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CODEN- ASCFC
Scientific Research and Social Responsibility

Self-Regulation or External Control

In recent years, there has been a plethora of lectures, articles, editorials and books, by scientists as well as laymen, on the general theme that science must be made more responsive to society's needs and aspirations. In part this has been due to the growing trend of censure, or external control, being imposed upon science by non-scientists through legal and economic pressures. To those engaged in scientific research, such action is anathema, quickly categorized as an example of J.W. Forrester's counter-intuitive behavior of social systems. There is, however, even among the most vocal protagonists of social responsibility from the scientific community, a patent lack of intensive, non-vocal effort to motivate scientists to become involved in the decision-making process—at all levels.

A lack of social responsibility on the part of the scientific community does indeed exist. Unsafe engineering design, in products ranging from children's toys to automobiles and nuclear reactors, is quite visible to the public. Ineffective drugs, placebos and sale of dangerous chemicals have only increased the public's wariness of the "technological fix." The Kefauver–Harris amendment, precipitated by the Thalidomide tragedy, went so far as to reverse a fundamental principle of American jurisprudence by stating, in effect, that the pharmaceutical industry—and, with it, its scientists—is guilty until proven innocent. With the invention of death control before birth control, people find that longer life, without more food and more thorough training to cope with technological advances, only means more humans living longer on the edge of destitution.

There is a growing reaction for the censure of basic research even from within the scientific community. The emotionally charged attempts of the anti-abortion movement to legally suppress all future fetal research are not hindered by Nobel laureate J.D. Watson's testimony, before the House Science Subcommittee, suggesting that research on in-vitro fertilized ova be made illegal. The current War on Cancer in fact, if not by intent, is seriously affecting, if not suppressing, all non-targeted basic biomedical research not specifically related to "cancer."

One of the manifestations of the social irresponsibility of science is the prevalence of erroneous problem solving—the "cybernetic sacrilege" of viewing complex systems with superficial oversimplification. A fundamental element of any problem-solving/decision-making process is determining whether the observed phenomenon is an "isolated" state of superficial cause or a manifestation of some subtle, underlying source. Many current decisions treat our complex problems as states instead of symptoms. The implementation of these ineffective solutions is abetted by the lack of involvement of the scientific community and the ignorance of the general public.

The problem of science and social responsibility can hardly be viewed as a state—it affects society and is affected by social reaction. We have just reviewed three of the most obvious symptoms. What is the source of these symptoms and are there any solutions? Since these are rather formidable questions to pose, let us consider a more narrow version. The solution of this more restricted problem may be one of the solutions of the question originally posed.
To date, the majority of debates on the social relevance of scientific research—and who should control its direction—have centered on the value of basic (non-targeted) research. Proponents of such research argue, dogmatically, that it is the foundation of science and the vehicle of progress. Its critics reply that since targeted research is obviously more relevant to current problems, it is obviously more important. The proponents of basic research retort that diminishing, instead of expanding, current basic research efforts, we will create a vacuum (of knowledge) in the future.

Rather than embark on a discourse of the cultish mystique of creativity and scientific discovery, let us ask instead: “What is the source of this prejudice?” One answer might be socialization with their peers, or even a sense of professional self-preservation. But we believe the most fundamental source is training (or the lack thereof) of the public and its servants as to the realities—and fallibility—of science and the scientific method.

If we agree that the quality and character of education is the underlying source of the problem of external (and ignorant) control of scientific research, then what solutions can we formulate to rectify the situation? Certainly the obvious stop-gap solutions have been proposed. Some of these include providing scientific advisors to the current decision makers, having scientifically trained politicians, and forming politically active scientific societies. These are all reasonable suggestions, but they may be difficult to implement. Scientists and engineers seem very reluctant to enter the political arena or include in their professional societies committees that can act as lobby groups. Consider, therefore, the possibility of mass education at all levels.

If the general public were made aware of the potentials and limitations of scientific research, then it is possible that an appreciation of science through understanding, not faith, would yield a society more tolerant of the faults of modern science and technology. Such a society, while still demanding social relevance and solutions to current problems, might also realize the need for non-targeted research. With a better understanding of the limitations of engineering design and pharmaceutical research, the consumer would be more cautious and less easily fooled. This might prompt manufacturers to be more critical of the quality of their products (due to reduced sales?) and social responsibility, by a somewhat devious means, might begin to appear. By the same logic, a better educated society would begin to recognize erroneous problem solving more readily and demand more effective short-term and long-term solutions.

To implement such a program, at all levels, would require a concerted effort on the part of the scientific community to motivate the public to learn about science. This may be easier than it seems at first. The endeavors of science and scientists, properly presented, can be interesting and even glamorous. With the availability of the mass media, such vehicles as prime-time television programs and newspaper articles can be effectively utilized. If these efforts are not corrupted or diluted, but actually show both the limitations and potentials of scientific research, the short-term goal of informing the adult population may be achieved.

What may be more important to both science and society is the implementation of a long-term effort to educate our children at the secondary or even elementary school level. With proper implementation and motivation, a future society can be better informed and better able to cope with technological advances. Scientific research and technological advances will be encouraged and promoted not only because of their intrinsic value but also because more of these children will become scientists.

In retrospect, we might say the lack of social responsibility attributed to science is not due to the fact that it produces socially irrelevant knowledge, but merely that it has failed to properly disseminate the socially relevant knowledge it produces. If science must be controlled by societal forces it should be controlled by a science-cognizant society.

G. M. Samaras
The Tenth Anniversary of the American Society for Cybernetics is a milestone in its annals. The founding of ASC in 1964 was an event filled with hopes. But in spite of great expectations, only the future could tell whether image and message would find willing acceptance and influence the direction of the regulating forces in complex systems. The past is prologue, nostalgic perhaps, and a learning experience. Many constructive ideas have been crowding in on the Society, some important and exciting. During the first ten years ASC has seen numerous changes within and without. The leadership roster carries venerable names, some now scarcely remembered. The Society has followed the times. The proceedings of the annual symposia have dealt with, analyzed and examined current issues for future trends.

The most recent development, the introduction of the “new” ASC FORUM, breaks with tradition. It has a mission: to promote cybernetics literally and on a broad basis. Dialogues are to be encouraged and, even as in this issue, space will be provided for controversial subjects. Cybernetics is still an enigma of sorts. It is to be featured as an important tool for decision making in complex systems.

Beginning with this issue, the ASC FORUM will be jointly managed by the American Society for Cybernetics and the American Society for Information Science, each organization bringing to this undertaking its special expertise. This new partnership promises that the goal will be fully achieved. We propose to develop the ASC FORUM into an outstanding publication in cybernetics. The Board of Editors (all members have promised their active cooperation) has been greatly expanded. The most important innovation is the composition of the new Board, the link to universities, research institutes and institutions of higher learning. The marketing organization of ASIS, responsible for distribution, assures that the ASC FORUM will become widely known throughout the professional communities in the United States and abroad.

A second development, which may have a slower impact on the build-up of cybernetics as a tool for decision making, is the formation of an educational committee which has been charged with the responsibility for designing a comprehensive curriculum in cybernetics and general systems. While the details have not yet been worked out in toto, the first announcements referring to two- and three-day seminars in specific fields related to cybernetics and information science will be sent out shortly. The essential part of this program is the tie-in with many of the leading universities and institutions of higher learning throughout the United States. Specific bridgeheads are being established in academia to assure that developments in the educational field will be acceptable to, followed up, and applied by, these institutions, many of which have greatly neglected cybernetics.

Back to the ASC FORUM: The new format is the result of much planning and deliberation before the decision was reached to provide more space without causing a conflict with the Journal of Cybernetics. The task is clear. What will count from now on is that this quarterly will become the arena for spiritual fights to clarify complex issues in a way that makes it dynamic, instructive and good reading material. The substance must be of high quality. In addition, we will continue to supply newsworthy information on members and events.

For a short while we had been thinking of publishing, as one of the first items, a short history of ASC. But who could have resisted the alternative and not let Warren S. McCulloch speak instead? We are grateful to his wife, Rook McCulloch, to let us have his autobiographical notes, which are a gem and a delight to read. This is the most fitting contribution to our “new” FORUM.
WARREN STURGIS MCCULLOCH
1898 – 1969
Recollections of the Many Sources of Cybernetics*

Warren S. McCulloch

Introduction

Norbert Wiener—five years my senior—wrote elegantly of "Control and Communication in the Animal and the Machine" as he saw it coming, and christened it "Cybernetics." For years his scientific activities had been leading to it. He had been unable to pursue experimental biology because of nearsightedness and manual clumsiness, yet he never lost interest in physiology. He had trained as a philosopher, with a bent for symbolic logic, but few posts for logicians were available. In World War I, after an engineering apprenticeship for General Electric at Lynn, he was at last enlisted and joined the staff of Mathematicians and Engineers working on ballistics at the Aberdeen Proving Grounds. He had already become a mathematician with a background in biology, engineering, philosophy and logic when I, as a second class seaman, was engaged in marlin-spike seamanship and semaphore. To me these were topology and communication. What background I then had was from an early interest in theology and a few courses in geometry, algebra, theory of numbers and synthetic projective geometry, plus a thorough working knowledge of spherical trigonometry that I had picked up from old whaling captains. Whereas Wiener emerged from the war an ardent student of mathematics relevant to the real world of physics, engineering and biology, I plunged into the epistemic problems of mathematics that had captured my fancy as a freshman at Haverford College. I remember well that when I told our Quaker philosopher, Rufus Jones, that all I wanted to know was: "What is a number that a man may know it; and a man, that he may know a number?" he prophesied, "Friend, thee will be busy as long as thee lives." He was right. Though I did not know it then, I had become a cyberneticist.

My Training in Epistemology

Since I was in the officers' training school at Yale when my active duty terminated, I remained there, majoring in philosophy and minoring in psychology. After the hard work of Haverford, Yale presented few academic difficulties and I always had time for extra courses. What struck me even then was that one almost always learns important things from the wrong professors: it was in economics, for instance, for which I read Das Kapital, that I acquired both adequate German and the right slant for understanding Hegel. After that I read Kant's Critique of Pure Reason in German. In Woodruff's course in biology I learned how to read and respect Aristotle—unfortunately only in translation. I learned the great epistemic problem of relativity from Boltwood, who taught us theoretical chemistry. Through his knowledge of living things I learned to understand his logic and hence his metaphysics. The "mental" and "material" axes merely intersected in the substance—the This One. From then on Mechanism and Teleology held no contradiction. They have in common the dialectical argument with its logic of becoming which is necessary for cybernetics, and they complement each other from the sides of matter, or body, and form, or mind.

It is Kant's conception of the synthetic a priori and his conviction that this depended upon the physical, the anatomy and physiology of the body, that, coming through Helmholtz became the Leitmotiv of Rudolf Magnus, perhaps most explicitly in his Linnaean Lecture on "The Physiological A Priori." Magnus' student, J.G. Dusser de Barenne, was my teacher.

The main theme of the work of my group in neurophysiology in the Research Laboratory of Electronics at the Massachusetts Institute of Technology has been in this tradition, namely, experimental epistemology, attempting to understand the physiological foundation of perception. It should never be forgotten that Kant told the great neuroanatomist, Semmeling, that the cerebro-spinal fluid could not be the sensorium commune.

Returning to Boltwood, he was very clear about the bearing of relativity, not only on our physical frames of reference, in the sense of space, time and movement, but also on any other set of axes appropriate to an observer coping by measurement and perception with his own changes in a changing world. In particular, he dealt with olfaction. The nose of a chemist working with an ester must be able to identify both halves of the salt, the organic base and the organic acid. By sniffing, say, for the acid alone, he can alter his perception of the salt, and in this way analyze the compound. Mixtures of compounds are more difficult. But the rate of olfactory adaptation and its persistence

*The late Warren McCulloch wrote this paper in 1969. We are pleased to have this opportunity to print these personal notes toward an autobiography at this time.
differ for both the particular radicals and their concentrations.

Boltwood had earned his Ph.D. for separating the isotopes of lead, and so was well aware of the revolution in physics when the billiard balls of antiquity turned out to be almost empty space with particles floating about in regular orbits. He had followed every revolution in physics since 1908 and expected more; notably that man would learn to split the unsplittable—the atom—and that it, or some isotopes, would explode in ways that might trigger others, and so explain the stars.

Complaining to me of his increasing forgetfulness, Boltwood asked me to spend one evening (5:00 p.m. to 5:00 a.m.), with him while he outlined for me the history of science. I had already had a year of study of it, but it had never before come together in my mind with that organization and that selection of the requisite diverse items. He began with guesses as to what the Greeks could have inherited from Egyptians, Mesopotamians, and Phoenicians, whom he called “Semitic sailors.” Then he rapped through the pre-Socratics with a general thesis phrased, “They were not afraid to think.” I cannot recover the details, but the general thesis was clear and stuck. If you want to get behind the fake explanation of how our world comes to be as it is, then you must find the facts for yourself and accept nothing in addition but Logic and Mathematics. Then he said: “Skip the Socrates and sociology.” From the Schoolmen I knew better than he the Scholastic period, and kept my mouth shut.

He began again with Galileo Galilei, who split natural science down the middle by refusing to admit mind, or anima, as an explanation of physical events. We agreed that this was the way to make physics, but that it left the mind—axima—a sort of self-sufficient item capable of wandering around the world and having ideas, even perceptions, of a world devoid of anima. He thought this was a great triumph, for it produced physics (Galileo), astronomy (Kepler), and celestial mechanics (Newton). But...and after a long time: “When I was isolating the isotopes at night, I spent the day training pairs of cats to climb ladders fastened at the top; and, with gloves on their front paws, to box for the top rungs. If you want to train animals, you can succeed by giving them permission to do what they like to do. Remember that students are animals with curiosity and a willingness to help, so the best way to teach them to become scientists is to tackle a tough problem with them and make mistakes!”

One other thing that I learned from Boltwood concerns holistic arguments at which our professors of science were then scoffing. He pointed out that one has an electromotive force (B.M.F.) only in a closed loop of three components, and any substitution of another metal for one of these creates two new junctions but only one new measurement. Hence a single junction potential is not measurable because it is not thinkable.

Our psychologists had gone I.Q.-ing and become statistical. When I doubted the appropriateness of their ways I was told to read C.S. Peirce who had found that the normal distribution curve fitted his vast collection of data. Peirce thought it did, but many years later E.B. Wilson gave me his analysis of the variance of Peirce’s data. The curve, like most real ones, was too flat on the top, too steep on the sides, and too high in the tails.

Anymore, I had begun to read Peirce. Then I turned to Russell and Whitehead, (Principia Mathematica)—who in the calculus of atomic propositions were seeking for the least event that could be true and false. Leibnitz’s problem of the petit perception had become the psychophysical problem of the just noticeable difference, called JND, which has since been found in the middle range of perception to be roughly proportional to the size of the stimulus. Let us suppose that in estimating the weight of loaded pillboxes we had a JND of two per cent. Suppose now we had pillboxes arranged in ascending order increasing by one per cent of their weights. Then each even pillbox differs by the JND, or 2%, from its even neighbor and the odds differ by the same amount from their odd neighbors, but none are discernible from their immediate neighbors in the whole series. Clearly the subject is in receipt of signals such that when they sum he perceives the differences.

In searching for these unit signals of perception I came on the work of Rene Descartes, fortunately the complete work, which contained his conclusions of eight years of dissection in Leiden. In the translations of Descartes we used at Yale none of this appeared. For him the least true or false event would have been his postulated hydraulic pulse in a single tube, now called an axon, true if it was properly excited, and false if it arose ectopically. He thought that nerves were composed of parallel tubes too fine for him to see them individually even under his magnifying glass. Each tube was filled with liquid in which pulses of hydraulic pressure went from brain and spinal marrow to muscles, causing them to contract; and each tube had a fine thread in it which, as the muscle contracted, signaled back to the control nervous system to close down the valve. As far as I know, this is the first use of the notion of inverse feedback, and so of the reflex. Finally, Descartes had the notion that there were enough tubes in parallel to transpose the images of the eyes into the brain, but he did not believe that there could be enough tubes in parallel to convey the picture to the central valves; so from there on he conceived that it had to be conveyed by temporal sequences of pulses which need no more resemble the picture they describe than our words resemble the thing they describe. This is probably the first coding theorem, and to one familiar with the Morse Code this was particularly appealing!
What I learned of Descartes in philosophy courses was that he was somewhat impious in saying we had to get straight our ideas of God if we wanted to make good machines, and that he was obviously a poor metaphysician because he had left the mind as a spectator running up and down the side lines. That was exactly what Galileo had done to create physics. Descartes' automaton was good physics. As for me, I do not want my formal and final "causes" to be efficient or material "causes," but I want all four to make sense of living things and machines that simulate them; from all of which I conclude that cybernetics really starts with Descartes rather than with Leibnitz. That it is only in recent years that I have been able to understand Leibnitz will become clear when we come to computers.

The best course in philosophy I had at Yale covered Locke, Berkeley and Hume. Because I had inherited the copy of Locke's Concerning Human Understanding that had persuaded my great-grandfather Bradley to free all of his slaves, I had read the text and was heartily disappointed. To me it seemed thoroughly confused. With my Scholastic background he seemed never to distinguish essence from existence. So I was delighted when I came on Newton acceding Locke of mistaking ideas for common notions—and he might have added: confusing both with their notation. These distinctions are crucial in cybernetics. Among communication engineers one is apt to fall into their jargon and refer to a given circuit, hardware in a box, as the "logic"; and to speak of a "number in memory," meaning the magnetic state of certain ferries. It is like speaking of a gramophone disk of printed score as Beethoven's Fifth Symphony. These are capable of being in places—one or many at a time—whereas the symphony, as essence, is certainly not. The other distinction, between a sphere, i.e., the locus of all points equidistant from a given point, and a ball, however nearly round, is, I think, nearer to the gist of Plato's myth of the cave of the sun, for the sphere belongs to the pure mathematician, the ball to the physicist, the engineer and the biologist.

I learned this chiefly from Jay Hambidge of the Yale Art School, who was measuring Greek vases looking for the "root rectangles." Plato had said that, of all forms that are divided into three equal rectangles that one is most beautiful whose altitude is to its base as one to one. Early Greek vases began with two to one and two equal parts, then went on to two to one, then to three to one, and finally to five. This, the Golden Section, made by dividing a line into two parts such that the lesser is to the greater as the greater is to their sum, defines a rectangle with which, by using a carpenter's square and a compass, one can get a "whirling square," which, rounded off, is a good approximation to an equiangular spiral of which construction Plato says scornfully that it is not mathematics. He would probably have said the same of ratios of successive Fibonacci numbers tending to the Golden Section.

The scales of a pine cone develop in right and left hand sets of spirals and the number of right hand spirals of scales is always prime to the number of left hand spirals. Hambidge started me counting the spirals of pine cones. The numbers are ordinarily as 3 to 5, 5 to 8, 8 to 13, 13 to 21, and some large ones 21 to 34. I am told that there are some with 34 to 55. Certainly no one ever laid out for a pine cone how it should grow. In 1911, D'Arcy Thompson had published the answer. Given that the growing central stalk has two branches at nearly the same level, and not precisely opposite, spiralling up the stem at any pitch and velocity, then when one counts down to the lowest recognizable spiral the rights and lefts are necessarily Fibonacci. This, then, is a self-organizing system that with the absolute minimum of information produces the pine cone. Please note that it has strictly no symmetry of rotation, for it must be turned 360° to coincide with itself. Clearly it needed one thing more to account for its digitalized form. Hambidge started me counting then, about 1920-1921, and I continued to count pine cones until 1964. Since a pine cone has three to five spirals one way and eight times the other. So you call yourself an idiot for you have counted the number up this way must be the same as the number down the other multiplied by the reciprocal of eight/nine times (say, 5/8, i.e.; by 8/5). Well, it just is not so. Finally you yourself an idiot for you have counted the top five times one way and eight times the other. So the difference is 3; only it is not! Then you turn the cone over and start from the top and find the second equal number simple consequence of the second least constraint.

That chance and the nature of numbers account for the self-organization of such a growing system nudges the cyberneticist toward Peirce's notion that given a stochastic world, order will evolve. It will become reasonable. My difficulty was that it took me from 1920 to 1964 to learn to count. If, as you count down the pine cone, you mark each spiral that you count on the right hand side, and on the left green, you will find out what you are really doing.

To return briefly to Berkeley; I easily understood his text as a sort of stretching of the epistemology of relativistic physics to mundane perceptions, (F.S.C. Northrop just told me Einstein had thought so!) but it left me incredulous. My classmates never understood Berkeley and I know my professor was profoundly puzzled. To think of the Holy Ghost as tensor invariance has the flavor of Lewis Carroll's Alice climbing through the looking glass onto a new leaf of space which is a chessboard, upon which the pieces are alive with a memory that works both ways.

That two-way memory Hume lacked. He had a succession of perceptions but no perception of succession. He was about the same age when he wrote his Tractatus as I was when I read it, and being a
fellow Scot I could fairly see the wheels going around in his head. What Duns Scotus had called "a firm proposition residing in the soul," Hume called "a habit of the mind." And, whereas Scotus distinguished carefully between *Causa causalis*, or bound cause, and *Causa casualis*, or accidental cause, Hume makes no distinction. The biological purport of the bound cause is the law of the conservation of species; like begets like. Without causality the primary properties of physical things went the way of their secondary qualities, but strangely enough, logic and arithmetic remained and allowed him to define equal numbers: "When each has a unit corresponding to a unit of the other we pronounce them equal," and he continues, "and it is for the want of such a measure that geometry can scarce be considered an exact and perfect science." Hume had already seen that with number one could compute precisely forever. I had begun to find my way toward the nature of number, but I went the wrong way.

In 1920 I began to attempt to construct a logic for transitive verbs of action and contra-transitive verbs of perception, etc. I continued it through many pitfalls until February 1923, when I came under the influence of T.H. Morgan who talked to me much and earnestly about genes and inheritance. He was disturbed by having to consider a gene as a physical thing on a given chromosome, which in any case is a very specific hunk of matter, and, at the same time, having to think of a gene as a determinant of a set of properties in a homogeneous population. Just as two men can be of one mind, so identical twins are of one genetic complex. Here was a least message that could be written many places and read out differently in different context. Just as hereditary information could flow through generations, so could sensory information flow through ranks of neurons. This was certainly the right model. Both nets are anastomotic; that is, information from every input may be effective at every output, and that by many paths.

I was on my slow way. In 1923, I took my Master's degree in psychology with a thesis on which I had started out of incredulity of Hambidge's assertion that root rectangles and the Golden Section are aesthetically preferred by most people. He was right. It does not take long to train a man to the limit of his capacity in judging lengths, areas, and volumes. If his JND is two per cent in length, it is two per cent in area and two per cent in volume. Therefore he cannot be making estimates of areas or volumes on the basis of length, for he would be off by a factor of two and three times as much. Hambidge had said that even so his knowledge of shape was well-nigh perfect. Using black cards with white lines, I started to test this, not by asking: "Which of these shapes have such and such properties?" or: "Set this card to such and such a ratio," but simply: "Which shape do you like better of these two?" and: "Set this card to any shape you like." Trained observers, architects, sculptors, painters, came rapidly to the root rectangles and the Golden Section. People unused to making esthetic judgments came more slowly. But what astounded me and made me keep on testing long after I had turned in my thesis was this, that a man with a JND of two per cent would set the card correctly to the third decimal point, so that I had to read it with a vernier. Hambidge was correct! Man does live in a world of relations.

My Acquisition of the Experimental Techniques for a True Epistemology

During those years in psychology I had come to know Woodworth very well and had acquired a healthy interest in physiological psychology and learned to appreciate Gardner Murphy, from whom I learned the history of all the schools of psychology from the pre-Socrates to behaviorists. The rest of my time I had spent in mathematics, physics, chemistry and neuroanatomy.

The next fall I entered medical school with the avowed intent of learning enough physiology of man to understand how brains work. The four years at the College of Physicians and Surgeons was a hard grind, but I was repaid by excellent and enthusiastic teachers in neuroanatomy, comparative neuroanatomy, neuropathology and neuropsychology. Everything else seemed secondary to the function of the master tissue! When I began to see patients, it was injuries of peripheral nerves, of spine, and skull that intrigued me. Neurology and neuropsychiatry delighted and enthralled me. I had intended to go back to the laboratory as soon as I graduated, but I joined Foster Kennedy's staff at Bellevue, first as intern and then as resident on his neurological service, which included neuurosurgery. Those were the years 1927, 1928 and 1929. But, in spite of the busy life of the intern, I was forever studying anything that might lead me to a theory of nervous function. My fellow intern, Samuel Bernard Wortis, accused me of trying to write an equation for the workings of the brain. I am still trying to!

This was the period of post-encephalitic Parkinsonism, and our commonest cases had pill-rolling tremors. Here was a problem of activity in a closed loop coming out of the ventral root of the spinal cord, returning from the contracting muscle over the dorsal root, and so out again to the same muscle. From what little we knew of the delays in the spine and of the conduction times, it was difficult to say whether a tremor of, say, three to five per second was par for the course.

The question that Sam and I discussed most often was whether reverberation in this loop was a vicious circle or whether something central was reverberating at that frequency and sending out signals for contraction from one part of the loop. He also had many
epileptic patients, some due to injury, infection, and chemical insult to the brain, and others of familial types. Here we wondered whether there was not in them a vicious circle within the brain. We even wondered whether curarizing the patient might not stop not only his thrashing about, but also the central nervous activity concomitant with it. Yet I doubt whether any of us in those days thought of reverberant activity in brains either as intrinsic to them or as a normal process. No anatomist had produced convincing pictures of loops closed within the central nervous system, though Ramón y Cajal had suggested them. No spontaneous activity of neurons was known and no activity that was regenerative over a closed path.

The first theoretical paper on this score appeared in Brain in July of the following year, 1930, entitled “A Theoretical Application to Some Neurological Problems of the Properties of Excitation Waves Which Move In Closed Circuitry,” by Lawrence S. Kubie. (Unfortunately, I did not come on it then. Nearly forty years later, it reads as if it were written today.) Some months or so later, S.W. Ranson had to postulate closed loops for other purposes. Alexander Forbes told Birdie Renshaw and me that he did believe in delay chains but not in reverberating chains, and gave us the references to those papers, but that was years later.

Kubie, like the rest of us, was very familiar with the reflex, as defined about 1819 by Magendie, as a process begun in some part of the body initiating impulses that proceed over dorsal roots to the spinal cord whence they are reflected over ventral roots to the part where they arose and there stop or reverse the process that gives rise to them. I had even struggled through a muddy German outline of Sechenov’s Reflexes of the Brain, but nowhere had I found Kubie’s papers that today sound so modern.

Here indeed were some early sources of cybernetics.

The year 1929 was important to me in two ways: my Yale classmate, C.H. Prescott, working at the Bell Telephone Laboratories, introduced me to an older mathematician, R. Hartley, who was trying to quantify the amount of information that could be transmitted over a noisy line, and it was he who gave me a reference to the definition of information by C.S. Peirce as a third kind of quantity, being the disjunction of all of those statements in which the term in question was subject or predicate, antecedent or consequent. This was the bud of the American definition of information as a quantity. It flowered in Shannon’s Mathematical Theory of Communication in 1948. Shannon’s definition rests on an ensemble of possible messages in a language known to sender and receiver, and is a measure of the improbability of the message, its “surprise value,” written \(-\Sigma p \log p\), which resembles negative entropy, but lacks the dimensional constant for energy.

The second of those surprising ways of looking at information in 1931 came one evening when one of our friends translated to a group of us a paper of Goedel’s which had just appeared. It was the one on the arithmetization of logic.

In the years 1929-1931 I went on with neurophysiology and studied mathematics and mathematical physics, with little direct relation to cybernetics, but I taught physiological psychology at Seth Low Junior College in Brooklyn, New York, where I explored my theory of information flowing through ranks of neurons very successfully, until I tried to close the loop. Not then having the notion of delay, I thought: “One cannot close the loop, one cannot be one’s own ancestor!” I knew I needed more intellectual tools, but not what they were.

Then, in the depth of the Depression, I went to Rockland State Hospital to earn money, but fortunately there I met Eilhard von Domarus. He was a superb clinician and a well trained philosopher. I learned much of both from him, and in return I helped him rewrite in English his Ph.D. Thesis On the Philosophic Foundations of Psychology and Psychiatry, which he had written in an incredible mixture of German, English, Latin and Greek for my old friend Professor F.S.C. Northrop of Yale. It took over a year of nightly sessions to do it, but I came away with a real understanding of what was important for me in German philosophy and German neuropsychiatry, without which I would never have come to a definition of thinking that fits cybernetics. His analysis of the abuses of the word “consciousness” in our language has helped me out of many an altercation. The important meaning in forensic medicine is a triadic relation: A is conscious of B with C if A and C can both bear witness as to B. This cannot be sought in any one man, whereas Peter Abelard’s “conscientia,” things thought about together, and so the idea of ideas, can be embodied in a single brain and, today, in a digital computer. Domarus was thirty years ahead of his world.

I went next to Yale to work with Dusser de Barenne.

The ensuing years at Yale were filled with scientifically enlightening discoveries, so much so that Dusser would slam me on the back and shout: “We discover too much!” He was the only student of Rudolf Magnus who brought on the great tradition from Helmholtz and Kant; he was a psychiatrist whose problem was sensation and perception and their embodiments in activities of the nervous system. He was the inventor of the method of local strychninization of the brain, which made it possible to identify the central structures whose chemical excitation elicited all the clinical signs of hyperalgasia and hyperaesthesia and paralgasia, which we neurologists know as behavioral signs, not merely as symptoms or complaints. By combining these studies with electrical recordings, we were able to map the directed pathways of the regions of the cortex to other structures and other regions of the cortex, producing maps of circuit action from which the wiring diagram could be
deduced. These pathways are even today being slowly confirmed by the meticulous methods of neuroanatomy. This is but one of the many things that went well as science, but of importance for cybernetics was its setting in the flesh. Its setting in the blood came from measurements of pH and similar items, usually with the help of Leslie Nims, Physical Chemist at Yale. For me it proved that brains do not secrete thought as the liver secretes bile, but that they compute thoughts the way computing machines calculate numbers.

At that time I found of all Whitehead's philosophy his analysis of the percipient event most helpful, and it pushed me into studying the *Monadology* of Leibnitz. When one is working on the physics and chemistry of the anesthetized brain, as I was, one is doing biophysics and biochemistry necessary for neurophysiology, but falling short of physiology because the nervous system is then deprived of its functions; but even if it were working properly it would still be only physics and chemistry and not physiology unless one were studying the function also. Here the seventeenth paragraph of the *Monadology* begins:

Moreover, it must be confessed that perception and that which depends upon it are inexplicable on mechanical grounds, that is to say, by means of figures and motions. And supposing there were a machine, so constructed as to think, feel, and have perception, it might be conceived as increased in size, while keeping the same proportions, so that one might go into it as into a mill. That being so, we should, on examining its interior, find only parts which work one upon another, and never anything by which to explain a perception.

Two of Leibnitz's machines that add, subtract, multiply and divide we know to exist: one is in Munich, the other (or an early copy of it) I have seen in the Museum of Scientific Instruments in Florence. The action is by gears with which Leibnitz was not entirely satisfied. It is interesting that, except for the abacus, logical machines of computation preceded discrete numerical machines, which we call digital, by so many centuries. Leibnitz wanted a truly discrete step, for reasons Hume later specified, more in keeping with the tradition of the improbable Ramon Llull whom Leibnitz admired. Llull (1235-1315) had used, as symbols, two intersecting circles for two arguments, Venn pushed it to six, but his closed forms are no longer circles. Lewis Carroll pictured six by using a 4 x 4 square with a diagonal in each direction in every square, Minsky and Selfridge have shown how by sine waves doubling in frequency and diminishing in amplitude one can extend these forms indefinitely. C.S. Peirce had broken down the Lullian circles into a single chiastic (X) symbol, the left for the first argument, the right for the second, above for both, below for neither, but the editor did not see fit to include them in the collected works, although Peirce had asked that they be used for his amplex, *i.e., not both and neither nor,* which Shafer had to rediscover for himself many years later. I made the chiastic symbols for myself in 1941 to teach logic. Any of these symbolisms can be used to signify to what Boolean functions of its inputs a given device will respond. They proved most useful in visualizing probabilistic functions and representing so-called "don't care conditions."

The Anastomosis of Cybernetics

In the fall of 1941 I went to Chicago to build up a team of specialists to lay the biological foundations for the Department of Psychiatry of the University of Illinois. This was to be primarily neuroanatomy and neurophysiology and whatever physics and chemistry I thought appropriate. We were not going to work on behavioral problems that were being handled well in several other places. As neurophysiologists we were concerned with the chemistry of the brain. Because the ratio of O₂ consumed to CO₂ produced by the brain is nearly one, and because many psychiatric patients were known to be resistant to insulin, we worked on the regulation of carbohydrate metabolism in health and disease.

I was fortunate in our electrical engineer, Craig Goodwin, who was excellent in the theory and design of regulatory devices.

He called my attention to Clark Maxwell's famous paper "On Governors," and to H.S. Black's paper on "Stabilized Feedback Amplifiers" of 1934. He helped me think through all of the diseases of the systems with inverse feedback. I understood automatic volume controls and self-tuning devices. Above all, I learned from him that when the mathematics of our hardware, of nonlinear oscillators and their couplings, was beyond us, we could still *build* a working model and think in terms of it. Moreover, with him I learned to think statistically about brain waves.

In 1941 I presented my notions on the flow of information through ranks of neurons to Rashevsky's seminar in the Committee on Mathematical Biology of the University of Chicago and met Walter Pitts, who then was about seventeen years old. He was working on a mathematical theory of learning and I was much impressed. He was interested in problems of circularity, how to handle regenerative nervous activity in closed loops.

I had to suppose such loops to account for epileptic activity of surgically isolated brain and even of undercut cortex. Lorente de Nó had shown their significance in vestibular nystagmus. I wanted them to account for causalgia persisting after amputation of a painful limb and even after section of the spinothalamic tract; I wanted them to account for the early stages of memory and conditioning. I wanted them to account for compulsive behavior, for anxiety and for the effects of shock therapy. These appeared to be processes that once started seemed to run on in various
ways. Since there obviously were negative feedbacks within the brain, why not regenerative ones? For two years Walter and I worked on these problems whose solution depended upon modular mathematics of which I knew nothing, but Walter did. We needed a rigorous terminology and Walter had it from Carnap, with whom he had been studying. We, I should say Walter Pitts, finally got it in proper form and we published in 1943, A Logical Calculus of the Ideas Immanente in Nervous Activity. H.D. Landahl immediately joined us in a note applying the logical calculus statistically. The crucial third part of our first article is rigorous but opaque and there is an error in subscript. In substance what it proved via its three theorems is that a net made of threshold devices, formal neurons, can compute those and only those numbers that a Turing machine can compute with a finite tape.

The formal neurons were deliberately as impoverished as possible. Real neurons can not only compute any Boolean function of their inputs, but many others. Threshold devices can compute only those functions which, in a Birkhoff lattice, or N-dimensional cube, can be separated by a hyperplane. With two arguments this is fourteen out of sixteen, only the exclusive or and if and only if are missing. With three arguments only one hundred four out of two hundred fifty-six are what are called “threshold computables.” The problem is one of counting the number of planes, and so again I learned to count. We need a Euler to solve it in general. The threshold functions have been computed by Robert Winder for seven arguments, and he estimates the fraction of all functions diminish for large N more rapidly then $2^{2N}$.

I am tired of hearing of “the Pitts and McCulloch neuron,” but it was a fair description of available hardware and this is one reason why reliable nets of unreliable components are beyond our technology to date.

Fortunately for our calculus, von Neumann used our article in teaching the general theory of digital computers and it gave rise to the algebraic theory of finite automata. It formed the basis on which I solved the Hierarchy of Values Determined by the Topology of Nervous Nets, and, again with Walter Pitts, of How We Know Universals.

In 1943, Kenneth Craik published his little book called The Nature of Explanation, which I read five times before I realized why Einstein said it was a great book. Of his life I know only secondhand from Professor Drevor of Edinburgh and from Sir Frederick Bartlett and Lord Adrian, all of whom considered him a genius. Thanks to their interest, Dr. Stephen L. Sherwood was entrusted with Craik’s residual writings which we studied and organized and Sherwood edited to form a second little book The Nature of Psychology. Unfortunately for science, Craik was struck by a truck and died at the age of thirty-two, but his work has changed the course of British physiological psychology. It is close to cybernetics. Craik thought of human memory as a model of the world with us in it, which we update every tenth of a second for position, every two tenths for velocity, and every three tenths for acceleration as long as we are awake. Shortly after the War, there formed in London a group of young scientists, each with two sciences, who thought at one time of calling themselves by his name, but finally became the Ratio Club. From this group and their friends came Ashby’s pioneering Design for a Brain: Grey Walter’s tortoise; and the work of Albert Utley at the National Physical Laboratory, Teddington, England, on perceptive machinery on a probabilistic basis.

But I want to emphasize Donald MacKay, now Professor of Communication at the University of Keele, who, with Dennis Gabor and Colin Cherry, initiated the English School of Information theory which is as appropriate to some scientific and biological problems as the Shannon measure of the American School is to communication. In a communication code the price of a simple BIT, or answer to a yes or no question, remains constant even when we halve the area of ignorance by every question, as we can in playing Twenty Questions. In making measurements, the price varies: the range of uncertainty diminishes inversely as the square root of the number of samples we average. For this and other reasons the English speak of “amount of information” as a vector whose length and direction represent complementary measures of information-content, whereas the Americans speak of information as a quantity, Peirce’s third kind of quantity. It is what you get with one meter per logon.

Before I turn away from England there are two others, not guilty of the solemnity of the square hat. I doubt if Stafford Beer has any degree, though I happen to know he has refused several professorships, preferring to stay in business and make cybernetics pay himself and industry handsomely. He knows how to pull able people to him and how to defend them. He has the humor, the diction and the prose of G.K. Chesterton whom I knew in New York City in the early twenties. I met Stafford when he was modeling industries and factories on computers for British United Steel. That was about half a year after the first international conference in Namur, where he and Gordon Pask gave the outstanding papers. Gordon, after a couple of years toward an M.D. degree, shied off into electrical engineering problems and formed his own company to work on self-organizing systems and on teaching machines in which the machine learns the learner.

But I must return to 1943. I had known of Norbert Wiener’s work on adjustable filters from C.H. Prescott, and of his attempts at biology from Stanley Cobb. I had met Norbert with our mutual friend, Arturo Rosenbluth, who was working with Walter B. Cannon on homeostasis, on the so-called “Porter phenomenon” and on the supersensitivity of denervated structures, all
cybernetic problems, I was amazed at Norbert's exact knowledge, pointed questions and clear thinking in neurophysiology. He talked also of various kinds of computation and was happy with my notion of brains as, to a first guess, digital computers, with the possibility that it was the temporal succession of impulses that might constitute the signal proper. Then Norbert spoke of predictive filters in terms of the response of linear systems to noise and, finally, of almost periodic phenomena that might constitute the signal proper. Then Norbert brought in feedback, and I knew that it was the temporal succession of impulses that it was, probably in the spring of 1941 came Pearl Harbor. It must have been shortly thereafter that Julian Bigelow came to Boston and began to help Norbert Wiener on problems of prediction for gun laying and I know not what else. They worked first on linear prediction, using the method of least squares, and then on nonlinear predictions. Part of these studies were published in a book we called the *Yellow Peril* for its yellow cover and its toruous mathematics.

Norbert told me, and I believe wrote somewhere, that it was Julian who had impressed on him the importance of feedback in guidance. To be more precise, what Julian did contribute most surely was this, that it was not some particular physical thing such as energy or length or voltage, but only information (conveyed by any means) as to the outcome of one's previous act that, for example, a pilot needed to fly a plane to its destination. This I take to be the crucial notion for gun laying and I know not what else. They worked first on linear prediction, using the method of least squares, and then on nonlinear predictions. Part of these studies were published in a book we called the *Yellow Peril* for its yellow cover and its torturous mathematics.

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Be that as it may, Norbert, Arturo and Julian wrote a paper on teleological mechanism, the substance of which was presented at a Josiah Macy Foundation meeting in New York City in 1942. Thereafter Arturo and I conspired with Frank Fremont-Smith to hold a series of interdisciplinary meetings to spread these ideas to those who could use them. Before these could get under way, came the meeting of engineers, physiologists and mathematicians at Princeton in the later winter of 1943-1944, described by Norbert in the introduction to *Cybernetics*. Here I first met Johnny von Neumann.

Winter 1943-1944

Lorente de Nó and I, as physiologists, were asked to consider the second of two hypothetical black boxes that the allies had liberated from the Germans. No one knew what they were supposed to do or how they were to do it. The first box had been opened and exploded. Both had inputs and outputs, so labelled. The question was phrased unforgettably: "This is the enemy's machine. You always have to find out what it does and how it does it. What shall we do?" By the time the question had become that well defined, Norbert was snoring at the top of his lungs and his cigar ashes were falling on his stomach. But when Lorente and I had tried to answer, Norbert rose abruptly and said: "You could of course give it all possible sinoidal frequencies one after the other and record the output, but it would be better to feed it noise—say white noise—you might call this a Rorschach." Before I could challenge his notion of a Rorschach, many engineers' voices broke in. Then, for the first time, I caught the sparkle in Johnny von Neumann's eye. I had never seen him before and I did not know who he was. He read my face like an open book. He knew that a stimulus for man or machine must be shaped to match nearly some of his feature-filters, and that white noise would not do. There followed a wonderful duel. Norbert with an enormous club chasing Johnny, and Johnny with a rapier waltzing around Norbert—at the end of which they went to lunch arm in arm. The later part of this meeting was spent listening to engineering and mathematics appropriate to these problems. We all agreed that there should be an interdisciplinary gathering of this kind more often.

That was in the academic year 1943-1944, and was followed from 1946 os by the ten Macy meetings on circular causal and feedback systems. I had a fairly free hand and excellent advisors in gathering the group. Frank Fremont-Smith and I agreed that it should never exceed 25 regular members including guests, and that we should always have at least two of each kind: two mathematicians, two neurophysiologists, two neuroanatomists, two psychologists, two engineers, two neuropsychiatrists, etc. Every speaker knew there was at least one in the audience who knew his jargon. I agreed to chair, provided Frank would sit next to me and kick my stupid shins. I could count on Margaret Mead's keeping a flowsheet of the discussion in her head and on Walter Pitts' understanding everybody. Even so, working in our shirt sleeves, for days on end at every meeting, morning, lunch, afternoon, cocktails, supper and evening, we were unable to behave in a familiar friendly or even civil manner. The first five meetings were intolerable. Some participants left in tears, never to return. We tried some sessions with and some without recording, but nothing was printable. The smoke, the noise, the smell of battle are not printable. Of our first meeting Norbert wrote that it "was largely devoted to didactic papers by those of us who had been present at the Princeton meeting, and to a general assessment of the importance of the field by all present." In fact it was, characteristically, without any papers, and everyone who tried to speak was challenged again and again for his obscurity. I can still remember Norbert in a loud voice pleading or commanding: "May I finish my sentence?" and hearing his noisy antagonist, who was pointing at me or at Frank, shouting: "Don't stop me when I am inter-
rupting." Margaret Mead records that in the heat of battle she broke a tooth and did not even notice it until after the meeting. We finally learned that every scientist is a layman outside his discipline and that he must be addressed as such. The sixth through the tenth meetings went more calmly and were edited by Hans-Lukas Teuber, Margaret Mead and Heinz von Foerster, and published by the Josiah Macy Foundation, so I need not describe them. I would like to express my perennial gratitude to Frank Fremont-Smith and all the unrepentant interrupters for my education in the circular causal and feedback difficulties, which taught me to tolerate other peoples' and even my own follies or foibles. Since then I have wept at more than one interdisciplinary meeting, but never departed on that or any other score. I had learned to listen to others and even to myself.

I first saw Norbert's book, Cybernetics, proper, in galley at a Macy meeting. Norbert's cataracts were opaque and he had written his equations large on the blackboard, but could not read them. I had only an hour's chance to skim through the book and we discussed its possible circulation which we both underestimated grossly. The historical introduction is true to the events as I had experienced them; remember, mutatis mutandi, he was a roundhead; I, a cavalier! His philosophy was one I understood and with which I largely concurred. The mathematics was superbly to the point and the text accompanying the equations was sufficient for the mathematical moron. I could and did praise it to him then and there, but he remained overanxious and too tense about the book's real value. He was like a prophet with a message that had to be delivered, not merely like a twelve year old boy seeking approbation. I could easily understand how Julian Bigelow must have felt about it, for Julian disliked publicity and sought completion before publication. Years later, when the book had become a best seller and Norbert a figure in the public eye, Norbert wrote what I believe is the best description of cybernetics thus:

The whole background of my ideas on cybernetics lies in the record of my earlier work. Because I was interested in the theory of communication, I was forced to consider the theory of information and, above all, that partial information which our knowledge of one part of a system gives us of the rest of it. Because I had studied harmonic analysis and had been aware that the problem of continuous spectra drives us back on the consideration of functions and curves too irregular to belong to the classical repertory of analysis, I formed a new respect for the irregular and a new concept of the essential irregularity of the universe. Because I had worked in the closest possible way with physicists and engineers, I knew that our data can never be precise. Because I had some contact with the complicated mechanism of the nervous system, I knew that the world about us is accessible only through a nervous system and that our information concerning it is confined to what limited information the nervous system can transmit.

It is no coincidence that my first childish essay into philosophy, written when I was in high school and not eleven years old, was called "The Theory of Ignorance." Even at that time I was struck with the impossibility of originating a perfectly tight theory with the aid of so loose a mechanism as the human mind. And when I studied with Bertrand Russell, I could not bring myself to believe in the existence of a closed set of postulates for all logic, leaving no room for any arbitrariness in the system defined by them. Here, without the justification of their superb technique, I foresaw something of the critique of Russell which was later to be carried out by Goedel and his followers, who have given real grounds for the denial of the existence of any single closed logic following in a closed and rigid way from a body of stated rules.

To me, logic and learning and all mental activity have always been incomprehensible as a complete and closed picture and have been understandable only as a process by which man puts himself en rapport with his environment. It is the battle for learning which is significant, and not victory. Every victory that is absolute is followed at once by the "Twilight of the Gods," in which the very concept of victory is dissolved in the moment of its attainment.

We are swimming upstream against a great torrent of disorganization, which tends to reduce everything to the heat-death of equilibrium and sameness described in the second law of thermodynamics. What Maxwell, Boltzmann and Gibbs meant by this heat-death in physics has a counterpart in the ethics of Kierkegaard, who pointed out that we live in a chaotic moral universe. In this, our main obligation is to establish arbitrary enclaves of order and system. These enclaves will not remain there indefinitely by any momentum of their own after we have once established them. Like the Red Queen, we cannot stay where we are without running as fast as we can.

We are fighting for a definite victory in the indefinite future. It is the greatest possible victory to be, to continue to be, and to have been. No defeat can deprive us of the success of having existed for some moment of time in a universe that seems indifferent to us.

This is no defeatism, it is rather a sense of tragedy in a world in which necessity is represented by an inevitable disappearance of differentiation. The declaration of our own nature and the attempt to build up an enclave of organization in the face of nature's overwhelming tendency to disorder is an insolence against the gods and the iron necessity that they impose. Here lies tragedy, but here lies glory too.

These were the ideas I wished to synthesize in my book on cybernetics.

My former student, and still my collaborator, Jerome Y. Lettvin, had gone to Boston City Hospital in neurology in 1943, and he had brought Norbert and Walter Pitts together on account of Walter's and my paper on the "Logical Calculus." Walter agreed with Norbert as to the importance of activity in random nets. Walter found a way of computing this by means of the probability of a neuron being fired in terms of the probability of activity in the neurons affecting it. He presented this at one of the Macy cybernetics meetings. Johnny von Neumann saw its importance in

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attacking the problem of shock waves. Walter became a Guggenheim Fellow and went on to work with Norbert as described by him in the introduction to Cybernetics. They worked on the synchrony of heart muscle, which is random in detail of connections of neighbors by protoplasmic bridges, and on the pools of motor neurons with fairly random connections to them fromafferent peripheral neurons playing upon them. Walter and I had less time together until Norbert arranged with Julius A. Stratton and Jerome B. Wiesner for me to join Jerry Lettvin and Walter in the Research Laboratory of Electronics at Massachusetts Institute of Technology, where we have remained together until Walter’s death on May 14, 1969.

After the publication of Cybernetics Norbert rarely had time for the Macy meetings, but they continued to make us a group disciplined in interdisciplinary give-and-take, which came out at the first Hixon Symposium in 1950. Johnny von Neumann had spent hours after the meetings and other long sessions with me, for he had become excited by the possibility of machines, a program and an assembler with a sea of parts and a duplicator of the tape, there is no reason why this cannot be a simplest machine that can reproduce itself. If, by error, it makes a simpler, that machine will fail to reproduce, but if a more complicated, it may reproduce and machines may so evolve. With Heinz von Foerster, Johnny discussed self-organization, the problem being the possibility of things essentially chaotic becoming organized, whether stars or crystals, and, if organisms, whether this organization was inherently informational. Did the system accept its information from the environment to stave off “entropy” or disorganization? What has this to do with learning, above all learning to perceive?

With me Johnny had talked chiefly of the possibilities of building reliable computers of unreliable components, which became the theme of my Toward a Probabilistic Logic. He always nudged me to continue. I did, and much later, with the help of Manuel Blum and Leo A.M. Verbeek, managed to make a logic where the functions, not merely the arguments, were only probable. It did not give the kind of redundancy of coding which Shannon was able to make use of in proving the noisy-channel coding theorem which forms the basis of information theory. Our kind of probabilistic logic did not therefore give rise to an analog of channel capacity in the form of a minimum level of redundancy above which arbitrary reliable computation with unreliable components could be achieved. This was finally achieved in my laboratory by W. Winograd and J.D. Cowan. In constructing our probabilistic logic we had been forced to abandon von Neumann’s multiplexing scheme in which each neuron listens to two and speaks to two, for one in which each neuron listens to many and speaks to many—the so-called anastomotic net. Winograd and Cowan used anastomotic nets, but:

...in such a way as to incorporate the redundant error-correcting codes used in coding for noisy communication channels into the logical functions computed by each neuron in the net. The probabilistic logic which results from their construction is functionally redundant, there are many more possible functions stored in the net than in our own anastomotic nets, and it is this redundancy which is used to obtain reliable computation. Given the assumption that the rate at which neurons malfunction is not tied to the number of inputs playing on each one, the Winograd-Cowan construction is in one-to-one correspondence with that of Shannon’s which led to the noisy channel coding theorem and the existence of a channel capacity. On this basis there is an analogous computational capacity for reliable computers made of unreliable components.

The interesting thing in the present context is that for sufficiently extensive coding almost any code, say one made at random, is nearly optimal. In 1967 Stuart Kauffman came to me for three months, to work on his ideas that a random net of neurons, each listening to two neurons, and each speaking to two, and the sixteen Boolean functions tossed into the neurons at random, would form a good model of epigenesis. These nets must exhibit recurrent behavior sequences called state cycles. Kauffman found that with two inputs per element, nets typically do have very short, stable state cycles, and very few state cycles. Interpreting each state cycle—a stable mode of behavior of the model genetic net—as a distinct cell type, he modeled the epigenetic landscape as flow among the “cell types” induced by random minimal perturbations to a running system. He found that such systems “flow” down an epigenetic landscape to a subset of “cell types” and remain trapped among them. In addition, each “cell type” can only directly flow into a few other cell types—a characteristic of all metazoan cells. Simulation proved him right in model genetic nets of anything up to 8,000 components, beyond which computer memory balks. In all the ensuing time I have tried, and so have several mathematical friends, to get a closed recursive procedure to continue when computers have bogged down for lack of memory, but to date with no success. It does look as though one does not need miracles or intricate clockwork to explain ontogenesis, but only unfamiliar properties of chance and number.

Unfortunately for cybernetics, Johnny became chief scientist of the Atomic Energy Commission and then died young. Norbert kept on struggling with three kinds of problems: The first, the statistical; the second, the coupling of nonlinear oscillators; and finally, continuous nonlinear prediction. These, and combinations of them, proved too tough and the last four years of his life, 1960-1964, were chiefly taken up with philosophical problems, often with an ethical or moral slant. This resulted in an unfortunate use of the word “cybernetics” in international meetings of a
group of physicians, which Norbert christened "the Rheumatism of Cybernetics", and by other groups of would-be social reformers, all of which extensions Norbert has expressly rejected in his answer to Margaret Mead and Gregory Bateson in the very introduction to Cybernetics. There is no doubt but that there are cybernetic problems in them, but we lack sufficiently long runs of uncorrupted data to apply the mathematical tools at our command. These things made us hesitate to set up an American Society for Cybernetics. When we did, it was to forestall the opportunists and the do-gooders. Perhaps we will succeed. The Russians have.

In the realm of neurophysiology cybernetic ideas are really at work. Regenerative and inverse feedback are properly handled. Better conceptions of the distribution of functions over tissues with only partially ordered connections are appearing, and our own paper by J. Lettvin, H. R. Maturana, W. McCulloch and W. Pitts, "What the Frog's Eye Tells the Frog's Brain" pointed the way to a more profitable approach to experimental epistemology. M. L. Minsky and S. A. Papert recently reviewed the limitations of perceptrons without closed loops determined in large measure by their logical depth, and have given a fairly up-to-date bibliography. In practice Louis Sutro's group, including me, has found Azriel Rosenfeld's procedures most useful for specifying the binocular vision required by a robot to get about in a three-dimensional world.

Lewey G. Gilstrap (of Adaptronics) has so solved the electronics of adaptive control of air- and spacecraft, that control even for vertical takeoff and landing, may soon cease to be a pressing problem.

The behavior of closed loops in digital devices lay dormant waiting for mathematical development from "The Logical Calculus of the Ideas Immanent in Nervous Activity" of 1943 to 1956 when David Huffman gave us a theory of shift registers in terms of prime polynomials over Galois fields. As this branch of modular mathematics is comprehensible in the lower predicate calculus, it is known to be consistent and complete. Two years ago, James L. Massey invented an algorithm that will produce the minimum linear shift register to embody a given sequence, say of zeros and ones. A shift register is linear if it consists of a string of delaying components, each signaling to the next and the last to the first by a line going through one or more gates each of which passes on that signal, provided it does not simultaneously receive one from a particular delay or vice versa. By using other logical components in this loop, it is often possible to construct shorter strings of delayers to do the job. José L. Simões da Fonseca showed how to linearize all of these problems in theory, and he and Massey found the algorithm for making them. These can, in theory, account for the learning of a sequence of skilled acts, as in learning to play a given piano composition. In theory now this aspect of cybernetics can go to any elaboration on a sound basis. This should help us to understand the basal ganglia which presumably are the habitat of these delay chains.

Modern electrical engineering theory and practice in control, feed forward, interval timing and autocorrelation to bring signals up out of noise bid fair to give us a clear notion of cerebellar activity. The cerebrum, whose business seems to be to take habits, make guesses and lay plans, certainly presents no insuperable problems in the handling of information on the induc­tive side with its appropriate uncertainty, and the mechanism of multiple-trace formation is under competent chemical, physical and physiological investigation, even including the hippocampus, without which man makes no new traces. Such problems as remain in cortex and thalamus seem purely parochial.

Back in 1952 I had drowned myself in data on the reticular formation of the brain, the abductive organ that commits the entire animal to one rather than to any other of a small number of incompatible modes of behavior, like fight, fly, sleep, eat or make love. For man there may be 14 at least and 18 at most such modes according to whether one includes incompatible reflexes, say for swallowing and breathing. I do not like to experiment when I have no hypothesis to disprove. The anatomy of the reticular core, thanks to the Golgi work of the Scheibel's and Valverde, the sections at right angles to the core through the bends of the flexures of the midbrain, thanks to Walle Nauta, and the studies of the behavior of its cells singly and in combination, thanks to Vahe Amassian and the Scheibel's, have, since, 1952, given us a clear notion of cerebellar activity. The cerebrum, whose business seems to be to take habits, make guesses and lay plans, certainly presents no insuperable problems as remain in cortex and thalamus seem purely parochial.
Christopher Longyear, and I are also at work on it,
each from his own angle. There are nearly enough of
us in close communication to achieve “critical mass.”
That is where cybernetics is today.
It was born in 1943, christened in 1948, and came
of age five years ago in the early 60’s. In its short
majority it has certainly done best for those fields
where it was conceived. It has been a challenge to logic
and to mathematics, an inspiration to neurophysiology
and to the theory of automata, including artificial
intelligence, and bionics or robotology. To the social
sciences it is still mere aspiration.
Above all, it is ready to officiate at the expiration
of philosophical Dualism and Reductionism, to use
MacKay’s phrase, “The Nothing-buttery” of Mentalism
and of Materialism. Our world is again one, and so are
we. Or at least, that is how it looks to one who was
never a prodigy and is not a mathematician.
Examination of Brain Function in an Habilitative Mode

Mark N. Ozer

Introduction

The procedures followed during evaluation of brain function determine to a major degree the perspective with which one views the nervous system. Such procedures have evolved during the past hundred years in response to making specific decisions for treatment of patients with neurological disease. It is necessary to reconsider the assumptions implicit in existing examination approaches in order to recognize when other procedures may be more congruent with needs for treatment and reflective of a more functional model of the nervous system. The context for such rethinking has been the need for design of more useful procedures for examination of children with various degrees of developmental difficulties. These new procedures may have implications for the evaluation of patients with more classical neurological syndromes and most particularly for the rehabilitative needs of such patients.

The assumptions within which the evaluation of brain function in clinical neurology has developed were those constraints established by Hughlings Jackson. His concept was that the brain as an organ was concerned with sensory impressions and motor responses. Neurologists assume in the classical clinical neurological examination that all the functions of the central nervous system and their changes under pathological conditions are expressible in terms of excitation (sensory) and reaction (motor), thereby deliberately ignoring the element of level of consciousness (1).

Nevertheless, it has long been understood that this application of the reflex arc to explain the function of a living organism is a hypothetical construct. Reflex action at any one point in time and place is the outcome of the interaction of that point with the remainder of the organism. Each reflex must be considered the functional expression not of a single level, but of the whole...[and] according to a given situation, the past and future of the organism (2).

In a series of clinical studies Bender and associates (3-5) emphasized the effects of concurrent stimulation and level of consciousness in the performance of the patient. Their efforts were devoted to localization of lesions in the nervous system as the major issue of clinical neurology. Although no structure was considered to be identical with a function, patterns of disturbance in the type and symmetry of performance were used for establishing the site in the nervous system at which a lesion might be expected to exist. Double simultaneous stimulation of homologous and non-homologous parts of the body as well as deliberate use of short-acting barbiturates were the clinical procedures by which focal disturbances were elicited.

The dimensions added to the model of brain function as reflected in the examination protocol were the effects of level of consciousness and the presence of concurrent competing stimuli. Moreover, explicit manipulation of these factors went on during the clinical examination. In a more general sense, therefore, there was greater awareness of the conditions under which one examined the patient as affecting the results obtained. The organism was no longer viewed as defined by a reflex model alone but as a more nearly integrated whole, in terms of the context in which examination went on.

This principle of integration is crucial to a wider understanding of the activities of the nervous system in the living organism. We do not believe reflex action to be the true model of the dynamics of an organism, "unless...[it is] the outcome of the activity of the whole...and not of one of its fragments and...a functional instrument for self-preservation and self-realization" (6). The dimension that must therefore be added to the examination protocol is explicit awareness that the performance of the patient is a result of his or her interaction with the examiner in response to the conditions set. Symptoms then may be viewed as instruments by which the diseased organism attempts to maintain life and function under the new conditions. What has therefore been added to the clinical neurological examination is greater awareness of the interactive quality of the examination. How the examiner behaves in asking particular questions affects what the patient says. The patient is, however, still being tested. The patient’s behavior is the dependent variable while the examiner’s behavior is not viewed as affected in turn. The examiner remains an observer rather than a participant in the interaction.

It is important to recognize the relationship of such clinical evaluation procedures to the specific issues and assumptions of clinical neurology. In the child with developmental problems other issues may be paramount and other evaluation procedures more pertinent. The localization issue of clinical neurology is far less pertinent. In the absence of progressive disease, treatment for the child is rarely that of the physical...
removal or radiological destruction of the offending lesion. The concept of a single causative factor whose removal would presumably lead to recovery is also far less pertinent. The underlying cause for developmental difficulties in the child is frequently unknown and currently unknowable. It is frequently irrelevant to the treatment of the child seen usually after the offending agent is no longer operative.

Moreover the potential clearly exists for increased growth and adaptation on the part of the child in the future. The study of the patient is far less clearly related to the concept of fixed physical lesions so prominent in the pathological cast of clinical neurology. The "disease" is not progressive but the child is developing. This process of development may be considered as an increase in the degree of organization of an individual considered as a functional adaptive system (7). The clinical protocol for examination in developmental problems must place particular emphasis on this continuum of growth and change in which the examination itself may represent a significant episode. Focus on the process of growth in the child may also lead to an awareness of a similar temporal quality in the evolution of adaptive behavior of the adult patient with brain injury. The examination itself may be an opportunity for such treatment to be carried out in other settings over a longer time period.

The Neuro-developmental Observation

A 15-minute protocol has been developed as a model for interactive examination of children with developmental problems. It is called the Neuro-developmental Observation (NDO). It seeks to illustrate a process of decision-making that may lead to treatment of these problems, just as the clinical neurological examination has done so for other kinds of problems subject to surgical or other physical methods of treatment. The first assumption that underlies the NDO is that the behavior elicited during an examination is affected by the conditions provided during the examination (see Figure 1). If one changes the conditions then one changes, within certain limits, the function of the organism. The second assumption is that the treatment of developmental problems is not primarily short-term and physical and in the hands of the professional diagnostician. Rather it must be viewed as long-term, and in the hands of those who are responsible for the child's continued development. These so-called primary participants are the child, his parents and teachers. The third assumption is that the child, as well as the primary adults in his environment, has a potential for development across time. It is the procedures for such development that one is sampling in the relatively short-term interaction between these primary participants and the professional diagnostician. The context for such development may be involvement in a relatively reciprocal fashion of the diagnostician and the primary participants. The fourth, and even more basic, assumption is a broad view of the nervous system. The nervous system is viewed as the physical embodiment of the means by which the organism maintains his or herself within the biological constraints (temperature, etc.) and adapts to both continuity and change in the internal and external milieux (8).

It is such a growth process that may be sampled during the time and other constraints under which the diagnostician must work. It is such an adaptive process that must both be simulated and stimulated. The evaluation process is viewed as a dynamic one in which conditions are explicitly varied so that the child's behavior is modified. The objective is both to explore some problem-solving strategies and to explicitly communicate these options to the child and the responsible adults. The clinical examination is a short-term sampling of the change that may occur in the child. The child may be expected to learn something during the examination itself. It is this process of learning that is made explicit and self-conscious by being made relatively mutualistic in its interaction.

In the model NDO protocol, a set of prototype tasks is used to illustrate the procedures of this interactive type of evaluation. The first group of tasks is an adaptation from those used in the study of aphasic patients by Henry Head as modified by Benton (9). The eventual goal is to have the child follow an instruction such as: "Touch your right ear with your left hand." The child is first asked to identify the hands correctly, then the body parts. He is then asked to recognize the segmented quality of the longer instruction in carrying it out. That is, he may recognize that there are at least two parts into which the instruction may be divided. The issue is not whether or not he has a problem in following more complex commands. Rather he is offered a possible solution. As he uses the adaptive strategy of "segmentation" it is explicitly identified and he is reinforced for having used it as well as for the results obtained.

The child is then either told or shown or moved through a second series of tasks. The objective now is to illustrate the value of using different modalities of input as well as for the child to take increasing responsibility for helping define the conditions to be provided him. For example, the child is told: "With both hands together, touch your head, then your knees, then your stomach." If he proceeds incorrectly, he now has two options. He may ask the examiner to

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THE EXAMINATION PROCESS

THE PATIENT IS MADE AWARE THAT HE AFFECTS THE EXAMINER AS WELL AS BEING AFFECTED—AS A CONTEXT FOR ENHANCED ADAPTATION.
show him or move him through the activity as well as
tell him again, after having experienced the use of
these various alternative input modalities. Again the
child is given recognition for defining a problem-solving
strategy as well as for the results obtained.
The third set of tasks is designed to illustrate both
the aspect of competing stimuli and a greater degree of
independent function. After a demonstration, the child
is asked to connect circles randomly printed on a sheet
of paper. After the behavior of connecting the circles
is established, the child is then asked to connect circles
on a second sheet containing competing geometric
forms (lines, crosses and triangles). After a timed
interval of connecting circles in the presence of these
visual distractions, the examiner then names the
competing figures for an equivalent length of time.
Usually 10-second intervals are used. The child now
is offered the option on his or her request to connect
circles on a sheet of paper identical to the one used
for initial training where there are no competing
figures and the examiner is now silent. The number of
circles connected during this third 10-second interval is
compared to the performance during the previous
intervals. It is generally higher than under the previous
conditions. The child is given recognition for having
requested the option of reducing distracting stimuli as
a problem-solving strategy as well as for his increased
rate of performance.

Discussion

This clinical model is an intrinsic part of a large-
scale service program for children with developmental
problems (10). The issue to be explored here is the
implication of such an examination for a model of
brain function. It has been suggested that the evalua-
tion of a functioning organism must reflect those
characteristics which make it an organism. One such
aspect is the activity of the organism toward his or her
maintenance and self-realization in relationship to the
surrounding environment. Such a relationship is con-
sidered an interactive one as the context for evolution
of the organism across time. The examination model is
explicitly designed to reflect this quality.

What the examiner does is determined by the needs
of the child in terms of modifying the conditions in
which the child carries out the prototype tasks. More-
over the options are to some degree determined by the
child. The particular options illustrated during the
NDO are but a sample. They do offer an initial
approach by which one may vary the stimuli provided
in terms of time (segmentation across time), kind
(input modality) and space (number of competing
stimuli at any one time). As such they offer specific
problem-solving strategies that may be applied to other
tasks of higher priority in the real life setting for the
child. The process of application requires selection by
the child and the other primary participants of what
they perceive as useful strategies for them to use, as
well as the task priorities of their specific environment
in which those strategies may be applied.

It is not, however, the particular strategies or their
number that are crucial. It is the recognition that one
may explore strategies for solving problems that is
being simulated. The objective of evaluating brain func-
tion in the area of development problems may differ
from the classical issues of clinical neurology. The
product of such an evaluation may be to define
options for enhanced development. The very process
of examination may then be the context for such en-
hancement by viewing the examination as a truly inter-
active one in which the consultant is a participant as
well as an observer. It is the view of the organism as
an adaptive evolving system that is being modeled.

The application of these principles to rehabilitative
needs of the adult patient requires the following prem-
ises. First is that the localization issues of clinical
neurology have been dealt with. Second is that the
neuro-psychological study of the patient must relate to
treatment or rehabilitative needs. Third is that the
patient and those others who remain responsible for
his case have the best opportunity both to define their
goals in rehabilitation and to select the strategies by
which they may reach such goals. The responsibility of
the consultant professional is to demonstrate and
clarify such strategies and to help the other primary
participants define their goals.
The examination procedures may thus serve as a
short-term teaching exercise. The context for such a
demonstration would be those tasks defined by the
patient and/or those responsible for his care. The
objective is to explore a limited number of problem-
solving strategies by which this task or several such
tasks might be accomplished. The value of such a
demonstration may be increased to the extent that it is
clear that the strategies used are what is to be
communicated. What has been modeled is an educa-
tional procedure with the patient which may be
generalized to other tasks that may need to be learned.
The context for such learning would be tasks that
relate directly to the patient’s real life needs. The NDO
described above uses prototype tasks as a model.
Diagnostic information is obtained in reference to
“what works” rather than the presence or absence of a
disability.

It is necessary to rethink our modes of evaluation
of patients with handicaps in order to begin to collect
such strategies from those patients who have adapted
in various ways. Such data as to “what has worked”
may then be communicated to new patients for their
selection for their own needs. The strategies that may
be helpful to such patients are not as yet specified.
The disability is defined by the environment in real
terms. The role of the consultant is to demonstrate
how that environment, including the patient, might
develop ways of coping with these disabilities. It is
further suggested that the process of defining limited
realizable goals and strategies for coping in reaching such goals may be in itself therapeutic. The diagnostic session in this new model is in itself a sample of treatment. To the degree that it involves the primary participants in the process of learning, to that degree the effects of professional resources may enhance the rehabilitation of the patient. The implications of such a search may lead us toward an awareness of the brain as an adaptive system rather than as one fixed by formalin.

Bibliography

In this issue we present part one of a series on Cybernetics at the University of Illinois by Richard H. Howe and Heinz Von Foerster. In the first part, Heinz Von Foerster describes the course, called Cybernetics of Cybernetics.

1. Cybernetica

The Departments of Electrical Engineering and of Physiology and Biophysics offer to their undergraduate and graduate students each semester general topic courses (EE 272, 490, and B.Ph. 199, 491). Since on earlier occasions a fusion of my sections of these courses into one class worked well, I contemplated offering for the Academic Year 1973/1974 a two-semester compound course for these Departments with a subject-matter that would not only have ramifications into the biological and the engineering sciences, but also perform an integrating function on these sciences as well. Cybernetics appeared to me as an appropriate subject matter. Particularly, thanks to Norbert Wiener's explanatory clause for cybernetics as "Communication and Control in the Animal and the Machine," I could pacify—though not enthuse—my colleagues in the life sciences and in engineering, for there appeared in the contemplated title the words "animal," which would satisfy the former, and "machine," which would persuade the latter. Moreover, a course on cybernetics would give me an opportunity to celebrate Norbert Wiener, this kind and competent man, by observing through this course the 25th birthday of his most beloved brain child.

However, unlike other disciplinarians who may not—even should not—apply their competences to themselves (pyrotechnicians, analytic chemists, surgeons, etc.) the cybernetician must apply his competences to himself lest he will lose all scientific credibility. What if the "expert" on communication and control cannot communicate, or excuses his irresponsible acts by claiming that he has been controlled by someone else? He will be placed among the fakes of whom there are enough without him. Consequently, a course on cybernetics must be conducted cybernetically.

This appears to be more than a herculean task, for it seems that the entire "educational machinery," from an infant's way of learning how to walk and to talk, through institutionalized forms of instruction as in kindergarten, grade and high schools, to institutions of higher and continued adult education, this machinery is attempting to do just that, and has failed: the gap is growing wider and wider.

We submit, again without proof, that this is so because of an almost universal confusion in which "knowledge" is seen as a commodity, i.e., is identified with substance rather than with process. We hear from distinguished speakers: "...Universities are Depositories of Knowledge that is handed down from generation to generation..." but—alas—A's nervous activity is just A's nervous activity and not B's. An educational system that confuses learning with the dispensing of goods called "knowledge" may cause some disappointment in the hypothetical receivers, for the goods are just not coming: there are no goods.

We are not proposing to aid this machinery by introducing still another device that is based on this delusion; instead we propose to provide the "initial ignition" to get the primary process going again. We allude here to the second order concept of "learning of learning" in which "subject matter" assumes the role of an arbitrary vehicle, a means for locomotion.

While cybernetics began by developing the epistemology for comprehending and simulating first order regulatory processes "in the animal and the machine," cybernetics today provides a conceptual framework with sufficient richness to attack successfully second order processes (e.g., cognition, dialogue, socio-cultural interaction, etc.).

We propose to make this conceptual framework accessible to a large and diversified audience by a publication whose design is the short range goal of this effort.

* * * * *

First of all, such a course should have at the outset, visible to all participants, a tangible primary goal that may, through interaction by the participants, evolve into others with conceptual ties to the first one; second, the topic should serve as a vehicle for an understanding of how the topic is understood; third, it should transform an accidental assembly of anonymous students into a group of interacting individuals; and, finally, it should account for this transformation. In search of an appropriate tangible goal for a course of cybernetics, friends suggested to me that a need in the scientific community could be fulfilled if the class creates a collection of up-to-date Cybernetica. This, however, would require material support beyond providing an instructor.

Although this concept of a course as outlined before does not fit precisely the conventional image of an engineering class in a Midwestern University, I
found moral encouragement in my departments, which was, because of the present state of financial affairs in institutes of higher learning, all I could hope for. Moreover, since this program proposes not to proceed along a path that has been demonstrably trodden before by many others, Governmental Agencies that support basic research today will ipso facto dismiss it as worthless, because its worth has as yet not been proven.

At that point, Point—a foundation on the West Coast—came to our rescue and bestowed the Biological Computer Laboratory with a grant. Since one of the rules of Point is not to respond to a proposal requesting a grant, we submitted our proposal after having received one. Here are some (modified) excerpts:

**PROPOSAL**

There is a hiatus between what is known and “common knowledge.” In developing countries like, for example, the United States, this hiatus is widening at an accelerating pace. We shall not waste ink, time and patience to prove there is such a hiatus, nor are we going to argue that this is “bad.” We shall simply address ourselves to the problem of how to narrow this gap.

Knowing that this proposal would be accepted, it was now easy to compose the description of a course that would follow the outline of this proposal. Following a precedent established by Margaret Mead, I called this course “Cybernetics of Cybernetics.”

This is a project oriented course. Its principal aim is to arrive at a format (model) for a publication (monograph, anthology, reader, source book, handbook, catalogue, primer, or whatever) that, when published, shall serve as a nucleus for a comprehensive presentation of the full range of methods and concepts in cybernetics as they are available today with regard to cognitive, social and cultural processes. However, a knowledge of cybernetics is not required as a prerequisite in this course.

“First-Order Cybernetics” developed the epistemology for comprehending and simulating biological processes as, e.g., homeostasis, habituation, adaptation, and other first-order regulatory processes. “Second-Order Cybernetics” provides a conceptual framework with sufficient richness to attack successfully such second-order processes as, e.g., cognition, dialogue, socio-cultural interactions, etc.

It is the purpose of this course to make this conceptual framework accessible to a large and diversified audience (from high school students to university professors, from local organizers of voluntary action programs to administrators of large civic systems), by a publication whose design should be accomplished on or about midterm of the Spring Semester, 1974. The book to be designed will be a thousand (1000) page volume, 8½” by 11”, to be run off on rotary presses. Moreover, besides its internally fully interacting organization by means of cross-referencing, concordance, glossary, and newly to be developed graphic means, this volume is to be abundantly illustrated, comparable to McLuhan-Fiore’s *The Medium is the Massage* or the *Whole Earth Catalog*, so that going through this volume should be an intellectual as well as a visual feast.

Students who wish to participate in this course should be prepared to meet exacting production schedules and a considerable workload. Last day for dropping this course will be the date as posted in the University Calendar. Only those students should apply who believe in learning by doing.

This description sufficed to discourage about thirty-four thousand potential participants (about the student body on our campus), for only 29 came and signed up for this course. In the beginning some of these students left us, but others came to join us and our group grew to 45. All years of academic progress, from freshmen to Ph.D. candidates, were represented—with a tilt toward the younger generation. Despite his own stringent schedule, Professor Herbert Brün from our Department of Music joined me in this venture, participated in almost all sessions, saw to it that our dialogue did not degenerate into monologue, and kept us alert to the crippling effects of language when it controls—instead of being created by—our thoughts. Kenneth L. Wilson played his double role as student and T.A. to perfection. He threw all his energy and empathy into this class, and that a tangible result indeed emerged goes to his credit.

Too much material has accumulated during the 60 sessions that were held in two semesters, and too little time (three weeks) has passed to allow for a comprehensive assessment of this course. Perhaps this can be done on a later date. However, the following articles in this series will provide a “raw” look at the course.
Position Paper

Cutting the NSF-OSIS Budget: Potential Disaster for Information Science and Technology

The Budget of the United States Government for FY 1975 clearly shows that the shapers of public opinion and the makers of public policy do not understand what we are about and what we do. In FY 1975, the National Science Foundation's program and facilities obligations will be close to $600,000,000. Estimated obligations for FY 1974 are $569,000,000. This is an increase of nearly $30,000,000. The accompanying narrative states that this increase "reflects the administration's support for fundamental knowledge necessary for economic growth and long-range solutions of problems of national concern." The OSIS estimated obligations for the same period are $8,000,000 in FY 1974 and $5,000,000 in FY 1975.

Even the Office of Management and Budget would not have made a 40% cut from the OSIS FY 1974 budget if it had understood the importance of information science, or if it had felt that a cohesive, vocal and strong profession with some congressional friends might be heard from.

The following is the statement submitted by Joshua I. Smith on behalf of ASIS to the Subcommittee on Science Research and Development of the Committee on Science and Astronautics of the U.S. House of Representatives on the National Science Foundation Authorization Act 1975, H.R. 12816, regarding science information systems, 19 March 1974.

Joshua I. Smith

Introduction

MR. CHAIRMAN AND MEMBERS OF THE SUBCOMMITTEE:

My name is Joshua I. Smith. I am Executive Director of the American Society for Information Science. I appear before you with the approval of the Society's Executive Committee to present this statement. With me are Mr. Dale Baker, President-elect of the Society and Director of Chemical Abstracts Service; and Dr. Burton Adkinson, former Executive Director of the American Geographical Society and notable Information Scientist. Mr. Herbert S. White, President of the Society, was unable to be here today.

We do indeed appreciate the opportunity to meet with you to express our views regarding a trend which clearly indicates diminishing support for scientific and technical information activities and lack of overall national policy regarding scientific and technical programs. This in an era when we are experiencing a transition period from an industrial to a post-industrial society. Specifically, we are concerned about the drastic cuts in the budget for the Office of Science Information Service (OSIS) of the National Science Foundation (NSF) while there is a marked upswing for support of all other programs in NSF.

I would like the record to show that we have not come here today at the instigation of the Office of Science Information Service, nor are we here to play the role of a lobby or pressure group. We are here because we, as the major professional society in Information Science, are disturbed—as members of Congress are disturbed—that this great country of ours in not moving in the proper direction to adequately establish and maintain our position of leadership and standard of excellence in the Information Age that we built up in this Industrial Age.

We are disturbed because the OSIS currently represents the only existing Federal body seeking to develop ways in which scientific and technical information...
activities can be supported. We are disturbed and mystified as to why so little has been done about previous recommendations made by both governmental and non-governmental groups calling for improvements in Government information programs. We are disturbed that there exists no organization to establish policy and provide leadership in this Information Age.

About ASIS

The American Society for Information Science (ASIS) is a non-profit professional association of nearly 4000 members, organized for scientific, literary and educational purposes, and dedicated to the creation, organization, dissemination and application of knowledge concerning information and its transfer. ASIS' purpose is to develop advanced methodologies and techniques that contribute to the more efficient use of information.

Briefly, Information Science is concerned with how man communicates with man. It is the study of how information is transferred—from the point of generation to the point of use—and all the intermediate steps of collecting, organizing, interpreting, storing, retrieving, disseminating and transforming information. As a discipline, Information Science stresses the application of modern technologies to the handling of information. ASIS acts as a bridge between research and development on one hand, and the requirements of diverse types of information systems on the other. It provides a forum for the discussion, publication, and critical analysis of work dealing with the theory and practice of all elements involved in the communication of information.

ASIS was founded in 1937 as the American Document Institute (ADI), consisting originally of representatives of affiliated societies, foundations, and Government agencies. In 1952, ADI admitted individual members, thus becoming the national professional society for those concerned with the problems of documentation and information services. In the years since, spurred by the problems created by the "information explosion" and by the potential of automatic devices for literature searching and for information storage and retrieval, the membership grew, not only in numbers, but also in variety of professional interests. Reflecting this change in its total range of activities, as well as the emergence of Information Science as an identifiable configuration of disciplines, the members voted to change the name of ADI to ASIS in 1968.

The key word describing the ASIS membership is diversity. ASIS membership includes information specialists from such wide-ranging fields as Librarianship, Computer Science, Management, Chemistry, Engineering, Linguistics, Operations Research, Symbolic Logic, Data Processing, Communications, Publishing, Economics, Mathematics, Education, and all disciplines concerned with information handling. The individual members are employed by universities, Government agencies, computer manufacturers, software companies, research institutes, the information-products industry, and a wide variety of other types of organizations.

ASIS members are primarily involved (about 65%) in the operation of libraries and information facilities, and in management and administration of information departments in companies and Government agencies. They are also involved in research and development, teaching, consulting, marketing, and other endeavors. Their subject backgrounds range from the physical sciences and engineering, to the biomedical, to the behavioral and social sciences, as well as to the arts and humanities. ASIS members are leaders in other organizations, such as the Information Industry Association (trade association for profit companies), the National Federation of Abstracting and Indexing Services (the organization for information input), and the Association of Scientific Information Dissemination Centers (the organization for information output).

ASIS has official liaison with 40 organizations, including the American Association for the Advancement of Science (which represents more than 100,000 scientists), the American Chemical Society (which has over 100,000 members), the Association for Computing Machinery, the National Microfilm Association, and the American Library Association. In addition, ASIS is a constituent society of the American Federation of Information Processing Societies (an umbrella organization for 13 information-handling associations), an institutional affiliate of the International Federation for Documentation (consisting of members from more than 30 countries), and a voting representative on pertinent committees of the American National Standards Institute. Thus, ASIS reaches hundreds of thousands of individuals through its various programs and activities.

The Society is organized into 20 local Chapters, 11 Student Chapters, and 15 Special Interest Groups. The local Chapters are situated in the following regions of the United States: Carolinas, Central Ohio (Columbus), Chesapeake Bay (Baltimore), Chicago, Delaware Valley (Philadelphia and Wilmington), Frontier (Arizona, Colorado, Kansas, Nebraska, New Mexico, Oklahoma, Utah, and Wyoming), Indiana, Los Angeles, Metropolitan New York, Michigan, Minnesota, New England (Boston), Northern Ohio (Cleveland), Pacific Northwest (Washington, Oregon, Idaho, Montana, and Alaska), Pittsburgh, Potomac Valley (Washington, D.C.), San Francisco Bay, Southern Ohio (Cincinnati-Dayton), and Upstate New York. There is also a Chapter in Western Canada.

It is appropriate, therefore, that ASIS give this testimony, since we are the major society in the field of Information Science and Technology, and can therefore speak to the broad issues that face us. We would not be representing our professional community if we
did not respectfully put our views before this important arm of Congress.

Support of Information Programs in the Post-Industrial Period

If we think of the world at large, most of it is still essentially pre-industrial, in that at least 90% of the labor force is engaged in extractive work: mining, fishing, timber, agriculture. The industrial societies are essentially those in which the majority of the labor force is engaged primarily in industry and manufacturing. The United States is probably the first post-industrial nation. In the U.S. the majority of the labor force is engaged in services—trade, finance, real estate, education, research, administration, government, etc. A pre-industrial society is essentially one based on raw materials, as a game against nature, and in which there are diminishing returns. An industrial society is organized primarily around energy, and the use of energy for the production of goods. A post-industrial society is organized around information and knowledge, the utilization of information in complex systems, and the use of information as a way of guiding the society. The idea of a post-industrial society is unique in human history and is dependent upon the organization of information, without which we could no longer know where we are going to be going, and, as an old Talmudic aphorism puts it: "If you don't know where you are going, any road will take you there."

OSIS Cutback—A Dangerous Trend

During the sixties and the early seventies, this country was well into its post-industrial phase and there were a number of Federal agencies involved in Information Sciences research programs. With the passage of time, these programs disappeared or were reduced to the point of near invisibility, with the exception of the National Science Foundation's Office of Science Information Service (OSIS). OSIS, as you are aware, is a unique element of the Foundation in that it operates not only under the statutory authority that brought NSF into being (NSF Act of 1950), but also under Title IX, National Defense Education Act of 1958, which specifically established the Office of Science Information Service.

In the late sixties, OSIS had a budget of about $15 million and a personnel count of about 79 professionals and administrative people. The OSIS' estimated obligations are $8 million in FY 1974 and $5 million for FY 1975, and it now has a personnel count of 23 people. This is at a time when the Federal agencies are spending about $1 billion a year for their scientific and technical information programs.

This "meat axe" reduction is inconceivable in light of the general NSF budget figures which show estimated obligations for FY 1974 as $561 million and close to $600 million for FY 1975. The accompanying narrative to the NSF budget states that the increase of close to $30 million "reflects the administration's support for fundamental knowledge necessary for economic growth and long range solutions of problems of national concern." What is not revealed is the lack of understanding of the importance and cost of organized information programs and information handling; the continuing proliferation of scientific and technical information as a worldwide phenomenon; the requirement for modern, computerized information systems that store and disseminate technical knowledge; the increasing number of scientists, engineers, industrial innovators, governmental officials, students, and others that require up-to-date scientific and technical information and the need for research and development that will increase the effectiveness, efficiency, and economical operation of the rapidly proliferating information systems and services.

This retrogressive development clearly shows that the shapers of public opinion and the makers of public policy do not understand the stakes involved—the importance of the OSIS program—and what a blow the drying up of the program means to those of us in the world of Information Science.

We recognize that cuts in the NSF-OSIS budget are a result of several factors, some internal to NSF, others applicable to the OMB, and still others traceable to Congress itself. However, we are concerned that the value of this group not be overlooked, because of the potential benefits it can provide the whole field.

A potential disaster that comes with the cutback in OSIS funding is the lack of support for experimental projects which can lead to better operations, both in terms of economics and effectiveness to the user. OSIS, before the cut, was a major source for this kind of funding to universities, non-profit societies and for-profit organizations.

The cutback in OSIS funding has been made despite the fact that within other Federal agencies support for information programs is provided. For example, Public Law 92-225 provides for the establishment and operation of a major information center in support of the Drug Abuse Office and Treatment Program. Public Law 92-218, Section 407, provides funding for an international information data bank in support of the National Cancer Program.

Increased Foreign Support for Information Science

The trend of diminishing support, as evidenced by the cutback in funding at NSF-OSIS, is certainly counter to that of various foreign nations whose information services are becoming much more aggressive and more strongly supported. For example, in West
Germany at present, there is a major build-up of information activities in science and technology, supported jointly by private and federal groups. The European Economic Community is expanding from an earlier base of nuclear information activities into a whole new broad-range program of information services. The USSR is currently developing its integrated information systems for science and technology at a cost of one billion rubles per year ($1.46 billion).

Japan is also embarking on a long-term national plan for modern, effective, scientific/technological information services and centers. The recent Japanese "white paper" titled The Plan for Information Society—A National Goal Toward Year 2000, recommends a major, centrally planned development of what the Japanese call "the information society." They propose a five-year investment of 1,000 billion yen ($3.2 billion). Their argument is that, with such a national investment, they can sustain an annual rate of growth of GNP in excess of ten percent per year, contrasted with a seven percent growth rate if they follow a U.S.-style laissez-faire policy of information investment.

Obviously, these nations have established policies and programs in development of their R&D programs in science and technology which must have good supporting information programs. These efforts by foreign governments will lead to useful international coordination of information efforts, providing the U.S. level of competence in information is assured.

Issues

As information scientists we are concerned about the large investment necessary to develop information utilities that permit economical on-demand access to information. This involves high cost for research and development and implementation. We are concerned about the effects and impact of information technology on the culture of our people. By 1980, it is expected that computers will penetrate the home-consumer market as well as provide services for Government, business, and education. This penetration may be through time-sharing services for household or office, or information and entertainment accessible from inexpensive terminals attached to a telephone or a television set.

A market in excess of $20 billion per year is projected for two-way information services to the home in the 1980's. Presently, there are more than 100 million telephones in operation in the United States with more than 700 million circuit miles of cable, wire and radio relay systems crisscrossing the land.

More than 97% of American homes now have television. Out of more than 60 million households with television sets, approximately 6 million are served by cable, which allows for two-way interaction.

We are concerned and involved in the study of alternatives to information-delivery problems other than traditional means such as print and conferences. Electronic media, micrographics and various combinations of the two offer possible solutions to the pressures for faster and more accurate information. These also have the desirable potential of reducing the amount of energy and pollution since information access and delivery require little or no travel and fewer natural resources.

We are concerned about the social effects of information technology. The information-poor are also the economically poor and less able to obtain information. Will advancements in information technology be primarily for the well-educated, economically advanced segment of society, or will we find ways of short-circuiting this unfortunate situation?

Here are some key issues which need attention.

ECONOMICS

The economics of information in the post-industrial or knowledge society may well represent the single most important activity over the next few years. The information industry is growing and will become the fastest growing industry in this country during the last quarter of this century.

RELIABILITY IN TECHNOLOGY

Use of computers within various time and cost limits will shape attitudes regarding the application of technology.

PRIVACY

With information more accessible, through machine-readable data bases, than ever before, threats to individual rights become keen. Decisions are made from this information which affect the lives and future of individuals.

EDUCATION IN INFORMATION SCIENCE/TECHNOLOGY

Many of the problems that exist are caused by a lack of education regarding computers and other information technology. Education is needed for the public at large as well as for those to be trained in Information Science and Technology. More and more universities are offering courses and degree programs in information science and it is expected that courses on the organization of knowledge will be offered on the undergraduate and high school levels.
TELECOMMUNICATIONS

The phenomenal growth in telecommunications is a direct result of the many applications and potentials in this area. CATV, digital communications network, and satellite communications offer many solutions and alternatives socially, economically, and pragmatically.

MEDIA

Printing is rapidly being replaced by microforms and video technology. Combining CATV with video recording offers individual addressing and home recording of video transmission.

PROPRIETARY USE AND RIGHTS OF INFORMATION

New technology makes copyright infringement quite easy and convenient. Many issues must be addressed before any thorough understanding can be achieved. This will indeed be a key issue in the years ahead.

SOCIAL IMPACT OF INFORMATION TECHNOLOGY

The Office of Education says that Sesame Street reached 8 million out of 12 million children in the age group of 3 to 5 years and reaches 90% of the disadvantaged children in this age group. The program is not only popular and effective but also economical, less than one cent per child per day. With adequate support more successful examples of information technology can be developed to serve the poor and disadvantaged.

USER NEEDS

The focus of information activities and programs must be redirected from concern with document handling to the needs of the user. Areas of consideration are 1. the value of information derived by the user; 2. time and capacity to absorb information by the user; and 3. involvement of the user in the designing of information systems.

STANDARDS

Greater emphasis on standardization is required in order to achieve compatibility among information systems and to avoid wasteful duplication among developing information systems.

Conclusion

There is a need for a National Information Policy that can strengthen the development and application of science and technology, and that can be coordinated with national policy on science and technology. Emphasis must be placed on the broad responsibility of the Government to be a supporter and catalyst in devising mechanisms for effective use of information, and to deal with national information programs on a planned and coordinated basis. There is also the crucial need for the Government to systematically evaluate the implications and capabilities of the continuing technical advances made in computers, communications, micrographics, and related technologies.

We must develop more satisfactory mechanisms for locating and bringing together the diverse social and technical information bearing on interdisciplinary programs (such as health, education, environmental quality, transportation, urban services), so as to ensure more cooperative programs and cut down on unnecessary duplication. A policy on coordination of international programs regarding technical information activities should also be developed.

References

Letters to the Editor

The Editorial by Charles H. Dym—"Two Cultures: A Dangerous GAP" in the last issue of the FORUM (VI., No. 1) has generated opinion pro and con. Two samples are printed below, along with the Editor's answer.

Pro...

Dear Editor:

Immediately upon receipt of the March issue of the Forum I experienced a strong inclination to write you, an impulse I have on several occasions come to regret having postponed responding to.

My purpose in writing you is three-fold: First, I wanted to congratulate Charles Dym upon having written something that has needed to be said for a long time. Second, what Mr. Dym said was said so effectively that it cannot avoid having impact in many important academic circles if his editorial were to reach them; and Third, to ask your permission to do what I can to broaden the circulation of the editorial, always, of course, identifying the author and the ASC FORUM as its source.

Paul S. Windsem
3100 Tice Creek Drive #3
Walnut Creek, California 94595

...and Con

Dear Editor:

One of your associate editors, Mr. Dym, has written a sorrowing and dignified piece, recommending dignity and responsibility to that regrettable minority of scientists who, though eminent, are said to be emotionally immature, and furthermore noisy, in pressing their views on important issues, without making it perfectly clear that though they are scientists, and are talking partly as scientists, they are also talking partly on the basis of their incompletely scientific beliefs. Hooey! With activities called science taking up a noticeable fraction of the U.S. and world budget, and defended as contributing to everything good, from national security to the avoidance of world ecological catastrophe, those activities called science are now part of the political process, and are so by their own dynamic, and by social needs.

Some politicians are dignified and responsible, but most of as have our personal lists of those whom we consider undignified and irresponsible; it will turn out similarly, among those who try to influence scientific policy: some will retain dignity, either as a personal style preference or as a useful facade, while others will end up on somebody's list of Bad Guys, because they have found other presentations of themselves and their ideas to be more congenial to themselves, or more effective in communicating. Politics (or the art of living together in cities) is part of real life, and to the extent that science is part of real life, both afferently and efferently, it will tend to adopt political activities when these are needed or thought to be needed. Does Mr. Dym think that those job-classified as Scientists, within the Department of Defense, for example, trade any less on the glamor sometimes attaching to that label (for instance, when presenting arguments for ABMs), than do their critics (for instance, when presenting arguments against ABMs)? If Establishment scientists often happen to be dignified, does this make them any more virtuous, or merely more effective in persuasion?

I doubt that Mr. Dym is scientifically qualified to pass on others' "emotional maturity"; therefore I conclude that his sorrowing dignity is mainly a way of trying to "put down" some folks whose ideas and methods he dislikes. To paraphrase an earlier (and occasionally dignified) U.S. President, if you don't like the heat, Mr. Dym, why don't you renounce eating off of Federal monies?

Donald O. Walter, Ph.D.
Adjunct Associate Professor
Physiology Department, UCLA
Los Angeles, California 90024

Editor's Reply

Dear Editor:

I would have to agree with Donald Walter that there are times when science cannot escape politics. Determining the national budget for scientific projects is one of them, as he suggests. In this instance, the issue is not whether scientists should engage in politics. It is how discourse shall be conducted. Does dignified behavior make "Establishment scientists" more virtuous? No. More effective? Yes.

When it comes to scientists who are militant members of the social cause-of-the-month club, which may involve them in matters far beyond the domains of their professional competence, it doubtless may be "more congenial" to some of them to make shrill demands and shout down their opponents, but this will not alter the fact that such behavior has damaging consequences. By playing the fool, they invite contempt at a time when primitive passions, emotionalism and the spirit of anti-intellectualism and anti-rationality are already mounting powerful attacks on science. If the desire to rush in and join, rather than oppose, these tendencies does not constitute emotional immaturity, then I leave it to Professor Walter to explain what does.

As to his suggestion that I "renounce eating off of Federal monies," I hope he will not be too disappointed to learn that my firm does not receive one penny of Federal funds, directly or indirectly. Unhappily, because of taxes, the flow is in the reverse direction.

Finally, I cannot refrain from commenting on Professor Walter's estimate of my intentions, which he expresses by the curious locution that I must be "trying to 'put down' some folks." If he means that I am taking issue with them, that obviously is the point to any criticism. But if he means that I am trying to lessen their dignity—why, heavens to Betsy, how could he think such a thing!

Charles H. Dym
Dym, Frank & Co.
Washington, D.C.
Nominations

ASC President Roy Herrmann has appointed a three-member Nominating Committee, headed by Gary D. Bearden, Vice President (Administration). The two other members are Dr. Louise B. Speck, a Director, and Mrs. Bonnie W. Dunning, Vice President (Education).

A request for names of qualified candidates to fill the positions of President, all Vice Presidents, Treasurer and Secretary as well as two directorships in the upcoming elections will be mailed out to every ASC member in good standing. Candidates to be suggested must be members of this Society.

Please return your recommendations as soon as possible.

Congratulations

William E. Hanna, Jr., member of ASC's Board of Directors, has been elected Major of Rockville, Maryland. His forceful personality will make itself felt throughout the political and economic life of suburban Washington, and we expect that the first cybernetician holding such an office will have the key to solving the many problems that beset everyday life. We wish him every possible success.

Call for Members

Select Committee on Education in Cybernetics

The Society plans to form a Select Committee to study, evaluate, and plan education programs in the field of cybernetics. It is felt that the number and quality of offerings in the field are currently inadequate.

The Committee will study current offerings, plan and promote seminars, courses, and, if appropriate, degree programs in the field of cybernetics.

Society members who are interested in serving on this Committee and who feel they can make a contribution are encouraged to write Gary Bearden, Vice President, American Society for Cybernetics, Suite 530, 1130 17th Street, N.W., Washington, D.C. 20036.

Please include a short biographical sketch and a statement of how you think you can participate and contribute to the effort.
22-26 July 1974
XVI TRIENNIAL CONGRESS of the International Institute of Administrative Sciences (IIAS), Mexico City, Mexico. Bill Hanna will be a member of the U.S. delegation serving as U.S. Correspondent on Informatics and Decision Making.

5-10 August 1974
IFIP CONGRESS '74, Stockholm, Sweden.

12-14 August 1974
SECOND INTERNATIONAL CONFERENCE ON COMPUTER COMMUNICATIONS, Stockholm, Sweden. Dr. Carl Hammer is Deputy Secretary General. Further information available from Edward E. Boyer, ICCC Executive Secretary, 1860 Wiehle Avenue, Reston, VA 22090.

19-21 August 1974

3-6 September 1974
EUROPEAN MEETING of the Econometric Society, Grenoble, France.

21 September 1974
THIRD ANNUAL REGIONAL CONFERENCE on the Interdisciplinary Aspects of General Systems Theory, Society for General Systems Research, Middle Atlantic Division, University of Maryland, College Park, MD.

23-27 September 1974
7th INTERNATIONAL SYMPOSIUM ON HUMAN FACTORS IN TELECOMMUNICATION, co-sponsored by the Bell Telephone Laboratories, General Telephone & Electronics Corp. (GTE) and the Canadian Telecommunications Carriers Association (CTCA), Montreal, Canada.

30 September-3 October 1974
FOURTH INTERNET CONGRESS ON PROJECT MANAGEMENT IN THE SEVENTIES, sponsored by the International Management Systems Association (INTERNET) and Association Francaise pour la Cybernétique Economique et Technique (AFCET), Paris, France. Contact: Secretariat Permanent, C.U.D., avenue de Pologne, 75775 Paris CEDEX 16, France.

13-17 October 1974
37th ANNUAL MEETING of the American Society for Information Science (ASIS) on Information Utilities. Regency Hyatt House, Atlanta, Ga. Contacts: Dr. V. Slamecka, School of Information and Computer Science, Georgia Institute of Technology, Atlanta, GA 30332, or Robert McAfee, Jr., ASIS National Office, 1155 Sixteenth Street, NW, Suite 210, Washington, DC 20036

16-18 October 1974
JOINT NATIONAL MEETING, Operations Research Society of America and The Institute of Management Sciences, on urban, environmental and energy problems, San Juan, Puerto Rico. General Chairman: Armando Riesco, P.O. Box 234, Mayaguez, Puerto Rico 00708.

21-23 October 1974
SYMPOSIUM ON ADAPTIVE ECONOMICS, Mathematics Research Center, University of Wisconsin, Madison, WI. Proceedings will be published in book form.

27-31 October 1974
1974 INTERNATIONAL SYMPOSIUM ON INFORMATION THEORY, sponsored by IEEE. Center for Continuing Education, University of Notre Dame, Indiana.

29-31 October 1974
FIFTH INTERNATIONAL SYMPOSIUM ON OLFACTION AND TASTE at Howard Florey Institute of Experimental Physiology and Medicine, University of Melbourne, Parkville, Victoria 3052, Australia.

31 October-2 November 1974
ASC CONFERENCE ON COMMUNICATION AND CONTROL IN SOCIAL PROCESSES, Wharton School, Philadelphia, PA.

28-30 December 1974
NORTH AMERICAN MEETING of the Econometric Society, San Francisco, CA.
About the Authors

(In alphabetical Order of Last name)

ROY HERRMANN
President of Center of Cybernetic and Interdisciplinary Research, Inc., and of Institute for Socio-Economic Studies, Ltd., Washington, DC; formerly, professor of management sciences and operations research. The George Washington University; professor, naval science, U.S. Naval Academy, Annapolis, MD; chairman, College on Logistics, a Subdivision of The Institute of Management Sciences. B.S., College Francais, Berlin; Doctor of Political Science and Economics, University of Rostock, Germany. ASC President, 1972, 1973, 1974.

WARREN S. McCULLOCH
Before his death in 1969, he was a senior staff member of the M.I.T. Research Laboratory for Electronics. He received his B.A. from Yale University in 1921, his M.A. from Columbia University in 1923, and his M.D. from Columbia Medical School in 1927. He was Professor of Psychiatry at the College of Medicine, University of Illinois, until 1952. His society memberships included: Fellow AAAS, Fellow IEEE, Member of Aerospace Medical Association, the American Academy of Science, and the New York Academy of Science. His work on the input-output relationships in neural structures opened up new disciplines, including neurocybernetics and bionics.

MARK N. OZER
Associate professor, child health and development, associate neurologist, Children's Hospital, Washington, DC; attending neurologist and director. Program for Learning Studies, The George Washington University Medical School; formerly, assistant resident, Stanford University Hospital; assistant resident and chief resident, neurology, Mt. Sinai Hospital, New York; neurologist, USAF Hospital, South Ruislip, England; NIMH fellow, Walter Reed Army Institute and Washington School of Psychiatry; instructor neurology, Howard University Medical School, A.B., Harvard; M.D., Boston University.

JOSHUA I. SMITH
Executive Director, the American Society for Information Science. Formerly the Associate Executive Director of ASIS, Mr. Smith was director of the Clearinghouse on Library and Information Sciences (CLIS) for the Educational Resources Information Center (ERIC) and developed the ASIS publications program.

Mr. Smith was manager of the Databook Division for Plenum Publishing Corporation in New York. Before that, he held several management positions with the University of Akron in Akron, Ohio, including assistant director for operations in the Center for Information Systems and director of the State Technical Services Program for Northern Ohio.

Mr. Smith is a cum laude graduate of Central State University in Wilberforce, Ohio. After graduation, he was an instructor in biology and counselor for men there. He also taught mathematics, biology, and chemistry in the Washington, D.C. Public School System.

Mr. Smith belongs to the following organizations: Alpha Phi Alpha Fraternity, Alpha Kappa Mu National Honor Society, Alpha Phi Gamma (national journalism society), Beta Kappa Chi National Scientific Honor Society, American Library Association, and ASIS.

HEINZ VON FOERSTER
Professor of biophysics at the University of Illinois, Biological Computer Laboratory, Department of Electrical Engineering. Physics diploma, Institute of Technology, Austria; Ph.D. in physics, University of Breslau, Germany. One of the founders, past chairman of ASC and actively involved in its development since 1964.
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The FORUM is designed to provide not only cyberneticists, but also intelligent laymen, with an insight into cybernetics and its applicability to a wide variety of scientific, social and economic problems. Contributions should be lively, graphic and to the point. Tedious listings of tabular material should be avoided.

The Editors reserve the right to make stylistic modifications consistent with the requirements of the FORUM. No substantive changes will be made without consultation with authors. They further reserve the right to reject manuscripts they deem unsuitable in nature, style or content.

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Types of Manuscripts: Three types of contributions are considered for publication: full-length articles, brief communications of 1,000 words or less, and letters to the editor. Letters and brief communications can generally be published sooner than full-length manuscripts. Books, monographs and reports are accepted for critical review. Two copies should be addressed to the Editor.

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