# Introduction

This book represents the outcome, after more than a decade, of a program of work undertaken jointly with Dr. Arturo Rosenblueth, then of the Harvard Medical School and now of the Instituto Nacional de Cardiología of Mexico. In those days, Dr. Rosenblueth, who was the colleague and collaborator of the late Dr. Walter B. Cannon, conducted a monthly series of discussion meetings on scientific method. The participants were mostly young scientists at the Harvard Medical School, and we would gather for dinner about a round table in Vanderbilt Hall. The conversation was lively and unrestrained. It was not a place where it was either encouraged or made possible for anyone to stand on his dignity. After the meal, somebody—either one of our group or an invited guest—would read a paper on some scientific topic, generally one in which questions of methodology were the first consideration, or at least a leading consideration. The speaker had to run the gauntlet of an acute criticism, good-natured but unsparing. It was a perfect catharsis for half-baked ideas, insufficient self-criticism, exaggerated self-confidence, and pomposity. Those who could not stand the gaff did not return, but among the former habitués of these meetings there is more than one of us who feels that they were an important and permanent contribution to our scientific unfolding.

Not all the participants were physicians or medical scientists. One of us, a very steady member, and a great help to our discussions, was Dr. Manuel Sandoval Vallarta, a Mexican like Dr. Rosenblueth and a Professor of Physics at the Massachusetts Institute of Technology, who had been among my very first students when I came to the Institute after World War I. Dr. Vallarta used to bring some of his M.I.T. colleagues along to these discussion meetings, and it was at one of these that I first met Dr. Rosenblueth. I had been interested in the scientific method for a long time and had, in fact, been a participant in Josiah Royce's Harvard seminar on the subject in 1911–1913. Moreover, it was felt that it was essential to have someone present who could examine mathematical questions critically. I thus became an active member of the group until Dr. Rosenblueth's call to Mexico in 1944 and the general confusion of the war ended the series of meetings.

For many years Dr. Rosenblueth and I had shared the conviction that the most fruitful areas for the growth of the sciences were those which had been neglected as a no-man's land between the various established fields. Since Leibniz there has perhaps been no man who has had a full command of all the intellectual activity of his day. Since that time, science has been increasingly the task of specialists, in fields which show a tendency to grow progressively narrower. A century ago there may have been no Leibniz, but there was a Gauss, a Faraday, and a Darwin. Today there are few scholars who can call themselves mathematicians or physicists or biologists without restriction. A man may be a topologist or an acoustician or a coleopterist. He will be filled with the jargon of his field, and will know all its literature and all its ramifications, but, more frequently than not, he will regard the next subject as something belonging to his colleague three doors down the corridor, and will consider any interest in it on his own part as an unwarrantable breach of privacy. These specialized fields are continually growing and invading new territory. The result is like what occurred when the Oregon country was being invaded simultaneously by the United States settlers, the British, the Mexicans, and the Russians—an inextricable tangle of exploration, nomenclature, and laws. There are fields of scientific work, as we shall see in the body of this book, which have been explored from the different sides of pure mathematics, statistics, electrical engineering, and neurophysiology; in which every single notion receives a separate name from each group, and in which important work has been triplicated or quadruplicated, while still other important work is delayed by the unavailability in one field of results that may have already become classical in the next field.

It is these boundary regions of science which offer the richest opportunities to the qualified investigator. They are at the same time the most refractory to the accepted techniques of mass attack amid the division of labor. If the difficulty of a physiological problem is mathematical in essence, ten physiologists ignorant of mathematics will get precisely as far as one physiologist ignorant of mathematics, and no further. If a physiologist who knows no mathematics works together with a mathematician who knows no physiology, the one will be unable to state his problem in terms that the other can manipulate, and the second will be unable to put the answers in any form that the first can understand. Dr. Rosenblueth has always insisted that a proper exploration of these blank spaces on the map of science could only be made by a team of scientists, each a specialist in his own field but each possessing a thoroughly sound and trained acquaintance with the fields of his neighbors; all in the habit of working together, of knowing one another's intellectual customs, and of recognizing the significance of a colleague's new suggestion before it has taken on a full formal expression. The mathematician need not have the skill to conduct a physiological experiment, but he must have the skill to understand one, to criticize one, and to suggest one. The physiologist need not be able to prove a certain mathematical theorem, but he must be able to grasp its physiological significance and to tell the mathematician for what he should look. We had dreamed for years of an institution of independent scientists, working together in one of these backwoods of science, not as subordinates of some great executive officer, but joined by the desire, indeed by the spiritual necessity, to understand the region as a whole, and to lend one another the strength of that understanding.

We had agreed on these matters long before we had chosen the field of our joint investigations and our respective parts in them. The deciding factor in this new step was the war. I had known for a considerable time that if a national emergency should come, my function in it would be determined largely by two things: my close contact with the program of computing machines developed by Dr. Vannevar Bush, and my own joint work with Dr. Yuk Wing Lee on the design of electric networks. In fact, both proved important. In the summer of 1940, I turned a large part of my attention to the development of computing machines for the solution of partial differential equations. I had long been interested in these and had convinced myself that their chief problem, as contrasted with the ordinary differential equations so well treated by Dr. Bush on his differential analyzer, was that of the representation of functions of more than one variable. I had also become convinced that the process of scanning, as employed in television, gave the answer to that question and, in fact, that television was destined to be more useful to engineering by the introduction of such new techniques than as an independent industry.

It was clear that any scanning process must vastly increase the number of data dealt with as compared with the number of data in a problem of ordinary differential equations. To accomplish reasonable results in a reasonable time, it thus became necessary to push the speed of the elementary processes to the maximum, and to avoid interrupting the stream of these processes by steps of an essentially slower nature. It also became necessary to perform the individual processes with so high a degree of accuracy that the enormous repetition of the elementary processes should not bring about a cumulative error so great as to swamp all accuracy. Thus the following requirements were suggested:

1. That the central adding and multiplying apparatus of the computing machine should be numerical, as in an ordinary adding machine, rather than on a basis of measurement, as in the Bush differential analyzer.

2. That these mechanisms, which are essentially switching devices, should depend on electronic tubes rather than on gears or mechanical relays, in order to secure quicker action.

3. That, in accordance with the policy adopted in some existing apparatus of the Bell Telephone Laboratories, it would probably be more economical in apparatus to adopt the scale of two for addition and multiplication, rather than the scale of ten.

4. That the entire sequence of operations be laid out on the machine itself so that there should be no human intervention

from the time the data were entered until the final results should be taken off, and that all logical decisions necessary for this should be built into the machine itself.

5. That the machine contain an apparatus for the storage of data which should record them quickly, hold them firmly until erasure, read them quickly, erase them quickly, and then be immediately available for the storage of new material.

These recommendations, together with tentative suggestions for the means of realizing them, were sent in to Dr. Vannevar Bush for their possible use in a war. At that stage of the preparations for war, they did not seem to have sufficiently high priority to make immediate work on them worth while. Nevertheless, they all represent ideas which have been incorporated into the modern ultra-rapid computing machine. These notions were all very much in the spirit of the thought of the time, and I do not for a moment wish to claim anything like the sole responsibility for their introduction. Nevertheless, they have proved useful, and it is my hope that my memorandum had some effect in popularizing them among engineers. At any rate, as we shall see in the body of the book, they are all ideas which are of interest in connection with the study of the nervous system.

This work was thus laid on the table, and, although it has not proved to be fruitless, it led to no immediate project by Dr. Rosenblueth and myself. Our actual collaboration resulted from another project, which was likewise undertaken for the purposes of the last war. At the beginning of the war, the German prestige in aviation and the defensive position of England turned the attention of many scientists to the improvement of anti-aircraft artillery. Even before the war, it had become clear that the speed of the airplane had rendered obsolete all classical methods of the direction of fire, and that it was necessary to build into the control apparatus all the computations necessary. These were rendered much more difficult by the fact that, unlike all previously encountered targets, an airplane has a velocity which is a very appreciable part of the velocity of the missile used to bring it down. Accordingly, it is exceedingly important to shoot the missile, not at the target, but in such a way that missile and target may come together in space at some time in the future. We must hence find some method of predicting the future position of the plane.

The simplest method is to extrapolate the present course of the plane along a straight line. This has much to recommend it. The more a plane doubles and curves in flight, the less is its effective velocity, the less time it has to accomplish a mission, and the longer it remains in a dangerous region. Other things being equal, a plane will fly as straight a course as possible. However, by the time the first shell has burst, other things are *not* equal, and the pilot will probably zigzag, stunt, or in some other way take evasive action.

If this action were completely at the disposal of the pilot, and the pilot were to make the sort of intelligent use of his chances that we anticipate in a good poker player, for example, he has so much opportunity to modify his expected position before the arrival of a shell that we should not reckon the chances of hitting him to be very good, except perhaps in the case of a very wasteful barrage fire. On the other hand, the pilot does *not* have a completely free chance to maneuver at his will. For one thing, he is in a plane going at an exceedingly high speed, and any too sudden deviation from his course will produce an acceleration that will render him unconscious and may disintegrate the plane. Then too, he can control the plane only by moving his control surfaces, and the new regimen of flow that is established takes some small time to develop. Even when it is fully developed, it merely changes the acceleration of the plane, and this change of acceleration must be converted, first into change of velocity and then into change of position, before it is finally effective.

Moreover, an aviator under the strain of combat conditions is scarcely in a mood to engage in any very complicated and untrammeled voluntary behavior, and is quite likely to follow out the pattern of activity in which he has been trained.

All this made an investigation of the problem of the curvilinear prediction of flight worth while, whether the results should prove favorable or unfavorable for the actual use of a control apparatus involving such curvilinear prediction. To predict the future of a curve is to carry out a certain operation on its past. The true prediction operator cannot be realized by any constructible apparatus; but there are certain operators which bear it a certain resemblance and are, in fact, realizable by apparatus which we can build. I suggested to Professor Samuel Caldwell of the Massachusetts Institute of Technology that these operators seemed worth trying, and he immediately suggested that we try them out on Dr. Bush's differential analyzer, using this as a ready-made model of the desired fire-control apparatus. We did so, with results which will be discussed in the body of this book. At any rate, I found myself engaged in a war project, in which Mr. Julian H. Bigelow and myself were partners in the investigation of the theory of prediction and of the construction of apparatus to embody these theories.

It will be seen that for the second time I had become engaged in the study of a mechanico-electrical system which was designed to usurp a specifically human function—in the first case, the execution of a complicated pattern of computation, and in the second, the forecasting of the future. In this second case, we should not avoid the discussion of the performance of certain human functions. In some fire-control apparatus, it is true, the original impulse to point comes in directly by radar, but in the more usual case, there is a human gun-pointer or a gun-trainer or both coupled into the fire-control system, and acting as an essential part of it. It is essential to know their characteristics, in order to incorporate them mathematically into the machines they control. Moreover, their target, the plane, is also humanly controlled, and it is desirable to know its performance characteristics.

Mr. Bigelow and I came to the conclusion that an extremely important factor in voluntary activity is what the control engineers term *feedback*. I shall discuss this in considerable detail in the appropriate chapters. It is enough to say here that when we desire a motion to follow a given pattern the difference between this pattern and the actually performed motion is used as a new input to cause the part regulated to move in such a way as to bring its motion closer to that given by the pattern. For example, one form of steering engine of a ship carries the reading of the wheel to an offset from the tiller, which so regulates the valves of the steering engine as to move the tiller in such a way as to turn these valves off. Thus the tiller turns so as *to* bring the other end of the valve-regulating offset amidships, and in that way registers the angular position of the wheel as the angular position of the tiller. Clearly, any friction or other delaying force which hampers the motion of the tiller will increase the admission of steam to the valves on one side and will decrease it on the other, in such a way as to increase the torque tending to bring the tiller to the desired position. Thus the feedback system tends to make the performance of the steering engine relatively independent of the load.

On the other hand, under certain conditions of delay, etc., a feedback that is too brusque will make the rudder overshoot, and will be followed by a feedback in the other direction, which makes the rudder overshoot still more, until the steering mechanism goes into a wild oscillation or *hunting*, and breaks down completely. In a book such as that by MacColl,[1] we find a very precise discussion of feedback, the conditions under which it is advantageous, and the conditions under which it breaks down. It is a phenomenon which we understand very thoroughly from a quantitative point of view.

Now, suppose that I pick up a lead pencil. To do this, I have to move certain muscles. However, for all of us but a few expert anatomists, we do not know what these muscles are; and even among the anatomists, there are few, if any, who can perform the act by a conscious willing in succession of the contraction of each muscle concerned. On the contrary, what we will is *to pick the pencil up*. Once we have determined on this, our motion proceeds in such a way that we may say roughly that the amount by which the pencil is not yet picked up is decreased at each stage. This part of the action is not in full consciousness.

To perform an action in such a manner, there must be a report to the nervous system, conscious or unconscious, of the amount by which we have failed to pick up the pencil at each instant. If we have our eye on the pencil, this report may be visual, at least in part, but it is more generally kinesthetic, or, to use a term now in vogue, proprioceptive. If the proprioceptive sensations are wanting and we do not replace them by a visual or other substitute, we are unable to perform the act of picking up the pencil, and find ourselves in a state of what is known as *ataxia*. An ataxia of this type is familiar in the form of syphilis of the central nervous system known as *tabes dorsalis*, where the kinesthetic sense conveyed by the spinal nerves is more or less destroyed.

However, an excessive feedback is likely to be as serious a handicap to organized activity as a defective feedback. In view of this possibility, Mr. Bigelow and myself approached Dr. Rosenblueth with a very specific question. Is there any pathological condition in which the patient, in trying to perform some voluntary act like picking up a pencil, overshoots the mark, and goes into an uncontrollable oscillation? Dr. Rosenblueth immediately answered us that there is such a well-known condition, that it is called purpose tremor, and that it is often associated with injury to the cerebellum.

We thus found a most significant confirmation of our hypothesis concerning the nature of at least some voluntary activity. It will be noted that our point of view considerably transcended that current among neurophysiologists. The central nervous system no longer appears as a self-contained organ, receiving inputs from the senses and discharging into the muscles. On the contrary, some of its most characteristic activities are explicable only as circular processes, emerging from the nervous system into the muscles, and re-entering the nervous system through the sense organs, whether they be proprioceptors or organs of the special senses. This seemed to us to mark a new step in the study of that part of neurophysiology which concerns not solely the elementary processes of nerves and synapses but the performance of the nervous system as an integrated whole.

The three of us felt that this new point of view merited a paper, which we wrote up and published.[2] Dr. Rosenblueth and I foresaw that this paper could be only a statement of program for a large body of experimental work, and we decided that if we could ever bring our plan for an interscientific institute to fruition, this topic would furnish an almost ideal center for our activity.

On the communication engineering plane, it had already become clear to Mr. Bigelow and myself that the problems of control engineering and of communication engineering were inseparable, and that they centered not around the technique of electrical engineering but around the much more fundamental notion of the message, whether this should be transmitted by electrical, mechanical, or nervous means. The message is a discrete or continuous sequence of measurable events distributed in time—precisely what is called a time series by the statisticians. The prediction of the future of a message is done by some sort of operator on its past, whether this operator is realized by a scheme of mathematical computation, or by a mechanical or electrical apparatus. In this connection, we found that the ideal prediction mechanisms which we had at first contemplated were beset by two types of error, of a roughly antagonistic nature. While the prediction apparatus which we at first designed could be made to anticipate an extremely smooth curve to any desired degree of approximation, this refinement of behavior was always attained at the cost of an increasing sensitivity. The better the apparatus was for smooth waves, the more it would be set into oscillation by small departures from smoothness, and the longer it would be before such oscillations would die out. Thus the good prediction of a smooth wave seemed to require a more delicate and sensitive apparatus than the best possible prediction of a rough curve, and the choice of the particular apparatus to be used in a specific case was dependent on the statistical nature of the phenomenon to be predicted. This interacting pair of types of error seemed to have something in common with the contrasting problems of the measure of position and of momentum to be found in the Heisenberg quantum mechanics, as described according to his Principle of Uncertainty.

Once we had clearly grasped that the solution of the problem of optimum prediction was only to be obtained by an appeal to the statistics of the time series to be predicted, it was not difficult to make what had originally seemed to be a difficulty in the theory of prediction into what was actually an efficient tool for solving the problem of prediction. Assuming the statistics of a time series, it became possible to derive an explicit expression for the mean square error of prediction by a given technique and for a given lead. Once we had this, we could translate the problem of optimum prediction to the determination of a specific operator which should reduce to a minimum a specific positive quantity dependent on this operator. Minimization problems of this type belong to a recognized branch of mathematics, the calculus of variations, and this branch has a recognized technique. With the aid of this technique, we were able to obtain an explicit best solution of the problem of predicting the future of a time series, given its statistical nature, and even further, to achieve a physical realization of this solution by a constructible apparatus.

Once we had done this, at least one problem of engineering design took on a completely new aspect. In general, engineering design has been held to be an art rather than a science. By reducing a problem of this sort to a minimization principle, we had established the subject on a far more scientific basis. It occurred to us that this was not an isolated case, but that there was a whole region of engineering work in which similar design problems could be solved by the methods of the calculus of variations.

We attacked and solved other similar problems by the same methods. Among these was the problem of the design of wave filters. We often find a message contaminated by extraneous disturbances which we call *background noise*. We then face the problem of restoring the original message, or the message under a given lead, or the message modified by a given lag, by an operator applied to the corrupted message. The optimum design of this operator and of the apparatus by which it is realized depends on the statistical nature of the message and the noise, singly and jointly. We thus have replaced in the design of wave filters processes which were formerly of an empirical and rather haphazard nature by processes with a thorough scientific justification.

In doing this, we have made of communication engineering design a statistical science, a branch of statistical mechanics. The notion of statistical mechanics has indeed been encroaching on every branch of science for more than a century. We shall see that this dominance of statistical mechanics in modern physics has a very vital significance for the interpretation of the nature of time. In the case of communication engineering, however, the significance of the statistical element is immediately apparent. The transmission of information is impossible save as a transmission of alternatives. If only one contingency is to be transmitted, then it may be sent most efficiently and with the least trouble by sending no message at all. The telegraph and the telephone can perform their function only if the messages they transmit are continually varied in a manner not completely determined by their past, and can be designed effectively only if the variation of these messages conforms to some sort of statistical regularity.

To cover this aspect of communication engineering, we had to develop a statistical theory of the *amount of information*, in which the unit amount of information was that transmitted as a single decision between equally probable alternatives. This idea occurred at about the same time to several writers, among them the statistician R. A. Fisher, Dr. Shannon of the Bell Telephone Laboratories, and the author. Fisher's motive in studying this subject is to be found in classical statistical theory; that of Shannon in the problem of coding information; and that of the author in the problem of noise and message in electrical filters. Let it be remarked parenthetically that some of my speculations in this direction attach themselves to the earlier work of Kolmogoroff[3] in Russia, although a considerable part of my work was done before my attention was called to the work of the Russian school.

The notion of the amount of information attaches itself very naturally to a classical notion in statistical mechanics: that of *entropy*. Just as the amount of information in a system is a measure of its degree of organization, so the entropy of a system is a measure of its degree of disorganization; and the one is simply the negative of the other. This point of view leads us to a number of considerations concerning the second law of thermodynamics, and to a study of the possibility of the so-called Maxwell demons. Such questions arise independently in the study of enzymes and other catalysts, and their study is essential for the proper understanding of such fundamental phenomena of living matter as metabolism and reproduction. The third fundamental phenomenon of life, that of irritability, belongs to the domain of communication theory and falls under the group of ideas we have just been discussing.[4]

Thus, as far back as four years ago, the group of scientists about Dr. Rosenblueth and myself had already become aware of the essential unity of the set of problems centering about communication, control, and statistical mechanics, whether in the machine or in living tissue. On the other hand, we were seriously hampered by the lack of unity of the literature concerning these problems, and by the absence of any common terminology, or even of a single name for the field. After much consideration, we have come to the conclusion that all the existing terminology has too heavy a bias to one side or another to serve the future development of the field as well as it should; and as happens so often to scientists, we have been forced to coin at least one artificial neo-Greek expression to fill the gap. We have decided to call the entire field of control and communication theory, whether in the machine or in the animal, by the name *Cybernetics*, which we form from the Greek *χυβερν της* or *steersman*. In choosing this term, we wish to recognize that the first significant paper on feedback mechanisms is an article on governors, which was published by Clerk Maxwell in 1868,[5] and that *governor* is derived from a Latin corruption of *χυβερν της*. We also wish to refer to the fact that the steering engines of a ship are indeed one of the earliest and best-developed forms of feedback mechanisms.

Although the term *cybernetics* does not date further back than the summer of 1947, we shall find it convenient to use in referring to earlier epochs of the development of the field. From 1942 or thereabouts, the development of the subject went ahead on several fronts. First, the ideas of the joint paper by Bigelow, Rosenblueth, and Wiener were disseminated by Dr. Rosenblueth at a meeting held in New York in 1942, under the auspices of the Josiah Macy Foundation, and devoted to problems of central inhibition in the nervous system. Among those present at that meeting was Dr. Warren McCulloch, of the Medical School of the University of Illinois, who had already been in touch with

Dr. Rosenblueth and myself, and who was interested in the study of the organization of the cortex of the brain.

At this point there enters an element which occurs repeatedly in the history of cybernetics—the influence of mathematical logic. If I were to choose a patron saint for cybernetics out of the history of science, I should have to choose Leibniz. The philosophy of Leibniz centers about two closely related concepts—that of a universal symbolism and that of a calculus of reasoning. From these are descended the mathematical notation and the symbolic logic of the present day. Now, just as the calculus of arithmetic lends itself to a mechanization progressing through the abacus and the desk computing machine to the ultra-rapid computing machines of the present day, so the *calculus ratiocinator* of Leibniz contains the germs of the *machina ratiocinatrix*, the reasoning machine. Indeed, Leibniz himself, like his predecessor Pascal, was interested in the construction of computing machines in the metal. It is therefore not in the least surprising that the same intellectual impulse which has led to the development of mathematical logic has at the same time led to the ideal or actual mechanization of processes of thought.

A mathematical proof which we can follow is one which can be written in a finite number of symbols. These symbols, in fact, may make an appeal to the notion of infinity, but this appeal is one which we can sum up in a finite number of stages, as in the case of mathematical induction, where we prove a theorem depending on a parameter $n$ for $n = 0$, and also prove that the case $n + 1$ follows from the case $n$, thus establishing the theorem for all positive values of $n$. Moreover, the rules of operation of our deductive mechanism must be finite in number, even though they may appear to be otherwise, through a reference to the concept of infinity, which can itself be stated in finite terms.

In short, it has become quite evident, both to the nominalists like Hilbert and to the intuitionists like Weyl, that the development of a mathematico-logical theory is subject to the same sort of restrictions as those that limit the performance of a computing machine. As we shall see later, it is even possible to interpret in this way the paradoxes of Cantor and of Russell.

I am myself a former student of Russell and owe much to his influence. Dr. Shannon took for his doctor's thesis at the Massachusetts Institute of Technology the application of the techniques of the classical Boolean algebra of classes to the study of switching systems in electrical engineering. Turing, who is perhaps first among those who have studied the logical possibilities of the machine as an intellectual experiment, served the British government during the war as a worker in electronics, and is now in charge of the program which the National Physical Laboratory at Teddington has undertaken for the development of computing machines of the modern type.

Another young migrant from the field of mathematical logic to cybernetics is Walter Pitts. He had been a student of Carnap at Chicago and had also been in contact with Professor Rashevsky and his school of biophysicists. Let it be remarked in passing that this group has contributed much to directing the attention of the mathematically minded to the possibilities of the biological sciences, although it may seem to some of us that they are too dominated by problems of energy and potential and the methods of classical physics to do the best possible work in the study of systems like the nervous system, which are very far from being closed energetically.

Mr. Pitts had the good fortune to fall under McCulloch's influence, and the two began to work quite early on problems concerning the union of nerve fibers by synapses into systems with given overall properties. Independently of Shannon, they had used the technique of mathematical logic for the discussion of what were after all switching problems. They added elements which were not prominent in Shannon's earlier work, although they are certainly suggested by the ideas of Turing: the use of the time as a parameter, the consideration of nets containing cycles, and of synaptic and other delays.[6]

In the summer of 1943, I met Dr. J. Lettvin of the Boston City Hospital, who was very much interested in matters concerning nervous mechanisms. He was a close friend of Mr. Pitts, and made me acquainted with his work.[7] He induced Mr. Pitts to come out to Boston, and to make the acquaintance of Dr. Rosenblueth and myself. We welcomed him into our group. Mr. Pitts came to the Massachusetts Institute of Technology in the autumn of 1943, in order to work with me and to strengthen his mathematical background for the study of the new science of cybernetics, which had by that time been fairly born but not yet christened.

At that time Mr. Pitts was already thoroughly acquainted with mathematical logic and neurophysiology, but had not had the chance to make very many engineering contacts. In particular, he was not acquainted with Dr. Shannon's work, and he had not had much experience of the possibilities of electronics. He was very much interested when I showed him examples of modern vacuum tubes and explained to him that these were ideal means for realizing in the metal the equivalents of his neuronic circuits and systems. From that time, it became clear to us that the ultra-rapid computing machine, depending as it does on consecutive switching devices, must represent almost an ideal model of the problems arising in the nervous system. The all-or-none character of the discharge of the neurons is precisely analogous to the single choice made in determining a digit on the binary scale, which more than one of us had already contemplated as the most satisfactory basis of computing-machine design. The synapse is nothing but a mechanism for determining whether a certain combination of outputs from other selected elements will or will not act as an adequate stimulus for the discharge of the next element, and must have its precise analogue in the computing machine. The problem of interpreting the nature and varieties of memory in the animal has its parallel in the problem of constructing artificial memories for the machine.

At this time, the construction of computing machines had proved to be more essential for the war effort than the first opinion of Dr. Bush might have indicated, and was progressing at several centers along lines not too different from those which my earlier report had indicated. Harvard, Aberdeen Proving Ground, and the University of Pennsylvania were already constructing machines, and the Institute for Advanced Study at Princeton and the Massachusetts Institute of Technology were soon to enter the same field. In this program there was a gradual progress from the mechanical assembly to the electrical assembly, from the scale of ten to the scale of two, from the mechanical relay to the electrical relay, from humanly directed operation to automatically directed operation; and in short, each new machine more than the last was in conformity with the memorandum I had sent Dr. Bush. There was a continual going and coming of those interested in these fields. We had an opportunity to communicate our ideas to our colleagues, in particular to Dr. Aiken of Harvard, Dr. von Neumann of the Institute for Advanced Study, and Dr. Goldstine of the Eniac and Edvac machines at the University of Pennsylvania. Everywhere we met with a sympathetic hearing, and the vocabulary of the engineers soon became contaminated with the terms of the neurophysiologist and the psychologist.

At this stage of the proceedings, Dr. von Neumann and myself felt it desirable to hold a joint meeting of all those interested in what we now call cybernetics, and this meeting took place at Princeton in the late winter of 1943–1944. Engineers, physiologists, and mathematicians were all represented. It was impossible to have Dr. Rosenblueth among us, as he had just accepted an invitation to act as Head of the laboratories of physiology of the Instituto Nacional de Cardiología in Mexico, but Dr. McCulloch and Dr. Lorente de Nó of the Rockefeller Institute represented the physiologists. Dr. Aiken was unable to be present; however, Dr. Goldstine was one of a group of several computing-machine designers who participated in the meeting, while Dr. von Neumann, Mr. Pitts, and myself were the mathematicians. The physiologists gave a joint presentation of cybernetic problems from their point of view; similarly, the computing-machine designers presented their methods and objectives. At the end of the meeting, it had become clear to all that there was a substantial common basis of ideas between the workers in the different fields, that people in each group could already use notions which had been better developed by the others, and that some attempt should be made to achieve a common vocabulary.

A considerable period before this, the war research group conducted by Dr. Warren Weaver had published a document, first secret and later restricted, covering the work of Mr. Bigelow and myself on predictors and wave filters. It was found that the conditions of anti-aircraft fire did not justify the design of special apparatus for curvilinear prediction, but the principles proved to be sound and practical, and have been used by the government for smoothing purposes, and in several fields of related work. In particular, the type of integral equation to which the calculus of variations problem reduces itself has been shown to emerge in wave-guide problems and in many other problems of an applied mathematical interest. Thus in one way or another, the end of the war saw the ideas of prediction theory and of the statistical approach to communication engineering already familiar to a large part of the statisticians and communication engineers of the United States and Great Britain. It also saw my government document, now out of print, and a considerable number of expository papers by Levinson,[8] Wallman, Daniell, Phillips, and others written to fill the gap. I myself have had a long mathematical expository paper underway for several years to put the work I have done on permanent record, but circumstances not completely under my control have prevented its prompt publication. Finally, after a joint meeting at the American Mathematical Society and the Institute of Mathematical Statistics held in New York in the spring of 1947, and devoted to the study of stochastic processes from a point of view closely allied to cybernetics, I have passed on what I have already written of my manuscript to Professor Doob of the University of Illinois, to be developed in his notation and according to his ideas as a book for the Mathematical Surveys series of the American Mathematical Society. I had already developed part of my work in a course of lectures in the mathematics department of M.I.T. in the summer of 1945. Since then, my old student and collaborator,[9] Dr. Y. W. Lee, has returned from China. He is giving a course on the new methods for the design of wave filters and similar apparatus in the M.I.T. electrical engineering department in the fall of 1947, and has plans to work the material of these lectures up into a book. At the same time, the out-of-print government document is to be reprinted.[10]

As I have said, Dr. Rosenblueth returned to Mexico about the beginning of 1944. In the spring of 1945, I received an invitation from the Mexican Mathematical Society to participate in a meeting to be held in Guadalajara that June. This invitation was reinforced by the Comision Instigadora y Coordinadora de la Investigación Cientifica, under the leadership of Dr. Manuel Sandoval Vallarta, of whom I have already spoken. Dr. Rosenblueth invited me to share some scientific research with him, and the Instituto Nacional de Cardiología, under its director Dr. Ignacio Chávez, extended me its hospitality.

I stayed some ten weeks in Mexico at that time. Dr. Rosenblueth and I decided to continue a line of work which we had already discussed with Dr. Walter B. Cannon, who was also with Dr. Rosenblueth, on a visit which unfortunately proved to be his last. This work had to do with the relation between, on the one hand, the tonic, clonic, and phasic contractions in epilepsy and, on the other hand, the tonic spasm, beat, and fibrillation of the heart. We felt that heart muscle represented an irritable tissue as useful for the investigation of conduction mechanisms as nerve tissue, and furthermore, that the anastomoses and decussations of the heart-muscle fibers presented us with a simpler phenomenon than the problem of the nervous synapse. We were also deeply grateful to Dr. Chávez for his unquestioning hospitality, and, while it has never been the policy of the Instituto to restrict Dr. Rosenblueth to the investigation of the heart, we were grateful to have an opportunity to contribute to its principal purpose.

Our investigation took two directions: the study of phenomena of conductivity and latency in uniform conducting media of two or more dimensions, and the statistical study of the conducting properties of random nets of conducting fibers. The first led us to the rudiments of a theory of heart flutter, the latter to a certain possible understanding of fibrillation. Both lines of work were developed in a paper,[11] published by us, and, although in both cases our earlier results have shown the need of a considerable amount of revision and of supplementation, the work on flutter is being revised by Mr. Oliver G. Selfridge of the Massachusetts Institute of Technology, while the statistical technique used in the study of heart-muscle nets has been extended to the treatment of neuronal nets by Mr. Walter Pitts, now a Fellow of the John Simon Guggenheim Foundation. The experimental work is being carried on by Dr. Rosenblueth with the aid of Dr. F. Garcia Ramos of the Instituto Nacional de Cardiología and the Mexican Army Medical School.

At the Guadalajara meeting of the Mexican Mathematical Society, Dr. Rosenblueth and I presented some of our results. We had already come to the conclusion that our earlier plans of collaboration had shown themselves to be practicable. We were fortunate enough to have a chance to present our results to a larger audience. In the spring of 1946, Dr. McCulloch had made arrangements with the Josiah Macy Foundation for the first of a series of meetings to be held in New York and to be devoted to the problems of feedback. These meetings have been conducted in the traditional Macy way, worked out most efficiently by Dr. Frank Fremont-Smith, who organized them on behalf of the Foundation. The idea has been to get together a group of modest size, not exceeding some twenty in number, of workers in various related fields, and to hold them together for two successive days in all-day series of informal papers, discussions, and meals together, until they had had the opportunity to thresh out their differences and to make progress in thinking along the same lines. The nucleus of our meetings has been the group that had assembled in Princeton in 1944, but Drs. McCulloch and Fremont-Smith have rightly seen the psychological and sociological implications of the subject, and have co-opted into the group a number of leading psychologists, sociologists, and anthropologists. The need of including psychologists had indeed been obvious from the beginning. He who studies the nervous system cannot forget the mind, and he who studies the mind cannot forget the nervous system. Much of the psychology of the past has proved to be really nothing more than the physiology of the organs of special sense; and the whole weight of the body of ideas which cybernetics is introducing into psychology concerns the physiology and anatomy of the highly specialized cortical areas connecting with these organs of special sense. From the beginning, we have anticipated that the problem of the perception of *Gestalt*, or of the perceptual formation of universals, would prove to be of this nature. What is the mechanism by which we recognize a square as a square, irrespective of its position, its size, and its orientation? To assist us in such matters and to inform them of whatever use might be made of our concepts for their assistance, we had among us such psychologists as Professor Klüver of the University of Chicago, the late Dr. Kurt Lewin of the Massachusetts Institute of Technology, and Dr. M. Ericsson of New York.

As to sociology and anthropology, it is manifest that the importance of information and communication as mechanisms of organization proceeds beyond the individual into the community. On the one hand, it is completely impossible to understand social communities such as those of ants without a, thorough investigation of their means of communication, and we were fortunate enough to have the aid of Dr. Schneirla in this matter. For the similar problems of human organization, we sought help from the anthropologists Drs. Bateson and Margaret Mead; while Dr. Morgenstern of the Institute for Advanced Study was our adviser in the significant field of social organization belonging to economic theory. His very important joint book on games with Dr. von Neumann, by the way, represents a most interesting study of social organization from the point of view of methods closely related to, although distinct from, the subject matter of cybernetics. Dr. Lewin and others represented the newer work on the theory of opinion sampling and the practice of opinion making, and Dr. F. C. S. Northrup was interested in assaying the philosophical significance of our work.

This does not purport to be a complete list of our group. We also enlarged the group to contain more engineers and mathematicians such as Bigelow and Savage, more neuroanatomists and neurophysiologists such as von Bonin and Lloyd, and so on. Our first meeting, held in the spring of 1946, 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. It was the sense of the meeting that the ideas behind cybernetics were sufficiently important and interesting to those present to warrant a continuation of our meetings at intervals of six months; and that before the next full meeting, we should have a small meeting for the benefit of the less mathematically trained to explain to them in as simple language as possible the nature of the mathematical concepts involved.

In the summer of 1946, I returned to Mexico with the support of the Rockefeller Foundation and the hospitality of the Instituto Nacional de Cardiología to continue the collaboration between Dr. Rosenblueth and myself. This time we decided to take a nervous problem directly from the topic of feedback and to see what we could do with it experimentally. We chose the cat as our experimental animal, and the quadriceps extensor femoris as the muscle to study. We cut the attachment of the muscle, fixed it to a lever under known tension, and recorded its contractions isometrically or isotonically. We also used an oscillograph to record the simultaneous electrical changes in the muscle itself. We worked chiefly with cats, first decerebrated under ether anesthesia and later made spinal by a thoracic transection of the cord. In many cases, strychnine was used to increase the reflex responses. The muscle was loaded to the point where a tap would set it into a periodic pattern of contraction, which is called *clonus* in the language of the physiologist. We observed this pattern of contraction, paying attention to the physiological condition of the cat, the load on the muscle, the frequency of oscillation, the base level of the oscillation, and its amplitude. These we tried to analyze as we should analyze a mechanical or electrical system exhibiting the same pattern of hunting. We employed, for example, the methods of MacColl's book on servomechanisms. This is not the place to discuss the full significance of our results, which we are now repeating and preparing to write up for publication. However, the following statements are either established or very probable: that the frequency of clonic oscillation is much less sensitive to changes of the loading conditions than we had expected, and that it is much more nearly determined by the constants of the closed arc (efferent-nerve)–muscle–(kinesthetic-end-body)–(afferent-nerve)–(central-synapse)–(efferent-nerve) than by anything else. This circuit is not even approximately a circuit of linear operators if we take as our base of linearity the number of impulses transmitted by the efferent nerve per second, but seems to become much more nearly so if we replace the number of impulses by its logarithm. This corresponds to the fact that the form of the envelope of stimulation of the efferent nerve is not nearly sinusoidal, but that the logarithm of this curve is much more nearly sinusoidal; while in a linear oscillating system with constant energy level, the form of the curve of stimulation must be sinusoidal in all except a set of cases of zero probability. Again, the notions of facilitation and inhibition are much more nearly multiplicative than additive in nature. For example, a complete inhibition means a multiplication by zero, and a partial inhibition means a multiplication by a small quantity. It is these notions of inhibition and facilitation which have been used[12] in the discussion of the reflex arc. Furthermore, the synapse is a coincidence-recorder, and the outgoing fiber is stimulated only if the number of incoming impulses in a small summation time exceeds a certain threshold. If this threshold is low enough in comparison with the full number of incoming synapses, the synaptic mechanism serves to multiply probabilities, and that it can be even an approximately linear link is possible only in a logarithmic system. This approximate logarithmicity of the synapse mechanism is certainly allied to the approximate logarithmicity of the Weber-Fechner law of sensation intensity, even though this law is only a first approximation.

The most striking point is that on this logarithmic basis, and with data obtained from the conduction of single pulses through the various elements of the neuromuscular arc, we were able to obtain very fair approximations to the actual periods of clonic vibration, using the technique already developed by the servo engineers for the determination of the frequencies of hunting oscillations in feedback systems which have broken down. We obtained theoretical oscillations of about 13.9 per second, in cases where the observed oscillations varied between frequencies of 7 and 30, but generally remained within a range varying somewhere between 12 and 17. Under the circumstances, this agreement is excellent.

The frequency of clonus is not the only important phenomenon which we may observe: there is also a relatively slow change in basal tension, and an even slower change in amplitude. These phenomena are certainly by no means linear. However, sufficiently slow changes in the constants of a linear oscillating system may be treated to a first approximation as though they were infinitely slow, and as though over each part of the oscillation the system behaved as it would if its parameters were those belonging to it at the time. This is the method known in other branches of physics as that of secular perturbations. It may be used to study the problems of base level and amplitude of clonus. While this work has not yet been completed, it is clear that it is both possible and promising. There is a strong suggestion that though the timing of the main arc in clonus proves it to be a two-neuron arc, the amplification of impulses in this arc is variable in one and perhaps in more points, and that some part of this amplification may be affected by slow, multineuron processes which run much higher in the central nervous system than the spinal chain primarily responsible for the timing of clonus. This variable amplification may be affected by the general level of central activity, by the use of strychnine or of anesthetics, by decerebration, and by many other causes.

These were the main results presented by Dr. Rosenblueth and myself at the Macy meeting held in the autumn of 1946, and in a meeting of the New York Academy of Sciences held at the same time for the purpose of diffusing the notions of cybernetics over a larger public. While we were pleased with our results, and fully convinced of the general practicability of work in this direction, we felt nevertheless that the time of our collaboration had been too brief, and that our work had been done under too much pressure to make it desirable to publish without further experimental confirmation. This confirmation—which naturally might amount to a refutation—we are now seeking in the summer and autumn of 1947.

The Rockefeller Foundation had already given Dr. Rosenblueth a grant for the equipment of a new laboratory building at the Instituto Nacional de Cardiología. We felt that the time was now ripe for us to go jointly to them—that is, to Dr. Warren Weaver, in charge of the department of physical sciences, and to Dr. Robert Morison, in charge of the department of medical sciences—to establish the basis of a long-time scientific collaboration, in order to carry on our program at a more leisurely and healthy pace. In this we were enthusiastically backed by our respective institutions. Dr. George Harrison, Dean of Science, was the chief representative of the Massachusetts Institute of Technology during these negotiations, while Dr. Ignacio Chávez spoke for his institution, the Instituto Nacional de Cardiología. During the negotiations, it became clear that the laboratory center of the joint activity should be at the Instituto, both in order to avoid the duplication of laboratory equipment and to further the very real interest the Rockefeller Foundation has shown in the establishment of scientific centers in Latin America. The plan finally adopted was for five years, during which I should spend six months of every other year at the Instituto, while Dr. Rosenblueth would spend six months of the intervening years at the Institute. The time at the Instituto is to be devoted to the obtaining and elucidation of experimental data pertaining to cybernetics, while the intermediate years are to be devoted to more theoretical research and, above all, to the very difficult problem of devising, for people wishing to go into this new field, a scheme of training which will secure for them both the necessary mathematical, physical, and engineering background and the proper acquaintance with biological, psychological, and medical techniques.

In the spring of 1947, Dr. McCulloch and Mr. Pitts did a piece of work of considerable cybernetic importance. Dr. McCulloch had been given the problem of designing an apparatus to enable the blind to read the printed page by ear. The production of variable tones by type through the agency of a photocell is an old story, and can be effected by any number of methods; the difficult point is to make the pattern of the sound substantially the same when the pattern of the letters is given, whatever the size. This is a definite analogue of the problem of the perception of form, of *Gestalt*, which allows us to recognize a square as a square through a large number of changes of size and of orientation. Dr. McCulloch's device involved a selective reading of the type imprint for a set of different magnifications. Such a selective reading can be performed automatically as a scanning process. This scanning, to allow a comparison between a figure and a given standard figure of fixed but different size, was a device which I had already suggested at one of the Macy meetings. A diagram of the apparatus by which the selective reading was done came to the attention of Dr. von Bonin, who immediately asked, "Is this a diagram of the fourth layer of the visual cortex of the brain?" Acting on this suggestion, Dr. McCulloch, with the assistance of Mr. Pitts, produced a theory tying up the anatomy and the physiology of the visual cortex, and in this theory the operation of scanning over a set of transformations plays an important part. This was presented in the spring of 1947, both at the Macy meeting and at a meeting of the New York Academy of Sciences. Finally, this scanning process involves a certain periodic time, which corresponds to what we call the "time of sweep" in ordinary television. There are various anatomic clues to this time in the length of the chain of consecutive synapses necessary to run around one cycle of performance. These yield a time of the order of a tenth of a second for a complete performance of the cycle of operations, and this is the approximate period of the so-called "alpha rhythm" of the brain. Finally, the alpha rhythm, on quite other evidence, has already been conjectured to be of visual origin and to be important in the process of form perception.

In the spring of 1947, I received an invitation to participate in a mathematical conference in Nancy on problems arising from harmonic analysis. I accepted and, on my voyage there and back, spent a total of three weeks in England, chiefly as a guest of my old friend Professor J. B. S. Haldane. I had an excellent chance to meet most of those doing work on ultra-rapid computing machines, especially at Manchester and at the National Physical Laboratories at Teddington, and above all to talk over the fundamental ideas of cybernetics with Mr. Turing at Teddington. I also visited the Psychological Laboratory at Cambridge, and had a very good chance to discuss the work that Professor F. C. Bartlett and his staff were doing on the human element in control processes involving such an element. I found the interest in cybernetics about as great and well informed in England as in the United States, and the engineering work excellent, though of course limited by the smaller funds available. I found much interest and understanding of its possibility in many quarters, and Professors Haldane, H. Levy, and Bernal certainly regarded it as one of the most urgent problems on the agenda of science and scientific philosophy. I did not find, however, that as much progress had been made in unifying the subject and in pulling the various threads of research together as we had made at home in the States.

In France, the meeting at Nancy on harmonic analysis contained a number of papers uniting statistical ideas and ideas from communication engineering in a manner wholly in conformity with the point of view of cybernetics. Here I must mention especially the names of M. Blanc-Lapierre and M. Loève. I found also a considerable interest in the subject on the part of mathematicians, physiologists, and physical chemists, particularly with regard to its thermodynamic aspects in so far as they touch the more general problem of the nature of life itself. Indeed, I had discussed that subject in Boston, before my departure, with Professor Szent-Györgyi, the Hungarian biochemist, and had found his ideas concordant with my own.

One event during my French visit is particularly worthwhile noting here. My colleague, Professor G. de Santillana of M.I.T., introduced me to M. Freymann, of the firm of Hermann et Cie, and he requested of me the present book. I am particularly glad to receive his invitation, as M. Freymann is a Mexican, and the writing of the present book, as well as a good deal of the research leading up to it, has been done in Mexico.

As I have already hinted, one of the directions of work which the realm of ideas of the Macy meetings has suggested concerns the importance of the notion and the technique of communication in the social system. It is certainly true that the social system is an organization like the individual, that it is bound together by a system of communication, and that it has a dynamics in which circular processes of a feedback nature play an important part. This is true, both in the general fields of anthropology and of sociology and in the more specific field of economics; and the very important work, which we have already mentioned, of von Neumann and Morgenstern on the theory of games enters into this range of ideas. On this basis, Drs. Gregory Bateson and Margaret Mead have urged me, in view of the intensely pressing nature of the sociological and economic problems of the present age of confusion, to devote a large part of my energies to the discussion of this side of cybernetics.

Much as I sympathize with their sense of the urgency of the situation, and much as I hope that they and other competent workers will take up problems of this sort, which I shall discuss in a later chapter of this book, I can share neither their feeling that this field has the first claim on my attention, nor their hopefulness that sufficient progress can be registered in this direction to have an appreciable therapeutic effect in the present diseases of society. To begin with, the main quantities affecting society are not only statistical, but the runs of statistics on which they are based are excessively short. There is no great use in lumping under one head the economics of steel industry before and after the introduction of the Bessemer process, nor in comparing the statistics of rubber production before and after the burgeoning of the automobile industry and the cultivation of *Hevea* in Malaya. Neither is there any important point in running statistics of the incidence of venereal disease in a single table which covers both the period before and that after the introduction of salvarsan, unless for the specific purpose of studying the effectiveness of this drug. For a good statistic of society, we need long runs *under essentially constant conditions*, just as for a good resolution of light we need a lens with a large aperture. The effective aperture of a lens is not appreciably increased by augmenting its nominal aperture, *unless the lens is made of a material so homogeneous that the delay of light in different parts of the lens conforms to the proper designed amount by less than a small part of* *a wavelength. Similarly, the advantage of long runs of statistics under widely varying conditions is specious and spurious.* Thus the human sciences are very poor testing-grounds for a new mathematical technique: as poor as the statistical mechanics of a gas would be to a being of the order of size of a molecule, to whom the fluctuations which we ignore from a larger standpoint would be precisely the matters of greatest interest. Moreover, in the absence of reasonably safe routine numerical techniques, the element of the judgment of the expert in determining the estimates to be made of sociological, anthropological, and economic quantities is so great that it is no field for a newcomer who has not yet had the bulk of experience which goes to make up the expert. I may remark parenthetically that the modern apparatus of the theory of small samples, once it goes beyond the determination of its own specially defined parameters and becomes a method for positive statistical inference in new cases, does not inspire me with any confidence unless it is applied by a statistician by whom the main elements of the dynamics of the situation are either explicitly known or implicitly felt.

I have just spoken of a field in which my expectations of cybernetics are definitely tempered by an understanding of the limitations of the data which we may hope to obtain. There are two other fields where I ultimately hope to accomplish something practical with the aid of cybernetic ideas, but in which this hope must wait on further developments. One of these is the matter of prostheses for lost or paralyzed limbs. As we have seen in the discussion of *Gestalt*, the ideas of communication engineering have already been applied by McCulloch to the problem of the replacement of lost senses, in the construction of an instrument to enable the blind to read print by hearing. Here the instrument suggested by McCulloch takes over quite explicitly some of the functions not only of the eye but of the visual cortex. There is a manifest possibility of doing something similar in the case of artificial limbs. The loss of a segment of limb implies not only the loss of the purely passive support of the missing segment or its value as mechanical extension of the stump, and the loss of the contractile power of its muscles, but implies as well the loss of all cutaneous and kinesthetic sensations originating in it. The first two losses are what the artificial-limbmaker now tries to replace. The third has so far been beyond his scope. In the case of a simple peg leg, this is not important: the rod that replaces the missing limb has no degrees of freedom of its own, and the kinesthetic mechanism of the stump is fully adequate to report its own position and velocity. This is not the case with the articulated limb with a mobile knee and ankle, thrown ahead by the patient with the aid of his remaining musculature. He has no adequate report of their position and motion, and this interferes with his sureness of step on an irregular terrain. There does not seem to be any insuperable difficulty in equipping the artificial joints and the sole of the artificial foot with strain or pressure gauges, which are to register electrically or otherwise, say through vibrators, on intact areas of skin. The present artificial limb removes some of the paralysis caused by the amputation but leaves the ataxis. With the use of proper receptors, much of this ataxia should disappear as well, and the patient should be able to learn reflexes, such as those we all use in driving a car, which should enable him to step out with a much surer gait. What we have said about the leg should apply with even *more* force to the arm, where the figure of the manikin familiar to all readers of books of neurology shows that the sensory loss in an amputation of the thumb alone is considerably greater than the sensory loss even in a hip-joint amputation.

I have made an attempt to report these considerations to the proper authorities, but up to now I have not been able to accomplish much. I do not know whether the same ideas have already emanated from other sources, nor whether they have been tried out and found technically impracticable. In case they have not yet received a thorough practical consideration, they should receive one in the immediate future.

Let me now come to another point which I believe to merit attention. It has long been clear to me that the modern ultra-rapid computing machine was in principle an ideal central nervous system to an apparatus for automatic control; and that its input and output need not be in the form of numbers or diagrams but might very well be, respectively, the readings of artificial sense organs, such as photoelectric cells or thermometers, and the performance of motors or solenoids. With the aid of strain gauges or similar agencies to read the performance of these motor organs and to report, to "feed back," to the central control system as an artificial kinesthetic sense, we are already in a position to construct artificial machines of almost any degree of elaborateness of performance. Long before Nagasaki and the public awareness of the atomic bomb, it had occurred to me that we were here in the presence of another social potentiality of unheard-of importance for good and for evil. The automatic factory and the assembly line without human agents are only so far ahead of us as is limited by our willingness to put such a degree of effort into their engineering as was spent, for example, in the development of the technique of radar in the Second World War.[13]

I have said that this new development has unbounded possibilities for good and for evil. For one thing, it makes the metaphorical dominance of the machines, as imagined by Samuel

Butler, a most immediate and non-metaphorical problem. It gives the human race a new and most effective collection of mechanical slaves to perform its labor. Such mechanical labor has most of the economic properties of slave labor, although, unlike slave labor, it does not involve the direct demoralizing effects of human cruelty. However, any labor that accepts the conditions of competition with slave labor accepts the conditions of slave labor, and is essentially slave labor. The key word of this statement is *competition*. It may very well be a good thing for humanity to have the machine remove from it the need of menial and disagreeable tasks, or it may not. I do not know. It cannot be good for these new potentialities to be assessed in the terms of the market, of the money they save; and it is precisely the terms of the open market, the "fifth freedom," that have become the shibboleth of the sector of American opinion represented by the National Association of Manufacturers and the Saturday Evening Post. I say American opinion, for as an American, I know it best, but the hucksters recognize no national boundary.

Perhaps I may clarify the historical background of the present situation if I say that the first industrial revolution, the revolution of the "dark satanic mills," was the devaluation of the human arm by the competition of machinery. There is no rate of pay at which a United States pick-and-shovel laborer can live which is low enough to compete with the work of a steam shovel as an excavator. The modern industrial revolution is similarly bound to devalue the human brain, at least in its simpler and more routine decisions. Of course, just as the skilled carpenter, the skilled mechanic, the skilled dressmaker have in some degree survived the first industrial revolution, so the skilled scientist and the skilled administrator may survive the second. However, taking the second revolution as accomplished, the average human being of mediocre attainments or less has nothing to sell that it is worth anyone's money to buy.

The answer, of course, is to have a society based on human values other than buying or selling. To arrive at this society, we need a good deal of planning and a good deal of struggle, which, if the best comes to the best, may be on the plane of ideas, and otherwise—who knows? I thus felt it my duty to pass on my information and understanding of the position to those who have an active interest in the conditions and the future of labor, that is, to the labor unions. I did manage to make contact with one or two persons high up in the C.I.O., and from them I received a very intelligent and sympathetic hearing. Further than these individuals, neither I nor any of them was able to go. It was their opinion, as it had been my previous observation and information, both in the United States and in England, that the labor unions and the labor movement are in the hands of a highly limited personnel, thoroughly well trained in the specialized problems of shop stewardship and disputes concerning wages and conditions of work, and totally unprepared to enter into the larger political, technical, sociological, and economic questiqns which concern the very existence of labor. The reasons for this are easy enough to see: the labor union official generally comes from the exacting life of a workman into the exacting life of an administrator without any opportunity for a broader training; and for those who have this training, a union career is not generally inviting; nor, quite naturally, are the unions receptive to such people.

Those of us who have contributed to the new science of cybernetics thus stand in a moral position which is, to say the least, not very comfortable. We have contributed to the initiation of a new science which, as I have said, embraces technical developments with great possibilities for good and for evil. We can only hand it over into the world that exists about us, and this is the world of Belsen and Hiroshima. We do not even have the choice of suppressing these new technical developments. They belong to the age, and the most any of us can do by suppression is to put the development of the subject into the hands of the most irresponsible and most venal of our engineers. The best we can do is to see that a large public understands the trend and the bearing of the present work, and to confine our personal efforts to those fields, such as physiology and psychology, most remote from war and exploitation. As we have seen, there are those who hope that the good of a better understanding of man and society which is offered by this new field of work may anticipate and outweigh the incidental contribution we are making to the concentration of power (which is always concentrated, by its very conditions of existence, in the hands of the most unscrupulous). I write in 1947, and I am compelled to say that it is a very slight hope.

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Instituto Nacional de Cardiología,

Ciudad de México
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# Cybernetics or Control and Communication in the Animal and the Machine

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