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CORRESPONDENCE *Oct., 1951*

N. WIENER - MC 22

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Cybernetics. BY NORBERT WIENER. New York: The Technology Press, John Wiley and Sons. 1948. 194 pp. \$3.00.

CYBERNETICS is Professor Wiener's name for the entire field of control and communication theory, whether in the machine or in the animal, and signifies the art of pilot or steersman. The new science of cybernetics, introduced by this important book, appears as the direct outgrowth of the work of an informal partnership of scientists and engineers from widely separated fields of specialization. The Introduction gives an account of this cooperative effort—a story of special significance for those who are engaged in an interdisciplinary approach to problems in economics.

It is not easy to assess the potential value of cybernetics for economics, or for that matter to predict confidently even the general nature of the future relationship between the two fields. This very difficulty is a topic for cyberneticists, since it involves the problem of communication between professions, and is discussed in quite general terms in a brief final chapter on "Information, Language, and Society." Indeed, the only material of obvious relevance to economics appears in the final chapter where Wiener lets off a bit of steam in a fashion rather out of character with the balance of the book. The brief but provocative economic argument is made in terms of the concepts of the von Neumann-Morgenstern theory of games with the conclusion that the extreme lack of efficient homeostatic processes in a system based on selfish competition must lead to social and economic upheavals like those actually experienced. The critical social mecha-

nism is singled out in these words: ". . . Of all of these anti-homeostatic factors in society, the control of the means of communication is the most effective and most important" (p. 187).

There is one highly mathematical chapter, entitled "Time Series, Information, and Communication." Since Wiener is responsible for a good deal of the most fundamental mathematical work in these fields, over a period of many years, he has much to say on these topics that is of importance for economists. The treatment is so concise, however, that it would be extremely hard for anyone who is not already expert in the field to get much from it. This chapter and one other, on "Feed-back and Oscillation," help to set the tone, but the mathematical details fortunately seem quite unimportant for an understanding of the remainder of the book.

Nearly every reader, whatever his field of specialization, will find the central portion of the study of real interest. This consists in an effort to explain a wide variety of functions of the nervous system in terms of theories and mechanisms taken over from communications engineering and related fields. Thus, memory, learning, and thinking in the individual are related to relays, chemicals, and mathematical logic as they enter in consideration of modern communications systems and high-speed electronic computers. Coding and scanning are related to the psychological problems of gestalt and universals. Feed-back, oscillation, and capacity are introduced naturally in a discussion of nervous disorders and psychopathology. The coverage in topics is so broad, in fact, that one is apt to be disappointed and even a bit surprised not to find some attempt at an explanation of the mechanism that determines human preferences and calculates utility functions.

This reviewer will hazard the guess that the most likely future connection between cybernetics and problems of economics will in some fashion be concerned with individual differences. Wiener has touched on this point indirectly in his economic argument when he employs such terms as: nonsocial animals, knaves, fools, altruistic, and selfish. Some of these differences might conceivably be treated as variables in a utility function, but it seems more reasonable to give them a primary role in the economic model so that they cannot be overlooked so easily.

It must be noted that the book is marred by numerous typographical errors; these are especially troublesome in the mathematical formulas in the chapter on time series. References are infrequent and incomplete and there is no index. These defects are apt hardly to be noticed, at least on first reading, in the reader's haste to get through to the end of the racy, stimulating story.

M. M. FLOOD

The RAND Corporation

[ca. Oct., 1951]

Dear Professor Wiener,

On several occasions I have met up with cybernetics — in your book, in conversations.

Subjects such as the relation between the communication structure & government, information, communication interest me.

I am by profession a pure mathematician (topology). But I feel that cybernetics is a general field of important application of math — perhaps even of structures as abstract as topology & partially ordered sets.

I shall be N. Y. this year (at Columbia). Who are the cyberneticians around here? And what are the important papers that would give me a picture of full scope of work up to now?

Sincerely,

S. Stein

319 W 105

NY 25 NY

[ms 10-18-51]

[ca. Oct, 1951]

MEMORANDUM TO MEMBERS OF THE
JOSIAH MACY, JR. FOUNDATION CONFERENCES

RE: GUESTS FROM ABROAD

Professor E. P. Sharpey-Schafer
Department of Medicine
St. Thomas Hospital Post-graduate Medical School
London, England

TO ATTEND: The Conference on SHOCK AND CIRCULATORY
HOMEOSTASIS, October 22-23, 1951.

ARRIVAL: October 19, 1951

DEPARTURE: Approximately November 2, 1951

CONTACT THROUGH: Dr. Ephraim Shorr, Associate Professor
of Medicine, Cornell University Medical
College, 1300 York Avenue, New York, N.Y.

Professor Schafer has been primarily interested in factors concerned with the regulation of cardiac output in man, under both normal and abnormal conditions. He has made numerous studies correlating changes in peripheral blood flow with changes in cardiac output and in blood pressure. Among the Techniques which he has employed are a newly developed sensitive measurement of cardiac output using the direct Fick procedure, and catheterization of the heart and various large blood vessels.

During the last war, he demonstrated that the hypotension which accompanies post-hemorrhagic fainting is related to vasodilation in skeletal muscle. He also showed that change in venous pressure, related to increased or decreased filling of the heart, was the principal factor affecting cardiac output under various conditions, such as hemorrhage, diabetic coma and anemias.

More recently, he has been interested in the pulmonary circulation, especially with reference to congenital heart disease.

Professor Schafer will be accompanied by his wife, Dr. Sheila Howarth, Senior Research Fellow, Institute of Cardiology, St. Thomas Hospital Medical School, London, England.

Dr. Frank Winton
Professor of Pharmacology
University College
London, England

TO ATTEND: The Conference on RENAL FUNCTION, October
18-19, 1951

ARRIVAL: October 17, 1951

DEPARTURE: October 26, 1951

CONTACT THROUGH: Dr. Robert F. Pitts, Professor of Physiology,
Cornell University Medical College,
1300 York Avenue, New York, N.Y.

Dr. Winton has for many years worked in the field of renal physiology and is perhaps best known for his development of the pump-lung-kidney preparation. His interests have included the dynamics of the renal circulation, and it is on this general subject that he will introduce a session at the Macy Conference on Renal Function. He will also speak at the October 25th meeting of the Harvey Society on "Hydrostatic pressure affecting flow of urine and blood in the kidney."

Dr. G. W. Harris
Laboratory of Physiology
University of Cambridge
Cambridge, England

TO ATTEND: The Conference on ADRENAL CORTEX, November
15-16, 1951

ARRIVAL: November 14, 1951

DEPARTURE: November 28, 1951 (Approximately)

CONTACT THROUGH: Dr. C.N.H. Long, Dean, Yale University
School of Medicine, 333 Cedar Street,
New Haven, Conn.

Dr. Harris has done a great deal of work on the regulation of ACTH secretion. This work, if proved correct, will link up the control of ACTH secretion through a neuro-humoral agent, reaching the gland through the portal systems of blood vessels. At the Conference on Adrenal Cortex, Dr. Harris's topic will be "The Hypothalamus and Regulation of ACTH secretion."

Frank Fremont-Smith, M.D.
Medical Director

BOOK REVIEW: GP, September 1951

CYBERNETICS. Ed. by Heinz Von Foerster. Pp. 251. Price, \$3.50. The Josiah Macy, Jr. Foundation, New York.

The transactions of the Seventh Conference on Cybernetics in March, 1950, have been collected, edited, and published under the auspices of the Josiah Macy, Jr. Foundation. The Conference, in bringing together a group of outstanding scientists from different fields in informal friendly discussion, has attempted to overcome some of the blind spots in scientific investigation, intercommunication, and dissemination of information resulting from extensive specialization. It is hoped that scientists with a variety of technical backgrounds who are concerned with a common problem, may through the informal interchange get new ideas or correct old ones, and find new ways for cooperation and communication.

Seven discussions concerning the common problem of language and communication are presented from different specialized approaches. The ideas and theoretical viewpoints are presented in a nascent and developing state. They are extremely complex and while advanced, are highly problematical and theoretical. A considerable degree of specialized knowledge, and a willingness for slow, serious, reflective contemplation are necessary to derive profit from reading this book. It is a valuable book for a number of specialized categories such as semanticists, philologists, psychologists, psychiatrists, anthropologists and a variety of other disciplines.

Steven Hammerman, M.D.

Brooklyn College

Bedford Avenue & Avenue H, Brooklyn 10, N. Y.

Department of Biology

October 1, 1951

Professor Norbert Wiener
Department of Mathematics
Massachusetts Institute of Technology
Cambridge 39, Massachusetts

Dear Professor Wiener:

This month, the Installation of a new Sigma Xi Club will be held at Brooklyn College in the presence of Professor George A. Baitzell, Executive Secretary of The Society of Sigma Xi. We are exceedingly anxious to bring men of distinction to the Brooklyn College campus, the largest co-educational college in America, through the medium of Sigma Xi. Our mathematics department is large and one of the most active units of the college and the science department staffs include approximately 70 members of Sigma Xi.

At the preliminary meeting last week I was instructed by unanimous vote to extend our invitation to you to address the Sigma Xi Club. This will be an historical occasion in a way because it is the first regular meeting of the Sigma Xi group at Brooklyn College. I hope that it will mark the birth of another active and productive organization under the auspices of The Society of Sigma Xi. We sincerely hope that our earnest desire to hear you on some phase of Cybernetics will appeal and fit into your calendar. We will be happy to arrange the meeting at your convenience during the week of November 26th or the week of December 14th. It will be a Day meeting and preferably on a Monday or Wednesday between 12 and 2 P.M. or at the Tea Hour later in the afternoon.

We regret that the Sigma Xi Club is not in a position to offer an honorarium but of course will reimburse you for all expenses. We sincerely hope that the group here at Brooklyn College may have the privilege of hearing you discuss Cybernetics this Fall and that it will prove to be a pleasant occasion for you.

The Sigma Xi Club will be very grateful for a favorable response to our invitation to address its first regular meeting.

Sincerely yours,

Ralph Holt Cheney

Ralph Holt Cheney, Chairman; Program Committee
Sigma Xi Club, Brooklyn College

RHC:bhs

[ans 10-10-51]

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 1, 1951

Mr. Hayward Cirker, President
Dover Publications, Inc.
1780 Broadway at 57th Street
New York 19, N.Y.

Dear Mr. Cirker:

I am afraid that my recollection of Kestelman's book on Modern Theories Of Integration is not sufficiently definite to enable me to give an opinion on the value of republishing it. As I am out of the country, I suggest that you send a copy to Professor N. Levinson or to Professor Hurewicz, both in the mathematics department of M.I.T. for an opinion.

Your letter of June 20 just reached me the other day after following me about on two continents, which explains my belated answer.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D. F. Mexico
October 1, 1951

Mr. Newhall Douglas
Extraco
517 Sherwood Road
Ho-Ho-Kus, New Jersey

Dear Mr. Douglas:

I have a dim recollection that I have heard of something corresponding to your letter of June 19, in the work of a Mr. Benson, resident in Southern California and a descendant of the former archbishop of Canterbury. I am here in Mexico away from all my records; and this vague lead is perhaps the best thing I can give you. Your inquiry of June 19, just reached me after following me about on two continents, hence this belated answer.

Sincerely yours,

[Ans 11-10-51]

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 1, 1951

Mr. Clifford G. Pollock
214 N. Penna Ave.
Morrisville, Pa.

Dear Mr. Pollock :

This is merely to tell you that your letter of June 21 only arrived the other day after following me about two continents. Naturally I am sympathetic with any use you may make of my book, but I am afraid I can take no active part with giving you any help in the matter. Each of these requests is of course an isolated thing from the point of view of the person making it, but I assure you that it is far from isolated from the point of view of the author.

I am forced in selfdefense to adopt a general policy of not participating in any work involved in such discussions as you are holding. For if I ^{were} to adopt such a policy, I should have time for nothing else.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 1, 1951

Cher M. Schuetzenberger:

Quand j'étais à Thonon les Bains, j'ai attendu de vous voir chez nous ou de recevoir des nouvelles de votre part. Je voudrais bien savoir l'état actuel de notre livre, aussi bien que telle intention vous pourriez avoir de venir au Mexique. Si vous venez, tout est prêt dans le laboratoire à vous recevoir. On trouve là une groupe très internationale de jeunes gens de science qui travaillent avec mon ami, Dr. Rosenblueth.

Nous sommes établis ici dans un petit appartement pas loin du Bois de Chapultepec, qui correspond au Bois de Boulogne. Nous sommes épuisés par deux semaines de fêtes universitaires commémorant la fondation de l'université de Mexico il y a quatre siècles. Les Mexicains ont le sens des fêtes, mais pas le sens de proportions là-dedans. A cause de ces fêtes et aussi de l'altitude d'à peu près 2500 mètres, je suis tout à fait épuisé, mais j'ai des idées sur une dynamique statistique universelle que je crois pouvoir appliquer même à la théorie de la physiologie.

Il y a un mois que je suis passé par les Etats-Unis ou je n'ai été resté qu'une semaine. J'ai trouvé tout très bien chez ma fille aînée et nous attendons l'arrivée d'un autre petit-enfant bientôt. Quand à Peggy qui vous avez vue à Paris, elle est en retour en Angleterre d'une visite d'un mois aux Etats-Unis et va reprendre son travail auprès de Maldane. Avec les meilleurs souhaits pour votre succès je me soussigne bien cordialement,

P. S. Je reviens aux Etats-Unis la fin de janvier. Si vous avez l'intention d'être au Mexique au même temps que moi, il faut agir très tôt. Envoyez votre réponse par avion, s'il vous plaît.

Nilo 16, Apt.2
Mexico, D.F. Mexico
October 1, .951

Mr. John R. Silber
Graduate Philosophy Club
Yale University
New Haven, Connecticut.

Dear Mr. Silber:

I am finding the demands for outside lectures on me so great that I am obliged to take a definite attitude in the matter. While I do not intend to be accountable to any one for what I do in a particular case, I hereby announce that it is against my policy to give lectures which are not part of my academic program. I have found that there is no other way by which I can secure the time for the significant part of my work. I therefore regret to inform you that I shall be unable to lecture to your group at Yale.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 1, 1951

Rabbi Julius Rosenthal
2901 East-West Highway
Chevy Chase, Maryland

Dear Rabbi Rosenthal:

At the time in question I shall be in Mexico.
However, I am not available for any talks to groups
to which I have not a very compelling personal or
professional obligation.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 1, 1951

Mr. Harold Rugg
Woodstock, New York

Dear Mr. Rugg:

I have just completed an autobiographical book on myself which may appear within the year. When it appears you are welcome to use such material from it which may correspond with the custom of the reviewer and the permission of the holder of the copyright. This book has taken me from three to four months of hard ^{work} to do, and in general, if it had not happened that I have just done this job, it would take an appreciable part of this time to answer this request. Moreover, as I am away from M.I.T. for several months, I have no access to any material accumulated there.

Sincerely yours,

In view of the amount of work entailed by my informing one author after another about my personal career, I am compelled to adopt the point of view that my books must speak for me and that I must ignore all demands on what is after all the use of a literary property which belongs to me.

Sincerely yours,

Columbia University
in the City of New York
[NEW YORK 27, N. Y.]
DEPARTMENT OF MATHEMATICS

October 2, 1951

Professor Norbert Wiener
Massachusetts Institute of Technology
Cambridge, Massachusetts

Dear Professor Wiener:

Some months ago, Enrico Bompiani of the University of Rome invited me to come to Italy as a Fulbright scholar during the year 1952-53. The notion of following the example so recently set by you seemed very reasonable and consequently I am taking appropriate steps with the responsible body in Washington. In order to have the opportunity of traveling professionally in other countries, particularly France, Switzerland, and England, I thought it best to increase my dollar potential, and so am simultaneously submitting an application for a Guggenheim fellowship. I have taken the liberty of placing your name among my references. I hope that when a request comes to you for information concerning me, you will find it possible to dictate a few words relative to my candidacy. You will receive at that time all relevant material describing my proposed projects and so I shall not elaborate on them to you at this point.

I presume you had a splendid summer in Europe. In your absence, we took care of New Hampshire which regaled us with a phenomenal summer. Please extend my warm greetings to Mrs. Wiener.

Very sincerely yours,

Edgar R. Lorch

EDGAR R. LORCH

ERL/ja

[ans 10-18-51]

72 PERRY STREET
NEW YORK 14, N. Y.

October 3, 1951

Dear Dr. Weiner:

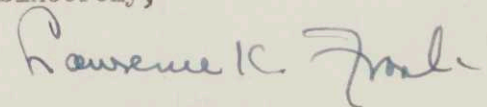
Last spring when my latest book, "Nature and Human Nature" was published, I asked the publishers to send a copy to your office at Tech, where it was probably received and put aside for your return. I am writing to tell you it was sent and to say that I hope sometime when you get caught up you will have an opportunity to look it over, and perhaps we can get a chance to talk about it when next I see you. If you find anything in it that you consider unsound or undesirable, I would be grateful for your comments.

I hope you enjoyed your visit in France. From one or two friends who saw you there, I gathered that you were having a good time and making a considerable impression upon those who heard you in Paris. I assume that you have received a copy of the volume by Gregory Bateson and Jurgen Reusch on Communication, in which Gregory discusses and elaborates on some of your suggestions about information and negative entropy.

May I add that I am becoming somewhat concerned about using the term "negative entropy" as a way of dealing with living organisms. It seems to me confusing for students of organisms to use the term "negative entropy" to designate organized goal-seeking activities. It is somewhat like the situation we have today where we have no adequate conception of health except as the absence of disease, again because the doctors and pathologists first made us familiar with disease, and so we have to talk of "negative disease." Can't we develop a new conception of the process involved in organized activities and especially in organisms that will more adequately recognize that they are engaged in something other than in postponing entropy, in the older sense of disorder or the more recent sense of obtaining information and thus increasing negative entropy, as just pointed out in science by Rothstein. This is in part what I was trying to do in my volume on "Nature and Human Nature" and if I have made some egregious blunders, I would be genuinely grateful for your help in clarifying my confusion.

With best wishes, and hoping to see you in the not too distant future.

Sincerely,



Lawrence K. Frank

Dr. Norbert Weiner
Massachusetts Institute
of Technology
Cambridge, Massachusetts

[ans 10-26-51]

From L. K. F.

GENETIC PSYCHOLOGY AND ITS PROSPECTS

BY

LAWRENCE K. FRANK

Reprinted from
The American Journal of Orthopsychiatry
Vol. XXI, No. 3, July, 1951

SYMPOSIUM ON GENETIC PSYCHOLOGY*

4. GENETIC PSYCHOLOGY AND ITS PROSPECTS

LAWRENCE K. FRANK

Formerly Director of the Caroline Zachry Institute, New York City

PROBABLY the oldest expression of the genetic conception is that found in medicine, which long ago recognized the importance of the history of the patient for accurate diagnosis. Likewise written history again indicates an early concern for tracing the origin and past development of human societies.

But it was the conception of organic evolution in the nineteenth century which fostered the modern genetic approach. As John Dewey expressed it years ago:

It is astonishing that, in the face of the advance of the evolutionary method in natural science, any logician can persist in the assertion of a rigid difference between the problem of origin and of nature; between genesis and analysis; between history and validity. Such assertion simply reiterates as final a distinction which grew up and had meaning in pre-evolutionary science. It asserts, against the most marked advance which scientific method had yet made, a survival of a crude period of logical scientific procedure. . . . The significance of the evolutionary method in biology and social history is that every distinct organ, structure, or formation, every grouping of cells or elements, is to be treated as an instrument of adjustment or adaptation to a particular environing situation. Its meaning, its character, its force, is known when, and only when, it is considered as an arrangement for meeting the conditions involved in some specific situation. This analysis is carried out by tracing successive stages of development—by endeavoring to locate the particular situation in which each structure has its origin, and by tracing the successive modifications through which, in response to changing media, it has reached its present conformation.¹

Unless we pause to reflect we may not realize what a revolutionary change has taken place and is taking place today in our basic assumptions. Only a short time ago we were firmly convinced that we lived in a more or less static universe in which every organism had been created at one time and had persisted unchanged from the beginning; that all events were determined by cause and effect relations. Whatever happened was to be accounted for in terms of classic physics, which recognized change, but change viewed as alterations in position, resulting from gravitation and thermal agitation.

Until recently we have had no genuinely dynamic concepts or ways of thinking of events in terms of processes such as growth, development, maturation, aging. It is not very long ago that we believed the human fetus

* The four papers included under this heading were presented at Clark University, Worcester, Massachusetts, on April 20, 1950, in a program commemorating the sixtieth anniversary of Clark University.

¹ John Dewey. *Essays in Experimental Logic*, p. 93. Univ. of Chicago Press, Chicago, 1916.

started as a minute homunculus which gradually enlarged in size and the child was viewed as a small-sized adult.

Personality, insofar as we had such a conception, was believed to be a mysterious psychic entity, given at birth along with the soul, and it was more or less idle to discuss the development of personality, except in terms of the struggle with evil.

Today genetic psychology recognizes as its central and most difficult problem that of personality development—how personality emerges, as the infant mammalian organism grows, develops and undergoes a series of life experiences that shape, and often stunt and distort, his inherited functions and capacities; how he becomes a participating member of social order, a bearer of traditions, a personality who has learned to live in our symbolic cultural world of meanings, values and purposes.

We owe this dynamic conception of personality development to the pioneers in the field of genetic psychology, chiefly to Sigmund Freud who gave us the first systematic statement of personality development, viewed as a developmental sequence. We also owe to Dr. Freud what has been less clearly recognized, namely, that an understanding of this developmental process is possible only as we realize that the life experiences of the identified individual are always *relative* to him as a unique organism-personality. This means that the significance of all experience is idiomatic for each developing personality, since he himself finds or imputes the meaning of all his experience and responds thereto in the patterns and with the feelings that are uniquely his own.

The failure or inability to grasp this conception of the idiomatic significance of life experience for the identified individual has led to many misunderstandings and much experimentation designed to test objectively and statistically what has significance only idiomatically or clinically.

What seem to be the crucial problems in genetic study of personality development may be put in this way.

We are confronted with an individual who grows and develops as an organism-personality, who is continually changing but persists as an identified person, undergoing a succession of changes that we can clearly observe and often measure as an organism, but which we find difficult to trace as a personality.

In this sequence of changes we are perplexed by the operation of processes that may produce different products, depending upon when, where, how and on what they operate. Thus we may observe the same or similar dynamic process operating in different individuals at different times and in different contexts to produce different personalities. Moreover, we may also observe different processes producing similar or equivalent personalities when operating at different times, in different life situations, in different individuals.

The same or nearly identical conduct and emotional response, exhibited by a number of individuals, may be the product of different life experiences or expressions of very different affective reactions; the same or similar personality processes may be revealed in different actions, speech or emotional reactions.

These operations are baffling, when we try to fit them into the static conceptions of unvarying cause and effect (or multiple causation) or stimulus and response formulations, or try to study them in terms of the relation of two variables. We often make highly significant observations but then interpret them according to the customary classifications of data as entities, thereby ignoring the pattern of dynamic activities of the living persons upon whom these observations have been made.

It is interesting to note how much of these assumptions of earlier scientific methods and problem formulations persists, even in the formulations of those who assert that they are thinking dynamically, but who still rely upon classic formulations that are still valid for other problems than human conduct and personality development. Personality development involves study of the dynamic circular processes in an identified personality who has had a life history that is operating today in the altered structures, functions, behavior and feelings established in his life history.²

As Dewey and Bentley have pointed out, we cannot understand human behavior in terms of the self-action of animism, or of the inter-action of cause and effect as in particle physics and billiard ball collisions; we need a conception of *trans-actions* which are dynamic circular processes occurring in a field.³

Most of our theories of learning are framed in static terms of simple interaction, as in the impact of stimulus-situations upon an organism whose behavior is viewed as governed by that stimulus, its frequency, recency, and effect or reinforcement so that certain random responses sooner or later are established by repeated reinforcement of rewards and punishment. This trial and error learning does occur, especially in animal experiments, and its scientific study is important; but learning also takes place, especially in human personalities, by a different process, as gestalt psychology and psychoanalysis have shown.⁴ Moreover man, apparently uniquely, has the capacity for learning to learn, by what Gregory Bateson has called *deutero-learning*.

If we look forward and speculate upon the prospects of genetic psychology, we may find some indications of its probable future development by looking at some of the implications of the genetic method as they are now being developed and applied. We should, however, be on guard against the *genetic*

² Cf. the writer's paper *The Locus of Past Experience*. J. Philosophy, 20: 327-329, 1923.

³ John Dewey and Arthur Bentley. *Knowing and the Known*. Beacon Press, Boston, 1949.

⁴ Cf. the writer's paper *The Problem of Learning*. Psychol. Rev., 33: 329-351, Sept. 1926.

fallacy, as Suzanne Langer has described it: "the error of confusing the origin of a thing with its import—of tracing a thing to its most primitive form and then calling it 'this archaic phenomena.'"⁵ This is the practice of reductionism, of reducing every organized complex or event to its component elements and ignoring the obvious fact that those components are found in an organized complex wherein they are behaving in highly significant patterns that differ from their behavior when not found in an organized complex. Also it may be pointed out that genetic science is not so concerned with predictions—long considered *the* criteria of scientific achievement—as it is with the question of origin, of accounting for the present in terms of the past.

As suggested earlier, genetic psychology is confronted with the question of persistence of an organism-personality who changes. We have no satisfactory language for that kind of problem since we have historically conceived of persistence only as fixity and recognized change only as motion or the shift to random and disorderly events as in entropy.⁶

It has been customary in the life sciences to ignore or forget evolutionary development when studying organic behavior and functional processes, then to assume that organisms exhibit only random, disorderly behavior like molecules in a gas which act with statistical regularity and orderly frequencies (order from disorder, as Schroedinger has put it). Only recently have biology and psychology recognized that the functional process and the behavior of organisms are produced by organized structure-functions (products of evolution) which exhibit orderly, patterned activities, self-regulation, and can postpone entropy or exhibit negative entropy.⁷

These older assumptions are essentially static in that whatever happens is regarded as the effect of a potent cause acting upon a more or less inert object or organism (stimulus and response). Psychology has also invoked conceptions of self-acting motives, of a more or less animistic character. In consequence purposive behavior has been viewed either as an outcome of trial and error, as in gas laws, or of efforts at tension reduction, or as self-action by unnatural or supernatural powers or agents. Organic persistence and human conduct arise, not as order from disorder, but as order from preceding order, as Schroedinger has emphasized.⁸

Today we are beginning to formulate a dynamic conception of organiza-

⁵ Suzanne Langer. *Philosophy in a New Key*. Harvard Univ. Press, Cambridge, Mass., 1942.

⁶ Cf. F. S. C. Northrop. *Entities and Organization in Current Biological Thinking*. Sigma Xi Quart., 21: 1, 1933. "Ancient biology provided for form with a resulting fixity of types, at the expense of entities and the fluidity of species; modern biological theory provides for the nonfixity of types and the presence of physicochemical and genetical entities, at the expense of organization."

⁷ Cf. Erwin Schroedinger. *What Is Life?* Macmillan, New York, 1946.

⁸ Cf. *Teleological Mechanisms*. Annals N. Y. Acad. Sci., Vol. 50, Art. 4, 1948. See Introduction by L. K. Frank.

See Kurt Goldstein. *The Organism: A Holistic Approach to Biology*. American Book, New York, 1939. See James S. Plant. *The Envelope*. Commonwealth Fund, New York, 1950.

tion which, as in embryology, is in terms of field theory. In field theory what we call "parts" are not to be conceived as separate, independent, randomly self-acting entities. The "parts" are what we have selected for observation within a dynamic organized complex; these so-called "parts" are continually acting, reacting, interacting, *transacting*, by a reciprocal circular process, as shown in organic functioning. By their dynamic, circular activity the "parts" create and maintain the "whole" which reciprocally organizes and governs the activities of the "parts" and thereby gives rise to that organized "whole," with its dynamic circular relations to the environing field. This dynamic circular activity of "parts-whole" in a field is what we call "organization," which is not an entity or a superior power or force or something imposed upon parts in a dominance-submission relationship, but what arises from this circular process of the "whole" and its "parts."

We cannot pick out one of these "parts" in an organized "whole" and endow it with causal potency as a self-acting agent, forgetting that this "part" and its activity is significant only as a participant in the "whole" which patterns its activity. Thus, in the living organism, every cell, tissue and organ is engaged in a continuous process of functioning that creates and maintains the intact organism which, in turn, governs what each cell, tissue and organ does in its specialized functioning, to maintain the living organism as a self-repairing, self-regulating organism.

Such dynamically organized "wholes" are ceaselessly operating in order to persist and maintain their organizations; even the solid, seemingly unchanged rocks persist only by the unceasing dynamic activity of their constituent molecules and atoms within their persistent patterns or crystal structures.

It is becoming increasingly difficult, if not impossible, to deal with living processes except as we conceive of their occurrence in an organized patterned configuration, as a "field." Thus students of genetics today are concerned not only with the number of genes, but with the location of each gene in the pattern of chromosomes, since the location, or "position effect," is believed to govern the gene's specific potentialities. Moreover, stereochemistry today is no longer content with the purely quantitative analysis of components to find out how much of each element is present; the search today is for the structure, or spatial configuration of the chemical molecule to find out where each individual atom is located in the molecular structure since the chemical activity of a compound is governed by the location of each single atom in the molecular configuration.

These recent developments are indication of the growing recognition of the significance of patterns or configurations which the older quantitative and analytic procedures not only ignore but make impossible to disclose, when they treat all their observations or measurements as anonymous units in a frequency distribution.

The living organism can persist only by a ceaseless process of *transactions* in an environment, taking in, retaining, expelling, reorganizing, continually altering its constituents and all its dimensions in order to maintain its organization, as recent studies with isotopes and tagged atoms are showing. Indeed the growing evidence indicates that the living organism functions by continually altering its structure, as an ever-changing, but persistent configuration, through which the environment is continually ebbing and flowing.⁹

Years ago the French student of constitutions, Dr. Sigaud, is reported to have said that the difference between a machine and an organism is that a machine can continue to operate only as long as its dimensions remain unchanged, while an organism can survive only by continually changing its dimensions.

It seems clear that if we want to understand the dynamic processes of living organisms, especially of developing personalities, we cannot get very far by trying to study them in terms of random, disorderly events, as in gas laws, and treating our observations as we do convergent cause and effect relationship.¹⁰

We should therefore recognize the necessity of enlarging our thinking to embrace the conception of pattern and organization and of dynamic circular processes and devise methods for their study, as exhibited by identified organism-personalities.

This does not involve any rejection or attack upon classical theory and methods: quantum physics has not invalidated classic physics in formulating its dynamic problems and devising new methods for their study. We can make this step with less hesitation if we will remember that in the millions of years of evolution there have emerged a variety of organized configurations or organisms, plant and animal and in-between, which operate, not as elementary particles, moving about at random, but which have developed these organizations operating through dynamic circular processes, that exhibit the kind of order found in dynamic configurations. The organism grows, develops and learns to act purposefully, within the environing field it selectively creates. Indeed, as we are recognizing, we live in a world in which purposive behavior, a "natural teleology," as J. E. Woodbridge called it,¹¹ has evolved, along with the persistence of the most elementary random particle activity. We cannot rely upon sampling techniques and statistical

⁹ Cf. Ludwig Von Bertalanffy. *Theory of Open Systems in Physics and Biology*. Science, 111: 23, Jan. 13, 1950.

Lawrence K. Frank. *Gerontology*. J. Gerontology, 1: 1-12, Jan. 1946. Also: *Structure, Function and Behavior*. J. Phil. Science, 2: 1935.

Niels Bohr. *On the Notions of Casuality and Complementarity*. Science, 111: 51, Jan. 20, 1950.

¹⁰ See Irving Langmuir, Science, 97: 1-7, 1943, for discussion of difference between convergent events that exhibit cause and effect relations, and divergent events that do not yield to the classic cause and effect formulations and statistical treatments.

¹¹ F. J. E. Woodbridge. *Nature and Mind* (chap. on "Natural Teleology": 113 *et seq.*). Columbia Univ. Press, New York, 1939.

manipulation of anonymous data to understand purposive conduct in identified personalities nor to reveal the dynamic processes in personality expression.¹²

A dynamic formulation is characterized by its ability to account for events without invoking special forces or *ad hoc*, self-acting agents or spontaneous motives, but rather by viewing what occurs as the product of ongoing, cumulative dynamic processes in a "field" which is created by these operations which are reciprocally responsive to that "field." The field concept, as Einstein has pointed out, is a dynamic concept:

It needed great scientific imagination to realize that it is not the charges nor the particles, but the field in the space between charges and the particles, which is essential for the description of physical events. . . . The theory of relativity arises from the field problem. . . . The contradictions and inconsistencies of the old theories force us to ascribe new properties to the time-space continuum, to the scene of all events in our physical world.¹³

This field approach in psychology has been stated in these words:

. . . it is clear that an exhaustive knowledge of the relations between external events and stimulus patterns, or between stimulus patterns and perceptions, will never provide an explanation of the nature of perceptual awareness or of human behavior. The situation can only be treated as a transaction; the individual, the environment, and what transacts between them involve, as you have seen, an "assumptive world" based on the past and betting on the future. Time-clocks and foot-rules cannot measure these, and prediction of behavior cannot be in such terms. The processes involved in human behavior take place in a milieu more inclusive than space and time but they are not out of grasp if you can free yourself from traditional ways of thinking.¹⁴

In field theory we are confronted with what I have called "biological relativity," which recognizes different dimensions or products as equivalents when viewed relative to the field in which they occur. This is what occurs in the clinical assessment of an individual when the physician interprets all the findings on a patient, including the quantitative reports as relative to that patient's organism, his age, sex, occupation, previous experience, present and past illnesses, and then treats each such modified finding as *relative* (not absolute) to all the other findings for that same identified patient. Moreover, biological relativity recognizes that the same basic processes may be operating in different organism-field complexes despite the wide differences in the

¹² Cf. Florence C. Goodenough. *Semantic Choice and Personality Structure*. Science, 104: 451-456, Nov. 6, 1946.

Lawrence K. Frank. *Projective Methods*. Charles C Thomas, Springfield, Ill., 1948.

¹³ Albert Einstein and Leopold Infeld. *The Evolution of Physics*, p. 259. Simon and Schuster, New York, 1938. Note: It may be pointed out, for those who prefer mathematical concepts and methods, that it would be more appropriate for data on human behavior to use tensors instead of simple vectors.

¹⁴ Merle Lawrence. *An Inquiry into the Nature of Perception*. Leaflet issued by Princeton Univ. Press, 1949, to accompany *Studies in Human Behavior*.

See also Hadley Cantril et al. *Psychology and Scientific Research*. Science, 110: Nov. 4, 11 and 18, 1949.

measurements of these organisms-fields and the observed activities or measured products of those processes.

Again we are learning from genetic sciences, especially paleontology, how during the ages, different organisms have evolved with a highly selective capacity for selective perception and utilization of what they need in the environment which they share with other organisms, each living as if in a world of its own. We are realizing that evolution took place, not by a one-sided process of organisms using or adapting to the environment, as if they were passively submitting to environment or living like tenants in rented quarters, but that the ongoing dynamic processes of the environment produced various organisms which reciprocally maintained and altered that environment by their selective awareness, and responses, through dynamic transactions with other organisms and with the events in that environment. This indicates a field concept in evolution that is just beginning to emerge in the newer dynamic theories of ecology.¹⁵

In thinking of the organism-environment as a "field," which each organism establishes out of the totality of the environment, we see how the organism lives, not passively but dynamically, creating the *life space* in which it lives, by a continuous dynamic process of selective awareness and patterned response, purposefully seeking and evoking the stimuli to which it is prepared to respond.

We can carry forward this conception of organic evolution and functioning to the study of the human personality, likewise viewed as a dynamic circular process wherein the personality as he develops selectively establishes his unique *life space* by investing the environment with the meanings, the goals and values derived from his cultural tradition, which the individual himself idiomatically imputes to the world of events and people, and then responds to what he himself "sees" there. Here we may find a way of resolving the old dichotomy of the individual and his society, by recognizing that social order is what the members of the group maintain by their patterned activities and relationships and that this social order in turn governs their activities. Yet each individual lives in a highly selected *life space* within that larger social life.

This circular process, of putting the meaning, the stimulus value if you wish, into situations, into people, and then responding to them in terms of what they mean to that individual, does not involve any mysterious subjective entities or psychic operations. It appears as an extension, a refinement, of the basic processes that have been operating for ages but have reached in man a greater complexity and sensitivity not otherwise observable and operate as highly idiomatic processes.

¹⁵ Cf. G. Evelyn Hutchinson. *Circular Causal Systems in Ecology in Teleological Mechanisms*. Annals N. Y. Acad. Sci., Vol. 50, Art. 4, Oct. 1948.

It does not seem possible to study personality development and expression without recognizing that human perception is patterned by cultural traditions which the unique individual learns to utilize idiomatically in ordering his experience and directing his conduct. This in turn implies that every stimulus-situation has its peculiarly idiosyncratic meaning for each personality who has learned to perceive all events according to their meaning for him and for the repertory of patterned conduct (action, speech, belief and feelings) he has developed to meet this selectively perceived life space.

Only a genetic study of the individual will enable us to discover how these patterns were learned and established as the personality process.

The word "transformation," as used in biology, especially embryology, may be used as a term to designate these changes which take place in the persistent organism-personality, wherein his basic organic needs and functional processes and his naïve impulsive behavior are progressively transformed, sometimes distorted or stunted, as the individual grows and develops and in the process builds up his "life space," as he learns to think, speak, act and feel as a member of society and as a bearer of traditions.

We can say, upon the basis of systematic observation, that each child exhibits his own rate and patterns of growth and physical development as an expression of his unique heredity. We may also assert that there is an orderly, regular sequence of organic growth, development and maturation through which each child, unless stunted or otherwise blocked, must pass from conception to old age, subject at each stage to the various hazards and dangers, the demands, prohibitions and privileges of each age period and his own specific immunity-vulnerability.

We cannot speak with the same confidence about the course of personality development, but, again acknowledging our debt to Sigmund Freud and Miss Anna Freud, we recognize that the early experiences of the infant and young child are of major significance. In early years apparently the child learns how to learn, according to the *eidos* and *ethos* of our culture, always in accordance with his specific individualized experience. He develops the selective awareness, the idiomatic patterns of response, including emotional response and chronic feelings, as he develops and meets the enlarging social world he encounters. He also utilizes the basic assumptions, the organizing conceptions of our cultural traditions, for establishing his "private world" in which he may incorporate a wide variety of distorted, fantastic and conflicting ideas also derived from traditions but individually interpreted. He also learns to utilize all the prescribed rituals, social practices and symbolic performances for living in our social order.

These early life experiences are coercive because learning is a cumulative process; early experiences set the patterns by and into which subsequent experiences are organized, as the child and adolescent meets his life tasks, creat-

ing his own personality problems as his idiomatic version of what traditions forbid, prescribe and permit. These early patterns are not unchangeable, as the effectiveness of psychotherapy, especially of child therapy has shown, but they are usually persistent and coercive because the individual continues to "see" the world and to deal with it under their guidance.

The major contributions to the understanding of personality development have come from clinical study of those suffering from a variety of personality difficulties; we know more about psychopathology than we do about the basic processes of personality development that produce both pathological and nonpathological products. It is to be noted also that since the literature gives us chiefly the etiology of neuroses and the pathological products of traumatic experience, we tend to assume that these are the only possible products. This is apparent in the widespread conviction that human nature is essentially evil and self-destructive, a conviction long asserted in theological teachings, but now reaffirmed by some psychotherapists.

Much of the psychological study of personality development is biased by this pathological concern so that nonclinically oriented students are endeavoring to study or undertake experiments upon "neurotic" behavior as if it could be fractionated into specific cause and effect or stimulus and response relations and experimented upon objectively. Objectivity is often interpreted to mean that if the experimenter presents the subject with the same situations, instructions, stimuli, problems, etc., always in the same way, it will mean to the subject what the experimenter wants it to mean. This is an assumption difficult to maintain in the face of the growing evidence of patterned perception and of how each personality selectively interprets all situations according to their meaning and affective significance for him alone. Here is where the clinically trained psychologist or psychiatrist diverges from the laboratory-trained psychologists, especially those trained in animal experiments where individually patterned perception is less coercive but not negligible and the subjects have no cultural traditions.¹⁶

It seems clear that we will probably modify many of our present-day ideas and terminology as we gain more understanding of what takes place in personality development. The continued use of instincts to designate some coercive mechanism or primitive organic need implies a fixity, even rigidity, which is difficult to reconcile with such diversified transformations of functions as observed in man, who apparently has the least instincts of all organisms and who uses these functional processes, like sex, for all manner of goal-seeking and symbolic fulfillments. Likewise, the discussion of personality process in terms of more or less specific self-acting entities (such as id, ego, superego), forces, mental mechanisms, etc., while useful for professional,

¹⁶ Edward C. Tolman. *A Stimulus-Expectancy Need-Cathexis Psychology*. Science, 101: 160-166, Feb. 16, 1945.

especially clinical communication, may block understanding of the dynamics of the personality process. These terms are artifacts, or figures of speech, often taken literally, and treated as if actual, like the expression, "the unconscious." In the development of modern science advances have come whenever we have dispensed with specific *ad hoc* entities and self-acting agents and have recognized the processes without these assumed agents, forces, powers, etc., operating to produce events. It also seems probable that we will alter our criteria of credibility, relinquishing the tests of credibility that are appropriate to the older, static formulations and problems of relations of two variables, and devise new tests more appropriate to the study of circular processes. This is clearly shown in physics, which has developed a host of new methods and new criteria for the study of identified events and field situations. These new criteria do not involve rejection of the old which are still appropriate to the problems and methods of classical particle physics.

Thus Clerk Maxwell pointed out years ago the significance of the "dynamical method":

In the statistical method of investigating social questions . . . [persons] are grouped according to some characteristic, and the number of persons forming the group is set down under that characteristic. This is the raw material from which the statist endeavors to deduce general theorems in sociology. Other students of human nature proceed on a different plan. They observe individual men, ascertain their history, analyse their motives, and compare their expectation of what they will do with their actual conduct. This may be called the dynamical method of study as applied to man. *However imperfect the dynamical study of man may be in practice, it evidently is the only perfect method in principle.* . . . If we betake ourselves to the statistical method, we do so confessing that we are unable to follow the details of each individual case, and expecting that the effects of widespread causes, though very different in each individual, will produce an average result on the whole nation, from a study of which we may estimate the character and propensities of an imaginary being called the Mean Man.¹⁷

Some years ago Eddington remarked that physics was classical on Monday, Wednesday and Friday, and quantum on Tuesday, Thursday and Saturday. It might be said that likewise psychology today is both static and dynamic, but cannot advance the study of genetic problems until it can be consistently dynamic when dynamic concepts and methods are required to deal with dynamic circular processes.¹⁸

As genetic psychology approaches the crucial problem of personality development, it faces these theoretical problems in all their urgency and complexity. Thus far we are still groping and fumbling for a conceptual formulation to focus our observations and guide our studies along more dynamic

¹⁷ Kurt H. Wolff. *The Unique and the General: Toward a Philosophy of Sociology*. J. Phil. Science, 153: 192-210, 1948.

¹⁸ Cf. L. K. Frank. *Projective Methods*.

lines and to give some clues to the understanding of what is taking place in children before our eyes.

We may today look forward to the future course and prospects of genetic psychology as the refinement and elaboration of some of these dynamic conceptions of process. We may expect genetic psychology to study the various transformations through which what we call personality emerges and operates in the life career of the identified individual.

As organisms, with all the wisdom of the body derived from our mammalian ancestors, we are well equipped to live in the geographical world of nature. But as human beings, members of society, we must learn to live in a social order and in the symbolic cultural world of traditional meanings and values. The problem we face in genetic psychology is to discover how we learn to live in these worlds, how we transform the functional needs and capacities of the mammalian organism, the inherited wisdom of the body, into the patterns required for group living, for goal-seeking, purposive striving, symbolic conduct, but continue to exist as organisms and to live as unique individuals.

Our initial orientation to the understanding of these processes came from the psychoanalyses of adult neurotics whose life experiences, as recalled in later years, revealed the hitherto unsuspected significance of their experiences of parental child care, rearing and education. Later the analyses of neurotic children brought further findings, largely interpreted according to the conceptions and assumptions derived from analysis of adults, but increasingly modified by observation of children.¹⁹

In the light of these analytic (or more appropriately, genetic) findings we may look more specifically at the sequence of transformations through which the individual passes from birth on. We may distinguish several crucial aspects or developmental tasks,²⁰ shaping the emerging personality, beginning at birth.

Thus it seems clear that the infant undergoes a series of transformations of his organic functional processes, capacities and needs, so that his internal environment becomes regulated by outside demands, prohibitions and requirements and transformed, in part at least, into patterned, purposive conduct and symbolic activities.

We are waiting for genetic psychology to help us understand how these various transformations take place. Space does not permit any extended discussion but it should be noted that organic needs and functional processes are utilized for various nonorganic purposes; for example, the inspiration and expiration of air not only meets the organic need of oxygen and removal of carbon dioxide, but also is utilized for crying, sobbing, and later for pat-

¹⁹ Erik H. Erikson. *Childhood and Society*. Norton, New York, 1950.

²⁰ Robert J. Havighurst. *Developmental Tasks and Education*. Univ. of Chicago Press, Chicago, 1948.

terned speech. Tactual sensitivity and the need for mothering, cuddling, caressing (like the licking and nuzzling of infrahuman cubs) not only soothes and relaxes the infant but is transformed into intimacy, interpersonal relations, autoeroticism, etc. What we have called infantile sexuality appears to be largely tactuality, including genital tactuality, that between the age of six and puberty often becomes latent and is then revived at adolescence as a crucial function in adult sexuality and its vicariates. Little is known about the genetic development of tactuality, its patterning, its suppression and later expression.

Likewise biological hunger (the organic need of nutrition) is met by eating which is transformed into appetite for specific food stuffs, eaten at regular intervals set by parents rather than by organic requirements, with all kinds of ceremonies, rituals, concomitants and deferred or symbolic fulfillments; these become the goals of adult purposive striving which biological hunger and eating serve as transformed into such goal-seeking activities. Eating may become a source of enjoyment or of tension, a weapon to fight parents, a way to please parents.

Elimination, the spontaneous evacuation of urine and feces, not only fulfills an organic need but is transformed into regulated releases, at particular times and places, set by parents, not by organic needs, with definite goal-seeking patterns of conduct, striving for continence, cleanliness, sanitation, modesty and persistent feelings of various kinds, shame, anxiety, guilt, etc.

Along with these experiences the child develops an image of his own body, with feelings toward his functional processes and their direct and transformed and deferred fulfillments.

The child in this way surrenders some of his physiological autonomy, gives up some of his organic self-regulation as he accepts external regulation and control of his own functions by parents and learns to transform his organic needs and functional processes into purposive, goal-setting activity. He learns to seek deferred, often symbolic fulfillments, developing various kinds of purposive conduct for these goals that becomes understandable only as the genesis of such conduct reveals how a functional process and the organic need it serves have been transformed. The tracing of these early transformations has been one of the chief concerns of the psychoanalysts.

Likewise genetic psychology will show how the child's growing capacity for motor activity, his naïve impulsive behavior toward things, animals, people and situations is increasingly governed by parental regulation. This involves learning to inhibit his behavior according to the prohibitions against taking, breaking, striking, hitting, biting the property or person of others, so that he learns to transform his impulsive behavior into orderly, patterned purposeful conduct as directed by adults. We want to know how he transforms parental prohibitions into self-administered inhibitions. It also in-

volves learning to perform the required patterned activities and rituals of manners, etiquette, grooming, masculine and feminine conduct and the like. We want to know how the child learns to perform these prescribed actions when no parent is there to direct him, and to respond in the appropriate forms to the situations as they have been defined by the absent adult. How does the child learn to relinquish his direct contacts with the environing world of objects, animals, persons and events as he has naïvely sensed them and learn to perceive the world and to respond in the prescribed patterns of social conduct and parental demands for symbolic activity?

The child learns to live in a world *defined by parents* in terms of don'ts and do's for private property and integrity of the person, and thereby is inducted into the social world of prescribed rules and ceremonies, rituals and authority, of interpersonal relations, where the child learns to impute meanings to events and then to respond appropriately to the meanings that he has learned to invest in these situations.

Since these experiences expose the child to many deprivations, frustrations, coercions and other interferences with his activities, they arouse what are called emotional reactions of greater or less intensity toward the adults who impose them. What the child learns is always colored or infused by the emotional reaction to the adults who govern his learning experiences. We are waiting for understanding of how acute emotional reactions such as anger, rage or fear become established as concomitants of the performance of all these functional transformations and patterned conduct and often become persistent feelings—chronic affective reactions, such as anxiety, guilt or hostility—toward the world and especially toward persons.

Genetic psychology, we hope, will show how this learning, with emotional or affective coloring, operates to establish the highly selective awareness, patterned perception and conduct, especially toward persons, that we now recognize as the idiomatic orientation of the individual, who thereby creates and maintains his "life space," perceiving all events in a *time perspective*.²¹

The child is also inducted into use of language, first responding to verbal communication and then learning to use language, so that he learns that every thing, animal, person, event has a name and a meaning which he must recognize and impose upon things and people. Also through language he learns the traditional concepts and assumptions as interpreted by his family and so is inducted into the symbolic cultural world of meanings, values, purposes and goals as defined by religion, philosophy, law, morals, folklore and science.

The child develops his own private frame of reference as his idiomatic

²¹ Lawrence K. Frank. *Society as the Patient*. Rutgers Univ. Press, New Brunswick, N. J., 1948. See chap. on "Time Perspectives."

version of these traditional beliefs and assumptions. Thereby he develops what is most specifically, if not uniquely, human, namely, symbolic living.²² The individual learns not only to recognize symbols, but to respond to them with the meaning which he idiomatically gives them as his own personal interpretation; these meanings may be more or less equivalent to their socially sanctioned interpretations or may be wholly bizarre and distorted. The goals the individual personality seeks to attain as transformations of organic needs may be attainable, socially sanctioned, or antisocial, or may be symbolic goals that can never be attained or given up because the individual has no awareness of what he is doing in his endless pursuit of such goals.²³

The crucial problem of personality development is that of symbolic learning, especially how the individual, in all his functional processes, overt actions and speech and his internal speech, maintains his symbolic private world at whatever cost to himself and others.

The way the child develops his conceptions of the world, how he takes over from adults their assumptions and beliefs, how he builds into his private frame of reference, the many misunderstandings and confusions he learns are but little known. Since thinking and reasoning have been regarded by most students of personality as primarily for rationalizing wish fulfillment, relatively little attention has been given to the role of ideas and beliefs and of concept formation in personality development and to the process of learning to live in a symbolic cultural world of meanings and values.²⁴ Here we should recognize that only a "transactional" conception of the individual and his several environments will enable us to escape the older dualistic and animistic assumptions to a dynamic conception of man and nature wherein personality development can be studied as a process similar to all other processes.²⁵

Throughout these cumulative experiences the child is gradually transformed from a mammalian organism into a participating member of his society, a bearer of traditions who learns to live in a cultural world defined by those traditions. In the process of these transformations, he emerges as a personality who acts, feels, thinks, speaks, and otherwise responds more or less like others in his group, but always in his own idiomatic way. The individual perceives the world selectively as his awareness has been patterned by his unique life experience and he lives as if in a "private world" of his own, putting meaning and responding with feelings to every person and situation which he individually perceives, as *he* feels toward his "life space" and the persons therein.

²² Suzanne Langer. *Philosophy in a New Key*, especially Chap. II.

²³ Lawrence S. Kubie. *The Repetitive Core of the Neurosis*. *Psy. Quart.*, 10: 23-43, 1941.

²⁴ Cf. Heinz Werner. *Comparative Psychology of Mental Development*. Follett Pub., New York, 1948.

²⁵ Cf. Lawrence K. Frank. *Nature and Human Nature*. Rutgers Univ. Press, New Brunswick, N. J., 1951. Also Plant. *The Envelope*.

Just as he learns to live in a world defined by others, so he learns to think and feel about himself as defined by others, developing an image of the self as the chief actor in his "private world." Probably the crucial aspect of personality development is this image of the self which the child develops primarily from the way parents, teachers and other significant persons describe, criticize, denounce, punish or praise and love him. Each individual personality in learning to live in a symbolic cultural world must create an image of the self and deal with that symbol of the self as he thinks of himself, talks to himself, and as others think and speak to him and treat him.²⁶

The core of a healthy personality apparently is an image of the self that the individual can accept and live with, without feeling too guilty, anxious or hostile, without being self-defeated or destructive to others. This is an exigent problem for genetic psychology, especially the question of how to develop healthy personalities, not just avoiding neuroses.

The child continues as the mammalian organism that appeared at birth; he continues to function as an organism, but is transformed into a personality. As a personality he engages in the continuous circular process of putting meaning into every experience and then responding to the meaning which he himself has invested or imputed to the situations and persons according to what he has learned as appropriate to such meanings. As a personality he learns how to transform the world of nature and other people into the meanings and values to which his life is continually directed, in the goal-seeking, purposive conduct into which his organic needs and functions have been transformed.

The personality, conceived as a dynamic circular process, presents many challenging problems to genetic psychology. How does this development of personality occur as a generalized process operating in all members of a cultural group, and how does it operate to produce the highly idiomatic personality of each individual?

How does the individual personality with these early established patterns meet the life tasks of adolescence and the maturation of sexual function, of mating and child bearing, of jobs and group living, of involution and of aging? How does he progressively supersede or replace his previously learned functional patterns, overt conduct, his relationships, his ideas and feelings at each of these turning points, with new patterns more appropriate to his age, or fail to do so and hence is compelled to meet life, handicapped by patterns established at an earlier stage of maturation? How does the individual personality maintain his identity and continue his basic personality processes by reformulating his personal problems at each stage of develop-

²⁶ As Florence Goodenough has pointed out, the study of these "private worlds" is a problem of discovering the signs that are indicative of that closely guarded private world. Cf. *Semantic Choice and Personality Structure*. See also Frank. *Projective Methods*.

ment so that he can continue to maintain his life space, his private world, and meet life in his idiomatic way? How does he equalize the impact of the changing environment upon his fluctuating, and later his aging, organism, thereby maintaining more or less constancy in his environment field, as Kurt Goldstein has pointed out?

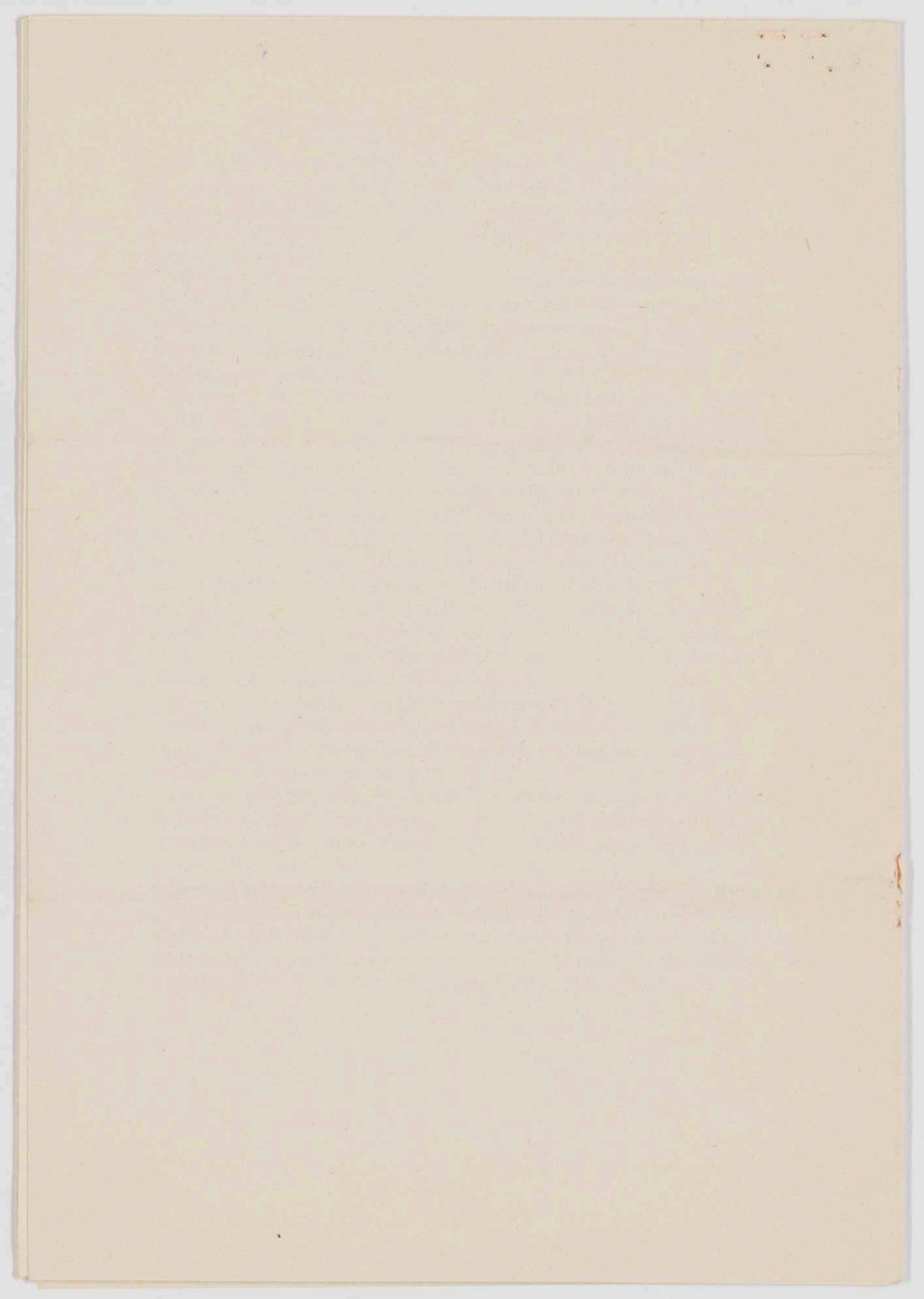
As genetic psychology continues to study these problems and develops appropriate methods it will contribute ever more understanding of the basic processes of personality development, and thereby enable us to grapple more effectively with the urgent problems of our disorderly social life and our disintegrating culture.

As genetic psychology helps us to realize that social order is maintained by the patterned reciprocal activities of all the individuals making up that society, which in turn, like all dynamic organizations, governs the conduct of those individuals, we will see that this circular process can be redirected by what we do to and for the emerging personality of children.

As we gain more insights into the processes of personality development, we will undoubtedly gain in understanding of the many tragic conflicts and self-defeats in all areas of human living, especially in all our interpersonal relations. The social scientists who now tend to think in terms of large super-human systems, social, economic, political, operated by impersonal "forces," above and beyond man's reach, will realize that these "forces" are only metaphors, symbols of the activities of the many personalities who can and will change, as human history so clearly shows.

Social science needs a dynamic conception of human nature to replace its old conception of a rational, calculating individual, and genetic psychology needs a clearer recognition of the social, cultural environment in which personality develops and operates.

But probably the most important contribution of genetic psychology will be in showing that human nature is not fixed, unchangeable and evil, as our traditions have taught us. Human nature is flexible and plastic, with many potentialities which can be developed for good or ill, and patterned into different kinds of personality-character structures, as shown by the many different cultures all over the world. As we guide and direct the processes of personality formation in childhood and youth, we may utilize these human potentialities to redirect our culture and contribute to the establishment of a social order dedicated to the dignity of man, woman *and* the child.



Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 3, 1951

Mr. Paul Brooks
Houghton Mifflin Company
2 Park Street
Boston 7, Massachusetts

Dear Mr. Brooks:

I was delighted with your letter the other day, and am forwarding to you under this cover a copy of the manuscript of the autobiographical book, which I am calling at least provisionally "The Bent Twig". I am sending it to you for a month's option. I have gone over every piece of the manuscript, and I am reasonably satisfied with the continuity, but I am prepared to use the benefit of your advice in the final revision.

Since I am at a distance of three thousand miles, I had thought it better to have a personal representative in Boston with whom to take up such issues as may arise there. Without giving any specific rights to the M.I.T. Press, I have taken the liberty of sending a copy to Mr. Fassett, Jr. of the Technology Press as a personal friend to help me in matters having to do with dates, spelling, and the like, and to attend to such issues as need to be thrashed out with you and are more easily handled across the Charles than the Rio Grande.

I am sending you a copy of the letter I am writing to Mr. Fassett as well as a copy of a letter I am sending to Mr. Morley of Eyre and Spottiswoode. I am also sending to each of these gentlemen a copy of the letter I am sending to the others as well as a copy of this present letter. If and when you come to a decision, I should like to have it communicated to them as well as to myself.

With all best wishes to you personally, to my friends at Houghton Mifflin, and to a firm which has given me very best treatment, I am,

sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 3, 1951

Professor Gyorgy Kepes
School of Architecture and Planning
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge 39, Massachusetts

Dear Professor Kepes:

I can only reiterate that I have assumed every single obligation which I can physically and mentally assume for the present year. I am just seeing a book through for the publisher this week, and I have another book consisting of my French lectures to attend. I am also making a major contribution to a book done jointly with Dr. A. Rosenblueth. In view of all this I must in selfdefense refuse to make any further commitments.

Sincerely yours,

The Technology Press



MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE 39, MASSACHUSETTS

October 4, 1951

Dr. Norbert Wiener
Instituto Nacionale de Cardiologia
300 Avenida Cuatamos
Mexico, D. F., Mexico

Dear Norbert:

Your passage through Cambridge was altogether too swift for so plodding a fellow as I to have a chance to catch up with you. I regret that fact mainly because it would have been so great a pleasure to see you again and to catch up a little on the events of the past six years. I regret it also because as my letter to you in Paris last June indicated, I am concerned that your new work on cybernetics have every opportunity for the best presentation and the highest success.

The Wiley people have let me know of your conversation in New York August 27. I take it that the new volume which you have in contemplation will be a really new book with perhaps even a new point of view. I am all for this. A lot has happened since CYBERNETICS launched a new discipline. If your new work does constitute a completely revised estimate of what has been done in the field and how the field looks at the moment of writing, it will be an extremely valuable contribution intellectually and it should bring to you a satisfying return, both philosophically and materially.

When you have a moment, please let me have a line. I hope we can keep in touch about the project by correspondence during your interlude in Mexico so that upon your return here, I can qualify in some slight measure at least as a guinea pig in discussions.

With every good wish,

Very truly yours,

A handwritten signature in dark ink, appearing to read "F. G. Fassett, Jr.".

F. G. Fassett, Jr.
Director

*no answer
letter previously written*

cc to Mr. J.S.Snyder

Lora S. Levy
Brandeis University
Waltham, Massachusetts

October 4, 1951

Dear Professor Wiener;

Currently I am engaged in writing a thesis concerning the validity of the dichotomy that has existed from the 19th century up to the present time between the poet and the scientist, and which many poets and scientists consider still existing. Having read your book "The Human Use of Human Beings", I am certain that whatever time you may find yourself able to give me will be of the greatest value: I am finding it very difficult to locate those who even recognize the subject, let alone know it. I should be most grateful for any interview for which you find the time.

Sincerely,

Lora S. Levy

Lora S. Levy

[ans 10-18-51]

Copy

Nilco 16, Apt. 2
Mexico, D.F. Mexico
October 4, 1951

Mr. F.G. Fassett, Jr.
Director, The Technology Press
Massachusetts Institute of Technology
Cambridge 39, Massachusetts

Dear Fassett:

I found the letter which you sent me when I was in France so pleasant and encouraging that I am venturing to ask you a big favor. I have just completed an autobiographical book entitled "The Bent Twig" which I am enclosing under this cover, and which I am sending to you for whatever interest you may have in it. I am not offering it to Technology Press, but if you have any interest in it of that sort, I request that you let me know what your plans might be. I am offering the book on a one month option to Houghton Mifflin Company, with the understanding that I myself shall take charge of the negotiations for the foreign rights. As you see by the accompanying letters, I am also sending a copy to F.V. Morley in England of Eyre and Spottiswoode, who took charge of the English publishing of my last book "The Human Use" on behalf of Houghton Mifflin Company in the United States.

I am suggesting that you find some one around Tech who can help me during my absence in the spelling of names and such historical matters as are hard to handle from another country. I am also hoping that you can see your way clear without any responsibility to do me the good offices of friendship for which I shall feel so grateful to you.

If the Technology Press or the Tech Library should show any interest in acquiring physical possession of my manuscript, as they have of the two earlier general books which I have written, it is up to you and I shall do whatever you feel necessary.

I am counting on some very interesting times when we get together again at M.I.T.

Sincerely yours,

Copy

Nilo 16, A pt. 2
Mexico, D.F. Mexico
October 4, 1951

Mr. F.V. Morley
Eyre and Spottiswoods
15 Bedford Street
Strand, London, W.C.2

Dear Mr. Morley:

The two letters which I am enclosing will make it clear to you that I have just finished my autobiographical book entitled "The Bent Twig" which I am enclosing under this cover. I am submitting it to you in accordance with our verbal agreement in London late last spring. If it looks like a dud to you don't hesitate to say so, but if you think it is not without merit, I am ready to hear what you propose to do about it. I am retaining freedom of action in the matter of the European rights, but if you give the book the green light, I should like you to check directly with Mr. Paul Brooks so there is no misunderstanding.

I am very appreciative of the sympathetic attitude in respect to the "Human Use" and of the way in which you have pushed it. I set a great value by your own favorable opinion as well as that which you relate to me of my friend and contemporary T.S. Eliot. I hope that you and Peggy will have an opportunity to get together again when she returns to London early in October.

Sincerely,

THE INSTITUTE FOR CANCER RESEARCH

7701 Burholme Avenue

Fox Chase

Philadelphia 11, Pa.

October 5, 1951

Prof. Norbert Wiener
Nilo 16, Apt. 2
Mexico D. F.
Mexico

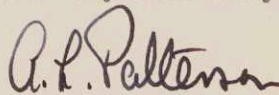
Dear Professor Wiener:

Thank you very much for your letter of September 28th. I have accumulated the extra information that the functions I need are the Sonine polynomials and that, of course, gives me a generating function to work with. Unfortunately, I haven't been able to spend too much time on the problem, but I think it will go. However, I would be very glad to hear of your work on the subject.

It is not surprising that you question the relationship of this sort of thing to the cancer research problem. It turns out, however, that David Harker has produced a new approach to the study of complicated structures such as proteins which involves an expansion of molecular groups in some sort of a radial system. It, therefore, occurred to me that one might be able to simplify treatment by using an orthogonal system which was self-reciprocal under the Fourier transform. Thus, my interest is not too far from biological problems. Actually, the main part of my job here is concerned with structure problems on fairly small molecules.

With again many thanks for your interest.

Yours very sincerely,



A. L. Patterson

alp/jk

8/X/1951

Dear Professor Wiener,

I'm just coming back home in Paris after some two months in bed (septicaemia - a rather usual ^{thing} to me) so that I have a terrific guilt complex for a delay in writing the book.

As a matter of thing I'm hard on it now and I hope that I'll be able to compensate this waste of time. -

Now, as to the point of coming in Mexico I am far from sure to get the grant in due time for being there before your departure:

I am disappointed but if the thing was planned for the next year it would be probably easier. - Anyway I'll send you the manuscript and I hope that we can arrange it by purely written communication channels. -

In order to maintain a better contact
with you and the work under you could
it be possible to get materials from Dr.
Rosenbluth's lab.?

I have read your book on human ex
~~act~~ human beings and I must say that it
seems to have been widely appreciated in
France and specially by peoples with the
same philosophical point of view as me.

I address you my felicitations
to you and Mrs. Wiener for all happy
events in your family and, with apolo-
gies for the ~~too~~ ~~thought~~ lazy way I
expressed my reconnaissance I remain

Yours respectfully

J. L. E. H.

INSTITUTE FOR THE UNITY OF SCIENCE

American Academy of Arts and Sciences
28 Newbury Street
Boston 16, Massachusetts

Prof. Norbert Wiener
Mass. Institute of Technology
Cambridge 39, Mass.

The Institute for the Unity of Science, working with the American Academy of Arts and Sciences, will continue the discussion meetings of the past years on problems common to different disciplines. The major concern of these meetings has, in the past, been with the role of science as a logical system of thought and as an organized social activity.

At present, a series of meetings is planned on the 3d Tuesday of each month, barring interference with a holiday. You are cordially invited to join us on those evenings in the Lounge of the Academy at 8 P.M. Between the lecture and the discussion, refreshments will be served at a nominal charge.

The first meeting will be held on Tuesday, October 16, when Dr. D. Gabor (Lecturer in Electric Engineering, Imperial College, London, and, at present, Visiting Lecturer at the Massachusetts Institute of Technology) will talk on "Information Theory and Scientific Method."

Besides these monthly meetings, two study groups of the Institute are scheduled to continue their activities, started last year. The two groups are the one on Cybernetics and Communication Theory and the other on Science Values.

Please note on the enclosed postal card if you wish to remain on the mailing list of the Institute to receive announcements of future monthly meetings; also, if you are interested in participating in either of the study groups. Please check correct spelling of your name and address as they now appear on our mailing list. (See envelope in which this came to you.)

--PROGRAM COMMITTEE

- | | |
|--------------------|----------------------|
| Percy W. Bridgman | Willard V. O. Quince |
| Karl W. Deutsch | Harlow Shapley |
| Philipp Frank | S. S. Stevens |
| Gerald Holton | Richard von Mises |
| Roman Jakobsen | Walter A. Rosenblith |
| P. E. LeCorbeiller | Secretary |

October 9, 1951

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 10, 1951

Mr. Ralph Holt Cheney
Department of Biology
Brooklyn College
Bedford Avenue and Avenue H
Brooklyn 10, N.Y.

Dear Mr. Cheney:

I shall not be in the United States until February at the earliest. However, this letter is to notify you that I have completely withdrawn from public lecturing and that neither this year nor any time in the future shall I be available for such lectures. I have found a program of outside lecturing completely incompatible with my academic and and scholarly duties.

Sincerely yours,

PROCEEDINGS OF THE AMERICAN MATHEMATICAL SOCIETY

A. C. SCHAEFFER
MEMBER OF EDITORIAL COMMITTEE

UNIVERSITY OF WISCONSIN
NORTH HALL, MADISON 6, WISCONSIN

October 12, 1951

Professor Norbert Weiner
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts

Dear Professor Weiner:

Thank you for your report on the manuscript by
Cameron, Lindgrén and Martin entitled "LINEARIZATION OF
CERTAIN NON-LINEAR FUNCTIONAL EQUATIONS. Their paper has
been accepted for publication in the Proceedings on the
basis of your report.

Yours sincerely,



A. C. Schaeffer

ACS:jd
#158

7-20

BURDEN NEUROLOGICAL INSTITUTE.

STOKE LANE,
STAPLETON,
BRISTOL.

TELEPHONE: FISHPONDS 53221-2.

Professor Norbert Wiener,
Nilo 16, Apt. 2,
Mexico, D.F.
Mexico.

12th October 1951.

Dear Norbert,

Your letter of the 3rd was a great pleasure and relief. The last we heard of you was from Geneva where the reports of your health were rather gloomy and I am so very glad to learn that you are capable of doing too much.

I will send such data as I have about Craik to Mr. Smith in New York. Did you ever see his monograph "The nature of explanation." ?

We had a very nice time with Peggy and she is coming down again this weekend. I gather there has been some disappointment in London and she is not likely to go on with her work. Perhaps this is not too bad. I cannot feel that counting flies is really her vocation.

We are all most entertained by your description of the Central American folk-ways.

The Proceedings of the Information Theory Symposium a year ago have just come out and I wonder whether you have had a copy. It is a most impressive volume full of very witty, meaty fruity stuff. I can understand about 10% of it and am intending to take a month off to make sure I really understand that. The articles on Prediction, Noise and so forth seem to me quite brilliant and I hope you will get a chance of looking through them. They stem from your own work of course, but represent a considerable clarification of some points. I am still working with my General Theory of Adaptive Behaviour, but alas the mathematics are beyond me and I have to rely on qualitative and graphical representations.

-contd-

I will write again after Peggy's visit. Please give my regards to your wife and Rosenblueth. The whole family also asks me to send greetings.

Yours,

Craythall

Your obligations to mankind are NOT, NOT, fulfilled by entertainment of Mexicans. What sort of feedback do you have - "You have gone too far go further!"? Pity is poor physiology who rely on your malice.

[15-21-51]

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE 39, MASS.

DEPARTMENT OF MATHEMATICS

October 15, 1951

Dear Professor Wiener:

Several people, including myself, are trying to organize a Struik Defense Committee whose main object, I presume, would be to try to raise some money for Struik's defense. We have prepared the enclosed statement and are trying to get some important people to sign it. We would like to know if you would do so, and allow your name to be used as a signer of it. It is of course understood that if you do ~~xxxxxxxxxxxxxxxxxxxxxxxx~~ that no further action would be taken without your consent, i.e. signing it entails no commitments of any kind. ~~xxxxxxx~~

Best regards,

Aulrose

[Ans. 10-25-51]

STATEMENT BY STRUIK DEFENSE COMMITTEE

On September 13, 1951, Professor Dirk J. Struik of the Massachusetts Institute of Technology was indicted under the laws of Massachusetts on charges of advocating, advising, counseling and inciting "the overthrow by force and violence of the government of the Commonwealth of Massachusetts," and conspiracy to advocate, advise, counsel and incite "The overthrow by force and violence of the government of the Commonwealth of Massachusetts and the government of the United States of America."

Professor Struik has said: "I have never advocated orally or in writing the overthrow of the Commonwealth of Massachusetts or of the United States of America." Those who know Professor Struik, either in his position at the Massachusetts Institute of Technology, or as a citizen, have confidence in his integrity and are convinced that these charges are without foundation.

Professor Struik is a mathematician of international reputation and has been on the faculty of the Massachusetts Institute of Technology for twenty-five years. He has also worked in fields of science and history related to mathematics. In his book, Yankee Science in the Making, published in 1948, he studied the ways in which science has influenced our American democracy. Moreover he has lectured for many years on questions of foreign policy, science and social philosophy. Basing his views on his Marxist philosophy, he has defended the conclusions at which he has independently arrived, regardless of whether his ideas were popular or unpopular. He has never refused to present his opinions openly before diverse groups -- political, social, cultural, religious. He has been particularly interested in American-Soviet understanding as a prerequisite for world peace.

In a public statement of policy issued in 1949, the Executive Committee of the Corporation of the Massachusetts Institute of Technology declared: "They [the faculty] must be free to examine controversial matters, to reach conclusions of their own, to criticize and be criticized. Only through such unqualified freedom of thought and investigation can an educational institution, especially one dealing with science, perform its function of seeking truth."

The persecution of men and women for their ideas is taking place with increasing frequency in our country. We here express neither agreement nor disagreement with Professor Struik's opinions, but we consider that his indictment is a new attempt to stifle serious political discussion. This indictment menaces the freedom of expression of all of us, and in particular that of teachers. At a time like this, in which the American people have to make decisions of the gravest importance, it is imperative that all opinions be heard without intimidation.

A committee is now being formed by a group of citizens to see that the significance of this case is made known to the public, and to see that adequate funds are raised for Professor Struik's defense. Although it is called the Struik Defense Committee, it is not in reality defending any one individual. It is maintaining American principles and the rights of every individual. We invite you to join us in sponsoring this committee and to allow your name to be used in presenting its program to others.

Helmut Heckscher
4 Potter Park
Cambridge 38, Mass.

October 15, 1951

Professor Norbert Wiener
MIT
Cambridge, Mass.

Dear Sir:

As a member of the program committee of the International Student Association in Cambridge I would like to ask you whether you could speak to us some Sunday evening in February or March next year. Most of our members, American as well as foreign, are students at MIT or Harvard who regard our 'Center' on 32 Garden street as a cultural as well as social institution. To be able to hear you speak would naturally be considered a great privilege by all of us and would constitute one of the highlights of the semester.

If you can and will speak to us, our program committee would like to suggest a topic such as : "From Archimedes to Cybernetics, a birds-eye view of applied mathematics," but of course you need not feel bound to this topic. The audience- due to the limited size of our rooms- would not be very large, but as in the past we usually had question periods at the end of the talks, the small size of the audience has always appeared to us as an advantage.

Whether your answer be affirmative or not, I would be very grateful to you if you would let me know soon, so that we can go ahead with our plans. For your convenience I enclose a stamped, self-addressed envelope.

Very truly yours

Helmut Heckscher

[ans 10-19-51]

THE OHIO STATE UNIVERSITY

HOWARD L. BEVIS, *President*

COLUMBUS 10

DEPARTMENT OF SPEECH

October 15, 1951

Mr. Norbert Wiener
Sandwich, New Hampshire

Dear Mr. Wiener:

The Department of Speech is planning to present a series of campus lectures by outside speakers during the summer quarter, 1951. These talks might begin about the middle of July, if schedules can be met. The general topic of the series is: A Theory of Rhetoric for the 20th Century.

The talks are planned particularly for speech students, who would be invited to attend. But they would be open to the University and the general public. There would be no admission fee charged.

Our general plan well might be to have the speaker give two or three talks over a two or three day spread. It might be arranged to have him meet interested students in informal conferences.

We should be happy to hear from you on the matter, to learn if you are interested and available. We would welcome any suggestions you might have. It is possible you are now working on something close to our general subject.

Sincerely yours,

Earl W. Wiley
Earl W. Wiley,
Professor of Speech

eww:ms

[ms 11-21-57]

INSTITUTE FOR THE UNITY OF SCIENCE

American Academy of Arts and Sciences
28 Newbury Street
Boston 16, Massachusetts

October 17, 1951

Dear Sir:

The first meeting this fall of the study group on Cybernetics and Communication Theory will be at 7:45 P.M., Wednesday, October 24, in the Psychology Lecture Hall (Room 201) of the Harvard Psychological Laboratories in Memorial Hall. At this meeting and the following one, Wednesday, November 7, Prof. Karl W. Deutsch (Dept. of English and History, M. I. T.) will discuss: "Cybernetics and the social sciences: (1) some basic considerations; (2) some cases of application."

As previously announced, this year's meetings will be concerned chiefly with applications of the general theory to specific fields. Topics for future meetings include: Psychology, Language, Neurophysiology, Computing Devices.

Decision has not yet been reached as to a permanent place for these meetings. The committee will welcome suggestions as to suitable meeting places and as to possible speakers and topics.

The Steering Committee

Mary A. B. Brazier
Karl W. Deutsch
Walter A. Rosenblith
Frederic A. Webster

Telephone:
Cleeve Hill 241

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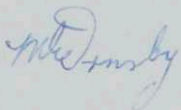
17th October, 1951.

Prof. N. Wiener,
Apartamentos Continental,
49 Calle Humboldt,
Mexico, D.F. Mexico.

Dear Prof. Wiener,

Before going abroad on leave,
Dr. Bronowski asked me to let you
have a copy of his paper entitled
"Mathematician's Paradox", which
I now enclose.

Yours sincerely,



M. E. Ornsby (Miss)
Secretary to Dr. J. Bronowski.

EYRE & SPOTTISWOODE

(PUBLISHERS) LIMITED

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TEMPLE BAR 8514
(9 LINES)

15 BEDFORD STREET
STRAND, LONDON, W.C. 2

Telegrams
EXALTEDLY LESQUARE
LONDON

17th October, 1951.

Dr. Norbert Wiener,
Nilo 16, Apt. 2,
Mexico, D.F.,
Mexico.

Dear Dr. Wiener,

Many thanks indeed for your letter of the 4th, which has just reached me, and which brings the good news that the manuscript of "The Bent Twig" is on the way. I shall look forward to reading it with very real interest and will report as soon as I have had the chance of doing so.

This has been a summer of perpetual interruptions, but I hope there will be a better chance of getting Peggy to come and see us during the next term. I want to persuade her to attend to other things than fruit flies. She seemed to be getting a great pleasure out of them, but they are dull things, I feel, at best.

I had the pleasure of taking my brother Christopher to the Sherlock Holmes Exhibition in Baker Street.

More anon, when, and I hope soon, "The Bent Twig" arrives.

Yours,

F. V. Modley

The Saturday Review

OF LITERATURE

25 West 45th Street
New York 19, N. Y.

Raymond Walters, Jr.

October 18th
1951

Dear Mr. Wiener:

Thank you for your frank letter about the "Loneliest Girl in the World." As a matter of fact, we have decided to follow your advice as to what treatment to give it in our magazine.

Sincerely,

Raymond Walters, Jr.

Raymond Walters, Jr.

Mr. Norbert Wiener
Nilo 16, Apt. 2
Mexico, D.F. Mexico

RW/bs

318 W. Forest Street
MICHIGAN STATE NORMAL COLLEGE
YPSILANTI, MICHIGAN

October 18, 1951

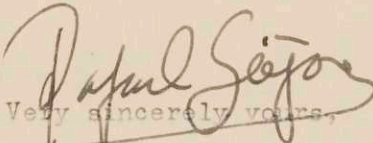
Department of Mathematics
(Cibernetics)
Massachusetts Institute of Technology
Cambridge, Massachusetts

Gentlemen:

My purpose is to request information on cibernetics. I ask on behalf of Dr. Eduardo Ortiz, of Argentina, who is extremely interested in that science, especially in recent developments at MIT.

I would be very grateful, therefore, if you would send me a bibliography of information on the subject.

Thank you.


Very sincerely yours,

[ans 11-21-51]

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 18, 1951

Mr. Robert A. Bone
Chairman, "Focal Point"
1987 Yale Station
New Haven, Conn.

Dear Mr. Bone:

I am very much honored by your request to lecture before the "Focal Point". However, there are two obstacles which make it impossible for me. One is that I am in Mexico until February and the other is that I have found it necessary to cut all lecturing out of my schedule, as I find that it has become an occupation consuming the time which I need for further scientific work. I am sure you will understand.

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 18, 1951

Professor Theodore T. Kozlowski
Department of Botany
University of Massachusetts
Amherst, Mass.

Dear Professor Koz lowski:

Thank you very much for your invitation to lecture at the School of Science Convocation at the University of Massachusetts. However, on the day in question I shall still be in Mexico, and in addition, I am giving up lecturing in the future , as I find that it interferes too much with my scientific research.

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 18, 1951

Miss Laura S. Levy
Brandeis University
Waltham, Mass.

Dear Miss Levy:

I am out of the country for a period of months, and I am not in a position to give you the material you request. If you look me up sometime after January when I am at M.I.T. again, I may be able to do something for you.

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 13, 1951

Professor Edgar R. Lorch
Department of Mathematics
Columbia Univerity
New York 27, N.Y.

Dear Professor Lorch:

I shall be glad to indorse your application for a Guggenheim when the papers come into my hands. I have had a most interesting time in Europe, and I have been busy with a lot of new research of which I should like to tell you when we next meet. With best wishes from house to house,

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 18, 1951

Dr. Adrian C. Moulyn
160 Post Road
Darien, Conn.

My dear Dr. Moulyn:

I am out of the country at present, so that the question of ^{the} presenting of your material before one of our groups does not become practical before February. Meanwhile I am passing on your note to one of my friends, Dr. Arturo Rosenblueth of the Instituto Nacional de Cardiologia here in Mexico City, who will be very much interested.

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 18, 1951

Mr. S. Stein
319 W. 105 Street
New York, N.Y.

Dear Mr. Stein:

I am in Mexico until the end of January when I shall possibly pass through New York. We may be able to see one another there. As far as cybernetic people are concerned around New York, I suggest strongly that you look up Dr. Claude Shannon at the Bell Telephone Laboratories in Murray Hill, New Jersey.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F., Mexico
Oktober 18, 1951

Sehr geehrter Herr Stüler:

Ich weiss ganz gut, dass mein Buch "Die Cybernetic" eine Menge Druckfehler enthält, und sogar einige, die etwas mehr als Druckfehler sind. Zur Zeit der Druckrevision habe ich mich einer Augenoperation unterziehen müssen. Ich habe die ganze Lage mit meinen Verlegern in den Vereinigten Staaten und in Frankreich besprochen, und wir sind zu der Entscheidung gekommen, dass das Beste ist zu warten, bis die innere Entwicklung der Cybernetic das Schreiben eines wirklich neuen Buches berechtigt. Ich werde vielleicht binnen zwei Jahren dazu kommen, und das neue Buch wird in etwa drei Jahren erscheinen. Diesmal habe ich die Zeit die ganze Sache langsam und genau zu besprechen, und ich habe viel mehr Stoff von mathematischem, sowie physikalischen, biologischen und philosophischem Charakter hinzuzufügen.

Mit bestem Dank für Ihr Interesse,

Ihr ergebener

Wilo 16, Aprt. 2
Mexico, D.F. Mexico
October 19, 1951

Prof. Dr. D. van Dantzig
2e Boerhaavestraat 49
Mathematech Centrum
Amsterdam-O., Holland

Dear Professor van Dantzig:

I think that you may be able to get some more material on cybernetics from the Macy Foundation in New York. I might also be able to get some more papers to you through the department of mathematics once I am back at M.I.T. in February. At present I am planning a new and more comprehensive book on cybernetics, but it will be a matter of a year or two before the project becomes a reality.

Many thanks for your interest in my work. Wishing you all success in your project, I am,

Sincerely yours,

Nilo 16, Aprt. 2
Mexico, D.F. Mexico
October 19, 1951

Mr. Jerome Rothstein
Director, Evans Signal Laboratory
Belmar, New Jersey

Dear Mr. Rothstein:

I appreciate very much your sending me your article. It looks to me like very good stuff and I intend to use it in my own future work. In particular I have some definite ideas of a generalized statistical dynamics in equilibrium, in which the probability distributions of a certain quantity in the present depends on their entire past history. Thus the phenomena which I am discussing are more general than the Markoff processes. I am particularly interested in following out your suggestion to study such a system in the case where the range of observable quantities is limited. Like the quantum mechanics such a system gives a statistical linearity on the base of a non-linear theory of the fundamental processes.

I believe that ordinary quantum mechanics may be subsumed under such a theory by introducing a non-observable quantity of the nature of phase, thus returning to the five-dimensional relativity of Fock and Klein, which is being resuscitated these days. The probability density appears as a sum of a quantity having phase over all conditions of the system which are admissible instead of a sum of a fixed quantity all over the same states.

I think it is important to notice that if we are going to take a strict operational position in physics, neither position nor momentum is observable. To observe position we need light of infinitely high frequency, which does not exist; and to observe momentum which needs the use of light of zero frequency, which while it exists can be used to illuminate objects. I do not think that we can get a readily operational quantum theory without an explicit consideration of the coupling of the thing observed to the instruments of observation; and this involves a consideration of the physics of systems of many particles. In other words the classical basis of quantum mechanics, which consists of the study of the property of an uncoupled system, is unphysical and is merely a shorthand of a station in which the observer is not merely represented from the outside but is intrinsically a part of the system discussed.

It will not be many months before I begin writing a new book which is to be a revision of my cybernetics with special attention to its application to fundamental physics. I shall consider myself at liberty to use some of the ideas contained in your notes of course with due acknowledgement. If there is anything in this letter concerning which you would like to write me, please do so by all means.

Sincerely yours,

23/ X/ 1951.

21 Rue de Malte
Paris XI^e

Dear Professor Wiener,

Fortunately enough, I am enjoying a good deal of free time now, so that the book is substantially progressing. More accurately, the whole material included in the eight first lessons (i.e. up to and including Plancherel's theorem) is written down and you will have it in a ten days.

I stress that this is a very preliminary version needing a good deal of smoothing, filtering and extrapolating and I hope to be fed back with serious criticisms and advices. Two points are lacking: an exposition of the alternating process for which I need material from you for it is very shortly alluded to in the papers you let me and an explanatory section on the relationship between group characters and the duality (series/integral) of Fourier. Now I am on the Tauberian theorems and, I admit, a bit at a loss. Some more details or a further bibliography would be of great help for preventing me of confining me to the tools used with wider scope in your book on Fourier Integral. I feel badly how easier it would be if I could be with you now.

As much as I would be grateful for this, as much I must say that I am confident and pleased with this work and I thank you in both directions of time for taking me for ~~this~~ it.

Yours respectfully

M. L. F. H.

[Jan 10-27-51]

84 Early Street
Morristown, New Jersey
October 24, 1951

Dear Dr. Wiener:

I have read with great interest your "Cybernetics" and "The Human use of Human Beings". With so much interest, that I am taking Calculus evenings, at Rutgers, so that I might more understand your mathematics.

I must, however, take exception to parts of your "Human use of Human Beings", (which being philosophical in nature, is liable to exception). The exception shall be on the education of the American youth, or lack of education if you prefer.

CASE HISTORY

I was born in New York City (Queens County) to middle class parents I went to the local public school, then to the local Junior High School. Since I was interested in science, I went to Peter Stuyvesant High School. My course of study was the usual: social sciences, German, mathematics, and the sciences (chemistry, physics and biology). The next two years were spent in the Army. I received my B.A. from Hunter College of the City of New York (Veteran's Session). My course of study included: social sciences, German, Greek, mathematics and the sciences (Chemistry, physics and biology). I am now employed as assistant pharmacologist in the Maltbie Laboratories, Newark, New Jersey. The Pharmacological Labs, however, are located in Morristown, New Jersey.

The above case history, is, I believe normal in every respect, save one. This one is the study of Ancient Greek. Since you too have studied these people (thru their language), I shall speak to you as Greek to Greek.

You deplore our education, because we thereby lack in culture. Culture, as we Greeks know, could easily be shown to be the arts of social intercourse. Culture as the barbarians know, is the asthetic appreciation of art (in its various forms). This sophisticated attitude is not acceptable

to the Greeks, because it tends to exclude the mass rather than try to include them.

The proper topic of conversation is man, his life, and his death. Natural corollaries include ethics, morals, the relation of man to man, and man to his gods. Beyond this, there is no topic of conversation.

A thorough study of history, Latin, Greek, poetry, English (if you will), will not tend to include the above mentioned topics. Rather, it shall tend to exclude them. But these are the very courses you would have us have.

You have said, or have implied, or would imply, that modern youth cannot, nor will nor speak about ethics, etc. This also must I take exception to.

While going to High School, I was president of a young peoples group in my church. (This was before Greek). There we individually lectured, and discussed questions of ethics (not religion). In the Army (that pagan and ill-mannered fortress). I could always find a debate. Presently, I am again president of a young adults group in my church (in a different town), where we again discussed ethics. Last week's topic was on the question of "God's being Perfect". It was decided that he was not. For this statement to be shown and proven in a Presbyterian Church, by Presbyterian youth, is to me proof of their intellectual stature.

This state must be as a result of modern education. Could such a thing have been said 100 years ago, in the era of a "good and solid education"?

Again sir, as Greek to Greek, what of the matter of form and purpose. What a shock it was to the classical scholars to find out that the Greeks colored their statues. But then again, how perfectly acceptable to a Greek. Was there ever a human being who's skin had the color of marble? Again, to the Greek, a thing was beautiful only in it's proper surroundings. A techné should be learned only if it has a use. There is only one instance (that I know of) when the Greek worshipped an old thing; that was the Iliad. This was worshipped, however, not because it was old, but rather because it had very clear examples of virtue contained within.

So we have modern education, which is based on modern civilization. 18th century education in the 20th century would be out of place, and being out of place, it would be wrong, kakos.

If we consider the teaching of "culture" in the American school, we (I) find them poor; again kakos. As you have said, there are two (at least) versions of the Civil War, one for the Northern Schools, and one for the Southern. Philosophy classes are survey courses. Logic teachers are too dogmatic, or they are sophists. The study of English Literature is out of key with English Civilization. The study of Politics is nauseous, while to study Economics is to be appalled. To study music and art, is to be driven mad, and to become too overdependent on titles for the identification of art objects.

You have said that there are too few composers; I say that there are none. But then, this is another discussion.

To conclude sir, I must take exception to your saying that modern education is unfit to provide material for modern civilization. I say that modern education does not fit one with an ethical sense of right and wrong; but then scholastic education has never done that. I say that modern youth like to discuss topics of metaphysics, but modern civilization does not provide the time for this. This civilization sir, is of your making, and of our father's making. We live in it, and are called fools for so doing.

To return to "Cybernetics". I am on one of the lowest rungs of my professional ladder, as a mere B.A. I feel a great future for Cybernetics in physiology, and I would like to read the pertinent literature. I would greatly appreciate the bibliography on this department of Cybernetics.

Yours truly,
Kristen M. Tvede
Kristen M. Tvede

Oct. 25, 1951

Dear Norbert -

You have probably followed events here in connection with my indictment. I am suspended from M.I.T., get my salary, & use the enforced vacation in writing a companion volume to my Differential Geometry, now in M 441 - algebraic geometry.

My lawyer, Oliver Allen, & I, are convinced that this case can be won. We have strong support - an example is the fact that my bail - \$10,000 - was raised in less than two days by friends. What the exact accusation is we do not know, the Commonwealth does not seem to be in a hurry to issue a Bill of Particulars. I need not tell you that the whole case is phony - I never advocated "overthrow of the government," & even less engaged in a "conspiracy" - with Winner, & two ladies, one of whom is already for several years in Chicago, & the other in France.

A defense committee is being set up. Warren Ambrose is very active in it, & Hurewicz is also doing work. I enclose a statement. Do you care to sign it?

I hope that you can raise interest in your

circle for the importance of the case as a frontal
attack on civil liberties. The more people know
about it the better. Letters or petitions *amicus*
curiae, or addressed to the DA or the governor, are
not advisable at present. Friends are.

Sorry I could not come to Mexico in Sept.,
but my lawyer advised against it.

Good luck with your work!

Sincerely

Die Strike

52 Glendale St
Belmont 70
Mass

Did you see the excellent article
by I. F. Stone in the "Commons" of Oct.
9, 10, 11?

[ans 11-14-51]

STATEMENT BY STRUIK DEFENSE COMMITTEE

On September 13, 1951, Professor Dirk J. Struik of the Massachusetts Institute of Technology was indicted under the laws of Massachusetts on charges of advocating, advising, counseling and inciting "the overthrow by force and violence of the government of the Commonwealth of Massachusetts," and conspiracy to advocate, advise, counsel and incite "The overthrow by force and violence of the government of the Commonwealth of Massachusetts and the government of the United States of America."

Professor Struik has said: "I have never advocated orally or in writing the overthrow of the Commonwealth of Massachusetts or of the United States of America." Those who know Professor Struik, either in his position at the Massachusetts Institute of Technology, or as a citizen, have confidence in his integrity and are convinced that these charges are without foundation.

Professor Struik is a mathematician of international reputation and has been on the faculty of the Massachusetts Institute of Technology for twenty-five years. He has also worked in fields of science and history related to mathematics. In his book, Yankee Science in the Making, published in 1948, he studied the ways in which science has influenced our American democracy. Moreover he has lectured for many years on questions of foreign policy, science and social philosophy. Basing his views on his Marxist philosophy, he has defended the conclusions at which he has independently arrived, regardless of whether his ideas were popular or unpopular. He has never refused to present his opinions openly before diverse groups -- political, social, cultural, religious. He has been particularly interested in American-Soviet understanding as a prerequisite for world peace.

In a public statement of policy issued in 1949, the Executive Committee of the Corporation of the Massachusetts Institute of Technology declared: "They [the faculty] must be free to examine controversial matters, to reach conclusions of their own, to criticize and be criticized. Only through such unqualified freedom of thought and investigation can an educational institution, especially one dealing with science, perform its function of seeking truth."

The persecution of men and women for their ideas is taking place with increasing frequency in our country. We here express neither agreement nor disagreement with Professor Struik's opinions, but we consider that his indictment is a new attempt to stifle serious political discussion. This indictment menaces the freedom of expression of all of us, and in particular that of teachers. At a time like this, in which the American people have to make decisions of the gravest importance, it is imperative that all opinions be heard without intimidation.

A committee is now being formed by a group of citizens to see that the significance of this case is made known to the public, and to see that adequate funds are raised for Professor Struik's defense. Although it is called the Struik Defense Committee, it is not in reality defending any one individual. It is maintaining American principles and the rights of every individual. We invite you to join us in sponsoring this committee and to allow your name to be used in presenting its program to others.

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 25, 1951

Professor Ambrose
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Mass.

Dear Ambrose:

I am absolutely in agreement with you that Struik is a good man and that any interpretation of his lectures on Marxism as subversive activity or treason is a gross verbal abuse of the facts. However, in periods of emotional confusion as the present, indictment is often taken to be conviction, and the fact that he is good man is of no protection against the persecution of his determined enemies. I therefore feel that Struik needs the best of legal help to cope with the present situation. I am enclosing a personal check for one hundred dollars to apply to the fund for Professor's defense.

When you see Struik, will you tell him that I am greatly distressed by his predicament and that I wish him all success in a clear and unambiguous acquittal.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D. F. Mexico
October 25, 1951

Dr. Edwin Gordy
4312 Spruce Street
Philadelphia, 4, Penna.

Dear Dr. Gordy:

I am present away from M.I.T. and in Mexico and cannot answer with any authority concerning work now undertaken there. I advise you to write to Prof. J. Wiesner of the Electrical Engineering Department who will be able to answer you more fully.

Sincerely,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 25, 1951

Professor William R. Ransom
13 Barrows Road
Reading, Mass.

Dear Professor Ransom:

I am under the impression that I have sent the Century Fund fifty dollars and the stub is here to prove it. We have not got the return check here to prove it, since we left the bank statement in the States. We mailed the check from France. Could you check the matter up whether the check was received or not. My memory of Maulsby is very fond and I knew his boys very well. I am enclosing an additional fifty dollars.

I am very sorry to hear of your retirement, although I can only imagine your retirement to be a very active one. Best regards to you, Mrs. Ransom and my Tuftonian friends.

Sincerely,

[ans. 10-29-51]

Nilo 16, Apt. 2
Mexico, D.F. Mexico
Oktober 25, 1951

Sehr geehrter Herr Doktor:

Ich danke Ihnen für Ihr Interesse an meiner Arbeit, muss aber gestehen, dass ich wenig Vertrauen in rein feld-theoretischen Erklärungen der Nervenerscheinungen habe. Ich gebe zu, dass Nervenerscheinungen vielleicht nicht völlig kanalisiert sind; es lässt sich aber nicht leugnen, dass der kanalisierte Teil überwiegend wesentlich ist. Ich habe auch keine Hoffnung, dass man mit einem Schlag das Problem lösen kann.

Sie werden mir verzeihen, dass ich so direkt bin, aber bei wissenschaftlichen Unterhaltungen, wenn die Direktheit fehlt, fehlt alles. Zur Zeit bin ich von meiner Schule abwesend, habe also Ihr Manuskript nicht zur Hand. Soweit ich weiss, bin ich mit O. Wiener nicht verwandt.

Hochachtungsvoll,

"SCIENTIA,"

Organo internazionale di sintesi scientifica - Revue internationale de synthèse scientifique - International Review of Scientific Synthesis - Internationale Zeitschrift für wissenschaftliche Synthese - Revista internacional de síntesis científica

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LA DIRECTION

~~Milano~~ 26 Octobre 1951

~~23, Via A. D. Togni~~

~~STECHEX PRONIXOIREX XX~~

ASSO (COMO)

PROF. WIENER

PARIS XV°

5, rue Léon Lhermite

Cher Monsieur,

veuillez m'excuser si je me permets de vous rappeler notre correspondance du Février écoulé au sujet d'un article de vous pour "Scientia" sur Que-ce que c'est la cibernetique.

M. le Prof. Persico m'informe de ne vous avoir pas vu à Rome. Je viens donc vous rappeler votre promesse et j'espère que vous aurez maintenant un peu de temps pour rédiger pour nous un bref article (six pages, environ 2500 mots) sur ce sujet de la Cibernetique; article pour un grand public non spécialisé auquel il faut parler un langage simple (tant que possible) en s'adressant à l'intuition plutôt que se valoir du langage mathématique, tout en conservant à l'article sa valeur scientifique.

J'espère dans votre adhésion à notre proposition, car ce sont précisément les savants les plus autorisés (qui partant ont les idées plus claires) qui peuvent laisser de côté pour une fois l'expression technique pour s'adresser à un public plus large.

Je vous prie d'agréer, Monsieur, l'expression de mes sentiments les plus distingués

S. "SCIENTIA."

(Dott. Paolo Bonetti)

*Chief Italian journal -
mention au article
le 27/10/51 G. B. Wien
Haber*

R.11.CO.

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 26, 1951

Mr. Lawrence K. Frank
72 Perry Street
New York 14, N. Y.

My dear Mr. Frank:

I have read over the paper on "Genetic Psychology and its Prospects", and I am making these remarks as they occur to me in reading. If I had my way, I would impose a \$64.00 fine on every use of the word "field theory" by those who are not fully conscious of its implications. Field theory for Einstein and in physics represent a direct descendant of Leibnitz' view of an infinitely divisible world. It goes back to the biological attitude to the spermatozoon and the embryo as the homunculus. This attitude you very correctly criticize. In physics a field theory implies that the world is essentially a continuum, and that the transformation of representing the world on a larger and larger scale does not involve some terminal but finite implication by which we find a purely granular structure.

Now physically this point of view of Einstein's is already considerably discredited. Non-linear, partial differential equations such as those of Einstein change their character very fundamentally under a high degree of amplification. I am very sceptical about the attempts of such unphysical mathematicians as Garret Birkhoff to apply hydro-dynamics over an excessive range of scales. The fact is clear that the non-linearity of hydro-dynamics phenomena ultimately involves shockwaves and that these shockwaves are of dimensions such that the known granular structure of the molecules will seriously interfere with them.

I thus consider that the Leibnitzian attempt to move as freely in changes of scale as within translation and rotation, has proved a failure or at least a very qualified success. In dealing with society where the unit, the human being, is much nearer in the scale to the society as a whole than the molecule is to the table, or in dealing with the human being which only contains something like 10 to the 10th neurons, field theory is bound to come an even worse cropper than it has in physics. It is a very convenient and sloppy way of ignoring the very limited divisibility of things. Furthermore, I do not think it is what we really want, or that it serves in any particular way to make easier the study of personality and individuality. I do not for a moment mean to assert that the neurons and their sympathetic action give a complete account of what happens in nervous activity. I merely mean to say that their granularity is important, and that even when it is associated with phenomena

which may run across boundaries from neuron to neuron, these phenomena themselves are modified in a very significant way by the histology of the nervous system. So please, let me hear no more of field theory except as a facile over-simplification of a very complicated class of phenomena.

As I read over your article I am impressed with the fact that my cybernetic point of view, while I think that it will eventually be of very considerable use in the study of psycho-analysis, as for example has been evidenced by Bateson's book, is miles away from the present vocabulary and methods of psycho-analysis as shown in your paper. Cybernetics is essentially an analytical method and is at its best in studying systems whose elements are fairly well known and of definite properties, in such a way that the properties of the organism emerge from the properties of the elements.

With all its difficulties neurology is not too far from this state; nor even is sociology a hopeless field for the application of cybernetic methods, as the recent work of Karl Deutsch on the problem of nationality has shown. To a certain crude extent we can take the behaviour of the individual as known, and can try to build up a theory of social behaviour from this. In psychology however, the problem of an adequate knowledge of the elementary processes is far from complete, and although Bateson has done valiant work in attempting to bring psycho-analytic processes under the heading of cybernetics, his work is and must be sketchy. The vocabulary of psycho-analysis abounds in vagueness and clichés to the extent that it is often very difficult to find a substantial agreement in the application of this or that term.

I am afraid that I cannot acquit your article of a not too clear use of some of these ponderous terms. To a certain degree this is unavoidable, for a large part of psycho-analysis is still far from the scientific stage. The vague general terms which we use are in many cases the best we can do at the present time, and will serve descriptively if not taken too seriously. Bateson's work is more of a study of communication in psycho-analytic practice than of the communicative nature of the phenomena observed by the psycho-analyst. I have considerable hopes that out of this may come a deeper knowledge of psycho-analysis itself; but these hopes are drafts on the great bank of the future, and I have not the slightest idea of what its paid-up capital may be.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 27, 1951

Dr. James V. Neel
1135 E. Catherine Street
Ann Arbor, Michigan

Dear Dr. Neel:

I have not been able to penetrate beyond the most superficial layers of this paper. It seems to be a paper on semantics written in what purports to be English by a person very incompletely acquainted with the semantics of the English verb. In a delicate subject like this a failure on the part of the author to write the language which he intends in a precise and accurate way, is fatal to the intelligibility of the article. I am afraid that I am much too busy to undertake the task of translating the article for my own benefit into standard English.

The article contains a considerable amount of symbolism. The symbolism seems to consist largely in the geometric juxtaposition of the symbols in accordance with a code which is not made reasonably obvious. This is the equivalent fault in mathematics of the authors fault in English and makes all precision of statement impossible.

This does not take me far enough to convince me that the article does not have in it something of merit. I do not say that it has, but I confess myself simply unable to give the article the further examination necessary to find the needle in the haystack. If the author or any other person wants my opinion of the material, it must be sent me in a form in which the English language approximates closer to the usual norm and in which some attempt is made to explain in a clear way the code of the symbolism.

Sincerely yours,

Nilo 16, Apt. 2
Mexico, D.F. Mexico
le 27 octobre, 1951

M. Le Baron de Gouttes
Chateau d'Arcambal, Lot
Region de Cahors
France

U Monsieur le Baron:

Je viens de recevoir votre aimable lettre dans laquelle vous parlez de la creation d'un centre experimental dans votre chateau et dans laquelle vous m'offrez les pieces de votre chateau pour y continuer mes recherches dans la Cybernetique. Je trouve votre idee tres interessante, mais il faut que je sache encore plus des circonstances de ce projet avant d'arriver a une decision. J'ai ecrit a Dr. Schuetzenberger qui a suivi au cours de conferences que j'ai donne au College de France cette annee et qui est en train de rediger mes conferences avant leurs publications, et je l'ai prie de se mettre a votre disposition pour parler avec vous de vos plans d'un centre de recherches scientifiques.

Je ne suis plus en France que j'ai quitte en aout. A present je me trouve au Mexique, ou je continue mes recherches pour quelques mois avant de revenir a mon ecole, the Massachusetts Institute of Technology a Cambridge, Mass, U.S.A. en Fevrier. A ce temps-la j'espere d'avoir plus de nouvelles de vous et de votre plan pour le discuter a mon ecole.

Avec beaucoup de remerciements pour votre invitation et pour votre bonne volonte a la science, je vous assure de mes sentiments les plus respectueux.

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 27, 1951

Dear Dr. Schuetzenberger:

I have just received your letter. I am sorry to hear of your illness and that you will not be here with us at the Instituto, but I am glad to hear that the book is coming along well. Now I am in receipt of a letter from France which may mean interesting possibilities in the future. I have just received a letter from a Baron Jean de Gouttes, Chateau d'Arcambal, Lot. in the region of Cahors. He asks me if I would be interested in the creation of a center of experimental research at his chateau. He wishes to devote this center to experimental research in physics, chemistry, together with a biological laboratory. He offers to put at my disposition large rooms, so that I may develop there my work on cybernetics. He also wishes to find out if I will give him the task of translating Cybernetics into French. He seems to be of the opinion that I am still in France and that I shall be there for some time. While of course I am now in Mexico and shall spend at least next term and probably several years at the Massachusetts Institute of Technology, the offer of the Baron is in fact interesting to me.

It is not clear from the Baron's letter just to what extent he is prepared to give financial support in the founding of this institute, nor exactly what he would consider my role in the undertaking. I hear that he is a rich man, but I doubt whether he realizes the very large expenses which the foundation of such an institute demands, if it is to be first rate. Before going into the matter further, I should be very much obliged if you would get in touch with the Baron and let me know exactly what he means and how serious he is. There might be a possibility for a rather permanent employment for you in this matter, so I think that I am not asking too much of you to give me a report on the situation and even go to Cahors if necessary. Please make it clear to the Baron that I am interested, but that I cannot go into the matter further without more data.

Just this moment the mailman brought your letter and I am pleased to know that I shall soon have the first eight lessons of my lectures. I am going to fill in as soon as I find time material on the alternating processes and references on group characters. I shall send to M.I.T. for a copy of my reprint on Pauterian theorems or at least a reference to the paper. Meanwhile all thanks.

Sincerely,



HOUGHTON MIFFLIN COMPANY

2 PARK STREET · BOSTON 7

October 29, 1951.

Dr. Norbert Wiener,
Nilo 16, Apt. 2,
Mexico, D. F. Mexico.

Dear Dr. Wiener:

Knowing that you are anxious for early word on *THE BENT TWIG*, Dorothy and I and other members of our staff read the manuscript promptly and we have discussed it at some length. For those of us who are your friends, it is a particularly interesting document. It is amazing with what accuracy and in what detail you have been able to recall the events of those early years. And in addition to the factual information, the manuscript has a great deal of charm and human quality.

Since this is such a very personal book and has such direct bearing on your own later career, we feel that it would be most appropriately published by your own associates at the Technology Press. Their facilities would be better than ours for reaching the people who have been long interested in your work and therefore in the personal background behind it. This is the sort of book that requires what you might call rifle rather than shotgun treatment. Houghton Mifflin, owing to its large organization and overhead, is necessarily confined to the shotgun. This was quite appropriate with *THE HUMAN USE OF HUMAN BEINGS*, a book basically of ideas, but we have reluctantly concluded that we could not do so well with a personal testament.

I am sending a copy of this letter to Mr. Fassett and holding the manuscript awaiting his instructions. A copy also goes to Mr. Morley of Eyre & Spottiswoode.

With all good wishes,

Sincerely yours,

A handwritten signature in blue ink that reads "Paul Brooks".

Paul Brooks

PB/MM

[Ans 11-8-50]

The Technology Press



MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE 39, MASSACHUSETTS

October 29, 1951

Dr. Norbert Wiener
Nilo 16, Apt. 2
Mexico, D. F.
Mexico

Dear Norbert:

It was a great pleasure to have your letter of two weeks ago and subsequently to receive the manuscript of your autobiographical book, "The Bent Twig." I have read the manuscript with considerable interest and satisfaction. It is not, it appears to me, the kind of thing that falls within the scope of The Technology Press. I shall be much interested in the response of the Houghton Mifflin people to it and I shall, of course, be glad to do anything that I can as *amicus curiae* (construe that genitively or datively as you will).

I look forward to your return for there are many things that we have to talk over. I'm going to New York tomorrow and after my return will write you further.

With every good wish,

Very truly yours,

Fus

F. G. Fassett, Jr.
Director

via Air Mail

[ans 11-8-51]

La Quadra

Hotel Compele

Mr. Louee

recd. Dr. Wygodt

Wopda Basin



CAMBRIDGE 70 MASSACHUSETTS
MASSACHUSETTS INSTITUTE OF TECHNOLOGY

October 29, 1901

Dear Mr. Louee:

I am very glad to hear that you are so much interested in the research of the Boston Millin Society. I shall be glad to let you know the scope of the research of the Boston Millin Society. I shall be glad to let you know the scope of the research of the Boston Millin Society. I shall be glad to let you know the scope of the research of the Boston Millin Society.

I look forward to your return for there are many things that we have to say over. I'm glad to see your connection and after my return will write you further.

With every good wish,

Very truly yours,

Director
F. G. Louee, Jr.

Mr. Louee

WILLIAM R. RANSOM
PROFESSOR OF MATHEMATICS
TUFTS COLLEGE

P. O. ADDRESS: TUFTS COLLEGE
MEDFORD 55, MASS.

Dear Norbert, —

The 2nd Cent Fund office
says it received your check from
France on May 6.

And thank you for your
\$50 toward the Maulaby
Scholarship Fund. This brings
it up to \$4446.

I suppose you have
springlike weather in Mexico
City all winter, and hope your
health permits you to enjoy it.

This coming Saturday

is Home Coming Day at Tufts
and I shall be glad to give
fresh news of you to many people
who remember a small boy who
has since become so distinguish-
ed a man.

With warm regards,

William Rawson.

13 Barrows Rd.,
Reading, Mass..
Oct. 29, 1951,

FANTASY ADVERTISER

1745 KENNETH ROAD

• GLENDALE 1, CALIFORNIA

29 October 51

Dear Norbert Wiener:

It's unfortunately true that a person of renown is subject to a wide variety of indignities over which he has little control. However, I think that he is entitled to a knowledge of them as they occur -- wherefore the enclosed.

All good wishes,



R. A. Squires

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 29, 1951

Mr. Helmut Heckscher
4 Potter Park
Cambridge 38, Mass.

Dear Mr. Heckscher:

I am not inclined to accept a lecture on such a title as : " From Archimedes to Cybernetics". To cover such ground in the space of an hour will lead either to an unintelligible, diluted lecture, or will require such an effort of concentration as I am unwilling to undertake. I shall be glad to lecture to you preferably in March or April. I suggest as a topic: "The Relation between Pure and Applied Mathematics. This topic, while it may seem rather complicated, is one on which I have strong views which differ considerably from those held in certain influential circles.

Sincerely yours,

[Ans 11-20-57]

Nilo 16, Apt. 2
Mexico, D.F. Mexico
October 29, 1951

M. P. Ostoya
Radiodiffusion et Television Francaises
Emissions Culturelles
15 Rue Cognac-Jay
Paris, France

Cher M. Ostoya:

Je suis ici au Mexique depuis deux mois. Les premieres deux semaines etaient occupees par des travaux litteraires dans une pension de la ville de Cuernavaca. C'est un Jardin d'Eden non sans ses serpents. A mon retour dans la capitale j'ai subis a deux semaines de fetes pour le 4ieme centenaire de l'universite. J'ai recu un degre honoraire de docteur et j'ai beaucoup souffert a cause d'un beret octogonale que j'ai du porter et des maintes receptions et fetes. J'ai recommence mes travaux avec le Dr. Rosenblueth et nous avons deja obtenu des resultats tres interessants sur les phenomenes electriques des nerves.

Pour le moment je suis un peu moins presse et je voudrais bien vous donner mes remerciements a cause du travail que vous avez fait dans la redaction de mes conferences et de la maniere aimable avec laquelle vous avez manie mes affaires. J'ai recu les copies de mes conferences aussi bien que l'argent a Thonon-les-Bains. Je ne sais pas quand je reviendrai en France, mais il est bien possible que ce soit en deux ou trois ans. A ce temps-la, nous esperons vous voir encore une fois pour continuer nos relations d'amitie. Madame me joint en souhaitant a vous tout succes et toute prosperite.

Margaret Wiener
Bien a vous.

P. S. Mon mari tombait malade a Thonon-les-Bains d'un mal de tete tres severe et a du passer quelques semaines a l'hopital a Geneve. Il se trouve beaucoup mieux maintenant. Je vous remercie encore une fois.

Lübeck, den 30. Oktober 1951

Sehr geehrter Herr Prof. W i e n e r !

Für Ihren freundlichen Brief vom 6. September danke ich Ihnen recht sehr! Ich habe aus gegebenem Anlass noch eine kurze Zusammenstellung meiner Gedanken gemacht, und darf annehmen, dass auch diese für Sie von Interesse ist, besonders durch die auf den letzten Seiten erfolgende Anknüpfung an die Psychologie des Tieres. Ich habe gefunden, dass man sogar Eigenheiten der Sprache als Ahnenerbe aus dem tierischen Stammbaum herleiten kann: Die Tiere haben Warn- und Schrecklaute, die für die Artgenossen oder die Jungen einem Tätigkeitsbefehl gleichkommen. Teilweise sind das recht differenzierte Systeme. Im Augenblick, da der Mensch zum Bewusstsein erwachte und sich fragte " Was habe ich daraufhin zu tun?", musste er sich mehr oder weniger im Besitze eines Wortschatzes von Tätigkeitswörtern befinden. Tatsächlich lässt sich nachweisen, dass die meisten alten indogermansichen Wörter nicht als Gegenstandsbezeichnungen entstanden sind, ~~da~~ man etwa konventionell zur Orientierung in der Umwelt geschaffen hätte, wie man denken möchte, sondern dass sehr oft noch der Ursprung vom Tätigkeitswort nachzuweisen ist. Beispiele : Hund, Hand beide zusammenhängend mit dem englischen Verbum to hunt, Jagen, also beides bedeutend "Greifer oder Fänger". "Kind" hängt zusammen mit "gignere" erzeugen, also "das Erzeugte", ebenso wiederzufinden in "tekonon" Kind (griechisch), bedeutend das "Gemachte", mit einem Wortstamm, der in dem Worte Technik weiterlebt. U.sw. nach Belieben.

(Der Titel "Der ~~xxxxxx~~ Mensch, das verpfuschte Wesen" knüpft an an Carrell "Man, the Unknown", dessen Titel in der deutschen Ausgabe hieß "Der Mensch, das unbekannte Wesen".) Ich bin persönlich der Ansicht, dass die Erfahrungen der letzten 20 Jahre, richtig ausgewertet, eine genügende Kenntnis des Menschen ergeben könnten, um von dieser Seite keine allzugroßen Ueberraschungen

allzugrossen Ueberraschungen mehr zu erleben. Man muss nur die stetige Linie vom Tier über den Primitivmenschen bis zur Gegenwart fortzusetzen wissen und die Elemente des primitiven Denkens genügend genau kennen, um sie heute auch noch erkennen zu können. — Die Psychologie scheint aber Carrell etwas ferner gelegen zu haben.

Im speziellen möchte ich folgende Reihe machen: Man hat den Menschen ein tool making animal genannt. Man kann beim Tier anfangen, es ist tool using, insofern es Schnauze, Schnabel, Pfoten u.s.w. als angeborene Werkzeuge benutzt, gelegentlich wohl auch Steine und Baumäste als Wurfwaffe. Der Mensch der Frühzeit als tool making animal macht sich Werkzeuge, der fortgeschrittene Mensch ist das tool improving animal, indem es seine Werkzeuge verbessert, und schliesslich ist die letzte Stufe der heutige Techniker, der seine Produkte auf dem Prüfstand nachprüft, das tool examining animal. — Nun ist der Uebergang vom Konkreten zum Abstrakten immer eine Spätererscheinung(— und macht nebenbei bemerkt so viel Schwierigkeiten, dass meine Beobachtungen mich zur Konstatierung eines Kon-Abs-Kollapses geführt haben als ziemlich regelmässiger Erscheinung: Was im Konkreten eine Einsenwahrheit ist, geht im Abstrakten in die Binsen.) deshalb folgen die Stadien im Abstrakten mit grosser Verzögerung: Ist der Geist das tool, so ist das tool examining, die Psychologen, noch sehr jungen Datums. Für die Geschaffenen Hilfsmittel Sprache Δ und Mathematik liegt die Prüfung bei den Mathematikern vor: Axiomatik und Logistik, bei der Sprache aber soll die Ueberprüfung wohl erst noch kommen. Ich habe bei meinen eigenen diesbezüglichen Bemühungen bewusst die Philosophie noch ziemlich beiseite gelassen, um eine allzugrosse Komplikation von vornherein zu vermeiden, habe aber aus dem, was mir doch so über den Weg lief, vor allen Dingen aus den Schriften von Bertrand Russell, den Eindruck gewonnen, dass ~~KE~~ zwar ganz richtig erkannt ist, dass die Philosophen die Sprache mit dem verwechseln, was sie darstellen soll, dass aber die Sprache doch wohl

der Hoffnung hingeben, Ihnen mit diesen Ausführungen nicht lästig zu fallen.

Mit vorzüglicher Hochachtung und mit nochmaligen Dank für Ihren freundlichen Brief bin ich

Ihr

PS. Die Verbesserung des Wirkungsgrades des menschlichen Gehirns scheint mir aus folgenden Gründe besonders dringend: Der Physiker kann durch ein Experiment jeden Sachkundigen überzeugen, der Chirurg kann eine Operation vormachen und dadurch allmählich immer mehr Anhänger seiner Methode gewinnen. Wenn es aber um Angelegenheiten eines ganzen Staatswesens geht, kann man nicht mit Einzelbeispielen experimentieren. Selbst Teilexperimente können für das Ganze ohne Beweiskraft sein (Wenn z. B. Gegner des Impfens darauf hinweisen, dass sie gesund bleiben, kann das daran liegen, dass der Impfschutz der übrigen Bevölkerung ihnen die Gelegenheit zur Ansteckung vom Leibe hält) Bei Wissenschaften aber, deren Methode nicht in Ordnung ist, muss man erwarten, dass selbst Erkenntnisse von 10 oder 20 Prozent der Beteiligten durch den Unsinn, den die Uebrigen machen, wieder restlos verschüttet werden. Sie befinden sich in der Lage etwa einer Pioniergruppe, die eine kleine Oase in einen Urwald gerodet hat, aber diese nicht gegen die aus dem Urwald herüberwähende Samenfülle verteidigen kann, von weiteren Fortschritten ganz zu schweigen.

In dieser Lage - Urwald mit vielleicht vorhandenen, aber hoffnungslos in der Unterlegenheit befindlichen Pionieren - befindet sich vor allen Dingen die Wirtschaftswissenschaft. Dieses Problem halte ich für besonders drohend und daher dringend. Es gilt aber auch für jede andere soziologische Wissenschaft, wenn hier auch wohl ein schneller Fortschritt nicht so dringend notwendig ist. -

Aber man muss wohl sagen, dass die ökonomischen Wissenschaften mindestens 150 Jahre hinter der Entwicklung der Technik herhinken, wobei von dem Fortschritt der Atomtechnik noch ganz abgesehen ist.

Nach meinen Erfahrungen wird die besondere Lagerung bei solchen Problemen der Gesamtheit oft verkannt. Man verlässt sich darauf, dass eine gefundene Wahrheit sich allmählich durchsetzen wird, ebenso wie etwa eine historisch durch einen neuen Beleg zu stützende Erkenntnis. Max Planck aber hat in seiner Biographie als schmerzliche Erkenntnis ausgesprochen, dass es ihm niemals gelungen sei, Erkenntnisse, für die er vollkommen einwandfreie, aber nur theoretische Beweise erbringen konnte, zu allgemeiner Anerkennung zu bringen. Wenn nicht unsere ganz Zivilisation daran scheitern soll, dass sie einmal eingeschlagene Entwicklungsrichtungen nicht mehr abzuändern versteht, so muss also hier in erster Linie der Hebel angesetzt werden.

Ich habe den Eindruck, als ob die Herleitung der hier im Wege stehenden Ladehemmungen des menschlichen Gehirns doch geeignet ist, die Psychologin Bewegung zu bringen.

Ich darf hoffen, für diese mir nachher noch eingefallene Bemerkung Ihre gütige Aufmerksamkeit nicht missbraucht zu haben.

D.O.

allzusehr als ein naturgegebenes Produkt behandelt wird, nicht als ein in seinen Urbestandteilen aus Beobachtungen, Erfahrungen u.s.w. sozusagen "abgeschriebenes" Produkt von Leuten, die ihre eigenen vorzeitigen Deutungen hier hineingeheimnisst haben in einer Weise, die für uns fast unmerklich geworden ist, und sich nur dem sehr misstrauischen Beobachter offenbart, die aber doch noch sehr stark irreführend nachwirkt. (Dieser Eindruck gründet sich besonders auf die philosophische Grundlegung der Naturwissenschaften, die Max Hartmann im Anschluss an Nicolai Hartmann zu geben versucht).

Nach dem bis jetzt gewonnenen Eindruck würde sich eine Unmenge von Auseinandersetzungen als Scheinproblem erweisen, wenn man diesen Dingen nachgeht.

2) Ich habe der Sprache angekreidet, dass sie einen "hier bei mir" gemachten Befund auf ein "Objekt als Ursache" sozusagen hinüberwirft. Be- z.B. sagt: "Die Rose ist rot" oder "die Sonne ist rot". Stelle ich ei- ne elektrische Anziehung oder Abstossung fest, so wird auch die se hin- übergeworfen: Dort sitzt die Ladung, die stösst ab bzw. zieht an."

Fasst man aber die "hier" und "^{hier}~~hier~~" gemachten Beobachtungen nur als solche in einen geschlossenen Gesamtbegriff zusammen, so ergibt sich der Begriff "Feld", eine Art Stenophonie für die Gesamtheit aller dort zu machenden Beobachtungen. Er macht also das Hinüberwerfen rückgän- gig! Einstein (Einstein - Infeld, die Evolution der Physik) erklärt die Einfügung des Feldbegriffes für eine wissenschaftliche Grosstat. Wir sind also hier gezwungengewesen, von einer voreiligen Deutung ab- und auf ursprüngliche Befunde zurückzugehen. Ich habe den Eindruck, dass wir ein solches Zurückwerfen noch öfter erleben werden, insbeson- dere scharf unterscheiden werden müssen zwischen Wörtern, die reine Be- Befunde darstellen, solchen, die derartige Befunde "stenophonisch" zu- sammenfassen, und solchen Wörtern, die bereits irgend eine Deutung, vielleicht sehr versteckt, enthalten.

Da ich von fremden Arbeiten im Augenblick nicht viel erfahre, habe ich Ihnen vielleicht nichts Neues gesagt. Andernfalls darf ich mich

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE 39, MASS.

DEPARTMENT OF MATHEMATICS

October 30, 1951

Professor Norbert Wiener
Instituto Nacional de Cardiologia
300 Avenida Cuahatemoc
Mexico D.F., Mexico

Dear Professor Wiener:

I am very grateful for your kind answer to my telegram and am very pleased to give your name as a reference in my application.

I suppose that the Guggenheim foundation will send you a copy of the data I am submitting to them, but at any rate I am enclosing also a copy here for your information.

After discussing with Norman, I decided to apply for a fellowship which, if granted, would allow me a year of quiet work without teaching.

With all my thanks again and my kindest regards to Mrs. Wiener and yourself, I am

Yours sincerely,



Raphael Salem

RS:rg

The past mathematical work of the undersigned is in analysis and particularly in the field of trigonometric series. His thesis published in Paris in 1940 contains results obtained during the years 1928 - 1939 concerning the convergence of Fourier series, their absolute convergence, the structure of their coefficients, and the extremal properties of trigonometric polynomials.

The results obtained from 1940 up to the present date have been mainly published in the U.S.A. (some of them in Denmark, Holland, Hungary, Poland and Sweden). These results deal principally:

1) with the study of the properties of perfect sets of measure zero which are sets of absolute convergence for trigonometric series.

2) with the study of the properties of sets of uniqueness and sets of multiplicity for trigonometric series. Symmetric perfect sets have been studied from this viewpoint, and in particular symmetric perfect sets with constant ratio of dissection. This study has brought into light the relationship between the problem of uniqueness and the properties of a certain class of algebraic integers which plays also a role in problems of diophantine approximation. Studying this class of algebraic integers for its own sake, the author has been able to prove that it has the remarkable property of being closed.

3) with the study of continuous monotonic functions purely singular and having a perfect set as spectrum. The problem of the order of magnitude of the Fourier-Stieltjes coefficients has been extensively studied and the results obtained in this field have permitted to answer a question raised by Norbert Wiener on the properties of the translations of functions of the class L^p ($1 < p < 2$) on the whole real axis.

4) with other points of the theory such as: absolute convergence of Fourier series, uniform convergence, singularities of Fourier series of continuous functions, ^{approximation by} ~~trigonometric~~ partial sums, relations between convergence and capacity of sets (with A. Zygmund).

5) with properties of lacunary trigonometric series and the similarity between lacunary trigonometric functions and independent functions in the sense of probability calculus, especially from the point of view of the central limit theorem and of the law of the iterated logarithm (in collaboration with A. Zygmund).

6) with other fields in the theory of functions of a real variable such as orthogonal and quasi-orthogonal functions; convexity theorems (with A. Zygmund); Riemannian sums.

7) in the field of the theory of a function of a complex variable with properties of lacunary power series and power series with integral coefficients.

8) in the field of number theory with properties of certain sequences of integers (with D.C. Spencer) and with problems in diophantine approximation (with J.F. Koksma)

During the period of his fellowship, if granted, the undersigned would like to do further research in the direction of his past results and work mainly:

a) in the problem still unsolved of the characteristic properties of sets of uniqueness and sets of multiplicity for trigonometric expansions, at least for the class of symmetric perfect sets with constant ratio of dissection.

b) in problems concerning the structure of the coefficients of Fourier series and Fourier-Stieltjes series as related with the properties of the function expanded or of its partial sums (in particular for Fourier-Stieltjes series the boundedness of the mean value of order one of the partial sums).

c) in the problem of the properties of sets of absolute convergence for trigonometric series.

Publications of R. Salem

1. In the United States

1. A New Proof of a Theorem of Menshoeff, Duke Math. Jl. 8, pp. 269-272, June 1941
2. The Absolute Convergence of Trigonometrical Series Duke Math. Jl. 8, pp. 317-334, June 1941
3. On Some properties of Symmetrical Perfect Sets, Am. Math. Soc. Bull. 47, pp. 820-828, October 1941
4. On Trigonometrical Series Whose Coefficients do not Tend to Zero, Am. Math. Soc. Bull. 47, pp. 899-901, December 1941
5. On Sets of Multiplicity for Trigonometrical Series, Am. Jl. of Math. July 1942 64, pp. 531-538
6. On Singular Monotonic Functions of the Cantor Type., Jl. of Math. & Phys., Vol. 2, pp. 69-82, August 1942
7. On Sets of Integers which Contain No Three Terms in Arithmetical Progression (with D.C. Spencer) National Academy of Sciences (Proceedings) Vol. 28 No. 12 pp. 561-563 December 1942
8. The Influence of Gaps on Density of Integers (with D.C. Spencer), Duke Mathematical Jl., Vol. 9 No. 4 December 1942 pp. 855-872
9. On a Theorem of Zygmund, Duke Mathematical Jl. Vol. 10 No. 1, March 1943 pp. 23-31
10. On Some Singular Monotonic Functions Which are Strictly Increasing, Am. Math. Soc. Trans. 53, pp. 427-439 May 1943
11. Sets of Uniqueness and Sets of Multiplicity, Am. Math. Soc. Trans. Vol. 54, pp. 219-228 Sept. 1943
12. A Singularity of the Fourier Series of Continuous Functions, Duke Math. Jl. 10, pp. 711-716, December 1943
13. A Remarkable class of Algebraic Integers. Proof of a Conjecture of Vijayaraghavan, Duke Math. Jl. Vol. 11 pp. 103-108
14. Sets of Uniqueness and Sets of Multiplicity (Part II) Am. Math. Soc. Trans. Vol. 56, pp. 32-49 July 1944
15. On a Theorem of Bohr and Pal, Am. Math. Soc. Bulletin, Vol. 50 pp. 579-580 August 1944.

16. Lacunary Power Series and Peano Curves (with A. Zygmund)
Duke Math. Jl. Vol. 12, pp. 569-578 December 1945
17. Power Series with Integral Coefficients, Duke Math. Jl.
Vol. 12, pp. 153-173 March 1945
18. Approximation by Partial Sums of Fourier Series (with A. Zygmund)
Transactions of the Am. Math. Soc. Vol. 59, pp. 14-22
January 1946
19. Capacity of Sets and Fourier Series (with A. Zygmund^d),
Transactions of the Am. Math. Soc. Vol. 59, pp. 23-41
January 1946
20. On a Theorem of Banach (with A. Zygmund), Proc. Nat. Acad.
of Sciences, Vol. 33, pp. 293-295, October 1947
21. On Lacunary Trigonometric Series (with A. Zygmund^d), Proc.
Nat. Acad. of Sciences, Vol. 33, pp. 333-338, November 1947
22. On Lacunary Trigonometric Series (Part II), (with A. Zygmund)
Proc. Nat. Acad. of Sciences, Vol. 34, pp. 54-62, February 1948.
23. A Gap Theorem (with M. Kac and A. Zygmund)
Trans. Am. Math. Soc., Vol. 63, pp. 235-243, March 1948.
24. Rectification to the papers - Sets of Uniqueness and Sets
of Multiplicity I and II - Trans. Am. Math. Soc. Vol. 63,
pp. 595-598, May 1948.
25. A Convexity Theorem (with A. Zygmund), Proc. Nat. Acad. of
Sciences, Vol. 34, pp. 443-447, September 1948
26. Convexity Theorem. An invited address delivered to the
New York meeting of the Am. Math. Society, October 31, 1948.
Bulletin of the Am. Math. Soc.

2. In France

(A) Comptes Rendus de l'Academie des Sciences de Paris:

1. Determination de l'ordre de grandeur a l'origine de certaines
series trigonometriques (1928)
2. Sur une propriete generale des coefficients de Fourier
des fonctions sommables (1928)
3. Conditions necessaires et suffisantes pour que des con-
stantes arbitrairement donnees $a_n \cdot b_n$ soient les coefficients
de Fourier d'une fonction sommable (1931)
4. Sur les proprietes extremales de certains polynomes
trigonometriques (1933)

5. Sur une propriete de certaines series de Fourier (1933)
 6. Sur une propriete des series de Fourier des fonctions de carre sommable (1933)
 7. Sur les series de Fourier (1933)
 8. Sur les series de Fourier des fonctions de carre sommable (1933)
 9. Generalisation de certains lemmes de Van der Corput et application aux series trigonometriques (1935)
 10. Sur certaines fonctions continues et les proprietes de leurs series de Fourier (1935)
 11. Sur une methode de sommation, valable presque partout, pour les series de Fourier des fonctions continues (1937)
 12. Sur une generalisation du procede de sommation de Poisson (1937)
 13. Approximations diophantiennes et series trigonometriques (1937)
 14. Sur la convergence presque partout de certaines series trigonometriques (1938)
 15. Sur la convergence des series de Fourier (1938)
 16. Sur un test general pour la convergence uniformes des series de Fourier (1938)
 17. Sur les proprietes descriptives des ensembles de points de divergence des series trigonometriques (1939)
- (B) Essais sur les Series Trigonometriques (Paris, Hermann, 1940)
- (C) La loi du Logarithme Itere pour les Series Trigonometriques lacunaires (with A. Zygmund). *Bullet. des Sciences Mathematiques*, 2^e serie, Vol. 54, 1950.

3. In Denmark

Sur les sommes Riemanniennes des fonctions sommables -
Matematisk Tidsskrift, B, (1948)

4. In Holland

1. Sur les sommes Riemanniennes (*Composito Mathematica*, 7 (1940)
(Joint paper with J. Marcinkiewicz)
2. On sets which do not contain a given number of terms in
arithmetical progression. (with D.C. Spencer) *Nieuw Archief
voor Wiskunde*, Vol. XXIII, No. 3.

5. In Hungary

Uniform Distribution and Lebesgue Integration. (with J.F. Koksma)
Joint paper for the jubilaum of F. Riesz and L. Fejer -
Acta Scient. Math. Szeged. 1950, Vol. XII. B.

6. In Poland

1. Sur les transformations des Series de Fourier (Fundamenta Mathematicae, 33, (1945) pp. 108-114
2. Sur une extension du Theoreme de Convexite de M. Marcel Riesz, Colloquium Mathematicum, Wroclaw 1947, Vol. I, pp. 6 - 8.

7. In Sweden

1. On singular monotonic functions whose spectrum has a given Hausdorff dimension. Arkiv for Matematik, Vol. 1 N°26, 1950
2. Uniform distribution and capacity of Sets. To appear in the special volume of the Comm. of Lund Math. Seminar dedicated to Marcel Riesz's "Festschrift".

Robert Z. Schreffler

~~Juniata College~~

Huntingdon, Pa.

71 Standing Stone Ave.

Oct. 30th 1951

Dr. Norbert Wiener
M.I.T.
Boston, Mass.

Dear Dr. Wiener,

Recently, a friend of mine who works at M.I.T. and I were talking about mathematics and she suggested that I write you concerning a particular solution of mine to the old problem of angular trisection.

This solution of mine is worked out geometrically by a progressive system of construction.

I can solve for any angle up to 180° and can also trisect the space between two parallel lines.

This solution I worked out in late summer of 1947 and I've been unable

to disprove it since then.

Now, the question is this, how could I have this system checked, and if it is a solution to angular trisection with straight-edge & compass, would I be able to submit it as a master or doctor's thesis anywhere for an advanced degree?

I've never submitted this to any school and have only recently decided to see if I could do anything with it. My fondest hope would be to be able to receive a degree from the Massachusetts Institute of Technology.

Have there ever been other solutions to angular trisection? What would you suggest that I do?

Respectfully,
Robert Z. Schuffler.

PROF. DR. BALTH. VAN DER POL

GENÈVE

VDP/803

GENÈVE, 18 October 31st 1951

22, CHEMIN KRIEG

Professor Norbert Wiener,
Massachusetts Institute of Technology,
Cambridge 39,
Massachusetts,
U. S. A.

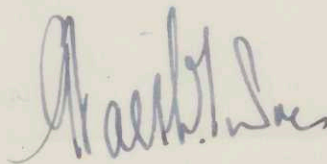
Dear Norbert,

Since you left Geneva my wife and I have often wondered how you were getting on because we were very much upset in finding you suddenly in Geneva so far from well, and we would therefore much appreciate hearing from you.

I recently attended a very interesting colloquium (on the island of Porquerolles near Marseille) on non-linear oscillations, organized by the I.U.T.A.M. (International Union for Theoretical & Applied Mechanics). It seems that the mechanical people are now going much deeper into non-linear oscillations than their electrical colleagues.

With my best regards, also to your wife from both of us,

Yours sincerely,



[ans 11-21-51]