BIO-TELEMETRY

The Use of Telemetry in Animal Behavior and Physiology in Relation to Ecological Problems

RROCEEDINGS OF THE INTERDISCIPLINARY CONFERENCE, NEW YORK

Edited by LLOYD SLATER

Sponsored by
The American Museum of Natural History.
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in co-operation with
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Sponsored by
THE AMERICAN MUSEUM OF NATURAL HISTORY
OFFICE OF NAVAL RESEARCH
FOUNDATION FOR INSTRUMENTATION EDUCATION AND RESEARCH
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PREFACE

THIS book has materialized from almost 1000 pages of tape-transcribed discussion which took place during a unique four-day conference at the American Museum of Natural History in New York City at the end of March 1962. Three days of that meeting were behind closed doors and much of what was said not only dealt with new and previously unreported work, but was also presented with beguiling candor and informality. On the fourth day a summary was made to a sizable public audience in the Museum's auditorium.

Putting this material together has been sort of an editor's dream—with minor nightmare tinges. Because the closed discussions took place without restraints, all of the people who were caught on tape were asked to "polish-up and remove the noise" from what they said. The result is a sterling collection of post-meeting papers which, while now somewhat inhibited by second thoughts involving accuracy and restraining optimism, somehow retain the breezy flavor of the sessions. So a unique meeting has produced, we believe, a rather unique (often quite readable) technical document.

As the title of the book suggest, this was an interdisciplinary conference which brought biologists together with engineers to consider how the wonders of modern electronics might be used to study the behavior and physiology of animals and man. As such, then, this is a book mainly about technique; it is not a book devoted to the results of scientific research. But the reader will quickly see that in the realms of bio-telemetry it is often impossible to divorce the technique from the reason for using it; that the ends must often necessarily involve and justify the means. Because of this the contents offer an interesting blend of technical and scientific reporting.

Aside from its logic as the first platform for a collection of reports on bio-telemetry, there was sound reasoning on the way the conference was organized which gives this book, we believe, a certain text-like utility beyond the usual proceedings of a scientific conference. It also includes a few unexpected bonuses. The first you will see is the provocative keynote address delivered to the group by Warren McCulloch. True, this talk is not about bio-telemetry as such. But it sets such a note of excitement and optimism about the new look in biology that we feel it is the perfect opener for this optimistic book.

Another bonus is the detailed and often pithy give-and-take which has been recorded as "discussion" after each paper. For this we are greatly indebted to the several people, such as Otto Schmitt, John Busser, Frank Haahn, Leland Bagby, John Tester, Howard Baldwin, Manfred Clynes, Carl Berkley, Lendell Cockrum (interdisciplinary types all) who agreed to act as discussants in various sessions.

Finally, it is the editor's privilege to express the collective appreciation for those who attended the conference to the American Museum of Natural History and its gracious Director, Dr. James Oliver. Equal regards and fervent thanks from the group also go to that prime-mover for the conference, Dr. Sidney Galler, and to the U.S. Navy for the splendid backing it gave to his idea. If this book proves stimulating and useful it is mainly due to the vision and persistence of Dr. Galler, and if it can be dedicated to one person, let it be to him.

August, 1962 LLOYD E. SLATER

The meeting was held, conference style, around tables in the basement classroom of the Museum's Hayden Planetarium. Standing at the lecturn is Dr. Norman J. Holter, who presided at the second session.

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Keynote Address

THE COLLOQUY OF LIVING THINGS

THE COLLOQUY OF LIVING THINGS

WARREN S. MCCULLOCH

Since 1952 Dr. Warren S. McCulloch has been a staff member in the Research Laboratories of Electronics, Massachusetts Institute of Technology. He got his degree in medicine from Columbia University in 1927 and spent a number of years doing research and teaching in physiological psychology. He then went to Yale University as a Sterling Fellow and later studied the activity of the central nervous system while an Assistant Professor at that Institution. In 1941 Dr. McCulloch became Director of the Laboratory for Basic Research in the Department of Psychiatry at the University of Illinois and also held the title of Professor of Psychiatry and Clinical Professor of Physiology. It was during this period that Dr. McCulloch became well known as one of the founders of the group that has developed Cybernetics and in intervening years has published some of the fundamental principles in this field.

I WOULD like to take you back to the earliest days of the Josiah Macy Jr. Foundation's wildest attempt to persuade human beings to talk to each other. The group was organized by Frank Fremont-Smith. It began on account of what had happened during the war and then was locked up "for secret". It concerned systems and computers and processes that depend upon some circularity in the passage of the information. This study of such circular causal or feedback processes, was later christened Cybernetics.

Nothing that I have ever lived through—and I was chairman of all ten sessions—has ever been like the first five of those meetings. We had designed the group very carefully, so that there were always two members from any field. Hence, every man could count on at least one person in the audience who would understand his shop talk. The first five meetings were unpublishable. You never have heard adult human beings, of such academic stature, use such language to attack each other. I have seen member after member depart in tears, and one never returned. There was one very tough customer in the group. She kept a flow diagram of the conversation for us. I am sorry she is not with us tonight; Dr. Margaret Mead. I well remember one of our scientists, and I won't say who, shaking his fist in Margaret's face and shouting "Hell man, if you don't think that the squirrel knows what the blue jay is saying when you go into the woods with a gun, you've never been hunting. Hell, man, you've only been blundering around the woods with a gun." This is a mild sample—such is the Colloquy of Living Things.

By contrast, today's meeting was an admirable example of getting on well together. Somehow we are learning to talk to one another. It has always been a tough chore. We had so much trouble in Chicago with the development of difficult lingos in different laboratories that we finally instituted a fantastic program, thus: We took one young scientist after another out to the west side of Chicago, where there was a string of high schools having parent and teacher meetings. We told each scientist that he had to explain his newest discovery, not to the men, but to the wives. It worked. It took us some eight years of that colloquy of living things before we were able to hold meetings in Chicago with engineers, bio-chemists, and M.D.s and have them understand each other. These things are not easy. So much for history!

What I would like to do now, primarily for the sake of the engineers present, is to say something about what is peculiar about the biological way of thinking. You have to remember that our biology began to grow up with the city states of Greece. Their fundamental notions were derived from the way those cities ran.

THREE LAWS CRUCIAL TO BIOLOGY

There were several great laws of society which formed the substance of the biological notion on which medicine was founded. You find them sometimes better expressed in the Hippocratic group, sometimes by Empedocles. The first of these is the law of equality of unequals. The equality of unequals in the Greek city state meant what we mean, right from the Constitution on, by the equality of all men before the law. They did not think that an intelligent man and a stupid man were equal in intelligence; nor a strong and a weak man equal in strength. They meant that all were equal before the law. This comes over into biology in a very curious form, and is one of the cornerstones of the theory that grew up much later (first, incidentally, within a kind of ecology) in our theory of the relation to one another of living cells, tissues and organs of animals. But it is one of the cornerstones of biology. In an animal, if the skin cells die, the animal dies. If the neurons die, the animal dies. Each kind has to be maintained. Each does its own chore and each has its own say in that community of living things.

The second law, and this I am sure the engineers will recognize, in an economic sense, is, *general because first, general because best*. That opinion prevails, becomes general, because it is best. Thus, a man becomes the General of the Army because he is the first, the ablest. This law of selection operating promptly in a Greek city state (and believe me, it involves very rapid turnover) is the law which we know much later in evolution. We are just beginning to realize the extent to which it bites into engineering tactics. That device which is best eventually wins. It is a Biological Law.

The third law that is crucial in biology comes a different way around. It comes from the difference between what a man meant and what got understood. The Greeks had a theory that in the formation of the offspring a kind of tape was fed in by the male which then reacted with the machinery of the female to produce life. Of course, the tape might be defective or the machinery of the female might be defective, but the program—no! The program is "an ideal mixture", to use their phrase. The program is what is not wrong. If there is an error, the holes in the paper are wrong or the machine does not read it right. This made the great distinction which later you will find as the difference between Bound Causes and Accidential Causes. I hear it every day from our programmers at MIT. The program was all right. Something

FIG. 1. Warren S. McCulloch, Researcher in Cybernetics at Massachusetts Institute of Technology, delivers the final line of his keynote address: "The Colloquy of Living Things". Seated at the table are Committee Members for the meeting. They are (left to right) John Flynn, Lloyd Slater, C. DeWolf Gibson (Toastmaster and Vice President of the Museum), Sidney Galler and Wesley Lanyon.

went wrong with the tape or something went wrong with the machine. This crucial distinction between that for which something was built, which should have carried through the ideal mixture, and the resultant of such an ideal mixture, perverted, distorted, torn by some accident, is at the foundation of their biology, and it still remains there. It carries with it the notion of final cause. Biology without a notion of final cause, generally goes on the rocks very rapidly, and, at its best, it deteriorates into biophysics. I'm sorry. I do not mean biophysics in the ordinary sense. This is a machine looking for a job. I mean biophysics in the best sense.

The point is very simply this. Living organisms must survive or they are not there for us to see them. The engineers' equipment must serve its function better than something else or it won't be there at all.

Purposefulness is in the world about which we speak. It was Carnot (an engineer) who made the distinction between work and energy. It is the communication engineer who has had to make an equally sharp distinction between signal and noise. Now, do not misunderstand me. I have been reading the most delightful nonsense in Science—the controversy concerning final causes—the objection being that they are not efficient, or that they don't get things done. This is exactly what Aristotle meant. They were final; they were not efficient!

Aristotle in this, as when one considers the gall bladder of the whale or the origin of eels, is right. It is a biological distinction and he made it correctly. If it is a biological distinction then it holds its place in Engineering!

You know, from these notions comes the first great fundamental of biology: Like begets like.

A second great fundamental came from Mendel. It involved sweet peas. He was able to make out the distinction between those that were dominant and those that were recessive. He was very fortunate and very careful. When he delivered his paper, the mathematicians got up and went out and then the biologists got up and went out—the mathematicians because the mathematics was too difficult, the biologists because nothing in biology is that mathematical.

MATHEMATICS AND BIOLOGY

We have had the same fight as Mendel right along. We, who are biologists, are nomally accused of having gone into biology because we were incompetent in mathematics. I'm sorry. Most of the time the mathematics necessary for biology does not exist. There are few people, Kac, Karl Menger, and Ulam in this country and half a dozen elsewhere, including Kolmogorov, who have been wresting with these problems—problems that are mathematically so tough that it is going to take a sixteen year old to crack them. I say that after having worked on one of the simplest of these problems for about seven years by myself, I was joined by Manuel Blum of Venezuela—he was just a little over eighteen years old then—and he promptly cleaned up one whole field of it (he still is with me).

The next problem I want to tackle is a much tougher one, let alone the mathematics. I'll try to spell it out.

May I tell you the reason why the engineer is so far behind the nervous system when he tries to build communication devices? It is because he is still dealing with devices having only two inputs. For these we have mathematical proof that you cannot have an error-free capacity.

As a matter of fact, Jack Cowan and Sam Winograd have now shown that it is only as you increase the richness of the connections that you can apply communication theories—the important axiom of a theoretical capacity for transmission to computation in the nervous system. This work is so significant that it will appear in the Philosophical Transactions of the Royal Society. I couldn't solve such things. It took two youngsters, about twenty-five years old, to do it. I have looked at the problem since 1952.

Now I want to come back to another peculiar aspect of Greek thinking which is now beginning to bear fruit. It will take you into a field which is more controversial at the present moment than any I know of in the whole of the biological sciences including psychology.

The Greek theory of the talk between the male and the female that resulted in the offspring—the colloquy of living things—went over to a theory of communication based on a theory of knowledge. They maintained, and I think they are quite correct in it, that knowledge can only be constituted by some admixture of the knower and the known. They had a theory of knowledge that we call the "cardiocentric theory." They supposed that the great carrier of knowledge was the blood. The blood picked up, in your hands, when you grasped an object, something that came to you from the outside. It mixed with the blood. The mixture is characterized, both in the Hippocratic tradition and in Empedocles writings by the supposition that the voids of the one are filled by the fulls of the other. You have a residual of this in the use of the words, "to know a woman."

You may not realize how recently that theory died. In the 1920's I heard a New York neurologist, who shall be nameless, get up and affirm that he knew that time was in the temporal lobe and space was in the post-central convolution but that he had been unable to localize consciousness. Thereupon, Dandy, the great surgeon of Philadelphia, the first man to take off a whole frontal lobe for tumor of the brain, got up and said, "I'm sorry, I know to my cost! Consciousness is in the left anterior cerebral artery."

Remember that the Greeks knew a good deal about the nerves, in the sense that they knew where they went. They thought the nerves were reins, and our word for them simply means that, which controlled muscles and glands much as one would guide a horse. They thought nerves came largely from the brain and spinal marrow, whose business it was to cool the blood.

It has been a long and slow process to transfer the function of blood, above all, the anastomoses of the blood stream to begin the mixing, from the heart to the brain. We now still think of hormones and a few other such things, like agglutinins, as messengers running around our blood stream, but we have given up our theory of the mixture of substances for the concept of a mixture of signals transported by neurons.

MAKE-UP OF THE TAPE

But knowledge has a way of persisting; and it is about this persistence that I want to talk to you, for it leads to the most controversial affair in all biology at the present moment. Some years ago, we began to get our hooks into that tape the Greeks theorized about. It is called deoxyribose nucleic acid. I think Henry Quastler has finally a very decent attack on the code of such things. He is principal radio-biologist at Brookhaven and I just spent two days with him there reviewing the state of the art. You form a double helix of deoxyribose nucleic acid. It's got proteins situated around it. It exists in the nucleus of the cell. It is this helix of deoxyribose nucleic acid that is the tape. Accidents happened to it, but for the most part it goes on through the process of reproduction. From deoxyribose nucleic acid (DNA) there was derived the ribose nucleic acids (RNA). One set of RNA is made like a set of wheelbarrows to tote around amino acids and stick them into proper places on a second kind of ribose nucleic acid, called messenger ribose nucleic acid. The business of this messenger ribose nucleic acid is to drum onto some little protein structures, wrap itself around them and form a template for the making of proteins.

When you get a toxin into you, and your body goes about making an antibody to match it, it cannot undo the knots tied in the existing protein molecules. Therefore, those cells that are going to make antibodies have to make a lot more fresh protein. When the balance of proteins is disturbed by lack of concentration of this, that or the other deoxyribose nucleic acid the system starts to make a lot of messenger RNA. You can stain for it, and so estimate the quantity. This messenger RNA then makes fresh protein molecules which are wrapped the right way to fit around the protein or other foreign substance that is attacking those cells. This is "to know" in the old Greek sense. This template is known to be inherited at least four or five generations by individual cells.

Now, you have a second process. You call it memory. In this second process period, Hydën was the first to suspect that the nucleic acids were important, and he developed ways of measuring, very precisely, the exact quantity of ribose nucleic acid in cells. By combining his biological procedures with beautiful chemistry, he found that when animals were learning, those parts of the cortex which were involved in the learning had some cells in them—and it is only a statistical distribution—with much more ribose nucleic acid than is normally present.

Hydën's work was the beginning of one of the craziest sets of experiments proving and disproving the importance of ribose nucleic acid in learning.