, not the tidiness of his work habits.

The biologist always faces inescapable complications because of two characteristics of the nature of the living system he studies; first, it is staggeringly complex, and second, it has an evolutionary history, which means that its present configuration has been shaped by natural selection. The process of natural selection is, of course, comprehensible in a general sense, but it is underlaid by genetic mutations which are totally unpredictable as individual molecular events. In dealing with a system which has been subject to natural selection, dependence on logical formalism, or on the methods of classical physics are patently unrewarding. What is needed is a mode of operation tuned, not to the format in which scientific findings are communicated, but to the structure and fabric of the biological systems being studied.

The structure and fabric involves as a minimum, (1) many levels of integrative complexity, (2) elements initially triggered by individually unpredictable genetic events the results of which are (3) greatly amplified by the phenotypic consequences of development, then (4) enormously multiplied by replication, and (5) finally assembled into some adaptive system through the process of natural selection operating over many generations.

At even the simplest levels of organization naturally occurring biological entities consist of many interlocking elements each of which shares most or all of the characteristics described above. Such entities are at least in theory comprehensible in terms of physics and chemistry but their configurations, functions, and interactions are not very amenable to detailed prediction. The difficulty of making predictions of course increases with the complexity of the integrative level being studied.

All of us biologists deal, either directly, or indirectly with organisms. The system we study is comprehensible, but conforms only to the broadest of predictions and conforms hardly at all to specific detailed predictions. I suggest that the biologist is best advised not to seek general answers to specific questions as the physical scientists aspire to, but to reverse the process, and seek many different special answers to each general question.

Many kinds of organisms face similar problems, but the solutions that they have evolved are by no means uniform. Obviously we biologists should fit our methods to our materials. An interesting response to this challenge has been employed particularly by persons who have entered biology from the physical sciences or who are distressed by the variability in biology; they focus their research on inbred strains of genetically homogeneous laboratory animals from which, to the maximum extent possible, variability has been eliminated (see for example, Festing, 1979). These biologists have changed the nature of the biological system to fit their methods. Such a bold and forthright solution is admirable, but it is not for me. Before I became a professional biologist, I was a boy naturalist, and I prefer a contrasting approach; to change the method to fit the system. This approach requires that one employ procedures which allow direct scientific utilization of the successful long-term evolutionary experiments which are documented by the fascinating diversity and variability of the species of animals which occupy the earth. This is easy to say and hard to do.

SPECIALIZATION AND INNOVATION

Unfortunately for the approach to biological research which appeals to me, most of the disciplines of biology are organized by tradition and techniques into specialities appropriate for reductionist analysis, but inappropriate for synthesis—*i.e.*, for projecting one's findings to more complex levels of integration. Consequently we biologists frequently ignore some of the most essential attributes of the systems we are trying to study, because these systems are necessarily either some component of an organism or an organism itself.

I do not hesitate to assert that the organism is the central unit in the integrative hierarchy of biology. Organisms are so complex that there can be no single biological discipline with which they are coterminous.

Because of the finite nature of our minds we have subdivided biology into disciplines and specialties. These disciplines are artifacts of convenience and history. They give psychological support to us by setting limits on what we are supposed to know and limits to what we have to think about. They give us identity when we are young, and honors when we are old. They become bureaucratically entrenched and often achieve a depressing kind of institutionalized immortality, and they also obscure our view of biology.

Organisms are not functionally divisible even into such broad categories as behavior, physiology, anatomy, and genetics, let alone the more restricted areas of specialization with which most of us identify. At this stage in the history of biology it is neither practical nor profitable to espouse a philosophy of holism. Nevertheless, the magnitude of the mismatch between the social structure and traditions of biology, as reflected in its various disciplines, and the nature of biological systems can be minimized without adopting a spurious holism. One can abandon the comforts of expertise and deliberately work in the areas between established fields of specialization, or even work at the interface of several traditional disciplines.

Expertise is creatively stifling. It is only a slight exaggeration to say that a biologist who aspires to be creative or innovative is probably wasting his time if he knows precisely what he is doing, or if he is intellectually comfortable at his work.

In addition to working on the border (read "cutting edge") of a discipline, and working between disciplines (read "pioneering"), it is a good thing not to be too single-minded or persistent. Let the nature of your results change the direction of your investigation. After all, if you are sure of the answer before you start, your research is hardly worth doing. To accept redirection of your research requires that it not be too closely wedded to formal hypotheses, or to the lovely protocols which you used to entice money from a granting agency. It also requires continuous data reduction and a very tight link between the questioner and the data gatherer. A tight feed-back loop is assured if one asks one's own questions and also gathers one's own data. A lack of such rapid feed-back is almost assured if the planning is done by a research administrator and the data are gathered by technicians.

The flexible mode of operation which I propose allows one to take advantage of the nature of biological systems. I shall emphasize the organismic level of integration because that is the one with which I am most conversant. Organisms are not functionally divisable into the established fields of specialization with which most biologists identify. The organism is the real unit, one's specialty is an artifact of convenience. If one focuses on the organism and ignores disciplinary boundaries, one often finds that an organism offers favorable opportunities for certain kinds of questions and not for others. It may be refractory to answering your initial question or to the application of conventional techniques, but there are apt to be certain things it wants to tell you. If so, you can oblige it (and yourself) by changing your question. If you are bold enough to ask the question the system you are studying wants to answer, you can exploit the research opportunity more effectively than if you remained locked to your original question. You may even be scientifically creative, because you are tapping a key characteristic of the system you are studying rather than following a pattern set by the history and man-made conventions of some scientific discipline.

Biological disciplines tend to guide research into certain channels. Each special discipline has its own literature and its own jargon, which makes research workers reluctant to move freely from one field to another. One consequence is that disciplines are apt to become parochial, or at least to develop blind spots, for example, to treat some questions as "interesting" and to dismiss others as "uninteresting." As a consequence, readily accessible but unworked areas of genuine biological interest often lie in plain sight but untouched within one discipline while being heavily worked in another. For example, historically insect physiologists have paid relatively little attention to the behavioral and physiological control of body temperature and its energetic and ecological consequences. Whereas many students of the comparative physiology of terrestrial vertebrates have been virtually fixated on that topic. For the past 10 years, several of my students and I have exploited this situation by taking the standard questions and techniques from comparative vertebrate physiology and applying them to insects (see Heinrich, 1980). It is surprising that this pattern of innovation is not more deliberately employed. It is commonplace to find a biologist trained in one field and working in another. This represents a more demanding change than transferring questions and techniques between fields.

PUBLICATION, ANIMAL DIVERSITY, AND SERENDIPITY

Until its results have gone through the painful process of publication, preferably in a refereed journal of high standards, scientific research is just play. Publication is an indispensible part of science. "Publish or perish" is not an indictment of the system of academia; it is a partial prescription for creativity and innovation. Sustained and substantial publication favors creativity. Novelty of conception has a large component of unpredictability. The more research you complete, the more apt you are to make an original contribution. To be sure there are publications and publications. Consider the case of Oldfield Thomas, a prolific mammalian alpha taxonomist at the British Museum of Natural History during the first third of the 20th century. He published 1,000 titles—and then committed suicide. Or, the more familiar and less tragic example of the medical school professor who is an effective scientific entrepreneur, runs a big and expensive operation—and puts his name on every publication that comes out of his laboratory whether he knows what is in it or not. The issue is not number of titles but continuity of personal scientific effort. One is often a poor judge of the relative value of his own creative efforts. An artist's ranking of his own works is rarely the same as that of critics or of history. Most scientists have had similar experiences. One's supply of reprints for a pot-boiler is rapidly exhausted, while a major monograph that is one's pride and joy goes unnoticed. The strategy of choice is to increase the odds favoring creativity by being productive.

There is another and even more important reason for publishing. The more deeply, continuously, and productively one is enmeshed in research, including the final and compelling discipline of publication, the greater the opportunity for favorable serendipity. The faculty of making a fortunate discovery by accident is not confined to the three princes of Serendip recounted in Hugh Walpole's fairy tale over two centuries ago. Serendipity has always been a key element of scientific innovation, particularly in biology.

The chances for favorable serendipity are increased if one studies an animal that is not one of the common laboratory species. Atypical animals, or preparations, force one to use non-standard approaches and non-standard techniques, and even to think non-standard ideas. My own preference is to seek out species which show some extreme of adaptation. Such organisms often force one to abandon standard methods and standard points of view. Almost inevitably they lead one to ask new questions, and most importantly in trying to comprehend their special and often unusual adaptations one often serendipitously stumbles upon new insights.

Because of the usually unpredictable (and often chaotic) nature of biological dynamics, general theories are apt to be sterile. However, special theories focused on restricted questions are another matter. Indeed special and limited theories are an essential guide for biological research. They grow from observation and lead to more and different observations. This situation is highly favorable for creativity in that it produces a tight feedback loop between question and answer, and in addition, *ad hoc,* undogmatic theory can be a key element in promoting favorable serendipity (Oster, 1981).

A Prescription for Enhanced Creativity

Bernard Cohen, a perceptive historian of science, has said that a fundamental characteristic of scientific breakthroughs, from the simplest innovations to the most sweeping intellectual revolutions, is that they create something new by the transformation of already existing ideas and information. Such a process of scientific creativity is strikingly analogous to the evolution of adaptations through natural selection. A biologist can do worse than exploit the latter to achieve the former. His chances of making innovative contributions will be enhanced by avoiding the constraints of a single scientific discipline or technique, by transferring the ideas and questions of one discipline into a different one, by allowing his research materials to guide the pattern of his research even to the extent of determining the questions he asks, by facilitating serendipity through studying non-standard organisms and extremes of adaptation, by assuring a tight feed-back between data and limited theory, and by publishing steadily.

This prescription is not easy to follow. It places severe demands on one's ego strength and it strains one's self confidence. It forces one to abandon the security of expertise and the associated support and esteem of the fraternity of fellow specialists. It forces one to learn new techniques and to evaluate unfamiliar ideas. It configures one's research into a pattern where one cannot depend on the beauty and symmetry of one's research proposals to get funds. One may be forced to depend on the documentation supplied by the quality of the results of one's previous research. For a scientist starting out on his career the practical problems presented by this are obvious.

I have previously referred to the linkage between creativity and youth. Happily, all of us—young, middle-aged, and old—can take advantage of this linkage because the social structure of basic science is tightly linked to higher education, and in universities there is always a preponderance of young people.

George Bernard Shaw's familiar dictum that youth is too valuable a thing to be wasted on young people, becomes more appealing as one grows older. Fortunately, at their best, universities allow persons of all ages to profit by the youth of the students. Young scientists are supported by the creative exuberance of their peers. Middle-aged scientists are intellectually stimulated by their students, and old scientists, if they are lucky enough to attract good, strong-minded graduate students and post-doctoral fellows, can enjoy a lifetime of vicarious intellectual youthfulness. The moral is obvious. Whatever your age, to associate yourself with a first rate and broadly based program of doctoral and post-doctoral activities is the best assurance for enjoying a productive career in biological research.

The spectacular and revolutionary findings of molecular biology led to the establishment of a remarkable series of general relationships that apply widely to cellular organization and function. It is commonly assumed to be unlikely that additional intellectual revolutions of similar scope will affect our understanding of cells in the near future. The staggering complexity of the higher levels of integration make it even more unlikely that new general theories applicable to them will soon appear. However, one of the central features of the biological system is its diversity. "We can be confident that the millions of species, and their innumerable differentiated cells, will furnish a virtually endless frontier" (Davis, 1980). This has been the matrix within which the naturalist, the systematist, the ecologist and the comparative physiologist have traditionally operated. It is available to all biologists. New instrumentation and new understandings make, and will continue to make, this diversity a resource that cannot be exhausted. A future of ever increasing understanding of biology awaits our research.

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