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ABSTRACT

A panel discussion on "Biology in the next two decades" is reported. Panelists identified significant questions at various levels of biological inquiry (cellular, organ and tissue, organismal, population, and behavioral) and speculated about means for answering them. The necessity for relating information between levels was stressed. A distinction was made between questions significant for developing biological generalizations and those significant for the solution of social problems. Much concern was shown for population and environmental problems and discussants agreed that survival is likely to be the major question in the next twenty years. Two articles about journal literature relevant to biological education at the college level include an annotated bibliography of selected journals. An innovative multimedia program in general botany for non-science majors is also described. (EB)

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BIOLOGY IN THE NEXT TWO DECADES

Editor's Note: Because the education and training we provide now is not so much for the immediate but rather the distant future, it is judicious to occasionally take a long view, speculative as it may be. Such a perspective was taken in a panel discussion held in conjunction with the spring meeting of CUEBS Commissioners, *March* 29-30, 1969, in Washington. There follows an edited transcript of that discussion.

The panel was chaired by then CUEBS Chairman Henry Koffler, Chairman of the Department of Biological Sciences, Purdue University, and was composed of the following members of the Commission: C. Ritchie Bell, Professor of Botany, University of North Carolina; Garrett Hardin, Professor of Biology, University of California at Santa Barbara; Johns Hopkins III, Chairman, Department of Biology, The Washington University; and Edgar Zwilling, Professor of Biology, Brandeis University. The Commission Panelists were complemented by Stuart Altmann, Primate Laboratories, Emory University.

Part I: Panel Discussion

HENRY KOFFLER: The members of the panel have been asked to consider the following problems: 1. What are some of the most significant questions that within the next ten or twenty years are likely to be amenable to solution in your area of research? 2. What will be the means for answering them? I would like to paraphrase these questions from my own frame of reference. What do we mean by "significant"? Science is not just after facts, but after generalizations. Therefore, from an intellectual point of view, I judge significance by the breadth of the generalization that a given scientist has generated or to the development of which he has contributed. His contributions may have been based on the originality of his insights, but sometimes consist in the development of tools or techniques without which many complex scientific questions cannot be answered. What would the state of biology be without such instruments as microscopes and centrifuges, apparatus for electrophoresis, chromatography, spectrophotometry, computers, and so forth. Justifiably we respect those who provided these important tools for us. Of course, in social terms, science has additional parameters. Certainly the applicability of biology to the solution of pressing practical problems is another legitimate yardstick for measuring significance. I don't think society is willing to pay for our research only so that we can have fun. The satisfaction we derive from getting answers to as yet unanswered questions, to observe the marvelous beauty of the living world, or to obtain recognition from our peers, serve as incentives to make us work harder, but the public is more impressed by the usefulness of our discoveries.

In addition, personally biology has helped me in making moral decisions. Living matter has certain intrinsic attributes that result in inevitable consequences. Shouldn't these biological imperatives be included in our philosophical reflections. What is "good" for the organism? Of course value judgments will be made regardless of whether we contribute to them, but are we not in an enviable position to help establish guidelines to be considered when problems such as those dealing with individuality, interactions, maintenance of stability in the midst of change, and survival are approached? Perhaps, the significance of science also needs to be measured from this point of view.

About generalizations in biology. All of them deal with the nature of living matter—with what makes living things alive. The physicist deals with the total universe, but we deal only with a tiny segment although from an anthropocentric point of view the most significant part of this universe. *A priori*, can we ever generate generalizations that are as broad as a physicist's? I do not mean to detract from the intellectual achievement that many insights such as the theory of evolution represent but only raise a question regarding their inclusiveness. This leads me to another question: In addition to discovering concepts, theories, and laws in biobogy, is it likely that biologists will help catalyze new developments in other areas of inquiry?

Of course, you do not have to stick too closely to these questions, but they might set the stage for this discussion. We will start out with Johns Hopkins on biology at the subcellular and cellular level.

SUBCELLULAR-CELLULAR BIOLOGY

JOHNS HOPKINS: I think in order to get some view of where molecular biology may be going in the next couple of decades, it is instructive to look back over the last two decades at what has happened. In a general way the components which, I think, are present when most great scientific strides come, were there 20 years ago. First of all, there was a group of well-motivated, well-trained people. In the case of molecular biology they arrived on the scene in the late forties. They came from many fields: from physics and chemistry as well as from biology. Then there were great advances in technology: the introduction of radioactive isotopes, electronics, high-speed centrifuges, computers, chromatography, and x-ray diffraction methods. This technology was the second component. A third component was that of financial support. Twenty years ago Federal funding for bio-medicine was beginning on a large scale. Funds from the Atomic Energy Commission were available. Private foundations helped with research in cellular and molecular biology. A fourth component, the intellectual atmosphere, was right. Problems in biology were formulated in such a way that they were amenable to answer by this group of people with this technology, and this financial support behind them.

The last two decades have been the golden age of the nucleic acids and proteins and the golden age of ultrastructure. In the case of the nucleic acids, the structure of DNA and RNA are now fairly well known. There are details to be filled in but the general functions of nucleic acids are evident. The general idea of how proteins are synthesized has been worked out. We know a great deal about the structure of proteins, the primary structure, particularly, but also the three dimensional structure. In the case of ultrastructure, the introduction in the early fifties of the electron microscope and of techniques of thin sectioning, fixation and staining led to rapid strides in cytology with the description of a large number of new structures.

In the next twenty years there is the matter of filling in the details, doing more work along the lines that have been set out in the past, but I think one can be a little more specific about the way in which new endeavors are going to go, and the way some of this is going to be spelled out. First of all, I think there is going to be a lot of emphasis on nucleic acid research in cells of higher organisms and specifically: how is DNA arranged in the chromosome? how is the control of synthesis of DNA and RNA managed? the control of protein synthesis? The emphasis here is on control rather than on filling in more details of the scheme we now have. During the past two decades I think the Monod view that *E. coli* and elephants are the same has been shown very well. Perhaps the next twenty years will show that *E. coli* and elephants are somewhat different. One is going to do work on the chemical nature of repressors and the way in which repressors are bound to nucleic acids. One is going to find out a great deal more about the function and the chemistry of histones. More will be done along the line of Khorana's work in which the goal simply is to chemically synthesize the gene—a section of DNA—and to go through in a test tube the steps of synthesis which will result in a specific peptide. One will be able to tailor-make DNA segments as genes for specific amino acid sequences.

In the line of protein structure and function, I think there will be dramatic strides. One important thing has already happened: this is the ability to synthesize long peptides by solid phase methods, methods of Merrifield and the Merck group; this is beginning to pay off already. One will be able to synthesize proteins with enzyme activity and to study in great detail the fine points of the reaction mechanisms involved. It is going to be something that I think will pay off both in biology and in chemistry. One also will be able to change small parts of natural proteins in order to get different enzymatic activities in them. It is certainly going to become much easier to do primary structural analysis and also three-dimensional structural analysis of proteins because of improved technology of computers and x-ray instruments.

I think one of the most exciting aspects of the next twenty years is what is going to happen in lipid research. Lipid research in a way is just about where protein and nucleic acid research was thirty or forty years ago. One now knows very little about their chemistry. The traditional aqueous solution chemistry has not operated very well in elucidating lipid structure so that I think first of all we have to know more about the basic structure of lipids, then more about the biosynthesis of lipids and, finally, a great deal about lipid interactions—physical chemistry of lipid-lipid and lipid-protein interaction. This brings us up to the question of membrane structure which is going to be another exciting focal point for the next couple of decades. Now there are a number of conflicting views of membrane structure; I think there will be resolution of these conflicts soon. There will be a lot of work on the permeability of membranes. The view of the membrane that we have from electron microscopy will give way to a very dynamic view—a membrane that may be constantly changing, breaking and rejoining in various ways, a much more active structure than we now think of.

Cell biology is going to emphasize contacts between cells, communication between and among cells. There will be a lot of work on neurophysiology of single cells and of small groups of cells, cells grown in culture perhaps or cells from simple organisms. Insects are particularly amenable to this, and there will be study of chemical and electrical communications among their cells. I think there probably will be advances in cell culture which will make the present state of the art look rather primitive. Much more will be known about the nutritional requirements of cells in culture and one will be able to do better characterization of the genetics of cells in culture. Genetic tricks which have been useful in bacteria for some time will be applied to more complex cells.

Advances have been made in understanding membrane phenomena, cell movements, interaction among cells, and control of biosynthesis. This is leading molecular biologists into problems at other levels of organization from those they have been working at. What this really means is that a lot of the techniques, technology, instrumentation and personnel of molecular biology will be turning toward problems at the level of differentiation and of neurobiology, for instance, and also in the other direction towards problems of organic chemistry, physical biochemistry and such.

KOFFLER: In the logical continuum of this discussion, a consideration of differentiation comes next. Growth, development, and differentiation are extensions of gene activity, but clearly the environment, certainly the micro-environment, plays substantial roles. Of course, interplay among activities at several levels of biological organization is significant, such as the cell-cell interactions to which Dr. Hopkins has already referred. May I ask Dr. Zwilling to continue.

DIFFERENTIATION

Editor's Note: Owing to a press of time on his part and a disinclination to convert the oral statement to written form, Dr. Zwilling's remarks are not included. The substance of his comments were directed to the following points: 1) the increasing tie between molecular biology and the study of differentiation; 2) that population control may see widespread use of spermicides, with development of the latter involving knowledge of nucleic acids; 3) the possibility of examining fertilized ova to determine chromosomal configuration with a view to eliminating atypical carriers; 4) increased emphasis on the mechanisms involved in the expression of genomic constituents in organisms with especial attention on control and regulatory mechanisms; 5) that current studies on viruses will be carried over to eucaryotic organisms; 6) the role of cell interaction, and especially the effect of nerve cells on development of contiguous cells.

KOFFLER: By now we have put cells together to form tissues, and tissues to form organs. Organisms depend upon the structure, functioning, and integration of their organ systems. We shall now turn to Ritchie Bell for predictions regarding what organismal biology has in store for us.

ORGANISMAL BIOLOGY

C. RITCHIE BELL: For perhaps the last century or so, and especially the last few decades, there has been an unravelling of biology. We have gone from the level of the organism to the organ and on down to lower and lower levels, trying to find "The Truth." The truth has been rather elusive because we still don't have it. We possibly won't have it in twenty years. Unfortunately, the problems always seem to keep one a jump ahead of the solutions; the questions always outnumber the answers.

The most significant question that is likely to be amenable to solution in the area of organismal biology within the next ten to twenty years, I think, is the question of how the knowledge gained at the lower levels can be related to, and applied to, the entire organism. We have heard that the current view of the cell is a static cell, but that we soon hope to have a concept of a living cell. The next thing would be to consider again a living organ, and then an organism. And, of course, there are ecological levels beyond that to consider. I think that the big problem today, and one that is beginning to get some attention, is the knitting back together of the various parts of biology that, of necessity, we had to go in terms of specialization, super specialization and over specialization, to get some of the answers. Hopefully, we can now develop biological studies on a parallel basis so that as we attempt to refocus some of our interests and efforts and knowledge on the individual organism we do not lose workers and ideas from the highly developed fields of specialization below the level of the individual. It may be a new concept; that is, having both organismal biology and cellular biology going along parallel paths to their mutual benefit.

This raises another problem: the inter-education of biologists themselves. This would help with the previous problem of relating the information from the various discoveries at the lower levels to their appropriate roles at the upper levels, and would be an important step in education as well as in biology. Today someone finds an enzyme or a chemical or some means of controlling a particular reaction or solving a particular problem at a particular level in a particular situation. Then, unfortunately, because we have so much faith in our technology, and because we do desperately need answers in many areas of organismal biology, the jump is made to a higher level. We find out later that we jumped without a parachute. An example is the use of DDT and the unexpected chain reaction through fish to eagles and other wildlife which may bring about their extinction. I had a graduate student with a fairly serious physical disorder. He went to the hospital, was treated, and as a side effect got cataracts on both eyes. When this was pointed out to the physician, the reply was essentially, "Well, I was treating your other condition." The doctor didn't seem to worry about the total organism, namely, my graduate student. This, then, is the problem I was referring to—this lack of interrelationship between specialists working at a particular level and the specialists working at a higher level as regards the organism.

Perhaps the question is really the importance of living matter. If living matter is important, this would certainly imply that the organism is important. I don't know what DNA is selling for by the pound currently, but I do know you can buy it relatively cheap because I bought some for a demonstration. Is this price the value of DNA? No! What is the biological worth of isolated DNA? Nothing! The value of DNA, like the value of all the components of a biological system, is not the value that they have themselves: these components are fun to work with; they are fun to play with. But, their value is related to their importance in a living organism.

The really significant part of all the research that is going on today doesn't obtain in the test tube, nor does it obtain in the laboratory; it reaches significance, or "value", only when that particular bit of biological action hits the individual organism in its natural habitat or environment. That's where the problem is, and I think perhaps the public is going to help us with the motivation to solve it because they want application. I think that this is a fair request. Many of us enjoy our work because we *want* to do it; otherwise we would not put in 40, 50, 60, or 70 hours a week. So it is fun, but I think the people footing the bills have a right to request some application. The current "crunch" in the ecological field, where people are beginning to worry about the environment, is going to force a bit more application at many levels of research. If we get education between the biologists at different levels, and if we can then begin to help re-educate the public, at whatever levels seem appropriate, I think that the problem of thinking again in organismal terms can be solved.

I doubt if we will ever come to a situation in which we have every idea, every chemical, and every reaction thoroughly tested before a new product is released to the public. However, if we don't come closer to this ideal, and consider not only the whole organism but the organisms environment, we face disaster. A birth control formula that really gets rid of the spermatozan population might get wide use for a few years until it is discovered that the formula also causes degeneration of the liver or some other vital organ.

KOFFLER: Each of you has set the stage for the next speaker. We can't talk about organisms without wondering how they interact in groups and populations. Dr. Hardin has agreed to discuss this.

POPULATION BIOLOGY

GARRETT HARDIN: I would like to begin with some comments on remarks that Dr. Koffler made about the biologists moving into moral areas. I have myself been getting into such questions for a number of years. At first I did so with some diffidence; but presently, becoming shameless, I lost my diffidence and I now enter almost gleefully into moral questions. I do this because I've been thinking about the advice to let each man stick to his own specialty and not encroach on that of others. Who *is* the specialist in moral questions? As nearly as I can make out it is somebody who bears either the name of a philosopher or theologian. When I looked at the work the philosophers and theologians do on ethical questions I lost all diffidence about entering this area. It seems to me that the gravest shortcoming of these people is that they operate in a way that is essentially non-productive. When they tackle a moral problem, it is with an air that indicates they really cherish the insolubilities. The moment they even come within shouting distance of solving a problem, they immediately discover all sorts of weighty objections why this can't possibly be done and run the other way. I think this is a deeply engrained attitude of mind of theirs, that these things can't be solved, and they are jolly well going to prove to you that they can't be solved. I think it was C. P. Snow, who remarked that scientists "have the future in their bones." This is one of the striking differences between scientists and other people. A scientist or an engineer attacking a problem assumes as a matter of course that it has a solution. This is entirely different from the person who says, "Of course it is insoluble; so now let me anguish over it for 2 or 3 hours." You don't get anyplace that way. With that by way of preface, I am entering into fields where (if you wish) you can say a biologist has no business being; but I think he has as much business as anybody else.

With respect to population problems, let me skip entirely what one might call technical, biological population problems, the sort of thing that one does in a zoology course under the name of "population dynamics." There is going to be a lot of interesting work done here with animal populations and so on, but I shall ignore it completely and take up instead questions that are more closely related to my own interest in the human population problem. I think it is always worthwhile to try to do a little predicting, a little prophesying—not that you expect your prophesies to come true. You know they won't come true, but prophesying is a way of revealing your own prejudices and biases. After you've made your prophecy, write it out and file it, to be pulled out at the end of the period, and looked at for laughs. So here goes. I have taken the next twenty-five years and divided it into half decade steps. I have tried to identify the principal developments in each five-year period. After describing these, I want to mention some events that might well happen that would affect this timetable. Of course, there will be others that I didn't think of.

During the first stage, up to 1975, I would say the most important that will happen is this: there will be an intellectual acceptance of the idea that we live on a spaceship—that earth is a spaceship, and because of that we live in a finite environment for all practical purposes. Zero population growth is necessary. I think there will be an explicit intellectual rejection of the idea of the "commons." * Each person cannot take as much as he wants to out of the commons because this leads to complete destruction of the environment in which we live. In our nation, we will soon put a virtual stop to immigration. Once you accept the idea that

* Garrett Hardin, 1968. The tragedy of the commons. Science, 162: 1243-1248.

exploitation of the "commons" leads to ruin, then allowing immigration is seen as one way of arriving at such a ruin. We are a wealthy country. But if all the poorer people move in that isn't so much a matter of sharing the wealth as it is sharing the poverty.

I would also predict that in this five-year period, there would be a virtual stop to altruistic foreign aid. I put in the word "altruistic" because our foreign aid actually comes in two categories: altruistic and self-serving. (Of course, almost all the self-serving is disguised as ultruistic, but we know better, meaning by "we" the people in congressional committees.) The point is that altruistic foreign aid is the most dangerous of all foreign aid because it is a method of perpetuating the commons, that is, of sharing the poverty by sending our wealth out to the world. We might still send foreign aid to some countries, but it would be only because we regarded their survival or their prosperity as in our interest. Of course, there would probably still be a lot of double talk, but I would hope that at least we would arrive at the stage where we would get rid of altruistic foreign aid. All this I think will very likely happen by 1975.

During the next five-year period (up to 1980) I suggest that we may begin to adopt coercive measures for controlling population within the nation. I would include under this heading, taxing schemes and schemes of rewarding people who do not have children. I think we will accept coercive measures in steadily increasing numbers.

Specifically, I think we will change the direction of coercion exercised on welfare recipients. Many people raise this issue now. It is a terrible bug-a-boo. All we do is run away from it, but I think people finally will get tired of the evasion. The freedom to have children makes sense only if the person can take care of them himself. If, in fact, other people are taking care of the children then we cannot see any reason why the person should have freedom to have children.

By stage three—1981-1985—a fair beginning will have been made on the problem of the optimum. At present the problem of the optimum is one that economists have, for the most part, avoided entirely. Most economists are simply "growthmanship" economists. I would hope by the 1980's that the problem of the optimum would have been seriously tackled, and some limited consensus reached. This means, of course, that there has to be some consensus on "values." Shall we (for example) have more wilderness or more food for people? These are conflicting values and some sort of consensus has to be reached on the weighing of conflicting values.

Also there may be conscious embarking on a problem of negative growth. Earlier I said that the idea of zero population growth must become acceptable. You can implement the idea of zero population growth only if you accept the idea of the control of breeding. Once you accept the idea of control, it is no more difficult to have a negative population growth than it is one of zero, because both involve control. If you define the optimum and if, in fact, you find out that we have already passed the optimum (as I suspect we have), then society should begin on a program of negative growth.

Stage Four—1986 to 1990. There may be a recognition of the necessity of limiting the life span. I am presuming that there will be considerably more advance in medicine at the upper end of the life span. When you start on zero population growth, and even more so when you begin on negative population growth, you discover that for purely statistical reasons you have caused a great increase in the average age of the population. People will begin to ask, "Do we really want a population that is on the average 65 years of age or 70?" They will realize that this has political consequences that are probably undesirable. I think by this time they may be able to come to some agreement on what the limitations should be. They may elect not to make use of all the medical advances for lengthening life.

Also by this time, we may have reached some sort of agreement on qualitative discrimination of genetic quality at the lower level. Once you have control of population and restriction of breeding then this issue, that we usually try to avoid now, cannot much longer be avoided. People will get their noses out of joint when they discover that an appreciable number of really half-witted people have children. I think people can accept controls most easily at the lower limits. We can be talked into limiting reproduction at the very lowest levels; from here, control can creep upwards.

Finally the last stage, 1991-1995. There will be a real attack on the question, "Who shall judge?" This has always been a difficult question in eugenics. It is a fantastically difficult question; but, like all other questions, it is, I think, capable of being solved. With zero or negative population growth we will have a strong motivation to solve it. If at this stage we can really decide on some sort of a mechanism that leads to acceptable judgments then we will be in a position to consider what in old-fashioned times was called "positive eugenics." We might actually like to encourage the breeding of some groups, instead of merely discouraging that of others.

These are the five stages for which I am willing to stick my neck out because it will be interesting to see how far wrong I am. Let me now take up the second sort of question, and that very briefly is: What events might possibly happen that would have a significant effect on the whole timetable? What I have in mind here is the sort of thing that happened with Sputnik. Historical events do have striking effects at times in starting things moving, whether certain things happen or not. They at least alter the time schedule greatly.

Here are possible events that I think would have significant effects on our activity and our actions in the field of population.

First of all, and this is practically with us now, there is the issue of "genocide." It may soon become an explosive political issue. It comes up particularly with respect to the black-white problem. A number of statements by blacks have raised the issue of genocide in connection with birth control programs. Others have denied that this is an issue. There is sort of a sexual difference here: by and large it is black men that raise the issue of genocide, and black women (who have to have the children) don't raise the issue. But the issue will probably become more prominent, and may even become explosive. This is going to hasten thinking along this line tremendously. I think there will be a sort of double effect. The initial effect will be to decrease the extent to which we are willing to control populations; but I think there will be a secondary effect when we realize that we have to face the matter openly, that populations must be decreased.

The second event that I think will have a marked effect on the time schedule is massive world famine at some place or places in the world. A tremendous famine of the sort that the Paddock Brothers* envisage; say, a year in which 50 million people die. This will have a tremendous effect on thinking everywhere. The immediate effect of this, I think, will be to decrease birth rates in the more conscious countries where conscious control is more easily possible.

There is a third possible event; this is speculative, but I think it should be kept in mind. If the evidence of human spoilage as a result of malnutrition becomes more massive than it is at the present time, this knowledge may have a great effect on our view of population control. What I am thinking of is this business of Biafra. No doubt many of you in this room have given money for the Biafran children. Yet there is considerable evidence, first of all, from rat experiments which is good evidence and from human data which is more anecdotal and not so sound that if the brain is malnourished with respect to proteins in early stages, after re-instituting proteins in the diet the brain may never recover. There are, at least, strong reasons for suspecting that many of the Biafran children that we are saving by shipping proteins to may be past the point of no return. They may never recover intellectually. Well, if this sort of evidence develops to a larger extent, I think it will influence people's actions in population fields. (If it turns out not to be true, and let's hope it is not true, then it won't have any effect.)

* William and Paul Paddock, 1967. *Famine—1975!* Boston: Little, Brown & Co.

The fourth possible historical event is massive social disorder in the United States. This, I think, is possible and I think the general effect would be to decrease population growth. People living in times of great disorder hesitate to bring children into the world.

Fifth, if there were serious, but not devastating thermonuclear war. I cannot even spell out what sort of an accident I have in mind, but I am thinking of something where, say, in one localized place in the world, the beginnings of thermonuclear war takes place and then there is a drawing back so that it is not full scale, so that maybe not more than 50 million people are killed and another hundred million damaged from fallout. It is hard to predict exactly the effects of such an event. It might initially cause an increase in the birth rate, just through the sort of general moral shakeup that would occur. I think the eventual effect would be a decrease in population, a feeling that there was somehow some connection between population growth and the possibility of war.

Sixth, a massive pollution disaster of the various sorts that have been postulated. All would tend to decrease population.

Seventh, a spectacular disaster in the space program. This is, I think, quite possible. We may get men to the moon and then suddenly find we cannot bring them back. There might be only three men lost, and yet the emotional effect of this would be tremendous. If we keep up the space program, sooner or later we surely will have such an accident. It will emphasize that we live on "space-ship earth"; the general effect will be to decrease population growth.

And, finally, a welfare program scandal. We have had a little scandal already, but we may have more scandals of this sort. In general, such scandals would tend to decrease population probably by causing the adoption of repressive measures.

KOFFLER: Dr. Hardin has illustrated how many of our decisions are controlled by behavior. Principally, behavior is a device for assuring survival. Behavior is just as much a biological property as our anatomy, and is being actively studied, especially with animals. We hope that new knowledge in animal behavior will be useful in understanding also human behavior. During the last thirty years we have watched the impressive progress of molecular biology and cell biology. More recently the inevitable prospects of population biology. We should not forget that the study of animal behavior also has developed into a most promising area.

ANIMAL BEHAVIOR

STUART ALTMANN: I. One of the most significant things about the development of animal behavior is the fact that it is even represented here. If this meeting had taken place 10 or 15 years ago it is unlikely that animal behavior would have received representation as a significant area of biology. Looking to the future on the basis of the present rate of growth of research on animal behavior, the rate at which graduate students are being attracted to this field, and the rate at which the literature of this area is growing, it is clear that the study of animal behaviour is going to continue to grow rapidly.

II. More and more people are beginning to realize that field and laboratory work differ not so much in their logic as in their logistics. Different kinds of questions can be answered and different techniques are sometimes required, but the old idea that field studies can only provide questions whose answers depend upon analytic laboratory studies, is rapidly vanishing, and people are beginning to realize that each of these approaches has its own merits and its own ideas to contribute to the other. More and more, we are beginning to see a kind of mutualism or commensalism between those who are oriented toward field studies and those who are oriented towards laboratory work.

III. As for the distinction between descriptive and analytic studies, it is inevitable that descriptive studies are going to continue to make up the majority of what goes on in animal behavior for some time to come, simply because for virtually any species of animals you want to name, we just don't know the basic facts about how they behave and how the animals interact with their fellow beings. There is not a single species in the whole animal kingdom whose signaling code is now understood. On the other hand, we are accumulating a growing number of species or groups of species in which it is becoming possible to do analytic studies—to take the behavioral or social system apart, so to speak, and ask how it works.

Analytic studies usually start with systems which are, or are assumed to be, in equilibrium, but inevitably we are going to have to begin dealing with non-equilibrium systems—dynamic systems that are undergoing change. For example, research on dominance started with hierarchies that are stable, at least during the period of study, but more and more, we must ask questions about what happens to a social system when you get changes in status among the individuals in the group. We must look more carefully at the ontogenetic problem of how the individual's whole behavior changes as he becomes a member of the society.

These analytic studies will require techniques which now hardly exist. At present we really do not know how to describe adequately what an animal does. A lot of hard thinking is going on now on the basic problems of how you decide upon basic units of behavior, how you can obtain objective criteria for what is a social signal, and how you can get an adequate sampling of social interactions in a group of free-ranging animals, either in their natural habitat or in some simulated laboratory setup. These are extremely difficult problems, but within the next decade or two we are going to see some major headway made in this; we will then be in a much better position to study social systems.

John Kennedy once speculated that the social sciences are going to serve in the future as the stimulus to the development of new areas of mathematics in much the same manner as the physical sciences have done over the last few centuries. I suspect that he is right. Some kinds of problems in the study of social systems can be handled only by professional mathematicians. In our laboratory of primate social biology, there are three mathematicians and one biologist, and as that one biologist I can assure you it is a very rarified atmosphere in which to work. In work on animal societies, which by comparison with human social systems are relatively simple, we frequently encounter problems that are taking the skills of some very good mathematicians to solve.

I predict that, more and more, we are going to have analytic work done in this way; that is, by collaborations between biologists and mathematicians. Fortunately there are now a few people with adequate mathematical training who are willing to sit down with biologists and listen to them and find out from biologists what the animal's problem is.

IV. Now a few words about the boundary areas between animal behavior on the one hand and related fields on the other. I think there is going to be a lot more work done on the physiology of animals in ongoing, naturalistic situations We have the beginnings of this right now. For example, by using remote telerecording equipment, we can now obtain recordings of what happens to the heart rate of a baboon in the wilds of Africa when it is attacked by a leopard, as van Citters and his colleagues have done, and we can study the effects of hormones on the social behavior of totally free-ranging animals, as Saayman is now doing. I think that we are going to learn much more about the physiology of the animal when faced with its own problems, i.e. of the sort that the animal encounters in the wild.

V. In all likelihood, much more work will continue to be done on social behavior than on non-social behavior. The social processes are more interesting to many people, they are more challenging, and for many of us they have a certain inherent interest.

VI. Finally, from a taxonomic standpoint, there is every reason to believe that research on nonhuman primates will continue to grow more rapidly than work on any other group of animals. Research on the behavior and ecology of nonhuman primates is now doubling every five years, yet the doubling rate for science as a whole is roughly once every fifteen years. This is an incredible growth rate in one specialized area, and I suspect that it represents a far more rapid growth rate than now occurs in any similarly circumscribed area of ecology.

The reasons for this are obvious. The nonhuman primates are in a unique position in the animal kingdom—not just in their taxonomic relation to humans, but in terms of the research that is going on, in the following way. There has never been a group of animals that has been approached simultaneously by people from such diverse professions. There are people sitting under trees watching social groups of monkeys who have been trained in linguistics, psychology, sociology, anthropology, mathematics, or zoology. Of course, the initial result is chaotic. I can barely understand a structural linguist, and he in turn can barely communicate with a sociologist. But this multi-disciplinary approach to the behavior of a single taxonomic group of animals is producing conceptual advances that we have not had in any other group of animals. So that I suspect that this group of animals is going to continue to occupy a very central position in studies of animal behavior.

VII. If I summarize all of this, my inherent bias will become apparent. What I am saying is that if you want to know "where the action is going to be" in animal behavior, it is in collaborative, analytic field studies of free-ranging, nonhuman primates—and that is the kind of work that I do!

Part II: General Discussion

KOFFLER: Thank you very much. I think we can open this for discussion from the floor and also among the panel members themselves. Are there any questions from the floor?

ZWILLING: I wanted to make a couple of comments if I may, one with respect to some of the points that Bell made.

I agree that very frequently application is undertaken too quickly and without very good judgment involved. In most situations it is not the scientist that made the initial observation who is responsible for the injudicious application. I think this applies both to practical application as well as to scientific application. Many embryologists are guilty of not listening to Jacob in a too-ready translation of ideas to eukaryotic organisms. Many medical practioners don't look at the literature that the person who has developed the pharmaceutical product has put out. They look at some other literature put out possibly by a pharmaceutical house. I think this distinction has to be made, and something has to be done about some sort of control of application at that level rather than innovation because without the new developments we are not going to have anything to apply.

BELL: That is why I said that we will have to develop a parallel system so that we in biology can see that there is enough feed-back across that somehow we can enforce perhaps this crucial test.

ZWILLING: I realize the point you made. I am trying to exercise it because in other contexts, there is a tendency to say, "let's brake the innovative work that is being done in the laboratory."

I would like to make another remark, this about the presentation of Dr. Hardin. In pure recognition of the treason I am engaging in my own sub-discipline, I wonder whether it can be said that the most important aspect of biology that should be emphasized, and possibly emphasized more at the undergraduate level, is the problems with respect to population control, both biological and otherwise. In many ways if one goes to the extreme, one can make a case that advances in medicine and increased facility with food production are bad. Catastrophies in war which will diminish the population are good. Saving the environment is almost a fruitless exercise because with over-population you are not going to be able to appreciate it, use it, or be able to save it.

HOPKINS: I me dissent from this point of view. I am reminded of something that Irving Kristol wrote in the New York Times Magazine a couple of months ago (December 8, 1968) about sociology and students of sociology. He pointed out that students had passed through four years of sociology courses, their passions untouched by academic rigour. It seems to me that there is a certain danger in putting all of our emphasis, motivating emphasis, on such socially relevant problems as the kind Dr. Hardin touched on. At early levels of training there is much to be said for introducing discipline and rigour which then can be applied to these so-called relevant problems later on. I think one has to balance these two approaches.

KOFFLER: One point, Dr. Hardin, there is no question about resources being finite on the space ship. We could, of course, take the ultimate biological view and let the population follow the growth curve; eventually it flattens out.

HARDIN: It flattens out and so do we.

KOFFLER: Precisely, theoretically the system could be left to take care of itself. Living matter cannot continue growing beyond available resources. Either the sources for energy and building blocks become completely used up or waste products accumulate. Emotionally, it might be beneficial for the general public to visualize in the most extreme form what would happen if we allowed entropy to take over. Of course, because of the horrible consequences we will not permit such a situation to happen. Fortunately, there is one resource that I feel can be utilized much more effectively than it has—the originality and creativity of the human mind. The fact that we are not yet using all the available talent provides an element of hope that we often ignore. Many resources are finite, but they are reusable such as water and air. The compelling issue is to insist on purifying them so that they can be reused. We have not even begun to reach the limits of human ingenuity. We tend to think in terms of present day knowledge and do not fully take into account possible new discoveries that may change the face of this world just as the historical events that you referred to.

HARDIN: I would not disagree with that except that my own temperamental bias is to introduce the idea you suggested and then also introduce examples of where it has not worked out quite as well as we had hoped. This is what the ecologist is constantly doing. Each of these great new triumphs like DDT turns out to have a price. It is true you can do a great many things you did not think you could do, but then these so-called side effects limit you so that you should be at least dubious of these possibilities.

HURD:* Going back to a question that was raised about research and application, observations since about 1930 would indicate that basic research is gradually moving from the confines of the university to the research institute. The characteristic of research in a research institute, which is outside the context of the university, is that more attention is paid to application, in the sense of research and development, and not in application in the finest point. In the latter context, I am thinking particularly of work such as that of Lawrence and development of the cyclotron; the first day the cyclotron was in operation it was used in an experiment, the planning of which was made at the time the instrument was being developed and the idea was being conceived.

Already two-thirds of expenditures of money are outside of the university. If we hope to maintain the university as a center of the generation of knowledge then we have to pay some attention to not only its development of this knowledge in terms of some applications, but probably in the biological field in terms of some social applications. I would not quite hang the label "value" on it, but I would hang the label "appreciation" on it.

This must also ramify in the education of the non-biology major so that he can intellectually enjoy the game of what's going on and not have to derive it from *Readers Digest*, *Esquire*, and *Look*. As we look to the next twenty years, the way things seem to be going, much of our leisure time is going for an intellectual leisure. This intellectual leisure may bring back some of the joys I have read about in the Middle Ages, where one can have time just to read, enjoy discussions, enjoy debate, and be a part of this on an intellectual basis. This seems to me to be a form of bringing out many of the things that we discussed here this morning in a way that they can be taken to the educated citizen so that he can become a good sidewalk observer. At that point, I think we will have some of our research money problems taken care of.

FLINT:* I don't think that Altmann gave any leeway for future behavioral patterns that might develop due to changes in the environment itself. For example, we have learned that a great deal of attention is being given to the urban ghetto as an environment. Will future knowledge of behavioral patterns in man be amenable to some kind of solution through new kinds of cities, architecture, etc.? Do you have any prognostication on things of this sort?

ALTMANN: It is inevitable that to whatever extent we understand behavior we can modify it. Thus, the extent to which we can bring about beneficial changes in behavior by changing the environment will depend on the extent to which we understand the effects of environment on behavior. That's getting back to the whole question of just how rapidly we are going to progress. I don't think that in the next ten years we are going to have much to say to people working on, say the architecture of cities that is not already available from any number of other sources. Some pivotal social questions are involved but the answers to them are not going to come very quickly.

Let me illustrate with a seemingly mundane aspect of architecture and its relation to behavior. Some striking effects on the structure of family relations come about as a result of the degree to which the members of the family have communicative access to each other and have access to their neighbors. Architecturally, one might think of the import of the open versus the closed plan. The former tends to facilitate relations between individuals of the same families, while the latter hinders them. The kibbutz situation, which greatly facilitates social interactions not only between siblings, but between a whole group of age-class members, has dramatic effects on behavior that we are just beginning to understand. Until we do, we are not going to know what to tell an architect or city planner or highway engineer about how the things he does will affect behavior.

FINLEY:† I wonder if we are making the assumption that biology is going to advance and engineering, in the strictest sense, is going to remain static. For example, why would it not be possible to do some of the things we are beginning to do with population, and yet have a panel of engineers predicting what the environment on this spaceship will be like twenty years from now? Maybe they would come up with predictions that would say that we are going to see to it that your population doesn't overrun this planet.

* Paul DeHart Hurd, Professor of Education Stanford University.

* Franklin F. Flint, CUEBS Staff Biologist.

† Howard Finley, Chairman, Department of Biology, Howard University.

HARDIN: I suppose my general reaction is like that of Kingsley Davis at Berkeley.* The problem in population control lies, he says, not in the test tube nor in the laboratory, but in people's heads. We have quite adequate methods of birth control right now, but no program of "family planning" solves the problem because people always want too many children. No amount of technology changes that. The technology may make it easier to achieve a changed goal after you have decided on it, but we still have a problem in people's heads. Why do they want too many children? That is not an engineering problem.

KOFFLER: It is true that in trying to solve specific problems in the past engineers have been too complacent over the havoc in the larger system to which their activities have unwittingly contributed. It is only fairly recently that problems such as those involved in the space program have demanded of engineers to view systems from a total point of view. Interactions between biologists and engineers have become most essential. Regrettably most engineering schools do not encourage their students to study even a minimum of biology. When one considers that there is no engineering situation in which the biological component is not a critical factor, in many cases *the* critical factor, this lack of biological understanding has dangerous consequences. Here again, the space program may eventually have a profound influence since it is a model *par excellance* for an almost completely self-supporting system, in which the health and safety of human beings need to be protected. I am reasonably optimistic that many engineers will begin to think more of biological consequences intrinsic to the problems that they are tackling.

HARDIN: I quite agree with you. In other words, I see a spectrum of people in respect to their impulses. At one end are the philosophers and theologians, who as I say cherish their insolubilities. The engineer is the exact opposite. He says, "The difficult we do today, the impossible we do tomorrow." That is his attitude. By and large, engineers have done their work by assuming that man is infinitely pliable—you can mold him into anything you want to. This is why their solutions are so bad, and they are getting increasingly worse. The problem of transportation they have not seen as a problem of transportation; instead, they deal with the problem of moving cars on roads. The result is havoc. The problem of the city they see as a problem of boxes to put people in; again, the result is havoc. They have a too narrow view of humanity. However, I do think that there is a possibility that this may rather suddenly change and in this way. Engineering schools are now dealing with "life support systems" for satellites. This whole space program is fantastic and will end fantastically. Five billion dollars a year it costs and suddenly (say in 1973) the whole thing threatens to come to a complete stop. Suddenly innumerable people—scientists, engineers, etc. —are thrown out of work. They are probably foreseeing this now. I have a hunch that people in life support systems will say, "Well, let's take the system of spaceship earth and let's examine that." Once they start on that tactic they are going to realize that they are going to have to call in biologists, sociologists, all sorts of other people. They may, in fact, convert themselves into a new type of engineer. In other words, I think the most significant development that will take place in the near future will be the engineering of a new engineer. Then, following that, the new engineers will contribute significantly to solving earthly problems.

FINLEY: What I am trying to say is that certain historical events specifically demand certain courses of action. I can imagine some historical event happening to a population that would push engineers back just as quickly and as drastically as you predict. Do you see what I am saying?

HARDIN: Well, I think I do. On my own campus (and I am sure that such things are happening elsewhere, too) we have a self-appointed group trying to devise first a course, then a curriculum and then probably an institute or department on the "human habitat." Involved are people in geology, biology, chemistry, physics, and engineering. Many of these people have in the past shown no visible interest in human problems. They do now. I suspect this is happening spontaneously all over the country because it is "in the air." I think it will result in a change in the engineers, too. In other words, the engineers won't merely come in and re-engineer the world; they will themselves be altered by the interaction, for the better.

KOLLROS:* Where do we have the decision? The engineer is not going to decide to do these things to people. Somebody is going to have to decide that these things need to be done or have to be considered. Where is the decision-making going to be? If we follow Dr. Altmann in the question that was raised about the ghetto, and if we can find that living in a ghetto changes behavior in a particular way, then who will decide that we will have to attack the ghetto through behavioral means rather than through some other means? Who will decide that the behavior is good, bad or indifferent?

KOFFLER: Within our society we have a fairly well defined machinery for decision-making. All of us, in a sense, are contributing to this decision-making process, and as scientists we also have additional means for having our voices heard. I am not so sure that they are heard more loudly than other people's.

KOLLROS: I don't think the decision is at the level of the engineer.

* Kingsley Davis. 1967. Population policy: will current programs succeed? Science, 158: 730-739.

*Jerry J. Kollros, Chairman, Department of Zoology, University of Iowa.

KOFFLER: No, not at all. I don't think that this was the implication. The implication was that he could make a useful contribution by considering more knowlegeably the biological consequences of his activities.

BARTON:* We have a preliminary answer here as seen in such places as the Air Force and the Navy where a number of engineers are carrying out very interesting habitability studies. If you have ever been aboard a World War II submarine , realize that there has since been a lot of change in the engineering of these vessels, entirely from the point of view of habitability. Or, if you go anyplace where vast amounts of data must be presented to a small number of decision makers and be immediately perceived by these decision makers, you can see that the psychologists and the engineers have held hands in presenting display mechanisms which are extremely sophisticated, applications of just the kinds of engineering studies that we are starting to talk to.

ZWILLING: I think those are the relatively easy problems. The major problem is somewhere along the line of Dr. Hardin's projections. These are meaningful projections; a major decision will have to be made to the effect that having a child is a privilege and not a right. Now, that's going to be a much more knotty problem. That type of issue is going to require the sort of political considerations that are going to be very difficult.

KOFFLER: One assumption always is that somehow knowledge at the lower level of biological organization can be used to explain phenomena at a higher level. Are you optimistic that this can be accomplished? While so far this intellectual bridging has not been overly impressive, it is still my belief that it will be accomplished. In studying behavior, my own bias would be to study not only primates and man, but to take a very simple system with an as uncomplicated sensory and nervous system as possible, and to study it in complete detail. Perhaps some principles can then be gleaned that might hold for more complex systems that at the moment may not be amenable to full analysis. While it is true that bacteria and elephants are different it is also likely, as has been discussed by Hopkins and Zwilling, that higher organisms use mechanisms superimposed on common simple mechanisms. That is, understanding complex phenomena or even being in a position to ask intelligent questions about them may not be possible without basic generalizations provided by examination of simpler systems. I would say study the simplest organism that has a nervous system rather than monkeys. This can be done more rapidly, with fewer facilities and at a lesser cost, with the hope of obtaining generalizations that then can be applied.

Regarding the purpose of this discussion I am concerned over the fact that those representing the lower levels of biological organization posed specific scientific questions. However, regrettably, as soon as we left these levels we tended to talk in more general terms. For instance, Dr. Altman referred to crucial questions, but discussed mainly approaches. I am not going to let this meeting come to an end without insisting that you pose some specific scientific questions in your areas that still need answering.

Could we start with you Stuart? Why don't you react to my question regarding simple systems versus higher ones?

ALTMANN: I have two kinds of responses to the problem of choosing between "simple" versus "complex" animals. If you would like to study social interactions in *Vorticella*, that's fine. But I am not going to wait until you complete your work before I go out and watch monkeys. That's a matter of personal preference. I happen to find the complex societies of vertebrates very fascinating.

There is another, more intellectual type of answer to your question. For years, psychologists have been using animals like pigeons, mice and rats to study behavior. They have used these animals because of convenience and cost, but also because many psychologists believe that they are studying the universal characteristics of behavior, and if so, why not take the simplest and easiest animal you can handle, then apply the results to humans or any other animal? Yet, to whatever extent you study just those characteristics of behavior that are universal in the animal kingdom, you are by-passing the very aspects of human beings that separate us from all the other animals and make us a unique species. So it is this interest in humans in particular, but more generally, in the variability and great diversity of behavior and social systems in the animal kingdom, that we cannot satisfy by looking only at those aspects of behavior which are common to all animals.

You asked for specific research questions. Our problem is that we have too many of them and I hardly know where to begin. I will concentrate on a few that we have worked on recently.

Consider groupings of individuals, e.g. the cities of a nation or the troups of monkeys in a population. Under what conditions of birth, immigration, death and emigration in this system do you get an equilibrium distribution of individuals among the groups? This is an unsolved problem, both from the standpoint of realistic mathematical models of the dynamics of such systems and from the standpoint of adequate empirical data on the underlying population processes.

Another question is something like this. For a number of free-ranging animals we are now beginning to realize the impact of migration, not only on population dynamics, but

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also on genetics. For example, in the baboons that we study in East Africa it is becoming clear that genetic interchange between the social groups is due almost entirely to the adult males. The females apparently never leave the group into which they were born. So here we have a very specific genetic exchange mechanism. Yet, we don't know the conditions under which such intergroup migration takes place, nor the rate at which it occurs, nor what there is about the new group that attracts the migrant animal. Here is an area that is ripe for collaborative work between population geneticists and those studying social systems.

Another set of research problems center on the study of kinship selection. You can perpetuate your own genes by having offspring; that is the most direct, but not the only method. You can also do so by helping to perpetuate any lineage in which the individuals have the same kinds of genes that you do. For example, your nieces, and nephews, your grandchildren have a certain probability of sharing genes with you, and thus genes can be perpetuated through the influence of individuals who increase the likelihood that their own kind—whether their offspring or otherwise—will survive. The basic theory behind kinship selection has recently been worked fairly well out, but we do not yet have a single natural population of animals in which we know the effectiveness of kinship selection on population genetics. To do so requires that we have a group of free-ranging, recognizable animals in which we can observe who does what to whom, and we know the parentage and the lineage among the members of the group. There aren't very many such groups of animals, but here is a ripe research opportunity. It is a crucial issue in the relationship between social behavior and its effects on evolution.

BELL: The specific problem to solve in the next 20 years is survival. For organismal biology this is a matter of evolution and adaptation. As the physical, biological and social environment changes, we must know more about the ranges of tolerances and the evolutionary rates of all organisms. Some organisms may be very close to extinction merely because they have limited tolerances or low rates of adaptability or evolutionary potential of which we are unaware. We must study organisms to see how they may change and we must study the environment to see how it may be kept without change.

HARDIN: My general reaction to your proposal to attack the urban problem (just to state it too briefly and bluntly) by studying DNA would, to my mind, not be fruitful; at any rate, not soon enough fruitful. I think that all of us who are brought up in the conventional sciences have to be aware of a tendency that we sometimes have: to run away from a real and immediate problem by tackling it at so fundamental a level that we are assured that we will never get back to the problem in time to get embroiled in a controversy.

HOPKINS: A couple of years ago John Platt wrote an article, "Strong Inference" (*Science* 146: 347 (1964)), in which he discussed how certain fields of science move very fast, cover a tremendous amount of territory and then come to generalizations in a hurry. Other areas take a long time, do a thorough job and really don't get to generalizations very fast. There are several examples of fields that have worked out techniques for asking hard questions fast and getting immediate answers in order to scout a fairly large territory. One of the examples he used was molecular biology. The technique he suggested is the technique of asking sharp yes or no questions. One plots a course in this way with precise experimental questions asked. The answer can be only one of two things "yes" or "no." If "yes," then another question is asked, which again has a "yes" or "no" answer. If the question was originally answered "no," then one goes off in the other direction. One doesn't fill in until much later a lot of territory left behind. In a way I think some such technique for asking systematic, productive questions has to be worked out for these areas of behavior, sociology, environmental biology. I don't see that these precisely framed questions are coming out of the discussion we have had so far and this is the reason I welcome the chairman's challenge.

KOFFLER: This is really what I meant. All I am suggesting is that at any given level it would be more useful to take the least complex system, to study it completely, and then to compare it to more complex systems. I am afraid we tend to settle on organisms without considering carefully enough whether one's choices are the most suitable. Obviously, we are likely to pick organisms with which we are familiar and to some extent we are prisoners of our previous experiences. For example, graduate students tend to continue the type of work that they did either in their major professor's laboratories or where they stayed for post-doctoral work. I always encourage my students to reflect on the most significant questions that they possibly could ask and answer before starting out on their own. Once investments of time, emotion, energy, and equipment have been made, it is difficult to turn back. Incidentally, the practice of students following their professors' footsteps is another form of continuity—cultural continuity. We tend to imitate our professors, in this way the system changes only slowly. However, each human being has a chance to ask pressing unanswered questions and really come up with novel and significant answers rather than to continue merely what has been done. This is precisely what is needed in science. It is frustrating that even experts in a field often find it difficult to define the crucial questions, because if one can't define them one can't get answers.

HARDIN: May I speak to that point? Taking this business of population growth, reproduction, etc., I think you are right to ask some simple questions, and the problem is to pick out the fruitful simple questions. The ones that are along the direct line. For example, if you decide you will tackle the human population problem by studying the reproduction of any other animal you will be on the wrong track. This is not where the problem lies at all. Studying animal reproduction will not tell you why people want too many childern. When you examine it, the problem turns out to be very complex. We need to ask, "What are the immediate causes that lead a person to reproduce?" The answers are very peculiar, and very human.

KOFFLER: Of course cultural aspects are critical. To solve social problems one has to include human behavior as one of the essential ingredients, but if one seeks generalization before tackling specific social situations, one does not have to work with humans, one can work with marsupials. In fact if we had not worked with marsupials it is debateable whether we could talk about the pill today. This goes back to my original statement. "Significance" in science has many different connotations, one relative to making generalizations. The other connotation of course is social. In this discussion I have not placed one above the other, but merely indicated that the yardsticks are different. One may need to use different tools and ask different questions, not necessarily related ones, going after scientific generalizations than solving social problems. To answer the questions you posed, a lot of biological knowledge is not relevant. Truly, cultural knowledge is needed. However, if we didn't have the necessary scientific knowledge many of the cultural questions couldn't even be practically approached. At least now we have the vehicles to use. Of course, to get people to use the available knowledge involves a great many other problems that as yet are not scientific in nature. Hopefully, they are beginning to be approached in this manner.

BELL: I agree very much with the idea that we have not necessarily come to specifics in many cases. I think we might try to pose the problem in biology over the next 25 years, and I think we have a word "survival."

HOPKINS: I think we are admitting we are in a crisis situation—we are making value judgments, political judgments here. This is not the natural intellectual development of the subject necessarily, but we are imposing on it a panic situation which may well be necessary.

KOFFLER: I agree with Dr. Bell. In fact, I tried to introduce this when I asked the question to begin with. I would think a good point can be made that survival is the ultimate question in biology.

HOPKINS: I don't think it makes any difference whether it is biological survival or survival.

BELL: Let's just say biological because that it what we are interested in.

HOPKINS: It is a rather anthropomorphic view of the whole situation.

KOFFLER: It may be anthropomorphic, but unfortunately man now plays the key role in determining survival, including his own.

Winds of Change in Science Education Publications

By Dana L. Abell
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Last April, with a minimum of fanfare, *BioScience* launched a special section on biological education which promises to significantly increase the visibility of educational matters before professional biologists. Before we leap to say, "It's about time," we must acknowledge that *BioScience* has always treated educational articles kindly and has gone out of its way to give biological education a conspicuous place in the journal. This regular allotment of space (tentatively set at four to eight pages per issue, soon to appear every other week) means that *BioScience* will shift from a role of simply accepting articles on education in biology to one of actively stimulating their production. What is especially significant is that *BioScience* will almost certainly become a voice for *college* biology teaching (as no other journal is) since that is the interest and association of the vast majority of its readers. Recognizing this, the editorial board of *BioScience* has asked former CUEBS director Martin W. Schein to serve as consulting editor for this section, with regular assistance from the staff of the Office of Biological Education of A.I.B.S. We do say that it is about time, but in doing so we vigorously applaud this step by A.I.B.S. to help bring education into a coordinate position with "that other side" of the professional biologist's life.

The question whether the addition of an education section to *BioScience* is a step toward the establishment of an American Journal for Biological Education remains a moot one. Some proponents of the section have looked at it in exactly that light. Others express the hope that the section can survive in *BioScience* whatever happens elsewhere, for this may be the only continuing contact that the largest percentage of this particular group of biologists will have with current ideas and trends and progress in biological education. The latter view appears to be dominant right now, but much depends upon the number and quality of manuscripts that are received—itself a moot question.

Much is indeed happening elsewhere among journals in science education and it may be that this move by *BioScience* is a mere straw in the wind—a wind which is scarcely of gale force as yet but which is most certainly a turbulent one.

Included are: changes in both publisher and editors for the *Journal of Research in Science Teaching* and *Science Education* (John Wiley and Sons will now publish both; new editors are identified below); a new editor for *The American Biology Teacher* (Jack Carter of Colorado College, formerly BSCS Associate Director); the addition of two American associate editors (one each from AIBS and NABT) for the relatively new and very British *Journal of Biological Education* from Academic Press and the Institute of Biology in Great Britain; initiation of a new indexing service for the leading journals in education (CCM Information Systems, Inc.'s *Current Index to Journals in Education*); and the emergence of a new series of special bibliographic reports on selected topics in science education from USOE-ERIC's Science Education Information Analysis Center in Columbus, Ohio.

At the moment the most important of these changes seem to be those associated with John Wiley and Sons' entry into the science education periodicals market, particularly its late 1968 purchase of *The Journal of Research in Science Teaching*. Continuing as the official publication of the National Association for Research in Science Teaching (with the Association for the Education of Teachers in Science), this high quality quarterly will henceforth concentrate its editorial activities at Teachers College, Columbia University, with James T. Robinson (who is serving also as a CUEBS commissioner) as chief editor. The change appears to bode well for this already strong, if irregular, journal.

Less easy to assess is the significance of Wiley's purchase, also last year, of *Science Education*, which had fallen on hard times as an independent, privately published journal after severing its relationship with NARST in 1961. The Wiley people assure us that *Science Education* will continue, despite the recent gap in publication, and strong hopes are expressed that the uneven quality that marked recent years will steady up somewhere in the high end of the range. N. E. Bingham of the University of Florida assumed the editorship early this year.

In the long pull CCM Information Systems' new *Current Index to Journals in Education* will certainly have considerably more influence than any single journal in converting the present badly dispersed and decidedly uninspiring literature in science education to an actively growing, self-reinforcing and self-correcting system. Designed by USOE and CCM to serve needs that USOE-ERIC's abstracts, *Research in Education*, have failed to meet, this new service provides subject and author indexes to nearly 250 current periodicals in all fields of education. Some thoughts deriving from a recent look at the literature being cited in this index are offered elsewhere in this issue of *CUEBS News*.

More clearly in the category of pleasant surprises has been the appearance of a series of subject bibliographies from the ERIC Clearinghouse for Science Education, also called the Science Education Information Analysis Center, at Ohio State University. We hope to devote some attention in an early issue to the serious problems that this Center has faced and the surprising recovery that this series portends. In addition to being dispersed and uninspiring, the literature of science education is also chaotic, and it is pleasing to see that this Center, after seemingly adding a few elements of its own to the chaos, is bringing some of its holdings together in an unusually useful way.

Other hints of significant change are born on these same winds, but none is past the rumor stage as yet. What appears to be happening is that several groups of people are noting all at once that 40,000 or more college biologists are not being served with a periodical that relates clearly to the teaching side of their jobs and, more significantly, that forges a firm link for all biologists between educational experimentation and research on the one hand and teaching practice on the other. We keep asking, why the delay, and find that would-be editors are hesitating out of uncertainty that sufficient manuscripts can be drawn from the teaching biologists who must ultimately generate the real driving force for such a journal.

On the financial side, potential (organizational and commercial) backers, who remain characteristically secretive, seem to have stalled in sorting out and sizing up markets. "A potential of 40,000 readers is fine," they seem to be saying, "but how very much nicer it would be if we could expand that to 60 or even 100,000." Reaching out for those extra tens of thousands likens these publishers now to the indecisive donkey starving to death between three equally attractive piles of hay. Should this bigger market be all English speaking college biologists, or college teachers in all the sciences, or biologists at all instructional levels? Perhaps 40,000 of us represent mere stubble beneath the poor sick animal's feet, but we are at least a start. Is no one ready to apply the boot that will set him to moving?

Getting Biological Education into Print or Thoughts on Entering CCM's Current Index to Journals in Education

By Dana L. Abell
Senior Staff Biologist, CUEBS

The Announcement last winter of a new *Current Index to Journals in Education* bore belated fruit with the mailing of the first issue in early summer. The *Index*, which already is calling itself CIJE, may be worth more notice than most college biology teachers are accustomed to giving materials bearing the label "Education." Production of the *Index* is by CCM Information System, Inc., 866 Third Avenue, N.Y. 10022, with major financial and technical support from the U.S. Office of Education's Educational Resources Information Center (ERIC).

The list of Journals being indexed in CIJE is still quite tentative, and it doesn't yet include either *BioScience*, for its recently initiated education section, or the new British *Journal of Biological Education*. Examination of the first issue suggests, however, that the series may be worth keeping an eye on. Quite clearly, it will do things for the teachers in subject disciplines that ERIC's rather disappointing abstracting service, *Research in Education*, has failed to do, possibly because it is so closely limited to capital-E Education. USOE's indefensible block against working outside of education per se is still quite evident in the *Index* (even the very notable *Journal of Chemical Education* is excluded from this first version of the list!), but references to reports on innovations in biological education that this observer shouldn't have allowed himself to miss are definitely being included.

Enough unfamiliar citations appeared in this first issue in fact that I set about using the journal list for CIJE to check out an impression, formed after a year's dalliance with *Research in Education*, that no actively growing, self-reinforcing, self-correcting literature exists in biological education and that contributions are merely dropped into a loose and disorganized literature on a hit-and-run basis—better called the "published-my-thesis-and-got-out syndrome", perhaps.

Placing myself in the shoes of an innovative biologist who is seeking the stimulus of others' ideas in teaching college biology and who expects subsequently to get the results of his experience into print, I looked through the CIJE list of journals and checked representative issues of the unfamiliar titles in hopes that the hint of an active literature, gained from CIJE Vol. 1 No. 1, might actually be true. In the first run through the 236 journals in the CIJE list I found some 58 that I thought might be worth closer examination. But titles proved deceiving, and on looking at contents and editorial policies of these I found that only 15 actually appeared to be tailored to my interests, with only seven of them concentrating on science and only nine being oriented toward higher education. A single periodical, *The Journal of Medical Education*, appears in both of these tiny groups. Both of these categories will expand as the total list is enlarged, for the people at the Science Education Information Analysis Center (an ERIC clearinghouse) in Columbus, Ohio, report that only a few of their recommendations were accepted for this initial version of the *Index*. Subject-specific journals aimed largely at college levels will presumably be included soon. In biology, though, there are few journals that can be added as the indexing service expands. From journal titles, then, we must conclude that the situation facing a person who seeks a place in the literature to exchange ideas and experience in science education in a very current and stimulating way is indeed grim. True, results could easily be placed before a sizable audience whose interests and associations are with education per se in some of the remainder of those 58 journals, but this would make the feedback loop to new trials and to the active accumulation of varied experience far too devious.

The second step in checking out my impression of the nonexistence of a "growing literature" in biological education amounted simply to continuing the fantasy. Suppose that we have a person now who is ready to report the results of a rather interesting trial of an inductive approach to morphologic-taxonomic laboratories in invertebrate zoology. What journals might be most appropriate and what do their special characteristics tell us about this literature? The list that follows is a first approximation at an answer. It is derived more from a recent search of the literature for articles in support of the CUEBS Laboratory Panel's activities than from study of the CIJE list. It represents one observer's personal choice of journals (the annotations are definitely my own), and the total list is therefore at least as tentative as CIJE's. Perhaps the list and the annotations speak more emphatically to the question on a "growing literature" than I can.

A. Journals in Biological education:

1. *BioScience*—Major publication of American Institute of Biological Science, Washington, D. C., serving a varied fare to biologists in a wide variety of (but not all) disciplines; readership largely research oriented and predominantly associated with colleges and universities; recent papers in small section on biological education suggest that fate may deal this section on biological education nothing more than essays (speeches), discussions of curricula, and reports on specialized instructional equipment.

2. *Journal of Biological Education*—recent addition to British literature by Institute of Biology, London, and Academic Press; covers all educational levels but with bias toward problems peculiar to the British Isles, with correspondingly heavy use of a "very British" vocabulary; college biology still playing only a minor role; welcomes American contributions but receiving few.

3. *American Biology Teacher*—from National Association of Biology Teachers (Washington, D. C.); society and journal in a state of flux; both formerly oriented heavily toward high school and relatively unsuccessful in attracting either membership or manuscripts from college level; as college membership now ex-

pands, college coverage due for increase; editing of journal inconsistent in past, may be tightening up soon.

4. *Bios*—a quarterly from Beta Beta Beta, national undergraduate biological honorary, Drew University, Madison, N. J.; accepts articles from students and faculty, including non-members, on either research or teaching; emphasizes reports on undergraduate research; receptive to articles on teaching but few printed to date.

5. *Journal of Medical Education*—Association of American Medical Colleges, Evanston, Illinois; prints surprising amounts of high quality material on medical and (to a very limited extent) premedical education, including trial of widely applicable teaching techniques; a strong journal, but parochial in interest; circulates well beyond confines of the medical school.

B. Journals on science teaching, all disciplines.

6. *Journal of Research in Science Teaching*—Published for the National Association for Research in Science Teaching and the Association for the Education of Teachers in Science by John Wiley and Sons, N. Y.; well edited, high-quality journal with both research reports and discussions of concepts; all educational levels; readership largely in colleges of education.

7. *The Science Teacher*—National Science Teachers Association (Washington, D.C.); an attractively edited but sometimes journalistic publication; steady improvement in both content and appearance discernable in recent years; attracting many significant articles on pre-college matters; heavy slant toward high school continues but may change somewhat in the near future.

8. *Science Education*—Published privately until late 1968 by the late C. M. Pruitt; NARST-sponsored through 1961; now owned by John Wiley & Sons, N. Y.; until change of ownership, ranked as a lesser journal of uneven quality, with numerous obituaries providing very human insights to American education; continuing under new editorship; sweeping changes expected; the 10 issues per year include sprinkling of reports on innovations in college biology but heavily dependent upon Ed.D. dissertations.

9. *School Science and Mathematics*—a regional journal (Central Association of Science and Mathematics Teachers, Inc., Kalamazoo, Michigan) of extremely uneven quality, ranging from chatty expressions of opinion to highly stimulating ideas for innovations in teaching (notably in choice of content); intended emphasis on high school not adhered to but virtually no college-level articles printed.

C. Journals on higher education in general.

Most journals that are identifiable strictly with higher education choose some special slant which makes them relatively unreceptive to papers that are clearly rooted within the disciplines. Exceptions are the three excellent journals serving college teachers in chemistry (*Journal of Chemical Education*), physics (*American Journal of Physics*), and geology (*Journal of Geological Education*).

An example of a special, non-disciplinary slant is that of the prestigious *Educational Record*, from the American Council on Education, which is known informally as the "college presidents' association." This journal prints extremely penetrating essays on general problems of college education, bringing its administratively preoccupied readers back to higher questions of purpose in education. The *Journal of Higher Education* from the NEA affiliate, the American Association for Higher Education, and the Ohio State University Press, does much the same thing for an audience which sees a great deal more of the classroom. *The Junior College Journal* from the Washington-based AAJC, provides what its subscribers (unfortunately?) seem to want, namely articles on administrative matters. The disciplines figure in almost entirely in relation to specialized terminal programs. As might be expected, the *Journal of Teacher Education*, from another NEA affiliate, the National Commission on Teacher Education and Professional Standards (Washington, D.C.), deals largely with training programs in pedagogy.

To our knowledge, only one institutional publication, out of the many, limits itself to college-level material. This is *Improving College and University Teaching* from the Graduate School at Oregon State University, in Corvallis, Oregon. This journal does print articles that are likely to make the college biology teacher sit up and take notice. It is worth much more attention than it apparently gets.

From here one proceeds out into the general literature of education where pedagogy itself is the primary concern. Examples are *Journal of Education, Journal of Educational Research*, and *Journal of Experimental Education*. This literature is worth some attention by the biologist considering radical change, but the sort of report on which we are basing our fantasy here is certain to go virtually unnoticed in it and may even be practically unretrievable should it appear there, though CIJE may be changing that factor drastically.

Now, quite obviously the impression that we have sought to check through *CIJE* is true. There is no active literature in college-level biological education in the sense that an individual can follow developments on an almost month-to-month basis or in the sense that one experiment begets a whole sequence of others through which first the validity of an idea and then its limits are tested, variants are posed, anomalies investigated, and so forth. *CIJE* may help to change this situation but not rapidly and not on a large scale. Much more is needed, and that is our real purpose in bringing up a question that we knew in advance had a discouraging answer. My suggestions, which I will explain in the next issue of CUEBS News will be these:

1. Concentrate publication efforts in a very few journals.
2. Establish a quick-flux medium for exchange of teaching ideas and preliminary results from their trial.
3. Initiate an advisory program to help college biology teachers put their educational innovations on firm experimental ground.
4. Start a committee working to break the closed loop which blocks the establishment of a college oriented journal of biological education for North America until a substantial flow of manuscripts is assured yet prevents the initiation of the necessary experimentation until the influence of such a journal can be felt. If suggestions 1-3 have the potential to do this, the committee would make certain that they do so. If not, it would put the best available minds to work finding a successful way.

Perhaps you would care to comment on these matters and help us to make this next discussion over into an open forum.

Making General Botany Enjoyable

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Faced with the task of teaching a non-science major general botany cl ss in the Fall of 1967 to 500 students in one lecture section, I decided to present the material in a manner which would most please me if I were one of my own students. My own experience as an undergraduate included a general zoology megaclass which I loathed and cut completely after the first two weeks for the following reasons: 1) the lectures were dull; 2) the lecturer merely paraphrased the elementary text as his lecture material; 3) the heavy quiet of many humans put me to sleep every time. Consequently in my general botany megaclass I attempted to reduce non-content negatively and increase the likelihood of high (90%) attendance and high (50%) interest/attention density. The heavy quiet was removed by using Schlicting's successful technique of background music; my students nearly all approved, freely criticised selection, offered suggested music as well as records and recording equipment; eventually I used the recording studios of the campus radio station and the equipment of a local rock group. Several times live music, flute, jazz combo, guitar, scitar, was used. An interesting situation then developed whereby extreme audience interest occurred if no music were playing when they first entered the auditorium. The texts chosen were Carson's The Sea Around Us, a gentle easy-to-read story with lots of biology, and McLuhan's Understanding Media, which is an extraordinarily fine challenge to the rational mind as well as a good background for examining today's machine-oriented science, especially biology. I was much more interested in encouraging students to understand how we obtain data, and establishing the way in which they use botany in their everyday lives, than in having them memorize a lexicon of phytology.

Given an auditorium, a stage and a microphone (to be heard over the exhaust blowers at the rear), I realized that a megaclass was quite different from the usual small class with 10-20 young scholars hanging on my every tightly-organized paragraph. It was like a theatre of learning. Rather than use banal coercion through discipline and threats of punitive respon or non-attendance, I decided to use banal gimmickry in addition to music. I attended a mixed-media workshop called Group 212 under the direction of the painter Liikala near Woodstock, N.Y., where I learned a few fundamentals on how to organize and project various sets of sensual stimuli which can overwhelmingly spark the audience into attention during a typical stand-up lecture.

Each of the resulting class presentations was prepared by listing first the major topic (drugs, phytosex, war and competition, anthropomorphism, DNA as an information service, symbiosis etc.) and then assembling all possible visual, tactile, and even olfactory images readily available. Visual material included slides, movies, and physical objects which were then ordered into two sequences (only one of which was usually presented), one for maximum comprehension and one for random mixing of the same information. Slides and films were projected singly, together and superimposed, depending upon the effect produced versus the effect desired. Presentations were of 30-40 minutes duration which permitted an "Open Forum" afterwards, where students asked questions and made comments. Interest was great, attendance often overflowing, and publicity derogatory. Ninety percent of the seats were generally filled, often by 50-75 "guests" who included students not registered in the course, angry and curious faculty, townspeople, and visitors from proximal colleges. I realized that attendance was not a final goal, but agree with P. T. Barnum, that "unless you get 'em inside, you might as well quit", and figured that if students wanted to come, then they might listen to my lectures and observe the visual data. Student response was spectacular and enrollment doubled from 500 to over 1000 for the second offering of the course. Many students were confused by the unusual format, but most wrote in anonymous evaluations that they enjoyed the course, learned how to think about Nature for the first time, and suddenly realized how important botany and biology were to everyone's everyday life—and who could ask for more?

SPECIAL ANNOUNCEMENTS

SYMPOSIUM ON UNDERGRADUATE STUDIES IN ENVIRONMENTAL SCIENCE

Of interest to undergraduate biology teachers should be a symposium scheduled for the AAAS Annual Meeting, Boston, December, 1969, entitled "Undergraduate Studies in Environmental Science." The first session will be oranized around two principle statements on the topics: "Education for Today's Ecological Crisis" and "Education for Tomorrow." These presentations will be followed by discussion among several expert witnesses of the movement. The second session, with similar format, will be organized, interestingly, by leaders of the National Student Movement. The students will choose speakers and discussants, largely from the Student Movement. The program is being arranged by Everett M. Hafner, Dean, School of Natural Science, Hampshire College, Amherst, Massachusetts, who may be contacted for further details.

FELLOWSHIPS FOR COLLEGE SCIENCE TEACHERS

The third Shell Merit Fellowship program for college and university science teachers will be held at Stanford University, June 22 to July 17, 1970. The purpose of the program is to explore ways in which science faculty members of colleges and universities may assist and provide leadership for the improvement of the science curriculum of especially the elementary and junior high schools.

Attention is focused primarily on the development of college science courses in academic departments for the nonscience major, particularly for those students who may ultimately decide to teach in the elementary schools.

For further information contact:

Dr. Paul DeHart Hurd
Director, Shell Merit Program
School of Education
Stanford University
Stanford, California 94305

JUNIOR COLLEGE RESEARCH

A special interest group in junior college research is being formulated within the American Educational Research Association (AERA), according to Dale Gaddy of the ERIC Clearinghouse for Junior Colleges. The group will be open to members and non-members of AERA.

The purpose of the group is to provide a forum for the sharing of ideas and findings of research relating to junior/community colleges. Its first meeting is planned for the 1970 AERA convention in Minneapolis (March 2-6).

Persons interested in joining the group are requested to contact Dr. Gaddy at the following address:

ERIC Clearinghouse for Junior Colleges
96 Powell Library
University of California, Los Angeles
Los Angeles, California 90024

A membership fee of one dollar ($1.00)—made payable to AERA—should accompany each response. Also, each respondent should indicate whether or not he tentatively plans to attend the 1970 AERA convention.

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Viewpoints!

By Dana L. Abell
Senior Staff Biologist, CUEBS

(Ed. Note: Viewpoints! alternates among members of the Executive Staff; it is intended to reflect their individual views and not necessarily those of CUEBS.)

In the search for ideas that will move college biology a sizable jump along the road toward some bright new future no one ever seems to suggest what may be the simplest cure of all for some of our current ills, namely that we biologists just go to work and teach each other how to lecture. Too often, I suspect, we tend to reject the easy solution to a difficult problem just because it seems unreasonable that there could be a simple way out. In the excitement of the chase it is easy to overlook the fact that the radical changes that most of us in this business are seeking usually require tremendous amounts of effort, and what's worse, are fraught with uncertainties that often make the "tried and true" attractive indeed. If we allow that radical changes are needed in some parts of our teaching job (the laboratory, for one), then both economy and logic seem to demand that improvements in other areas come simply by doing what we have always done but just one heck of a lot better. Let's see what might be involved if we were to take this view of the lecture in undergraduate biology.

Now obviously, the range in lecturing abilities among teaching biologists runs from phenomenally good to incomparably bad, and locating a mode accurately between these limits with the sort of information that we have is worth more laughter than discussion. From evidence gained, though, by sampling around at professional meetings, by tip-toeing down hallways (come on admit it, we all do it!) at colleges where I have worked or visited, and by asking during some of my site visits for CUEBS if I can watch the very best of them in action, my own conclusion is that biologists stand with most other scientists somewhere down in the low end of the range. The clincher has been that those very best of lecturers usually seem to be "off form" the day I am there. My feeling, in fact, is that the guy down there at the "incomparably bad" end has plenty of company. I know because I was there once and I wasn't lonely at all.

All of us can recite horror stories, I suspect, about the plunge from the womb-like security of graduate school into a world that demands instant competence in teaching. My own agonies were highlighted, retrospectively, by a compliment from a friend who had happened in on a recent, relatively fortunate trip of mine to the lectern. The remark that I was "a good lecturer" came in such an offhand manner, in fact, that I doubt that he even faintly suspects how long it took me to become aware of such things as personalizing or pragmatizing a point to drive it home and varying pace and tone of voice to gain emphasis or to suggest a digressive or illustrative tack.

There are dozens of things that I've learned in that long and painful haul up from the bottom that I'd enjoy passing on to a newcomer. One of them might be the point about building an awareness of relationship and sequence by reaching back to repeat points, now in a varied tone perhaps, and by dropping in anticipatory references to serve as flags for future direction. Another would surely be the technique of planting a question through a series of observations that not only make the question obvious, but make it an apparent pivotal point in the story I am trying to tell. But *developing* an answer rather than just telling it is something I haven't been able to do well. How very much I would like to talk this over with one of the newer teachers I've seen who seem to come by this skill more easily than my generation ever did! I am sure we could both gain by exchanging ideas and trying our techniques in the other's presence and then carefully sifting through the result for each other.

But like the timid father trying to tell his sophisticated son about sex, we still don't seem to want to talk about these things. I am encouraged in the knowledge that some new set of factors is causing our beginning teachers to emerge from the "womb" with more confidence than I had, so that it is possible for them to get more rapidly to the point of thinking about what they are saying rather than how they look. (Oh, those miserable years!) Here, though, things are still breaking down and neither we nor they seem to be pushing on from the initial questions about how to lecture to the really crucial ones of why and when. For them, just as it is for us, the lecture is still a block of time that we have to fill—the fifty minutes when the schedule says we will stand before our respective audiences and talk. The easy thing to do seems to be simply to tell them what we want them to know, and unfortunately, it still seems to be the logical thing as well. True, there has to be a major information input somewhere in the course; and we can all remember lecturers who conveyed much interesting and valuable information to us. But without their skill and their technique are we wise to cast our lectures in the same mold? Aren't we, in our collective mediocrity, all risking making these most vital of classroom contacts into what one lecturer on not lecturing recently called "exercises in information pushing and student stuffing?"

Where, in fact, are we going to put the demonstrations of a "scholarly mind at work" that the very notable Report on Undergraduate Instruction from the University of Toronto* describes as the most essential beginning and most satisfying end of a sophisticated educational experience? Where do the initial steps come in conveying an understanding of the processes of scientific thought and an appreciation of what Bronowski calls "style in science?"

The answers to these questions are certainly not all in, but it seems to me that we need to make the lecture do a lot more for us than just grind out facts, however skillfully. Beyond the problems of technique that I have mentioned there are obviously deeper problems of tactics and style and beyond these are even more serious ones of strategy and of basic aims.

So it turns out that our simple solution is not a simple one at all—but it may be a great deal easier than rejecting the lecture altogether, which is virtually the only way we hear the lecture mentioned now in our search for ideas upon which to base a new kind of college biology. I'm convinced that it is a lot easier—so convinced, in fact, that I am hard at work assembling a list of "ways to lecture without really lecturing" that is already 13 items long. Perhaps we can try some of them out on you, descriptively, in an early issue of *CUEBS News*. I'll bet I've thought of some that you haven't!

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* University of Toronto. 1967. Undergraduate Instruction in Arts and Science. Report of the Presidential Advisory Committee on Undergraduate Instruction in the Faculty of Arts and Science. University of Toronto Press, 149 p.

AVAILABLE CUEBS PUBLICATIONS

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Publication
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7. * The consultant bureau. Revised, August, 1967 (for those interested in obtaining curriculum consultant service).
9. Report of the northeastern regional conference on courses and curricula in the biological sciences. April, 1965.
10. Report of the southeastern regional conference on courses and curricula in the biological sciences. July, 1965.
15. Biology in a liberal education: Stanford Colloquium report. February, 1967.
16. * Guidelines for planning biological facilities. August, 1966 (materials including description of facilities consultant service).
19. Biology for the non-major. October, 1967.
20. * Testing and evaluation in the biological sciences. November, 1967, Reprinted by OBE/AIBS, October, 1968.
22. Basic library list for the biological sciences. March, 1969.
23. Teaching and research. May, 1969.
25. The pre-service preparation of secondary school biology teachers. June, 1969.
26. Biology in the two-year college. April, 1969.
27. Biological prerequisites for education in the health sciences. June, 1969.

* Request by individual letter, to AIBS Office of Biological Education, 3900 Wisconsin Avenue, N.W., Washington, D.C. 20016.

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Baker, E. G. Stanley 1969. Symposium on undergraduate biology for the professional student. Journal of Medical Education 44(1)

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