NORBERT WIENER AND THE GROWTH OF NEGATIVE FEEDBACK IN SCIENTIFIC EXPLANATION; WITH A PROPOSED RESEARCH PROGRAM OF "CYBERNETIC ANALYSIS"

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Negative feedback has become ubiquitous in science both as a technique and as a conceptual tool. As a technique, negative feedback has a long history; devices based in its use were made in antiquity. It has only been during the last century, however, that rigorous quantitiative methods have become associated with the applications of negative feedback. These methods originated in communications engineering and during the World War II period spread rapidly to other areas of science where further applications were soon made. During this process of dissemination negative feedback was transformed into a powerful conceptual tool, of general application, having to do with the organization of behavior.

The central figure responsible for both the dissemination and transformation of negative feedback was the American mathematician, Norbert Wiener, who, as a child prodigy, had developed graduate level proficiency in science, mathematics and philosophy before he

was twenty. Wiener's multidisciplinary background and interests were critically important in allowing him to interact with professionals in many different fields and thereby to disseminate the feedback ideas.

Wiener and two colleagues were the authors of the 1943 paper, "Behavior, Purpose and Teleology," which stimulated a number of interdisciplinary meetings. These meetings were important in spreading the feedback concepts to the different disciplines. Participating in these meetings were, among others, Gregory Bateson, Wolfgang Köhler, Margaret Mead, Warren S. McCulloch, F. S. C. Northrop, John von Neumann and Wiener. The successful assimilation of feedback by the various disciplines in spite of the problems associated with modern discipline specialization provides a lesson in how these problems may be overcome. In the case of feedback, the climate for its assimilation was made considerably more receptive by concurrent developments in computer science and neurophysiology which mutually reinforced the robotic view.

The role of negative feedback in scientific research and the significance of this role have not yet been fully identified. Such an identification must be made in order to evaluate the historical events which led to the assimilation of negative feedback. I attempt to define the role of negative feedback in scientific research in terms of a program called "cybernetic analysis." This program develops the behavioral and functional roles of negative feedback in terms of "adaptive goal-directed behavior"; such behavior occurs when a system can maintain a certain state or tend toward a certain state even while being disturbed by external influences. This behavior is exhibited both by organisms and by mechanical devices controlled by negative feedback.

Until now the idea that systems could be directed toward an end has been unacceptable because goal-directedness has been associated with the outdated notions of teleology and final cause. The ability of negative feedback to account for goal-directedness mechanistically not only challenges the view that organisms alone can exhibit such behavior, but also stands to revise the scientific view of goal-directedness in general. With the new legitimacy of both adaptive and non-adaptive goal-directedness, the path is opened for more effective analysis of scientific problems.

Despite the great value of Wiener's Cybernetics in focusing attention on the many new robotic developments of the World War II period, it tended to obscure many of the critical points made in the earlier (1943) paper with regard to the role of negative feedback in scientific explanation. The term "cybernetics" came to be a great source of confusion because of Wiener's initial presentation, a presentation which mirrored many of the earlier events in the interdisciplinary meetings which led to the writing of the work. It is suggested here that the term "cybernetic analysis" be used to designate that type of problem analysis which utilizes the hypothesis of a negative feedback mechanism to account for adaptive goal-directed behavior. The use of the term "cybernetics" in this manner will not only succinctly identify one of the great unnamed developments in science, but give the word renewed meaning in terms of the literal roots from which Wiener first derived it.

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ACKNOWLEDGEMENTS

Scientific laboratory research and historical research both require exceptional resources if high quality work is to be performed. Whereas the laboratory scientist cannot expect to obtain precise measurements with poor equipment, the historian cannot expect to make a worthwhile study without access to the many people, books, documents and papers which relate to the subject matter. I feel extremely fortunate to have had access to such resources.

Although Norbert Wiener died in 1964, many of the people he worked with are still active and some of these generously gave of their time so that I could interview them personally and on the telephone. My thanks, then, go to Prof. Julian Bigelow, Dr. G. E. Hutchinson, Dr. George Kreezer, Dr. Jerome Lettvin, Dr. William T. Martin, Prof. P. Masani, Mr. Oliver Selfridge, and Dr. Heinz von Foerster, all of whom helped me greatly. I would especially like to acknowledge my debt to Prof. Bigelow. Besides giving me a vast amount of his time, he reinforced my view directly that the principals of this study were an extraordinarily talented group of people. I especially appreciate his help on the details of the fire control project. Dr. von Foerster, being one of my first interview subjects, had to contend with some very broad and at times unfocused questions. His graciousness in filling me in on a vast number of events is greatly appreciated. He has also provided me with some extremely useful unpublished documents. The less formal, but nevertheless very helpful, conversations I had with

Steve J. Heims, H. H. Goldstine and Mrs. Rook McCulloch are also greatly appreciated. I note in passing that although there may be problems with doing the history of modern science, there are also unique advantages. The historian of the seventeenth century cannot interview the subjects of his study. Although personal interviews remain the best approach to interviewing, use of the telephone is invaluable when travel is impossible. I recommend this procedure to historians of modern science.

Other personal resources which enabled the completion of this study include the history of science faculty at Oregon State University who introduced me to the history of science in all its diversity. My thanks here go to Drs. Paul Farber, R. J. Morris, Dan Jones, and J. Brookes Spencer. As my major professor for this work, Dr. Spencer has been invaluable in helping me to clarify the expression of my thoughts. I greatly appreciate the great amount of work he did toward the end of the project when we were under a crushing schedule.

My principal resource for published works has been the Oregon State University Library. The role of a library is critical in a work such as this and I was very fortunate to have such a good library close at hand. Michael Kinch and Donald Unger made it even more pleasant to use.

This study has been vastly aided by the multitude of Wiener's papers available at the Institute Archives and Special Collections of the Libraries of the Massachusetts Institute of Technology. I would like to thank archivists Jane McCavitt and Helen Slotkin who helped me get to the materials quickly and efficiently. Mr. C. Dewing of the Polar and Scientific Archives Division of the National Archives helped me to locate some very useful Wiener documents, and Mr. Stephen Catlett of the American Philosophical Society in Philadelphia helped me with the papers of Warren S. McCulloch. Dr. Eugene Fichter of the College of Engineering, Oregon State University helped me greatly in pursuing the topic of robotics.

Production of this work has been made much more pleasant by conversations with Joel Hagen and Pete Hokanson. I would like to thank my brother Arthur Hellman, not only for his moral and critical support, but also for helping to defray the costs of production of this work. Leona Nicholson took on the burden of typing a manuscript with bottom footnotes rather than end notes. I know that I, and I hope the reader will, appreciate this effort.

No matter how excellent the resources or interesting the subject matter, one cannot do good work unless the desire to do it exists. I have been inspired by the environment of the Pacific Northwest, but more personally by the wonderful home life provided by my wife Roberta and our two children. In the midst of pursuing her own career and doing her part in the care of the children, Roberta has managed to give me all the extra time I needed to pursue this project. As any parents who have been in the situation of raising children while pursuing careers will know, this was a most generous gift. Finally, my deepest appreciation goes to my parents who set an example I am still trying to follow.

Norbert Wiener

The M.I.T. Museum

TABLE OF CONTENTS

Chapter

LIST OF FIGURES

NORBERT WIENER AND THE GROWTH OF NEGATIVE FEEDBACK IN SCIENTIFIC EXPLANATION; WITH A PROPOSED RESEARCH PROGRAM OF "CYBERNETIC ANALYSIS"

CHAPTER ONE

INTRODUCTION

Negative feedback has become ubiquitous in science both as a technique and as a conceptual tool.[1] As a technique, negative

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[1]It is not possible to document here the enormous number of specific applications of negative feedback which have occurred and are occurring in the various scientific disciplines. Nevertheless, some indication of the broad scope of the use of this technique can be given:

In the engineering fields, of course, the technique has become sufficiently established for there to be a large number of textbooks specifically devoted to feedback control; see, for example, Otto J. J. Smith, Feedback Control Systems (New York: McGraw-Hill Book Co., Inc., 1958). The largest utilization of feedback ("feedback" in this work will refer to negative feedback unless otherwise indicated) outside of engineering has been in the biological sciences. Here the use of feedback is not only substantial, but increasing. A computer search of recent biological literature (the BIOSIS data base) indicates that the number of items listed for which feedback was an important enough aspect of the research to be included in the title almost doubled between the periods 1969-1976 and 1976-(August) 1981. In the earlier period about 150 items per year were listed, while in the later period the number had increased to about 290 items per year. During the first half of this year alone more than 200 items are listed in Biological Abstracts under "feedback" as the key word in the research title (Biological Abstracts 71 pt. 8 [1981], p. 1917). Naturally, many more studies utilize feedback but do not include the term itself in the title of the research ("feedback" in the title of research usually, but not always, indicates negative feedback). The major fields of application for negative feedback in current biological research include molecular biology, physiology, psychology (biofeedback) and ecology.

Very useful overviews of particular applications of control theory with numerous further references include: H. Kalmus, ed., Regulation and Control in Living Systems (London: John Wiley &

feedback has a long history; devices based in its use were made in antiquity, but it has only been during the last century that rigorous quantitative methods have become associated with the applications of negative feedback. These methods originated in the field of communications engineering and during the World War II period spread rapidly to other areas of science where further applications were soon made. During this process of dissemination negative feedback was transformed into a powerful conceptual tool, of general application, having to do with the organization of behavior.

Sons, 1966); W. S. Yamamoto and J. B. Brodbeck, eds., Physiological Controls and Regulations (Philadelphia: W. B. Saunders Co., 1965); and Richard W. Jones, Principles of Biological Regulation: An Introduction to Feedback Systems (New York: Academic Press, 1973). The degree to which negative feedback has become assimilated in science is evident in the large number of introductory textbooks, in widely varying fields, which present the fundamentals of the technique. See, for example: David Kirk, Biology Today 2nd ed. (New York: Random House, 1975), pp. 443-445; and E. P. Odum, Fundamentals of Ecology 3rd. ed. (Philadelphia: W. B. Saunders Co., 1971), pp. 34-70.

Negative feedback has been utilized in the social sciences, although not as successfully and not to as great a degree as in the biological and engineering sciences. Two applications in political science include: Karl W. Dutsch, The Nerves of Government: Models of Political Communication and Control (London: The Crowell-Collier Pub. Co., The Free Press of Glencoe, 1963), and Stephen David Bryen, The Application of Cybernetic Analysis to the Study of International Politics (The Hague: Nijhoff, 1972). The use of the term "cybernetic analysis" in the latter work is not the same as its formal use in the present work.

Some other publications of general interest with respect to negative feedback, its use and implications, include: Walter Buckley, ed., Modern Systems Research for the Behavioral Scientist: A Sourcebook (Chicago: Aldine Pub. Co., 1968); the General Systems Yearbook (Ann Arbor, Michigan: Society for General Systems Research, 1956-1981); and the journal Cybernetica.

The study of the processes by which negative feedback became assimilated by sciences other than engineering is important because of the relevance of these processes for the problems associated with extreme specialization in science. We have become a society of specialists. This specialization makes difficult the transfer of techniques from one field to another due to the highly divergent training of the workers in various fields. The success of the dissemination of negative feedback across interdisciplinary lines in spite of these problems is thus valuable in demonstrating how these same problems may be overcome.

The first thesis of this dissertation is that the central figure in the dissemination of the negative feedback techniques was the American mathematician, Norbert Wiener. I will attempt to show that Wiener's unusual multidisciplinary background and interests, as well as his forcefulness of intellect, enabled him to cross the borders of specialization and propagate the negative feedback ideas into many diverse fields. In support of this thesis, I will explore in some detail Wiener's background and his involvement in the specific events which led to the spread of the feedback techniques.

But as noted above, the dissemination of the negative feedback techniques was accompanied by its transformation into a conceptual tool. It was not necessary for this transformation to occur; the technique might have been adopted without any realization on the part of those employing it that a movement of great significance was occurring in the wider application of feedback. It took a remarkable individual to initiate this larger understanding. The second thesis of this work is that it was, again, Norbert Wiener who was primarily responsible for initiating this transformation of the negative feedback technique into a broader conceptual tool. Thus Wiener is doubly the central figure of this work.

Although Wiener helped to initiate the transformation of negative feedback from a technique into a more general concept of even deeper significance, this significance is still greatly open to interpretation. That this is the case should not be in the least surprising. Interpretations of all ideas in the history of science are continually being renewed. The problem of interpretation is particularly great in this case because the concept is so recent that not very much in the way of interpretation has occurred. This situation is further aggravated by the fact that the significance of negative feedback has often, and, in fact, generally been lumped together with larger, more diffuse conceptions such as "cybernetics" and "general systems theory." Thus the concept of negative feedback has often not been properly considered on its own merits. In spite of the widespread use of negative feedback, then, the conceptual significance of this use remains to be fully evaluated. In the second chapter of this dissertation I develop and present an analysis of the deeper significance which may be seen to underlie the general application of the negative feedback methods. This presentation contains the third thesis of this dissertation. Briefly stated, it is that the use of negative feedback provides a bridge between holistic and reductionistic explanation, and that the conceptual understanding of this use makes possible a revision of the scientific view of goal-directedness in nature. To develop this thesis I first formally characterize the role of negative feedback in scientific research, a characterization which, to my knowledge, has not been previously made. This characterization takes the form of a research program (of which more presently) delineating the place of feedback in scientific research. In my analysis of the implications and underlying assumptions of this program I attempt to present what I believe to be the significance of negative feedback in explanation. This significance is then used as a perspective to evaluate Wiener's and others' roles in the transformation process. In short, some transformation of negative feedback from technique to conceptual tool occurred, but it has not been exactly clear what the implications, nature and significance of this transformation has been. In Chapter Two I have attempted to establish what this transformation was and then have used this result in interpreting the historical events which led to it.

Because of the complexity, richness, and recency of the subject material, the interpretation of the current importance of feedback, a non-historical endeavor, occupies a full chapter of this work, an unusual amount for an historical study; but without this interpretation an evaluation of the historical events would be made much more difficult. It is, of course, hoped that this interpretation will indeed help in clarifying the status of feedback as an explanation today. One of the primary functions of the history of science is to help shed light on contemporary problems. The present work has been undertaken with this view in mind.

Norbert Wiener, the central figure of this study, was an extraordinary individual. As a world-famous child prodigy, he had developed graduate level proficiency in science, mathematics and philosophy before he was twenty. Later, as a professor at the Massachusetts Institute of Technology, he became a world-ranking mathematician. His interests, however, were not limited to mathematics and he had a virtually insatiable desire to understand the basic problems of all fields. This desire is well-illustrated in his characteristic statement, "a clearly framed question which we cannot answer is an affront to the dignity of the human race."[2]

Wiener was the guiding force and one of the three authors of the 1943 paper, "Behavior, Purpose and Teleology,"[3] a paper which was critical in initiating both the dissemination and transformation of the negative feedback techniques. A major reason for the influence of the paper was the fact that it analyzed the role of negative feedback within a broader conceptual framework useful in many diverse fields of science. The paper prompted an important series of interdisciplinary meetings devoted to the discussion of possible applications of feedback in various sciences. These meetings not only helped bring about the assimilation of feedback in

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[2]N. Wiener to P. de Kruif, August 3, 1933, Norbert Wiener Papers (MC 22), Institute Archives and Special Collections, Massachusetts Institute of Technology Libraries, Cambridge, Massachusetts.

[3]Arturo Rosenblueth, Norbert Wiener and Julian Bigelow, "Behavior, Purpose and Teleology," Philosophy of Science 10 (1943), pp. 18-24.

diverse fields, but also helped stimulate Wiener to write Cybernetics,[4] his best known work, published in 1948, a work which, in turn, had much to do with the further spreading of the feedback ideas. Wiener and the series of events stimulated by the 1943 paper are thus central to the study of the dissemination and transformation of negative feedback.

Chapter Three is devoted to Wiener's unusual background and emphasizes those aspects of Wiener's upbringing and early formation of character which were most important in developing his multidisciplinary skills. These skills were crucial in enabling Wiener to interact with professionals in different scientific fields and thereby to spread the feedback ideas. One feature of this chapter is the presentation of extended excerpts of Wiener's letters to his parents as a post-graduate student while he was working under Bertrand Russell in Cambridge, England during the year 1913. Almost all of these letters, which give tremendous insight into Wiener's character and development, appear in print for the first time.

Chapter Four continues Wiener's development from his initial appointment as a mathematics professor at the Massachusetts Institute of Technology in 1919. Here the emphasis is first on the mathematical tools which Wiener developed and later utilized in

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[4] N. Wiener, Cybernetics, Or Control and Communication in the Animal and the Machine (New York and Paris: John Wiley & Sons, Inc. and Hermann et Cie, 1948; 2nd ed. (New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961).

formulating his cybernetic synthesis; a broad construct through which he presented his ideas on the significance of feedback. Wiener's collaboration with the young engineer, Julian Bigelow, during World War II is presented in great detail as it was from this collaboration that the wider applicability of feedback first became apparent to Wiener. The details of this collaboration have also not previously been published.

The view presented in the "Behavior, Purpose and Teleology" paper and which Wiener helped to advance in the 1940's, was that the behavior of certain automatic machines known as servomechanisms was similar in type to the purposeful, or goal-directed, behavior of living beings. The servomechanism behavior resulted from the incorporation of negative feedback within the structure of the devices and the suggestion was advanced in the paper that negative feedback might also be responsible for similar behavior in living beings. This suggestion regarding negative feedback can be categorized under the more general view of "robotics" which holds that human capabilities can be replicated mechanically. The proposals concerning the potential of negative feedback to explain living behavior stirred much interest in the 1940's, not only because of Wiener's effective involvement, but also because there were many other concurrent advances in robotics which created an especially fertile climate for the mutual reinforcement of developments in this field. These developments included not only advances in basic feedback theory, but also the construction of sophisticated servomechanisms and computers, and progress in neurophysiology.

Chapter Five is principally devoted to describing the robotic advances which were so important in establishing a receptive environment for the proposed expansion of the feedback methods. The importance of this environment, including Wiener's involvement, is also illustrated in this chapter by the inclusion of several "antecedent cases," writings which presented ideas strikingly similar to the ones Wiener proposed but which failed to be influential.

Finally, Chapter Six describes the series of meetings stimulated by the "Behavior, Purpose and Teleology" paper. These meetings were conscious attempts at a multidisciplinary solution to scientific problems, a highly unusual endeavor in an age which was already highly specialized. We observe in these meetings the problem of scientists from different disciplines attempting to assimilate and organize a virtual avalanche of technical development. Thus the study of these meetings is highly relevant to the problems stemming from the extreme specialization in modern science. But most importantly for this study, these meetings were the primary vehicle for the initial dissemination to and the early transformation of negative feedback by the larger scientific community. In this work, the meetings are discussed primarily from this perspective. Chapter Six ends with a discussion of the writing of Cybernetics in relation to its effect on the general assimilation of negative feedback.

With the appearance of Cybernetics, the concept of negative feedback became enmeshed with the many other robotic developments which were then emerging. During this process some of the important and extremely useful distinctions made in the earlier "Behavior, Purpose and Teleology" paper seem to have been lost. As a result, in spite of certain internal problems with the 1943 paper, it remains as one of the most penetrating insights into the role of negative feedback in scientific explanation. Chapter Two, then, commences with a brief synopsis of this important work. In the analysis which follows I discuss the strengths and limitations of the paper and from this analysis I construct a research program to formally identify the role of negative feedback in scientific research. In deference to Wiener's own derivation of the word "cybernetics" from the Greek name for an early class of feedback devices, I refer to the research program as "cybernetic analysis." Although the methodology of negative feedback has not, to my knowledge, been formalized in this manner before, the program itself does reflect the actual current use of negative feedback in scientific research. The formalization facilitates a discussion of the assumptions underlying the use of negative feedback and the significance of this use. In short, cybernetic analysis represents the formal abstraction of a role that negative feedback has already been playing in scientific research, and this formalization makes possible a fuller understanding of the significance of this role.

It should be noted that after Wiener derived the term "cybernetics," in terms of negative feedback, he went on to consider a much larger range of topics in his book _Cybernetics_. Thus the program of cybernetic analysis refers to the more limited literal meaning of the word rather than the broad class of topics which Wiener came to consider. Since Wiener first used the term in 1948, there has been a great deal of confusion surrounding the meaning of "cybernetics." As this work will show, the reasons for this confusion are directly related to the manner in which cybernetics was originally presented by Wiener, and the events leading to this presentation. In the conclusion I argue that the term can be given a more definite and useful meaning in terms of the program of cybernetic analysis as given in Chapter Two. This meaning emphasizes the role of negative feedback and hence the roots of the word as originally derived by Wiener.

In summary then, the three theses of this dissertation are as follows:

1. Norbert Wiener was the central figure responsible for the dissemination of the technique of negative feedback from engineering to the larger scientific community.

2. Norbert Wiener was the central figure responsible for the transformation of negative feedback from a technique used to control mechanical devices into a powerful conceptual tool, of general application, having to do with the organization of behavior.

3. The utilization of negative feedback may be interpreted to provide a bridge between holistic and reductionistic explanation, and this understanding leads to a radical revision of the scientific view of goal-directedness in nature.

These theses were developed during the course of my research. Initially my investigations were focused on the development of "cybernetics." It soon became apparent, however, that cybernetics was never a well-defined concept; its lack of definition made it a poor topic for historical investigation. Negative feedback, on the other hand, was a well-defined topic which was of critical importance in the developments relating to cybernetics and which also had, in my view, far-reaching implications by itself. These implications, however, have not been widely addressed. As a result, it took a great deal of effort to identify the topic of negative feedback and to extract it and its importance from the broad and very ill-defined history of what is generally termed cybernetics. To save the reader this same effort, I have chosen to first present a discussion of negative feedback and its importance as stated in the third thesis. This discussion will aid the reader in providing a perspective for interpreting the historical events which follow in the subsequent chapters. Thus the third thesis is presented first, in Chapter Two, to which we now turn.

CHAPTER TWO

THE ROLE AND SIGNIFICANCE OF NEGATIVE FEEDBACK IN SCIENTIFIC RESEARCH AND EXPLANATION

As described in the Introduction, it is best to initiate a discussion of the role and significance of negative feedback in scientific research with a close examination of the 1943 paper, "Behavior, Purpose and Teleology."[1] Besides Wiener, the two other authors of the paper were Arturo Rosenblueth, a prominent Harvard physiologist, and Julian Bigelow, at that time a young engineer who had recently joined Wiener to work on a war research project.[2] This remarkable little essay, barely seven pages in length, remains as one of the most penetrating insights into the relationship between behavior which appears purposeful and the principle of negative feedback. As noted, however, there are certain problems inherent in the paper as the reader may very well discover in reading the next section. Here the paper is presented in its own terms and without criticism. A critical analysis of the paper follows in section II.

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[1]Arturo Rosenblueth, Norbert Wiener and Julian Bigelow, "Behavior, Purpose and Teleology," Philosophy of Science 10 (1943), pp. 18-24.

[2]The circumstances surrounding the writing of this important paper are described in Chapter Three (see below, pp. 152-157).

I. "Behavior, Purpose and Teleology"

The authors state at the outset the two goals of their essay: one is "to define the behavioristic study of natural events and to classify behavior"; and the second is "to stress the importance of the concept of purpose."[3] Behavior is defined as "any change of an entity with respect to its surroundings."[4] A "behavioristic study" is equivalent to what today is sometimes termed a "black box" approach; that is, when input stimulii are applied to a system only the external changes of the system are measured. The behavioral study represents an "input-output" approach where the overt behavior is considered but no attempt is made to account for this behavior in terms of the system's internal structure, or "intrinsic organization," to use the term of the paper. A study of the system's intrinsic organization and its relation to the behavior of the system is termed by the authors a "functional study." The complimentary aspect of behavioral and functional studies will be important for the statement of cybernetic analysis.

Behavior is next subdivided into two classes: active and passive. Entities exhibiting active behavior have their own energy source so that the output behavior is not limited in energy by the

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[3]A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior," p. 18.

[4]Wiener was quite interested in the scientific program of "operationalism," related to behavioralism and initiated by the physicist P. W. Bridgman in the 1920's. Wiener and Bridgman both attended Josiah Royce's seminar on the scientific method when they were graduate students at Harvard (see below, pp. 129-130).

energy of the input, that is, the "input does not energize the output directly." In passive behavior, on the other hand, "all the energy in the output can be traced to the immediate input" (e.g., the throwing of an object), or else the object may control energy which remains external to it throughout the reaction (e.g., the soaring flight of a bird).[5]

A pattern followed throughout the paper is to define each level of behavior dichotomously and then to treat subsequently only one aspect of the dichotomy, as is illustrated in the authors' diagram of the classification scheme (Figure 2-1). In correspondence with this pattern, passive behavior is now disregarded while active behavior is subdivided into two subclasses termed "purposeful" and "purposeless." The authors state:

> The term purposeful is meant to denote that the act or behavior may be interpreted as directed to the attainment of a goal--i.e., to a final condition in which the behaving object reaches a definite correlation in time or in space with respect to another object or event. Purposeless behavior then is that which is not interpreted as directed to a goal.[6]

The authors defend this definition of purpose as a useful concept in spite of the operational vagueness of the phrase "may be interpreted." They say, in support of their view, that the notion of purpose is indispensable to human physiology where purpose, that is, attaining a goal, is a "physiological fact": we can do physically what we <u>intend</u> to do.[7]

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[5]A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior," p. 18.

[6]Ibid.

[7]Ibid., p. 19.

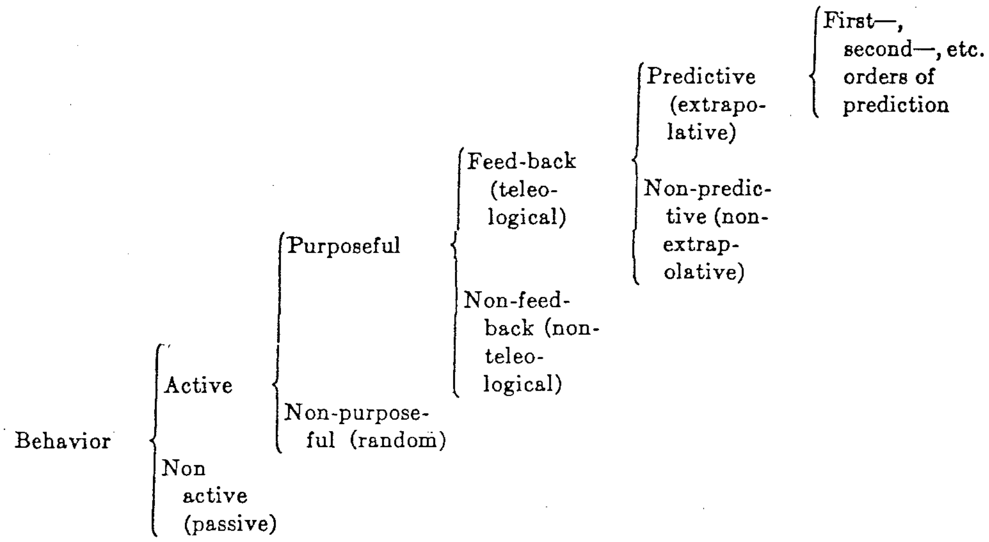

Figure 2-1. Classification of behavior in "Behavior, Purpose and Teleology" (reproduced from A. Rosenblueth, N. Wiener and J. Bigelow, "Behavior," p. 21).

Purposeless behavior is now disregarded while purposeful behavior is subdivided into two classes, teleological and non-teleological. It is in this subdivision that negative feedback first plays a role. The authors state:

> Purposeful active behavior may be subdivided into two classes: 'feed-back' (or 'teleological') and 'non-feed-back' (or 'non-teleological'). The expression feed-back is used by some engineers in two different senses. In a broad sense it may denote that some of the output energy of an apparatus or machine is returned as input; an example is the electrical amplifier with feed-back. The feed-back is in these cases positive---the fraction of the output which reenters the object has the same sign as the original input signal. Positive feed-back adds to the input signals, it does not correct them. The term feed-back is also employed in a more restricted sense to signify that the behavior of an object is controlled by the margin of error at which the object stands at a given time with reference to a relatively specific goal. The feed-back is then negative, that is, the signals from the goal are used to restrict outputs which would otherwise go beyond the goal. It is this second meaning of the term feed-back that is used here.[8]

An important clarification of the distinction between feed-back and non-feedback behavior follows:

> All [teleological] behavior may be considered to require negative feedback. If a goal is to be attained, some signals from the goal are necessary at some time to direct the behavior. By non-feed-back behavior is meant that in which there are no signals from the goal which modify the activity of the object <u>in the course of the behavior</u>.[9]

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[8] Ibid.

[9] Ibid., pp. 19-20. In the original text the term "purposeful" appears where "teleological" has been substituted in the above quoted statement. It appears that the use of the term "purposeful" was either an error or the result of a misprint since the authors had just defined purposeful behavior in terms of feedback <u>and</u> non-feed-back subclasses. This classification is confirmed by the diagram presented by the authors to illustrate their entire classification scheme for behavior (see fig. 2-1). It would seem from this diagram that the term "teleological" was intended, and thus this term has been here substituted for "purposeful."

An example is given to illustrate behavior that is non-feedback, but purposeful: when a snake strikes to catch its prey it launches its head at the prey on a trajectory from which it does not deviate. It cannot receive signals from the goal to modify this behavior because the launch is so rapid that there would not be adequate time for the new signal to be processed and acted upon by the snake. Thus the trajectory of the snake's head is directed toward the attainment of a goal, but it is not modified in the course of this behavior by changes in the position of the prey. This behavior is opposed to the "more effective" behavior of some machines and living organisms which employ "a continuous feed-back from the goal that modifies and guides the behaving object."[10]

A description of a feedback malfunction, "hunting," is then given, followed by a description of how this malfunction might be used to account for the tremor exhibited by patients with certain types of brain injuries (see below, p. 156). This observation leads to the statement that "The analogy with the behavior of a machine with undamped feed-back is so vivid that we venture to suggest that the main function of the cerebellum is the control of the feed-back nervous mechanisms involved in purposeful motor activity."[11]

Yet another subdivision of behavior is now made. Non-feedback purposeful behavior is disregarded while purposeful feedback (teleological) behavior is subdivided into predictive and

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[10]Ibid., p. 20.

[11]Ibid.

non-predictive subclasses. Predictive feedback behavior occurs when the object's behavior is not only influenced by feedback, but by predictions it makes concerning the future conditions of the goal. Thus, "A cat starting to pursue a running mouse does not run directly toward the region where the mouse is at any given time, but moves toward an extrapolated future position." The cat, of course, also reacts by feedback to changes in the mouse's position. An amoeba, on the other hand, seems to show no ability to extrapolate future conditions. The authors point out that predictive and non-predictive feedback behavior are both demonstrated by certain machines.[12] Since different applications require different degrees of complexity of prediction different orders of predictive feedback behavior are indicated. Different systems will be limited to differing degrees in the complexity of their extrapolations. Limitations might include the number, type and sensitivity of receptors linking the system with the environment, and also the complexity of "internal organization" with the system. Systems with highly limited receptors or internal organization would not be able to perform the complicated feedback-extrapolation operation necessary to enable, for example, the capture of a rapidly flying insect. With regard to internal organization, the authors state:

> Thus, it is likely that the nervous system of a rat or dog is such that it does not permit the integration of input and

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[12] Wiener and Bigelow were at this time engaged in a war-time research project, the goal of which was to produce a better method to aim antiaircraft guns. Predictive feedback was the critical element of their attempted solution (see Chapter Three, pp. 160-161).

> output necessary for the performance of a predictive reaction of the third or fourth order. Indeed, it is possible that one of the features of the discontinuity of behavior observable when comparing humans with other high mammals may lie in that the other mammals are limited to predictive behavior of a low order, whereas man may be capable potentially of quite high orders of prediction.[13]

In defending their overall classification scheme for behavior the authors state:

> It leads to the singling out of the class of predictive behavior, a class particularly interesting since it suggests the possibility of systematizing increasingly more complex tests of the behavior of organisms. It emphasizes the concepts of purpose and teleology, concepts which, although rather discredited at present, are shown to be important. Finally, it reveals that a uniform behavioristic analysis is applicable to both machines and living organisms, regardless of the complexity of the behavior. . . . A further comparison of living organisms and machines leads to the following inferences. Whether they should always be the same may depend on whether or not there are one or more qualitatively distinct, unique characteristics present in one group and absent in the other. Such qualitative differences have not appeared so far.[14]

Thus the advantages of the behavioral approach are seen to include first the definition of the useful concepts of purpose and teleology in scientifically testable terms, and second a unification of the description of behavior in both the life and physical sciences. The fact that machines and organisms can be described in terms of the same behavioral classifications is seen as being quite important, but at the same time it is emphasized that, at the present time, there are "deep" functional differences between the way in which machines and organisms achieve this similar behavior. Thus the authors observe:

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[13] Ibid., p. 21.

[14] Ibid., p. 22.

> Structurally, organisms are mainly colloidal, and include prominently protein molecules, large, complex and anisotropic; machines are chiefly metallic and include mainly simple molecules. From the standpoint of their energetics, machines usually exhibit relatively large differences of potential, which permit rapid mobilization of energy; in organisms the energy is more uniformly distributed, it is not very mobile. Thus, in electric machines conduction is mainly electronic, whereas in organisms electric changes are usually ionic.[15]

A further illustration of the functional differences responsible for similar behavior is given in terms of the eye and the television receiver:

> Scope and flexibility are achieved in machines largely by temporal multiplication of effects; frequencies of one million per second or more are readily obtained and utilized. In organisms, spatial multiplication, rather than temporal, is the rule; the temporal achievements are poor---the fastest nerve fibers can only conduct about one thousand impulses per second; spatial multiplication is on the other hand abundant and admirable in its compactness. This difference is well illustrated by the comparison of a television receiver and the eye. The television receiver may be described as a single cone retina; the images are formed by scanning---i.e. by orderly successive detection of the signal with a rate of about 20 million per second. Scanning is a process which seldom or never occurs in organisms, since it requires fast frequencies for effective performance. The eye uses a spatial, rather than a temporal multiplier. Instead of the one cone of the television receiver a human eye has about 6.5 million cones and about 115 million rods.[16]

Finally, a defense of the use of teleological concepts, as defined in the paper, is given. It is noted that, as defined, teleology is severed from the concept of final cause. At the same time, the useful concept of purpose is maintained:

> In classifying behavior the term 'teleology' was used as synonymous with 'purpose controlled by feed-back.' Teleology has been interpreted in the past to imply purpose and the vauge

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[15] Ibid., p. 23.

[16] Ibid.

> concept of a 'final cause' has been often added. This concept of final causes has led to the opposition of teleology to determinism. A discussion of causality, determinism and final causes is beyond the scope of this essay. It may be pointed out, however, that purposefulness, as defined here, is quite independent of causality, initial or final. Teleology has been discredited chiefly because it was defined to imply a cause subsequent in time to a given effect. When this aspect of teleology was dismissed, however, the associated recognition of the importance of purpose was also unfortunately discarded. Since we consider purposefulness a concept necessary for the understanding of certain modes of behavior we suggest that a teleological study is useful if it avoids problems of causality and concerns itself merely with an investigation of purpose.[17]

II. "Behavior, Purpose and Teleology": Analysis

Although the "Behavior, Purpose and Teleology" paper remains the most useful work in suggesting the role of feedback in scientific explanation, it is not entirely adequate to characterize this role completely. This inadequacy stems, in part, from the self-imposed restriction to the behavioral approach. While the authors made great progress in defining purpose and teleology in behavioral terms, their strict severence of these terms from any kind of causality, "initial or final,"[18] restricted the role of these terms in scientific explanation. Science is interested in the causes of phenomena as well as their classification. Since the behavioral approach does not deal with causality, it must be integrated with the other type of study defined in the paper; the functional study. The explanation of how the parts of a system work, which is treated in this second type of study, is obviously an important part of scientific explanation.

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[17] Ibid.

[18] Ibid.

The status of negative feedback with regard to functional and behavioral studies must be clarified for our purposes. As we shall see, an internal problem of the joint paper was to treat negative feedback as determinable behaviorally, when it is actually functionally determined. The actual behavioral definitions given by the authors for purposeful and teleological behavior are somewhat problematic in that they involve a redefinition of fairly well established terms, leading to possible confusion in this regard.

These problems of the paper, with regard to the full development of the role of negative feedback in scientific explanation, will now be considered in more detail. Their proposed resolution, also treated in this section, will lead us to the program of cybernetic analysis, the formal research program of negative feedback which I wish to advance.

The Problem of Terminology

The authors of "Behavior, Purpose and Teleology" defined the behavior of an object or system as being purposeful and teleological if the behavior satisfied certain criteria. First, one had to be able to interpret the behavior as being directed to the attainment of a goal; that is, "to a final condition in which the behaving object reaches a definite correlation in time or in space with respect to another object or event."[19] Second, the object or system had to exhibit the behavioral characteristics of a negative feedback system; that is, the object or system receives signals from the goal which modifies the activity of the object in the course of the behavior.[20]

How does this use of the terms "purpose" and "teleology" correspond with their traditional meanings? In certain respects the usage is quite different. Purpose and teleology are traditionally a form of explanation as opposed to a form of behavior. Traditionally, teleology takes the goal or end of a process to be the cause of the events leading up to this goal or event.[21] The antecedent events

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[19] Ibid., p. 18.

[20] Ibid., pp. 19-20.

[21] "Cause," here, is used in the sense of "final cause," the Aristotelian term classically associated with teleology (see below, p. 26). As will be presently described, however, the modern usage of the term "causality" does not include the process designated by final cause. Partly for this reason, modern writers do not generally speak of "teleological causation," but of "teleological explanation." The nature of the relationship between teleological explanation and causality, from the modern point of view, is highly controversial;

occur "for the sake of" the goal, or "in order to" bring it about. Thus if we ask the question, "Why does the heart beat?" the teleological answer is "in order to maintain the circulation of the blood." Such teleological explanations are quite common in everyday life, and in certain sciences, especially biology.

Teleological explanation is modelled on the human ability to envision a goal and then take action "in order to" bring about this goal. Such actions based on this ability have been termed "goal intended" by R. B. Braithwaite.[22] Thus the goal is the "purpose" of a goal-intended action, as is illustrated in the statement, "My purpose in going to New York was to visit my father." Because so many processes in nature, and biology in particular, appear to be directed toward a goal as a human might direct events toward a goal, the human derived idea of purpose was often transferred to nature in

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as a form of explanation, however, teleology must deal with causality in some sense. Richard Bevan Braithwaite has written:

> . . . in a teleological explanation the explicandum is explained as being causally related either to a particular goal in the future or to a biological end which is as much future as present or past. It is the reference in teleological explanation to states of affairs in the future, and often in the comparatively distant future, which has been a philosophical problem ever since Aristotle introduced the notion of 'final cause'; the controversy as to the legitimacy of explanations in terms of final causes rages continually among philosophers of biology and, to a less extent, among working biologists.

[See Richard Bevan Braithwaite, "Causal and Teleological Explanation," in John V. Canfield, ed., Purpose in Nature (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), p. 32.]

[22]Ibid.

general. This anthropomorphic character of purpose has been described by Moritz Schlick:

> . . . what are we to understand by 'purpose'? No doubt it is a concept derived from human action. What we call purpose is nothing but the anticipated outcome of our actions. In each action that is consciously carried out, the goal of the action is envisaged by consciousness. Hence, the existence of a purpose presupposes the presence of consciousness capable of representation. Wherever consciousness is absent or wherever a conceptual system is utilized which does not refer to consciousness, it is therefore impossible to speak of "purposes" in the original sense of the word.[23]

The idea of "final cause" in Aristotelian philosophy represented a model for teleological explanation; a model which David Ross, the noted Aristotle scholar summarized in the statement: "It is not that B must be because A has been but A must be because B is to be."[24] Of course not all events were teleologically determined in Aristotle's view. The color of ones eyes, for example, could be explained by antecedent events, the "efficient cause." Nevertheless, in many cases, especially those involving the overall development of organization in organisms, teleological explanation was required. This anthropomorphic basis of Aristotle's philosophy of causation is one of the primary reasons for terming his world view "organic." In it nature is alive and conscious. It is capable of carrying out goals just as humans are.

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[23]Moritz Schlick, "Philosophy of Organic Life," in Herbert Feigl and May Brodbeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, Inc., 1953), p. 527.

[24]David Ross, Aristotle (New York: Barnes & Noble, Inc. 1960), p. 79.

With the rise of the mechanistic world view during the seventeenth century, and its continued growth throughout the eighteenth, nineteenth and twentieth centuries, the validity of this "backwards causality" under any circumstances was severely questioned. In the modern view, events are entirely determined by other events concurrent with and preceding the event to be explained. Final cause, in other words, has been discredited as explanation and only the use of efficient cause has become proper. When we give a "causal" or "deterministic" explanation today we imply this process of antecedent causation in terms of known natural law. A "mechanistic" explanation, then, denotes explanation in terms of efficient cause; that is, a causal chain in which, by natural law, one event invariably leads to the next and culminates in the phenomenon to be explained.[25]

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[25]The term "mechanism" has been subject to a multitude of definitions. A good discussion of some of these is given in Jan Eduard Dijksterhuis, The Mechanization of the World Picture, tr., C. Dikshoorn (Oxford: Clarendon Press, 1961), pp. 495-501. The definition of mechanism presented above, in the present work, in terms of efficient cause and causal chains seems to me to be the most inclusive and, at the same time, the most accurate representation of the current meaning of the word. This definition is, in fact, suggested by Anthony Quinton in Alan Bullock and Oliver Stallybrass, eds., The Harper Dictionary of Modern Thought (New York: Harper & Row, Pub., 1977). He notes that the "traditional opponent of mechanism is teleology, the view that some, perhaps all, events must be explained in terms of the purposes which they serve, and thus that the present is determined by the future rather than by the past" (p. 379). Mechanism, for Quinton, is the "theory that all causation is, in Aristotle's terminology, efficient, i.e., that for an event to be caused is for its occurrence to be deducible from the antecedent (in some cases contemporaneous) condition in which it occurs, together with the relevant universal law of nature" (Ibid.).

The concept of a "causal chain" is formally defined by Braithwaite:

The problem with teleological causation for the modern mind, steeped in the mechanistic tradition, is that we are unable to conceive of a physical method by which the future could effect the present.[26] Thus the authors of "Behavior, Purpose and Teleology" state without comment, "Teleology has been discredited chiefly because it was designed to imply a cause subsequent in time to a given effect."[27]

In one area, however, teleological causation does satisfy modern causation criteria. In the case of actions carried out by human intention the intention is temporally antecedent to the realized goal and thus acts as an efficient cause. But it is the ability to conceive the future that gives humans this ability, an ability which lower animals and plants are not thought to have. We can say that we are "trying" to get out of a house when we are pulling on the door

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> . . . a spatio-temporally continuous chain of events . . . form[s] a causal chain if every event in the chain nomically determines its neighbors in the chain in such a way that the causal law relating the explans-event with the explicadum-event is a consequence within a true deductive system of higher-level laws which relate only spatio-temporally continuous events.

See R. B. Braithwaite, "Causal and Teleological Explanation," p. 29.

[26]The phrase, "unable to conceive of a physical method" reflects, of course, our mechanistic bias. If one accepts the suppositions of traditional teleology, teleological causation represents a process which is just as "determined" as that of efficient causation.

[27]A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior," p. 18.

in order to get out. We might explain a dog's behavior under the same circumstances in similar terms. But would we say that a fish flaps about on the sand in order to get back in the water? Would we say that plants bend toward the sun in order to get more sunlight? How can we explain these apparently goal-directed activities in entities which are not conscious in the human sense? How can these entities achieve a goal without "knowing" what they are doing? Do birds build their nests with no conception of the final product? How does the dividing zygote "know" how to differentiate the cells so as to produce a human rather than a mass of protoplasm? These and questions like them, relating to life processes which seem to carry out intention, have been so difficult to answer in mechanistic terms that it has often been claimed that life must have some special property which enables this behavior. This property has been given different names; the "entelechy" of H. Driesch, and the "élan vitale" of Bergson, are just two of them. What these vitalistic views all have in common is that they depend on teleological causation; final cause acts as efficient cause. Attempted mechanistic explanations for life processes from Descartes until the beginning of this century were thwarted by the apparent qualitative differences between the behavior of living and non-living entities. Thus when the turn-of-the-century biologist, Hans Driesch, cut an embryo in half and then observed each half develop into a normal, full organism, he took this development as evidence that some non-mechanical, vital, goal-directing agent was at work in the embryo. A machine would necessarily be "too stupid" to compensate for this violent disturbance to the embryo. As Anatol Rapoport has commented, the term "mechanical process"

> . . . carries overtones of stupidity, at least of failing to take into account the variable environment. . . . Driesch evidently thought that the two separated halves of the embryo, if they were guided by mechanical process, should have exhibited 'stupid' behavior. Unaware of the changed circumstances, they should have continued blindly along the same path as if they had not been separated. . .[28]

Driesch's vitalistic views were not, of course, typical of the many biologists and naturalists who felt that organic processes could, indeed, be explained in mechanistic terms.[29] The important point is, however, that until negative feedback became understood as a mechanism to account for goal-directed behavior, there was, in fact, no plausible mechanism to account for this critical organic property. Thus, the mechanistic potential to explain organic phenomena was, in fact, severely limited.

The advent of "intelligent" machines in the areas of servomechanisms and computers during World War II initiated a profound reconsideration of the potential of "mere" mechanisms (see

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[28]Anatol Rapoport, "The Impact of Cybernetics on the Philosophy of Biology," in Norbert Wiener and J. P. Schadé, eds., Progress in Biocybernetics, vol 2 (New York: Elsevier Pub. Co., 1965), p. 147.

[29]See Garland Allen's discussion of "mechanistic materialism" in biology in his Life Science in the Twentieth Century (New York: John Wiley & Sons, 1975), pp. xix-xxiii. A particularly interesting case of a biologist who attempted to account for organization in mechanistic terms is that of Lamarck. Because of the lack of any specific known mechanisms to account for organization, Lamarck was eventually forced into a vitalist position in spite of his professed adherence to the mechanistic view. See Christopher Michael Dobson, "Lamarck's Approach to an Understanding of Man" (Masters dissertation, Oregon State University, 1978), pp. 20-28.

Chapter Five). But to state that mechanisms can act "purposefully" risks the danger of imparting to their action the presence of a final cause which, as we have seen, has been traditionally associated with "purpose" and "teleology." The solution to this problem involves retaining the behavioral content of the terms purpose and teleology, but dropping the terms themselves. The essential aspect of both purposeful and teleological behavior in the 1943 paper was goal-directedness; the observation that the "act or behavior may be interpreted as directed to the attainment of a goal."[30] Rather than saying that a certain entity is acting "purposefully," then, we will say that its actions are goal-directed. But in order to maintain the important distinction between purposeful and teleological behavior made by the authors of the 1943 paper, the distinction of the presence or absence of negative feedback, it will be necessary to designate two types of goal-directed behavior. For reasons which will soon be made clear, we will designate these two categories as adaptive goal-directed behavior (or, in brief, "adaptive behavior") and non-adaptive goal-directed behavior (or, in brief, "non-adaptive behavior"). We define these terms as follows:

Adaptive goal-directed behavior is that which occurs when a system continues to manifest a certain state or property, G, or to develop towards G, while being subjected to changes in its external environment or in some of its internal parts.

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[30] A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior," p. 18.

Non-adaptive goal-directed behavior is that which occurs when a system continues to manifest a certain state or property, G, or develop towards G, but only while it is not being subjected to changes in its external environment or in some of its internal parts.[31]

In terms of the 1943 paper, what is here called non-adaptive would be termed purposeful, and what is here called adaptive would be termed teleological. The definitions here are entirely behavioral. The fundamental difference between adaptive and non-adaptive behavior is that adaptive behavior persists even while being subjected to disturbances; that is, the behavior "adapts" to the disturbance. Non-adaptive behavior does not have this property and this distinction is crucial in understanding the role of negative feedback in explanation. It should be noted that the determination of whether the behavior is goal-directed in either sense is an experimental question and thus conclusions in regard to this determination will only be as strong as the experimental evidence. This experimental aspect of the definitions will be important in treating certain objections to the behavioral approach (see below, pp. 53-63).

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[31]The definitions presented here stem from one given by Ernst Nagel for "the structure of systems which have goal-directed organization." Nagel did not distinguish between adaptive and non-adaptive behavior in his definition and thus certain important modifications had to be made. Additional modifications result from the fact that Nagel's definition may be interpreted as not being entirely behavioral. See, Ernst Nagel, "Teleological Explanation and Teleological Systems," in S. Ratner, ed., Vision and Action (Rutgers, N.J.: Rutgers Univ. Press, 1953); reprinted in H. Feigel and M. Brodbeck eds., Readings, p. 546.

The term "negative feedback" does not appear in the adaptive behavior definition because these definitions are entirely behavioral and negative feedback is a mechanism rather than a form of behavior. We will return to this point presently. But if experiment leads us to the conclusion that the behavior of a system is adaptive, then we may assume, as a hypothesis, that the mechanism causing the behavior is one of negative feedback. This assumption is justified by the fact that servomechanisms and other negative feedback devices exhibit the same behavior pattern. Once made, the assumption becomes quite useful because the entire repetoire of feedback theory (see below, pp. 185) becomes available to help make predictions as to how the behavior of the system will vary when the disturbances applied to it are varied. It should be noted that for relatively simple systems, the mechanisms of which are well understood, predictions of the system's behavior under different disturbances can be made from knowledge of this mechanism and its "causal chain" alone. The value of feedback theory is that it can give predictions concerning behavior under different disturbances when nothing is known about the workings of the internal mechanism itself. As Braithwaite has commented, ". . . in this case we are unable, through ignorance of the causal laws, to infer the future behavior of the system from our knowledge of the causal laws; but we are able to make such an inference from knowledge of how similar systems have behaved in the past."[32] Of course the question of whether or not the assumption of negative feedback was well taken

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[32] R. B. Braithwaite, "Causal and Teleological Explanation," p. 40.

can be judged by the success of these predictions.[33]

The Role of the Functional Study

One of the stated goals of the "Behavior, Purpose and Teleology" paper was to "define the behavioristic study of natural events and to classify behavior."[34] The authors also state,

> All [teleological] behavior may be considered to require negative feed-back. If a goal is to be attained, some signals from the goal are necessary at some time to direct the behavior.[35]

This statement is troublesome. It is possible to obtain a goal without feedback. The authors' own example of the snake striking at its prey illustrated this possibility. But further, can one use negative feedback as a behavioral classification? Can one identify a system as employing negative feedback or any other mechanism by observing its behavior only? Or does one have to examine the inner structure of the system to determine if negative feedback is being used? The authors seem to define it as a behavioral characteristic

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[33]It is equally clear that successful predictions do not validate the assumption in an epistemological sense. The working value of a scientific theory, however, is frequently measured by its ability to predict phenomena, and it is in this sense that the phrase "well taken" is used here. This is not to say that scientific theories which yield consistently good predictions have no philosophical or intellectual impact. Quite the opposite is the case. See below, pp. 30-31.

[34]A. Rosenblueth, N. Wiener, and J. Bigelow, "Behavior," p. 18.

[35]Ibid., p. 19. See note 9.

alone, but in actuality, one cannot determine if a system employes negative feedback without a functional study.[36] Two entities may exhibit the same behavior, but one cannot assume they are doing so for the same reasons. The fact that servomechanisms operate on the principle of negative feedback is no guarantee that organisms do, even if they both behave similarly. In order to establish the functional identity a functional study is required; a study which the authors actually do pursue in the paper. They are concerned, for instance, with the functioning of the retina and the speed of nerve transmission in a snake. Thus the paper, which was ostensibly a behavioral study, eschewing any consideration of any causality, "initial or final," was, in reality, quite concerned with matters of function. In fact, the major force of the paper came not only from its demonstration that organisms behaved like machines, but also its provision of a mechanism to account for this similar behavior; the mechanism of negative feedback.

Thus if it appears that a system is adaptive, and if we assume that this behavior is the result of negative feedback, the next apparent step is to perform a functional study; a study to determine how the interaction of specific parts of the system produces this behavior. The functional study can be guided by the known characteristics of negative feedback mechanisms, but more importantly, the functioning of the parts is seen as a means to an end. The

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[36]That behavior cannot be an indication of internal structure was apparently first pointed out by John von Neumann (see below, pp. 267-268).

guiding question becomes, how do the parts produce the adaptive goal-directed behavior? It had previously been assumed that adaptive behavior required some special, perhaps non-mechanistic property in living beings. Negative feedback mechanisms provided a new possible explanation. These mechanisms were the expression of the physical organization of the system (see Chapter Five) and not any vitalistic property. The importance of the acceptance of goal-directedness in mechanistic terms cannot be overemphasized, and this importance will be treated in the next section.

Another way in which the functional study is guided by the behavioral study involves, once again, prediction. The functional study will predict, on the basis of the understanding of the feedback mechanism, how the behavior of the system will change when the component characteristics are varied. Once again, these predictions can be experimentally tested. This approach has been quite common when negative feedback has been used in scientific explanation.[37]

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[37]A typical example of how behavior guides the functional study in terms of feedback is that involving the elucidation of allosteric mechanisms in biochemistry. See Albert L. Lehninger, Biochemistry, 2nd ed. (New York: Worth Pub. Inc., 1976), pp. 234-235.

CYBERNETIC ANALYSIS

In the preceeding section we found that in order to fully characterize the role of negative feedback in scientific explanation certain modifications had to be made in the "Behavior, Purpose and Teleology" paper. Nevertheless, the basic behavioral approach of the paper, its separate classifications for goal-directed behavior with and without feedback, and its distinction between behavioral and functional studies proved to be valuable.

The problems with the paper included the possible confusion over the redefinition of the terms "purposeful" and "teleological" and the uncertainty over the status of negative feedback as a behavioral or a functional consideration. We approached the first problem by dropping the terms purpose and teleology altogether and substituted instead the two behaviorally defined terms related to non-adaptive and adaptive goal-directed behavior. A discussion of the status of negative feedback led us to the importance of the functional study. Both the behavioral and functional studies lead to predictions of behavior, predictions which can be tested by experiment. The degree to which a system is goal-directed in either sense is thus an experimental question.

Based on the considerations of the previous section, then, I will attempt to summarize the overall methodology of scientific research based on negative feedback. It is most convenient to present this methodology as a series of steps in an overall program. These steps are as follows:

1. Identify a system of interest.[38]

2. Perform a behavioral study to determine if the system is adaptively goal-directed; that is whether the system persists in its behavior even while being subjected to disturbances (see definition of adaptive goal-directed behavior on p. 31).

3. If the system is behaviorally determined to be adaptively goal-directed, make the hypothesis that this behavior is the result of a negative feedback mechanism or mechanisms within the system.

4. Test this hypothesis by attempting to apply feedback theory to the system and thereby to arrive at successful predictions of behavior changes when the disturbances applied to the system are varied.

5. Perform a functional study to determine how the components of the system interact to produce the negative feedback mechanisms(s) of the system.

6. Test functional theories by making predictions of behavior when components or component interrelationships are modified.[39]

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[38]Robert Lilienfeld, in his critique of systems theory, advances as a criticism the view that the systems approach is flawed because a "system" can never be completely isolated from its surroundings. While this statement is true, it is not damaging because, in practical terms, systems are commonly identified and used successfully for research purposes. For Lilienfeld's views see his The Rise of Systems Theory (New York: John Wiley & Sons, 1978), p. 256.

[39]The program stated here is in analytical form. Negative feedback also plays an important role in the synthesis of behavior, especially in engineering applications. Thus "cybernetic synthesis" also occurs in science and technology. Because of space limitations this aspect of negative feedback cannot be treated in this work.

In deference to Wiener's derivation of the term "cybernetics," which involved the Greek and Latin roots for a class of early negative feedback devices (see p. 278), I term this program of research cybernetic analysis. Although the methodology of negative feedback has not, to my knowledge, been formalized in this manner before, the fact remains that this method is used throughout the sciences where negative feedback is utilized as a research tool. The particular interplay of functional and behavioral studies with respect to negative feedback described by cybernetic analysis is quite commonplace in science.[40] It should be noted, however, that the program represents the fullest use of negative feedback in research, and that in practice, different researchers frequently deal with different and

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[40]As noted in the introduction, it is impossible to fully describe or cite all the research which is being performed on the basis of negative feedback. A rigorous defense of my statement that cybernetic analysis mirrors the usage of negative feedback in science would require a large research project in itself; but I think those familiar with the use of negative feedback in science would agree that the program of cybernetic analysis described here is a fair statement of the manner in which negative feedback is used as an analytical tool in science. It goes almost without saying that the behavioral aspects of the system must be determined before it can be usefully hypothesized that the system operates on the basis of a negative feedback mechanism, and before the elucidation of this mechanism in functional terms can be accomplished. Step four, the application of feedback theory to make predictions of behavior, is often not practical in biological applications where the frequency response methods from control engineering (see below, pp.185-191) are difficult to apply. The step is included, however, to indicate the total possible use of negative feedback in scientific research. Two very useful presentations detailing applications in biology where control theory has been successful are: William S. Yamamoto and John R. Brobeck, eds., Physiological Controls and Regulations (Philadelphia: W. B. Saunders Co., 1965); and H. Kalmus, ed., Regulation and Control in Living Systems (New York: John Wiley & Sons, 1966).

isolated steps of the program. This application of the various steps to a particular problem may even occur many years apart.

In the investigation of any of the well known homeostatic properties of organisms, for example, such as the maintenance of body temperature in mammals or the blood pH, it is clear that the behavioral property of regulation had to be observed before any attempt could have been made to functionally account for this behavior. Such studies of behavioral regulation, in fact, comprised a good portion of early physiological research.[41] When the negative feedback mechanism became generally understood some of this behavior became explainable in terms of this mechanism; that is, predictions of behavior changes resulting from environmental disturbances and component characteristic variations, became possible when the behavior was analyzed in terms of negative feedback.

Thus we see that cybernetic analysis is a formal statement of the manner in which negative feedback is already being used in scientific research. The formal statement is useful in clarifying this role, as well as facilitating a discussion of the implications, underlying assumptions and limitations of the use of negative feedback in research, the discussion of which follows.

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[41] See J. S. Wilkie, "Early Studies of Biological Regulation: An Historical Survey," in H. Kalmus, ed., Regulation and Control in Living Systems (New York: John Wiley & Sons, 1966), pp. 259-289.

Limitations and Importance of the Program

The program of cybernetic analysis represents a research strategy. Its success is based on the accuracy of its predictions with regard to behavior. One cannot claim, therefore, that those who attempt this program hold that all complex phenomena, including life processes, are already understood in these terms, or even that these phenomena must necessarily be explainable in these terms. In most complex systems the functional elucidation of the feedback structure is immensely complicated by the complex properties of the structural components themselves. Thus a nerve cell may be one element within a feedback system, but it is itself an enormously complex system. In complex systems, where the functioning of the components themselves is not understood, one cannot expect to proceed beyond the behavioral stage of cybernetic analysis.

Another limitation is that the program of cybernetic analysis does not represent an epistemological statement or claim. It does not attempt, on philosophical grounds, to refute the notions of final cause and traditional teleology. It ignores them. Thus the many arguments that a mechanistic approach such as that used in cybernetic analysis does not represent "a legitimate teleological expression,"[41a] to use the phrase of one writer, simply do not apply here. This is not to say, however, that cybernetic analysis does not have profound intellectual and philosophical implications. The

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[41a]Larry Wright, "The Case Against Teleological Reductionism," Brit. J. Phil. Sci. 19 (1968), p. 211.

nature of the relationship between scientific developments and larger "world views" is, of course, a topic of active investigation in the history and philosophy of science. Among the many interpretations of this relationship from which one might choose, it is most convenient here to utilize that of Larry Laudan.[42]

Laudan views science as basically a problem solving activity. Progress in science stems from solving problems. In so far as cybernetic analysis results in successful predictions of behavior it will, by this view, represent scientific progress. The concept of the "research tradition" is highly important for Laudan. A given research tradition stems not only from a line of empirical problems within science, but also from conceptual problems related to the world view in which the scientific activity is enclosed. Laudan devotes much effort to fully exploring the concept of the research tradition, and it will be useful to state three of its major characteristics as stated by Laudan:

> 1. Every research tradition has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous, others will be temporal successors of earlier ones;
> 2. Every research tradition exhibits certain metaphysical and methodological commitments which, as an ensemble, individuate the research tradition and distinguish it from others;
> 3. Each research tradition (unlike a specific theory) goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history extending through a significant period of time. (By contrast, theories are frequently short lived.)[43]

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[42] Larry Laudan, Progress and Its Problems (Berkeley: Univ. of Cal. Press, 1978).

[43] Ibid., pp. 78-79.

In examining these characteristics and also certain developments in science prior to 1943 (see Chapter Five, pp. 192-193), it becomes clear that cybernetic analysis represents the latest manifestation of the well-established research tradition of robotics. Briefly stated, the robotic view holds that all the behavioral capabilities of humans can be duplicated mechanically. Cybernetic analysis is not a theory within the robotic tradition but rather, as just noted, its current manifestation. The linking of cybernetic analysis with its research tradition of robotics provides the means for understanding how cybernetic analysis as a research program is yet able to affect the philosophical and intellectual climate. This influence comes about because, as Laudan notes, a successful research tradition, one that solves many problems, can challenge an established world view, and may sometimes alter it. As he writes:

> . . . research traditions and theories can encounter serious cognitive difficulties if they are incompatible with certain broader systems of belief within a given culture. Such incompatibilities constitute conceptual problems which may seriously challenge the acceptability of the theory. But it may equally well happen that a highly successful research tradition will lead to the abandonment of that worldview which is compatible with the research tradition. Indeed, it is in precisely this manner that many radically new scientific systems eventually come to be 'canonized' as part of our collective 'common sense.' In the seventeenth and eighteenth centuries, for instance, the new research traditions of Descartes and Newton went violently counter to many of the most cherished beliefs of the age on such questions as 'man's place in Nature,' the history and extent of the cosmos, and, more generally, the nature of physical processes. Everyone at the time acknowledged the existence of these conceptual problems. They were eventually resolved, not by modifying the offending research traditions to bring them in line with more traditional world views, but rather by forging a new world view which could be reconciled with the scientific research traditions.[44]

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[44]Ibid., p. 101.

The numerous great developments in robotics during the World War II period, reflected in the production of "intelligent" machines in the form of sophisticated servomechanisms and computers (see Chapter Five), seriously challenged the older world view with regard to the potential of "mere" mechanisms. The example of Driesch's interpretation of the embryo experiments given above illustrates an extreme example of this older view. The newer view of mechanistic processes attributed to them a behavioral potential far beyond any which had generally been plausible given the relatively limited behavior of machines before the twentieth century robotic developments. The characteristics of this new mechanistic potential will now be examined.

Goal-directedness and Mechanism

Basic to the new estimate of the potential of mechanistic explanation is the concept of goal-directedness. In the formulation of cybernetic analysis, two types of goal-directed behavior were defined, adaptive and non-adaptive. The tendency of modern science has been to associate all goal-directedness with the unacceptable concepts of teleology and final cause. This association has led to profound problems in science because it has always been true that, behaviorally, many, of not most, systems, organic and inorganic, do exhibit goal-directedness. Since goal-directedness was associated with teleology, however, it had to be denied whereever it occurred in forms not related to conscious human intention. This dissociation has led to a kind of blindness in which it is denied that goal-directed systems actually do have that property. An interesting example illustrating this problem, relating to equilibria phenomena such as pendulum motion, will be given in the next section (see below, pp. 53-57).

As it has become apparent that goal-directedness, especially in its adaptive form, can result from mechanical and not teleological sources, science has become free to state problems in these very useful terms.[45] Progress (in Laudan's sense of problem solution) is much more likely to be made, for example, when the problem of temperature regulation in the body is stated in goal-directed terms than when it is not: The goal-directed question, "How does the body maintain constant temperature?" provides a framework around which functional studies can be made. If we insist on not recognizing the goal-directedness of the system the functional study becomes unfocused. To not recognize the goal-directedness of the system is to deny the existence of precisely that aspect of the system which it is most important to explain. Thus, the dominant methodology of modern science, the reductionist approach, which has not recognized the goal-directedness of systems, has been forced into describing, in ever greater detail, the functioning of the parts of systems without ever recognizing the most interesting property of the system as a whole. Now that, due to the development of cybernetic analysis,

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[45]Of course, for practical work in the life sciences, the teleological approach has been used continuously. But this approach has been viewed more as a convenience than as a true reflection of reality, at least by non-vitalist biologists, who have dominated modern biology.

goal-directedness no longer needs to be associated with final cause, the reductionist approach is freed to deal with this most important system property. Morever, the goal-directedness of the systems specifies what the functional or reductionist study must explain. With regard to the regulation of body temperature, for example, we can now ask, "How does hormone X contribute to the maintenance of body temperature?" Previously, one would expect that if one studied all the hormones and other relevant metabolic factors in the body, the maintenance of constant body temperature would inevitably result from this functional study, much as the crystal structure of common salt inevitably results from the known electrochemical properties of the sodium and chloride ions. Clearly, however, with the enormously greater complexity of biological and other systems outside of the relatively simple and idealized world of physics and chemistry, the chances of an "inevitable behavior" emerging from a <u>purely</u> functional study in these areas are much smaller, if, indeed, they exist at all. Without the recognition and use of the goal-directed nature of the system, then, the functional study is unlikely to account for the behavior of the total system. With this recognition, and the use of the negative feedback mechanism to account for goal-directedness, a path is opened to explain this critical "holistic" property in mechanistic and reductionist terms. The behavior of the whole at least with regard to this property, <u>is</u> explained in terms of its parts, the particular interactions of which comprise the negative feedback mechanism. Thus the mechanism of negative feedback provides a bridge between holistic and reductionistic explanation.

When the system is not viewed as being goal-directed, it causes the functional study to lack direction, and progress in accounting for the system's behavior will be hindered. This is not to say that all functional studies are useless unless put in the context of a behavioral study. One can study the properties of the individual nerve cell, for instance, and learn much about its functioning without reference to the larger behavior patterns which it helps to create. This basic research is an invaluable element of science. Nevertheless, if one is faced with a system which is experimentally determined to be goal-directed, one needs to evaluate the functional properties of the components, perhaps determined by previous basic research, in light of the total behavior.

The program of cybernetic analysis has really just begun. But if it continues to successfully solve problems it stands to change our basic conception of nature from one dominated by "chance and necessity" to one dominated by the natural occurrence of goal-directed systems. Of course the question immediately arises as to how natural systems become organized to produce goal-directed behavior. Strictly speaking, this question is not relevant to the program of cybernetic analysis which takes given systems and attempts to account for their behavior. But it should be noted that the statement of the question above implies a world view which is foreign to that of cybernetic analysis. The assumption behind the question is that we must start with chaos and then account for order.

As Stafford Beer has argued,[46] true chaos is almost nowhere to be found in nature. All events we observe seem to conform to natural law, whether these events occur inside the atom, inside the cell, or in another galaxy. Almost all the behavior in the universe can be categorized under the headings of either non-adaptive or adaptive goal-directed behavior. Cybernetic analysis, in emphasizing these characteristics of goal-directedness reflects a much truer picture of nature than the chaotic assumptions of the posed question.

Another question which should be addressed is, "Does cybernetic analysis rest on the assumption that nature as a whole is goal-directed?" The answer is "No." Cybernetic analysis stems from the fact that certain systems, organic and inorganic, exhibit common behavioral properties of adaptive goal-directedness. Whether nature as a whole exhibits these properties is certainly an interesting question, but it remains a question, and not an assumption.[47]

The change to a methodology which can operate in terms of goal-directedness will, in a practical sense, do much to alleviate a problem which has plagued scientific research since the rise of the

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[46]Stafford Beer, "Below the Twilight Arch," in General Systems Yearbook vol. 5 (Ann Arbor, Mich.: Society for General Systems Research, 1960), pp. 9-20.

[47]When one addresses questions in terms of "nature as a whole," the phenomenon of evolution immediately comes to mind. Even though it is almost certainly premature to expect a full solution to the problem of evolutionary mechanisms, the framing of this problem in terms of cybernetic analysis will at least greatly clarify the statement of some of the problems: Does the fossil record indicate any of the behavioral properties of goal-directedness? If so, is this directedness adaptive or non-adaptive? Can current functional theories account for this behavior?

mechanistic world view; the problem of the relation between the behavior of the parts of a system and the behavior of the system as a whole: "Can the behavior of the whole be determined, or 'reduced to,' the sum of the component behaviors?" This question is examined in somewhat more detail in Chapter Five. But, as we have already seen, vitalists, such as Driesch felt that the behavior of organisms could not be reduced to component effects in this manner. The evidence that machines can, indeed, behave purposefully does much to resolve this aspect of the mechanist-vitalist debate. While cybernetic analysis is strictly a research program, and its success in accounting for biological behavior is at this time quite limited with regard to the total range of biological behavior, it has elucidated the regulation of certain biological processes with great success (see footnote 1, p. 1). These successes in the organic domain, limited as they are, still indicate the great potential of the program.

The relationship of the whole to the parts is a problem in general for all of science, not just biology. The problem has sometimes been stated in the form, "Is the whole equal to or <u>more</u> than the sum of its parts?" In view of cybernetic analysis the answer is, "It depends on how the parts are 'summed.'" In holding that goal-directedness is an expression of the organization of the parts of the system, it is clear that if the parts are "summed" in an organized manner there can be goal-directed behavior. A watch does not have to lose any parts to cease functioning. All we need do is line the pieces up in a row and we have no watch. Cybernetic analysis simply recognizes the fact that many man-made systems display the behavioral characteristics of organization, that is, goal-directedness, and in particular, adaptive goal-directedness, and suggests that it is reasonable to attempt to analyze all systems displaying a similar behavior in terms of these man-made systems which we already understand. This point of view, especially with reference to the mechanist-vitalist debate, has been well described by the British biologist, H. Kalmus:

> In so far as cells, organisms or societies are more than the sum of their parts, they are so by virtue of controlled interactions or regulations between these parts. These interactions can only be understood in terms of the systems within which they operate and not simply by studying the isolated parts. This Aristotelian, 'holistic' approach familiar to technologists and naturalists is in some ways easier than the atomistic, Democritean study of the parts as practised in classical physics and the biological ancillaries; a boy can 'understand' the principles of a clock in a qualitative way many years before he can grasp quantitatively the mechanical properties of the parts of the clock. To understand 'fully' the working of the clock he will later in his life require information of both the parts and the whole system. . . . The Aristotelian and Democritean approach . . . will always stay in opposition to each other and will both be needed to complement each other in any attempt to understand life. The anti-thesis between holism and atomism, which at the beginning of the century was confused by the ideological quarrel between vitalists and mechanists, can now be resolved by the science of cybernetics---the study of systems, which appear goal directed and simultaneously have a transparently causal structure [emphasis added].[48]

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[48] H. Kalmus, "Control and Regulation as Interactions within Systems," in H. Kalmus, ed., Regulation, pp. 1-2. Kalmus' designation of the Aristotelian vs. the Democritean approaches may be considered as roughly equivalent to the behaviorist-functional approach of cybernetic analysis. In both cases it is emphasized that a full understanding of the system will only be obtained when these two types of studies are used to complement each other.

In concluding this section it should be reemphasized that cybernetic analysis is a research program, an undertaking prompted by the fact that machines can demonstrate adaptive goal-directed behavior, a type of behavior which previously had been thought to be unique to life. Once this behavior became explainable in mechanical terms, the path was opened for the growth of the "intelligent machine." As noted above, however, an intelligent machine requires far more than feedback for its operation. For example, the functioning of the components to produce a calculating or extrapolating ability is important in such systems. Although feedback may be utilized in the components which together produce abilities such as these, feedback is generally not the main concept required for their understanding. Feedback systems also require the measuring of many quantities and the transmission of this information to various parts of the system. Again, this measurement and transmission require functional understanding independent of the feedback properties of the system. The manner in which these abilities could be reproduced mechanically was greatly clarified during the first half of this century and especially during the World War II period. These robotic developments, although not directly a part of cybernetic analysis, were invaluable for giving the program a firm foundation. As we shall see in Chapter Six, the development of cybernetic analysis and these robotic advances were totally interwoven. One may view cybernetic analysis as the most general expression of the robotic tradition today, but it is an expression which has been greatly enhanced by these other robotic developments.

It is for this reason that a large portion of Chapter Five is devoted to an exposition of the associated events in robotics.

OBJECTIONS TO THE BEHAVIORALISTIC APPROACH TOWARD GOAL-DIRECTEDNESS

It will be useful to examine several objections to the behavioral approach described here which have been raised by various writers. These objections may, in general, be characterized as follows: First, the behavioral definition of goal-directedness does not discriminate between systems that are goal-directed and those that are not, and, second, behavioral goal-directedness cannot really be equated with purpose. These objections will now be examined.

Goal-directedness and Equilibrium

When, under standard conditions, a pendulum is displaced from its rest point and released, it invariably returns to its rest point. Should one say that the pendulum's behavior is directed toward the goal of restoring equilibrium? Is there not a fundamental difference between this behavior, and, for example, the maintenance of temperature in the body? Ernst Nagel has described this problem in the following manner:

> When a ball at rest inside a hemispherical bowl is displaced from its equilibrium position, restoring forces come into play that in the end bring the ball to rest at its initial position. Is this a goal-directed process, whose goal is the restoration of equilibrium? Were the process so classified, <u>every</u> process in which some equilibrium state is restored would also have to be designated as goal-directed; and in consequence, the designation would be applicable to well-nigh all processes, so that the concept of being goal-directed would not be differentiating, and would therefore be

superfluous. On purely 'intuitive' grounds, however, the answer to the question just raised is negative. . . .[49]

Nagel follows G. Sommerhoff[50] in asserting there is a qualitative difference between simple equilibria behavior such as the ball in the hemisphere or the pendulum, and behavior which is goal-directed, or in Sommerhoff's term, "directively correlated." Specifically, the equilibrium behavior is not viewed as goal-directed both on intuitive grounds and the conclusion that such a designation would lead all processes to be categorized alike. The technical distinction between the two types of behavior made by Sommerhoff, and followed by Nagel, involves the requirement that in goal-directed behavior one of the variables in the system must be able to exhibit several values for any given value of another variable relevant to the behavior. This requirement is termed by Sommerhoff, "epistemic independence."[51] If, for example, we consider a standard pendulum, we find that the relevant variables of the pendulum's displacement and the restoring force are not epistemically independent. For every value of the displacement there is only a single value of the restoring force. It is claimed that the requirement of epistemic independence can be used to distinguish goal-directed processes from those which, "on purely 'intuitive' grounds" are not goal-directed.

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[49]Ernst Nagel, Teleology Revisited and Other Essays in the Philosophy and History of Science (New York: Columbia Univ. Press, 1979), pp. 288-289.

[50]G. Sommerhoff, Analytical Biology (London: Oxford Univ. Press, 1950), pp. 99-102.

[51]Ibid., p. 100.

Whether or not the requirement of epistemic independence makes the distinction it is designed to is not of great importance here. What is important is that some distinction is required between the two types of behavior, or else, as Nagel indicated, we would be designating a superfluous term. We have already made the required distinction, however, in our definitions of adaptive and non-adaptive goal-directed behavior. The pendulum is an example of non-adaptive goal-directed behavior. It persists toward its real state only while it is not being disturbed. The disturbance must cease before the pendulum will return permanently to its equilibrium position. If the pendulum were able to retain its rest position while being subjected to disturbing forces it would be demonstrating the qualitatively different property of adaptive goal-directed behavior. A simple pendulum does not have this property, however. A target-seeking missile, on the other hand, will correct its motions even while a disturbance, such as a change in wind force, acts on it. It is an adaptive system. The distinction of behavior in terms of adaptiveness and non-adaptiveness would seem to be more easily determinable by experiment than the requirement of epistemic independence, which is subject to certain problems of interpretation.[52]

But what do we say to Nagel's "intuitive" reaction that the pendulum's behavior should not be described as goal-directed?

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[52]See Andrew Woodfield, Teleology (New York: Cambridge Univ. Press, 1976), pp. 67-72 and E. Nagel, Teleology Revisited, pp. 289-290.

First, Nagel's exposition did not make the distinction, made here, between adaptive and non-adaptive goal-directedness. Thus in his terminology the equilibrium behavior perhaps should not have been described as goal-directed. Sommerhoff was much in the same position. The basic point to be made here is that the pendulum's behavior is goal-directed, but not adaptively so, and this important distinction represents the qualitative difference sought by the authors. The intuition that phenomena such as the pendulum should not be designated as goal-directed probably stemmed from the tendency to consider all goal-directed behavior in adaptive terms.[53] Once the distinction between adaptive and non-adaptive behavior is made, however, there is no longer any reason to object to terming as "goal-directed" phenomena such as the pendulum, so long as the term is properly qualified.

The lack of the distinction between adaptive and non-adaptive goal-directed behavior has led, in the past, to the restriction of the term "goal-directed" to behavior which is here described as "adaptively goal-directed." This restriction is problematic for science, and, in fact, counter-productive. As stated above (p. 45), whether for an adaptive or non-adaptive system, the goal-directed nature of the system is of great conceptual importance in framing

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[53]Sommerhoff defined the terms "effective" and "appropriate" in ways which might have made them useful to define an equivalent to non-adaptive goal-directed behavior, but he did not use this approach in his treatment of equilibrium phenomena. Sommerhoff also used the term "adaptive" much as it is used here, but he did not make use of the non-adaptive category. See G. Sommerhoff, Analytical Biology, pp. 48-51.

the functional study. If we arbitrarily limit goal-directedness to the adaptive case, this conceptual advantage will be lost for a great number of systems which are goal-directed but not adaptively so. An example here will not only illustrate this advantage, but also indicate another use of the word "purpose." When we say, "The purpose of the spark plug in the internal combustion engine is to ignite the vapor mixture," we are assuming a goal-directed system in which we are specifying one element of the causal chain which results in the overall behavior of the system. It is understood that the ignition of the vapor mixture will cause further events leading to the turning of the engine. "Purpose," here, then, is associated with the functioning of the component; that is, its role in the causal chain. The idea of function implies a perceived goal. Thus, whenever we talk of the functions of components we are talking about a goal-directed system. If we invalidate the use of goal-directedness for non-adaptive systems, then we in turn invalidate the use of the concept of function within these systems as well. This approach is clearly unacceptable as the idea of function is basic to our understanding of all systems; that is, the understanding is not complete until a successful functional study is made. The common use of the function concept in explaining non-adaptive systems, such as the engine shows that these systems are commonly considered to be goal-directed. The designation of non-adaptive goal-directedness simply recognizes this fact.

The Relationship Between Goal-directed Behavior and System Structure

We recall that in the "Behavior, Purpose and Teleology" paper the term "purpose" was equated with the attainment of a goal. In behavioral terms we determine if a system is purposeful, or goal-directed, by the formal definitions of adaptive and non-adaptive goal-directedness given above (pp. 31-32). The basic viewpoint of cybernetic analysis is that this goal-attaining behavior is an expression of the physical organization of the system. But how do we know that the observed behavior accurately reflects the goal embodied in the organization? Behavior can be deceptive, especially in complex systems. The problem of the relationship between goal-directed behavior and the system structure is central to many of the criticisms made of the behavioral approach. A particularly interesting exchange, relating to this problem, took place between Richard Taylor, a professor of philosophy at Brown University, and Rosenblueth and Wiener. Taylor wrote a critique of "Behavior, Purpose and Teleology" in 1950 which was immediately rebutted by Rosenblueth and Wiener. A rejoinder by Taylor followed.[54]

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[54]Richard Taylor, "Comments on a Mechanistic Conception of Purposefulness," Phil. Sci. 17 (1950), pp. 310-317; Arturo Rosenblueth and Norbert Wiener, "Purposeful and Non-Purposeful Behavior," Phil. Sci. 17 (1950), pp. 318-326; Richard Taylor, "Purposeful and Non-Purposeful Behavior: A Rejoinder," Phil. Sci. 17 (1950), pp. 327-332. The rebuttal of Wiener and Rosenblueth provides an extremely valuable amplification of the original "Behavior, Purpose and Teleology" paper. In their rebuttal, Rosenblueth and Wiener do, indeed, designate as "purposeful," equilibrium phenomena such as the pendulum and the magnetic compass, but curiously, they no longer make the explicit distinction (made in their earlier paper with Bigelow) as to the difference between

In their rebuttal to Taylor, Rosenblueth and Wiener wrote:

> . . . let us consider a car following a man along a road with the clear purpose of running him down. What important difference will there be in our analysis of the behavior of the car if it is driven by a human being, or if it is guided by the appropriate mechanical sense organs and mechanical controls?[55]

Taylor, in his rejoinder, replied:

> Now I submit that, from observable behavior alone, one cannot certainly determine what the purpose of the behaving object is, nor indeed, whether it is purposeful at all. Surely the observable behavior of the car and its driver might be exactly the same, whether the purpose is, as supposed, to overrun a pedestrian, or merely, as a joke, to frighten him, or indeed, to rid the car of a bee, the driver being in this case wholly unaware that his car is endangering another person. If, however, purpose were definable solely in terms of observable behavior, as these writers suppose, then any driver who appeared to behave as if he were trying to run down a pedestrian, but who yet pleaded that he had no such intention, would not simply be probably lying, but could not possibly be telling the truth.[56]

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purposeful and teleological behavior. Feedback, in fact, does not play a significant role in this later paper at all. Instead, the compass behavior is differentiated from the adaptive behavior on the basis of the "passive-active" behavior distinction made in the original paper. Although it may not be true that all non-adaptive goal-directed behavior is passive, this distinction is probably generally true. It takes energy to resist disturbances and most systems do not use the energy of the disturbance to resist it; they require their own energy source which puts these systems in the active category of Rosenblueth and Wiener. Still, the active-passive designation cannot be behaviorally determined. Why Rosenblueth and Wiener discarded the important and useful distinction with regard to negative feedback is an unanswered question. It seems that Wiener had already discarded it when he wrote Cybernetics in 1947, and this action of Wiener's led to a serious problem with the first chapter of his book. See below, pp. 283-285.

[55] A. Rosenblueth and N. Wiener, "Purposeful Behavior," p. 319.

[56] R. Taylor, "Rejoinder," p. 329. This example of Taylor's is essentially the "multiple goals" objection of Isreal Schleffler. See J. Canfield, Purpose in Nature, p. 56.

Several points need to be made here. First, Taylor has limited in crucial ways the observable behavior. We are asked to make an experimental judgment on the basis of a single incident, and one which is not fully reported. Did or did not the driver hit the pedestrian? If a driver runs down a pedestrian (or different ones) time after time under similar conditions we would have reasonable cause to assume the purpose of the driver. More generally, accidental coincidences will always occur and thus behavioral analyses will always require that the behavior be repeatable or have a statistical bias toward repetition. As Rosenblueth and Wiener stated:

> To give causation degrees means to correlate changes in initial conditions with the corresponding changes in later conditions. This requires the entire apparatus of statistics and probability. However, in a science subject to statistics and probability it is possible to enquire not only to what extent a phenomenon causes another, but also to what extent a purpose causes a result.[57]

The statistical aspect of data evaluation points to an important characteristic of cybernetic analysis. We can never be absolutely certain that the goal toward which the system appears, by its behavior, to be directed, is the "final" goal of the system. The

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[57]A. Rosenblucth and N. Wiener, "Purposeful Behavior," p. 320. See also p. 325, point "d." In the interesting comment quoted above, the authors are using "purpose" in a behavioral sense. That is, if we hypothesize from behavior studies that the system is directed toward a certain goal (or "purpose"), we can make statistical correlations between the goal and the behavior of the system. Thus, in the example of the driver, it takes such a statistical correlation to correlate the actions of the driver with his purpose. The statistical correlation of a single event is useless; the conclusion is never absolute, but becomes more and more probable as the number of events increase.

system may repeatedly go to a certain state, leading us to one conclusion, and then suddenly exhibit new behavior. If we observe a thermostat-furnace system, with the thermostat set at 70 degrees, we can observe many cycles of the system which will indicate that the goal of the system is to maintain this temperature. But if it is actually a time-controlled thermostat, set to shut off the furnace at nine o'clock in the morning, we will have to revise our conclusion regarding the system's behavior when the nine-o'clock data is collected. This limitation of non-certainty is, however, a limitation of any experimental determination. The fact that conclusions can be revised on the basis of new data is an essential part of the scientific method.[58]

The basis of many problems related to human behavior is that the human is an enormously complex system. If a timed thermostat can fool us by not immediately displaying the full expression of its structural potential, should we be surprised that it is difficult to behaviorally discern the "true" purposes or goals of humans? This observation does not invalidate the view that goal-directedness is a structural property, it only invalidates the idea that behavioral studies must give an absolute indication of the full structural potential of the system. As has just been observed, to expect such an absolute

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[58]Other objections raised by writers such as R. Taylor and I. Schleffler do not take the statistical aspect of the determination of goal-directedness into consideration. The objection of the "missing goal" belongs in this category. See R. Taylor, "Comments," p. 329; and J. Canfield, ed., Purpose in Nature, pp. 41-42.

indication would be unrealistic in any realm of scientific inquiry.[59]

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[59]For an interesting discussion of the issues related to the consideration of mind as mechanism and the "mind-body" problem, from the robotic viewpoint, see Norman J. Faramelli, "Some Implications of Cybernetics For Our Understanding of Man: An Appreciation of the Works of Dr. Norbert Wiener and Their Implications for Religious Thought" (doctoral dissertation, Temple Univ., 1968), pp. 144-247. For a fascinating view of mind as a subjective awareness of the goal-directed cellular organization of the organism, see, Edmund W. Sinnot, Cell & Psyche: The Biology of Purpose (New York: Harper & Row, Pub., 1961).

Summary of Response to Objections

One may summarize the responses to the two main classes of objections to the behavioral approach, raised at the beginning of this section, as follows:

With regard to the problem of definition of goal-directed systems we see that once the distinction between adaptive and non-adaptive goal-directed behavior is made the necessity for equating simple behavior with complex behavior is removed, and yet, the inevitable conclusion that simple phenomena, such as the pendulum, are somehow goal-directed is preserved. Goal-directedness is freed to become the central theme of the study of both natural and man-made organization.

With regard to the problem of the interpretation of goals by behavioral studies we recall that the primary objective of the behavioral approach in cybernetic analysis is to obtain an understanding of adaptive goal-directed systems. This approach inevitably involves experimental data which must be evaluated statistically, and hence conclusions will only be as strong as the statistical inferences. No absolute certainty of the ultimate behavioral potential of any system can be obtained behaviorally. This limitation does not detract from the usefulness of conclusions made on a statistical basis, especially when these conclusions are successfully used to make predictions relating to further behavior of the system.

SUMMARY

In order to precisely state the role of negative feedback in scientific explanation, we must first define the behavior of systems employing negative feedback. This definition has been given under the category of "adaptive goal-directed behavior" (p. 31). The fact that a system persists toward a certain state, or goal, even while being disturbed is assumed to result from a negative feedback mechanism within the system, an assumption which can be tested by the success of predictions made from negative feedback theory. A functional study of the system is required to explain how the components interact to produce the negative feedback mechanism. Again, the success of the functional study is determined by the success of predictions of behavior made from it. The use of negative feedback in both the behavioral and functional studies has here been subsumed under a research program entitled "cybernetic analysis." The methodology described in this program mirrors the actual use of negative feedback in scientific research although this role has never, to the author's knowledge, been formally defined.

A crucial distinction in the definition of cybernetic analysis is that between adaptive and non-adaptive goal-directed behavior. Without this distinction the concept of goal-directedness becomes useless because all but totally random systems become classified alike. Once the distinction is made, however, this problem is eliminated and the scientific view of nature shifts to one dominated by the goal-directedness of natural systems. This enormous

shift of world view is beneficial because it reduces dependence on unfocussed reductionist investigations in which the functioning of the individual components is emphasized in isolation from the overall behavior of the system. The functional, or reductionist, approach is transformed from a mindless enumeration of component characteristics into a study in which these characteristics are understood in light of the overall behavior of the system. This benefit applies both to adaptive and non-adaptive goal-directed systems. In providing a mechanistic basis for the explanation systemic behavior, cybernetic analysis provides a bridge between holistic and reductionistic explanation. No claim is made that all adaptive goal-directed behavior must be explainable in terms of cybernetic analysis, which is a research program. The relative value of the program is an experimental issue. Results of the use of negative feedback in many areas indicates the potential of the program, however.

The early paper, "Behavior, Purpose and Teleology," modified in certain respects, remains the best introduction to cybernetic analysis. The driving force behind that paper and the many other events relating to the dissemination transformation of the feedback concept was Norbert Wiener, the central figure of this dissertation, to whom we now turn.

CHAPTER THREE

THE EARLY DEVELOPMENT OF NORBERT WIENER

The term "extraordinary" is apt to describe almost every aspect of Norbert Wiener's life and character.[1] As a child prodigy he was the subject of newspaper and magazine articles. Later, as a mathematics professor at the Massachusetts Institute of Technology, he was not bound by the limits of that discipline and became highly influential in the fields of biology, engineering, philosophy and the social sciences. Wiener was gifted to an extreme degree. But the realization of this natural potential was unquestionably stimulated by the unusual home environment in which he was raised; an environment in which scholarship was made akin to religion. The person

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[1]The principal source of information concerning Wiener's childhood and adolescence is his own two-volume autobiography: Ex-prodigy (New York: Simon and Schuster, Inc., 1953; reprint ed., Cambridge, Mass: The M.I.T. Press, 1979), hereafter cited as Wiener Exp; and I am a Mathematician: The Later Life of a Prodigy (Garden City, N.Y.: Doubleday, 1956; reprint ed., Cambridge, Mass: The M.I.T. Press, 1973), hereafter cited as Wiener, Iamm. A very large number of Wiener's letters and papers, including some material from as early as when Wiener was age eight, are preserved in the Norbert Wiener Papers (MC 22), Institute Archives and Special Collections, M.I.T. Libraries, Cambridge, Mass. (hereafter cited as Wiener Papers).

Although not infallible, Wiener's autobiography is generally quite accurate as an historical record, at least as far as can be determined by a comparison with his extant letters and papers of a given period. A few exceptions to this reliability are noted in the body of this text. As a child prodigy, Wiener was the subject of several newspaper and magazine stories. One of these, Bruce Addington, "New Ideas in Child Training," American Magazine 72 (1911), pp. 286-294, is especially valuable in illuminating Wiener's father's philosophy of educating his children (see below, pp. 71-72).

The first two volumes of Wiener's collected works, which when completed will total four volumes, have been published. Both of the primarily responsible for this environment was Wiener's father, Leo Wiener, a philologist and professor of Slavic languages at Harvard.[2] The elder Wiener had strong and very unconventional convictions concerning the manner in which children should be educated, and he utilized his first-born child, Norbert, to test these ideas. Wiener was, in effect, part of his father's educational experiment. Wiener's father thus played an immense role in his development, a role

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published volumes contain the complete bibliography of Wiener's published works. See Norbert Wiener, Norbert Wiener: Collected Works, 2 vols., ed., P. Masani (Cambridge, Mass: The M.I.T. Press, 1976-1980).

Among the secondary sources concerning Wiener's life and works, the most useful include: Steve J. Heims, John von Neumann and Norbert Wiener (Cambridge, Mass: The M.I.T. Press, 1980); N. Levinson, "Wiener's Life," in Bull. Am. Math. Soc. 72, no. 1, pt. 2 (1966), pp. 1-38 (part two of this issue was a tribute to Wiener and included contributions by, besides Levinson, W. Rosenblith, J. Wiesner, M. Brelot, J. P. Kahane, S. Mandelbrojt, M. Kac, L. Doob, P. Masani and W. L. Root, as well as a bibliography of Wiener); P Masani, "Wiener, Norbert: His Life and Work," in Belzer et al, eds., Encyclopedia of Computer Science and Technology, vol. 14 (New York: Marcel Dekker, Inc., 1980), pp. 78-103; Hans Freundenthal, "Wiener, Norbert," in Charles Coulston Gillispie, ed., Dictionary of Scientific Biography, vol. 14 (New York: Charles Scribner's Sons, 1976), pp. 344-347; Norman J. Faramelli, "Some Implications of Cybernetics for our Understanding of Man: An Appreciation of the Works of Dr. Norbert Wiener and their Implications for Religious Thought," (Ph.D. dissertation, Temple Univ., 1968); and W. Rosenblith et al, "Norbert Wiener: 1894-1964," in J. Nervous and Mental Disease 140 (1965), pp. 1-16 (this number was a tribute to Wiener and contains short contributions by, among others, Anatol Rapoport, D. M. MacKay and W. S. McCulloch).

[2]Wiener devoted long sections of his autobiography to his father and his father's influence on him. There are, on the other hand, only brief and passing references to his mother, Bertha Wiener. Most of Wiener's intellectual correspondence was addressed to his father alone. It does seem, as Wiener indicated, that his father, a dominating figure, primarily shaped his early intellectual development. One might expect that his mother had a larger role than was indicated by Wiener, but this role has not been recorded.

which can best be understood by examining Wiener's family background and the influence of his unusual home environment on his early development.

Leo Wiener was Born in Byelostok, Russia in 1862. The Wiener family tradition traces its ancestors through Aquiba Eger, the Grand Rabbi of Posen from 1815 to 1837, and possibly Moses Maimonides. Leo Wiener was apparently studying Latin and Greek in Europe as a young man when he was influenced by a student meeting to emigrate to America. In the original plan he and a student friend were to "found a vegetarian-humanitarian-socialist community in Central America." When the other student did not follow through with the plan, Leo was stranded in New Orleans, virtually penniless.[3]

A combination of odd jobs and wandering led him to Kansas City where his native interest in languages prompted him to join a local course being given in Gaelic. He excelled in the course and eventually became its teacher. He, in fact, soon became the leader of the local Gaelic society, becoming known in the process as the "Russian Irishman."[4] This position represented the start of Leo Wiener's career in philology which was to last three decades.[5]

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[3] Wiener, Exp, pp. 10-15.

[4] Ibid., p. 19.

[5] Wiener later perceived a connection between his own philosophy of mathematics and his father's view of philology. Wiener wrote in 1955:

> "Father was a philologist who regarded the history of languages not as the quasi-biologic growth of almost isolated organisms but rather as an interplay of historic forces. For him, philology was a tool of the cultural historian, exactly as

In 1893 Leo Wiener married Bertha Kahn, the daughter of a department store owner in St. Joseph, Missouri. He took a position teaching Modern Languages at the University of Missouri in Columbia, and there Norbert was born November 26, 1894.

Leo Wiener lost his position at the University of Missouri through an internal political struggle and took his family to Boston where he thought the prospects would be favorable for finding another teaching position. He was employed briefly at Boston University and then at the New England Conservatory of Music. Finally, he obtained an instructorship in Slavic Languages at Harvard. He remained at Harvard as a philologist of considerable renown until his retirement in 1930.

Wiener's Childhood

When the Wiener family moved to the Boston area, Norbert was an infant. He grew up in a strongly academic environment. His father had an extensive library and there were frequent academic and literary visitors to the house. His father's library and interests were not limited to language-related subjects; science and mathematics were strongly represented as well.

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the spade is of the archeologist. It is not surprising that the son of a father who could not be contented with the formal and the abstract in the study of languages should himself fail to be contented with that thin view of mathematics which characterizes those mathematicians who have not made a real contact with physics." (Wiener, Iamm, p. 358.)

Wiener demonstrated a natural affinity for scientific books at an extremely early age. He received a volume on mammals for his fourth birthday and began reading very soon thereafter. By the age of seven his reading matter ranged from "Darwin and Kingsley's Natural History to the psychiatric writings of Charcot and Janet."[6] Wiener also wrote that by the time he was seven his reading in biology and physics were advanced beyond that of his father.[7] He went to the Agassiz Museum regularly, soon knowing by heart the exhibits there, and recalled that at this early age he "longed to be a naturalist as other boys long to be policemen and locomotive engineers."[8] These interests were extraordinary for a boy not even ten years old, but they did not seem so to Wiener. As he later wrote:

> It is, however, impossible for the child, whether he be prodigy or not, to compare the earlier stages of his intellectual development with those of other children until he has reached a level of social consciousness which does not begin until late childhood. To say that one is a prodigy is not a statement which concerns the child in question alone. It is a statement which concerns the relative rate of his intellectual development with that of others. And it is a thing which his parents and teachers can observe far earlier than he can himself. In one's earlier stages of learning, one is one's own norm, and if one is confused, the only possible answer is that of the Indian, 'Me not lost, wigwam lost.'[9]

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[6]Wiener, Iamm, p. 18.

[7]Wiener, Exp, p. 69.

[8]Ibid., p. 64.

[9]Ibid., p. 44.

Thus the young Wiener did not view his early almost insatiable desire to read as being unusual. He was brought up in a highly intellectual atmosphere and his behavior to him seemed the norm rather than unusual. This attitude was strongly reinforced by his father, who described his approach to the education of his children in an 1911 interview.

> I believe, to begin with, that children are naturally more intelligent than parents seem to regard them, and that if their natural intelligence is recognized and wisely directed they will display a most gratifying brightness and responsiveness. Instead of leaving them to their own devices---or, worse still, repressing them, as is generally done---they should be encouraged to use their minds, to think for themselves, to come as close as they can to the intellectual level of their parents.
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> This is not so hard a task as one would imagine. It requires, though, on the part of the parents, a constant watchfulness over their words and actions. When in the presence of their children they should use only the best of English, must discuss subjects of real moment and in a coherent, logical way; must make the children feel that they consider them capable of appreciating all that is said. In a word, the parents must from the beginning surround their children with an intellect-stimulating environment; or, as you would perhaps prefer to say, must utilize the power of 'suggestion' as an aid in their development.
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> What is no less important, every child should be carefully studied to determine aptitudes. One child will have a natural bent for mathematics, another for reading, another for drawing, and so forth. Whatever it is, it can be utilized by the parent as affording a line of least resistance along which to begin the educational process. Take the case of my boy Norbert. When he was eighteen months old, his nurse-girl one day amused herself by making letters in the sand of the seashore. She noticed that he was watching her attentively, and in fun she began to teach him the alphabet. Two days afterward she told me, in great surprise, that he knew it perfectly.
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> Thinking that this was an indication that it would not be hard to interest him in reading, I started teaching him how to spell at the age of three. In a very few weeks he was reading quite fluently, and by six was acquainted with a number of excellent books, including works by Darwin, Ribot, and other scientists, which I had put in his hands in order to instill in him something of the scientific spirit. I did not expect him to understand everything he read, but I encouraged him to question me about what he did not understand, and, while endeavoring to make things clear to him, I tried to make him

> feel that he could, if he would, work out his difficulties unaided. The older he grew the more I insisted on this, on the one hand keeping up his interest by letting him see that I was interested in everything he was doing, and on the other encouraging him constantly to think for himself. . . . My contention is that the way to teach a child is to train him first, last, and all the time, how to think; to ground him in the principles of reasoning, so that he can utilize and apply them in the study of any subject.[10]

There is little question that Wiener not only thrived on his early scientific reading, but that its content influenced his later life and actions. Much of what he read at this early age seems to have stayed with him all his life. In the first volume of his autobiography, Ex-Prodigy, published when he was 59, he recalls reading at age seven or eight an article in physiology which "excited in me the desire to devise quasi-living automata, and that the notions I acquired from it survived in my mind for many years until they were supplemented in my adult life by a more formal study of neurophysiology."[11] We have here an extraordinarily early interest in some of the problems which were to occupy him some forty years later during the development of cybernetics. This early interest is also evident in his statement that when he was about eight and reading in botany and zoology, "it was the diagrams of complicated structure and the problems of growth and organization which excited my interest fully as much as the tales of adventure and discovery."[12]

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[10]Addington, "New Ideas," pp. 291-292.

[11]Wiener, Exp, p. 65.

[12]Ibid., p. 64.

Scientific toys also fascinated the young Wiener. He remembered playing a great deal with miniature electric motors, microscopes, magnifying lenses, a megaphone and a kaleidoscope. When Weiner was seven his father hired a chemistry tutor for him and a chemistry lab was set up in the nursery; Wiener later admitted that it was the "smellier of the experiments" which most appealed to him.[13] But books, and in particular, scientific books, played the central role in his unusual early education. Wiener later wrote, quite simply "I devoured every scientific book on which I could lay my hands."[14]

At age seven Wiener still had not had any formal schooling. It was then decided that he should enter a local school in the third grade. Although his reading skills, reasoning power and general intellect were far beyond his years, he was actually below average in certain basic skills. His handwriting was clumsy and awkward. He was also quite show in arithmetic skills, still counting on his fingers when his classmates had advanced beyond that point. Even given this odd combination, it was decided by his teachers and parents that he really belonged in the fourth grade. The fourth grade teacher would not tolerate Wiener's shortcomings, however, and the result was that Wiener's father took him out of school altogether and assumed full responsibility for his education.

Wiener states that his early problems with arithmetic skills stemmed from the fact that his powers of understanding were beyond

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[13] Ibid.

[14] Ibid., p. 66.

his powers of manipulation.[15] While he could not perform the basic arithmetic operations rapidly and precisely, he was also troubled by more theoretical questions, such as why the communicative, distributive and associative laws were true. As Wiener has summed it up, "On the one hand, my understanding of the subject was too fast for my manipulation, and on the other hand, my demands in the nature of fundamentals went too far for the explanations of a book devoted to manipulation."[16]

Wiener's father's solution to his son's mathematical problems was, as soon as he had reassumed total responsibility for Norbert's education, to start teaching him algebra. During the next three years, then, starting at age seven, and "without excessive difficulty,"[17] the young Wiener not only learned algebra, but plane and analytical geometry and trigonometry from his father. Although his father was a philologist by profession, he had considerable mathematical talent of his own; it was only when Wiener became an advanced college student that he felt he had outpaced his father in this discipline.

With regard to his father's teaching of him, Wiener later commented that,

> I do not think his original purpose had been to push me. However, he had himself started his intellectual career very young, and I think that he was a little surprised by his own success with me. What had started as a makeshift was thus

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[15]Ibid.

[16]Ibid., p. 46.

[17]Ibid., p. 76.

> continued into a definite plan of education. In this plan, mathematics and languages (especially Latin and German) were central.[18]

Whether or not his father's original purpose had been to "push" Norbert, the tutoring process which took place was, to say the least, severe. The lessons often ended in a family scene with Wiener weeping, his father raging, and his mother doing her best to make peace. The particulars of the tutoring were well recalled by Wiener:

> Algebra was never hard for me, although my father's way of teaching it was scarcely condusive to peace of mind. Every mistake had to be corrected as it was made. He would begin the discussion in an easy, conversational tone. This lasted exactly until I make the first mathematical mistake. Then the gentle and loving father was replaced by the avenger of the blood. The first warning he gave me of my unconscious delinquency was a very sharp and aspirated 'What!' and if I did not follow this by coming to heel at once, he would admonish me, 'Now do this again!' By this time I was weeping and terrified. Almost inevitably I persisted in sin, or what was worse, corrected an admissible statement into a blunder. Then the last shreds of my father's temper were torn, and he addressed me in a phraseology which seemed to me even more violent than it was because I was not aware that it was a free translation from the German. Rindvieh is not exactly a complimentary word, but it is certainly less severe than 'brute'. . . .[19]

Wiener says that he learned to accept the scoldings, but what hurt him terribly was his father's publicizing of his "juvenile ineptitudes," repeating them at the dinner table and to company. Wiener was also repeatedly told by his father that his grandfather's worst traits were latent in him, waiting to be expressed. It is fair to say that these experiences traumatized the young Wiener. He felt

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[18]Ibid., p. 67.

[19]Ibid.

that his disgraceful performances and the resulting family fights were shattering the unity of his family and consequently his own security.[20] Despite these insecurities, however, the elder Wiener's intellectual aggressiveness and brashness were successfully transferred in both their positive and negative aspects. As Wiener later noted, "The arduous course of training to which I was put tended to isolate me from the world and to give me a certain aggressive, unlovable naiveté."[21] The characteristics of intellectual brashness and insecurity would remain with Wiener for life.

In spite of his traumatic educational experiences, Wiener idolized his father as a true scholar, a model of learning, a noble and uplifting figure "who held high the banner of intellectual truth," a man who "joined the best traditions of German thought, Jewish intellect, and American spirit," adding that he was "given to overriding the wills of those about him by the sheer intensity of his emotion rather than any particular desire to master other people."[22] With regard to his father's methods of teaching, he dryly commented, "When Father was teaching (but not always when he was teaching me), he tried to draw out his students interests rather than to compel them to think in preassigned directions."[23] Finally, Wiener later summed up his relationship with his father in the

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[20]Ibid., p. 68.

[21]Wiener, Iamm, p. 19.

[22]Ibid., p. 18.

[23]Wiener, Exp, p. 20.

following statement which reveals as much about the character and values of Wiener as it does about his father:

> From him I learned the standards of scholarship which belong to the real scholar, and the degree of manliness, devotion, and honesty which a scholarly career requires. I learned that scholarship is a calling and a consecration, not a job. I learned a fierce hatred of all bluff and intellectual pretense, as well as a pride in not being baffled by any problem which I could possibly solve. These are worth a price in suffering, yet I would ask this price to be exacted of no man who has not the strength to stand up to it physically and morally. This price cannot be paid by a weakling, and it can kill.[24]

Elsewhere Wiener posed himself the question, "Have I gained or lost from my father's unconventional training?" He responded:

> I do not know, for I have had only one life to live. My conjecture is that under a more conventional and milder regime I might have come through with less emotional trauma, but that I would not have developed the strong individuality of my scientific vein, which was due to early contact with a very powerful and very individualistic man. It was this struggle to maintain my individuality in the presence of a tremendously vigorous father which certainly gave the very specific form to my work which it later assumed.
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> While I might have achieved something under another training, one thing is clear: that without any training and guidance at all, my career would have been hampered and my productivity would have been distorted. It is very easy for a constitutionally vigorous mind to fritter its power away in trivialities. I put the highest value on my early contact with the standards of intellect, and even though quite a different contact might have set me up as a scholar in another way, the absence of contact would have left me an ineffectual crank.[25]

Physically, Wiener was quite sturdy as a child, and already somewhat stout. Far from enjoying only intellectual challenges, he loved hiking through the mountains, exploring ponds and brooks, rowing, and swimming. These activities remained lifelong enjoyments

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[24] Ibid., p. 292.

[25] Wiener, _Iamm_, p. 360.

for him and he later became an active member of the Appalachian Mountain Club. Much of Wiener's difficulty with work requiring manual dexterity stemmed from the fact that he was, from childhood, strongly myopic; a condition which could not be much improved even with corrective lenses. Wiener felt that his nearsightedness played an important role in the unfolding of his character, later stating,

> While this [myopia] had no direct effect on my physical vigor, it cut me out of that whole sector of boyish life which depends on skill at games. It also tended to accentuate my very marked physical clumsiness. This clumsiness was serious enough on its own merits, but it was further brought out by the way in which my father harped on it and used to humiliate me concerning it. . . .
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> With the inevitable isolation which my father's training gave me, I was a very self-conscious hobbledehoy, subject to alternate moods of conceit when I became aware of my abilities and of great disappointment when I accepted at their face value my father's strictures on my shortcomings, or when I contemplated the long and uncertain road to achievement to which my highly eccentric bringing-up had condemned me.[26]

By the time he was eight the family doctor warned that Wiener's extensive reading had weakened his eyes to the point where the very existence of any vision at all was seriously threatened. He was ordered by his doctor not to read at all for the next six months. During this period the lessons given by his father and his chemistry tutor continued orally. Wiener later noted, "This period of ear training rather than eye training was probably one of the most valuable disciplines through which I have ever gone, for it forced me to do mathematics in my head and to think of languages as they are spoken rather than mere exercises in writing."[27]

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[26]Ibid., p. 19.

[27]Wiener, Exp, p. 76.

By the time Wiener was nine, the family was living on a farm in the rural town of Harvard, outside of Cambridge, Massachusetts. Wiener's father was becoming very active professionally translating twenty-four volumes of Tolstoy, and consequently the time available for tutoring Norbert was reduced. Leo Wiener was now commuting to work, leaving his horse and carriage at the livery stable in the town of Ayer, and then taking the train to Cambridge. Ayer had a high school which was willing to admit young Wiener. Given the ideal location of the school on Leo Wiener's commuting route which would enable Norbert to travel with his father to and from school, and the preoccupation of his father with professional work, it was decided to enroll Wiener in Ayer High School. He was admitted in 1904.[28] Testing indicated that the nine-year-old Wiener belonged in the third year of high school. He graduated two years later, at age eleven.

Wiener's two years at Ayer went as smoothly as possible in a situation where Wiener was about ten and his fellow students about seventeen. The academic nature of the classes presented no particular challenge to him. He continued his studies of the classics, reading Caesar, Cicero and Virgil in Latin, as well as German and English literature. The algebra and geometry given were,

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[28] In Exp, Wiener mistakenly dates his entry into Ayer High School as occurring in 1903 (p. 93). The actual entry date was 1904, as his stated age of nine and his report card from Ayer, still extant in the Wiener Papers, indicate.

for Wiener, "largely review."[29] During the summer between his two years at Ayer, Wiener wrote his first philosophical paper, "The Theory of General Ignorance," by which Wiener sought to demonstrate the impossibility of man's being certain of anything. He applied this theory to science, mathematics, philosophy and religion.[30] Far from being only a childhood idea which he later outgrew, this epistemological scepticism was a continual current in Wiener's life, culminating, as we shall see, in his deep-seated probablistic or "contingent" world view.

Socially, however, Wiener described himself as an "under-developed child."[31] With not quite one year of regular schooling, and that in third grade, he was suddenly thrust into a world of adult-sized people. The chairs were much too big for him and once the teacher even held him on her lap during a lesson. During breaks Wiener would play with the students from the adjacent junior high school who were only two or three years older than he was.

At home he enjoyed his life on the working farm, hiking, playing with pop guns and with his pet goat and dog. He had "a great deal of

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[29] Wiener's report card from Ayer, still extant in the Wiener Papers, indicates that in his first year, entering at age nine, Wiener took courses in English, Virgil, Chemistry, German, Eng. History, Adv. Geometry and American History. During his second year, when he entered at age ten, he took courses in English III, Caesar, Cicero, Adv. Algebra and Sol. Geometry. In these courses he received mostly A's and no grade lower than B.

[30] Wiener, "The Theory of General Ignorance," Wiener Papers. See also, Iamm, p. 33.

[31] Wiener, Exp, p. 92.

time alone in the house to spend reading in father's library,"[32] and was particularly fascinated by Issac Taylor's book on the alphabet which he knew "nearly from cover to cover." He describes himself as being rebellious at the time and devising a secret plan to form a children's organization to resist the authority of their elders. He wondered if he had committed a crime equivalent to treason by even contemplating such an idea, but consoled himself by thinking "even if I had I was too young to be subject to serious punishment for it."[33] At night he would continue to recite his lessons to his father who listened with one ear while he was doing his translations.

The Ayer experience was fondly remembered by Wiener who later said that there he "was given a chance to go through some of the gawkiest stages of growing up in an atmosphere of sympathy and understanding."[34] This memory must certainly have been made more appealing by the tense years which were to follow as the eleven-year-old Wiener prepared to go to college.

Before dealing with his college career, however, one other matter should be addressed and that is the extent to which the very young Wiener could have understood the complex material which he absorbed at such an early age. There is little question that the early acquisition of rather advanced material broadened Wiener's background and helped to provide the strong base from which he

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[32]Ibid., p. 97.

[33]Ibid., p. 99.

[34]Ibid., p. 100.

carried out his highly interdisciplinary approach to the development of cybernetics. We have seen explicit examples where as he later said his early readings in neurophysiology, philosophy, and biology made a strong impression on him and helped to shape his later interest in specific problems. But it is clear that at this early age Wiener was reading purely out of an extraordinary curiosity. He was not then utilizing this material to solve problems, but later the material assumed its place in the larger pattern of his thinking. Thus he was not "understanding" his readings in the same way which he later would, and yet, the readings made an important incipient impression on him so that he could later utilize them. While it is unrealistic to think that Wiener was starting his cybernetic synthesis at this very early age, it is interesting to note that he was already interested in many of the same topics which would later constitute the problem material for cybernetics.

The highest form of learning, for Wiener, involved a process analogous to crystallization in which material previously assimilated is subsequently melded by the individual into a personal framework of understanding. Wiener later related this type of learning to his early intellectual interests:

> Probably much of my early reading was over my head at the time. It is not essential for the value of education that every idea be understood at the time of its accession. Any person with a genuine intellectual interest and a wealth of intellectual content acquires much that he only gradually comes to understand fully in the light of its correlation with other related ideas. The person who must have the explicit connection of his ideas fed to him by his teacher is lacking in the most vital characteristic that belongs to the scholar. Scholarship is a progressive process, and it is the art of so connecting and recombining individual items of learning by the forces of one's

> whole character and experience that nothing is left in isolation, and each idea becomes a commentary on many others.[35]

The problem of having connections made explicitly clear by others versus the self-assimilative process of learning which Wiener describes is a problem which eventually caused Wiener some difficulty. When he later taught classes, his lectures were frequently disorganized because explicit connections were not always made. The exposition of profound ideas without explicit connections was a problem which would also plague his most widely circulated work, Cybernetics. We will examine this problem further in Chapter Six.

An Eleven-Year-Old at Tufts

By the time Wiener graduated from Ayer High School in 1906 he was eleven years old. He had had a total of not quite three years of formal schooling and was now ready to go to college. His father considered sending him to Harvard but rejected this idea in part to avoid the inordinate publicity associated with having an eleven-year-old at Harvard. Instead, it was decided that Wiener would attend nearby Tufts. He took the entrance exams, finding them not difficult, and proceeded to matriculate in a rather standard university program, although at an accelerated pace, and graduated in three years.

Wiener's coursework at Tufts included Greek, German, physics, chemistry, biology, mathematics and philosophy, with a preponderance of courses in the latter three areas. He found the course in the

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[35] Ibid., pp. 65-66.

theory of equations difficult, but was successful at it. He was hampered in his English courses by his poor handwriting, which hindered his ability to freely express himself. In philosophy Wiener found the coursework "dilute," but he greatly supplemented this work by extensive outside reading of Spinoza and Leibnitz, both of whom became important life-long influences.[36]

Wiener describes his non-scientific reading during his stay at Tufts as "omniverous." Ironically, at the same time he was more than keeping pace with his college-level material, being interested, for example, in the representation of four dimensional figures in three-dimensional space, he also spent a great deal of time in the Children's Room of the Boston Public Library reading Jules Verne, detective, and other adventure stories.[37] His interests, however, were not limited to reading. With a neighbor he tried out electrical experiments. One device they made was an electromagnetic coherer, an early form of signal detector, and they also tried to make an electrostatic transformer. Wiener's early interest in amateur radio apparatus later blossomed into a professional involvement in communication engineering, a field which Wiener later felt was an important component of cybernetics.

Socially, Wiener faced the same sort of problems he had faced at Ayer. He was not just eight years younger than his fellow students, but a mere child compared to them. It is interesting to attempt to

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[36]Ibid., pp. 102-114.

[37]Wiener, Exp, pp. 107-108.

picture the innocent eleven-year-old Wiener trying to participate in the social conversations of the other students who were nineteen and older. Wiener does say that they would allow him to participate if "I wasn't too loud and too insistent," adding that, "I was not so much a mixture of a child and man as wholly a child for the purposes of companionship and nearly completely a man for purposes of study."[38]

Toward the end of Wiener's stay at Tufts he became particularly interested in biology. Although he had no trouble with the course-work, his dissections were "too fast and too sloppy." A trip to Wood's Hole was arranged to further assess Wiener's capabilities in this field. Wiener later summed up the test succinctly: "All that I remember is that my dissections were not particularly brilliant and that in a few days a notice appeared on the dock where I was working saying: 'No fish cut up here.'"[39]

His myopia undoubtedly contributed to his clumsiness. This problem was particularly evident during his last year when he took organic chemistry. Wiener wryly notes that he took the course "at probably the greatest cost in apparatus per experiment ever run up by any Tufts undergraduate."[40]

More serious problems haunted Wiener at Tufts, however. As he later put it, "This was the first time that I became fully aware of

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[38]Ibid., pp. 106-107.

[39]Ibid., p. 110.

[40]Ibid., p. 105.

the fact that I was considered a freak of nature, and I began to suspect that some of those about me might be awaiting my failure."[41] Reporters began to "pester" him in order to write stories on the child prodigy. The feeling of Wiener that others were awaiting his downfall led to insecurities which apparently lasted most of his life. These feelings are evident in a statement of Wiener's written some forty years later:

> There is a tradition, not confined to the United States, that the child who makes an early start is intellectually drawing on his life capital of energy and is doomed to an early collapse and a permanent second-ratedness, if not to the breadline and the madhouse.[42]

Wiener's feeling that the child prodigy is "desperately unsure of himself" has been corroborated by at least one study of the gifted child.[43] Wiener blamed his failure to be elected to Phi Beta Kappa upon graduation on his being a prodigy; feeling that the society was afraid to entrust the prodigy with the honor.[44]

Wiener's insecurities extended into an obsessional fear of death. He wrote that he could not read a novel "without figuring out the ages of the characters and how many years they had left them

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[41]Ibid., p. 116.

[42]Ibid., p. 117.

[43]Wiener's views regarding the insecurity of the gifted child has been supported by at least one study of this group: Z. F. Bereday and Joseph Lauwerys, eds., The Gifted Child (New York: Harcourt, Brace and World, Inc., 1962). This report cites Wiener in particular as having been unusually gifted even within the gifted group (p. 42). The report went on to say that "within this [gifted] group maladjustment tends to increase in severity and frequency with the increase in the intellectual disparity" (ibid.).

[44]Wiener, Exp, p. 116. Wiener did graduate Cum Laude.

to live." He measured his own years against his probable future life. This fear of death was reinforced by a "fear of sin," stemming from what Wiener refers to as his "doubly Puritan environment" of the New Englander and the Jew.[45]

Despite these problems, Wiener graduated from Tufts in 1909 with an outstanding record. His increasing interest in biology caused him, over his father's objections, to enroll as a graduate student in zoology at Harvard the following fall. His father felt that he should be pursuing a career in mathematics, but it was apparent that although Wiener was talented in this area, it was not yet his chief interest.

From Zoology to Mathematical Logic: Harvard, Cornell and Harvard Again

Concerning his desire to be a biologist, Wiener later wrote, "I had moved into biology, not because it corresponded with what I knew I could do, but because it corresponded with what I wanted to do."[46] Wiener was not a success at Harvard. He broke glass apparatus, bungled section cutting and staining procedures. Again Wiener attributed his failure to the inability to make his hands follow the directions of his thinking: "I would see the end to be accomplished long before I could labor through the manipulative stages that were to bring me there," adding that his habit of hurrying his work

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[45]Ibid., pp. 119-120.

[46]Ibid., p. 128.

rather than going into fine points was his tendency throughout life."[47]

After one unsuccessful term at Harvard, Wiener's father intervened and decided to take advantage of Wiener's ability in philosophy by enrolling him the following academic year in the Sage School of Philosophy at Cornell. Wiener deeply resented what he considered to be interference by his father, but nevertheless obtained a scholarship to Cornell and enrolled for the 1910-11 academic year.

Wiener refers to his year at Cornell as his "black year" mostly, it seems, because of his emotional problems being away from home for the first time and, in a sense, banished there by his father. In addition, he faced the problem of suddenly discovering, accidently, that he was Jewish. His parents, though both Jewish themselves, had never told the young Wiener of his background.[48] To make matters worse, his mother was anti-semitic and had instilled in Norbert quite

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[47] Ibid., pp. 128, 130.

[48] It seems incredible that Wiener, at age 15, did not know of his religious background. He later explained the circumstances leading to his discovery. His father had just brought him to Cornell from Boston on the "inter-urban trolley." Wiener's father, in a casual conversation with a Cornell professor, mentioned that Wiener family legend traced the family history to the great philosopher Maimonides. Wiener had never heard of Maimonides and looked him up in an encyclopedia. He later wrote:

> "I was naturally interested to have such an important figure on which to hand our family pride, but the implications of the legend came to me with a profound shock. For the first time, I knew that I was Jewish, at least on my father's side. You may ask how it was possible for an intelligent boy like me to have any doubts about this when my grandmother Wiener as far back as I could remember had received a newspaper printed in

negative feelings about Jews. Wiener suddenly had to reconcile himself to the fact that he was a member of what he had been taught was a gluttonous and arrogant group.[49] Wiener realized he could not suddenly object to anti-semitism solely on the basis that he had now found himself to be Jewish. And yet he could not become a true

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what I knew to be Hebrew characters. I can only answer that the world is complex, with ramifications not very understandable to an adolescent, and that it still seemed possible to me that there might be non-Jewish people in eastern Europe who used the Jewish characters. Furthermore, my cousin Olga had once told me that we were Jews; but my mother had contradicted this at a time when I had not yet learned to question the word of my parents.

At that time the social disadvantage of belonging to the Jewish group was considerably greater than it is now and there was definitely something to be said for allowing children to grow up through their early lives without consciousness of the social stigma of belonging to an unfavored group. I do not say categorically that this was the right thing to do; I merely say that it was a defensible thing to do and could be motivated---in fact, it was actually motivated---by a desire for the protection of the children. The moral responsibility of a policy like this is great. It is done nobly or it is done basely." (Wiener, Exp, pp. 143-145.)

[49] Although she had one non-Jewish grandparent, Wiener's mother was Jewish. Wiener later wrote:

"The responsibility for keeping the fact of my Jewishness secret was largely my mother's. My father was involved in all this only secondarily and by implication. . . . Father had been engaged in various negotiations with the Jewish Publication Society and with other similar Jewish organizations, and I gather these had involved considerable friction. Later I found that Father always claimed that the friction was the result of an arrogant insistence on the part of the Jewish organizations that a Jew was a Jew before he was a man, and that he owed inalienable allegiance to his own group before humanity itself. My father was always an individual, and was the last man in the world to stand pressure of this kind.

My mother's attitude toward the Jews and all unpopular groups was different. Scarcely a day went by in which we did not hear some remark about the gluttony of the Jews or the bigotry of the Irish or the laziness of the Negroes." (Ibid., p. 145-146).

convert to Judaism either for intellectual reasons he later described:

> There is something against the grain in the attitude of abnegation and of denial of personal judgment in the wholesale acceptance of any creed, whether in religion, in science, or in politics. The attitude of the scholar is to reserve the right to change his opinion at any time on the basis of evidence produced, and I was born and bread to the scholar's trade.[50]

Wiener's ultimate solution to the problem was to reject prejudice of all types as an evil. He subsequently befriended and aided minority individuals in this country, especially foreigners, and was interested in helping those with high ability who, because of their position in society, could not use these abilities to their full advantage.[51] His dislike of prejudice in all forms was reflected in his strong anti-nationalist feelings; he became something of a pacifist after World War II. He travelled widely from

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[50] Ibid., p. 154.

[51] Wiener's graduate students included those from Japan, China and South America. Y. W. Lee, from China, one of his first graduate students, later became one of his closest associates. Wiener especially liked the Mexican and Chinese cultures, spending much time in these two countries.

One of the most interesting cases of Wiener helping the downtrodden in society stems from his involvement with one Frank Scimone, a convict at Attica (New York) State Prison, who was serving a life term for second degree murder (personal communication from Harold J. Smith, Supt., Attica State Prison, Sept. 18, 1980). Scimone wrote to Wiener during World War II that he was interested in mathematics and asked for some help. Wiener not only replied, but sent him a library of mathematical works, visited him in prison, and soon had him doing war-related mathematical work for the government. Scimone was paroled in July, 1945 (see letters of Wiener to Scimone, Sept. 23 and Sept. 30, 1938 and Scimone's letter to Wiener of Sept. 3, 1941, all in the Wiener Papers).

his graduate school days onward and maintained a strong internationalist perspective both politically and academically. The social hardships Wiener endured because of his physical awkwardness, his experiences as a child prodigy, and his religious problems would have caused many people to become antisocial. Although Wiener retained many insecurities into adulthood, he apparently turned these difficult experiences into a means for gaining insight into social problems and improving his own character.

Wiener's work at Cornell was primarily in the classics and seventeenth and eighteenth century philosophy. He also took some courses in mathematics including one in the theory of functions of a complex variable which Wiener said was beyond him at the time.[52] Although his work did not suffer from his personal problems, it became apparent that his scholarship was not going to be renewed. His father intervened again, this time transferring him to the Graduate School of Philosophy at Harvard. Wiener spent the summer of 1911 at home waiting for the new school year. This period was an especially difficult one for him. In addition to the expected problems of a sixteen-year-old living at home with his parents, there were two other especially difficult areas. In July of that summer an interview with Wiener's father was published in which he stated that the idea that Norbert was unusually gifted was "nonsense," and that it was to the father's training "that we must attribute the

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[52] Wiener, Exp, p. 150.

results."[53] The interview had a devastating effect on Wiener. He later said, "It declared to the public that my failures were my own but my successes were my father's."[54] The second problem was that Wiener's father insisted that Norbert train his five-year-old brother, Fritz, using the same methods his father had used with Norbert at that age. Wiener resented this enforced role, especially because Fritz showed none of the unusual aptitudes that his older brother had had at the same age.[55] Wiener was quite unhappy living

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[53] Addington, "New Ideas," p. 291.

[54] Wiener, Exp, p. 159.

[55] Besides his brother Fritz, Wiener had two younger sisters, Constance, born in 1899, and Bertha, born in 1902. Wiener's father also attempted to apply his demanding educational methods to the sisters as well as the brothers, but in different areas. Wiener later wrote:

> "My father's revolutionary theories of education were confirmed in his eyes by the success which, with all my shortcomings, I had already found in intellectual work. It soon became clear that my sisters, although very clever girls by any ordinary standard, were not responding to my father's training as I had. And in part, my father did not expect as much of them. This was laid to their being girls, unable to stand up to the severe discipline to which I had been subjected.
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> Our family portioned out the fates of the family members in advance. The expectation that my sister Constance was to be the artistic one made my parents assign music, painting, and literature to be her field. . . . The case of my brother Fritz was of course a very different matter from that of my sisters. It was not until I was a graduate student at Harvard that he had reached the age where his education severely impinged upon us. He was destined by my parents for the same career of scholarship as I. This time there was no question of weaker demands on the weaker sex, and my father's educational theories had to be faced in their full significance." (Ibid., pp. 157-158).

Constance, in fact, became a mathematician.

at home, but it would be two years before he would become "emancipated." These two years would be spent completing his doctorate in philosophy at Harvard.

Wiener was sixteen when he entered the Harvard Graduate School. One of the first courses he took was with George Santayana, a course which instilled in him strongly the feeling that "philosophy was an intrinsic part of life, or [sic] art, and of the spirit."[56] There were also important contacts with Josiah Royce. Royce introduced Wiener to mathematical logic, and in Wiener's words, Royce's two-year seminar on the scientific method gave him "some of the most valuable training I have ever had."[57] The heterogeneous structure of the seminar group paralleled the wide scope of Wiener's interests throughout life. In the group was an expert on Hawaiian volcanoes, a linguist, an operational philosopher, the head of Boston's Psychiatric Hospital, and a physiologist.[58]

During his stay at Harvard, Wiener had not yet been captured by mathematics proper, but concentrated on mathematical logic, a field intermediate between philosophy and mathematics. One of the reasons he was happy with this field was that it was a field of mathematics in which his father did not have expertise. Mathematics was Wiener's own tool with which he could "storm the gates of success."[59] In 1912,

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[56] Ibid., p. 164.

[57] Ibid., pp. 165-166.

[58] Ibid., p. 166.

[59] Ibid., p. 122.

one year after entering Harvard, Wiener received his M.A., saying that "It did not represent any particular stage in the voyage leading to the Ph.D., but it was convenient to have in case I should meet any obstacle the next year."[60]

Wiener was especially taken by the presentation of mathematical philosophy of E. V. Huntington, a faculty member and neighbor of the Wieners, who had tutored him while he was still in high school. The work with Huntington revolved around the statements specifying a mathematical structure, and the need to find particular examples which would satisfy these postulates.

Wiener attempted to work with Royce on his doctorate, in mathematical logic, but Royce fell ill. Wiener worked instead with Karl Schmidt of Tufts who suggested that Wiener work on a comparison of the algebras of Schroeder, Whitehead and Russell. Wiener found the topic "easy" but stated later that when he actually studied with Russell he learned that he had missed "almost every issue of true philosophical significance."[61] This was certainly something of an overstatement.

But we should not imagine that when Wiener became a graduate student at Harvard he began to concentrate solely on one area of investigation. Throughout Wiener's life his interests were eclectic,

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[60]Ibid., p. 169.

[61]Ibid., p. 171. Wiener overstates here the insignificance of his work. See Masani, "Wiener," p. 80; and I. G. Guiness, "Wiener on the Logics of Russell and Schroeder; An Account of His Doctoral Thesis, and of his Discussion of It With Russell," Ann. Sci. 32 (1975), pp. 103-132.

and his time at Harvard was no exception. The overall picture we have is of a very successful and very young graduate student, doing quite well in his official program but fiercely interested in other areas as well. Wiener recalls avidly reading Ruskin's Modern Painters at this time, as well as the works of Swift, and especially the poetry of Heine, which he read, "not once, but many times over, lying face down on my bed and sucking the last savor out of the phrases I had scanned many times before."[62]

By the time Wiener received his Ph.D. in the area of mathematical logic, he was eighteen years old and had done graduate work in mathematics, biology and philosophy. He had what was the equivalent of the combined education of several graduate students. This achievement was made possible by his native interest, his great intellectual abilities, and his incredibly early start in the assimilation of various subjects. Regarding this last point, Wiener later wrote:

> I learned my algebra and geometry at so early an age that they have grown into a part of me. My Latin, my Greek, my German, and my English became a library impressed on my memory. Wherever I may be, I can call on them for use. These great benefits I acquired at an age when most boys are learning trivialities. Thus my energies were released for later serious work at a time when others were learning the very grammar of their professions.[63]

The fact that Wiener developed professional-level abilities in many areas was of great importance in later enabling him to facilitate the dissemination of the feedback concepts to the larger scientific

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[62]Wiener, Exp, p. 170.

[63]Ibid., p. 290.

community. He was not only able to communicate with the specialists in different fields, but communicate with them at a professional level. He thus became the center of the dissemination process.

Post-Graduate Study with Bertrand Russell at Cambridge and First Employment

During Wiener's last year at Harvard, that university granted him a traveling fellowship. He was accepted at Cambridge by Bertrand Russell under whom he was to work as a non-matriculating student. Wiener's father had written a letter of introduction to Russell, again stating the view that his son had excelled "not as the result of premature development or of unusual precocity, but chiefly as the result of careful home training, free from useless waste, which I am applying to all my children."[64] The entire Wiener family left for Europe in the Fall of 1913. Norbert took up residence in Cambridge while the rest of the family went to Munich where Leo Wiener was to do research.

In working with Russell, Wiener intended to continue his work in mathematical logic, but Russell convinced him that in order to follow this course he should undergo more extensive preparation in mathematics itself. The influence of Russell exemplifies an interesting pattern in Wiener's life, one in which Wiener often depended heavily on the guidance of others for germinal suggestions regarding interesting problems, and then pursued these problems with

[64]Quoted in P. Masani, "Wiener," p. 81.

an aggressiveness, intelligence and creativity of his own which led him to continually higher levels of achievement.[65]

In response to Russell's suggestion, Wiener took courses in mathematics, the most influential being with G. H. Hardy, whom Wiener later referred to as his "master in mathematical training."[66] In this course he worked with many of the tools which would later become important to him including the Lebesgue integral, the general theory of functions of a real variable and complex variables. But even though Wiener was becoming more and more deeply involved with mathematics, he still considered himself primarily a philosopher who was strengthening his mathematical background.

Cambridge was difficult for Wiener in many respects. In his autobiography he refers to this period as his "emancipation," both from his father and the pressures of being a well-known prodigy in America. It is clear from reading the following excerpts of letters to his family from this period that his love for England, its society, and his professors did not develop smoothly and immediately. His initial response, in fact, was almost entirely negative.

Wiener's letter of September 30, 1913 shows well the somewhat paradoxical combination of brashness and insecurity which characterized him, as well as his initial interaction with Russell. Wiener was eighteen years old at this time:

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[65]Other examples of this pattern will follow. See the interactions described below with D. C. Jackson (p. 123), Irving Barnett (p. 118) and Julian Bigelow (p. 148).

[66]Wiener, Exp, p. 190.

Dear father,-

Many thanks to Constance for her letter. I am taking good care of my appearance. I shave regularly. I took a sponge-bath in my wash-basin Sunday evening, and I am having one pair of shoes reheeled.

I have begun my work. Following Russell's advice, I bought Goursat's "Cours d'analyse," yesterday, and have covered more than 30 pages thoroughly already. It is a big book, in two quatro volumes of about 600 pages, each. I paid 12/0 per volume for a second-hand copy. Its first-hand price is 16/0 per volume. I had quite a time finding it in the bookstores, as most of them were out of it. Excuse my spending so much money, but as Russell told me, it is a book I should own.

Russell, as you know, invited me to his room Saturday evening. He had in his room at the same time another Fellow of the college, a mathematician. Between them, they made it very unpleasant for me. Half the time they were talking entirely between themselves, the other half, they were casting aspersions upon my mathematical knowledge. One would say, 'I wonder whether Mr. Wiener's mathematical knowledge is sufficient to enable him to take up this course of study with profit?' The other would answer, 'But this line of work requires very little real mathematical preparation, you know.' Then the first would say, 'But it isn't so much the amount one has studied as the way one has been trained that counts.' And so on for an hour. I have read a good deal about the studied insolence and conceit of the English University man, but this is the first time I have had the misfortune to encounter it.

As to my research-work, Russell's attitude seems to be one of utter indifference, mingled with contempt. I gave him my thesis to read, and he said he was so busy that he could not tell when he would get through, whereas you know that he told us he was free every evening. I told him about the particular work I was interested in, but asked him for advice as to what particular work to take up, but he said, 'Our method of doing research work differs from the German and American methods in that we let the students find their own problems, instead of assigning problems to them, and I think our method is better.'

Russell has invited me to his rooms, without, however naming any particular date, further than saying that Thursday is his especial 'evening at home.' However, I think that I shall be quite content with what I shall see of him at lectures! . . .[67]

The next day Wiener reversed his appraisal of Russell:

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[67]Wiener to his father, Sept. 30, 1913, Wiener Papers.

> Dear father,
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> I am afraid that I have sadly misunderstood Mr. Russell. Yesterday, as I was walking around back of Trinity, I came upon him, and he invited me to tea. He gave me back my paper, with a list of criticisms carefully made out (though, I believe, mostly invalid), and said, that as a technical piece of work it was very good, and showed a thorough acquaintance with the use of symbols. He said that he looked forward to my making things interesting in his course, and was, in general, very pleasant to me. He also gave me a copy of Vol. II of the "Principia."[68]

This rapid turnabout in Wiener's opinion of Russell exemplifies a pattern which Wiener repeated many times in his adult life. He was quick to condemn, but equally quick, in most instances, to apologize for being in the wrong when the evidence so indicated. Unfortunately, in this case Wiener again had further reason to be unhappy. The following excerpts from his letter to his father of October 18, 1913, represented a low point in his Cambridge stay:

> Dear father,
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> I met Santayana at Russell's rooms Wednesday, and had a pleasant talk with him. It seemed good to meet an American among these damned Britishers, whose idea of cordiality is to ask you to tea towards the end of next week. Imagine one Harvard student asking another to tea! I am hating this country more every day, and the people are such icebergs that I wonder how India can maintain its climate, now that it is a British possession. . . .
>
> The work I am taking under Mr. Hardy seems well within my comprehension, but in Dr. Baker's course, after nine or ten hours' steady work I was unable to complete even six out of nineteen problems. To my knowledge, however, I made no errors.
>
> My course-work under Mr. Russell is all right, but I am completely discouraged about the work I am doing under him privately. I guess I am a failure as a philosopher, and stand no chance of getting a renewal of my fellowship (for I dread to think what Russell will tell Perry about me). You see, I wrote a little paper stating my own views, rather hurriedly, and without, at the time, any access to the library. I unfortunately gave it to Russell to read. He wrote out a severe lot of criticisms about it, and invited me to his rooms to discuss the matter. Since I have not really gotten 'warmed up' to my

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[68] Wiener to his father, Oct. 1, 1913, <u>Wiener Papers</u>.

philosophical work yet, I made a botch of my argument. Russell seemed very dissatisfied, not only with my views, but with my philosophical ability, and with me personally. He spoke of my views as 'horrible fog', said that my exposition of them was even worse than the views themselves, and, like Löwenberg and Southard, accused me of too much self-confidence and cock-sureness. Now, I did not behave differently than I did in my Harvard seminaries, or when you were here, while his language, though he excused himself, it is true, was most violent.

The day I shall enjoy most of all during my stay in Cambridge shall be the day I leave it. I am heartily disgusted with the place, everyone in it, and, most of all, myself.[69]

Russell and the Cambridge faculty probably would have been astonished to know how much Wiener was distressed by their treatment of him. Wiener gave the impression of being extremely brash and, despite his own denial, "cock-sure of himself." Russell had this impression immediately upon meeting Wiener and his father, who had brought Wiener to Cambridge. Russell wrote in a personal latter:

> Do you remember the letter I read you from a man at Harvard, telling me of his son, an infant prodigy, a Ph.D. of 18, knowing everything, yet physically & mentally well-balanced and weighing 156 pounds? The two of them, father & son, turned up today & were great fun. The father looks like a Hindoo, but I think it would come off in water. The son is fat, bland and smug*. The father---as near as I can remember---informed me that he was half Polish, half Lithuanian, spent his youth in Warsaw, then did mathematics in Berlin, then went to America to found a vegetarian communistic colony, but changed his mind when he reached New Orleans, & became a farmer instead. Gradually he drifted back into teaching in Western Universities, was Professor of various subjects at various times---I forget what they were, but they were something like [miner]alogy, logic, & ancient history. Now he is Professor of Slavonic languages at Harvard.
>
> While this information was being poured out, his son---after a period of dead silence---suddenly woke up & began an equal torrent, on the subject of his Doctor's thesis---pulling out books from my shelves & pointing out, kindly but firmly, where my work is one-sided & needs his broad view & deep erudition to correct it. Both went on at once, like children

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[69] Wiener to his father, Oct. 18, 1913, Wiener Papers. Baker's course was on the theory of the functions of a complex variable.

> shouting 'Look at the castle I have built' 'No, look at mine' I believe the young man is quite nice and simple really, but his father & teachers have made him conceited. I asked him what he had read in philosophy---he at once reeled off the names of all the great philosophers, tho he couldn't remember the titles of their books. Mathematics of course he professed to know pretty well, tho' he admits it would be as well to know more. He is an absolute vegetarian & can only rarely be induced to eat even an egg. I do delight in queer people.
>
> *only superficially. Really he is frightfully nervous & excitable.[70]

Russell read Wiener's doctoral thesis and was not impressed by it, writing, "I have read his Dr.'s thesis & think him more infant than phenomenon. Americans have no standards."[71] And Russell later described the difficulty of attempting to teach the prodigy: "The youth has been flattered & thinks himself God almighty---there is a perpetual contest between him and me as to which is doing the teaching."[72]

Although Wiener later referred to this period of his life as his "emancipation,"[73] from the pressures of his father's domination and from the publicity associated with being a child prodigy, it is clear that Wiener's freedom was hard won. The antagonism between Wiener and Russell, combined with the problems of living in a strange new society led Wiener into severe emotional depressions, punctuated by periods of success when recognition was granted. These mood

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[70] B. Russell to Lady O. Morrell, Sept. 26, 1913, Humanities Research Center, The University of Texas at Austin.

[71] Quoted in Ronald W. Clark, The Life of Bertrand Russell (New York: Alfred A. Knopf, 1976), p. 216.

[72] Ibid.

[73] Wiener, Exp, p. 180.

changes, as well as his shifting levels of confidence in his mathematics and philosophy are well illustrated in excerpts from Wiener's letters to his parents during the three-week period, October 8 to November 1, 1913:

> Dear Mother,
> I am heartily sick of this old hole. I meet no one, and am as utterly isolated as it is possible to be. Whether I work or I loaf, I have no interest in anything. Russell has refused to make any suggestions as to what line of research I take up, yet he seems to think (and I am inclined to agree with him) that I have worked my present line of investigation dry. I cannot write a philosophical paper without talking it over beforehand, and I know of no one with whom I can talk it over. What little work I have done on my mathematics seems to have had no effect other than that of deadening my interest in it when I shall take it up in class.[74]

Two weeks later he has become somewhat more confident and assertive:

> Dear ma-
> . . . T'other day I got into a little philosophical tiff with G. E. Moore, one of the big bugs hereabouts. Of course I got neatly squelched. However, as a student that was with me at the time remarked, when G. E. is really disgusted with a person's philosophical ability, he gets very rude, while he was entirely polite to me.
>
> Friday the Moral Sciences club meets in Russell's rooms, and he reads a paper. I'm invited to butt in. Friday afternoon I go to tea with a Scotch phil. student, who will have some of his philosophical friends in also.
>
> My math. work isn't so bad as I thought it was; I apologized to Baker for the fewness and incorrectness of my problems, but he said that he had no fault to find with me, and that for the scantiness of my previous technical training and the time that had elapsed since I sent over my work last, I was doing very well. My work with Hardy is flowing along smoothly enough while, so far, I have no work to do in Russell's courses, for I have covered all the ground myself.
>
> I licked the work I did last summer into some sort of shape, and showed it to Hardy and Russell. Hardy seemed rather indifferent about it, but R. is highly interested and pleased by it. I think, as things look now, that I may get something of genuine mathematical value out of it---that I may develop,

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[74] Wiener to his mother, Oct. 8, 1913, Wiener Papers.

> perhaps a theory of circular ordinal numbers from it. I couldn't have done anything with it last summer, however, without books. But here, now that I have gotten into the swing of my work, I feel confident of accomplishing something. . . .
>
> You must remember that in the lecture-rooms my fellow-students are the pick of the mathematicians of the pick of the mathematical colleges of the pick of the mathematical universities of the world. And I feel that I am quite holding my own in these courses, though my technical knowledge is much less than theirs.[75]

Still, Wiener had great reservations about Cambridge students and his teachers. The stark contrast between the brashness, or, depending on one's point of view, conceit, of Wiener's self-confident analysis of some of the great lights of the British intellectual scene, and his own evident insecurities is rather remarkable. We also have an enormous cultural clash between the informal American and the formal British. Wiener, not quite nineteen at this time, writes as follows:

> Dear father,
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> . . . The Cambridge student, damn him, prides himself on a blasé attitude to everything in general. It is considered bad form to talk to a man at the Union without an introduction, and even students sitting beside one another in class all year long, or living next room to one another do not in general know one another. It is the deadest, most desolate place in the world.
>
> I have a great dislike for Russell; I cannot explain it completely, but I feel a destation for the man. As far as any sympathy with me, or with anyone else, I believe, he is an iceberg. His mind impresses one as a keen, cold, narrow logical machine, that cuts the universe into neat little packets, that measure, as it were just three inches each way. His type of mathematical analysis he applies as a sort of Procrustean bed to the facts, and those that contain more than his system provide for, he lops short, and those that contain less, he draws out. He is, nevertheless, within his limitations, a wonderfully accurate thinker.
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> Hardy is a typical Englishman; he plays cricket; lectures on his subject in a remarkably lucid manner; is, however,

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[75]Wiener to his mother, Oct. 22, 1913, Wiener Papers.

> utterly indifferent to the students under him; and mispronounces his French and his German in a particularly atrocious manner.
>
> Baker lectures at such a rate that few can follow him, but seems far more interested in his students than either Russell or Hardy. He has a marvellous mind---I can hardly understand a thing he says. . . .
>
> I hope it will not be necessary for me to stay another term in Cambridge.[76]

Wiener's letter of November 1, 1913 deals in a positive way with his coursework in mathematics under H. F. Baker and Hardy. His summary of his position points out his decreasing confidence in his future in philosophy.

> My work with Baker is on infinite series of complex terms, integration of functions of complex variables, elliptic integrals, convexity, Riemann surfaces, etc. My work with Hardy is an exhaustive analysis of the stages necessary to prove Cauchy's theorem. . . . It is a dead cinch, but Baker's course is <u>not</u>. Still, I feel that I am following Baker's work as well or better than most of the class. I put in 4 or 5 hours a day on it for the last few days, and this helped me tremendously to understand it.
>
> Both Russell's courses attain the acme of superficiality. He has not given us as much as anyone could gather in an hour or two from his book, for he proves no theorems in class. I am sure that Schmidts [sic] class has much more genuine knowledge of the subject than these classes will have. I know that not more than one, or two, at most, of his students have as much as looked at the Principia. Probably nobody there could make the most elementary use of the book. I find, however, that I remember the book so well that I can tell almost at any moment just what he is going to say.
>
> R. returned my paper on circular order yesterday. He said it was very interesting, and that he had no criticisms to make. . . .
>
> I must confess, I feel that my chances for reappointment next year are mighty slim. None of the men here, not even R.,

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[76]Wiener to his father, Oct. 25, 1913, <u>Wiener Papers</u>. These bitter views of Cambridge and its inhabitants are not recalled by Wiener in his autobiography. Quite to the contrary, he states: "I found the Cambridge environment far more sympathetic to me than I had found that of Harvard. Cambridge was devoted to the intellect." (Wiener, <u>Exp</u>, p. 196.)

> seem particularly interested in my work. I feel that R. will talk to Perry somewhat as follows---'Yes, his mathematical work is interesting, but his mathematical training is insufficient, and his philosophy shapeless.' I feel that attitude of his and of the other men in every word they say to me. It really humiliates me more than I can express to feel this.[77]

Toward the end of the term Wiener wrote to his mother saying that Russell was looking more favorably on his work, and giving him further guidance:

> . . . I have talked over my work with Russell, and he told me that I need not feel alarmed at my work in math., . . . He says that mathematical logic is my real line of work and that at that I am very good. He half apologised for the severe way he sat down on me & my philosophy at first, & said that he thought he had been too dogmatic, that he realized that there were no really valid arguments against my view, & that my standpoint was quite a legitimate alternative to his.[78]

But just before leaving Cambridge to visit his parents for the Christmas break Wiener was again frustrated with Russell:

> . . . I believe I have helped to sow the seeds of discontent here against the preposterous position of Mr Russell, who claims that we can be acquainted with certain individual things which we _know_ to be unanalysable: that we can name a thing without involving _any_ description of it. It is a position so intrinsically absurd that if a man of Mr. Russell's standing had not propounded it, no one would think of taking the trouble to refute it.[79]

Mathematics was proving to be the more successful field for Wiener. His first paper—a mathematical paper—was published in

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[77]Wiener to his father, Nov. 1, 1913, _Wiener Papers_.

[78]Wiener to his mother, Nov. 19, 1913, _Wiener Papers_.

[79]Wiener to his father, Dec. 2, 1913, _Wiener Papers_.

November of that year.[80] Also during this period he wrote "A Simplification of the Logic of Relations,"[81] later saying "this paper represents my true introduction into mathematical thinking and writing."[82]

Wiener's problems with Russell stemmed both from personal and from basic philosophical differences. Russell at this time was undergoing a crisis in his own thinking, making the transition from Cartesian dualism to a neutral monism.[83] For Russell, "sensibilia" now replaced matter as the ultimate physical reality, and it was this position to which Wiener objected. From the time of Wiener's first youthful paper, "The Theory of Ignorance," written at age ten, Wiener was a sceptic with regard to any absolute epistemology. This scepticism later appeared in Wiener's mathematical work. During his early Cambridge period he wrote a paper, "Is Mathematical Certainty Absolute?,"[84] answering in the negative. Wiener's scepticism toward

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[80] Norbert Wiener, "On the Rearrangement of the Positive Integers in a Series of Ordinal Numbers Greater than that of any Given Fundamental Sequence of Omegas," Messenger of Math. 43 (1913), pp. 97-105.

[81] Norbert Wiener, "A Simplification of the Logic of Relations," Proc. Cambridge Philos. Soc. 17 (1914), pp. 387-390.

[82] Wiener, Exp, p. 191.

[83] Clark, Russell, p. 214.

[84] Norbert Wiener, "Is Mathematical Certainty Absolute?," J. Phil. Psych. and Sci. Method 12 (1915), pp. 568-574.

epistemological certainty would later mature into a basic element of Wiener's world view—the statistical outlook.

Adding to these philosophical differences was the aversion Wiener, a New England Puritan by his own description, felt for the "libertine"[85] Russell. But whatever Wiener's feelings were about Russell at the time, it must be said that Russell played an important role in the positive development of Wiener's intellectual character. Russell also reinforced Wiener's existing view that scholarship should not be narrow, but broadly integrated. The importance which Russell placed on the "well-rounded education" can be seen in the fact that as impressed as he was with Ludwig Wittgenstein, he thought his success would be limited saying, "He has not a sufficiently wide curiosity or a sufficient wish for a broad survey of the world. It won't spoil his work on logic, but it will make him always a very narrow specialist, & rather too much the champion of a party---that is, when judged by the highest standards."[86] Russell also held regular Thursday night parties, or "squashes," which Wiener attended. At these parties were a diverse collection of literary, scientific and mathematical intellectuals.[87] Russell alerted Wiener to new important developments in modern physics, a field in which Wiener later made significant contributions. By the time the term ended, Wiener was able to write to Russell that he

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[85] Wiener, Exp, p. 192.

[86] Clark, Russell, p. 204.

[87] Wiener, Exp, p. 194.

felt "more a man"[88] for having studied with him. He would have returned for the spring term but Russell was leaving for Harvard. At Russell's suggestion, Wiener instead finished the year at Göttingen, continuing his mathematical studies.

At Göttingen Wiener took mathematics courses with David Hilbert and Edmund Landeau. Wiener admired Hilbert greatly, but criticized Landeau saying that he "had intelligence, but he had neither taste nor judgment nor philosophical reflection."[89] The aesthetic appreciation that Wiener had for mathematics was a very strong current within him; he later said that the creative rewards of the mathematician "are exactly the same character as those of the artist." Further, "Neither the artist nor the mathematician may be able to tell you what constitutes the difference between a significant piece of work and as inflated trifle; but if he is never able to recognize this in his own heart, he is no artist and no mathematician."[90]

Concerning the mathematics courses he took at Göttingen, Wiener later said, "I got something at the time from the mathematics courses, but much more by that sort of intellectual doubletake that allows one to realize at a later date the importance of what one has already heard but not understood."[91] The formal coursework

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[88]Ibid., p. 197.

[89]Ibid., p. 209.

[90]Ibid., p. 212.

[91]Ibid., p. 210. This remark fits in well with Wiener's view of the learning process described above (p. 82).

included group theory with Landeau and differential equations with Hilbert. Wiener's work in mathematical logic was now bearing fruit as he began to have papers published in this field, including "A Simplification of the Logic of Relations,"[92] and "Studies in Synthetic Logic."[93]

Wiener later observed that at Göttingen he was shocked to see that even at highly professional levels a man was often rated entirely by the company he kept and "not by the internal implications of his own work."[94] He was especially dismayed that a fellow student could dismiss Russell as an unimportant philosopher because he did not belong to any "school." Wiener later came to abhor academic "schools," partly, it seems, because of these experiences, and partly because he himself later felt that his work was discriminated against by the academic community on the basis that it did not belong to any recognized school of work.[95] One might view Wiener's

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[92]Wiener, "Simplification."

[93]Norbert Wiener, "Studies in Synthetic Logic," Proc. Cambridge Philos. Soc. 18 (1915), pp. 14-28.

[94]Wiener, Exp, p. 208.

[95]See, for instance, the comment of Faramelli, "Implications," p. 334, note 2. My comments are also based on interviews with Oliver Selfridge and Julian Bigelow, as well as Wiener's letters. In one letter of August 1949, Wiener wrote:

> "On the other hand, there is one thing which I wish to avoid: to make what I think ought to be a legitimate branch of science into an organized propaganda. I have generally been repelled by the tendency of scientists to assemble themselves gratuitously into schools, and it is something that I wish to avoid for my own ideas in every possible way." (Wiener to Parske, Aug. 12, 1949, Wiener Papers.)

later work in the development of cybernetics as evolving out of the need by Wiener to develop his own worth independently of the recognized "schools." It is certainly clear, at any rate, that Wiener felt that schools were a limiting factor in the development of science. The need to cross the disciplinary lines defined by these schools would become one stimulus for his development of cybernetics. These ideas are explicitly stated in the Introduction to Cybernetics, where he describes his motivation and that of his colleague, Dr. Arturo Rosenblueth:

> For many years Dr. Rosenblueth and I have 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.[96]

By the time Wiener finished the term at Göttingen in 1914, Europe was on the verge of World War I. Wiener returned to England in the Fall of 1914 to continue working with Russell, but was forced to hurry back to the United States as the danger to England

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[96]Norbert Wiener, Cybernetics: Or Control and Communication in the Animal and the Machine, 2d ed. (New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961), p. 2.

increased. He finished the academic year at Columbia, which he found to be almost barbarian in comparison to his experiences at Cambridge. On the advise of Russell, he studied with John Dewey, whom he later described as "word minded" rather than "science minded."[97] The fact that Wiener was studying with Dewey indicates that he still considered philosophy, in general, to be his field. But his enthusiasm and energy were strongly directed to the mathematical side of philosophy. He carried out research in mathematical logic, but this was done in isolation as there was no one at Columbia who could help him in this area. Wiener wrote to his father that he had made a discovery in symbolic logic, but that "there is nobody on this side who will appreciate it."[98] Wiener was not happy with the atmosphere which he considered to be terribly non-intellectual, and describes the period as the "low point" in his academic career between the two summits of his first European education and the later successes he would meet.

Wiener was invited to give the Docent Lectures at Harvard during the Fall of 1915. His topic was mathematical logic, and in particular he presented the constructivist approach in a manner after Russell and Hilbert.[99] While at Harvard, Wiener became more active mathematically, attending the Harvard Mathematical Society

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[97] Wiener, Exp, p. 222.

[98] Wiener to his father, Feb. 27, 1915, Wiener Papers.

[99] The constructivist approach was later useful in the development of computer science. See below, p. 269.

meetings regularly. A stronger push out of the formal field of philosophy came when Harvard failed to offer Wiener an instructorship in philosophy for the following year. This was a blow to Wiener. He was reduced to submitting his name to a teachers' employment agency; the next year, instead of teaching at Harvard, he found himself working as a mathematics instructor at the University of Maine in Orono. Wiener was terribly unhappy with the caliber of both the students and the faculty at Orono, saying about one of his lectures that he felt as if he "were giving a lecture on Musical Appreciation before a Deaf-mute Asylum."[100]

When World War I reached the United States Wiener asked to be relieved of his teaching duties in order to join the armed forces. His efforts to enlist were stymied by his poor vision, and consequently Wiener enrolled in Reserve Officer Training Corps program at Harvard. Although he finished the program, his medical record was not good enough to earn him a commission.[101] Wiener finally took a job doing war work as an engineer, helping to design ship's propellers for the General Electric Company. Here he used a "little mathematics in some thermodynamics problems," and in his spare time carried on his own research in mathematical logic.[102] Although he stayed at General Electric only a few months, the position made a tremendous impression on him. He later cited it as

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[100]Wiener to his father, Oct. 27, 1916, Wiener Papers.

[101]Wiener, Exp, p. 245.

[102]Ibid., p. 247-248.

an opportunity to get his hands "black in an industrial laboratory," and acquire "the satisfaction of working with tools as a member of an active team of men."[103] Wiener would later advocate attacking other scientific problems with the same team concept.

Wiener's father again intervened in his career, and obtained a job for him as a writer for the Encyclopedia Americana in Albany, New York. Again, Wiener found a way to make use of the experience: "With all the shortcomings and unpleasant sides of hack writing, it was a wonderful training for me. I learned to write quickly, accurately, and with a minimum of effort, on any subject of which I had a modicum of knowledge."[104] During this period, Wiener turned out articles on aesthetics, non-Euclidian geometry, metaphysics, and mathematical logic.[105] Although the experience amplified and capitalized on his broad range of interests, it is questionable whether Wiener's honing of encyclopedic writing skills aided or hindered his ultimate writing style. This style was curious in a number of respects. He presented profound insights almost casually, but nevertheless with a concise elegance. Wiener's apparently natural ability to express the essence of ideas concisely was

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[103] Ibid., p. 293.

[104] Ibid., p. 251.

[105] Wiener to his father, Oct. 19, 1917, Wiener Papers. When some thirty years later Wiener had occasion to re-read his aesthetics article, he was "astonished to recognize that my own literary style has changed so little with time. . . . Even today I would approve of what I have written." (Wiener to L. P. Dudley, Aug. 10, 1949, Wiener Papers.

undoubtedly reinforced by the demands of writing for an encyclopedia. At the same time, however, he was not encouraged, in this style of writing, to remain on any one topic for any length of time. This aspect of encyclopedic writing accentuated his tendency to rapidly shift from one aspect of a topic to another. To some, his later writing, and in particular his popular writing, seemed like a series of constant digressions interlaced with brilliant but unconnected ideas.[106] Wiener, however, almost certainly saw these apparent digressions as manifestations of his broad unified overview of the world. Apparently, he often did not realize the extent to which his audience lacked the background to see the unity of his remarks. Wiener saw patterns where others only saw lines. Wiener's prodigious facility in a wide number of disciplines aided him in perceiving these patterns, but they apparently did not aid him in the ability to convey these patterns to others. Many of his readers sensed that he was seeing a pattern in his presentation of Cybernetics, for example, but they could not perceive what the pattern was precisely. This problem was in large part caused by the balance and organization of the book itself, a problem which will be discussed further in Chapter Five.

In the spring of 1918, Wiener was requested to aid in the war effort by helping to compute ballistics trajectory tables at the Aberdeen Proving Grounds. Wiener later described this as a

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[106]See criticisms of Wiener's general writing style below (pp. 285-286).

transitional time in computing between the "rough old formal ballistics" and the "point-by-point solution of differential equations."[107] Wiener used mechanical calculating machines in this task which would give him a valuable perspective when he later helped to design electronic calculating machines. At the time Wiener described his work as "routine but interesting."[108] Later, however, Wiener remembered being impressed by the lack of "regimentation" present in the team effort to develop the range tables. This relative openness was in stark contrast to his experience during the Second World War, when "blinders" were put on individual scientists working with classified material; blinders which made the scientist "confine his efforts to some minute sector of a problem of whose larger implications he was held deliberately ignorant."[109] Here Wiener shows a prediliction for the view that scientific progress is best facilitated by a cross-disciplinary free exchange of ideas, and not by the division of a problem into minute parts, each of which is attacked in isolation by a specialist. As we shall later see, cybernetics was developed by Wiener partly to test the efficacy of the cross-disciplinary method.

At the end of World War I, Wiener was released from his military obligations. He worked briefly as a feature reporter for the Boston Hearld, where he did articles on the labor problems in

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[107] Wiener, Exp, p. 256.

[108] Wiener to Constance Wiener, ca. July 1918, Wiener Papers.

[109] Wiener, Exp, p. 257.

the ailing local textile industry. It is probable that this experience helped to shape Wiener's later concern for labor which he showed when it became clear that the development of cybernetics was likely to have a profound effect on the labor movement. There were many times in Wiener's life when he showed a great concern and sympathy for the down-trodden and the poor.[110] In many ways he was a liberal by nature, but a liberal of an unusual kind; a puritan liberal. This fascinating, and perhaps contradictory, aspect of Wiener's character will have to await a full biography.

Although Wiener was not employed academically directly after World War I, he kept busy at mathematical pursuits. He was, at this time, working on two mathematical articles which were published in the following year, 1920. In the Spring of 1919 Professor W. F. Osgood called a teaching opening to Wiener's attention. The opening was for a mathematics instructor at the Massachusetts Institute of Technology. At the time the mathematics department at Massachusetts Institute of Technology was primarily a service department for engineering students. The end of the war had produced a sharp increase in Massachusetts Institute of Technology's enrollment and hence there was a great need for new instructors. Wiener obtained the position and was appointed for the 1919-1920 academic year. Wiener would remain with Massachusetts Institute of Technology until his death 45 years later in 1964. He had become a professional mathematician.

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[110]Examples include his work in attempting to place refugee scientists from the Second World War and his help for minority members (see above, note 51).

CHAPTER FOUR

WIENER 1920-1943: PREPARATION FOR CYBERNETICS

The six months preceding the Fall of 1919, when Wiener took up his teaching duties at Massachusetts Institute of Technology, represented an especially portentous period in Wiener's career. The developments revolved around a number of chance events. His younger sister Constance, herself an aspiring mathematician, had been engaged to marry the rising young mathematician Gabriel Marcus Green. Following the end of World War I, however, there was a severe influenza epidemic in which Green died. Green's parents gave Constance his mathematical books. Wiener, who was then living with his parents, had access to these books which included volumes on functional analysis by Maurice Frechet, Henri Lebesque's book on integration and several other books in the field of modern mathematics.[1] Although the subject matter of these books had been treated in some detail in Wiener's courses at Göttingen and Cambridge, Wiener had apparently just then reached a critical point where the subject matter could be meaningfully assimilated by him. He later wrote that "For the first time, I began to have a really good understanding of modern mathematics."[2]

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[1]Norbert Wiener, Ex-Prodigy (New York: Simon and Schuster, Inc., 1953; reprint ed., Cambridge, Mass: The M.I.T. Press, 1979), p. 265.

[2]Ibid.

Wiener's student, and later colleague, Norman Levinson, found it remarkable that these books came into Wiener's possession by chance and that Wiener did not already have them in his library; a fact which to Levinson indicated "that in mathematics, as distinct from logic, his attitude was still very much that of an amateur."[3] The catalytic effects of Green's books, combined with Wiener's appointment as mathematics instructor at Massachusetts Institute of Technology transformed him from an amateur into a professional mathematician.

Wiener's new-found interest in modern mathematics received an immediate stimulus just as the summer of 1919 ended and the Fall term at Massachusetts Institute of Technology was about to begin. At this time the mathematician Irving Barnett happened to visit Wiener. Wiener, excited by his new-found interest in modern mathematics and for the first time facing employment as a professional mathematician, was casting about for a new research topic. He asked Barnet what he could suggest as a topic in the field of modern mathematics. Barnett suggested an area in function analysis which combined probability studies with the integration of paths in space.[4] Levinson has remarked that this suggestion of Barnett "completely influenced the whole course of Wiener's work and his greatest achievements all

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[3]Norman Levinson, "Wiener's Life," Bulletin of the American Math. Soc. 72, no. 1, pt. 2 (1966), p. 12.

[4]Norbert Wiener, I Am A Mathematician: The Later Life of a Prodigy (Garden City, New York: Doubleday, 1956; reprint ed., Cambridge, Mass: The M.I.T. Press, 1973), p. 35; and Wiener, Exp, pp. 274-275.

stemmed from this problem." He continued, "It was a happy accident that Barnett suggested a problem that turned out to be approachable by the mathematics of the time and yet was sufficiently difficult so that a solution was a real achievement. It was also a problem which when solved was to lead Wiener to more, equally important and pregnant problems."[5]

In terms of Wiener's later presentation of cybernetics, the most important result of Barnett's suggestion was that Wiener became deeply involved with the mathematics of physical problems in which stochastic, or probabilistic, considerations were primary. The consequences of this involvement were twofold:

1. Wiener became involved with communications problems related to the accurate transmission and reproduction of signals. This work would help shape Wiener's views on communication, a topic which was of great importance to his work in cybernetics.

2. Wiener developed statistical mathematical methods to explain physical phenomena. The success of these methods helped to shape Wiener's views on the ultimate nature of physical reality, our ability to know reality, and the status of scientific knowledge. The development of these methods was also important for his subsequent "fire control" research, research which directly prompted his development of cybernetics.

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[5]Levinson, "Wiener's Life," pp. 3-4.

THE DEVELOPMENT OF WIENER'S MATHEMATICAL TOOLS 1920-1931

In this section an attempt will be made to understand the types of problems that Wiener's mathematics enabled him to solve, rather than the technical methodology of the mathematics itself.[6]

The first problem Wiener attacked with success during this period was the problem of Brownian motion. Almost one-hundred years earlier it had been noticed that very tiny particles, such as pollen, when suspended in a fluid underwent unpredictable, wildly erratic motions. The understanding of this motion was greatly clarified in 1905 when Einstein utilized the kinetic theory of matter to show how the motion could be accounted for by molecular bombardment of the suspended particles by the fluid molecules.

Wiener's approach was different than Einstein's in that Einstein utilized the statistical mechanics of a large number of

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[6]Wiener's mathematical works were highly technical and are useful only to readers with the highly specialized background required to read them. The account here stems partly from these works but primarily from Wiener's own non-technical interpretation of them given in his autobiography and secondary sources related to his work. See, for example, Norman J. Faramelli, "Some Implications of Cybernetics for our Understanding of Man: An Appreciation of the Works of Dr. Norbert Wiener and their Implications for Religious Thought," (Ph.D. dissertation, Temple University, 1968), pp. 44-75; Steve J. Heims, John Von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death (Cambridge, Mass.: The Massachusetts Institute of Technology Press, 1980), pp. 58-78; P. Masani, "Wiener, Norbert: His Life and Work," in J. Belzer et al., eds., Encyclopedia of Computer Science and Technology, vol 14 (New York: Marcel Dekker, Inc., 1980), pp. 78-103; and Levinson, "Wiener's Life," pp. 12-30. See also the general references for Wiener's work given in Chapter Three (n. 1).

fluid molecules in order to predict the probabilistic motion of the Brownian particles, whereas Wiener achieved the same results by analyzing the collection of possible paths that an individual particle might undergo through the molecular bombardment. As Wiener later wrote.

> Here I had a situation in which particles described not only curves, but statistical assemblages of curves. . . . The Brownian motion was nothing new as an object of study by physicists. There were fundamental papers by Einstein and Smoluchowski that covered it, but whereas their papers concerned what was happening to any given particle at a specific time, or the long term statistics of many particles, they did not concern themselves with the mathematical properties of the curve followed by a single particle.[7]

The statistical assemblage of curves defined a set of curves for which the Lebesque integration method was well suited. Lebesque integration had been one of the topics in Green's books which had caught Wiener's attention and which Barnett had urged him to pursue.

One aspect of Wiener's description of the Brownian motion would be especially important for his later involvement in the aiming of anti-aircraft guns during World War II. This aspect is already evident in his 1921 description:

> When a particle is acted on by the Brownian movement, it is in a motion due to the impacts of the molecules of the fluid in which it is suspended. While the retardation a particle receives when moving in a fluid is of course due to the action of the individual particles of the fluid, it seems natural to treat the Brownian motion, in a first approximation, as an effect due to two distinguishable causes: (1) a series of impacts received by a particle, dependent only on the time

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[7]Wiener, Iamm, p. 38.

> during which the particle is exposed to collisions; (2) a damping effect, dependent on the velocity of the particle.[8]

In later war-related work, Wiener would be able to apply a similar type of analysis to the problem of shooting down an attacking aircraft. In that case, the aircraft became, in effect, a Brownian particle undergoing erratic motions; the damping of the erratic nature of the curves was again provided by the momentum of the particle, in this case, an airplane. This work related to the "fire-control project" is described in greater detail below (pp. 144-167).

During the 1920's Wiener became interested in two other fields which bore upon his later development of cybernetics. These were his involvements in the theory of light and the operational calculus of the communication engineers. His involvement in both these areas led Wiener to develop a theory of generalized harmonic analysis more powerful than the existing theory of harmonic analysis. Classical harmonic analysis of that period could deal only with two types of processes; those that occurred regularly in time, like the vibrating of a string, and those that had a definite beginning and end, like the throwing of a switch. Wiener's methods dealt with processes that were neither regular nor "transient," as the latter phenomena were known.

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[8] Norbert Wiener, "The Average of an Analytic Functional and the Brownian Movement," Proc. Nat. Acad. Sci. U.S.A. 7 (1921); reprinted in, Norbert Wiener: Collected Works, 2 vol., ed., P. Masani (Cambridge, Mass.: The M.I.T. Press, 1976-1980), 1 (1976), p. 451.

A strong motivation for Wiener to develop his methods of harmonic analysis came from the Chairman of the Electrical Engineering Department at Massachusetts Institute of Technology, Dugald C. Jackson.[9] For about 20 years communication engineers had been using calculating techniques developed by Oliver Heaviside, known as the operational calculus.[10] Although these calculating techniques worked satisfactorily no mathematical justification for their existence was known. Jackson requested Wiener to put the operational calculus on a formal mathematical foundation.[11] In doing so Wiener had to approach harmonic analysis from a more general standpoint. At this time he became acquainted with the Danish mathematician, Harold Bohr, who was working in the closely-related field of what Bohr called "almost periodic functions."[12] Bohr used these functions to account for the line spectra of light emission. Wiener was able to extend Bohr's work to describe not only the case where power was localized in certain frequency ranges, as was the case with line spectra, but also to the cases where power was spread out continuously across the whole range of the frequency spectrum, that is the case of "white light." The continuous power spectrum is

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[9] Wiener, Iamm, pp. 72-78.

[10] Heaviside was something of a hero to Wiener who later became deeply interested in his life. Wiener later wrote a novel (The Tempter [New York: Random House, 1959]) at least partly based on Heaviside's life. See Levinson, "Wiener's Life," p. 30.

[11] Wiener, Iamm, p. 78.

[12] Ibid., p. 93.

one of the major features of communications engineering problems, problems such as the transmission of telephone messages where a wide range of frequencies, continuous in character, needs to be transmitted in order to obtain sufficient fidelity. Thus Wiener's work both in the analysis of light and in formalizing the operational calculus led him to develop more generalized methods of harmonic analysis which then became useful in communications engineering. Wiener later described his development of almost periodic functions, and their connection with his earlier work in Brownian motion:

> Their discovery represented an important extension of harmonic analysis. I myself, as I have said, was working on extensions of harmonic analysis to which I had been led through an attempt to justify Heavisides' formal calculus. . . . With the aid of one or two tricks, from my own theory I was able to deduce not only the Bohr theory but a much wider range of results concerning continuous spectra as well.
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> In this I had to use ideas very closely akin to those which I had used in the study of the Brownian motion. In particular I had to make use of curves which are continuous but which are so crinkly that they can not properly be said to have a direction. . . . These curves had been more or less the step-children of mathematics and had been regarded as rather unnatural museum pieces, derived by the mathematician from abstract considerations, and with no true representation in physics. Here I found myself establishing an essentially physical theory in which such curves played an indispensible role.[13]

Although Wiener was not heavily involved in communication engineering itself in the 1920's, he had developed the mathematical tools which would enable him to become deeply involved in the field during the 1940's. Most of these tools were presented in his powerful synthesis, "Generalized Harmonic Analysis," a 124-page

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[13]Ibid., pp. 93-94.

paper published in 1930.[14] In this paper the subject of Brownian motion was, once again, a central topic. As Wiener pointed out, he was able to make a connection between Brownian motion and the continuous power spectrum of white light. He was able to analyze the phenomenon of electrical noise by the same methods. These connections were possible because all the processes are similar in type at the atomic level, consisting of "the disturbance of a linear oscillating system by haphazard disturbances" or by "perfectly irregular impulses."[15] Electrical noise, for instance, is caused by random surges of electrons which are then amplified by the circuit. Since these pulses are not a designed part of the signal, their amplification results in noise or static. This phenomenon is known as the "shot effect."

Wiener's mathematics was thus applicable to a wide range of phenomena which macroscopically behaved in a continuous manner, but which were, in fact, caused at the microscopic level by discontinuous, random events. Wiener was well aware of the diverse applications of his mathematics as early as 1926.[16] It was not until the 1940's, however, that these topics became of principle interest to Wiener.

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[14]Norbert Wiener, "Generalized Harmonic Analysis," Acta Math. 55 (1930), pp. 117-258; reprinted in Wiener, Collected Works, 2 (1980), pp.

[15]Norbert Wiener, "The Harmonic Analysis of Irregular Motion," J. Math. and Physics 5 (1926), pp. 159, 183.

[16]Ibid., pp. 136, 157-162. Applications in the shot effect and electrical tube noise are specifically mentioned here. It was characteristic of Wiener to look for physical applications of his mathematics.

Following that period, it was realized that the methods developed by Wiener for the analysis of Brownian motion in the 1920's were "indispensable" for the analysis of noise in electrical circuitry. As William Root wrote in 1966,

> Electrical noise is usually such that it not only admits being represented as a stationary stochastic process, but more specifically as a stationary Gaussian stochastic process. This is true because noise (voltage or current) is generally a macroscopic manifestation of the result of a great deal of independent activity at a microscopic level. The basic theory of Brownian motion developed by Wiener is indispensable for a meaningful study of stationary Gaussian processes.[17]

Wiener himself later noted that:

> I found that it was possible to generate continuous spectra by means of the Brownian motion or the shot effect and that if a shot effect generator were allowed to feed into a circuit that could vibrate, the output would be of that continuous character. In other words, I already began to detect a statistical element in the theory of the continuous spectrum and, through that, in communication theory. Now, almost thirty years later, communication theory is thoroughly statistical, and this can be traced directly back to my work of that time.[18]

A final important development of Wiener's during this period resulted from his collaboration with the German astrophysicist, Eberhard Hopf. Wiener and Hopf were both interested in statistical mechanics. Together they developed an equation, later known as the Hopf-Wiener equation, which described the radiation equilibrium of a star, but which had more general application, as has been described by P. R. Masani:

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[17] William Root, "Contributions of Norbert Wiener to Communication Theory," Bull. Amer. Math. Soc. 72, no. 1, pt. 2 (1966), p. 129.

[18] Wiener, Iamm, p. 79.

> It soon became clear that equations of the Hopf-Wiener type have wide applicability, as they cover many situations in which a barrier separates two different regimes, one of which can influence the other but not vice-versa. In stellar radiation the barrier is the surface of the radiating core, and conditions inside influence those outside. But Wiener also perceived that in problems of temporal development, 'the present' acts as a barrier between the influencing 'past' and the indeterminate 'future.' The fruitfulness of this perception emerged about 10 years later when Wiener's war work on anti-aircraft fire control led him to his theories of prediction and filtering, and he readily came up with a Hopf-Wiener equation from his data.[19]

The nature of the collaboration, as described by Masani, once again shows the great value of Wiener's interdisciplinary outlook and his exceptional ability to perceive analogies between different physical situations. Masani writes,

> I have gathered from Professor Hopf that his collaboration with Wiener was very short. They discussed the equation intensely one afternoon in Wiener's New Hampshire home. Wiener, who had come across somewhat similar equations in his study of electrical circuits, saw the causal significance of the equation when t is interpreted as time. (In the radiation problem t is the optical depth. . . .).[20]

Wiener was quite aware of his ability to weave the work of one discipline into that of another. During the early 1930's Wiener briefly collaborated with the young British Mathematician R. E. A. C. Paley.[21] Paley was a pure mathematician, which Wiener viewed as a weakness:

> He brought me a superb mastery of mathematics as a game and a vast number of tricks that added up to an armament by

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[19] Masani, "Wiener," pp. 97-98.

[20] Ibid., p. 97.

[21] Paley was killed in a skiing accident in 1933.

> which almost any problem could be attacked, yet he had almost no sense of the orientation of mathematics among the other sciences. . . .
>
> One interesting problem which we attacked together was that of the conditions restricting the Fourier transform of a function vanishing on the half line. This is a sound mathematical problem on its own merits, and Paley attacked it with vigor, but what helped me and did not help Paley was that it is essentially a problem in electrical engineering.[22]

By 1931 Wiener had developed all the mathematical tools he would require for his war work in prediction and smoothing theory; work from which his presentation of cybernetics would stem.[23] Before proceeding on to Wiener's war work during the period of 1940-1943, however, we will examine the significance of some of his early mathematical work in relationship to cybernetic analysis, cybernetics and his personal philosophy. We will also explore some other aspects of Wiener's personal and intellectual development which occurred prior to his war work.

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[22] Wiener, Iamm, p. 168.

[23] Levinson stated, "The mathematical problem of prediction as he formulated it was solvable by a synthesis of his own previous work. He could have handled it readily anytime after 1931, had he conceived of the problem." See Levinson, "Wiener's Life," p. 26.

THE EFFECTS OF WIENER'S MATHEMATICS ON HIS PHILOSOPHY

Having now examined some of Wiener's early mathematical work, we are in a better position to understand Wiener's philosophical position, especially with respect to the role of feedback in science and the later formulation of the "Behavior, Purpose and Teleology" paper. The common element in much of Wiener's mathematical work was his emphasis on statistical analysis. This emphasis carried over into Wiener's philosophy of science. He thought that nature, in general, had to be understood in statistical terms;[24] that there could be no knowledge of absolutes, a theme he first proposed at age ten in his paper, "The Theory of General Ignorance" (see above, pp. 79-80). Since absolute knowledge was not possible, in his opinion, only varying degrees of certainty were obtainable. The degree to which knowledge was certain depended on the evaluation of experimental results, and this evaluation, in turn, was determined by statistical methods.[25] Thus the status of knowledge was intimately related to experiment and measurement. These views were closely related to "scientific operationalism," a philosophy of science advanced in the 1920's by the physicist P. W. Bridgman. Wiener was quite familiar with Bridgman because both of them had

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[24] Wiener, Exp, p. 166. See also Wiener, Iamm, p. 33.

[25] Norbert Wiener, Cybernetics: Or Control and Communication in the Animal and the Machine, 2d ed (New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961), p. 33.

attended Josiah Royce's seminar on scientific method when they were graduate students at Harvard. This seminar is discussed above (see pp. 93-94). In his description of the seminar given in his autobiography Wiener recalled: "Percy Bridgman, who was even then beginning to be skeptical about the elements contained in experiment and in observation, and who understood the influence on physics of James's pragmatism, was definitely veering toward the operational position which he later assumed."[26] It is not surprising, then, that Wiener attempted to understand purpose and teleology in behavioral terms, terms which, as we saw in Chapter Two (p. 93), depend strongly on the statistical interpretation of experimental results.

Another aspect of Wiener's emphasis on the importance of statistical considerations was his interest in the second law of thermodynamics, entropy and irreversibility. Wiener later argued in the first chapter of Cybernetics that the standard scientific conception of nature, based on Newtonian physics, did not recognize the important fact that natural events are not, for the most part, reversible. Thermodynamic laws, Wiener argues, cause natural events to be directed in time and this directedness is exhibited both by living organisms and "modern automotons," or servomechanisms. The nature of this directedness, and its relationship to adaptive and non-adaptive goal-directedness as defined in Chapter Two, will be deferred until the general discussion of Wiener's Cybernetics

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[26] Wiener, Exp, p. 166.

(see below, pp. 283-285).

The inevitable increase of disorganization of the universe resulting from the second law of thermodynamics deeply affected Wiener's philosophy. He became something of an existentialist, seeing humanity as a brave but eventually doomed struggle to maintain order in the face of ever increasing entropy. In an often quoted section of his autobiography he wrote:

> 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 not fighting for a definitive 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 or organization in the face of nature's overwhelming tendency to disorder is an insolence against the gods and the iron necessity they impose. Here lies tragedy, but here lies glory too.[27]

Wiener, the "prophet of automation," was hardly immune to romantic stirrings.

Finally, one notes in Wiener's early mathematical work his unusual ability as a theoretical mathematician to find physical applications for his ideas, and, indeed, his tendency to use

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[27] Wiener, <u>Iamm</u>, p. 324-325.

particular problems, such as that associated with Brownian motion, to further his mathematical development. Characteristically, Wiener delved into many diverse areas of science to find these practical applications. His multidisciplinary interests led him into these different fields of specialization, but it was his extraordinary multidisciplinary <u>talent</u> which enabled him to make significant contributions to them. As we saw in Chapter Two, Wiener exhibited his diverse talents at an extremely early age, and the intense cultivation of these by his father helped in producing a most unusual and powerful intellect, capable of problem solution in many different areas. Wiener's later development of cybernetics would represent the culmination of this aspect of his character.

OTHER ASPECTS OF WIENER'S PERSONAL AND INTELLECTUAL DEVELOPMENT 1919-1940

When Wiener was appointed to the Massachusetts Institute of Technology faculty in 1919, at the age of 24, he was preceded by his reputation as a prodigy, but he had not yet established himself as a first rank mathematician. His fame helped him to achieve that goal, however, as he continued to have access to the leading scientists, mathematicians and philosophers of the time. His experiences at Harvard, Cambridge and Göttingen were early examples of this type of contact.

Wiener planned to travel to Europe during the summer of 1920 to attend the Strasbourg Mathematical Congress. He wrote in advance to Maurice Frechet, whose book on functional analysis he had read the preceding summer. Wiener inquired if they might work together. The "cordial invitation" that Wiener received from Frechet can be taken as an indication that even if Wiener was not yet highly accomplished in mathematics, he was viewed as someone who had the potential to become so.

Wiener's papers in Brownian motion, published in the early 1920's did not create much of a stir. But during the mid-1920's Wiener made several trips to Europe, one of which included a stay in Göttingen, at that time one of the most renowned centers of physics and mathematics in Europe. While there, Wiener gave a talk on harmonic analysis which he felt was very well received. Since some of the leading figures of European science were present at his talk, Wiener was ecstatic with his apparent success, feeling he had finally "arrived." The letter written to his father, July 9, 1925, part of which follows, clearly shows Wiener at the high point of his young career:

> Dear Dad:
>
> Courant is one prince. His offer is as follows: If I can wrangle a national research council fellowship out of the committee, I am to come here next summer, and receive a call from the Prussian government. I am to have an assistant whose thesis I shall superintend, while he will help me with the German and the organization of my book. I am to be very careful of the presentation of my book as that is my weak point. Unless I make a mess of the presentation (which I shall not do) the book will be accepted by Springer in their great series. My book and Bohr's will be in close relation to one another. I shall get my book into shape in a course of lectures here. Courant is giving me an invitation to show to the National Research crowd. . . .
>
> Bohr is one fine fellow. He is the whole cheese on analytic number theory, but treats me as an equal. He is one of the nine or ten leading mathematicians in the world.
>
> These guys seem to think that after G. D. I am one of the best American mathematicians. Ain't it great to have a cousin?
>
> Dad, if I get that call here, they will be falling over one another to offer me a big job in the States. I forgot to say that if I don't finish with my assistant in a year, C. will try to get him a Rockefeller fellowship to work nominally at Harvard, but actually under me.
>
> What do you think of your son? He is rapidly developing such a case of swelled head that he expects to wake up soon and find that this has been all a dream. It is too good to last.
>
> Dad, I am a made man. Remember, that in the whole world, only Paris is to be compared with this as a center for math. Courant is in charge of the place. If then I am thought worth this serious consideration here, of all places, I can't be so damned rotten. Of course, I know that my knowledge of math is less than that of their youngest docent. I know that my expository powers are abominable. I know that I am not an orderly thinker. But I think that when it comes to work along my own line of thought, I can make the fur fly. Bohr . . . [saw] . . . something of power in my work.
>
> I know I'm a damned conceited puppy, but it feels great all the same. Imagine a composer when a big orchestra first presents his piece, or an author who finds the publishers first coming to him instead of the other way round. It can't be! Or else I shall make some blunder that will queer me.

> Now, don't think your son is going crazy. Don't think either that he has taken too much of last night's Bowle. Confess, dad, that if the same thing had happened to you thirty years ago, you would have written the same incoherent sort of a letter as
>
> Your loving son,
> Norbert[28]

Thus, between his appointment as a mathematics instructor in 1919 and his stay in Göttingen during the summer of 1925, Wiener had become, if not a leading mathematician, at least one who was travelling in the circle of leading mathematicians. The following year Wiener received a Guggenheim Fellowship specifically requesting him to continue the topics he had discussed the previous summer at Göttingen, namely, the development of harmonic analysis as applied to almost periodic functions, haphazard motion, and related topics.[29] Wiener clearly felt his professional fortunes were on the rise. This perception was also reflected in his personal life. Just before returning to Göttingen during the summer of 1926 for what Wiener expected to be a major triumph, he married Margaret Engemann, an instructor of modern languages, whom Wiener had met several years

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[28] Wiener to his father, July 9, 1925, Norbert Wiener Papers (MC 22), Institute Archives and Special Collections, Massachusetts Institute of Technology Libraries, Cambridge, Mass. The "Bohr" referred to in the letter is Harold Bohr, while "G. D." is G. D. Birkhoff of the Harvard mathematics department. Wiener's talk at Göttingen may not have been as well received as he had thought. David Hilbert, for instance, who was one of the leading mathematicians in Germany, is quoted as saying that Wiener's talk to the Mathematics Club was "the worst there has ever been." See Constance Reid, Hilbert (New York: Springer-Verlag, 1970), p. 170.

[29] John Simon Guggenheim Memorial Foundation to Wiener, April 12, 1926, Wiener Papers.

earlier when she had been a student of his father's. The stay in Göttingen, however, was a disaster. The reasons for the sudden turnabout of events were complex. For one, Wiener may have misunderstood the extent to which support was promised him, but in large part the factors were political. Wiener's father had been a leading anti-German militant during World War I and before coming to Göttingen for his return trip Wiener apparently boasted publicly about the fact that the son of this anti-German militant was now being invited to the academic capital of that country.[30] As a result, Wiener's reception by Courant was quite cool. He was denied an assistant and official recognition of status, although he was allowed to give lectures. These were very poorly attended. The humiliation for Wiener was extreme; he later stated that the experience brought him "to the edge of a nervous breakdown."[31]

Wiener's situation was not made easier by the fact that his new wife could not immediately make the trip to Europe with him. When she did come Wiener's parents were not far behind, "partly to share in my supposed success," as Wiener later stated, but also to "keep a supervising eye over the newly-married couple."[32] Wiener was now

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[30]Constance Reid, Courant (New York: Springer-Verlag, 1976), p. 104.

[31]Wiener, Iamm, p. 115. Courant, for his part, later stated that Wiener was misunderstood at Göttingen; that the disorganization of his lectures was mistaken for a lack of depth of thought. See Reid, Courant, p. 105.

[32]Wiener, Iamm, p. 117.

nearly 32, but upon becoming aware of Wiener's difficulties in Göttingen, his father immediately became involved. As Wiener later wrote:

> It has always been harder for me to be safely wise than to blurt things out, and I told my father what had happened. Naturally, he was much more interested in his personal rebuff than in extricating me from my impossible situation. It was not a very happy week that we spent together in Göttingen, nor was it possible to keep my father and mother from going over my head and attempting to deal directly with the Göttingen people and the German educational authorities.[33]

The resulting letter that the elder Wiener wrote, denouncing Courant, hardly make Wiener's situation at Göttingen more comfortable.

Wiener and his wife left Göttingen on a belated honeymoon, marred by the depression of the Göttingen experience. They returned, in fact, not to Göttingen, as originally planned, but to Copenhagen, where Wiener fared better working with Harold Bohr.

The Göttingen episode is illustrative of the often severely strained professional relations which Wiener often had with his colleagues. Wiener was always insecure with regard to his professional status. As Norman Levinson has noted: "Unfortunately Wiener did have grave doubts all of his professional life as to whether his colleagues, especially in the United States, valued his work, and, unwarranted as these doubts were, they were very real and disturbing to him."[34]

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[33]Ibid., pp. 117-118.

[34]N. Levinson, "Wiener's life," p. 1. Wiener became notorious for his disputes with leading scientific figures. In the 1950's he had a falling out with Warren S. McCulloch which affected the development of cybernetics (see below, pp. 203-204). Many of these

During the period of the 1930's Wiener, through his mathematics, became involved with several areas of technology. With one of his first graduate students, Y. W. Lee, Wiener applied his statistical methods of harmonic analysis to produce electrical filter networks, which enabled the removal of noise from electrical circuits, and an electronic harmonic analyzer. These applications of his mathematics drew Wiener further into statistical aspects of communication engineering while at the same time provided some tools which would later prove useful in his development of cybernetics.[35] During the same period Wiener also became reinvolved with the field of automatic computing machines, which he had last left as a ballistic trajectory calculator during World War I. Vannevar Bush, at the time Chairman

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disputes may have been caused by Wiener's personal insecurities. As a child he had been convinced that the public was awaiting the downfall of the great prodigy. Later, he never felt secure about the value of his work. Levinson, for example, notes (p. 25) that:

> "Wiener felt uneasy about his mathematical work during the years immediately preceding World War II and pressed his colleagues to affirm that his productivity was not declining. He had always needed approval from those around him. He usual words of greeting became, 'Tell me, am I slipping?' Whether one knew what he had been doing or not the only response anyone ever made was a strong denial. However this was usually not enough and it was necessary to affirm in the strongest terms the great excellence of whatever piece of his research he himself would proceed to describe sometimes in the most glowing terms. Altogether such an encounter was an exhausting experience."

[35]Wiener later saw that filtering a signal and predicting its future value in time were related operations. His later war-time paper related to this observation became an important part of his cybernetic synthesis; see discussion of his paper, "Extrapolation, Interpolation, and Smoothing of Stationary Time Series," below pp. 162-163.

of the Electrical Engineering Department at Massachusetts Institute of Technology, was developing a mechanical analog calculating machine which would aid in the solution of differential equations. Bush asked Wiener to aid him in this project. Wiener's involvement culminated in a memorandum sent to Bush by Wiener in 1940 in which Wiener predicted many of the major operating principles of the modern electronic computer. In this memorandum[36] Wiener advocated the use of the binary system, memory storage, scanning, and electronic rather than mechanical arithmetic computation. Wiener foresaw many practical uses for this type of computer, including the solution of differential equations to enable the determination of lines of flow about an airfoil section, problems in elasticity and hydrodynamics. Thus Wiener was intimately involved with one of the major tools of modern automation long before automation became generally realized as a modern industrial process. Automation would become one of Wiener's major social concerns during the 1950's and 1960's.

Wiener's association with Y. W. Lee resulted in a one-year-stay for Wiener and his family at Tsing Hua University in Peiping during the academic year 1935-36.[37] Wiener, in retrospect, considered the trip to China a watershed year in his life, saying: "If I were to take any specific boundary point in my career as a journeyman in

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[36] N. Wiener, "Memorandum on the scope, etc. of a suggested computing machine," ca. Sept. 1940, Wiener Papers.

[37] The Wiener family now included two daughters, Barbara, eight, and Peggy, six.

science and as in some degree an independent master of the craft, I should pick out 1935, the year of my China trip, as that point."[38] Interestingly, it was just at this time that Wiener submitted himself for psychoanalytic treatment, later saying that at 42 years of age he realized he was "no longer a youngster,"[39] and that the burden of an extremely pressured life was beginning to tell on him. This action of Wiener's of course does not belie his appraisal of having reached a new higher level of professional achievement; in fact, it was, in all likelihood, this new degree of confidence that allowed him to make the decision to undergo treatment. His treatment was at first not satisfactory, and may have helped to establish some of Wiener's attitudes towards the "soft" sciences. He later said of the psychiatrists who treated him:

> Their answers to all human problems and to me were too glib and pat. Without denying in any way the therapeutic validity of much that they did, it did not seem to me that the intellectual roots of psychoanalysis had yet reached that degree of convincingness and scientific organization which carries with it full conviction.[40]

Wiener was just over 40 years of age. What had become of the eclectic interests of the childhood prodigy? Although he did not have time to puruse the breadth of topics he did when he was a child, the drive still remained. For one, he was an eclectic mathematician, ranging freely, as we have seen, through many diverse

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[38]Wiener, Iamm, p. 207.

[39]Ibid., p. 213.

[40]Ibid., p. 214.

fields of application. Although by necessity he concentrated most of his intellectual efforts in mathematics, he expressed his deep seated need to view everything in its largest context by framing his mathematical efforts in larger questions involving the nature of man and metaphysical issues. These views are expressed quite passionately in a highly interesting letter Wiener wrote to Paul de Kruif, the author of popular scientific works, in 1933. In this letter, Wiener expresses his philosophy of mathematics:

> Mathematics is a subject worthy of the entire devotion of our lifes. We are serving a useful place in the community by our training of engineers, and by our development of the tools of future science and engineering. Perhaps no particular discovery that we make may be used in practice; nevertheless, much of the great bulk of mathematical knowledge will be, and we are contributing to that bulk, as far as lies in us.
>
> Moreover, a clearly framed question which we cannot answer is an affront to the dignity of the human race, as a race of thinking beings. Curiosity is good in itself. We are here but for a day; tomorrow the earth will not know us, and we shall be as though we never were. Let us then master infinity and eternity in the one way open to us: through the power of understanding. Knowledge is good with a good which is above usefulness, and ignorance is an evil, and we have enlisted as good soldiers in the army whose enemy is ignorance and whose watchword is Truth. Of the many varieties of truth, mathematical truth does not stand the lowest.
>
> Since we have devoted our lives to Mathematics -- and she is no easy mistress -- let us serve her as effectively as we may. If we work best with an immediate practical problem in view, well and good. If mathematical fact comes to our mind, not as a chain of reasoning, built to answer a specific question, but as a whole body of learning, first seen as in a glass, darkly, then gaining substance and outline and logic, well and good also. The whole is greater than the parts, and in a lifetime of achievement, no one will care what particular question of practice was in the scholar's mind at such and such a moment.[41]

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[41]Wiener to Paul de Kruif, Aug. 3, 1933, Wiener Papers.

Wiener's assertion that "a clearly framed question which we cannot answer is an affront to the dignity of the human race" exemplifies his fiercely humanistic philosophy; a philosophy in which the highest value was placed in the intellect and understanding. As he wrote elsewhere; "To see meaning and understanding come where there has been no meaning and understanding is to share the work of a demiurge."[42] Like many he had an insatiable drive to understand, but unlike most he had extraordinary intellectual capabilities and remarkable insight. His desire to understand led him into almost every field of inquiry imaginable, fields into which he made brilliant, if often unorganized forays.[43] Viewing scholarship as a "calling and a consecration," and having learned not to be baffled by any problem which had hope of a solution,[44] it is not at all surprising that Wiener was a humanist, attempting to find solutions to earthly problems in human sources and not revealed ones. In particular, he refused to believe that life processes were "not understandable" in his own analytic terms. He would, however, require more conceptual tools than he had prior to 1940 to make a full attack on the problem. His war related research on

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[42]Wiener, Exp, p. 212.

[43]It is ironic that Wiener, who was so immersed in the study of organization, was a most unorganized writer. One of his greatest fears was death through disorganization, the "heat-death equilibrium" of the second law of thermodynamics, and yet the lack of organization in exposition was one of his greatest problems as a communicator (see below, pp. 279-281).

[44]See above, p. 77.

anti-aircraft fire control would not only provide these tools, but also help to focus his efforts on the problems related to the understanding of organismic properties and behavior.

THE WIENER-BIGELOW COLLABORATION

By 1938 it was clear to Wiener that the United States would soon be engulfed in a world war.[45] His age and poor eyesight precluded any active military service. As a result he cast about for an area of scientific research which might be of use in the war effort.

At first Wiener thought his contribution would be in the field of computers. As noted above, he had been involved in this field in the early 1930's when Bush had asked for his aid. The memorandum cited above, written in 1940, in which Wiener proposed a computing machine anticipating many of the operating principles of the modern electronic computer, was rejected by Bush as being impractical for a war-time project. Bush thought the machine had great potential, but its realization would involve a "long-range" development project.[46]

At about the same time Wiener's suggestions for a computer were being rejected by Bush, Wiener was appointed chief consultant in the field of mechanical and electrical aids for computation by the War Preparedness Committee of the American Mathematical Society.[47] This appointment was a natural outgrowth of Wiener's work aiding Bush, but after the computer project was postponed, Wiener needed a new project. He worked briefly on an attempt to use statistical methods on the problem of coding and decoding encrypted messages. Finding

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[45] Wiener, Iamm, p. 207.

[46] V. Bush to Wiener, Dec. 31, 1940, Wiener Papers.

[47] R. Richardson to Wiener, Sept. 19, 1940, Wiener Papers.

no success in this work, he moved on to the project which would occupy him for the next three years and from which his development of cybernetics would directly stem. This was the fire control problem associated with the antiaircraft defense effort during World War II. Before proceeding to the details of the fire control project[48] it will be useful first to discuss the essential problem of fire control.

In attempting to down an enemy aircraft, a projectile must be launched so that it arrives at a future position of the aircraft, a position determined by the displacement of the aircraft during the time of flight of the projectile. A correct launching of the projectile, then, involves predicting the future course of the aircraft and correctly launching the projectile so that it will intercept the aircraft at one of its future positions.

As aircraft speeds, maneuverabilities and altitude capabilities had increased considerably since World War I, the problem of shooting down an aircraft had become increasingly difficult. In an attempt to solve this problem mechanical gun directors were developed after World War I to aid in the aiming of antiaircraft artillery. These directors and their associated apparatus were

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[48]The project was sponsored by the National Defense Research Council, which was established in 1940 as a civilian agency of the United States Government in preparation for possible United States entry into the war. Section D-2 of the National Defense Research Council was established primarily for the investigation of fire control problems. Thus Wiener was one of many researchers investigating these problems. The contract number of the National Defense Research Council for Wiener's project was NDCrc-83.

designed to automatically compute the "lead" for the gun; that is, the angle ahead of the aircraft position at which the gun would have to be aimed in order for the projectile to intercept the aircraft successfully. The aircraft was tracked manually by means of the director; an apparatus operated by several men, one following the horizontal motion of the aircraft, one the vertical and another determining the range. By means of this tracking data, the computing gun director calculated the lead through a mechanical arrangement of gears, plates and curves.[49] The calculation made by these devices was analog in nature; it was based on direct rate evaluation of the motion rather than on strict numerical computation. In addition, the scheme used assumed that the aircraft maintained a straight line flight path. Directors of this type, which used extrapolations of measured flight paths were said to be "geometrical" in nature.[50]

Wiener's initiation into the fire control problem is described in a report summarizing the project he eventually undertook:

> D.I.C. Project 5980 originated in a suggestion of Professor Norbert Wiener at a conference on servo-mechanisms in

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[49]Examples of directors utilizing rate computing mechanisms of this type were the M-1 and M-2 directors which were in use in 1940. See Military Service Publishing Co., Antiaircraft Defense (Harrisburg, Pennsylvania: The Military Service Publishing Co., 1940), pp. 71-75.

[50]NDRC, Gunfire Control: Summary Technical Report of Div. 7, NDRC, vol. 1 (Washington, D.C., 1946), Polar and Scientific Archives Div., record group 227, National Archives, Washington, D.C., p. 54. At least two types of geometrical extrapolation were performed, based either on the tangent to the curve of the aircraft's motion, or the secant.

> November 1940, to the effect that electrical networks of the type approximating
>
> $$\left(\frac{1 + aiw}{1 + biw} \right)$$
>
> might afford a means of lead evaluation suitable for [anti-aircraft] Predictor use. The idea was discussed with specialists in electrical networks with representatives of the National Defense Research Committee and others, and also in several memos by Professor Wiener. A test was also made on the M.I.T. Differential Analyzer of several assumed network operators. Developments were encouraging, so that in December 1940 a small appropriation was authorized, establishing D.I.C. Project 5980 to investigate the matter.[51]

Just how Wiener intended to realize this operator function within a physical prediction apparatus is not clear.[52] What is clear, however, is that Wiener's initial approach included no provision for the detection and correction of prediction errors, important factors which would later be included in the project. The subsequent inclusion of these factors into the analysis was stimulated by the arrival into the project of Julian H. Bigelow, a young International Business Machines' engineer who had graduated from Massachusetts Institute of Technology in electrical engineering in 1935. Bigelow joined the project in January 1941, just two months after Wiener presented his ideas at the servomechanisms

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[51] Julian Bigelow and Norbert Wiener, "A. A. Directors, Summary Report for Demonstration (June 10, 1942)," Polar and Scientific Archives Division, record group 227, p. 1.

[52] Bigelow has commented that Wiener may well have been thinking of realizing the operator function in "cascaded phase shift networks capable of adjustment to provide distributed lead or lag effects" (personal communication). Wiener, with Y. W. Lee, had earlier been granted a patent for a network of this type.

conference.[53] Bigelow not only greatly influenced the direction of the fire control project, but also, by his interaction with Wiener played a considerable role in the development of the feedback ideas. Bigelow recently recalled how he came to be involved with Wiener. He had been drafted and had returned to Massachusetts Institute of Technology to collect some of his academic records. Bigelow continued:

> But when I got up there, I had to go see my department head, and he sort of grabbed me and said, 'we can't let you go in, we need you. We've got this fellow, Wiener, going around saying he knows how to win the war single handedly, so to speak, with his intellectual ideas. Nobody can find out what he's talking about, so we need you to work with him to see what there is to it.'[54]

An important contribution of Bigelow to the program of cybernetic analysis came from his identification of the essential role of feedback in fire control processes and his critical suggestion that in all likelihood animals (including man) controlled their motions through the use of continuous feedback mechanisms rather than by precise forward computation from set initial conditions.[55]

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[53]J. Bigelow and N. Wiener, "A. A. Directors," p. 2. Personal information on Bigelow obtained from my interview with him, June 10, 1979, Princeton, New Jersey.

[54]Bigelow interview (6/10/79). The department head was Carl Wildes.

[55]Where Wiener has written about his participation in the fire control project, he indicates that the feedback idea occurred jointly to himself and Bigelow. In 1958, for example, he wrote, "It had occurred to Bigelow and myself that such simple actions as driving a car were governed by negative feedbacks" (Norbert Wiener, "My Connection with Cybernetics: Its Origins and Its Future,"

Bigelow observed that the fire control problem was actually similar to the problem a human faces when he wants to catch a moving object, or the problem a predator faces when it desires to capture its prey. These cases involved, as did the fire control problem, a continual estimation of the future positions of the target as well

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Cybernetica 1 (1958), p. 9). Similar accounts are given by Wiener in Cybernetics, p. 6, and Iamm, pp. 251-254.

Bigelow, however, recalls that at the time he had been thinking about observational errors produced by fire control crew men hastily trying to acquire a target and "cranking in" data, which would contain overswing as well as other errors, to the director. He further recalls that this thought led him to remark to Wiener that continuous feedback could play an essential role in reducing these errors by permitting unwanted fluctuations to cancel out while continually reducing the main difference error (personal communication). Thus, by this view, Bigelow introduced feedback into the project.

Bigelow's recollection seems correct for several reasons: Wiener's initial approach, the operator method, contained no elements of feedback. Furthermore, Wiener, in his autobiography, states that when he went to China in 1935-36, he attempted to build a computer with Y. W. Lee, but that this attempt failed because, "What was lacking in our work was a thorough understanding of the problems of designing an apparatus in which part of the output motion is fed back again to the beginning of the process as a new input" (Wiener, Iamm, p. 190). Thus four years before the fire control project Wiener was not very familiar with feedback. There is no evidence that Wiener became more familiar with the concept in the intervening period, although, of course, he might have. Bigelow, when asked if he felt that Wiener had thought of the control of animal motion in terms of feedback, responded, "No, in fact I'm sure he never mentioned the idea to me in this way" (personal communication).

Given Bigelow's background as an engineer, his familiarity with feedback (see below, n. 56) and the fact that he joined the project at a time when Wiener was concerned with a different aspect of the problem, in which he utilized operator methods, the evidence generally seems to support the idea that it was Bigelow who made the feedback suggestion and who should receive most credit for introducing the feedback concept into the fire control project. This view is also supported by Warren S. McCulloch who stated, "Norbert told me, and I believe wrote somewhere, that it was Julian who had impressed on him the importance of feedback in guidance" (Warren S. McCulloch, "Recollections of the Many Sources of as continual correction of the pursuit path so as to effect subsequent interception in space. As noted above (p. 147), Wiener had been approaching the fire control problem on the basis of calculations involving the extrapolation of an operator function, with no explicit provision for error analysis. At this stage, Wiener apparently was chiefly concerned with the problem of how to arrive at an operator function which would properly characterize the dynamics of the aircraft in flight, rather than being concerned with the effects of tracking errors, and other disturbances on the prediction process. It would seem, then, that at this point Wiener was not then considering the entire system of the aircraft, the tracking apparatus, and the human operators, but only the aircraft itself. Bigelow, however, working from the parallels he drew from biology, suggested that for humans and other animals reaching for an object, a calculation of the future position of the object was not made, but rather a feedback process continually occurred which by successive approximation gradually brought the object and the interceptor together. A human, for example, in reaching for a glass, started the hand moving; any motion which tended to decrease the distance between the hand and the glass was continued, others were not. Thus a criterion was established, in this case a minimization of distance, which required that information concerning

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Cybernetics," ASC Forum 6 (1974), p. 12. I have not yet been able to locate the "somewhere" reference of McCulloch.

Wiener had great integrity with regard to matters such as assigning credit and it is very likely that he simply did not recall the sequence of events which which had occurred in casual conversation.

the relative distance between the hand and the glass be continually fed back through the eyes to the brain. The brain continually utilized this information to further instruct the arm and hand muscles as to their subsequent motion. Bigelow recently recalled the conversations in which he presented his ideas to Wiener:

> I said, you know, I've always been convinced that human operators, in fact, biological animal operators, dogs chasing rabbits, homing curves and so forth, that they don't really do things by extrapolating forward from an origin where you are now, but what you do is you have a concept of the whole space and you see sort of a separation there. What you do is use some operators which minimize the separation---there's a huge literature on guidance, homing and control problems with which Wiener was not very well acquainted in those days, and I happened to have more familiarity with the area than he did. But, in effect, instead of looking at it as a forward extrapolation procedure from known initial position constants, what I claim you do when you track a baseball and catch it, hit a tennis ball or something, is that you are constantly seeing this difference function and it doesn't matter how the curve is made as you come in to hit it. What you are interested in is any kind of process which minimizes the error of the distance from where you are to where you want to be.[56]

Bigelow further recalled that Wiener was excited by the idea, that "he picked this up, he got it fast"[57] and that his first association for the idea was with the concept of homeostasis from biology. Bigelow continued:

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[56]Bigelow interview (53). When asked what literature he was referring to, Bigelow responded that he was thinking of the literature associated with the homing operation of a ship on a local beacon, a problem that "had been analyzed since 1920," and predator-prey ("pursuit") curves from the middle of the last century. Bigelow has personal experience with these in relation to one of his first projects as a high school student, a project which involved the homing of an airplane on a beacon (interview with Bigelow by author, June 1981 and private communication from Bigelow).

[57]Bigelow interview (6/10/79).

> But what Wiener had was a marvelous sense of what framework in modern, contemporary scientific thought this would fit into. And he said it's basically a philosophical thing, it belongs in philosophy of science, it's important because it makes the distinction between several kinds of behavior and teleology.[58]

Thus in the midst of a war-time project on antiaircraft defense, ideas of a philosophical nature were generated, ideas which would provide the core of the paper, "Behavior, Purpose and Teleology,"[59] to be published some two years hence. The particular events leading to its writing and publication will now be considered.

"Behavior, Purpose and Teleology"

Wiener had immediately seen a larger philosophical context for the parallel Bigelow had drawn between feedback control in machines and in animals.[60] As Bigelow recalled[61] one of the first extensions Wiener made from the idea was to the biological principle of homeostasis. This principle, developed by the physiologist Walter B. Cannon during the 1930's, recognized as a characteristic of living organisms the ability to maintain constant physical and chemical states in spite of changing external conditions.[62] Wiener saw that the feedback principle might account for this different

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[58] Ibid.

[59] Arturo Rosenblueth, Norbert Wiener and Julian Bigelow, "Behavior, Purpose and Teleology," *Phil. of Sci.* 10 (1943), pp. 18-24.

[60] See Bigelow comment above, p. 151.

[61] Ibid.

[62] Walter B. Cannon, *The Wisdom of the Body* (New York: W. W. Norton & Co., 1932).

type of biological control.[63] He was thus beginning to perceive feedback as a general means of control, both in machines and organisms. This general application of feedback marked the beginning of its transformation from an engineering technique into a conceptual tool for all the sciences.

The fact that Wiener, the mathematician, immediately jumped to a fundamental biological application of feedback is not surprising in light of his background and early interest in biology. But the extent to which Wiener pursued this idea was unusual. This pursuit again demonstrates Wiener's ability to tenaciously pursue provocative suggestions in order to bring about some of his most profound achievements.[64]

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[63] Wiener was well aware of the principle of homeostasis because he knew Cannon personally. Cannon, who was at Harvard, was a friend of Wiener's father. The Wiener family had frequently visited Cannon's laboratory when Wiener was a child (Wiener, Exp, p. 59). Wiener also kept abreast of developments in neurophysiology through Cannon's protege, Arturo Rosenblueth.

[64] This observation is similar to one made by P. Masani who noted Wiener's "strong dependence on good external stimulation to set his creative powers in motion." Masani continued:

> "What he heard or read from great minds such as Russell, Perrin, Taylor, Born, Bush and Rosenblueth stimulated his own tremendously. But while the inception of Wiener's ideas depended on external stimulation, once germinated his ideas became his masters and were impervious to his intellectual gregariousness."

See P. Masani, "Wiener," p. 122. Masani's list of individuals who stimulated Wiener should also include I. Barnett, who greatly influenced the direction of Wiener's mathematical career, D. C. Jackson, who was influential in Wiener's becoming involved with communications engineering, and J. Bigelow who introduced Wiener to the greater potential of negative feedback in the explanation of guidance.

Wiener was able to pursue the feedback idea into other fields not only because of his multidisciplinary background, but because he had an on-going semi-professional involvement with scientists in other fields. As a graduate student at Harvard he had attended Josiah Royce's multidisciplinary seminar on the scientific method. During the 1930's Wiener attended a dinner seminar on neurophysiology and scientific method which also greatly influenced him. The seminar was held at the house of Wiener's good friend Arturo Rosenblueth, the colleague and protege of Walter Cannon at Harvard. Wiener and Rosenblueth shared a common interest in attacking problems arising from the highly specialized nature of modern scientific research. Rosenblueth's widely cultivated interests in the arts as well as the sciences also provided a basis of common interest. Wiener later recalled:

> Arturo and I hit if off well together from the very beginning, though to hit it off well with Arturo means not that one has no disagreements with him, but rather that one enjoys these disagreements. One point that we shared in common was an intense interest in scientific methodology; another, that we believed the divisions between the sciences were convenient administrative lines for the apportionment of money and effort, which each working scientist should be willing to cross whenever his studies should appear to demand it. Science, we both felt, should be a collaborative effort.[65]

Wiener later described the meetings of Rosenblueth's seminar as involving a free give and take of ideas, a "perfect catharsis for half-baked ideas, insufficient self criticism, exaggerated self-confidence and pomposity." He added, however, that "among the former habitués of these meetings there is more than one of us who

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[65] Wiener, Iamm, p. 122.

feels that [the meetings] were an important and permanent contribution to our scientific understanding."[66] It was, perhaps, unusual for a mathematician of Wiener's rank to attend a seminar stemming from the Harvard Medical School, but it was just this sort of involvement which allowed Wiener to pursue the feedback idea into biology with great success.

It was actually a potentially undesirable aspect of feedback regulation which played an important role in Wiener's pursuit of the generalized importance of feedback. Wiener and Bigelow knew that if the feedback regulation of a system was not proper, the controlled object or quantity would not be stable but would oscillate above and below the desired value. An early device which exhibited this type of behavior was James Watt's steam engine, the speed of which was controlled by a feedback device, the flyball, or centrifugal, governor.[67] Feedback regulation was used here to keep the engine speed constant, but if the regulation was not proper the speed would continually shift above and below this value by wide and increasing margins. This malfunction was termed "hunting."

Wiener reasoned that if human motor control involved feedback, then the same pathological behavior observed in engineering systems, that is, hunting, might be observed in humans under some conditions. Wiener later described the events which followed from this hypothesis:

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[66]Wiener, Cybernetics, p. 1.

[67]For a more detailed description of this governor, see the following chapter, pp. 177-179.

> 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. . . . 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.[68]

But Wiener had also immediately seen that the idea of feedback also had profound philosophical importance. In particular, the concept of feedback allowed the "purpose" of a system to be determined. The purpose of a system could now be defined by the goal of the feedback mechanism within the system.[69] The purpose of the governor on Watt's steam engine, for example, could be deduced from an analysis of its feedback structure.

This new definition of purpose was of the greatest importance because it separated the ever problematic notion of final cause from the idea of purpose, an idea of great utility. Thus teleology, or the study of behavior that is assumed to be goalful, could, by this view, become scientifically legitimate. These views concerning the relationship between purpose, feedback and teleology were jointly presented by Wiener, Rosenblueth and Bigelow in an article they

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[68] Wiener, Cybernetics, p. 8.

[69] See the following chapter for a fuller discussion of feedback systems.

wrote in 1942, entitled, "Behavior, Purpose and Teleology.[70] This article was published in Philosophy of Science in 1943 and contains the basic concepts of what, in Chapter Two, was termed "cybernetic analysis." As we shall see in Chapter Six, this paper was the prime stimulus for a series of interdisciplinary meetings which resulted in the dissemination of negative feedback not only as a technique, but also as a general concept to explain adaptive behavior. In completing the fire control project, Wiener developed ideas and methods which would lead to his incorporating negative feedback within a much larger set of ideas, those he would present in Cybernetics. Thus we now continue with a description of the conclusion of the fire control project.

The Fire Control Project: Conclusion

The discussions which led to the "Behavior, Purpose and Teleology" article had a significant effect on the direction of the fire control project itself. The fact that Bigelow's arrival had stimulated a basic reconsideration of the whole prediction process is reflected in the project summary report:

> From the first of January until the last of February 1941, an exhaustive study of networks as a means of prediction was carried out, both as to theoretical capabilities and practicability in A A Director service. Inevitably, this study developed into a complete investigation of the entire A A Director problem from a very broad viewpoint.[71]

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[70] A. Rosenblueth, N. Wiener and J. Bigelow, "Behavior."

[71] J. Bigelow and N. Wiener, "A. A. Directors," p. 2. The nature of the collaboration between Wiener and Bigelow was interesting. Wiener was subject to extreme mood changes that he

It became clear from these discussions, discussions which included the feedback concept, that the whole system of the airplane, the tracking devices and the human operators would have to be considered in order to construct the best possible prediction device. It also became clear that the behavior of all these factors was highly variable. Airplanes used erratic evasive actions and, in any case, did not fly perfect geometrical courses; human operators manning the tracking devices and automatic servos were far from perfect in following the path of the aircraft accurately; and the tracking devices themselves behaved differently under differing environmental conditions. Thus the "inherently statistical" nature

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says, in his autobiography, were caused by the use of the stimulent, Benzedrine which he said he required to endure all night computing sessions (Iamm, p. 249). His participation with Bigelow was erratic. Bigelow recently recalled,

> "Now Wiener would lecture on the blackboard and very often work for an hour or two, come to a halt, and then he would sort of disband the meeting for a day or two and I would go home and work on it and patch it up or make it more understandable to myself and then he would come again the next time and we would try to go further. He would bring in---what was continuously happening was that he was bringing in all kinds of odds and ends of probably relevant mathematics from his own background, including things like the discrete chaos and Brownian motion, various kinds of things from his earlier papers. He kept bringing these in and I kept being snowed and I got references from him to them and went and tried to read them and it was a very scary hit and miss process, but there was some kind of progress being made because the problem went through several different kinds of evolutions. We really didn't know what the problem we were trying to solve was from one day to the next because it was evolving with our own understanding."

(Interview with Bigelow, Oct. 25, 1980.)

of antiaircraft prediction became evident.[72] It was this statistical aspect of the problem which Wiener, who since the 1920's had been so immersed in statistical mathematical problems, pursued and developed.[73]

If the behavior of the system components had been totally random, no prediction would have been possible. In general, however, the components were not totally random in their behavior. Aircraft of various types underwent characteristic evasive actions and flight patterns. Manual tracking errors also had certain typical error characteristics.[74] It was soon realized that the problem could be considered to have a set of components which was unpredictable, and a set which was predictable.[75] As a result, Wiener and Bigelow were able to treat the aircraft's actual path as a "signal" which was contaminated by the "noise" of the various errors introduced by the tracking process and by other characteristics of the aircraft's

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[72] J. Bigelow and N. Wiener, "A. A. Directors," p. 2.

[73] Bigelow notes here that Wiener's work was primarily in the field of statistical mechanics rather than in "Fisher statistics," which deal with the correlation of data with experimental conclusions (personal communication).

[74] N. Wiener, "Statistical Method of Prediction in Fire Control: Final Report on Section D-2 Project #6," Dec. 1, 1942, in NDRC Div. 7, "Report to the Services No. 59," with anon. cover letter, March 27, 1945, Polar and Scientific Archives Div., record group 227, p. 5.

[75] J. Bigelow and N. Wiener, "A. A. Directors," p. 4.

flight.[76] The signal was assumed to be predictable for at least certain times in the future due to the physical constraints of the aircraft.

The recasting of the problem in terms of signal and noise allowed Wiener to use some of the mathematical tools he had developed when working on the problems of Brownian motion and generalized harmonic analysis. The noise imposed on the path of the Brownian particle consisted of disturbing random impacts from the molecules of the fluid. In the case of the aircraft the noise would consist of the "departure of the path from simple geometry because the pilot does not or can not fly an efficient geometric course---together with whatever cranking and instrumental errors may be introduced by the tracking apparatus and which can not be filtered there."[77]

Since Wiener had had great success previously using statistical methods in the similar problem of Brownian motion, he again chose this path in the fire control problem. In the case of fire control, however, he combined his statistical methods with the concept of feedback. In particular he wanted to produce a method which would minimize, on the average, the difference between the predicted position and the actual position of the aircraft. He stated that "a tentative definition of the best prediction is that in which the mean square distance between the actual and predicted position is a

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[76] Ibid., "cover letter."

[77] J. Bigelow and N. Wiener, "A. A. Directors," p. 4.

minimum, this mean being taken with respect to time."[78] This continual minimization process was where feedback was employed. The error in the prediction is what Wiener wanted to minimize statistically. In practice, Wiener and Bigelow visualized a process which used standard geometrical extrapolation of the flight path as the basic approach, but refined this approach by using feedback to determine the error in prediction, and a statistical process to minimize this error. Wiener referred to the geometrical extrapolation method as the "Bode method." He described it, and the modifications he would add as follows:

> . . . Bode's method is to fix a certain memory-point in the past of the motion of the plane; and to extrapolate linearily along this straight line. Any such method will of course have errors, as the motion is not in fact perfectly rectilinear. These errors may be observed and recorded, and a crude method of improving the Bode prediction would be to correct the future position predicted for the plane by cancelling out the present observed error. This method would involve a negative feedback. It can be made much more effective by replacing the present error of position of the plane computed statistically from this.[79]

The influence of the thought which went into the philosophical paper, "Behavior, Purpose and Teleology," and which Wiener had just co-authored, is evident in this technical statement. Here Wiener makes explicit use of negative feedback in terms of control. The use of feedback for control was the central theme of the philosophical paper.

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[78] N. Wiener, "Statistical Method of Prediction," p. 2.

[79] N. Wiener to W. Weaver, Jan. 15, 1943, Polar and Scientific Archives Division, record group 227.

In order to minimize the error of prediction it was important not only to have some knowledge of the character of the signal, that is, aircraft performance data, but also the ability to separate noise from this signal. For instance, if the removal of superimposed noise on a signal revealed the signal to be a simple sine curve, then a prediction of the future amplitude of the curve at a given time became possible due to the predictability of the sine curve. Wiener's major mathematical contribution stemming from the fire control project was a method he developed whereby noise could be filtered from signals, and the true character of the signal, if it existed could be ascertained and utilized to make a prediction of future values of the signal. Mathematically, any quantity which varies with time produces what is known as a "time series" when its values are plotted against time. Thus Wiener's methods constituted a general approach to the understanding of all time series phenomena where innate signal characteristics were masked by superimposed noise factors.[80] Wiener presented his methods of time series analysis in his paper, "The Extrapolation, Interpolation, and Smoothing of Stationary Time Series with Engineering Applications, February 1, 1942."[81] The technical details of Wiener's method are

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[80]The method is still widely used today, in seismology and oceanography, for example.

[81]N. Wiener, "The Extrapolation, Interpolation, and Smoothing of Stationary Time Series with Engineering Applications, February 1, 1942," in NDRC Section D-2, "Progress Report to the Services No. 19," Polar and Scientific Archives Division, record group 227. Besides the mathematical accomplishments of this paper, Bigelow notes, Wiener's approach was valuable in that he was one of the first

not important for this study. In spite of the notoriously difficult mathematics[82] its value and widespread applicability were soon realized. At the conclusion of the war, when the paper was declassified, it was published as a book.[83] Most importantly the time series work centered Wiener's attention on the statistical aspects of communication engineering. It was the underlying statistical nature of communication engineering that Wiener later used as the dominant theme in his presentation of Cybernetics. The result of this emphasis will be treated in Chapter Six.

In spite of these mathematical achievements and the theoretical advances in the fire control problem, however, the practical problems involved in the project remained great. One problem was the need for data representing true flight paths and resulting tracking responses. These data were needed to generate the statistical properties of these curves, properties which were necessary to produce autocorrelation coefficients, an important component of Wiener's method. Only a limited amount of such data

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researchers to understand that filtering a signal, smoothing it and predicting its future value were really related operations (Bigelow interview, 10/25/80).

[82]The paper was sometimes referred to as the "Yellow Peril," after the yellow cover of the report as it was first issued.

[83]N. Wiener, Extrapolation, Interpolation, and Smoothing of Stationary Time Series with Engineering Applications (Cambridge, Massachusetts: The M.I.T. Press, 1949). The volume is still in print, now with the title, Time Series (Cambridge, Massachusetts: The M.I.T. Press, 1964).

was obtained.[84] Even with the limited data, its processing was so tedious that in the end, only two actual flights were analyzed. Thus, the effectiveness of the proposed prediction scheme in separating actual signal from noise was far from generally proved.[85]

The foremost problem, of course, was to produce a physical device which would incorporate the theoretical advances made by Wiener and Bigelow. Wiener, by his own admission, was the "clumsiest of men," and said later in life "it is utterly beyond me even to put two wires together so they will make a satisfactory contact."[86] The greatest part of the burden of engineering design and construction fell to Bigelow. He constructed an electrical network, based on electrical filters that Wiener and Y. W. Lee had designed in the 1930's, which, with its associated apparatus was designed to

> . . . filter and predict a simulated aircraft 'track.' The characteristics of the filtering-predicting operation could be modified ('programmed,' to use the modern term) in the network by changing its electrical characteristics in accordance with the type of aircraft which was supposed to be the target, some statistical characteristics, aircraft dynamics and typical tracking errors having been analyzed by Wiener and Bigelow from flight path data previously attained. The effectiveness of the predictor network on a given sample of track was experimentally shown by means of a pair of lights projected on the laboratory wall, one (white) moving independently and the other (red) taken from a lagging position of the target motion

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[84] Late in the project accurate paths of two flights, analyzed from theodolite data, were obtained by Bigelow and Wiener during trips to Camp Davis. See N. Wiener, "Statistical Method of Prediction," cover letter.

[85] J. Bigelow and N. Wiener, "A. A. Directors," p. 6.

[86] Wiener, Iamm, p. 112.

> via the predictor network. The extent to which these could be superimposed indicated the closeness of the prediction.[87]

The detailed operation of the test apparatus which Bigelow constructed will not be presented here.[88] Although the test apparatus impressed some government officials,[89] test runs on real data showed little improvement over existing methods.[90] The statistical characteristics of the data did not allow long predictions or "lead times," and the use of "an elaborate, not too easily portable instrument,"[91] for short flight runs, where small guns were used, became questionable. Wiener concluded, "accordingly, there is less scope for further work in this field than we had believed to be the case."[92]

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[87] J. Bigelow, personal communication.

[88] Although the operation of the device was highly interesting, its explication, which would require considerable space and technical detail, would not add greatly to the main ideas being presented in this work. For a description and circuit diagram of Bigelow's device see J. Bigelow and N. Wiener, "A. A. Directors," pp. 9-13. Bigelow has stated recently that whatever successes the device had may not have really been attributable specifically to Wiener's damping coefficients, but simply to the fact that some damping was added; that although the demonstration gave impressive results, "we had no objective criteria as to how good it was compared to alternative (perhaps simpler) smoothing and lead deriving methods" (personal communication).

[89] In one report its operation was described as "astonishing" in certain respects (see NDRC, Gunfire Control, p. 55).

[90] Wiener concluded, "The author finds that an optimum mean square prediction method based on a 10 second past and with a lead of 20 seconds does not give substantial improvement over a memory-point method, nor over existing practice." N. Wiener, "Statistical Method of Prediction," p. 8.

[91] Ibid., p. 7.

[92] Ibid.

Fire Control Project: Summary for the Later Assimilation of Feedback

Although the Wiener-Bigelow fire control project had little impact on the war effort, its importance for the later assimilation of the feedback ideas by the larger scientific community was great. This importance may be broken down into the following significant developments:

1. Bigelow's ideas regarding the role of negative feedback in guidance helped Wiener to conceive of negative feedback as a general mechanism of control and regulation.

2. The project led Wiener, Rosenblueth and Bigelow to consider how purposeful action might be carried out mechanically. The mechanical gun director had to perform as a human might, extrapolating the future path of the aircraft and continually testing this extrapolation against the actual position. The consideration of the mechanical embodiment of purpose in the negative feedback mechanism, and the interpretation of purpose in terms of behavior, led to the writing of the important paper, "Behavior, Purpose and Teleology."

3. The project led Wiener to develop his methods for the extrapolation, interpolation and smoothing of time series. He would soon utilize these methods as one of the bases for his understanding of the processes of communication and control; processes which were central to his cybernetic synthesis. Thus, the development of these methods involved Wiener with the larger problem of how not only purpose, but communication and control, in general, might be

understood in mechanical terms which applied equally to machines and organisms. Wiener's concentration on these larger problems had much to do with the manner in which the feedback concepts were assimilated in the 1940's and 1950's.

The last point emphasizes the fact that Wiener was becoming interested in the entire problem of the mechanical replication of the behavior and capabilities of living organisms. As noted he subsumed this behavior under the two categories of "communication" and "control." During the 1940's there were many other dramatic developments which showed that other human capabilities could be reproduced mechanically. Not surprisingly, Wiener became interested in these developments and incorporated them into his own thinking. One of the purposes of the following chapter is to describe some of the more important of these "robotic" advances, as well as certain other factors relating to the general scientific atmosphere at the time the feedback ideas were being advanced.

CHAPTER FIVE

NEGATIVE FEEDBACK AND THE CONTEXT OF ROBOTICS

The Second World War stimulated an enormous technological development which has continued at an ever accelerating pace. It is generally recognized that the many advances in computing, electronics, communication and control engineering and a myriad of other fields have profoundly changed the nature of societies where these advances have been widely incorporated. The philosophical and intellectual effects of the rapid technological developments are less tangible but, nevertheless, equally profound. The scope and power of the mechanico-reductionist world view, a philosophical basis of modern scientific and technological achievement, has been immeasurably strengthened by the tangible technological products of great sophistication which have resulted from the application of this philosophy. With the rising confidence in mechanico-reductionism, it has become increasingly plausible to claim that life itself is explainable in terms of that view. The "Behavior, Purpose and Teleology" paper[1] and its assimilation represented an important event in the molding of the modern view with regard to the potential of mechanism to explain living phenomena because the paper was successful in promulgating the view that a critical characteristic of living organisms, what it referred to as

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[1]Arturo Rosenblueth, Norbert Wiener and Julian Bigelow, "Behavior, Purpose and Teleology," Philosophy of Science 10 (1943), pp. 18-24.

"teleological behavior," might well be explainable in terms of the negative feedback mechanism. The larger view, that all life processes and capabilities can be replicated mechanically, is part of an old tradition, one which may be termed the "robotic view."[2] There was an avalanche of technological development during the World War II period which stimulated an increasing confidence in the robotic view. Thus, the "Behavior, Purpose and Teleology" paper not only helped to stimulate the robotic view, but was itself received with more interest because of the rising interest in robotics. In particular, factors contributing to the favorable environment for the paper may be briefly indicated as follows:

First, by the 1940's, the concept of feedback had been put on a strong enough theoretical foundation to allow the great extension of its use as suggested by Wiener and his colleagues.

Second, there were many technological and theoretical developments during the period which by mutual reinforcement greatly strengthened the robotic view, of which the "Behavior, Purpose and Teleology" paper was part. These developments occurred in a number of fields, but the progress in computer science and neurophysiology was especially important for strengthening the robotic claims of the "Behavior, Purpose and Teleology" paper.

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[2] I would like to thank Dr. Eugene Fichter, assistant professor of industrial and general engineering, Oregon State University, for the above definition of robotics and his extremely interesting seminar on the same subject.

Third, there was, during the first half of this century, a perceived need in science and philosophy to explain the interaction of the parts of a system which enables it to exhibit coordinated behavior; that is, to explain the dynamic organization of systems. The "Behavior, Purpose and Teleology" paper represented a brilliant attack on this problem, an attack made in terms which could be useful in many areas of science, and which was strengthened by concurrent developments in feedback theory and application and robotics.

Finally, the intense involvement of Wiener, with his great prestige, multidisciplinary talents and personal charisma, all helped greatly in bringing his ideas to a larger audience.

This chapter will examine each of these factors in some detail, with the exception of Wiener's personal role, which will be treated in Chapter Six. Several "antecedent cases" are also examined in the present chapter. Through these cases it will become apparent that ideas similar in some ways to those presented in the "Behavior, Purpose and Teleology" paper but had been "in the air" for some time, but because the unique combination of factors, just noted, were missing in these other cases, they failed to be successful. Thus, the antecedent cases clarify the achievements of Wiener and his colleagues.

PROGRESS IN THE UNDERSTANDING OF FEEDBACK

The purpose of this section is twofold: first, it is designed to give the reader with little background in the subject a fuller understanding of the concept of negative feedback itself, the central concept of this work; second, the section will attempt to show that feedback theory underwent explosive growth during the period just preceding the appearance of "Behavior, Purpose and Teleology." This rapid growth, and the resultant strengthening of the theoretical foundations of feedback theory did much to make the wider claims for feedback made by Wiener and his colleagues more plausible.

Feedback has been briefly discussed in the last chapter in relation to the Wiener-Bigelow collaboration on the fire control project (see pp. 148-152 above). A diagrammatic representation of the feedback process in modern terms is given in Figure 5-1. This type of representation is known as a block diagram. In this case it is designed to represent the logic of feedback control in a general fashion; that is, it applies to any system which uses this type of control. An example of such a system might be that associated with keeping the speed of a motor constant as different loads are applied to it.

The overall process expressed in the block diagram above are as follows: A command is given to bring the system output to a certain state, Θ_r. The actual current state of the output, Θ_a, is measured and compared with Θ_r in the error detector. The error

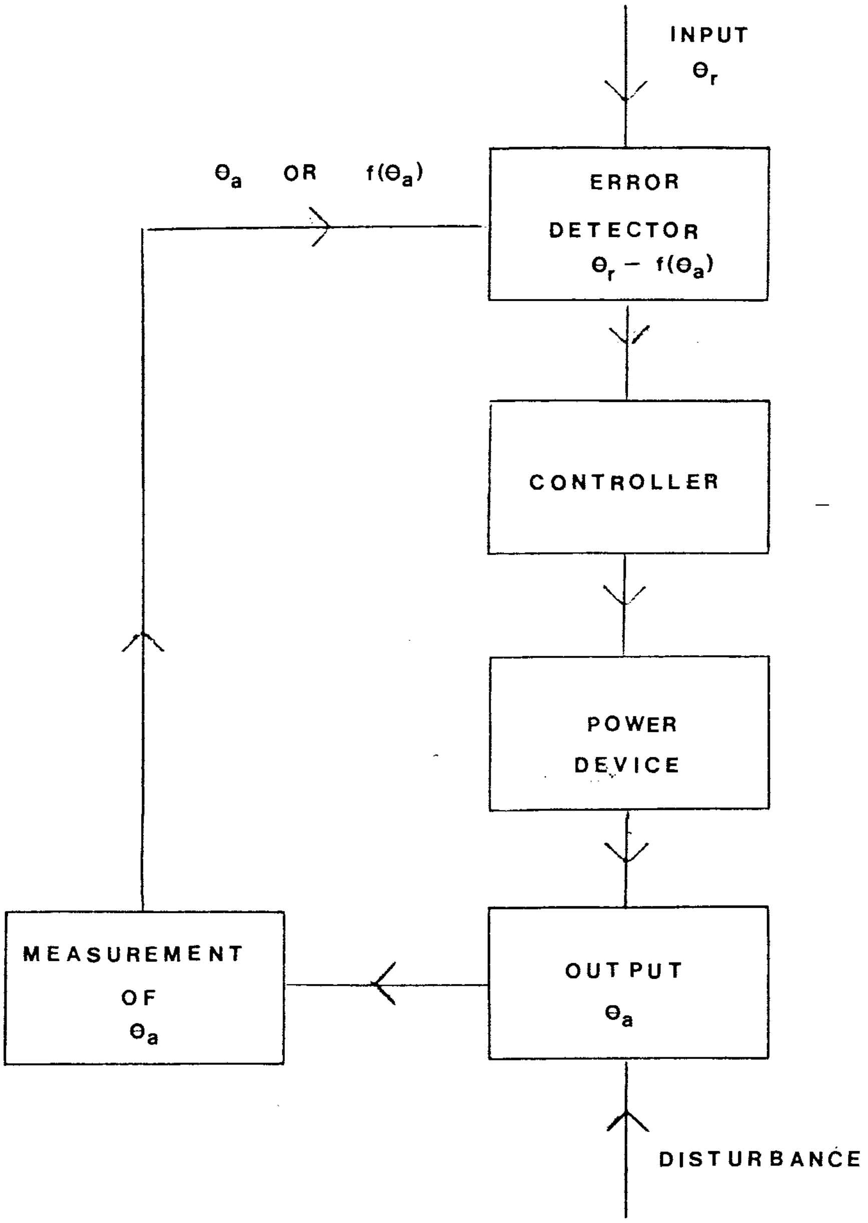

Figure 5-1. Block diagram of control by negative feedback (adapted from G. Thaler, Servomechanism Theory, p. 3).

will be the difference between Θ_r and Θ_a. This error, E, is used to adjust the controller of the power device, which in turn changes the system output. This process occurs continuously. Briefly stated, the output of the system is compared to the desired input; any error is used to adjust the system machinery so as to correct the output to remove the error. In many cases it is desirable, in correcting the system error, not to utilize the system output directly, but to use instead some function of the system output. Hence $f(\Theta_a)$ rather than Θ_a itself is fed back into the error detector.

It should be noted that the system changes the sign of the error in order to make the necessary correction; that is, if an error of (+ 5) units is detected, a correction of (- 5) units is applied to cancel the error. This type of system is referred to as a negative feedback system because of this sign reversal. The negative feedback system is sometimes referred to as a "closed loop system" because the measurement of the output returns to the input stage, completing the loop.[3]

An example of a typical negative feedback process occurs in the system which keeps the paper-rolling tension constant in the paper manufacturing process. This system is illustrated in Figure 5-2.

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[3]Closed-loop systems utilizing positive feedback are sometimes used when it is desired to maintain or increase the difference between the system output and the reference value. Recently the subject of positive feedback has become of interest to some researchers in cybernetics. See, for example, Magoroh Maruyma, "The Second Cybernetics: Deviation-Amplifying Mutual Causal Processes," in Walter Buckley, ed., Modern Systems Research for the Behavioral Scientist: A Sourcebook (Chicago: Aldine Pub. Co., 1968), pp. 304-313.

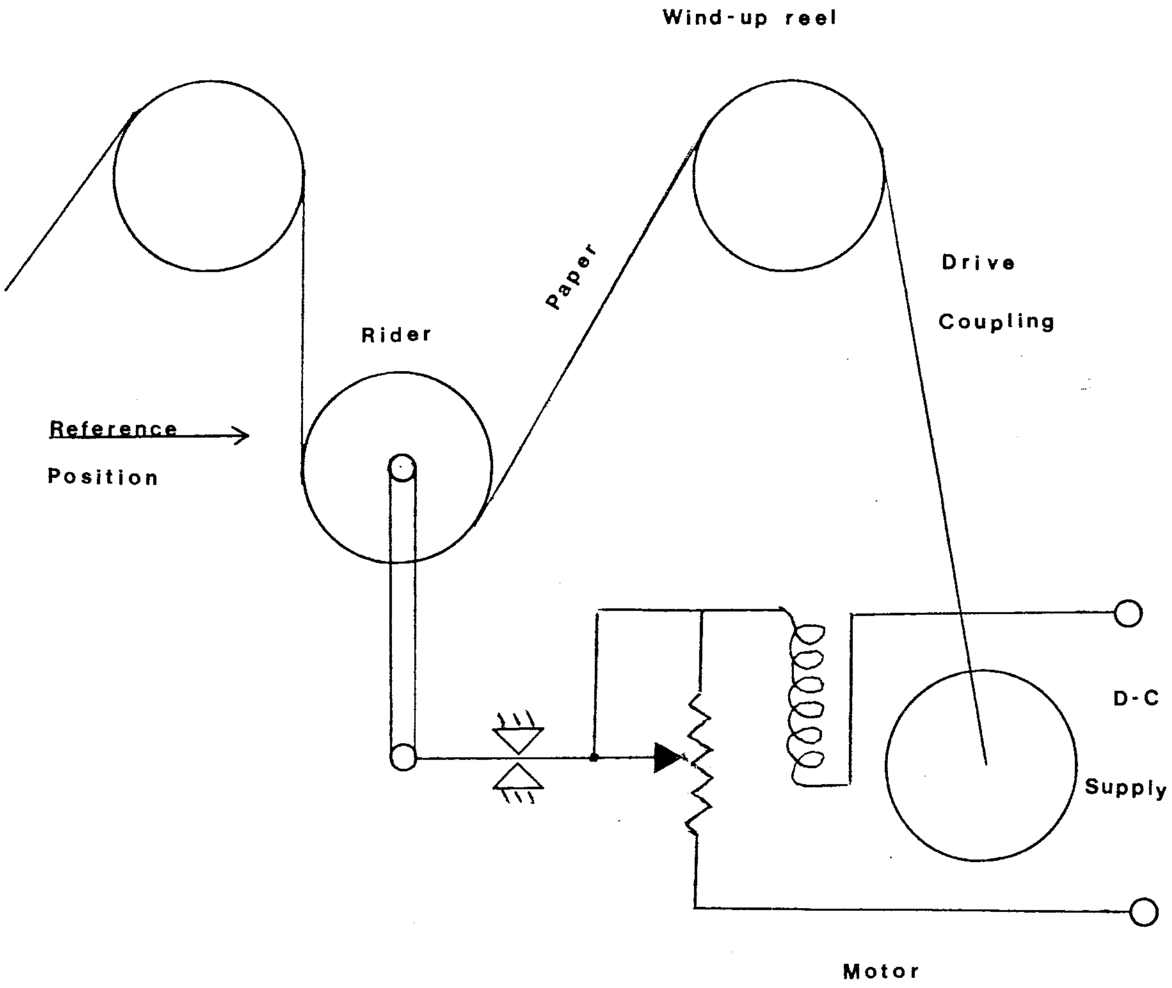

Figure 5-2. Control by negative feedback of paper winding tension (adapted from G. Thaler, Servomechanism Theory, p. 8).

The wind-up reel cannot be operated at constant rotational speed because as the diameter of the paper roll increases the linear take-up speed of the paper also increases, in turn increasing the paper tension. To keep this tension constant the speed of the wind-up reel must be decreased as the paper roll enlarges. The feedback system accomplishes this change by means of a spring-loaded rider wheel which moves up and down in response to tension changes. The vertical motion of the rider wheel is used, through mechanical linkages, to control the motor current and hence the motor speed. As the tension increases, for example, the wheel rises, lessening the motor current and thereby the speed of the wind-up reel.

In terms of the block diagram (Figure 5-1), the command tension is set by the initial position of the rider wheel; the position of this wheel serves both as an error detector and measure of output tension; the controller is the device which varies the input to the motor; the motor is the power device, and the paper tension is the output.

Bigelow's suggestion (see pp. 148-152) concerning feedback in relation to the act of grasping an object with one's hand can also be interpreted in terms of the block diagram: In this case the input command is the desire to grasp the object; the output position of the hand in reference to the position of the object is measured by the eye and brain combination. The controller is the brain while the muscles represent the power device. Although Wiener did not use the block diagram format, it was essentially this description of the grasping operation which he gave in _Cybernetics_.

After noting that feedback was now [1948] thoroughly understood from a quantitative point of view, he continued:

> 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.
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> To perform 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. . . .[4]

Devices utilizing feedback for regulation were built long before the theory of feedback regulation was understood and before the concept of feedback was recognized as such. It is not necessary in this work to establish in detail the development of feedback theory and technology. Such presentations have been given elsewhere.[5] Rather, certain highlights of this development will be presented so

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[4]Norbert Wiener, Cybernetics: Or Control and Communication in the Animal and the Machine, 2nd ed. (New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961), p. 7.

[5]Two very useful works in the history of feedback applications and theory are: Otto Mayr, Zur Frühgeschichte der Technischen Regelungen (Munich: R. Oldenbourg Verlag, 1969); reprinted as The Origins of Feedback Control, tr. by author (Cambridge, Massachusetts: The M.I.T. Press, 1970); and S. Bennett, A History of Control Engineering (London: Inst. of Electrical Eng., 1979). Mayr and Bennett are two of the current leading historians of control engineering. See also, A. V. Khramoi, Ocherk istorii razvitiya avtomatiki v SSSR, Dooktyabr'skii period (Moscow: Izdatel'stvo Akademii Nauk SSSR, 1956); reprinted as History of Automation in Russia Before 1917, tr. from Russian by Israel Program for Scientific Translations (Springfield, Virginia: U. S. Dept. of Commerce, 1969).

that the reader will be better able to appreciate the magnitude of the large changes which occurred in feedback theory and technology during the first half of the twentieth century.

Negative Feedback Developments Prior to 1935

From the vantage point of modern science it is now possible to recognize that certain ancient devices employed what today is termed negative feedback. Simple feedback mechanisms were used as early as the third century B. C. to regulate the water level for water clocks and other devices.[6] Many of these mechanisms employ the float valve, a device in which a float rises with the rising liquid level and by its motion shuts the inlet valve when a predetermined liquid level is reached.[7] The float thus acts both as an error detector and as a measure of output.

An early modern feedback regulator was the flyball, or centrifugal, governor used on the Watt steam engines, of the latter part of the eighteenth century.[8] The governor was utilized to keep the engine speed constant. An example of such a device is sketched in Figure 5-3. The flyball apparatus rotated around its central vertical axis at a speed proportional to the engine speed. The flyballs flew out or dropped inwards from the central shaft

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[6] O. Mayr, Origins, pp. 11-26.

[7] The same device is used today in carburetors, toilets and other devices.

[8] This governor was adapted by Watt from a similar governor used in grinding mills to regulate the distance between the stones and hence the coarseness of the flour. See O. Mayr, Origins, p. 110.

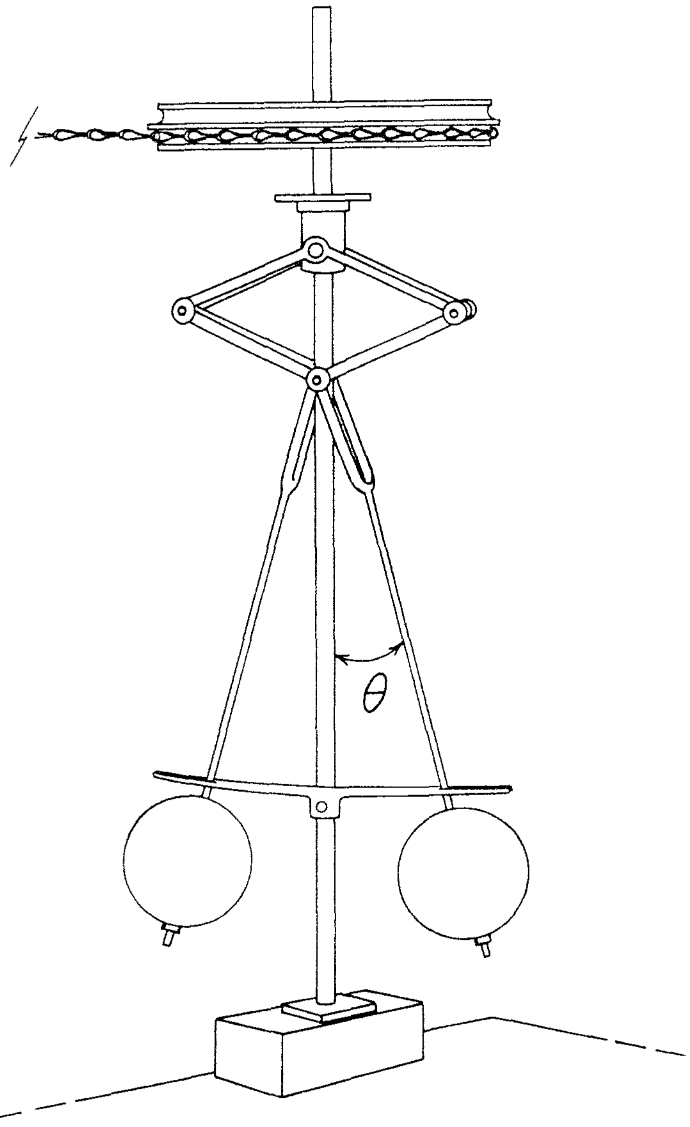

Figure 5-3. Watt's flyball or "centrifugal" governor (redrawn from a photograph of a model of James Watt's "Lap" engine of 1788; in O. Mayr, Feedback Mechanisms, p. 5).

depending on whether the engine speed increased or decreased. As the flyballs moved either our or in they caused the collar on the shaft to move either down or up along the shaft. The motion of this collar, through a series of mechanical linkages, caused the steam throttle to open and close, thereby varying the speed of the engine.

The linkage might be set so that the engine would operate at a speed such that the flyballs would extend out to an angle Θ as in Figure 5-3. If the speed dropped below this value, the flyballs would drop, moving the collar and causing the steam throttle to open, increasing the speed. Too great a speed, however, would cause the flyballs to extend out at an angle greater than Θ, moving the collar and linkages so as to shut down the throttle. Thus a dynamic equilibrium, or balance, was used to obtain an engine speed that was, within certain bounds, constant.

A problem associated with governors of this type[9] is that of <u>offset</u>. Although the governor could deal with temporary engine load changes, permanent load changes would cause the engine to stabilize at a speed higher or lower than the original speed, depending on whether the load decreased or increased. The amount by which the engine speed changed in response to a permanent load change was referred to as offset.[10]

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[9] Governors of this type were later known as "proportional" governors because the correction made to the system was proportional to the error detected.

[10] The cause of offset in the case of the flyball governor may be understood as follows: If the load permanently increases, the engine speed will initially decrease, lowering the flyballs and

Another more serious problem was associated with the governor. In this case the engine speed would increase and decrease in succession, with each variation in speed being larger than the one preceding it. Eventually the erratic speed changes produced could damage or even destroy the engine. This type of malfunction of feedback regulation became known as hunting. It is now known that hunting results when an over-compensation is successively applied to every error detected in the system.

Such problems associated with governors stimulated a number of mathematical inquiries into their performance during the first half of the nineteenth century. The problems were especially difficult to analyze because most engineering problems at that time were seen in terms of static balance,[11] while the governor worked on the principle of dynamic balance.[12] It was not until the early nineteenth century that the behavior of the governor was analyzed in dynamic terms; that the governor was seen as continually

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thus opening the throttle. The resultant increase in engine speed will again cause the flyballs to move outward. Any motion of the balls beyond angle Θ, however, will cause the throttle to shut down due to the original linkage adjustment. Thus the flyballs will return to the angle Θ, and the throttle will be set at the same opening it was for the original load. With the increased load at the same throttle setting the equilibrium speed of the engine must be decreased.

[11] S. Bennett, History, p. 52.

[12] Otto Mayr has argued that the notion of dynamic balance began to appear in the eighteenth century not in engineering, but in writings related to economics, notably by Adam Smith and David Hume. See Otto Mayr, "Adam Smith and the Concept of the Feedback System," Technology and Culture 12 (1971), pp. 1-22.

correcting the speed.[13]

The dynamic aspects of the governors' operation interested the physicist, James Clerk Maxwell.[14] Maxwell's approach[15] typified the efforts which would be made in the analysis of governors before World War II. This approach involved deriving the differential equations for the system of interest, attempting to solve these equations, and then studying the effects of parameter variations on the performance of the governor.[16] Often this approach had little practical value because of certain simplifying assumptions which were needed for the mathematical treatment, but which resulted in an unrealistic model of the system.[17] Despite difficulties with the theory of governors, advances were made in actual governing devices. Inventors, such as Elmer Sperry, for example, created greatly improved governors for the steering and stabilization of ships. These inventors relied on intuition and ingenuity rather than formal theory. As early as 1902 over 1,000 patents for speed-governing

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[13] S. Bennett, History, p. 52.

[14] Maxwell had been interested in many other dynamic problems, including those in electricity, magnetism, and those associated with the structure of Saturn's rings.

[15] James Clerk Maxwell, "On Governors," Proc. Royal Soc. (London) 16 (1868), pp. 270-283; reprinted in George J. Thaler, ed., Automatic Control: Classical Linear Theory (Stroudsburg, Pennsylvania: Dowden, Hutchinson & Ross, Inc., 1974), pp. 7-19.

[16] George J. Thaler, Automatic Control, pp. 1-2.

[17] S. Bennett, History, pp. 81-82.

devices were issued in the United States alone.[18]

One theoretical study of note was by N. Minorsky in 1922.[19] Minorsky was interested in a governor which would automatically steer ships on a straight course. His work was one of the first to point out some of the subtleties of feedback control and describe the technique of what is today termed "three term control."[20] Minorsky saw that a correction which was proportional to the measured error could never lead to a steady output because of necessary time lags in making correstions. He noted that in the steering of ships it is necessary to anticipate the magnitude of the error before the error becomes fully realized. This anticipation is based, Minorsky noted, on the rate at which the error is accumulating. As he stated,

> An efficient helmsman keeps the ship accurately on her course by exerting a properly timed "meeting" and "easing" action on the rudder, i.e., by taking into consideration the elements characterizing the motion from the dynamical standpoint, namely, the instantaneous angular velocity of yawing as well as its time variations.[21]

Minorsky showed that knowledge of positional deviation alone was not enough to provide good control characteristics. The rate of deviation variations, that is, the time derivatives of the deviation,

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[18] O. Mayr, Feedback Mechanisms in the Historical Collections of the National Museum of History and Technology (Washington, D.C.: Smithsonian Inst. Press, 1971), p. 16.

[19] N. Minorsky, "Directional Stability of Automatically Steered Bodies," J. Am. Soc. Naval Eng. 42 (1922), pp. 280-309; reprinted in G. Thaler, Automatic Control, pp. 20-49.

[20] S. Bennett, History, p. 143.

[21] N. Minorsky, "Directional Stability," p. 22.

were also required. This form of control is now referred to as derivative control.

Derivative control, while enabling the governor to more quickly respond to disturbances, still was not able to solve the problem of offset.[22] To accomplish this required a third method of control, integral control; a method which Minorsky stated implicitly.[23]

Minorsky's work had little immediate practical impact. Although the introduction of the theory of three-term control was important, there were considerable problems in designing and building reliable apparatus to measure and combine the three terms.[24] Theoretical developments of the early period culminated in the 1934 paper of

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[22]See George J. Thaler, Elements of Servomechanism Theory (New York: McGraw-Hill Book Co., Inc., 1955), pp. 37-39.

[23]The problem of offset was described in footnote 10. From that description it is apparent that the solution to the problem involves changing the steady state setting of the governor linkages. To make this change properly requires a new signal, related to the degree of offset after the permanent load change. As Thaler has stated,

> "Such a signal cannot be a derivative signal, because the error [offset] is not changing, nor can it be proportional to the error itself, because it would then decrease as the error decreased and a new equilibrium condition would be reached without eliminating the error. The remaining possibility is to introduce a signal proportional to the integral of the error. If a drive torque which is proportional to the time integral of the error is added to the normal drive torque which is proportional to the error, it is readily seen that the error must ultimately be reduced to zero. This is apparent from simple logic, since the integral term eventually becomes infinite if the error does not disappear and would then apply infinite torque."

(G. Thaler, Servomechanism Theory, p. 39).

[24]S. Bennett, History, p. 148.

H. L. Hazen, the "Theory of Servo-Mechanisms."[25] Hazen's work was stimulated in part by the need of Vannevar Bush for accurately controlled mechanical relays in a computer he was then building. This machine was the forerunner of Bush's "differential analyzer."[26] In his paper, Hazen described the operation of three-term control in many different types of servo-mechanisms and generalized the mathematical analysis. His work has been called "the first definitive paper in feedback controls."[27]

As powerful as Hazen's analysis was it suffered from the same problems which had faced earlier theoretical discussions. These technical problems have been summarized by Thaler as follows:

1. It was difficult to include all the real system factors in a mathematical model which was tractable to solution.

2. When equations of high order were generated roots could not be found; solutions for non-linear equations were not known.

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[25] H. L. Hazen, "Theory of Servo-Mechanisms," J. Franklin Inst. 218 (1934), pp. 279-331; reprinted in G. Thaler, Automatic Control, pp. 50-102. The term "servo-mechanism" has been subject to many definitions. Most frequently it refers to a mechanism in which a motor within a negative feedback system controls mechanical positioning, such as in the "servo-motors" used to direct anti-aircraft guns. But the term is also used as a general expression to describe machines that operate on the principle of negative feed-back. Hazen defined a servo-mechanism as a "power amplifying device in which the amplifier element driving the output is actuated by the difference between the input to the servo and its output" (H. Hazen, "Theory," p. 54).

[26] See Vannevar Bush and H. L. Hazen, "Integraph Solution of Differential Equations," J. Franklin Inst. 204 (1927), pp. 575-615.

[27] G. Thaler, Automatic Control, p. 4.

3. Even if the differential equations could be solved the solutions did not usually provide guidance for design changes.[28] Developments stemming from advances in electronic engineering just prior to World War II, and from the war itself, would do much to solve these problems.

The Frequency Response Method

The advent of World War II stimulated tremendous technological and scientific development.[29] Besides the research efforts carried out directly in the military services, the government established a civilian scientific research organization in 1940, entitled the National Defense Research Committee (NDRC), which, in 1941, was reorganized under the title of the Office of Scientific Research and Development (OSRD).[30] Through these agencies the government sponsored scientific research projects at universities and in private industry all over the country. In the 1941 reorganization 19 divisions specializing in different aspects of war

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[28] Ibid., p. 1.

[29] Much of this development is described in the Science in World War Two series, of which the most relevant volume for the present work is, Joseph C. Boyce, ed., New Weapons for Air Warfare (Boston: Little, Brown & Co., 1947). The front matter of this work contains information on the other six volumes in the series relating to developments in medicine, chemistry, physics, electronics, optics, metallurgy and the administration of the scientific war effort.

[30] Although the United States had not yet entered the war, there was wide-spread belief that this country would soon be involved. These agencies were established in preparation for the entry of this country into the war.

instrumentation were created. Other divisions were established for war-related medical problems. The Wiener-Bigelow fire control project was one of the thousands of projects sponsored.[31]

The need for accurate control of the large guns used in the war, mechanical computation devices, tracking devices and other apparatus stimulated a great deal of research into automatic control devices, most of which employed negative feedback. Much of the government-sponsored war research in this area was performed at Massachusetts Institute of Technology, where a research laboratory entirely devoted to servo-mechanisms was established. Work was also done at the Bell Telephone Laboratories and the General Electric Company.[32] The research bore dramatic results. A new method was developed by which feedback mechanisms of great sophistication could be designed. This method gave realistic results, made possible the simultaneous analysis of multiple feedbacks in one system and could be applied by a large number of practicing engineers, thus making it more powerful than the older methods. This new method, and the mathematical tools associated with its use, is now referred to as the

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[31]This project was initially sponsored under section D-2 of the NDRC, but was reclassified under Division 7 of OSRD.

[32]G. Thaler, Automatic Control, p. 262. The Massachusetts Institute of Technology Servo-mechanism Laboratory work was summarized after the war in H. M. James et al., eds., Theory of Servomechanisms, Radiation Laboratory Series, vol 25 (New York: McGraw-Hill Book Co., Inc., 1947).

frequency response method.[33]

The technical details of the frequency response method are beyond the scope of this work. An indication of its approach can be given, however. During the early 1930's advances were made in the design of electronic amplifiers utilizing feedback. It was in this electronic research area that the term "feedback" first became widely used in the late 1920's.[34] The most influential researchers in utilizing feedback in electronic amplification were Harry Nyquist, H. S. Black, and H. W. Bode.[35] Among the contributions of these works were a number of graphical techniques, such as the "Nyquist Plot," which made it relatively easy for a technician to predict in advance if a feedback amplifier would be stable in its operation.

During 1940, in a burst of activity related to preparation for the possible entry of the United States into the war, it became apparent, "practically simultaneously," to groups at Massachusetts Institute of Technology and the Bell Telephone Laboratories that the

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[33]For a brief overview of the development of this method, see Rufus Oldenburger, ed., Frequency Response (New York: The Macmillan Co., 1956), pp. v-ix. There were parallel developments in the frequency response method in Europe during World War II. These European developments will not be described here as they did not significantly affect the events in this country leading to the dissemination and transformation of negative feedback.

[34]S. Bennett, History, p. 1.

[35]Harry Nyquist, "Regeneration Theory," Bell Syst. Tech. J. 11 (1932), pp. 126-147; H. S. Black, "Stabilized Feedback Amplifiers," Bell Syst. Tech. J. 13 (1934), pp. 1-18; H. W. Bode, "Relations Between Attenuation and Phase in Feedback Amplifier Design," Bell Syst. Tech. J. 19 (1940), pp. 421-454. All these works are reprinted in G. Thaler, Automatic Control.

same methods which worked well for the design of electronic amplifiers could be used to help design feedback control in mechanical devices.[36] In this application analogies were made between electrical and mechanical properties, allowing the same techniques which were developed for electric circuits to be applied to mechanical devices. The mechanical property of inertia, for example, could be made equivalent to the electrical property of inductance.[37]

The essence of the frequency response method involved subjecting the system input to a series of sinusoidally varying disturbances of various amplitudes, frequencies and phases. The system's response to each sinusoidal component was then evaluated and the overall effect was obtained by a summation of all the responses to the various inputs.[38] This method emphasized the phase relationship between the input and output, an important relationship which was

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[36] R. Oldenburger, Frequency Response, p. v.

[37] Almost all the war research in servo-mechanisms was classified and not published until after the war. One of the first public presentations summarizing the war work was LeRoy A. MacColl, Fundamental Theory of Servomechanisms (New York: D. Van Nostrand Co., Inc., 1945). MacColl stressed the "essential identity" of the servo-mechanism theory with the "highly developed theory of feedback amplifiers" (p. 1). MacColl worked with Bell Telephone Laboratories during the war. An explicit statement of the electrical-mechanican analogies was given about the same time by another Bell researcher, Robert E. Grahm, in "Linear Servo Theory," Bell Syst. Tech. J. 25 (1946), pp. 616-651; reprinted in G. Thaler, Automatic Control, pp. 276-311. Grahm's article has been referred to as a basic "resumé" of the techniques developed at Bell Laboratories during the war (G. Thaler, Automatic Control, p. 262-263).

[38] R. Grahm. "Linear Servo Theory," p. 279.

not treated by earlier methods. Another useful aspect of the method involved the transfer function, a ratio of the system output to the system input, which summarized the overall effect of the system.[39]

As a result of the frequency response method the problem of informational noise within a servo system became subject for the first time to analytical treatment.[40] Any feedback system must transfer information. The measurement of the output state, for example, is a measure of information which must be transmitted to the error detector. This error is, in turn, information which must be transferred to the controller. Any transmission of information, whether it be electrical, acoustical, optical or any other form, is subject to disturbing factors collectively designated as noise. The reduction of these noise disturbances was made possible by the frequency response method.

Before concluding the topic of advances in feedback analysis, one important technical limitation of all the methods described here should be noted. In almost all cases the mathematical analysis

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[39]The transfer function has been defined as "the ratio of the steady-state sinusoidal output of that device [a linear servo component] to the steady-state sinusoidal input which is causing that output expressed in complex numbers" (G. Thaler, Servomechanism Theory, p. 53). The mathematical technique of the LaPlace transform was also used in obtaining the transfer function. Relatively simple manipulative operations of the transfer functions for individual servo components allowed the derivation of the overall transfer function for the system (G. Thaler, Servomechanism Theory, p. 74).

[40]For an early description of the treatment of noise by the frequency response method, see R. Grahm, "Linear Servo Theory," pp. 299-300.

required that the system under consideration be linear. In a linear system if an input A results in an output A', and input B results in an output B', then if A and B are simultaneous inputs, the output will be the sum of A' and B'. Many systems do not satisfy this requirement. Nevertheless, many of these systems are approximately linear and the methods developed during the 1940's represented a great advance in the analysis of these systems.

The net result of the development of the frequency response method was to create a connection between mathematical design techniques and the actual construction of servo devices by engineers. The marriage of practice and theory in automatic control devices created a new discipline, that of control engineering. The important role that World War II played in the radical altering of the automatic control field has been summarized by Otto Mayr:

> Practical control engineering made great progress during the Second World War, when each belligerent made efforts to gain superiority in this field. When after the war the secrecy was lifted, there suddenly became available (1) a mature technology of automatic control which had proved itself in dealing with the problems of radar, fire control, autopilots, guided missiles, and so on; (2) a theory that was universal and easy to manipulate; and (3) a staff of scientists and engineers who quickly spread this new knowledge, thus introducing the era of automation and cybernetics.[41]

The publication of "Behavior, Purpose and Teleology" in the midst of these rapid developments in control theory may be seen as one manifestation of the increased activity in the field. At the same time, the great new successes in the application of feedback increased the plausibility of the wider claims made for the role of

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[41] O. Mayr, Origins, p. 132.

feedback by the paper. Certainly this increased plausibility had much to do with the great excitement the paper generated.

THE EXPANSION OF THE ROBOTIC VIEW: DEVELOPMENTS IN COMPUTER SCIENCE AND NEUROPHYSIOLOGY

The view that human capabilities can be reproduced mechanically is part of an old and well established tradition, that of robotics. Of course, it is true that "robotics" is a fairly new word, but the idea itself is hardly new. It arose concurrently with the mechanistic world view in the seventeenth century.[42] Descartes, for example, held that animals were automatons which functioned like clockwork and that, in fact, a machine in the shape of an animal would not be distinguishable from the animal itself.[43] Even more extreme robotic views were given in the same century by Alfonso Borelli, who compared the heart to a wine press and considered the processes of walking and flying in terms of levers and other machines. La Mettrie is also well known for his extreme mechanistic interpretations of biological phenomena.[44] Thus as early as the

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[42] John Cohen, Human Robots in Myth and Science (New York: A. S. Barnes and Co., 1967).

[43] Leonora Cohen Rosenfeld, From Man-Machine to Beast-Machine: Animal Soul in French Letters from Descartes to La Mettrie (New York: Octogon Books, 1968), p. 6.

[44] For a detailed discussion of the rise of the mechanistic world view see Jan Eduard Dijksterhuis, The Mechanization of the World Picture, tr. C. Dikshoorn (Oxford: Clarendon Press, 1961). See also Herbert Butterfield, The Origins of Modern Science, rev. ed. (New York: The Free Press, 1957), pp. 133-139.

seventeenth century, the view that organisms could be viewed as automatons was advanced and widely circulated. The continuing technical and theoretical developments of subsequent periods further strengthened this view as the behavioral capabilities of machines became more complex and the detailed elucidation, in mechanistic terms, of physiological processes proved successful. These developments were especially rapid during the World War II period, and, significantly, many of these occurred in the mechanistic replication and interpretation of intelligence, a capacity which presented and still presents one of the greatest challenges for the robotic view. The developments relating to intelligence occurred primarily in the areas of mechanical computing and neurophysiology and, as was the case with the rapid growth of feedback theory during the same period, helped to create a fertile environment for the robotic proposals of the "Behavior, Purpose and Teleology" paper. In order to enable the reader to better appreciate the context in which this important paper was discussed, relevant developments in the fields of mechanical computing and neurophysiology will now be explored.

Developments in Mechanical Computation

The rapidity with which the mechanical computation field grew during the World War II period can perhaps best be seen by first briefly reviewing the nature of some of the earlier developments in the field. A mechanical calculator which could add and subtract was built by Blaise Pascal in the middle of the seventeenth century, while Leibnitz is generally credited for beginning the foundations of a mechanical basis for reasoning in his quest for "a general method in which all the truths of reason would be reduced to a kind of calculation."[45]

Machines with reasoning (branching) capacity did not become common until the middle part of the present century. But mechanical calculating machines underwent continuous development, primarily in the hope of producing more accurate mathematical tables. One of the most interesting attempts to construct such a machine was by Charles Babbage. His "Difference Engine" was designed to evaluate polynomials of one unknown by means of an algorithm involving the differences of several successive columns of numbers. There were great difficulties in the actual construction of the machine due to the inability to make parts to precise enough specifications. When the machine was finally built it could operate for long periods of time but was, in fact, not faster than a competent mathematician in

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[45]Quoted in E. T. Bell, Men of Mathematics (New York: Simon and Schuster, Inc., 1937), p. 123. Leibnitz also invented a calculator which could perform all four arithmetic functions. For a detailed description of the calculators of both Pascal and Leibnitz see L. Leland Locke, "The Contributions of Leibnitz to the Art of Mechanical Calculation," Scripta Mathematica 1 (1933), pp. 315-321. Although Leibnitz apparently believed that the reasoning process could be mechanized, he did not believe that human perceptions could be mechanized. He imagined building a big machine which was to embody human perceptions, and then entering it so that "one might go into it as into a mill." Upon going in, Leibnitz continued, "we should, on examining the interior, find only parts which work one upon another, and never anything by which to explain a perception" (quoted in W. S. McCulloch, "Recollections of the Many Sources of Cybernetics," ASC Forum 6 (1974), p. 10).

its operation.[46]

Babbage's machine was digital in its operation; that is, it used actual counting processes for its computations. Other examples of digital devices include the abacus and the old-style mechanical adding machines which utilized geared wheels with numbered teeth. The great majority of calculating machines built prior to World War II were not digital in their operation but analogical. The analogue process requires a measurement of some physical property which is made proportional to the quantity to be quantified. The mercury thermometer is an analogue device in which the length of the mercury column is proportional to the numerical value of the temperature. The clock face with hands displays numerical values of time which are proportional to the angles of rotation of the hands, hence it is also analogical in nature. An early analogue device designed to measure areas under the curves of smooth mathematical functions, that is, to integrate the functions, was the planimeter, or integrator, developed in the 1860's by James Thompson.[47] Thompson was the brother of William Thompson, Lord Kelvin, who used the integrator in his "harmonic analyzer," which was used to produce accurate tide tables. This integrator worked on the basis of two

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[46] Herman H. Goldstine, The Computer from Pascal to Von Neuman (Princeton, New Jersey: Princeton University Press, 1972), pp. 11-12. Babbage's machine was not completed by Babbage, but by one of his followers, Georg Schultz in 1853. Babbage designed a more ambitious computer, the "Analytical Engine," which he did not complete.

[47] Thompson's integrator was a later version of one invented by James Clerk Maxwell some ten years earlier. For its operation see H. Goldstine, The Computer, pp. 41-44.

wheels which were rolled in a particular fashion along the curve of interest. The number of rotations of the smaller wheel gave the area under the curve. Kelvin showed that if two of the integrators could be linked together mechanically, the process of solving differential equations could also be performed. The problem of physically linking the integrators was one which the technology of the day could not solve however.

The realization of Kelvin's suggestions for a machine to solve differential equations would not occur until the 1930's when Vannevar Bush produced his "Differential Analyzer," the epitome of analog machines. The machine utilized six of the Kelvin-style integrators, hundreds of gears and shafts and occupied a whole room. It could solve second-order linear differential equations about 50 times faster than a human could.[48]

Characteristic weaknesses of all analog machines, no matter how sophisticated, included the limits on the precision they could obtain and the speed with which they could operate. As Goldstine comments:

> Given a machine whose operation is describable by a mathematical formula, we quickly run up against two realities of engineering: every machine is built out of parts having certain tolerances and capable of running at certain speeds. These determine the accuracy and speed with which the mathematical system can be solved. Not only is there an upper limit on how exactly one can smooth physical surfaces, cut gears, etc., there is also a degradation that sets in with age when the parts become less accurate, so that the precision of

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[48]Vannevar Bush, "The Differential Analyzer: A New Machine for Solving Differential Equations," J. Franklin Inst. 212 (1931), pp. 447-488. See also, H. Goldstine, The Computer, pp. 84-94, 138.

> the total machine's response diminishes. Moreover, it is often true that the faster a mechanical device is driven the less accurately it depicts the mathematical situation, with the result that often accuracy and speed are linked together in a disastrous, or at least unhappy way. So it was with the differential analyzer. If it was run slowly, its accuracy was considerably better than when it was run fast.[49]

Digital machines, on the other hand, can be designed to any degree of precision. The great promise of Babbage's early digital machine was not realized for 50 years because of the early machine's cost and relatively slow speed. Interestingly, just when mechanical technology had advanced to the point where the Babbage-type machine could be built to operate at much greater speeds, a new technology evolved which completely eclipsed the mechanical computer technology altogether. The great breakthrough was in the field of electronic computing and it was stimulated, as was the development of servo-mechanism theory, by the immediate needs of World War II.

The brief reign of the mechanical digital technology centered around the electromagnetic relay, a device which was activated electrically, but nevertheless relied on the mechanical motion of its metal parts to accomplish its switching function within circuits. The on-off character of these relays made them ideal for use in binary digital calculations. Digital computers based on the electromechanical relay were planned in the late 1930's; one

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[49] H. Goldstine, The Computer, pp. 140-141.

machine was completed in 1940 and a second in 1944.[50]

The mechanical digital relay machines were conceived on the eve of World War II. Although they represented a vast improvement in speed over other digital mechanical machines, they were not quite as fast as Bush's differential analyzer. The speed of neither of these machines, however, was adequate for the generation of ballistics trajectory tables which was the principal desired utilization of computers during the war. Even Bush's differential analyzer, working full time, required a full month to generate one such table for one particular gun. As a result, Army officers responsible for the generation of ballistics tables were actively seeking out new and faster methods of computation.[51]

During the late 1930's attempts were made by J. V. Atvanasoff at Iowa State University to construct a digital computing machine based on electronic rather than electromechanical switching. A simple electronic (triode) vacuum tube provided the switching function instead of relays. The electronic switching process was 1,000 times faster than the electromechanical one, and thus the new

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[50] The Mark One, or "Harvard" machine, was a joint project of Harvard University and International Business Machines, and was completed in 1944. The Mark One was designed in 1937. See H. H. Aiken, "Proposed Automatic Calculating Machine (1937)," in Brian Randell, ed., The Origins of Digital Computers: Selected Papers (New York: Springer-Verlag, 1973), pp. 191-197. Another machine was completed at Bell Laboratories in 1940. See G. R. Stibitz, "Computer (1940)," in B. Randell, Origins, pp. 241-247.

[51] H. Goldstine, The Computer, p. 138. Goldstine was one of the two army officials in charge of the generation of ballistics trajectory tables.

method held great promise.[52] The great number of vacuum tubes called for in the operation of the electronic machine was a potential problem. To build a large scale machine would require thousands of these tubes acting in concert. creating an unprecedented engineering problem.

Word of Atvanasoff's method reached the Army Ordinance Ballistics Research Laboratory in Philadelphia, where it was decided in April, 1943 to underwrite the construction of a large digital, electronic computer at the Moore School of Engineering at the University of Pennsylvania.[53] The machine became known as the ENIAC (Electronic Numerical Integration and Computer). It represented an enormous undertaking incorporating 18,000 vacuum tubes, utilizing 140 kilowatts of power and occupying 3,000 cubic feet.[54] A large team was assembled for the construction of the ENIAC and it was completed in a little over two years, the formal dedication taking place in February of 1946.[55]

The ENIAC ushered in the electronic computing age. Its speed was three orders of magnitude greater than any other machine built before it. The excitement it generated is reflected in the writing

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[52] See the memorandum of J. V. Atvanasoff, "Computing Machine for the Solution of Large Systems of Linear Algebraic Equations (1940)," in B. Randell, Origins, pp. 305-325.

[53] B. Randell, Origins, p. 290.

[54] A. W. Burks, "Electronic Computing Circuits of the ENIAC," Proc. I.R.E. 35 (1947), p. 756.

[55] B. Randell, Origins, p. 291.

of A. W. Burks, one of its designers, writing at the time of its dedication. He compared speeds for computing ballistics trajectories:

> A skilled computer with a desk machine can compute a 60 second trajectory in about 20 hours; a differential analyzer can produce the same results in about fifteen minutes; the ENIAC can do it in thirty seconds, that is, it can compute the trajectory of a shell faster than the shell itself flies! Moreover, the ENIAC, which can handle either ten- or twenty-digit numbers, is much more accurate than a differential analyzer, and is, in fact, 1000 times as fast as any machine which gives comparable accuracy.[56]

The same writer went on to say:

> Because the ENIAC combines the desirable features of speed and reliability, it is capable of solving problems hitherto beyond the scope of science. Thus it inaugurates a new era, an era of electronic computation.[57]

More recently (1970), an historian of computers, Brian Randell, has noted:

> The ENIAC's importance in the development of computers is unquestioned. It was the first large electronic computer to become operational and many scientists and mathematicians visited the Moore School to learn about the machine, and in some cases, to use it.[58]

One of these scientists who came to the Moore School was John von Neumann, who became active in the project in August of 1944. He ultimately became the designer of the ENIAC's successor, the EDVAC.[59] Von Neumann's interest in computing would play a significant role in the activities relating to the development of

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[56] A. W. Burks, "Electronic Computing," p. 756.

[57] Ibid., p. 767.

[58] B. Randell, Origins, p. 291.

[59] H. Goldstine, The Computer, p. 186.

cybernetics (the "Macy Meetings"; see below pp. 232-233).

Clearly the art of automated computation became greatly more sophisticated. Although the workings of the ENIAC were not mechanical in the sense of the motion of gross physical parts, its operation was mechanical in that it was a machine. The explosive growth of computer power during the 1940's did much to enhance the robotic view because a human capability, computing, had not only been replicated mechanically, but in certain ways (speed and accuracy) surpassed. Of course the entire Industrial Revolution and the associated technology which developed with it can be viewed as aiding the robotic view. But the developments in computers and servo-mechanisms, which dealt with factors relating to human intelligence, were especially important. They provided a pragmatic legitimacy for the robotic view of intelligence that Descartes and the other early mechanists could not supply.

Developments in Neurophysiology

There were yet other important developments during the World War II period which tended to enhance the robotic view. The modern roots of what became known as "artificial intelligence" during the 1940's were established in the neurophysiological work of Warren S. McCulloch and Walter Pitts, and by Alan M. Turing in computation theory. Both lines of research dealt with the logical capabilities and potential of a binary, or "on-off," based logic system.

Turing's work was published in 1936.[60] He hypothesized a relatively simple machine, but nevertheless showed that it would be capable of calculating any computable number. The basis of operation of the "Turing machine" has been described by McCulloch:

> He [Turing] considers a machine, made of a finite number of discrete parts, capable of a finite number of distinct internal states. It works on an endless tape divided into squares, each of which contains one of a certain few possible marks or no mark. The machine can observe one square at a time; it can tell which mark, if any, is in the square; then, depending on its internal state, it can erase the mark there, print one, if it is vacant, or move the tape so as to scan the square before or behind, and alter its own internal state. Turing has made it almost certain that such a machine can compute any number a man can, with paper and pencil, according to any uniform method or algorithm.[61]

Since the marks which could be printed on the tape were either a one or a zero, the system was binary.

The work of McCulloch and Pitts in 1943, "A Logical Calculus of the Ideas Immanent in Nervous Activity,"[62] showed that the

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[60] Alan M. Turing, "On Computable Numbers, with an Application to the Entscheidungsproblem," Proc. London Math. Soc. Ser. 2, 42 (1936), pp. 230-265.

[61] Warren S. McCulloch, "Toward Some Circuitry of Ethical Robots or an Observational Science of the Genesis of Social Evaluation in the Mind-Like Behavior of Artifacts," Acta Biotheoretica 11 (1956); reprinted in W. S. McCulloch, Embodiments of Mind (Cambridge: The M.I.T. Press, 1965), p. 197.

[62] W. S. McCulloch and Walter Pitts, "A Logical Calculus of the Ideas Immanent in Nervous Activity," Bull. Math. Biophysics 5 (1943), pp. 115-133; reprinted in W. S. McCulloch, Embodiments, pp. 19-39. The reader should note that the word used in the title of this article is "immanent," meaning "operating within," and not "imminent," meaning "about to occur."

Pitts was the sole author of four articles on nervous nets in the preceding issues of the same journal. He was apparently the principal author of this article (McCulloch indicates this in his "Recollections," p. 11). Pitts (1924-1969) is a fascinating functioning of the brain was logically equivalent to a Turing machine. As McCulloch later stated, in an interpretation of his earlier collaboration with Pitts, "Pitts and I showed that brains

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character about whom not much is known. Like Wiener and von Neumann he was a child prodigy, but unlike them he was almost entirely self-taught, never having graduated from high school. He first became known, it seems, when, as a teenager in Chicago, he entered Rudolph Carnap's office at the University of Chicago and, without revealing his identity, pointed out some errors in a work of Carnap's and immediately left. It was also about this time that he completed the "Logical Calculus" article with McCulloch. Despite the best efforts of McCulloch (and later Wiener) to get him involved in a university degree program, he never would participate. According to most accounts he was one of the most widely knowledgable participants in the group which later developed cybernetics, and possibly one of the brightest of that extraordinary group. McCulloch later stated his debt to Pitts during a lecture in 1961:

> "But neurophysiology moved ahead, and when I went to Chicago, I met Walter Pitts, then in his teens, who promptly set me right in matters of theory. It is to him that I am principally indebted for all subsequent success. He remains my best advisor and sharpest critic. You shall never publish this until it passes through his hands." (W. S. McCulloch, "What is a Number that a Man May Know it, and a Man, that He May Know a Number?" in W. S. McCulloch, Embodiments, p. 9.)

Wiener wrote in 1945 in recommending Pitts for a Guggenheim Fellowship:

> "You will see by this that we consider Pitts not merely as a promising young man but as one of the really significant scientists of the country. His obtaining the Doctor's degree is purely a matter of formality. We are counting on incorporating him permanently in our department and regard him as one of us for the future. These accounts speak louder than any words I could say but if you prefer an expressed verbal estimate of his ability I should like to tell you that he is without question the strongest young scientist whom I have ever met. With his broad basis of knowledge both in the physiological sciences and in mathematics he occupies a place which is not filled by any other person. I should be extremely astonished if he does not prove to be one of the two or three most important scientists of his generation not merely in America but in the world at large." (N. Wiener to H. Moe, ca. Sept. 1945, Norbert Wiener Papers [MC 22],

were Turing machines, and that any Turing machine could be made out of neurons."[63] To obtain this equivalence, McCulloch and Pitts used systems of inter-connecting neurons, called "neural nets," where each neuron operated on an on-off basis and was subject to certain other restrictions.[64] The on-off character of the neurons provided

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Institute Archives and Special Collections, Massachusetts Institute of Technology Libraries, Cambridge, Massachusetts).

Pitts and McCulloch, after several previous incidents, had an irreparable personal falling out with Wiener in the early 1950's. This event apparently devastated Pitts and he did little if any professional work after that time. He is said to have burned his papers before he died in 1969. (Information on Pitts obtained from interviews with Dr. Jerome Lettvin, September 4, 1980 and Mrs. Rook McCulloch, July 25, 1981.)

[63] W. S. McCulloch, "Mysterium Iniquitatis of Sinful Man Aspiring into the Place of God," Scientific Monthly 80 (1955), pp. 35-39; reprinted in W. S. McCulloch, Embodiments, p. 159.

The explicit connection between neural networks, the brain and the Turing machine was made in the original "Logical Calculus" paper as follows:

> "It is easily shown: first, that every net, if furnished with a tape, scanners connected to afferents, and suitable efferents to perform the necessary motor operation, can compute only such numbers as can a Turing machine; second, that each of the latter numbers can be computed by such a net; and that nets with circles can compute, without scanners and a tape, some of the numbers the machine can, but no others, and not all of them. . . . If any number can be computed by an organism, it is computable by these definitions and conversely." (W. S. McCulloch and Walter Pitts, "Logical Calculus," p. 35.)

[64] W. S. McCulloch and W. Pitts, "Logical Calculus," p. 22. These restrictions were such that the neuron used in the paper, the "McCulloch-Pitts neuron," was not in any real sense a model of the actual biological neuron. The strength of the paper lay in its assertion of the theoretical basis upon which the behavior of any brain must be characterized. See Seymour Papert's "Introduction" in W. S. McCulloch, Embodiments, p. xviii.

the close logical connection with the binary operation of the Turing machine. The work of McCulloch and Pitts involved the processing of logical propositions by the structure of the neural net, that is, as McCulloch later stated, such nets could "realize any proposition which is a logical consequence of its input."[65] The robotic implications of the McCulloch-Pitts work were enormous. McCulloch later pointed out that from it, it follows that:

> We can build a machine that will do with information anything that brains can do with information---solve problems, suffer emotions, hallucinate on sensory deprivation, what you will---provided we can state what we think it does in a finite and unambiguous manner.[66]

And as von Neumann pointed out in 1949:

> It has often been claimed that the activities and functions of the human nervous system are so complicated that no ordinary mechanism could possibly perform them. It has also been attempted to name specific functions which by their nature exhibit this limitation. It has been attempted to show that such specific functions, logically, completely described, are per se unable of mechanical, neural realization. The McCulloch-Pitts result puts an end to this. It proves that anything that can be completely and unambiguously put into words, is *ipso facto* realizable by a suitable finite neural network.[67]

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[65] W. S. McCulloch, "Why the mind is in the Head," in L. A. Jeffress, ed., *Cerebral Mechanisms in Behavior; The Hixon Symposium* (New York: John Wiley, 1951), pp. 42-111; reprinted in W. S. McCulloch, *Embodiments*, p. 77.

[66] W. S. McCulloch, "Where is Fancy Bred?" in Henry W. Brosin, ed., *Lectures on Experimental Psychiatry* (Pittsburg: University of Pittsburg Press, 1961), pp. 311-324; reprinted in W. S. McCulloch, *Embodiments*, p. 221.

[67] John von Neumann, "The General and Logical Theory of Automata," in L. A. Jeffress, *Cerebral Mechanisms*; reprinted in Z. W. Pylyshyn, ed., *Perspectives on the Computer Revolution* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1970), p. 105. The above characterization by von Neumann of the McCulloch-Pitts

The close connection between the work of McCulloch and Pitts and the new digital computing field became apparent as early as 1945. Significantly, the link occurred through Wiener and John von Neumann. McCulloch had met Wiener through their mutual friend Arturo Rosenblueth in 1940 or 1941.[68] Pitts, not quite 20, was sent to work with Wiener in the Fall of 1943. Wiener recalled that Pitts did not have very much experience of the "possibilities of electronics," and that

> 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.[69]

The connection between the neural nets and digital computers was so close that John von Neumann was able to use the McCulloch-Pitts paper in his design of the EDVAC computer in 1945.[70]

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work has been repeated by many others. It is disputed, however, by S. Papert, who says that a "better formulation is to say that they provide a definition of 'computing machine' that enables us to think of the brain as a 'machine' in a much more precise sense than we could before." See W. S. McCulloch, Embodiments, pp. xviii-xix.

[68] W. S. McCulloch, "Recollections," p. 12.

[69] N. Wiener, Cybernetics, p. 14. A few months before, work had started on the ENIAC, a binary digital machine. Wiener was undoubtedly familiar with this development.

[70] J. von Neumann, "First Draft of a Report on the EDVAC (1945)" in B. Randell, Origins, p. 360. Von Neumann almost certainly heard of the McCulloch-Pitts work through Wiener.

As we have seen, the combined result of the work of Turing and of McCulloch and Pitts was to show that the extremely sophisticated computational and logical abilities of humans could, in theory, be realized by totally automated processes. It should be noted, however, that this work did not involve negative feedback and is therefore not directly antecedent to cybernetic analysis. When Wiener presented Cybernetics in 1948, he was interested in almost all developments which had recently enhanced the robotic view and he included the results of Turing and of McCulloch and Pitts.[71] McCulloch would soon become one of the principal proponents of cybernetics through his chairmanship of the Josiah Macy Jr. Foundation meetings on cybernetics. This important role of McCulloch will be treated in the next chapter.

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[71]In their "Logical Calculus" paper of 1943, McCulloch and Pitts did employ closed loops or "circles," but with positive rather than negative feedback. McCulloch had been interested in the use of positive feedback to help provide a neurological theory of memory in the form of reverberating neural circuits. See W. S. McCulloch, "Recollections," pp. 10-11.

THE NEED FOR A TOOL TO ACCOUNT FOR SYSTEMS PROPERTIES

In the first two sections of this chapter it has been shown that there were many theoretical and technological developments which created a fertile environment for the presentation of the "Behavior, Purpose and Teleology" paper during the 1940's. But was there any perceived need for such ideas as were contained in the paper? In order to discuss this question we should recall that the 1943 paper provided a means of accounting for the property of an entire system; that of its regulation in the face of disturbances. There was, in fact, a substantial need in various areas of science and philosophy at the time for a tool to deal with the elucidation of systems properties. This need primarily stemmed from the desire for a holistic, or what is sometimes called an "organic," mode of explanation. Although holism is a recurrent theme through history,[72] only certain of its twentieth century manifestations will be treated here. The breadth of this twentieth century development was indicated by R. H. Wheeler, writing in 1936:

> The logical pattern underlying twentieth century science is strikingly uniform from physics through biology and psychology to social science. . . . The pattern is organismic in that the major interest in science as a whole lies in the part-whole relation. Accordingly, the contemporary movement is expressing itself in the following different ways: in physics, e.g., an emphasis upon systems, fields and "wholes," method, and the discovery (or rediscovery) that physical nature is organic. In chemistry we find a similar trend,

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[72] Raymond Holder Wheeler, "Organismic Logic in the History of Science," Phil. of Sci. 3 (1936), pp. 26-61.

> although not so marked, e.g., in the breakdown of elementarism (isotopes, radiation phenomena) and the like. In biology, the outburst of organismic fact and theory has been tremendous. In psychology we need mention only the Gestalt and culture psychology movements. In social science, historians, cultural anthropologists, political scientists, sociologists and economists are turning to instruments of a similar logical character---fields, wholes, patterns, gradients, transformations, conservations and other concepts of dynamics.[73]

Thus there was a widespread recognition of the necessity to more fully relate the functioning of the parts of a system to its overall behavior. The "Behavior, Purpose and Teleology" paper, with its proposal of the negative feedback mechanism to elucidate this relationship, may be viewed as one manifestation of the "organismic" movement described by Wheeler. While a number of workers during this century stressed the need to consider how the behavior of the total system could be accounted for in terms of the functioning of its individual parts, the role of negative feedback in this functioning was not identified. This observation applies to a number of investigators who were early advocates of the systems approach to science.[74] Included in this category would be J. H. Woodger, the biologist who emphasized the need to consider the behavior of the total organism,[75] Charles Scott Sherrington, who

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[73] Ibid., p. 26.

[74] For an overview of the growth of General Systems Theory see Ludwig von Bertalanffy, "The History and Status of General Systems Theory," in G. J. Klir, ed., Trends in General Systems Theory (New York: Wiley Interscience, 1972), pp. 21-41.

[75] Joseph Henry Woodger, Biological Principles (New York: Harcourt, Brace and Co., 1929), pp. 273-325.

elucidated the "integrative action of the nervous system,[76] Wolfgang Köhler and the gestalt school,[77] L. J. Henderson, who investigated factors leading to the "fitness of the environment,"[78] and Alfred North Whitehead, who expounded his "organismic philosophy."[79] In certain places behavior vaguely implying a negative feedback mechanism is described,[80] but the important point is that no explicit reference was made to the principle itself and the manner in which feedback, as used in engineering, might be adapted to explain behavior in these various fields. An important exception to this generalization, however, is contained in the systems-oriented work of Walter B. Cannon. Cannon's work will be described in some detail here because although he did not explicitly cite negative feedback as an organizing mechanism of behavior, he came very close to doing so. In addition, his description of the

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[76] Charles Scott Sherrington, The Integrative Action of the Nervous System (New Haven: Yale University Press, 1911).

[77] Wolfgang Köhler, Gestalt Psychology (New York: Horace Liveright, 1929), pp. 187-223.

[78] L. J. Henderson, The Fitness of the Environment (New York: The Macmillan Co., 1913).

[79] Whitehead's organic philosophy is developed in several of his works. See Ann L. Plamondon, Whitehead's Organic Philosophy of Science (Albany, New York: State University of New York Press, 1979), pp. 41-68.

[80] The "co-ordination" required by Sherrington to explain the "reflex arc" of the nervous system, for example, implies the presence of a negative feedback mechanism, but this implication, coming at a time when the conceptual understanding of negative feedback was quite limited, stimulated no great effort to understand the nervous system in terms of engineering techniques. See C. S. Sherrington, Integrative Action, p. 7.

biological condition of homeostasis was extremely influential in the spread of the negative feedback techniques and concepts to biology. We recall that homeostasis represented one of the first applications that Wiener saw for negative feedback.

Cannon, a physiologist at Harvard, first defined the term homeostasis in 1926,[81] but it became best known through his popular work, The Wisdom of the Body.[82] Cannon observed that in order for an organism to survive, various aspects of the organism's internal physical and chemical characteristics, such as the pH of the blood and the temperature of the body, had to be kept within certain tolerable limits.[83] He then went on to define homeostasis:

> The cöordinated physiological processes which maintain most of the steady states in the organism are so complex and so peculiar to living beings---involving, as they may, the brain and nerves, the heart, lungs, kidneys and spleen, all working cooperatively---that I have suggested a special designation for these states, homeostasis. The word does not imply something set and immobile, a stagnation. It means a condition---a condition which may vary, but which is relatively constant.[84]

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[81] W. B. Cannon, "Physiological Regulation of Normal States: Some Tentative Postulates Concerning Biological Homeostasis," in John F. Fulton and Leonard G. Wilson, eds., Selected Reading in the History of Physiology, 2nd ed. (Springfield, Illinois: Charles C. Thomas, 1966), pp. 329-332.

[82] W. B. Cannon, The Wisdom of the Body (New York: W. W. Norton & Co., 1932).

[83] Cannon credited Claude Bernard, the nineteenth century French physiologist, as originating this idea (W. B. Cannon, Wisdom, p. 38).

[84] Ibid., p. 24.

Cannon set out to explore the "various physiological arrangements which serve to restore the normal state when it has been disturbed."[85] Although Cannon did not specifically identify negative feedback as a mechanism for homeostatis behavior, he, in fact, referred to it implicitly. How close he came is perhaps best seen in his discussion of the stabilization of the "fluid matrix," the blood-lymph system in which most of the homeostatic properties inhere. Cannon stated:

> A noteworthy prime assurance against extensive shifts in the status of the fluid matrix is the provision of sensitive automatic indicators or sentinels, the function of which is to set corrective processes in motion at the very beginning of a disturbance. If water is needed, the mechanism of thirst warns us before any change in the blood has occurred, and we respond by drinking.[86]

In modern terms we see that Cannon has postulated an error detector in the form of "sensitive automatic indicators or sentinels," which causes a controller, the thirst reflex, in the example given, to activate the power device, the body, which ingests water. This system may clearly be understood as a closed loop with negative feedback. When the thirst is satisfied the reflex no longer sends signals.

Cannon also realized the need to understand how the disturbance is "signalled" to the corrective organs, but stated that there was at that time (1932) no forthcoming explanation. In

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[85]Ibid., p. 25.

[86]Ibid., p. 270.

his words:

> But what sends in the signal and how the signal sends orders to the organs which make the corrections, must remain a mystery until further physiological research has disclosed the facts.[87]

Finally, Cannon also saw that his homeostatic regulation principle would apply to any system requiring stability, living or not:

> It seems not impossible that the means employed by the more highly evolved animals for preserving uniform and stable their internal economy (i.e., for preserving homeostasis) may present some general principles for the establishment, regulation and control of steady states, that would be suggestive for other kinds of organization---even social and industrial---which suffer from distressing perturbations. Perhaps a comparative study would show that every complex organization must have more or less effective self-righting adjustments in order to prevent a check on its functions or a rapid disintegration of its parts when it is subjected to stress.[88]

We recall that Cannon was a friend of Wiener's father; that Wiener, as a child, visited Cannon in his laboratory, and that he remained in close touch with Cannon and his ideas through Arturo Rosenblueth, one of Cannon's chief collaborators at Harvard. It is not surprising, then, that Wiener immediately thought of applying the feedback suggestions of Bigelow to the subject of homeostatic properties.

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[87] Ibid., p. 271.

[88] Ibid., p. 25. With regard to Cannon's comments on distressing social and industrial "perturbations," it is useful to note that Cannon was writing in the midst of the depression of the 1930's.

SOME ANTECEDENTS OF CYBERNETIC ANALYSIS

In Chapter Two I attempted to clarify the role of negative feedback in scientific explanation in terms of a program I designated "cybernetic analysis." The "Behavior, Purpose and Teleology" paper played a key role in the derivation of this program. The 1943 paper and thus the program itself have several interesting antecedents which occurred in the "organismic" movement described above. Some of these works contained ideas similar to those expressed in the 1943 paper, but expressed from a different point of view, in different language, or more superficially. Cannon's Wisdom of the Body, which we have just examined, is an example of such an antecedent work. Cannon's and other works were not as powerful in elucidating the role of negative feedback as was the "Behavior, Purpose and Teleology" paper. Nevertheless, a discussion of certain other of these antecedents will help the reader to better appreciate the unique and insightful contributions of the later paper.

Antecedents in Mathematical Biology and Philosophy

As Bigelow noted in his conversations with Wiener, predator-prey relationships, which had been expressed mathematically in the nineteenth century, approximate, in a certain sense, the negative feedback mechanism. These "pursuit" curves anticipated a larger twentieth century movement to develop a mathematical approach to biology. Two of the most active exponents of mathematical biology during the first half of this century were Alfred J. Lotka and Nicholas Rashevsky. Rashevsky's book, Mathematical Biophysics, was published in 1938,[89] and in 1939 he initiated a journal by the same name, a journal in which McCulloch and Pitts published most of their influential early work.

Lotka is of particular interest here because in his work, Elements of Physical Biology,[90] he considered some of the same problems as were treated in "Behavior, Purpose and Teleology," and approached the solutions to these problems in a manner similar to the later paper, and yet, because negative feedback theory was in its infancy, Lotka could not make the detailed connections with modern feedback theory that Wiener and his colleagues later did.

Lotka wrote that in order for animals to survive in their environment they needed to correlate changes in the environment which might affect their well being with their own actions. A predator, for example, must adjust its motions so as to catch its prey rather than relying on random collisions. As an illustration of how this correlation process could be carried mechanically, he described "an ingenious toy" which was then available, a toy beetle, the construction and behavior of which he described as follows (see Figure 5-4):

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[89] Nicholas Rashevsky, Mathematical Biophysics (Chicago: University of Chicago Press, 1938).

[90] Alfred J. Lotka, Elements of Physical Biology (Baltimore: Williams & Williams Co., 1925; reprinted as Elements of Mathematical Biology (New York: Dover Publishing, Inc., 1956).

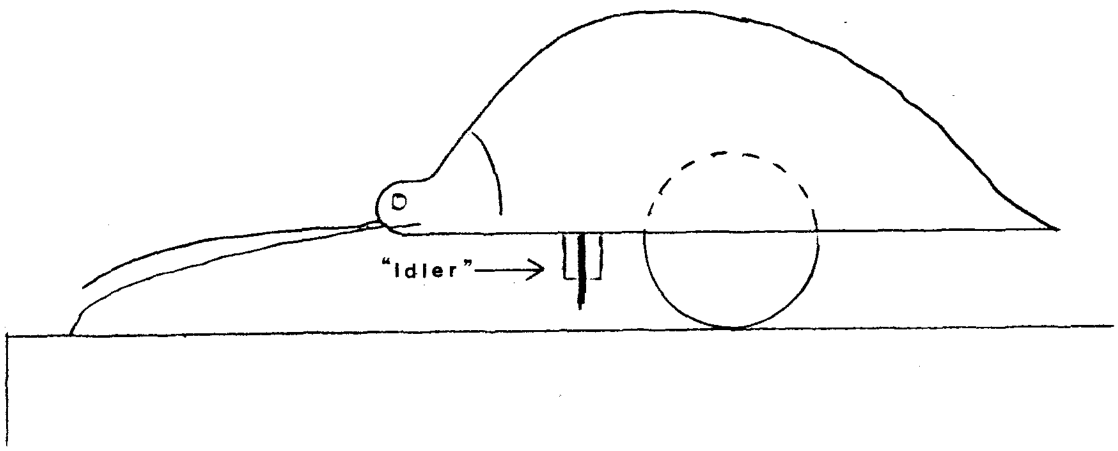

Figure 5-4. Lotka's "ingenious toy" (redrawn from Lotka, Elements, p. 341).

> The beetle 'walks' on two toothed wheels, of which one is an idler, while the other is rotated by a spring whose gradual release is ensured by a simple escapement device. At its forward end recokoning in the direction of motion (at the 'head') the toy is provided with a pair of antennae, of which one is a dummy, and rises clear of the table upon which the beetle is place to exhibit its talents. The other antenna is operative and is so bent downward as to glide along the table top, in contact with it. A little in advance of the propelling wheel is another smaller toothed wheel, running idle, and disposed transversely to the direction of the driving wheel. This transverse wheel clears the table without contact in the normal working position of the beetle. The animal, if placed somewhere near the center of the table, makes a straight track, apparently intent upon reaching the edge and seeking destruction in a species of mechanical suicide. But the moment the operative antenna clears the edge of the table, the body of the toy, till then held up by the contact of the antenna with the table surface, sinks down a fraction of an inch, and the transverse wheel now contacts with the table. In consequence the toy rotates until the running wheel is parallel with the table edge, and the insect continues its peregrinations with the operative antenna hugging the side of the table top.[91]

Lotka cited the behavior of this toy--what he called its "anticipatory correlation"--as evidence that "Mechanisms teleological in their operation can be constructed, which we would not in any ordinary sense of the word describe as conscious."[92] Lotka seems not to have been aware of the term "feedback," which was then just coming into use. The tremendous advanced in feedback theory associated with the World War II period had not yet occurred of course. Clearly, however, Lotka identified teleological behavior with what today would be called a primitive feedback

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[91] Ibid., p. 341.

[92] Ibid., pp. 381, 385.

device.[93]

The connection between what is now called feedback behavior and purpose had, in fact, been made before Lotka by writers in philosophy. One of these works, that by Ralph Barton Perry in 1917,[94] was, in certain respects, remarkably similar in content to "Behavior, Purpose and Teleology." Perry described the tendency of organisms to "maintain equilibrium," in the face of disturbances; that is, the organism had ". . . some mechanism of recovery which is released whenever the system rises above or falls below a certain zero point, and the effects of which are equal in quantity and opposite in sign to those of the disturbing agency."[95] Perry then continued "But [this] principle is apparently equally well illustrated by the operation of certain inorganic mechanisms." He cited the flyball governor and the thermostat as examples of such mechanisms and described their behavior in terms of "compensatory adjustment."[96] Perry argued against describing such behavior as purposive , however, on the grounds that in each case the behavior was stimulated by the necessity of physical law.[97]

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[93]The operative antenna of the beetle served both as an error detector and controller in terms of Figure 5-1.

[94]Ralph Barton Perry, "Purpose as Tendency and Adaptation," Phil. Rev. 26 (1917), pp. 477-495.

[95]Ibid., pp. 486-488.

[96]Ibid., p. 488.

[97]Ibid., pp. 494-495.

The work of Perry was similar to that propounded in the "behaviorist" movement of the 1930's, and primarily that aspect of the movement based in psychology. A number of investigators here teated purpose as behaviorally determinable and they stressed that when studied in this manner, purpose was freed of any vitalist connotations; that "the traditional conflict between mechanism and vitalism (causality and teleology) is removed by an experimental (as opposed to a spiritualistic or introspective) formulation of the meaning of 'purpose.'"[98] As noted in Chapter Two, the behavioral approach was critical for the "Behavior, Purpose and Teleology" paper, but the paper gained much of its strength from the fact that it not only considered the behavioral description of teleology, but provided a new mechanism to account for it, negative feedback. The later paper also made the important distinction between goal-directed behavior with and without feedback, a distinction which, of course, could not be made by these earlier writers who wrote before the concept was widely understood. Thus the 1943 paper was considerably more powerful in its analysis than these earlier works.

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[98] C. W. Churchman and R. L. Ackoff, "Purposive Behavior and Cybernetics," Social Forces 29 (1950), p. 32. Wiener was well aware of the roots of this movement. He had taken classes under Perry while a graduate student at Harvard (see Exp, pp. 165; 225-226).

J. O. Wisdom's "Basic Cybernetic Hypothesis" and W. R. Ashby's "Adaptive Equilibrium"

J. O. Wisdom, in 1952, in an influential article,[99] described the "basic cybernetic hypothesis" as follows:

> The basic hypothesis of cybernetics is that the chief mechanism of the central nervous system is one of negative feedback. The field of study is not, however, restricted to feedbacks of the negative kind. Secondly, cybernetics makes the hypothesis that the negative feedback mechanism explains 'purposive' and 'adaptive' behavior. Broadly speaking, what the cybernetic model does for our outlook is to make us understand how purposive behavior can be manifested by a machine, for 'purposive' can now be defined in terms of negative feedback. . . . Support for this striking hypothesis, which is of fundamental importance, is to be found both in rather general resemblances between organisms and electronic machines and in resemblances of more special kinds.[100]

Wisdom then traces the origins of the cybernetic hypothesis:

> The hypothesis seems to be due mainly to Ashby and Wiener, though others had written on some of the resemblances. Ashby introduced it in a paper in 1940. . . . and Wiener, who had the idea for some time, put it forward independently in a joint paper in 1943. . . .[101]

The "joint paper" was, of course, "Behavior, Purpose and Teleology," while the "Ashby" cited was W. R. Ashby, an English physician who later became active in the cybernetics field. The 1940 paper cited by Wisdom was entitled, "Adaptiveness and

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[99] J. O. Wisdom, "The Hypothesis of Cybernetics," Brit. J. Phil. Sci. 2 (1951-2), pp. 1-24; reprinted in Zenon W. Pylyshyn, ed., Perspectives on the Computer Revolution (Englewood Cliffs, New Jersey: Prentice-Hall, 1970), pp. 128-154.

[100] Ibid., p. 131.

[101] Ibid.

and Equilibrium."[102] This paper had none of the power of "Behavior, Purpose and Teleology" in terms of the analysis of mechanistic concepts of "purposeful" behavior; and it had almost no influence historically in the dissemination of negative feedback. It will be instructive to analyze Ashby's paper, however, because such an analysis will help to point out some of the subtleties of the "Behavior, Purpose and Teleology" paper, as described in Chapter Two, subtleties which did not appear in Ashby's work.

Ashby noted at the start of his paper that the "vague" concept of "adaptiveness" is one of the essential features of life. This concept is difficult to define, Ashby continues, not only because of its vagueness, but because different observers will disagree as to when an organism is exhibiting adaptive behavior. Ashby suggests that a more rigorously defined concept from physics be substituted for "adaptiveness":

> It would clearly be better if this concept could be changed for another which would be equivalent to it as far as its essential features are concerned but which would be free from these objections.
>
> It is suggested in this paper that the concept of 'stable equilibrium' may perhaps be equivalent to it.[103]

The concept of stable equilibrium is drawn from physics. Ashby illustrates it by describing how a cube, if tilted, returns to its original position, as does a pendulum after it is disturbed.

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[102] W. R. Ashby, "Adaptiveness and Equilibrium," <u>J. Mental Sci.</u> 86 (1940), pp. 478-483.

[103] Ibid., p. 478.

Ashby then continues:

> We may now define stable equilibrium more precisely: a variable is in stable equilibrium if, when it is disturbed, reactive forces are set up which act back on the variable so as to oppose the initial disturbance. If they go with it then the variable is in unstable equilibrium.
>
> Since the reactive forces are set up by the change in the variable and then come back to the variable to affect it, we are clearly dealing with a functional circuit.[104]

The "functional circuit" which Ashby subsequently describes in more detail may be related to the negative feedback mechanism, but the critical concepts of error measurement and detection are lacking. Further, the notion of stable equilibrium is difficult to reconcile with a dynamic goal state, as, for example, was the case in the fire-control problem. Here, the target, and hence the "goal" of the system was not stable. One of the great conceptual and practical advantages of the negative feedback method is that it is able to treat both dynamic and stable, or "static" goals.

Perhaps the most serious problem with Ashby's paper is his association of adaptive behavior with phenomena such as the pendulum. As the reader will recall from Chapter Two (see above, pp. 53-56), the behavior of phenomena such as the pendulum was precisely that which was described as non-adaptive. The pendulum could not return to its equilibrium position while it was being disturbed; it could not adapt. Ashby's association of adaptiveness with equilibrium phenomena failed to take into account the crucial distinction between adaptive and non-adaptive goal-directed

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[104] Ibid., p. 479-480.

behavior, a distinction which was clearly made in the joint paper,[105] and which is central to the role of negative feedback in scientific explanation.

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[105] This distinction was made in terms of "purposeful" and "teleological" behavior in the 1943 paper.

SUMMARY

The concurrent rapid advances in feedback theory, servomechanism development, computing science and technology, and the theoretical bases for artificial intelligence did much to increase the plausibility of the robotic world view; machines became more sophisticated in their behavior while at the same time certain aspects of human behavior became more understandable in mechanical terms. This heightened interest in the robotic view would aid the reception of the "Behavior, Purpose and Teleology" paper, as would the increased interest in the mechanistic explanation of coordinated behavior. Thus the middle and late 1940's represented an ideal time for the dissemination of the negative feedback ideas, but as several antecedent cases demonstrate, not only the timing, but the manner in which the ideas were presented was critical. The following chapter will explore the principle events relating to this initial dissemination, as well as the appearance of Wiener's Cybernetics, which played a great role in this process.

CHAPTER SIX

THE ASSIMILATION OF NEGATIVE FEEDBACK BY THE LARGER SCIENTIFIC COMMUNITY (1942-1948)

One of the principal reasons for the successful assimilation of the "Behavior, Purpose and Teleology"[1] paper was Wiener's ability to personally stimulate interest in it. We have seen that the time was ripe for the assimilation of the paper, but the fact that Wiener, with his considerable prestige and forceful intellect was able to spread his ideas to a wide group of influential scientists was invaluable for accelerating the rate at which his ideas were incorporated within the scientific community.

The process by which negative feedback became assimilated by the larger scientific community was marked by two distinct periods: that before the publication of Cybernetics in 1948 and that after. It is convenient to date the first period from 1942 when the ideas for the "Behavior, Purpose and Teleology" paper were generated, although the greatest number of events in this period occurred between 1945 and 1948. The end of the second period is not so easy to determine. It

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[1]Arturo Rosenblueth, Norbert Wiener and Julian Bigelow, "Behavior, Purpose and Teleology," Phil. of Sci. 10 (1943), pp. 18-24.

[2]N. Wiener, Cybernetics, Or Control and Communication in the Animal and the Machine (New York and Paris: John Wiley & Sons, Inc. and Hermann et Cie, 1948); 2nd ed. (New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961). Throughout this work I refer in pagination to the more widely available second edition which contained a corrected reprinting of the first edition and two added chapters. The corrections were mostly typographical and have no bearing on the material presented here.

might well be said that this period is still continuing.

Only the first period will be treated in this work. The second period, with its enormous number of pathways, including the absorption of the feedback methods by general systems theory, and the many utilizations of negative feedback within various disciplines, really requires a separate study. The early pattern of assimilation had a great deal to do with the later one, however. During the first period small groups of scientists from widely diverse fields met informally to exchange ideas. Many of the problems and challenges facing the early small group simply became more widespread after Cybernetics became a best-seller. A detailed study of the first period, then, should provide a basis for understanding the subsequent problems involved with the later assimilation of negative feedback.

The Princeton Meeting: January 1945

The number of rapid developments of the Second World War which were described in the last chapter were overwhelming to those trying to absorb them all. It was apparent to many that there might well be *some* common basis underlying the developments in servo-mechanisms, computers and neurological models, but what this common basis might be was perceived quite differently by different participants. Not surprisingly, the initial discussions and presentations were frequently ill-defined. Wiener's *Cybernetics* was, in fact, a product of this environment, and, at the same time, helped to perpetuate it.

It is important to recall that from 1942-1945 World War II was still in progress. This fact is important, not only to help place events in their proper context, but also because the war was a direct stimulus for the active professional integration of scientists involved in the many war-related projects. Even given the restrictions of wartime security, the war compelled a cooperative effort among workers in many different areas of technology and theory which was unprecedented.[3] In the computer field, for instance, there was a "continual coming and going," as Wiener put it, of those involved at the various research centers.[4] Wiener was part of this computer group, and in the process of "coming and going," communicated his ideas stemming from the "Behavior, Purpose and Teleology" paper to, among others, John von Neumann of the ENIAC project and Howard Aiken, of the Harvard computer project. As Wiener later stated, "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."[5] Wiener was stressing the view that if the principles by which the body carried out its extremely sophisticated behavior could be understood and translated into "hardware," the engineering discipline would benefit immensely. Conversely, as he had proposed with Rosenblueth and Bigelow,

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[3]The development of systems management techniques in the form of "operations research" in England during World War II is evidence of the greatly increased complexity of wartime projects; see Stafford Beer, Decision and Control: The Meaning of Operations Research and Management (New York: John Wiley & Sons, 1966), p.

[4]N. Wiener, Cybernetics, p. 15.

[5]Ibid.

engineering principles, such as feedback, had much to offer for those interested in the workings of the body. The result of the informal exchange of these views was a meeting held at Princeton January 6-7, 1945. As Wiener later recalled, "At this stage of proceedings, Dr. von Neumann and myself felt it desirable to hold a joing meeting of all those interested in what we now call cybernetics. . . . Engineers, physiologists, and mathematicians were all represented."[6]

Among those present at the Princeton meeting were Pitts and McCulloch. McCulloch later recalled that upon meeting Wiener four years earlier he had been "amazed at Norbert's exact knowledge, pointed questions and clear thinking in neurophysiology." He continued that Wiener "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."[7] Wiener had enthusiastically developed the relationship, arranging for McCulloch's major collaborator, the young Pitts, to work with him at the Massachusetts Institute of Technology starting

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[6]Ibid. Wiener went on to state that "this meeting took place at Princeton in the late winter of 1943-1944." This date is in error. Two separate letters in the Wiener Papers give the date of this meeting as January 6-7, 1945. They were Wiener to H. Goldstine, December 28, 1944 and J. von Neumann to Messrs. Howard Aiken, Leland E. Cunningham, W. E. Deming, H. H. Goldstine, R. Lorente de No, W. S. McCulloch, Walter H. Pitts, E. H. Vestine, Norbert Wiener and S. S. Wilks, January 12, 1945. The latter reference is von Neumann's summary memorandum of the meeting. It seems that all those addressed were present except Aiken, according to Wiener, in Cybernetics (p. 15).

[7]W. S. McCulloch, "Recollections of the Many Sources of Cybernetics," ASC Forum 6 (1974), p. 12.

in 1943. The influence of McCulloch and Pitts on Wiener is apparent in the fact that by November 1944, two months before the Princeton meeting, Wiener was privately suggesting that all the functions of the brain might be duplicated in electrical systems.[8] The neurophysiological views of Pitts and McCulloch were highly unusual for their time, but ideally suited to reinforce the ideas Wiener was attempting to develop. The early and continued intense involvement of Pitts and McCulloch was extremely fortunate for Wiener. At the same time this involvement may be seen as a direct result of Wiener's eclectic and far-ranging interests which were detailed in the second and third chapters of this work.

The scheduled agenda for the Princeton meeting was indicated in a letter written by Wiener for himself and the other two organizers, von Neumann and Aiken. Here we see the first statement of the theme which would become Cybernetics in 1948:

> A group of people interested in communication engineering, the engineering of computing machines, the engineering of control devices, the mathematics of time series in statistics, and the communication and control aspects of the nervous system, has come to a tentative conclusion that the relations between these fields of research have develped to a degree of intimacy that makes a get-together meeting between people interested in them highly desirable.
>
> Owing to the war, it is not yet the time to call together a completely open meeting on the matter, because so many researches developed in the war effort are concerned, but it seems highly desirable to summon together a small group of those

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[8]Edwin Boring of the Psychology Department at Harvard wrote to Wiener in November of 1944, "Your suggestion that all of the functions of the brain might be duplicated in electrical systems is one that I find attractive." Edwin Boring to Wiener, November 13, 1944, Wiener Papers.

> interested to discuss questions of common interest and make plans for the future development of this field of effort, which as yet is not even named.[9]

Four lears later Wiener would publish Cybernetics and thus "name" the field. The "Communication and control aspects of the nervous system," mentioned in this letter presaged Cybernetics' sub-title: "Control and Communication in the Animal and the Machine."

The considerable extent to which the earlier "Behavior, Purpose and Teleology" paper was still influencing Wiener can be seen in a letter of invitation sent to H. H. Goldstine:

> Professor Wiener and Commander Aiken make the tentative suggestion that the group be known as the Teleological Society. Teleology is the study of purpose of conduct, and it seems that a large part of our interests are devoted on the one hand to the study of how purpose is realized in human and animal conduct and on the other hand how purpose can be imitated by mechanical and electrical means. If this suggestion should meet the approval of the group and if at any stage it appears desirable for us to sponsor a periodical, then an appropriate name might be Teleology or we are contemplating an international Teleologia. This suggestion is made without any desire to force the hands of the group and simply to give the members something to think about.[10]

The study of how "purpose is realized in human and animal conduct and on the other hand how purpose can be imitated by electrical and mechanical means," was one of the major implications to stem from the "Behavior, Purpose and Teleology" paper, where the principle of negative feedback was postulated to account for such purpose. The

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[9] N. Wiener to E. H. Vestine, December 4, 1944, Wiener Papers.

[10] H. Aiken and J. von Neumann and N. Wiener to H. Goldstine, December 28, 1944, Wiener Papers. The secretary's markings on this letter indicate that it was typed for Wiener.

fact that the proposed journal name was to have been Teleology indicates the extent to which Wiener valued the purposive aspect of his thesis. Wiener and the organizers viewed the new ideas as having tremendous potential, as is indicated in the continuation of the letter:

> In addition to the name of the science, possible publication of the Journal, and similar matters, possible agenda include the questions of what steps should be taken to form a center for this sort of research and to approach various foundations for its support; what policy should be adopted about patents and inventions and other social relations of the group; what measures should be taken to bring our ideas to general scientific attention either at a meeting of the American Association for the Advancement of Science or elsewhere as may seem suitable; and how to protect the researches of the group from dangerous and sensational publicity. . . .[11]

Wiener described some of the events of the meeting in a letter to Rosenblueth who was then in Mexico and unable to attend. Here Wiener again emphasized the common basis of engineering and animal behavior:

> We held a meeting two weeks ago and it was a great success . . . . The first day von Neumann spoke on computing machines and I spoke on communication engineering. The second day Lorente de No and McCulloch joined forces for a very convincing presentation of the present status of the problem of the organization of the brain. In the end we were all convinced that the subject embracing both the engineering and neurology aspects is essentially one, and we should go ahead with plans to embody these ideas in a permanent program of research.[12]

Von Neumann's summarizing memorandum stated the research program would be initiated by dividing the group into four sub-units under the headings of: 1. filtering and prediction problems;

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[11]Ibid.

[12]N. Wiener to A. Rosenblueth, January 24, 1945, Wiener Papers.

2. application of fast mechanized computing methods to statistical problems; 3. application to differential equations; 4. connected aspects of neurology.[13] Significantly, the members of the group were assigned problems that kept them in fields in which they had already been working. Contrary to Wiener's letter, there is no indication at all of a consensus on the common subject matter. Indeed, when one reads von Neumann's summary, there is no indication whatsoever that the various members were working for <u>any</u> common basis. He did say at the end of the memorandum, "You will notice that I am not giving in this letter a description of the subject in which we are interested."[14] His reason was that the subject had already been covered in the letter of invitation sent to the members. But the fact was the research program outlined had no explicit relation to the synthesis described in the invitation letter. The reason for von Neumann's and Wiener's differing perception of the results of the meeting lay primarily in their different personalities. Wiener was much more interested in synthesis and philosophical investigation than was von Neumann.[15] In the case of the Princeton meeting it was apparent that Wiener and von Neumann were present for fundamentally different reasons. While Wiener was interested in purusing the general analogy

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[14]Ibid.

[15]For a discussion of the differences in scientific "style" and habits of work of von Neumann and Wiener, see Steve J. Heims, <u>John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death</u> (Cambridge, Massachusetts: The M.I.T. Press, 1980), pp. 116-140.

between man and machine, von Neumann was searching for ways to design a better computer.[16] Thus it is not surprising that in his summary memorandum of the meeting von Neumann placed almost no emphasis on the synthetic aspects which were so important to Wiener. Wiener immediately reacted to this omission, writing back to von Neumann a few days later,

> . . . I found one thing missing in your assignment of topics: namely, there was no single place where the problem of transition from the computing machine to the control machine was discussed. I think this is one of the most important aspects of our project and as it is closely related to the prediction and filtering problems, I have assumed that it goes to our subcommittee for a report.[17]

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[16] Ibid., pp. 186-187.

[17] N. Wiener to J. von Neumann, January 24, 1945, Wiener Papers. This letter also contains evidence that Wiener was then developing some of the ideas which would later be included in Cybernetics. He wrote:

> "The issues that come up here are those of transfer from continued data to counted data; of the final transition from counted data to the motion of a shaft effector; and the sensing of the motion of the effector by feed-back or other quasi-proprioceptor apparatus. I am quite convinced that feed-back method of proprioceptor needs to be supplemented by apparatus which rather reads the load than merely works with a linear computation of the input and output of the motor apparatus. I have already gone some way towards developing such a theory.
>
> I have also taken the liberty of emphasizing that the same type of proprioceptor arises in purely mechanical controls, purely organic controls, and controls with a mechanical and organic part combined together. However, you will see this in our report."

It is not clear what "report" Wiener is referring to here. He was working with Pitts on their assigned area of filtering and prediction problems. I have found no report of Wiener and Pitts which matches the description above.

Von Neumann's response to Wiener is interesting in that the reason he gives for his omission of the "transition" topic is the very reason why Wiener considered the topic important. Wiener thought that because the transition was not identified in a single place the report was greatly lacking. But von Neumann replied that because the topic arose in more than one place there was no need to treat it separately. Von Neumann wrote:

> I agree with you completely about your emphasis on control mechanisms. I omitted to mention it because I thought it might come up in connection with several (more than one) among the assigned topics, but this may have been a mistake.[18]

The two scientists could hardly have had a greater misunderstanding on this point. This interchange again illustrates the tremendous differences in the outlook between Wiener and von Neumann.

For various reasons the meeting which was to have followed the one at Princeton was never held. It is not clear that the "reports" were ever prepared. In addition, it was apparent that World War II was ending. As a result, the need to hold another gathering under tight security was removed and plans were made for a much larger and more open meeting, to be held under different auspices that spring. As many more people would be attending it would be necessary to, in effect, start over again.

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[18] J. von Neumann to N. Wiener, February 1, 1945, Wiener Papers.

Conferences Sponsored by the Josiah Macy, Jr. Foundation and the New York Academy of Sciences

The new meeting was sponsored by the Josiah Macy, Jr. Foundation and seems to have been largely initiated by McCulloch. The Macy Foundation had become involved three years previously, in 1942, when it had sponsored a conference on "Cerebral Inhibition" which was attended by Rosenblueth, McCulloch, Margaret Mead, Gregory Bateson, L. Kubie and others.[19] At this conference Rosenblueth presented the substance of the "Behavior, Purpose and Teleology" paper which, in 1942, had been written but not yet published.[20] The representative of the Macy Foundation, Frank Fremont-Smith, was especially taken with Rosenblueth's exposition of the relationship between feedback and purpose.[21] Thus, three years later, when McCulloch approached the Foundation concerning the possibility of sponsoring a meeting devoted to the relationship between feedback and purpose, he got a ready reception. McCulloch was appointed chairman, and the first

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[19] Heinz von Foerster, Margaret Mead, and H. L. Teuber, eds., Conferences on Cybernetics, Transactions of the Ninth Conference, March 20-21, 1953, New York, New York (New York: Josiah Macy, Jr. Foundation, 1953), p. xviii.

[20] W. S. McCulloch, "Recollections," p. 12.

[21] McCulloch wrote to Rosenblueth,

> "You know, of course, that it was your remark some years ago at a Macy gathering that implanted the feedback explanation of purposive acts in Frank's mind. I have done what I could to nurture that seed. Today it grows like a weed and gets cross-fertilized."

W. S. McCulloch to A. Rosenblueth, February 14, 1946, Warren S. McCulloch Papers, American Philosophical Society, Philadelphia, Pa.

"Macy Meeting" was held in the Spring of 1946 in New York City. It would be the first of ten such meetings, the last being held in 1953.[21a] The early Macy meetings and an associated conference sponsored by the New York Academy of Sciences, were the primary vehicle for the initial assimilation of the feedback methods by the larger scientific community.

It will be useful to examine these early meetings in some detail for several reasons. It was during these early meetings that:

1. The initial discussion of the uses of negative feedback was diffused into a larger group of topics; a pattern which would persist long after these discussions.

2. Wiener developed the outline of what he would present in Cybernetics.

3. Wiener became greatly concerned about the social responsibilities of scientists, a concern which strongly affected his later presentation of cybernetics.

In examining these early meetings, then, it will be necessary not only to see how negative feedback was discussed, but the context in which it was discussed, because this context greatly affected its assimilation. The following is a list of the early meetings, their dates, and original titles:

[21a]Transactions were published only for the last five conferences; see Heinz von Foerster, Margaret Mead, and H. L. Teuber, eds., Conferences on Cybernetics, Transactions of the Sixth Through the Tenth Conferences, 1949-1953 (New York: Josiah Macy, Jr. Foundation, 1951-1955).

Sponsor and Meeting Designation	Date	Title
Macy I	March 8, 9, 1946	Feedback Mechanisms and Circular Causal Systems in Biological and Social Systems
Macy II (S)	September 20, 1946	Conference on Teleological Mechanisms in Society
Macy II	October 17, 18, 1946	Conference on Teleological Mechanisms and Circular Causal Systems
New York Academy of Sciences (NYAS)	October 21, 22, 1946	Conference on Teleological Mechanisms
Macy III	March 13, 14, 1947	Teleological Mechanisms and Circular Causal Systems
Macy IV	October 23, 24, 1947	Circular Causal and Feedback Mechanisms in Biological and Social Systems
Macy V	March 18, 19, 1948	Circular Causal and Feedback Mechanisms in Biological and Social Systems [22]

[22]The dates of the above meetings have been taken from: Macy I and Macy II (S), G. Bateson for the Josiah Macy, Jr. Foundation to N. Wiener, June 19, 1946, Wiener Papers; Macy II and Macy III, W. S. McCulloch, "To the Members of the Fourth Conference on Teleological Mechanisms: An Account of the First Three Conferences," (unpublished memorandum, no date): New York Academy of Sciences, L. K. Frank, G. E. Hutchinson, W. K. Livingston, W. S. McCulloch and N. Wiener, "Teleological Mechanisms," Annals of the New York Academy of Sciences 50 Art. 4 (1948), p. 187; Macy IV and V, H. von Foerster, "Circular Causality: The Beginnings of an Epistemology of Responsibility" (personal communication).

I wish to thank Dr. von Foerster for making the latter two items available to me. His article on "Circular Causality" is scheduled for publication in R. McCulloch and N. Lindgrin, eds., How Do Brains Work (Cambridge, Massachusetts: The M.I.T. Press, n.d.).

Other useful works on the meeting include: H. von Foerster, The Josiah Macy, Jr. Foundation had a strong interest in promoting multidisciplinary attacks on problems, especially medical problems.[23] Frank Fremont-Smith, the Foundation's medical director at the time, described the fundamental aim of all the conference groups as being "the promotion of meaningful communication between scientific disciplines."[24] Recalling Wiener's observation (see above, p. 110) that he and Rosenblueth had long held that the most fruitful areas for scientific development lay in the "no-man's land between the established fields," it would seem that the Macy Foundation format provided an ideal forum for Wiener. This observation is problematic, as we shall see.

There were an extraordinary number of disciplines represented at the meetings sponsored by the Macy Foundation. The continuing members and the invited guests generally numbered about twenty-five. Some of the continuing members, besides Wiener, McCulloch, Bigelow, von Neumann, Pitts, and Rosenblueth, were, in anthropology, Gregory Bateson and Margaret Mead, in zoology, G. Evelyn Hutchinson, in philosophy, F. S. C. Northrop, in sociology, Paul Lararsfeld, in

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M. Mead and H. L. Teuber, "A Note by the Editors," in H. von Foerster et al, eds., Transactions, ninth conference, pp. xi-xx; W. S. McCulloch, "Summary of the Points of Agreement reached in the Previous Nine Conferences," in H. von Foerster et al, eds., Transactions, tenth conference, pp. 69-80; Steve J. Heims, "Encounter of Behavioral Sciences with New Machine-Organism Analogies in the 1940's," Jour. of the History of the Behavioral Sciences 11 (1975), pp. 368-373; and Steve J. Heims, John von Neumann and Norbert Wiener, pp. 203-207.

[23]The Josiah Macy, Jr. Foundation, Review of Activities, 1930-1955 (New York: Josiah Macy Jr. Foundation, 1955), pp. 20-25.

[24]Ibid.

psychology, Heinrich Klüver, and in neurology, Rafael Lorente de No and Gerhardt von Bonin. The conferences generally were held for two days, with meetings taking place in the morning, afternoon and evening.

It seems beyond doubt that the contents of the "Behavior, Purpose and Teleology" paper directly stimulated these meetings. "Teleology" had not been a frequently-used term in the sciences. Its inclusion in the title of four of the conferences was a direct reference to the 1943 paper. As McCulloch later stated:

> Our meeting began chiefly because Norbert Wiener and his friends in mathematics, communication engineering, and physiology, had shown the applicability of the notions of inverse feedback to all problems of regulation, homeostasis, and goal-directed activity from steam engines to human societies.[25]

Additional indication of the influence of the joint paper was given by the chairman of the New York Academy of Sciences Conference on Teleological Mechanisms, L. J. Frank, who stated, "it was the paper on Behavior, Purpose and Teleology, by Rosenblueth, Wiener, and Bigelow which largely initiated these discussions of teleological mechanisms."[26] We further recall that it was this joint paper which stimulated the Foundation's medical director, Fremont-Smith to take an active interest in the subject.

Given this information one might expect that detailed discussions of feedback mechanisms ensued; that the subject of how

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[25]W. S. McCulloch, "Summary of Nine Conferences," p. 70.

[26]L. K. Frank, "Foreword," in L. K. Frank, "Teleological Mechanisms," p. 196.

teleology could be accounted for in the mechanistic terms of feedback might be the dominant theme of the meetings. Such themes, however, did not characterize the meetings. The reasons were complex, but can be principally stated as follows:

First, the understanding of feedback in a technical sense was just becoming widely established among engineers during the 1940's. The large majority of the conference members had little or no acquaintance with this technical concept and received only the barest education in it during the meetings.

Second, there was a great amount of confusion concerning what actually constituted the subject matter of the conferences. Generally speaking, any topic under the heading of what, in this work, has been described as the "robotic view," was discussed. This confusion was caused not only by the lack of familiarity with feedback on the part of most of the conferees, but also by the fact that Wiener was at the time attempting and advocating a much larger synthesis, the synthesis he would later attempt in _Cybernetics_. Thus, when Wiener, as an intellectual leader of the conference, brought up a much larger range of topics the discussions followed suit. Much the same effects were caused by von Neumann who was principally interested in computers, and McCulloch, who was principally interested in the nervous system.

Wiener's Role in the Early Feedback Meetings

What were Wiener's new ideas which he was attempting to meld into a larger synthesis? Wiener was starting to develop those ideas in connection with the Princeton meeting of 1945 where he emphasized the common type of analysis which might be used to account for teleological behavior in organisms and machines. Certain aspects of Wiener's thought during the period of the early meetings can be seen in letters Wiener wrote in the period immediately following the Princeton meeting. When a college freshman wrote to Wiener asking him to advise him concerning the most promising scientific areas for study, Wiener replied, in part,

> . . . I do happen to have in view certain fields which are obviously going to be important in the near future and which I have an interest in developing. One of these is the region in which physiology and mathematics come together. In particular, both in the nervous system and in such muscular systems as the heart, we possess at present a great deal of information as to the mode of interaction of individual fibers where they make contact with one another. We possess, however, far too little an acquaintance with the way in which these elementary actions pile up into an organized behavior.[27]

Wiener went on to say that the future study of biology would require knowledge of physics, mathematics, biochemistry and electronic instrumentation. He added that "Closely related to the problem of the analysis of organization in living tissue is a problem of the

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[27]N. Wiener to Lawrence Weller, July 26, 1945, Wiener Papers (emphasis added). Weller, a college freshman, had written Wiener a letter of a few lines requesting advice on what subjects to study. Weller, whom Wiener did not know at all, was given a very lengthy response by Wiener.

synthesis in organization in such devices as computing and control machines." Wiener also foresaw "a revision of statistical theory in which the procession of events in time is fully considered."[28]

A few months after this response, Wiener further outlined his thinking and interests to his young colleague, Giorgio de Santillana. This letter indicates the extraordinary range of topics with which Wiener was then involved.

> . . . You know my stock in trade but as you want it stated by me I shall rehearse it to you again. First I have a complete theory of wave filters and predictors for the linear case. These are important elements in control apparatus and in the economic theory of time series.
>
> Second I have a less complete but well outlined theory on non-linear prediction on an ergodic basis. This has not yet been employed in practice, but it is perfectly capable of such employment in theory; and in conjunction with Walter and von Neumann, I have very definite ideas as to the relation between computing machines and the brain, mainly that both are in essence switching devices where the conjunction of the particular complexities of open and closed channels offer new channels to be open or closed as the case may be.
>
> Third in conjunction with feedback and prediction apparatus they constitute an adequate central part for automatic control devices such as automatic assembly lines, automatic control of chemical plants, etc.
>
> Fourth since your departure Walter and I have made substantial progress in the theory of random nets of switching devices and find that we are really working in very essential parts of the theory of state of liquids and gases.
>
> Fifth the prediction and filter theory which I have developed has led us to see that there are gaps in the present state of quantum theory. . . .
>
> Sixth I have worked as you know with Rosenblueth in the study of the applications of the theory of networks of sensitive tissues to the flutter and fibrillation of the heart.[29]

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[28]Ibid.

[29]N. Wiener to G. de Santillana, October 16, 1945, Wiener Papers. "Walter," is Walter Pitts.

A comparison of Wiener's Cybernetics, written some fifteen months after the first Macy meeting, with his statements in these two letters and also his statements made in connection with the Princeton meeting reveals many, if not most, of the same ideas in the earlier and the later material. Thus, by 1945 it seems Wiener had in hand the principal ideas of his attempted cybernetic synthesis as published in 1948. There is evidence that Wiener personally presented many of these same topics and ideas at the early Macy meetings. These observations are important because they show that at the early meetings Wiener was promulgating a much larger set of ideas than those contained in the "Behavior, Purpose and Teleology" paper. The fact that Wiener was actively advocating this larger set of ideas at the early meetings had a large influence on the manner in which negative feedback was discussed at them. The conferees may very well have believed they were meeting to discuss the ideas of the 1943 paper, but Wiener, in fact, had moved on to a larger set of ideas. It will be useful then, in order to better understand Wiener's influence on the early meetings, to present, if only in a very brief manner, some of the ideas which Wiener advocated in his cybernetic synthesis as published in 1948[30] These ideas were almost certainly very similar to those he gave at the early Macy meetings just prior to his writing of Cybernetics.

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[30]The outline of Wiener's ideas presented here stems from his paper, "Time, Communication and the Nervous System," in L. K. Frank et al, "Teleological Mechanisms," pp. 197-220, and Cybernetics.

The subtitle of Wiener's synthesis as he published it in 1948 was "Control and Communication in the Animal and the Machine." Wiener was stating that there were general theories of control and communication which could be applied equally to animals and machines. Since control and communication were defined as broadly as possible, most animal and machine activities were encompassed by the two terms. Significantly, that behavior of organisms which is sometimes viewed as purposeful, such as the grasping action, was included. The scope of the attempted synthesis now becomes apparent. It requires generating not only perfectly general theories of control and communication which could apply equally to machines and organisms, but also demonstrating that these theories can be utilized to explain almost all animal and machine behavior.

Wiener's base for the understanding of communication lay in his previous work in the extrapolation and smoothing of time series. To communicate is to send messages which carry information, a process that is forever being interfered with by the increase of entropy in accord with the second law of thermodynamics. As a result, the filtering of noise is required, a process that can be carried out by the Wiener time series methods. A theory of information transmission is required to understand how messages can best be sent.

In Wiener's view, an examination of animals and machines reveals different physical systems for the transmission of information, but the principles, in most cases, are the same. The systems rely on relatively simple binary elements. The on-off character of relays in a telephone switching network is very similar to the on-off character of the neuron with regard to the transmission of impulses. The digital computer and the nervous system are seen to operate on similar principles.

The transmission of information, in Wiener's view, allowed control. Early automata had no "receptors," they could not react to signals of information that indicated changes in the environment. On the other hand, the modern automaton is neither blind nor deaf. Various kinds of mechanical sensors ranging from strain gauges to microphones allow the sensing of changes in the environment and the creation of information concerning these changes to be processed by the automaton. Further, statistical regularities in the time series of events allow the prediction of changes in the environment through the Wiener time series methods, thus allowing extrapolation of future appropriate behavior by the automaton. The actual performance of the automaton can be improved by monitoring the results of its performance, that is, through negative feedback. These notions of communication and control apply equally to machines and animals, and to complex systems of them as well.

The concept of "information" was very important in Wiener's presentation. He and Claude Shannon of Bell Laboratories had been developing what soon became known as "informational theory."[31] This

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[31] Wiener gave a short definition of information ("negative of entropy") in his Teleological Mechanisms Conference paper; see L. K. Frank et al, "Teleological Mechanisms," p. 203. Chapter Three of Cybernetics gave a more complete presentation of information theory. It was also in 1948 that Claude Shannon's "A Mathematical Theory of Communication," appeared in the Bell System Tech. J. 27 (1948), pp. 379-423, 623-656. The joint appearance of these works initiated the modern development of information theory.

discipline dealt with problems such as the most efficient use of information channels. In attempting to extend engineering concepts such as feedback and information to areas outside the usual domain of engineering, what was crucial was that these concepts, which were perceived as possibly having great general utility in all areas, were now underlain by solid, quantitative theories. There was very little practical application for information theory at the time. But the fact that information had now been identified and quantified was extremely important. The transmission of information was seen as the glue of organization, whether that organization be living or not. The discovery of a quantitative theory of information was one of the major additions to Wiener's thought after the writing of the "Behavior, Purpose and Teleology" paper. Information theory thus became one of the "new topics" he presented at the Macy meetings. This topic must have confused the conferees greatly as the application of information theory to biological and social systems represents an enormous undertaking, one that even today has not been advanced very far.

Wiener's statistical mathematical methods were a basic element of his discussions of time series analysis and information theory. But his great interest in the implications of statistical thermodynamics were also important for his understanding of "the procession of events in time"; that is, the irreversibility of most natural events in accordance with the second law of thermodynamics. Wiener attempted to make a connection between this irreversibility and the directedness of the behavior of both servo-mechanisms and organisms.

He saw this connection as providing yet another common basis for the analysis of living systems and machines.

Thus we see some of the major points Wiener was attempting to present during the early feedback meetings. In his autobiography, Wiener summarized what he was attempting to present in his cybernetic synthesis as follows:

> My first goals were rather concrete and limited. I wanted to give an account of the new information theory which was being developed by Shannon and myself, and of the new prediction theory which had its roots in the prewar work of Kolmogoroff and in my researches concerning anti-aircraft predictors. I wished to bring to the attention of a larger public than had been able to read my 'yellow peril' the relations between these ideas and show it a new approach to communication engineering which would be primarily statistical. I also wished to alert this larger public to the long series of analogies between the human nervous system and the computation and control machine which had inspired the joint work of Rosenblueth and me. However, I could not undertake this multiform task without an intellectual inventory of my resources. It became clear to me almost at the very beginning that these new comcepts of communication control involved a new interpretation of man, of man's knowledge of the universe, and of society.[32]

The last sentence of Wiener's statement turned his goals which, by some estimates, may have been "concrete and limited," into a set of goals which was anything but that. Wiener wanted to present not only a scientific thesis, but the philosophical, sociological, intellectual and even political implications of this thesis.

As we have seen in the second and third chapters, Wiener was always interested in the wider implications of science, but the

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[32] N. Wiener, I Am A Mathematician: The Later Life of a Prodigy (Garden City, New York: Doubleday, 1956; reprinted, Cambridge, Massachusetts: The M.I.T. Press, 1973), p. 325.

Second World War, and in particular, the dropping of two atomic bombs in Japan, strongly sensitized him to the dangers of the misuse of science. Three months after the New York Academy conference, a scathing letter from Wiener, "A Scientist Rebels," appeared in the Atlantic Monthly.[33] In this widely noted comment, Wiener repeatedly criticized the military use of scientific research.

Thus, during the period of the early "Macy meetings," Wiener was immersed in a broad range of scientific, mathematical, technological, philosophical, sociological and political problems. When, as a leader of these early meetings, he brought up many of these topics, he greatly broadened the subject matter which otherwise might have been discussed. That Wiener and his work were, indeed, a leading influence at these meetings is apparent from several accounts (see below, pp. 257-258). This leading role has been graphically described by Steve J. Heims:

> Wiener was the dominant figure at the conference series, in his role as brilliant originator of ideas and *enfant terrible*. Without his scientific ideas and his enthusiasm for them, the conference series would never have come into existence, nor would it have had the momentum to continue for seven years without him. A short, stout man with a paunch, usually standing splay-footed, he had coarse features and a small white goatee. He wore thick glasses and his stubby fingers usually held a fat cigar. He was robust, not the stereotype of the frail and sickly child prodigy. Wiener evidently enjoyed the meetings and his central role in them: sometimes he got up from his chair and in his ducklike fashion walked around and around the circle of tables, holding forth exuberantly, cigar in hand, apparently unstoppable. He could be quite unaware of other people, but he communicated his thoughts effectively and struck up friendships with a number of the participants. Some were intrigued as much as annoyed by Wiener's tendency to go to sleep and even snore

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[33] The Atlantic Monthly 179 (January, 1947), p. 46.

during a discussion, but apparently hearing and digesting what was being said. Immediately upon waking he often would make penetrating comments. The psychoanalyst present, L. Kubie, was professionally interested in Wiener's 'obvious ability of processing material while asleep, and then without forgetting it, carrying it right over into waking speech. My theory is that this is the key to his creativity.'[34]

McCulloch, the chairman of the meetings, had interests which were almost equally as broad as Wiener's. The result was that often there seemed to be virtually no limits regarding the subject matter discussed at the meetings. It should be pointed out, however, that the meetings were designed to be exploratory in nature. There were many exciting ideas "in the air" resulting from the rapid advances in science and technology during the World War II period and the conferees were eager to discuss all the possible implications of these developments. The degree to which these meetings were successful in disseminating the feedback ideas, despite the lack of focus for much of the discussion, can be better appreciated through a closer examination of the manner and context in which feedback was discussed by the conferees.

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[34] S. Heims, Von Neumann and Wiener, p. 206. A British colleague of Wiener's, W. Grey-Walter, described Wiener's public naps as his "habitual defence against competition"; see W. Grey-Walter, "Neurocybernetics (Communication and Control in the Living Brain)" in J. Rose, ed., Survey of Cybernetics: A Tribute to Dr. Norbert Wiener (London: Iliffe Books, 1969), p. 88. Heims reports that when tensions later increased between Wiener and von Neumann, Wiener would "ostentatiously doodle or go to sleep very noisily," as von Neumann attempted to speak (S. Heims, Von Neumann and Wiener, p. 208).

But Grey-Walter, a neurophysiologist, reported taking "several records of Wiener's brain rhythms which proved, amongst other things, that his defensive naps were real deep sleep; he could drop off in a few seconds, but would awaken instantly if one spoke his name or mentioned any topic in which he was really interested" (Grey-Walter, "Neurocybernetics," p. 88).

The Discussion of Feedback at the Early Macy Meetings

The title of the first Macy meeting, "Feedback Mechanisms and Circular Causal Systems in Biological and Social Systems," indicated that feedback was at least initially expected to be a major theme of the conference.[35] Wiener did, in fact, give an expository talk concerning negative feedback on the first day of the first meeting. He described the negative feedback "circuit" of the Watt flyball governor and, according to McCulloch's summary of the meeting, stated that this device "differed from all previous automata in that it took cognizance of the world about it and of its own performance and operated so as to reduce the discrepancy between its intended performance and its actual performance." Further, according to McCulloch,

> From this he extended the concept to reflexes and to all purposive activity by indicating that one merely needed to supply such a circuit with appropriate receptors and effectors. Thusly they become goal-seeking devices, of which he mentioned several, more particularly those which had built into them computing devices, some of which might so base their action on previous information as to guess the future.[36]

The reference to prediction by computing devices which would operate on the basis of past information to predict the future is significant because Wiener was apparently speaking of the use of his time series methods to extrapolate behavior. This statement represents another

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[35]"Circular Causality" is not a synonym for negative feedback itself, but also includes the concept of positive feedback.

[36]W. S. McCulloch, "To the Members of the Fourth Conference," p. 2.

example of Wiener presenting a topic from his larger thesis; one which surely must have confused many of the conferees who were not at all familiar with the time series methods.

As a further problem, there was apparently some confusion resulting from Wiener's initial presentation of the negative feedback concept itself, a presentation which seems to have been rather cursory in some respects.[37]

The presentation of Wiener at the first meeting, with the possible exception of its repetition at Macy meeting II(S), seems to have been the only general expository statement on negative feedback made at all the Macy meetings. Feedback was but one topic in the enormous range of conversation topics which included, to name only a few, the latent period of human neurons, the theoretic requirements of the social sciences, and the proposal that "in the dominant social organization of mankind only that theory of the good could survive which was in line with man's science. . . ."[38] Nevertheless, within this diverse range of topics, possible applications of feedback were

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[37]McCulloch, in his summary of Wiener's remarks, writes of a "regenerative feedback" which kept Watt's steam engine "oscillating and turning." This feedback acted in addition to negative feedback which kept the engine "at constant frequency or speed regardless of head or load" (W. S. McCulloch, "To the Members of the Fourth Conference, p. 2). The use of regenerative, or positive, feedback in this description, if not totally unnecessary is certainly confusing. It is not clear whether the problem was in Wiener's presentation or McCulloch's understanding of it. The subtleties of feedback control, such as those related to offset, derivative and integral control, if covered by Wiener, did not make enough of an impression on McCulloch to be included in his summary.

[38]Ibid.

widely discussed. These applications included the possible expalanation of stock market fluctuations, the regulation of competing needs within a given organism, and even the "dominance in the pecking order of chickens." McCulloch reported that, "By this time we had become so weary of far-flung uses of the notion of feedback that we agreed to try to drop the subject for the rest of the [first] conference."[39] As a result, in a memorandum to the conferees for the following conference (Macy II), McCulloch counselled against the loose application of the feedback concept saying, "It can remain useful only so long as it remains a mechanistic hypothesis leading to empirical investigation of postulated processes and to the creation of the mathematics required for quantitative formulation."[40] McCulloch and Pitts had such research to report upon: their work on the visual cortext and the recognition of universals. Wiener and Rosenblueth would also report on their research of that summer on the clonus mechanism of the neuromuscular system (see below, pp. 225-226). Apparently, however, some conferees, especially those from the social sciences, felt their understanding of feedback was lacking. Bateson soon wrote to Wiener and Rosenblueth urging them to prepare a fuller written description of the feedback process, its subtleties and problems, as part of a larger general statement to augment the "Behavior, Purpose and Teleology" paper. He indicated

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[39]W. S. McCulloch, "Points of Agreement," p. 75.

[40]W. S. McCulloch, "To the Members of the Macy Conference on Feedback Mechanisms: October 17, 18 and 19, 1946" (unpublished memorandum, n.d.), Wiener Papers.

that detailed research would not be of great value to the conferees unless it was explicitly presented within a larger framework. Bateson also pointed out that except for the "Behavior, Purpose and Teleology" paper, which contained no technical detail about feedback, no published framework existed which could serve as unifying reference material for the conferees. Bateson suggested a list of topics to be covered in the proposed fuller presentation; a list which is interesting because it expresses the ideas that Bateson, a social scientist, felt needed greater clarification. He wrote:

> . . . what is needed from you is . . . a good general statement of the properties of feedback systems and of the lines of thought which they suggest. Except for the original article which you two did with Bigelow, there is, as far as I know, no general statement to which one can refer people . . . .
>
> The following list of topics will indicate what I want; don't, however, be bound by this list and feel yourselves free, please, to drop any of these. . . .: 1. Positive and negative feedback; 2. the circumstances under which the same causal circuit may behave as either degenerative or regenerative; 3. The implications of the term 'error activated' and analogies with purposive behavior; 4. On-off systems, quantitative systems, linear and non-linear systems; 5. The energy characteristics of systems in which the component parts have their own energy sources and act as 'relays'; 6. Entropy more relevant than energy; 7. Characteristics of steady states - oscillation, maximization, maximization of oscillation, etc.; 8. The conditions which determine stability and instability; 9. Types of pathology which occur in such systems - effects of changes in time constants, loading, transmission ratios, thresholds, etc.; 10. Hierarchies of feedback - the need for local as well as central mechanisms of control.[41]

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[41] G. Bateson to A. Rosenblueth and N. Wiener, March 27, 1947, Wiener Papers. In 1979 Bateson described the importance of the "Behavior, Purpose and Teleology" paper, saying that with it, "The central problem of Greek philosophy---the problem of purpose, unsolved for 2,500 years---came within range of rigorous analysis." See G. Bateson, Mind and Nature (New York: E. P. Dutton, 1979), p. 106.

Bateson's letter well indicates the massive number of topics raised at the early meetings and the great number of questions which were subsequently raised as a result.

The second Macy meeting (Macy II) included debate over the meaning of the term "field" in terms of its ability to direct objects into adaptive behavior, the concept of gestalt, and communication in ant colonies.[42] This meeting had been preceded by a special meeting for members of the social sciences (Macy IIS) which Wiener attended. The meeting was planned to include about twenty social scientists and to "give special attention to those branches of social science in which quantitative methods have been used." Planned presentations included introductory statements by McCulloch, Wiener, von Neumann and Bateson, as well as talks by Paul Lazarsfeld, Robert Merton, Talcott Parsons and Clyde Kluckholn.[42a] McCulloch recorded that the result of the special meeting was a request that the rest of the group "clarify the notions of 'field' and 'Gestalt' and discuss mathematical means for detecting causal connections, particularly circular ones."[42b] The latter part of this request indicates that if the introductory statements represented an attempt to clarify the mathematical concepts for the sociologists, this attempt left some very basic questions unanswered. As a result of this meeting, one

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[42] W. S. McCulloch, "To the Members of the Fourth Conference."

[42a] G. Bateson to N. Wiener, June 19, 1946, Wiener Papers.

[42b] W. S. McCulloch, "To the Members of the Fourth Conference."

evening of the subsequent Macy II conference was ". . . given over to a discussion of sociological and anthropological data which might be suitable for detecting causation and its circularities."[43] McCulloch, in his summary, states that this topic was taken up by Wiener who

> was unwilling to burden us with mathematical details and contented himself by stating that from any time series it was possible to compute a best prediction, that the more data were available the better became a prediction and that given two time series it was possible to determine to what extent either was predictable from the other and with what lag, and finally that our notion of causation itself was that of predictability with lag, insofar as causality has any empiric meaning. And hence that wherever sociologists or social anthropologists were able to collect time series which need be no more than enumerations of decisions at specified times, it would be possible to discover causal sequences in human conduct.[44]

Although Wiener, at this point, again seemed to be advocating the use of his time series methods, in this case for social issues, by the time he wrote Cybernetics one year later he had come to the conclusion that statistical runs in the social sciences were too short "to have an appreciable therapeutic effect in the present diseases of society."[45]

The second Macy meeting was held in association with the New York Academy of Sciences Conference on Teleological Mechanisms in October 1946. Although there is some uncertainty as to what was said at this conference, the published papers which "resulted" from it indicated a continuation of the pattern of an extremely broad discussion of diverse topics. The published papers included Wiener's

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[43] Ibid.

[44] Ibid.

[45] N. Wiener, Cybernetics, p. 24.

outline of his forthcoming Cybernetics,[46] a discussion of mathematical ecology by G. E. Hutchinson,[47] an investigation of residual pain in nerve-damaged patients,[48] and McCulloch's "A Recapitulation of the Theory, with a Forecast of Several Extensions."[49]

McCulloch's paper provides valuable insight into the manner in which the raison d'être of the meetings was viewed by one of the major participants. He wrote:

> It is important to recapitulate, at this time, certain aspects of the theory of teleological mechanisms, and to indicate one or two directions in which progress is to be expected. . . .
>
> The conference has considered two related questions; namely, 'What characteristics of a machine account for its having a Telos, or end, or goal?' and 'What characteristics of a machine define the end, or goal, or Telos?' It has answered the former in terms of activity in closed circuits, and the latter in terms

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[46] N. Wiener, "Time, Communication, and the Nervous System," in L. K. Frank et al, "Teleological Mechanisms," pp. 197-220. The table of contents for this article in the Annals indicates that the papers published were "the result of" the Conference on Teleological Mechanisms held October 21 and 22 in 1946. Bateson's letter to Wiener and Rosenblueth of March 27, 1947 (Wiener Papers) states, "About the manuscript for the Academy Conference on Teleological Mechanisms: I confess I was bothered when Wiener told me the other day that . . . I could not hope to see the manuscript until some time in the summer." It seems clear, then, that Wiener's contribution to the published proceedings was not written until the summer of 1947 at the earliest. Since the summer of 1947 is when Wiener wrote Cybernetics it may very well be that the outline of his synthesis given in the Annals was written simultaneously with Cybernetics.

[47] G. E. Hutchinson, "Circular Systems in Ecology," in L. K. Frank et al., "Teleological Mechanisms," pp. 221-246.

[48] W. K. Livingston, "The Vicious Circle in Causalgia," in L. K. Frank et al, "Teleological Mechanisms," pp. 247-258.

[49] W. S. McCulloch, "A Recapitulation of the Theory, with a Forecast of Several Extensions," in L. K. Frank et al, "Teleological Mechanisms," pp. 259-277.

> of entities which the mechanism could compute in terms of the discriminations it could make.[49a]

McCulloch amplified this rather cryptic statement in his extremely wide-ranging paper. He discussed the understanding of mechanisms responsible for purposeful behavior, "teleological mechanisms," in terms of their philosophical antecedents as well as the functional importance of negative feedback. In describing teleological mechanisms as a possible solution to the problem of the regulation of behavior, McCulloch gave credit to Wiener and his colleagues:

> It had become clear that the problem [of regulation of behavior], whether in organisms or man-made machines, was a question of signals in closed paths. Considerations of energy were immaterial. It was information that went around the circuit. Then began the collaboration between the physiologist, Rosenblueth, who was Cannon's chief collaborator, and the communication engineer---in this case Norbert Wiener---a mathematician if you will. In January, 1943, they published with Bigelow, in the Journal '<u>Philosophy of Science</u>,' an article on <u>Behavior</u>, <u>Purpose</u>, and <u>Teleology</u>, which heralded such discussions as we hear these days. They concluded that purposeful, or teleological, behavior is controlled by negative feedback.[50]

The great weight that Wiener's ideas had on these early meetings is reflected not only in the nature of McCulloch's discussion of negative feedback, but also in the manner in which McCulloch emphasized the importance of Wiener's time series methods in the prediction of behavior.[51] The time series methods were not, in fact, critical for the understanding of negative feedback or of

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[49a] Ibid, p. 259.

[50] W. S. McCulloch, "A Recapitulation," p. 261.

[51] Ibid., p. 264.

teleological mechanisms. Their importance was overemphasized by Wiener, probably because of his strong personal interest in them. As Heims has observed, "Wiener's tendency was to see the theory of time-series (Brownian motion, harmonic analysis, stochastic processes), which he had largely created in the first place, as more all-embracing and fundamental than others saw it."[52] It is clear, however, that Wiener had convinced McCulloch of the importance of time series analysis.

For many of the conferees, especially those lacking a strong technical mathematical background, the mystery of the sophisticated mathematics used by Wiener seemed to lend an air of authenticity to the discussions in which the most challenging problems were considered in terms of the new methods. A somewhat over-optimistic impression was given that progress was being made in the most diverse and general fields through the application of the new methods. McCulloch, for example, stated:

> He [Wiener] has pointed out how such autocorrelations and intercorrelations of data in time-series may detect the existence of causal connections, and their lags in one or both directions, provided the correlation is less than perfect. He has also demonstrated how, by these means, we may detect feedback, including inverse feedback, in Ecology, Anthropology, and Sociology. The data need be no more than decisions, actions or opinions in time, provided we have runs of sufficient length.[53]

The provision of "runs of sufficient length" was, as noted above, an element which Wiener himself, by the time he wrote Cybernetics, found

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[52] S. Heims, Von Neumann and Wiener, p. 151.

[53] W. S. McCulloch, "A Recapitulation," p. 264.

to be lacking in sociological applications. Nevertheless, it was easy for one to receive the impression from these meetings, in which discussions of extremely sophisticated mathematical techniques were juxtaposed with considerations of the most profound problems in almost every field imaginable, that significantly greater scientific progress was being made in the solution of these problems than was actually the case. In particular, the problem was that the use of sophisticated mathematical methods implied a great precision, or at least power, of analysis when, in fact, the application of these methods was of the most tentative nature. The early meetings seemed to have established this pattern of juxtaposition. It was a problem that later plagued Wiener's presentation of Cybernetics as well, and also (see below, pp. 281-282) general systems theory, in which many of the same ideas were incorporated. This problem was in part aggravated at the start by McCulloch who, as chairman, exerted a strong influence when he emphasized the success of the methods in complex applications such as "Ecology, Anthropology, and Sociology," and when he made statements such as the following one, which intermixed the technical language with extremely complex social problems.

> I know of no utopian dream that would be nearer to everybody's wishes, including my own, than that man should learn to construct for the whole world a society with sufficient inverse feedback to prevent another and perhaps last holocaust. There may at least be time for us to learn to recognize and decrease the gain in those reverberating circuits that build up to open aggression. We cannot begin too soon. . . . All we have a right to ask of the appropriate sciences are long-time runs of data. We know it will take years to collect these, but we must have them before we can determine whether the mechanism of negative feedback accounts for the stability and purposive

> aspects of the behavior of groups. This was one of our questions.[54]

McCulloch was far from the only conferee who saw almost unlimited potential for the new methods.[55] Indeed, the success that the meetings did have was due in large part to the extraordinary excitement they generated, of which more presently (see below, pp. 271-272). It should not be imagined, however, that there was no objection to the nature of the discussions as they occurred. Julian Bigelow recalls that the meetings appeared totally unfocused to him at the time.[56] John von Neumann raised some particularly important objections which will be explored with some of the other reactions to the meetings by the conferees.[57]

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[54] Ibid.

[55] L. K. Frank, in his "Foreword" to the published proceedings of the New York Academy of Science Conference on Teleological Mechanisms, stated,

> "It is suggested that we look at this conference as an important, perhaps a major, step toward the new climate of opinion now emerging in scientific, philosophical, and even artistic activities. . . . As I see it, we are engaged, today, in one of the major transitions or upheavals in the history of ideas, as we recognize that many of our older ideas and assumptions are now obsolescent and strive to develop a new frame of reference to give us a clearer and more comprehensive understanding of the basic processes underlying all events." (See L. K. Frank et al, "Teleological Mechanisms," p. 192.

The similar views of the philosopher F. S. C. Northrop are examined below.

[56] Interview with author, June 13, 1979.

[57] The pattern of discussions at the early meetings described thus far was continued in the third through the fifth meetings, with some further additions of subject material. At the third meeting, for instance, wide-ranging discussions of the modelling of the learning process and consciousness took place. Much of the fifth

Some Reactions of the Conferees to the Early Meetings

Certainly these early meetings were not typical of specialists' conferences in which a highly technical language is understood by all and the topics narrowly defined. The Macy meetings were extremely trying for most of those who attended. Representatives of the highest levels of their professions were asked to listen to, absorb, comment upon, and utilize material which was totally outside their disciplines. The first five meetings were, in many ways, a disaster. As McCulloch, the chairman, later recalled:

> . . . 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. 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 interupting.' 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.[58]

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conference was particularly concerned with considerations of the structure of language. Since the major pattern of the meetings with regard to the manner of presentation of the feedback ideas had been established by the time of the New York Academy of Sciences conference, no attempt will be further made to describe the proceedings of the later meetings prior to the publication of Cybernetics.

[58]W. S. McCulloch, "Recollections," pp. 12-13.

One difficulty was that many of the conferees from the social sciences were not prepared to absorb arguments which assumed a certain technical background. Compounding this difficulty was the dominance of Wiener, a personality with whom many found it difficult to contend. And personality factors notwithstanding, there was a basic point of disagreement between some of the social scientists and those who subscribed to the cybernetic view, then evolving. The debate concerned the idea of gestalt.[59] Wolfgang Köhler, one of the originators of gestalt theory, attended one of the early meetings and disagreed with the cyberneticians' contention that human functioning, including gestalt behavior, could be accounted for by breaking the system down into its parts, as Pitts and McCulloch had done, for example, with their neural net theories. On the contrary, according to Heims, Köhler and the whole gestalt school "preferred nonatomistic explanations; they insisted that the 'whole' must be taken as a unit which cannot be entirely derived from its parts."[60] In spite of these difficulties, some of the social scientists reacted very positively to the conferences. The anthropologist, Gregory Bateson, was able to reinterpret some of his earlier work in terms of cybernetic analysis and derived his later theory of the "double-bind" in schizophrenia partly from conversations with Wiener resulting from

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[59] This "encounter" is described in S. Heims, "Encounter," pp. 368-373.

[60] S. Heims, Von Neumann and Wiener, p. 372.

the conferences.[61] The zoologist G. E. Hutchinson seems to have been greatly influenced in his later work by his participation in the early meetings.[62] Margaret Mead remained interested enough to help edit the published transactions of the last five conferences.

It will be useful, before taking up certain further reactions of the conferees, to briefly examine some other research of McCulloch and Pitts, whose work in neurophysiology was a major influence on the Macy group. A particularly important work of McCulloch and Pitts, after their "Logical Calculus" paper of 1943 (see pp. 202-206) was "How We Know Universals: The Perception of Auditory and Visual Forms," published in 1947.[63] This paper was, at least in part, a response to the controversy over gestalt. McCulloch and Pitts dealt with particular problems such as how we can recognize musical chords regardless of pitch, and shapes regardless of size or orientation.

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[61] Ibid., p. 369. See also, G. Bateson, Mind and Nature, pp. 103-117.

[62] Hutchinson stated to me recently in an interview (November 18, 1981) that:

> "I think I began to see how [at the Macy meetings] the rather rudimentary mathematical theories we had in ecology could fit into much broader schemes. I would never have thought, for instance, of the particular generalization and the logistics involved in time lag if I had not taken part in those meetings."

Hutchinson added that the meetings were primarily an exercise in "intellectual free association," which he found quite illuminating.

[63] Walter Pitts and W. S. McCulloch, "How We Know Universals: The Perception of Auditory and Visual Forms," Bulletin of Mathematical Biophysics 9 (1947), pp. 127-147; reprinted in W. S. McCulloch, Embodiments of Mind (Cambridge, Massachusetts: The M.I.T. Press, 1965), pp. 46-66.

Their method consisted in showing how neural nets could be conceived which would "produce the same output for every input" for the various manifestations and/or orientations of an auditory or visual form.[64] As an example of the many methods presented by Pitts and McCulloch for the recognition of universals, we may consider their description of "reflex mechanism," a mechanism operating in the eyes which helps one to recognize a universal shape, such as a square. This description is particularly interesting because it involves negative feedback and also because it concisely illustrates an example of a robotic approach to the problem of gestalt. Although some of the anatomical nomenclature in this example is highly technical, the general reader may still obtain a satisfactory overview of the character of the process. The authors state:

> Consider the reflex-arc from the eyes through the tectum to the oculomotor nuclei and so to the muscles which direct the gaze. We propose that the superior colliculus computes by double integration the lateral and verticle coordinates of the 'center of gravity of the distribution of brightness' referred to the point of fixation as origin and supplies impulses at a rate proportional to these coordinates to the lateral and vertical eye-muscles in such a way that these then turn the visual axis toward the center of gravity. As the center of gravity approaches the origin, its ordinate and abscissa diminish, slowing the eyes and finally stopping them when the visual axes point at the 'center of brightness.' This provides invariants of translation. If a square should appear anywhere in the field, the eyes turn until it is centered, and what they see is the same, whatever the initial position of the square. This is a reflex-mechanism, for it operates on the principle of the servo-mechanism, or 'negative feedback.'[65]

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[64]Ibid., p. 47.

[65]Ibid., p. 56.

The philosopher F. S. C. Northrop made the combined contents of the "Behavior, Purpose and Teleology" paper and the Pitts-McCulloch work on the perception of universals a major portion of his vice-presidential address to the History and Philosophy of Science Section of the American Association for the Advancement of Science at its annual meeting in Chicago, December, 1948.[65a]

The remarks of Northrop bear closer examination for a number of reasons. First, the contemporary evaluation of the feedback ideas by a professional and respected philosopher of the time is worth noting. Second, the intensity of his remarks in support of these ideas at an influential forum was an important factor in their propagation. Third, Northrop's remarks show how at the outset the feedback ideas were seen as necessarily being bound up with other complex factors, in particular, the behavior of the human nervous system. Northrop, speaking in the same year as the publication of the Pitts-McCulloch paper, attempted to tie together their robotic methods for recognizing universals with the definition of purpose, as stated in the "Behavior, Purpose and Teleology" paper.

> Recent investigations by Warren S. McCulloch and Walter Pitts show that certain biological organisms, because of the character of the neuron nets in their nervous systems, must know universals, responding to symbols as their exemplars, rather than as mere particulars. Other investigations by Arturo Rosenblueth, Norbert Wiener, and Julian Bigelow show that not merely a human being but also robots with inverse or

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[65a]This address is "substantially" reproduced in F. S. C. Northrop, "Ideological Man in His Relation to Scientifically Known Natural Man," in F. S. C. Northrop, ed., Ideological Differences and World Order (New Haven: Yale University Press, 1949), pp. 407-428.

> negative feedback mechanisms have purposes that define their behavior.[66]

Northrop felt that the combined impact of these ideas was "revolutionary":

> In other words, the basic premise of both the traditional supposedly scientific naturalists and mechanists to the effect that natural and biological systems can have neither knowledge of universals nor normatively defined and behavior-controlling purposes must be rejected.
>
> The scientific demonstration of these exceedingly important conclusions of revolutionary significance for natural science, moral as well as natural philosophy, and for one's theory of the normative factor in law, politics, religion, and the social sciences must now concern us.[67]

Northrop considered feedback ideas and their relation to purpose as being extremely important, and even more so when combined with the neural net theories of Pitts and McCulloch. The utility of this connection is problematic, but the combination was one which was almost universally made by the principals in the development of cybernetics. The reasons for this assumption included the ambition of the conferees, who wanted to tackle the most interesting problems; the emphasis on application to human behavior by the chairman, McCulloch, and by Wiener, and perhaps the loosely-structured format of the early meetings which enabled widely varying topics to be discussed. Finally, it might be added that at this early stage of

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[66] Ibid., pp. 413-414. Northrop was one of the few principals in the early proceedings to call attention to the distinctions raised between "purposeful" and "teleological" behavior as defined by Rosenblueth, Wiener and Bigelow (Ibid., pp. 419-420).

[67] Ibid., p. 414.

the development of the "information sciences" the difficulty of solving problems related to human behavior with the tools just then developing, may not have been fully realized. The fact that Northrop was attempting to account for the existence of different political ideological systems in terms of these ideas is indicative of the level of problem which was being addressed.

These attempts to explain extremely complex behavior in terms of the cybernetic synthesis were premature, a fact which was noted by John von Neumann about one year before Northrop's remarks. Von Neumann questioned the wisdom of starting a theoretical investigation of automata with the most complex, rather than the least complex experimental phenomena. He wrote to Wiener in November 1946:

> Our thoughts -- I mean yours and Pitts' and mine -- were so far mainly focused on the subject of neurology, and more specifically on the human nervous system, and there primarily on the central nervous system. Thus, in trying to understand the function of automata and the general principles governing them, we selected for prompt action the most complicated object under the sun -- literally. In spite of its formidable complexity this subject has yielded very interesting information under the pressure of the efforts of Pitts and McCulloch, Pitts, Wiener and Rosenblueth. Our thinking -- or at any rate mine -- on the entire subject of automata would be much more muddled than it is, if these extremely bold efforts -- with which I would like to put on one par the very un-neurological thesis of R. Turing -- had not been made. Yet, I think that these successes should not blind us to the difficulties of the subject, difficulties, which, I think, stand out now just as -- if not more -- forbiddingly as ever.[68]

Von Neumann continued with a penetrating critique of the cybernetics program as it had thus far developed.

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[68] J. von Neumann to N. Wiener, November 29, 1946, Wiener Papers.

The difficulties are almost too obvious to mention: They reside in the exceptional complexity of the human nervous system, and indeed of any nervous system. What seems worth emphasizing to me is, however, that after the great positive contributions of Turing - cum - Pitts - and - McCulloch is assimilated, the situation is rather worse than better than before. Indeed, these authors have demonstrated in absolute and hopeless generality, that anything and everything Brouwerian can be done by an appropriate mechanism, and specifically by a neural mechanism -- and that even one, definite mechanism can be 'universal'. Inverting the argument: Nothing that we may know or learn about the functioning of the organism can give, without 'microscopic', cytological work any clues regarding the further details of the neural mechanism. I know that this was well known to Pitts, that the 'nothing' is not wholly fair, and that it should be taken with an appropriate dose of salt, but I think that you will feel with me the type of frustration that I am trying to express. (H. N. Russell used to say, or to quote, that if the astro-physicist found a general theory uniformly corroborated, his exclamation would be 'Foiled again!' since no experimenta crucis would emerge.) After these devastatingly general and positive results one is therefore thrown back on micro-work and cytology -- w[h]ere one might have remained in the first place. (This 'remaining there' is, of course, highly figurative in my case, who have never been there.) Yet, when we are in that field, the complexity of the subject is over-awing. To understand the brain with neurological methods seems to me about as hopeful as to want to understand the ENIAC with no instrument at one's disposal that is smaller than about 2 feet across its critical organs, with no methods of intervention more delicate than playing with a fire hose (although one might fill it with kerosene or nitroglycerine instead of water) or dropping cobblestones into the circuit. Besides the system is not even purely digital (i.e. neural): It is intimately connected to a very complex analogy (i.e. humoral or hormonal) system, and almost every feedback loop goes through both sectors, if not through the 'outside' world (i.e. the world outside the epidermis or within the digestive system) as well. And it contains, even in the digital part, a million times more units than the ENIAC. And our intellectual possibilities relatively to it are about as good as sombodies [sic] vis-a-vis the ENIAC, if he has never heard of any part of arithmetic. It is true that we know a little about the syndromes of a few selected breakdowns -- but that is not much.

> My description is intentionally exaggerated and belittling, but don't you think there is an element of truth in it? [Emphasis added.][69]

Von Neumann went on to describe a much simpler living system which might prove more tractable to investigation:

> Now the less-than-cellular organisms of the virus or bacteriophage type do possess the decisive traits of any living organism: They are self reproductive and they are able to orient themselves in an unorganized milieu, to move towards food, to appropriate it and to use it. Consequently a 'true' understanding of these organisms may be the first relevant step forward and possibly the greatest step that may at all be required.[70]

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[69]Ibid. The "Brouwerian" reference is to L. E. J. Brouwer, a mathematician of the early twentieth century, associated with the "constructivist" school of mathematics, a school which holds that "any legitimate mathematical object must be constructed in a finite series of stages, beginning with a small number of primitively acceptable objects, and proceeding from one step to the next by one of a few acceptable means of manipulation" (see William F. Aspray, Jr., "From Mathematical Constructivity to Computer Science: Alan Turing, John von Neumann, and the Origins of Computer Science in Mathematical Logic" [Ph.D. dissertation, University of Wisconsin-Madison, 1980], p. 50). The constructivist view well fit the Turing machine and some of the McCulloch-Pitts work, and hence von Neumann used the term "Brouwerian" to cover the type of effort which was then occurring in the robotics field.

Von Neumann's criticisms in this letter did not prevent him from subsequently utilizing the McCulloch-Pitts work to a large extent in his development of a "Logical Theory of Automata." He was, in fact, criticized himself for later trying to extend the theory to complex applications such as the human brain. What caused von Neumann to take up these ambitious applications after his early criticism of such an approach is not clear. See W. Aspray, "Mathematical Constructivity," pp. 350-408 for a description of some of von Neumann's later work in this regard.

[70]J. von Neumann to N. Wiener, November 29, 1946, Wiener Papers. Von Neumann proposed a program of research in this letter which did anticipate some of the later work done in molecular biology. He suggested studying the gene-enzyme relationship in viruses and bacteriophages, and using the electron microscope and x-ray analysis to determine the structure of cell components.

Von Neumann stated that these tiny organisms had a small enough number of atoms so that their interactions would necessarily be less complex, perhaps of the order of complexity of a "locomotive." Further, von Neumann indicated that "mechanical units" such as the phosphate bond, were prime candidates for study because they had so few elements.[71]

Although von Neumann's program was, to a large degree, carried out some twenty years later during the explosive growth of molecular biology, it was not adopted by Wiener, McCulloch and the other early leaders in the development of cybernetics. This insistence upon analyzing _all_ phenomena, no matter how complex, in terms of the new theories led the conferees into every area imaginable. The hope was, of course, that beneath the apparent diversity of topics a common ground of understanding could be found, a common ground, which, if found, would result in an enormous unification of knowledge. The unifying theme was originally construed in terms of negative feedback, or so, at least, the conference titles and the influence of the "Behavior, Purpose and Teleology" paper indicate. But as other topics, such as the time series analysis and the general functioning of the human nervous system were added by Wiener and others, the desired "common ground" became so large that individual topics became lost within it.

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[71] Ibid.

Early Assimilation: Analysis

What may one conclude about this first stage in the assimilation of the feedback methods? Clearly there was considerable confusion about what "new concepts and methodologies"[72] were being explored. Wiener may have had a synthesis in mind when he brought in topics from servo theory, information theory, time series analysis, neural networks and philosophy. It seems, however, that the other individual conferees, such as Bateson and McCulloch, simply extracted those elements which proved most useful for their own work. McCulloch, for instance, in his work on neural networks, did not have much need for time series analysis and did not utilize it, even though he spoke of it as being an important part of the new ideas.

There is no question that the inclusion of negative feedback within these other topics which were undergoing rapid concurrent development made the assimilation of the feedback concepts more difficult because the conferees first had to sort them out from the much larger set of ideas which was being discussed. It is also true that the subject matter of the first attempts to use the feedback ideas and the other new methods was too complex to allow successful application at such an early stage of the development of the methods; a point that von Neumann emphasized.

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[72] L. K. Frank, "Foreword," in L. K. Frank et al, "Teleological Mechanisms," p. 192.

The task of these early conferences was enormous. How does one introduce a concept from one field to specialists from what had been unrelated disciplines? During a time of extremely rapid developments in many fields, should one arbitrarily be conservative regarding the potential developments of these fields? How is one to know which disciplines will benefit most from an application of new concepts?

While it is true that many of the attempted applications of the new ideas were too ambitious, it was just this ambitious nature of the program which fascinated and attracted many of the conferees. Von Neumann was right in saying that it would have been more reasonable to start the analysis with a simple subject such as the behavior of a virus, but it is doubtful whether such a program would have attracted the wide range of conferees who were thus exposed to the new ideas. As it was, when the molecular biology applications of feedback were needed, the concept of feedback was already widely dispersed and available, primarily because of events stemming directly and indirectly from these conferences. What was important about this early stage of assimilation of the feedback methods was not whether the goals of the participants were realistic, but the very fact that they participated. One may view the confusion and lack of definition of subject material as an adjunct of the dissemination of radically new ideas to a widely diverse audience. The Macy conferences thus provide a vivid illustration of the problems associated with an interdisciplinary approach to subject matter.

The fact that Wiener was working on a much larger undertaking than the rest of the conferees had both good and bad effects. On the positive side, Wiener's drive toward synthesis was the principle factor which enabled him to associate with workers from many different disciplines. This factor was quite important for providing some semblance of unity for these early meetings. Wiener's multi-disciplinary interests, honed by his extraordinary achievements as a prodigy, and his later work, allowed him to talk at a professional level with almost all those involved. One need only recall that Wiener had had graduate study in biology, mathematics, logic, physics, and philosophy. On the negative side, Wiener's attempted synthesis created a situation which was ripe for abuse. Its almost completely general and ambitious nature allowed others to interpret in almost any fashion; a situation which arose frequently, especially after the publication of Cybernetics, when the ideas became much more widespread. There remains the unanswered question if, apart from its use in stimulating interest in the many new fields of development related to robotics after World War II, the overall cybernetic synthesis of Wiener had any scientific value. This is a question which will be addressed below. Since the publication of Cybernetics had such a great impact on the assimilation of negative feedback, we will now briefly examine the events leading to the publication of this book, and then discuss its role in the dissemination of the feedback methods and concepts.

Wiener's *Cybernetics*

To recapitulate, by 1945 Wiener had developed many of the ideas which would appear in expanded form in *Cybernetics* three years later. The fire control collaboration with Bigelow and the subsequent writing of "Behavior, Purpose and Teleology" in 1942 were of paramount importance in prompting Wiener toward his cybernetic synthesis. At first Wiener was interested in explaining teleological behavior in terms of negative feedback. From this start, Wiener's involvement with the subject grew until he attempted to account for not only purposive behavior, but the general functioning of living and non-living entities in terms of perfectly general theories of communication and control. These theories, in turn, led him to apply his tools of statistical and time series analysis. His increasing awareness of the social role of scientists led him to also consider the political and sociological implications of his ideas. The growth of the synthesis from the joint paper written in 1943 to the writing of *Cybernetics* during the summer of 1947 was stimulated not only by Wiener's desire to understand topics at the most fundamental level, but also by the diversity and high caliber of the professionals with whom he interacted.

Wiener was quite sensitive to the fact that as a scientist he would have to provide research to substantiate his claim that techniques and ideas from engineering could be equally applied to explain the behavior of living material. He had hoped that the Princeton meeting of 1945 would result in a research center to help carry out the program of the "Teleology Society" (see above, pp. 230-231). Wiener attempted to create such a center at Massachusetts Institute of Technology and to entice McCulloch, Pitts, and von Neumann to join it.[73] McCulloch did come to Massachusetts Institute of Technology subsequently (in 1952), but von Neumann became involved with computer projects at Princeton and never joined Wiener. In the Spring of 1945, shortly after the Princeton meeting, Wiener went to Mexico where he collaborated with Rosenblueth on research relating to the process of muscle contraction, especially in the fibrillation of the heart.[74]

In the following summer, of 1946, Wiener and Rosenblueth performed further research, this time on the topic of clonus, having to do with the periodic contraction and relaxation of muscles under stress. In the research Wiener attempted to apply servo-mechanisms theory to account for the periodicity of the muscle behavior.[74a]

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[73] See N. Wiener to A. Rosenblueth, January 24, 1945, and July 11, 1945, both in the Wiener Papers.

[74] See Cybernetics, pp. 16-17. An aged W. B. Cannon was with Rosenblueth during Wiener's visit.

[74a] See Cybernetics, p. 19. Wiener and Rosenblueth were not the first to propose specific research relating control theory to biology. In a remarkable parallel development, George L. Kreezer, who was at the Massachusetts Institute of Technology Radiation Laboratory during the war, proposed, in detail, a research project to model the homeostatic properties of the body in terms of the then newly evolving control theory techniques. Kreezer wrote in his research proposal of 1945:

> "The formulation of the present problem is the outgrowth of an interest I have had for many years in finding methods for formulating more satisfactory theories of adjustive processes in the organism. One outcome of this interest was some work

Wiener and Rosenblueth presented their results at the Macy II meeting in October of 1946.

During the Spring of 1947, Wiener attended a conference on harmonic analysis in France. On the way there he visited Turing and J. B. S. Haldane in England and observed some of the work which had been occurring in the growth of British computer science.[75] Wherever he went, Wiener discussed his ideas but found that "[not] as much progress had been made in unifying the subject and in pulling the various threads of research together" as had occurred in the United

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begun, a few years ago, on physical models of certain automatic adjustive processes. Certain aspects of this work led to my joining the staff of the Radiation Laboratory at M.I.T. where I became acquainted with methods developed for mathematical analysis of physical control systems, and recognized their appropriateness for the biological problems in which I was interested." (George L. Kreezer, "Plans for work," Wiener Papers; the date for this plan was obtained from the John Simon Guggenheim Memorial Foundation, which retains the original copy.)

Kreezer indicated in an interview with me (August 27, 1980) that Wiener was a referee for the Guggenheim Foundation and probably saw his proposal because soon after he submitted it Wiener invited him to his office and intimated that Kreezer might want to join his research group. Wiener almost certainly did see the proposal because a copy of it remains in his papers. Instead of joining Wiener, Kreezer, after receiving a Guggenheim Fellowship, left to work at the Institute for Advanced Study in Princeton, New Jersey, where he did, indeed, carry out his research. Kreezer's proposal not only anticipates much of Cybernetics, but also the program of cybernetic analysis as presented in Chapter Two.

[75]Some British scientists had been moving on a path similar to Wiener's. Among these were Kenneth Craik, who strongly influenced McCulloch, W. Grey Walter, and Ross Ashby. The National Physical Laboratory at Teddington, England was a center for work in robotics. Wiener was made more aware of these developments during his visit to England in 1947.

States.[76]

Later Wiener wrote:

> I found the British atmosphere entirely ripe for the assimilation of the new ideas which I was then developing concerning control, communication and organization. . . . Actually, the idea of a comprehensive book on these subjects began to come to a head when I reached Paris.[77]

While in Paris, Wiener was asked by the representative of a French publisher if he would "write up [his] ideas concerning communication, the automatic factory, and the nervous system as a booklet for his series?"[78] Wiener was having some financial problems at the time and agreed to write the book. At the same time, writing it was, as he later observed, "a burden off my soul."[79] Wiener returned to Mexico during the summer of 1947 and quickly wrote the whole book, working during the mornings, as during the afternoons he continued to research with Rosenblueth.

During the summer Wiener decided to call the synthesis of his ideas Cybernetics. He described the genesis of the term in the Introduction to the book:

> . . . 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,

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[76] N. Wiener, Cybernetics, p. 23.

[77] N. Wiener, Iamm, p. 315.

[78] Ibid., p. 316.

[79] Ibid., p. 329.

> 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 forces 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 χυβερνήτης 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 1968 [reference given], and that *governor* is derived from a Latin corruption of χυβερνήτης. 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.[80]

The topics presented in *Cybernetics* spanned a remarkable range: reversible versus non-reversible time, statistical mechanics, time series analysis, information, communication, feedback theory, computing machinery, the nervous system, the perception of gestalt, and the relation of these areas to psychopathology, language and society. Although all these topics may be broadly categorized under "robotics," their real center was Norbert Wiener's interest. Concerning the book, Wiener later wrote, "*Cybernetics* was a new exposition of matters about which I had never written authoritatively before and, at the same time, a miscellany of my ideas."[81] This

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[80] N. Wiener, *Cybernetics*, pp. 11-12. Wiener was apparently unaware that Ampere, in 1843, had used the term *cybernétique* to designate the science of government (André-Marie Ampere, "Essay sur la Philosophie des Sciences, Part 2," [Paris, 1843]; cited by Otto Mayr, *The Origins of Feedback Control* [Cambridge, Massachusetts: The M.I.T. Press, 1970], p. 2).

[81] N. Wiener, *Iamm*, p. 332.

amazingly candid and telling statement should be well marked by anyone attempting to understand the "meaning" of Wiener's cybernetics presentation. Cybernetics was a collection of Wiener's ideas relating to robotics, a "miscellany," rather than an integrated whole. He showed that certain aspects of the functioning and behavior of "animals and machines" could be understood in common terms. This demonstration was useful in that it did, as Wiener had hoped, help to provide some common terminology between the sciences; the terms "feedback" and "information" are outstanding examples. But valid as these contributions were, Wiener's observations on the equivalences between organisms and machines were scattered over a small number of selected topics in which Wiener had the greatest interest. Indeed, it could hardly be claimed that a book containing almost no biology had very much to say about that subject at all. Wiener's observations on selected topics in "control and communication"---terms so inclusive almost anything could be discussed under them---did not constitute a synthesis of these topics so much as a collection of Wiener's ideas relating to them. This is not to say that many of the ideas Wiener presented in Cybernetics were not profoundly interesting, but that there was not as much unity in the work as one might suppose from the fact that a single new word was coined by the author to cover all its contents. Thus Wiener's work was a "synthesis" more in that it was a collected expression of his thought rather than a true integration of topics with a well delineated theme. As just noted, the supposed theme of "control and communication" was too broad to be meaningful. There is virtually no activity of any system which is excluded from these categories. Thus, the "synthetic" statements of Cybernetics were not really as synthetic as they first seemed. This problem is illustrated in Wiener's statement that,

> . . . the many automata of the present age are coupled to the outside world both for the reception of impressions and for the performance of actions. They contain sense organs, effectors, and the equivalent of a nervous system to integrate the transfer of information from the one to the other. They lend themselves very well to description in physiological terms. It is scarcely a miracle that they can be subsumed under one theory with the mechanisms of physiology.[82]

The "one theory," cybernetics, was not one theory at all, but a collection of ideas, and not a complete one at that. Yet the somewhat sensationalistic aspects of the book, like the early Macy meetings, had its positive effects in drawing attention of researchers in diverse fields to the topics and methods described in the book. Wiener was thus successful in drawing researchers out of their highly specialized niches and enabling them to benefit from progress in other fields.

In terms of the present work, the most serious failure of Cybernetics was its curious omission of many of the most important clarifications and distinctions made in the earlier "Behavior, Purpose and Teleology" paper. Thus the work was important for what it did not say about negative feedback as well as what it did say. What it did say about feedback was largely contained in the Introduction and in Chapter IV, "Feedback and Oscillation." Wiener commenced this chapter with a discussion of feedback in terms of

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[82] N. Wiener, Cybernetics, p. 43.

medical problems, such as the purpose tremor, which had played such a great role in the generation of "Behavior, Purpose and Teleology." He briefly explained the flyball governor and the thermostat in terms of negative feedback. These discussions were qualitative, introductory in nature, and presented in three pages. At this point Wiener suddenly shifted into a highly technical, but erratic, presentation of certain mathematical aspects of feedback theory, such as stability criteria in terms of frequency characteristics of the disturbance. These portions consisted of topics which might have been presented in an advanced servo-mechanisms text of the period and thus required a highly technical background both in engineering and mathematics for their understanding. The technical portions were interspersed with highly suggestive implied applications. For example, after a technical discussion of multiple feedback stabilization in terms of the Minorsky problem related to steering engine control (see above, pp. 182-183), Wiener proceeded directly to a discussion of multiple feedbacks in voluntary and postural feedback behavior in the human body.[83] It is not clear, however, how the mathematics previously presented related to the discussion of human behavior. It is suggested, implicitly, that the two topics are related in terms of multiple feedback and it is implied that the same mathematics may be applied to help explain the vastly more complicated phenomena of human behavior. This connection is never made explicit, however. In one sense the failure to make this

[83]Ibid., pp. 105-108.

connection explicit is quite understandable. The fact was that no explanation of this behavior was known and Wiener could not have made the connection explicit if he had wanted to. He was attempting to stimulate investigation into the possibility that feedback analysis might be useful in explaining this behavior, and in this sense, the "suggestive" nature of the chapter was quite valuable. This, and other examples throughout the chapter, helped to stimulate interest in the feedback methods as a possible means to explain many complex phenomena in diverse fields. On the other hand, the chapter easily gave the impression that these complex topics were, in fact, understood in terms of the mathematics Wiener utilized; that the presentation, and, in fact, the whole synthesis, was on a stronger mathematical foundation than it really was. This problem was especially acute for the many readers with a non-technical background who were not prepared to evaluate the relevance of the mathematical sections.

Wiener concluded the chapter with a brief discussion of non-linearity and an extremely superficial discussion of homeostasis, which he stated required "too detailed a knowledge of general physiology" for an "introduction to the subject."[84] In what sense, if any, the work was introductory, however, is highly questionable. Wiener's presentation was extremely personal: he went into great detail in those areas in which he had expertise and virtually skimmed over the others. The feedback chapter was especially

[84] Ibid., p. 115.

illustrative of this tendency.

A presentation of feedback more relevant to the program of cybernetic analysis was given in the Introduction. Here Wiener described how the "Behavior, Purpose and Teleology" paper came to be written and argued that voluntary activity in humans might well be explainable in terms of negative feedback. He gave a brief description of the technique of negative feedback itself. But the presentation was short, only three pages out of the twenty-nine in the Introduction, and was not nearly as powerful in delineating the role of negative feedback in terms of behavior as was the 1943 paper. The fire control project, and the resultant writing of "Behavior, Purpose and Teleology" was used by Wiener more or less as one illustration of the process by which he became convinced that control and communication in the animal and the machine were part of a common theory. The Introduction was filled with many other such illustrations and observations. Thus, the most relevant portion of Wiener's work for cybernetic analysis played a relatively minor role in his presentation.

The first chapter of Cybernetics, entitled "Newtonian and Bergsonian Time," provided an ideal opportunity for Wiener to further pursue the ideas of the "Behavior, Purpose and Teleology" paper, but he did not do so. In the chapter, Wiener argued that a misleading picture of nature had arisen from the dominance of classical Newtonian physics in which events and time were both reversible. In Wiener's view all processes and events were directed in time by the second law of thermodynamics. Wiener wrote:

> There is not a single science which conforms precisely to the strict Newtonian pattern. The biological sciences certainly have their full share of one-way phenomena. Birth is not the exact reverse of death, nor is anabolism---the building up of tissues---the exact reverse of catabolism---their breaking down. . . . The individual is an arrow pointed through time in one way, and the race is equally directed from the past into the future.[85]

Wiener referred to irreversible time as "Gibbsian" or "Bergsonian" time, writing:

> This transition from a Newtonian, reversible time to a Gibbsian, irreversible time has had its philosophical echoes. Bergson emphasized the difference between the reversible time of physics, in which nothing new happens, and the irreversible time of evolution and biology, in which there is always something new. The realization that the Newtonian physics was not the proper frame for biology was perhaps the central point in the old controversy between vitalism and mechanism. . . .[86]

Finally, Wiener argued that because of their operation, in which response to the enviornment was possible,

> the modern automaton exists in the same sort of Bergsonian time as the living organism; and hence there is no reason in Bergson's considerations why the essential mode of functioning of the living organism should not be the same as that of the automaton of this type. Vitalism has won to the extent that even mechanisms correspond to the time-structure of vitalism; but as we have said, this victory is a complete defeat, for from every point of view which has the slightest relation to morality or religion, the new mechanics is fully as mechanistic as the old.[87]

Thus, Wiener was arguing that natural systems and "the modern automaton" are both directed in time. But the source of this direction is not at all clear. Wiener seemed to be saying in the first half

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[85] Ibid., p. 36.

[86] Ibid., pp. 27-38.

[87] Ibid., p. 44.

of the chapter that directedness stems from thermodynamic considerations, but he failed to make any connection between these considerations and the directed responses of organisms and servo-mechanisms to environmental disturbances. As a result, the reader is left puzzled and confused. It would seem that this chapter presented an ideal opportunity for Wiener to advance the argument of the "Behavior, Purpose and Teleology" paper, that directedness can be associated with purposeful and teleological behavior, and that negative feedback can be a crucial element in accounting for this behavior. The paper, however, was not mentioned or even cited, and negative feedback is also not mentioned or described. The later chapter on feedback itself makes no reference to these earlier problems. Cybernetics, in fact, virtually ignored the contents of the 1943 paper with its important clarifications of behavior and function. The book was much less effective than it otherwise might have been; Wiener's presentation of directedness in nature was terribly confused, while the detailed treatment of feedback in the later chapter suffered from the lack of conceptual development which the 1943 paper offered.

The presentation of Cybernetics, though frequently brilliant in clarity and insight, was hampered by an overall lack of continuity and Wiener's constant tendency to digress. The general criticism of Wiener's writing by Hans Freudenthal, although overly severe in some respects, is worth noting:

> His style was often chaotic. After proving at length a fact that would be too easy if set as an exercise for an intelligent sophomore, he would assume without proof a profound

> theorem that was seemingly unrelated to the preceding text, then continue with a proof containing puzzling but irrelevant terms, next interrupt it with a totally unrelated historical exposition, meanwhile quote something from the 'last chapter' of the book that had actually been in the first, and so on. He would often treat unrelated questions consecutively, and although the discussion of any one of them might be lucid, rigorous, and beautiful, the reader is left puzzled by the lack of continuity. All too often Wiener could not resist the temptation to tell everything that cropped up in his comprehensive mind, and he often had difficulty in separating the relevant mathematics neatly from its scientific and social implications and even from his personal experiences. The reader to whom he appears to be addressing himself seems to alternate in a random order between the layman, the undergraduate student of mathematics, the average mathematician, and Wiener himself.[88]

Freudenthal criticized Cybernetics as ". . . a collection of misprints, wrong mathematical statements, mistaken formulas, splendid but unrelated ideas, and logical absurdities." In noting that few, if any reviewers voiced any criticism of the book, he remarked that "mathematical readers were more fascinated by the richness of its ideas than its shortcomings."[89]

The fact that Cybernetics became a best seller in spite of its faults greatly aided the dissemination of the many new ideas and technologies which emerged from World War II. Wiener's style, whatever its faults may have been, generated an immense amount of interest in these new and important ideas by scientists and

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[88] Hans Freudenthal, "Wiener, Norbert," in Charles Coulston Gillispie, ed., Dictionary of Scientific Biography, vol. 14 (New York: Charles Scribner's Sons, 1976), p. 344.

[89] Ibid. For von Newmann's review of Cybernetics, see Physics Today (May, 1949), pp. 33-34.

non-scientists alike. The ideas which had been circulating among a relatively small group of scientists suddenly exploded to reach a huge audience. Soon Scientific American was devoting an entire issue to the developments.[90] Wiener was urged to write another work in a more "popular" vein to describe for the public at large the "meaning" of cybernetics. The work which resulted, The Human Use of Human Beings,[91] became another best seller.

Thus the pattern of lumping all robotic developments together, a pattern which, as has been shown above, started in the early Macy meetings, continued and became even more pronounced after the publication of Cybernetics. The impact of this pattern on the dissemination and utilization of the feedback concepts was both positive and negative. On the one hand, as we have seen, the great public success of Wiener's work circulated the feedback concepts much more widely than might otherwise have been the case. On the other hand, researchers needed to sort out the concepts from the jumble of sometimes over-sensationalized ideas and, at the same time, determine how actually to use the methods in particular scientific applications. This process occurred slowly but surely during the 1950's, especially in biology, as the feedback theories crept into basic research.

By the time Cybernetics became widely circulated, then, negative feedback had become firmly entrenched as one of the many

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[90] Scientific American 197 (September 1952).

[91] N. Wiener, The Human Use of Human Beings (Boston: Houghton Mifflin, 1950).

new robotic techniques which had emerged from the technological explosion of the 1940's. The technique would become incorporated at an accelerating rate in various areas of research. While the feedback methods were perceived to have great conceptual significance, the significance was the same as that applied to the robotic developments in general. This lack of differentiation may be attributed to the massive number of developments in a wide range of fields which had to be assimilated and understood in a very short period of time. The process of sorting out the significance of these developments is, in fact, still occurring. The present work is an attempt to contribute to this process.

CHAPTER SEVEN

CONCLUSION

Norbert Wiener's extraordinary innate abilities, upbringing and education enabled him to become the central figure in the spread of feedback techniques during the period of the 1940's. As an outstanding child prodigy and with the intense tutoring of his father, Wiener came to view scholarship as a kind of religion. As an adult, he successfully attacked fundamental problems in fields ranging from biology and philosophy to mathematics, physics and engineering. His approach to these problems was immensely aided by his multidisciplinary talents and background as he had developed professional proficiency in most of the different fields in which he would work.

Thus, when, during World War II Wiener became acquainted with the engineering technique of negative feedback, he was well prepared to discover the many ramifications of its application. In particular, his background in biology and philosophy enabled him to see that the technique could be viewed as a general means of control to bring about what is usually termed purposeful behavior. Wiener became the driving force behind the "Behavior, Purpose and Teleology" paper, co-authored with Arturo Rosenblueth and Julian Bigelow, which resulted from the wartime project and which concisely presented the profound relationship between negative feedback, and purposeful and teleological behavior.

The meetings at Princeton, in January of 1945, and those sponsored by the Josiah Macy, Jr. Foundation and the New York

Academy of Sciences, which were in large part stimulated by the "Behavior, Purpose and Teleology" paper, were the primary vehicle for the initial dissemination of negative feedback by the larger scientific community. Wiener's crucial role in these meetings has been developed in Chapter Six. In examining his involvement, it is clear that Wiener was certainly the central figure responsible for the spread of the feedback ideas, but it is equally clear that the process was only successful because Wiener was at the center of an enormously talented and gifted group of scientists. Figures such as Gregory Bateson, Julian Bigelow, L. J. Henderson, Margaret Mead, Warren S. McCulloch, Walter Pitts, Arturo Rosenblueth, John von Neumann and others too numerous to mention demonstrated their ability to step outside the straits of their individual disciplines and to attempt, under very trying circumstances, to assimilate material from fields outside of their professional employment. Even within this talented group, Wiener stood above the rest in this regard. Von Neumann and Pitts may have had equal abilities, but Wiener's emphasis of subject material, his personality, and manner of presentation made him the focus of the group; the "central figure" in the dissemination process described in the first thesis of the present work: namely, "Norbert Wiener was the central figure responsible for the dissemination of the technique of negative feedback from engineering to the larger scientific community."

The difficulties of communication and definition of terms which surrounded the early meetings may accompany virtually any effort involving the transmission of unfamiliar information between widely differing disciplines. It does seem, however, that in the case of the early Macy meetings, the situation was aggravated by the lack of a serious attempt to educate the conferees on the basic terminology being employed and the underlying concepts being discussed. As McCulloch later said, years after the conferences were over, "We finally learned that every scientist is a layman outside his discipline and that he must be addressed as such."[1] It may be difficult for and even damaging to the pride of an advanced professional to act as a "layman" in an academic setting, but such a transformation is necessary if the successful dissemination of ideas across widely differing disciplines is to occur to a significant degree.

The second thesis of this work stated that "Norbert Wiener was the central figure responsible for the transformation of negative feedback from a technique used to control mechanical devices into a powerful conceptual tool, of general application, having to do with the organization of behavior." The fact that Wiener's initial ideas on feedback were presented (in his joint paper with Rosenblueth and Bigelow) in a philosophical journal is evidence of the conceptual, rather than purely technical manner in which Wiener viewed feedback. In presenting the role of negative feedback in this manner, and subsequently pursuing these ideas in a similar fashion with leading scientists, Wiener not only helped to make negative feedback

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[1]See above, p. 261.

available as a technique to the various sciences outside of engineering, but also transformed the technique into a general concept to deal with "purposeful" behavior in many fields. The dissemination and transformation processes were thus linked by Wiener, mutually reinforcing the significance of both. It would seem from Wiener's successes that the linking of the conceptual value of a new development with its technological uses is an effective means for facilitating the dissemination of the new idea or technique. If Wiener is a valid example illustrating the manner in which conceptualization can occur, it certainly seems that it helps to be a genius. It was Wiener's incredibly wide background which helped him to interact with professionals in almost every field. While it may be unrealistic to expect to cultivate such abilities routinely, the current trend toward continuing education for the adult population would seem to make this goal more attainable.

Though Wiener was the central figure in the dissemination and transformation of negative feedback, he was also the central figure in its obfuscation within the larger range of robotic topics which came to dominate his interest. Chapter Five considered the extremely significant robotic developments which occurred during the World War II period and in which Wiener, with his multifarious interests could hardly fail to take an interest. This growing interest in the robotic field was reflected in Wiener's Cybernetics. Since Wiener first began using it in 1948, the term "cybernetics" has been a source of confusion for both the scientific community and the general public. It has never been certain whether cybernetics is a science, a technique, a philosophy, a point of view, or some combination of these. Having now examined the events which resulted in the appearance of Cybernetics, the sources of this confusion are clear. Wiener utilized his 1948 work as a vehicle to collect his thoughts on a multitude of subjects, most of which could be viewed as having some relation to robotics. The work was a "miscellany" of his ideas, including those relating to the subjects of computation theory, time series analysis, information and feedback theory, gestalt psychology, psychopathology, neurophysiology, statistical mechanics, thermodynamics and the irreversibility of time. The great problem in attempting to view this work as a synthesis is that all these subjects are imposing fields in their own right. It is true that they all may have a bearing on automation, robotics and the mechanical view of life, but this is true of almost every science. When one seeks to discover what it is necessary to know in order to mechanically replicate human capabilities it soon becomes apparent that hardly any aspect of human knowledge can be omitted. Indeed, the almost totally inclusive terms, "communication and control," with which Wiener chose to subtitle his work is evidence for this observation. Hence cybernetics cannot be viewed as a distinct science.

With his strong background in many fields of science and his unusual ability to perceive analogies between problems in various fields, Wiener was able to jump freely from one subject to another. Whether or not the pattern or the interconnections between the topics that Wiener seems to have seen, and which enabled him to class all his ideas under the single term "cybernetics," really existed or not is difficult to assess. If the pattern did exist, however, Wiener did not communicate it in Cybernetics which, as we have seen, was more a collection of only tenuously related observations. The class of subjects which Wiener treated in Cybernetics has come to be associated with the term cybernetics. But, as noted above, this class was almost totally inclusive, leading one writer to state that the term cybernetics "almost becomes another word for all the most interesting problems of the world."[2] Wiener may very well have used the term as a heuristic device to help arrange his own thoughts.

I have attempted to clarify the usage and meaning (or lack of it) of the term cybernetics in order to better draw the distinction between it and the major subject of this dissertation, negative feedback. The fact that negative feedback was included as one of the many topics included in Wiener's Cybernetics and was discussed in a similar manner in the events leading to its appearance greatly muddled the understanding of the role of negative feedback in scientific explanation. This role is extremely important and has great implications for the scientific world view. Chapter Two of this work represents my attempt to clarify this role and its importance and thus deals with the third thesis of this dissertation; that the "utilization of negative feedback may be

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[2]J. R. Pierce, Symbols, Signals and Noise: The Nature and Process of Communication (New York: Harper & Bros., 1961), p. 227.

interpreted to provide a bridge between holistic and reductionistic explanation, and this understanding leads to a radical revision of the scientific view of goal-directedness in nature."

I argued in Chapter Two that the outstanding characteristic of negative feedback in relation to scientific explanation is its ability to provide a mechanical basis for adaptive goal-directed behavior; behavior in which a system tends toward a certain state or goal even while being disturbed by external influences. This type of behavior previously had no mechanical basis of explanation. Since adaptive goal-directed behavior is an important characteristic of living organisms, and, in fact, has even been viewed as a defining characteristic of life, negative feedback has the potential to be an important tool in the understanding of life processes. Some progress has already been made in this direction.

The behavior of a system is a holistic property. It is the integrated result of the functioning of the parts of the system. The negative feedback mechanism, in accounting for the behavior of the system in terms of a particular interaction of its parts, thus does provide a bridge between holistic and reductionistic explanation in that reductionism is usually associated with understanding in terms of the parts of a system, and negative feedback utilizes the functioning of the parts to account for the holistic property of the system's overall behavior.

Since goal-directedness has been traditionally associated with the now inadmissible doctrine of final cause, it has become impossible to view goal-directedness as being a real characteristic of any system aside from certain "goal-intended" actions of the higher life forms. The recognition of the ability of the negative feedback mechanism to bring about goal-directedness in systems as a result of the necessary mechanical functioning of the system rather than as a result of final causes allows the extremely useful concept of goal-directedness to return to scientific explanation. At the same time, the determination of whether a system is goal-directed and whether, indeed, negative feedback mechanisms are responsible for this behavior in a functional sense, is an experimental question. The program of "cybernetic analysis" has been advanced to formally state an experimental method to make these determinations. This method is commonly utilized in science today although it has not, to my knowledge, been previously formalized in this manner.

If one accepts both the program of cybernetic analysis defined in Chapter Two as being representative of the manner in which negative feedback is used in science today, and also the importance of this usage, it becomes relevant to ask in what way Wiener's work contributed to cybernetic analysis. As was shown in Chapter Two, the "Behavior, Purpose and Teleology" paper, in which Wiener played the major creative role, provided the principal concepts for the program. Most significantly, the 1943 paper distinguished between functional and behavioral studies and between goal-directed behavior with and without feedback. Wiener's subsequent clarification of the importance of experimental factors in the determination of goal-directedness is also important for the statement of the modern program. That some of these distinctions were subsequently lost during the development of cybernetics and general systems theory is evident from the fact that the "equilibrium problem" (see above, pp. 53-56) is still a problem in recent literature even though it is quite readily solved in terms of the distinctions made by Wiener and his colleagues.

Cybernetic analysis represents a modern statement and an extension of the basic ideas from which Wiener went on to develop his larger and more diffuse "cybernetics." It has been argued here that the term cybernetics has little if any meaning. On the other hand, there is no established term to designate the important idea that adaptive goal-directed behavior, as defined in Chapter Two, can be analyzed and accounted for on the basis of the negative feedback mechanism. The reader will recall that throughout the present work the terms "feedback ideas," or "feedback concepts" have had to be employed when it was desired to convey this idea. Given the problem of this lack of terminology, and the historical development of the "feedback concepts" as presented in this work, I now propose that the term "cybernetic analysis" be used to designate that type of problem analysis in which the hypothesis of a negative feedback mechanism is made to account for adaptive goal-directed behavior. A more complete description of cybernetic analysis is, of course, given in Chapter Two. But whether or not one agrees in every detail with the particular method of statement for the program as given here, the type of analysis it delineates in terms of behavioral and functional studies, adaptive goal-directed behavior, and negative feedback, is sorely in need of a name. Even though Wiener did not fully establish the program in his work, he contributed its most essential concepts. It seems appropriate, then, to designate it with the term he derived to indicate the importance of negative feedback (see above, p. 278).

Since the term cybernetics seems to have lost whatever meaning Wiener originally intended for it, its usage with reference to the program of cybernetic analysis will not only give it renewed meaning, but at the same time help to fill a void by naming one of the as yet unnamed great developments in modern science. Rather than invent a new term to describe this development, it seems fitting to utilize the word derived by the individual who contributed most to the elucidation of the role of negative feedback in scientific explanation, Norbert Wiener. Perhaps, in the end, if a larger pattern, closer to the one Wiener claims to have perceived really does exist, the meaningful definition of cybernetic analysis will, in fact, help to bring it to light.

Epilogue

After the publication of Cybernetics Wiener became an international celebrity. He travelled widely, as he loved to so and spoke on an innumerable variety of topics, which he perhaps loved to do more. All the while he remained a professor of mathematics at the Massachusetts Institute of Technology. Wiener remained active professionally, although, characteristically, he seems to have pursued his own interests rather than attempting to keep up with contemporary developments. In 1958 the first journal devoted to cybernetics, Cybernetica, appeared and the term reached the peak of its popular and scientific usage in the early 1960's, except in the Soviet Union where the usage continued into the 1970's. Subsequently the term cybernetics became less prevalent as individual journals devoted to computing, pattern recognition, control theory and the other topics treated by Wiener appeared. Wiener could feel satisfied that he had done a great deal to bring attention to these subjects.

The man who once wrote, "I became a scholar partly because it was my father's will but equally because it was my internal destiny," continued to exhibit his scholarship in conversation and diverse writings until the end of his life. While travelling in Europe he died of a heart attack on March 18, 1964, leaving his wife Margaret and his two daughters, Barbara and Peggy. He would have been 70 years old that November.

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