CYBERNETICS

CIRCULAR CAUSAL AND FEEDBACK MECHANISMS IN BIOLOGICAL AND SOCIAL SYSTEMS

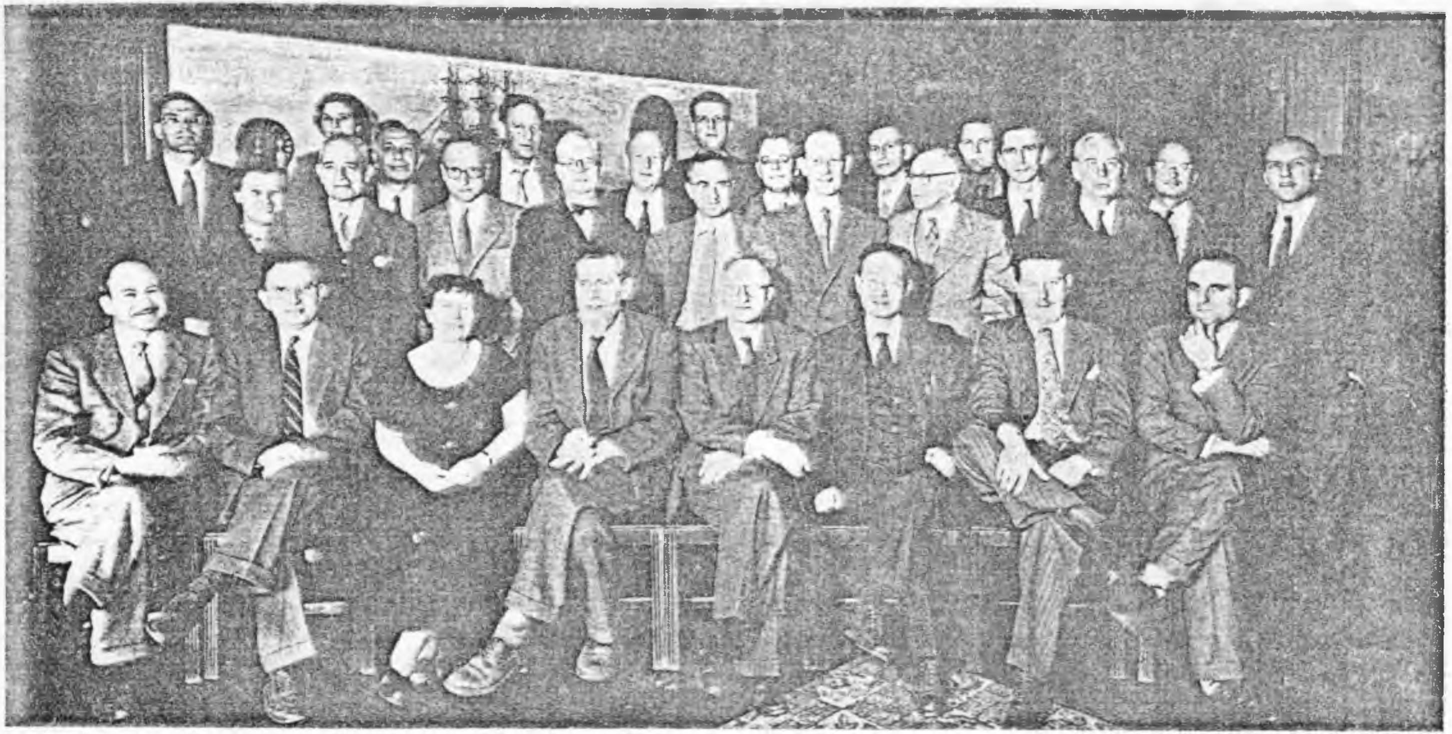
Transactions of the Tenth Conference
April 22, 23, and 24, 1953, Princeton, N. J.

Edited by
HEINZ VON FOERSTER
DEPARTMENT OF ELECTRICAL ENGINEERING
UNIVERSITY OF ILLINOIS
CHAMPAIGN, ILL.

Assistant Editors
MARGARET MEAD
AMERICAN MUSEUM OF NATURAL HISTORY
NEW YORK, N. Y.

HANS LUKAS TEUBER
DEPARTMENT OF PSYCHIATRY AND NEUROLOGY
NEW YORK UNIVERSITY COLLEGE OF MEDICINE
NEW YORK, N. Y.

Sponsored by the
JOSIAH MACY, JR. FOUNDATION
NEW YORK, N. Y.



1st Row: T. C. Schneirla, Y. Bar-Hillel, Margaret Mead, Warren S. McCulloch, Jan Droogleever-Fortuyn, Yuen Ren Chao, W. Grey-Walter, Vahe E. Amassian.

2nd Row: Leonard J. Savage, Janet Freed Lynch, Gerhardt von Bonin, Lawrence S. Kubie, Lawrence K. Frank, Henry Quastler, Donald G. Marquis, Heinrich Klüver, F. S. C. Northrop.

3rd Row: Peggy Kubie, Henry Brosin, Gregory Bateson, Frank Fremont-Smith, John R. Bowman, G. E. Hutchinson, Hans Lukas Teuber, Julian H. Bigelow, Claude Shannon, Walter Pitts, Heinz Von Foerster.

TABLE OF CONTENTS

Tenth Conference on Cybernetics

---

* This was distributed to the participants in advance of the Tenth Conference.

CYBERNETICS

CIRCULAR CAUSAL AND FEEDBACK MECHANISMS IN BIOLOGICAL AND SOCIAL SYSTEMS

Transactions of the Tenth Conference
April 22, 23, and 24, 1953, Princeton, N. J.

Edited by

HEINZ VON FOERSTER
DEPARTMENT OF ELECTRICAL ENGINEERING
UNIVERSITY OF ILLINOIS
CHAMPAIGN, ILL.

Assistant Editors

MARGARET MEAD
AMERICAN MUSEUM OF NATURAL HISTORY
NEW YORK, N. Y.

HANS LUKAS TEUBER
DEPARTMENT OF PSYCHIATRY AND NEUROLOGY
NEW YORK UNIVERSITY COLLEGE OF MEDICINE
NEW YORK, N. Y.

Sponsored by the
JOSIAH MACY, JR. FOUNDATION
NEW YORK, N. Y.

FOREWORD

The three presentations published herewith were presented and discussed at the Tenth and last Conference on Cybernetics (Circular Causal and Feedback Mechanisms in Biological and Social Systems) in the usual informal style as represented in the previous four publications of this conference series. On review of the verbatim transcript of the discussion, it became evident to the Editors that in this instance the presentations repeatedly interrupted by discussion would not produce an effective publication. Accordingly each of the authors has been asked to pull his material together into a single consecutive statement, and the discussion has been omitted.

FRANK FREMONT-SMITH, M.D.
Medical Director

THE JOSIAH MACY, JR. FOUNDATION CONFERENCE PROGRAM

Continuous advance in the field of medicine requires not only new discoveries at the frontiers of knowledge but also effective communication among investigators. New "break-throughs" at any one spot may depend upon the integration of insights and technical skills derived from widely disparate areas of scientific investigation.

The growing volume of scientific publications in itself imposes a heavy burden upon the investigator to keep abreast of advances in his own field and discoveries in other branches of science pertinent to his particular interest. But there is another aspect of communication, a more personal one which has to do with the tendency of scientists, at their meetings, to accept the lecture or other formal, and hence uninterrupted, presentation as the major means of communication. There are a number of obstructions to this method of communication, some obvious such as problems of national and technical language. Others, less evident and therefore more difficult to cope with, are psychological and cultural. These have to do with unrecognized blind spots, prejudices, and over-attachment to or dependence upon an "authority" or upon too narrowly conceived criteria of credibility. Such hidden obstructions to communication form a major source of misunderstanding and even hostility among scientists and threaten to delay the acceptance or proper evaluation of new data and particularly to prevent genuine cross-discipline understanding and multidiscipline team work.

The Conference Program of the Josiah Macy, Jr. Foundation has gradually evolved in an effort to deal more effectively with these hidden obstructions to communication. The participants are limited to twenty-five — fifteen to twenty regular members who attend the five annual meetings of each group and the remainder guests, invited to one or more meetings. The group lives together for 2 days at a small inn, away from the distractions of a large city, where the informality of arrangements contributes to the development of a warm and friendly atmosphere.

The emphasis upon discussion, provided for by limiting the presentations to one, or at the most two, per day and by encouraging interruptions at any time during the presentation, and the tradition, now well established, that "authority" carries little weight in evaluating the credibility of ideas, concepts, and data, help to make the conference a forum for searching examination of differences of opinion and of the reasons for contradictory experimental results. Overgeneralizations are quickly met with the question "with respect to what?"

In the atmosphere of such a meeting it is often possible to discover the basis for contradictory findings. Thus the "failure to confirm" is frequently found to be due to previously unrecognized differences in experimental procedure. At such a meeting a scientist may discover that his hostility to the concepts or data of another is engendered by the cross-cutting of two contradictory but overgeneralized conclusions. Once the overgeneralizations are "cut down to size," i.e., made to conform to the limits within which there are supporting data, the threat to his position disappears and hostility is often replaced by acceptance or constructive criticism.

Members of the conference group become friends; spontaneous collaboration follows naturally. With the growth of mutual confidence, the members bring unpublished data and plans for experiments to the conference in order to obtain critical judgment and suggestions from the group.

Finally, as an atmosphere of free-floating security is established, the group becomes increasingly creative. New suggestions for research and working hypotheses are freely put forward, to be discarded, amended or subsequently tested by experiment. Often the most constructive suggestion comes from a participant not immediately concerned with the problem under discussion and able, therefore, to see the issue with fresh perspective. A genuine partnership in the growth of ideas is the goal.

The transactions of the conferences are published in order to share the experience of the meetings with a larger audience. Although verbatim reporting is impractical, every effort is made to preserve the spirit of the conference. Each participant is given an opportunity to edit his own contributions. The Medical Editor in co-operation with the Foundation staff reserves, however, the liberty to make some rearrangement of material to provide better continuity for the reader.

The Foundation looks upon the Conference Program as an experiment in communication, still in progress.

FRANK FREMONT-SMITH, M.D.
Medical Director

INTRODUCTORY REMARKS*

WARREN S. McCULLOCH,
Chairman

I believe this group has been guilty of a certain irreverence with respect to the subconscious or the unconscious. Therefore, I should like to review how the idea of unconscious mental mechanisms came into the field of psychology. No less a figure than Leibnitz (1) was responsible for initiating it. He was interested only in perception, but he regarded it as a sort of integral of our awareness of the world. His "Petite Perception" was not something of which we could say, "I know this, I know that, I know the other." We could only say such things of the integral. Out of these *petites perceptibles,* which served, if you will, as infinitesimals in his calculus, he supposed that we formed our notions. He found himself coping with a kind of froth, under which there was a kind of sea, without being able to come to grips with the controlling variables.

That is not new, either in biology or in physics. In physics, it led first to the notion of potential energy, which certainly is not energy in the sense in which energy does anything. It is only that it may, when it gets going. In biology, it led to a corresponding observation of significant variables: the conservation of species which in Aristotle (2) became "Entelechy." Since we are not aware of all the significant variables, many of them, in every panpsychism, have to be supposed to operate *sub rosa.*

If we wish to follow the notion from then on, it will be found next appearing in Hume (3) in two forms: one as an instinct, or "habit of mind," which leads us to group events into what we thereafter call time and space; this we shall recognize later in Kant (4) as the forms of sensation, and its organization is the synthetic *a priori.* The other, which underlies the notion of causality, becomes in Kant the category of reason. I did my best to persuade Professor Jean Piaget to be with us, because I think he has a clearer

*This was distributed to the participants in advance of the Tenth Conference.

concept of this subject, and certainly better data on how the idea of causality arises in children, than any of the rest of us. It is certain that if what we call cause and consequence are separated sufficiently in time, then the consequence appears as a spontaneous act. Think for a moment that we have a ball rolling up to another ball; the first ball arrives at the second ball, and the second ball takes off. If it happens promptly, we have a notion that the first ball kicked the second. If it happens after ten minutes, the second ball did it on its own. What the mechanism is, and how it operates in us, I do not know, but it is fairly clear that it does, and I should have liked to hear an excellent observer of human beings, such as Professor Piaget, tell us how it arises in children, because I think that is the only way we are ever going to make sense out of it. I think Hume would have agreed.

Causality became, in Immanuel Kant's category of reason, a little bit twisted. The forms of sensation and the category of reason underwent a Hegelian twist and led to the basis of Marxism, on the one hand, and to the dynamic ego and much of our so-called dynamic psychology, on the other. I have studied rather carefully the beginnings of these notions and their spread in Europe. The German school is blatant. Its best protagonist was von Hartmann, who has written and published repeatedly on this subject, so that at the time Freud began to write, he had sold some nine editions, running to ten thousand or more items each. The best exemplar in Scotland was Laycock. (5) He was Professor of Neuropsychiatry in Edinburgh; and in 1869, the second edition of his book, *Mind and Brain*, appeared. You will find there a brief history of the growth of this idea on the Continent. It became in France the normal way of understanding hysteria in this period. You probably have not read a most entertaining volume which is entitled *Unconscious Memory* and written by Samuel Butler (6), author of *Erewhon* (7). It deals with unconscious memory, and is charming in its self-revelation, and in its horror at von Hartmann's notions.

I am inclined to believe that in all our discussions, the unconscious has suffered a gratuitous insult. I was a psychologist before I went into physiology, and I went into physiology because I was convinced that I had been dealing with a froth and that there were significant variables lying below any level which any flow, I don't care how relaxed it was, could ever reach. I believe that our willingness to participate in this conference indicates that we are not too much worried about how we appear to our neighbors or to ourselves and are quite willing to make fools of ourselves provided we can propose some mechanism which may be at the basis of what is going on. As every scientist knows, that is a hazardous affair, and I should mention that my own notions of how the brain ever gets an idea have had several holes poked in them.

Norbert Wiener, not I, first proposed that there was some scanning mechanism in vision (8). I tied it up with a particular structure and I was probably wrong; Lashley (9) is quite sure I was wrong. I think his arguments are not entirely conclusive, but they are persuasive. An erstwhile member of this group, Donald MacKay, while working in my laboratory, constructed a square which could be made to balloon out, shrink down at any preassigned rate, or could be coupled back from one's own brain waves. According to all my notions, that square should have upset our ability to perceive form. It failed completely to do so, and gave us peculiar distortions of color vision. I think MacKay has thrown the biggest rock that can be thrown through my hypothesis. I am rather upset by a paper which Sholl and Uttley (10) sent me not long ago, in which they said that there is no theory of perception which is subject to a test. I can only answer, "I did propose one, and it is probably wrong." To insist on being wrong is to insist on there being something which can be checked. It is my notion that every scientific hypothesis has a reasonable expectation of being disproved; certainly none can be proved, and it is my great woe, with most of my friends who are interested in psychodynamics (above all, those who are particularly interested in the subconscious), that I fail to find hypotheses set up by them which are capable of experimental disproof. The best of psychoanalysts, I am quite sure, are as much troubled by this as I am.

Ten years ago I tried an experiment. There was a meeting of the American Psychiatric Association in Detroit. I was then intrigued by the Lear Komplex, which is the subject of a paper coming out of Vienna concerning the play of King Lear, and it proves that Mrs. Lear is the all-important person, because she is not once mentioned. My experiment was the following: In the hotel in Detroit there was a rather large and comely bear. Drs. Frank Fremont-Smith and Molly Harrower cottoned on to the bear and they paraded it through the hotel. They gave me the idea of what I think is the nicest of all yarns I have ever invented. The story is the following: They brought the bear to the conference room. One psychiatrist after another would look at the bear sitting among them, and then snap his head back to the front. You could count ten, and each one would take a second look and snap his head back to the front. Several years later I asked Drs. Fremont-Smith and Harrower whether any of the psychiatrists had said anything to either of them about seeing a bear at the meeting. They said, "No." I have told this story wherever psychiatrists were gathered together, and shall continue to tell it. My esteemed friend, Dr. Alexander Forbes, heard me tell the story and at once went to Dr. Harrower and asked, "Did you really have a bear at the meeting?" She replied, "No."

STUDIES ON ACTIVITY OF THE BRAIN

W. GREY-WALTER
Burden Neurological Institute
Stapleton Bristol, England

CYBERNETICS IS A term that means all things to some men and nothing to many. To me, as a British neurophysiologist, it means, on the one hand, the mechanical apotheosis of reflexive action, on the other, the incarnation of information. If these conferences have done nothing else, they have encouraged some of us who would otherwise have remained isolated in the laboratory to risk an occasional sortie out into the universe in order to experience unity. Not Unity with a mystic capital, but the simple unity of principle in communication and causality that Norbert Wiener was the first to emphasize so clearly. Cybernetics may change its maiden name and breed a score of changelings, but we shall never again be targets for the gibe of knowing more and more about less and less.

I was brought up in a fairly rigorous school of physiology in Cambridge, England and spent some of my earlier years studying the nerve impulses in peripheral systems. Then for a while I was engaged in the study of conditioned reflexes. It was rather a nice juxtaposition I reached, passing from the purely academic study of peripheral neurophysiology to my first direct acquaintance with the Pavlovian school of conditioned reflexology, under the wing of a pupil of Pavlov's. At this time I met Pavlov himself and discussed with him the lines they were following.

The first thing I learned, in branching out from axonology to conditioned reflexes, was that not all animals are the same. In physiology, it has always been a sort of axiom that one sort of nerve from an animal will be like the same sort of nerve from another sort of animal. You could speak of *the* action potential of *the* nerve. What became clear in studying conditioned reflexes was that often you cannot speak of *the* dog at all; usually you can speak with assurance only of this particular dog at this particular time.

That introduced me to the general notion that individuality and personality — and not only consistent personality factors but personality as a function of time — would be an important thing in investigating how brains work and how we get ideas. From that, I went on to the study of the brain itself in application to clinical problems.

A few years later I arrived at the more elegant problem of normal brain functions in relation to exactly those matters that Dr. McCulloch has outlined for us — this question of how the brain does get ideas and why it is that the mechanism of idea formation seems to be so inaccessible and yet to obsess people so much. My aim during the last 5 or 10 years has been to try to identify and specify the elements of "awareness," if you like, in terms of physiological mechanism, not trying to creep up furtively to the brain through the spinal cord, but to examine the brain as a whole organ and see whether information coming out of it was going to tell us anything about how it works, and what it prefers to work on.

The proposition I am going to bring up for discussion is that, in fact, quite apparently superficial study of brain mechanisms can tell us an extraordinary amount about how the brain works, much more than we ever expected a few years ago. You have all seen the sort of work that has been done in brain physiology in humans, mainly for clinical purposes. The ordinary electroencephalograms and pictures of that sort are very tantalizing and disappointing. If you work out the amount of information you can obtain from an ordinary brainwave record in terms of what is there, the results are very discouraging. The redundancy is absolutely fantastic. I think a first class research team of workers dealing with ordinary brain records would be very lucky to obtain one part in a hundred thousand of the information from their brain records into their own brains, to understand it and appreciate it, and be able to see what it means; and that, I think, is why we all have been so discouraged until recently about the possibility of examining the human brain in its normal, conscious state, from outside and without interfering with the individual too much — simply because of this vast redundancy.

There is nothing more discouraging than to have to read a language in which you understand, for example, only one sign, not even one word but one sign, in a hundred thousand. Most of us in trying to read Chinese might recognize, after a while, the simple sign for "man" or something like that; but we would not be content with so little. It is a very discouraging experience to have a cipher constantly slipping through one's fingers. That is just about the situation of the average lay physiologist dealing with the brain.

We have spent the last few years in developing all sorts of ways of displaying the activity of the brain, mainly in conventional terms, to see how it relates to the psychological events that go on during growth and the establishment of a series of private notions. Parallel with that sort of work, we have taken the other obvious step of taking our results as they have come out of the machinery, gradually, and building models of them, not just for fun, but to see whether, if we take the mechanisms as they seem to be working in the flesh and transpose them from the paper to the metal, the models will work the way the mechanisms seem to work when they are in the brain. This is a perfectly reasonable and logical step. It is simply a question of putting on to the table, in the form of mechanical devices, the algebraic expressions which our records, in effect, are.

One field of endeavor in studying the brain or mind has been to make pictures of how it looks, of its structure. I think probably many of us here have suffered for some years, in our various schools, the embarrassment of looking at pictures of the brain or the spinal cord, stained, drawn and photographed in various ways, and trying to discover what in this was related to what had actually been happening there. I myself have always found particular difficulty in understanding these things.

We have made some pictures to give an idea of the amount of information we can deal with. The method used to obtain these pictures is derived from a system used during the war for radar purposes. The signals which we get from the head, from electrodes attached in the usual way, are amplified by a factor of a million or so, and are made to modulate the brilliance of small cathode-ray oscilloscopes which are assembled in a pattern, as though you were looking down on the head of a patient from above. You can see 22 tubes displaying in their correct spatial positions the outputs from the various channels. There is, then, a picture of rhythmic, flashing lights, which gives a very good simulation of what Sir Charles

Sherrington called "an enchanted loom where flashing shuttles weave a dissolving pattern." Unfortunately, it *is* a dissolving pattern; in other words, it is constantly changing, and the eye follows the changes but it does not extract the invariants; to extract the invariants from this assembly of patterns, we have to make use of photography which we do not consider a satisfactory method of recording, but the photographic emulsion is a good integrator and builds up the integral of the invariant components very well.

We distinguish the invariant components from the irregular and noisy ones by having on each tube a scanning vector. What is seen on the tubes, if nothing is happening, is a line of light, and if there is no signal coming in at all, it is motionless. But this line of light can be rotated around the tubes like the hands of a clock in various ways. It can be rotated at a controlled speed, so that it turns, for example, ten times per second, in which case something appears that looks rather like a slice of pineapple or a cartwheel with luminous spokes; or, more interestingly, the rate at which this line is scanned round the tubes can be made dependent on the brain itself. Any one component or part of the display system can be used to drive the scanning machinery inside the apparatus. Now, the thing we hoped for from this device, which seems to be a reasonably satisfactory one, is that in doing this, we are making the activity in the brain drive the machine, in such a way that the different parts of the display system are reacting to whatever the brain is doing at any moment. The time ratios are thus local time. There need be no absolute time scale. We are not trying to constrain the brain activity to display itself on an arbitrary, absolute time scale of seconds or milliseconds. We are allowing time to vary; the speed at which this vector goes around may be quite erratic; but the speed it goes at is determined by what is happening in the person's brain at that time and that place; so whatever else is happening elsewhere in the brain at that time and at that rate will be synchronized in rotation on the clock faces. What you have, in fact, are 24 channels comprising 2 monitors, and 22 clock faces. The hands of these 22 clocks, all driven by a master generator, are, as it were, lit up by the brain activity at that particular time; so a picture results only of those events in the brain which are synchronized with, or driven by, or are driving, the activity you have chosen to act as a synchronizer. In this manner, a direct impression of coincidence, congruence, and contingency can be acquired. I think causality is the root problem of the whole brain function and hope to be able to go into that question in more detail at a later time. What are the causal relations between these various events?

The technical virtues of the display system are that there is almost no redundancy. Furthermore, noise – whether physical noise in the sense of thermal agitation, or brain noise, i.e., the sort of noise in the background which has to do with the administrative work of the brain in keeping itself alive and fed, and so on – is not seen at all. This machine might almost be defined as a device with which it is impossible to see noise, because noise, by definition, produces no pattern. You cannot synchronize the machine on noise from any one channel and have the noise of any other channel synchronized, by definition, because the noise in one region of the brain has no phase relation or regular time relation with the noise in any other channel. Therefore, those events which are significantly related to one another in some time sense are chosen.

The device can also be arranged not only to be driven by the brain activity, but also to drive the brain activity, to administer to the individual under study a series of stimuli – light signals, sound signals, or touch signals – which in themselves may give rise to a pattern in time or space; and this pattern of input signals can be delivered at any desired rate. We use this equipment regularly in both these ways; that is, we use it, first of all, with the brain driving the machine, and then the reverse process, with the machine driving the brain.

The results are rather surprising, first of all, in relation to this notion of scansion which Dr. McCulloch has mentioned and which has been attributed to various authors. We have observed, unequivocally, some type of time dissection, occurring in the brain. I coined a word for this which I am using now for only the second time in public; the word is "abscission." I have deliberately tried to avoid using the word "scanning," because it has so many associations in ordinary speech and it has become rather tainted, perhaps, with too many simple-minded ideas. But I think there certainly is some type of abscission, of cutting off, of signals in the brain and their reprojection on a local time base.

We see such abscission particularly clearly as a separating out of the parts of a visual time pattern, in the visual association area of the back of the head. The appearance is easily recognized; one part of the pattern appears in one place and another part of the pattern appears later in another place. Abscission certainly happens; it is there that the incoming ensemble is broken down into parts. But the exciting thing to me when I saw it the first time was the fact that as long as the pattern which the person is seeing is novel or surprising or interesting, the abscission process has its converse in other parts of the brain. The abscission process is turned around and the pattern is abstracted; it reappears again in parts of the brain not generally regarded as being attached very directly to peripheral mechanisms, in the association or silent areas. In those regions, the pattern appears not only in its original form, in whatever time pattern has been introduced, but in all possible transforms of its elements. If a threefold stimulus is put in, it may come out as a three-in-one pattern, or the various signals, when obtained, may be closed or opened up, or two of them together and the third nearby; this continues for a matter of a few minutes. If you leave a person subjected to this type of stimulation for a minute or two, possibly 20, 50, or 100 repetitions of this situation may take place. Then in time the appearance fades away. If the stimulation is stopped before its novelty has worn off, the pattern is preserved for several seconds in the temporal association areas, and then it slowly melts away.

The fading is not a question of fatigue. The time is too short for fatigue to mean anything in relation to the tiredness of the brain. The excitement fades away, as far as we can see, because it becomes boring. The repetition of this pattern, which is a perfectly real signal, a real physiological stimulus, begins to lose meaning to the person. When it is not followed by anything, when it does not seem to signify or imply anything, this remote, diffusely projected and abstracted pattern fades away and subsides. If you then change the pattern at all, if you introduce into it even a slight alteration of the rhythm or shape or brilliance, then immediately the distant synthesis shows again; the whole pattern is re-established; it re-forms in the association areas.

We are now going to take an important step further – to associate the arbitrary, novel, but mental signal pattern with an unconditioned stimulus. We have not yet got very far with this. All we have done so far is to establish a defensive reflex which, as we all know from personal experience and training, is much more easily established than an appetitive one. All I need say at present is that when an unpleasant sensation is given in association with certain patterns of this type, the abscission-abstraction-preservation process is enormously enhanced, building up to something quite literally sensational.

The size of photographs taken to record this phenomenon is small compared with ordinary recording, and the patterns are minute by ordinary recording standards. But this elaborate extraction of information goes on in a perfectly regular, automatic way and the results can be recognized and identified quite well. It is satisfying to note that they are of a very personal, very individual character, and are enormously dependent on the subject's experience.

Information about the way these things happen must be handled at the present stage in terms of a rigorous hypothesis. There are two auxiliary methods of study which suggest themselves: One is the making of models which do this sort of thing; the other one is the study of people who have widespread and deep-seated lesions in their brains. In the last few years, neurosurgeons have begun to remove not little bits of brains but half-brains. It is quite commonplace now in neurological clinics to see people who have had this so-called hemispherectomy operation. It is not really hemispheric, in the physiological sense, as the whole hemisphere is not taken out. Quite a large amount of the thalamus and the base of the brain is left, but the cortex is removed almost completely.

We have studied a few of these people, one boy in particular, to see how much the cortex is involved in the abscission process. It is obviously important, in seeking to relate this process to the information from anatomy and physiology, to see whether we are dealing with something which is whole-brain function, or whether the different parts contribute different things.

signals that come in. What comes out from this second system is a long stretched output which, from the physiological standpoint, is an after-discharge. This, it will be recognized, is a perfectly standard or classical physiological process.

In such hypotheses it is obviously desirable not to postulate that the brain is doing anything magical. If a sense organ or the spinal cord will account for an experience of the brain, that is a little more satisfactory than some quite new thing. Moreover, I take it for granted that the chemical substances, of which we hear so much and know so little, may be the prime movers. Acetylcholine and other mechanisms must be involved. I disclaim any notion that the brain is a purely electrical machine.

The second set of operative signals, then, is a stretched series, which long outlasts the duration of the stimulus. It preserves the information that a particular signal or set of signals has occurred. After it has occurred, these two signals have to be combined in some way, not with one another again, but each with the complementary stream of signals from some other stimulus source. This is the important point; a signal comes in, for example, through the eyes, and is divided into these two streams which go forward to the brain as coded information to nearly all areas. I have dubbed these streams of inoperative but still significant signals, "propaganda." They are to be propagated only, not acted on; they are not to be remembered because they *mean* nothing. There are going into our nervous system all the time innumerable streams of information, 99 out of 100 of which are insignificant to us. If we paid conscious attention to them, we should go crazy. The brain is dealing with them summarily. I mean such facts as that the lights in the room are on, a fact which is not important to us in itself but only for the light provided for our work. It might be important. For example, if one of the lights started to flicker or go out, it might be important. We are prepared to pay attention to such facts, but they are not memorable.

Such things become memorable only if two streams of signals are repeated often enough to overlap in their complementary forms more often than would be expected by chance. This is where the seam in our fabric comes in. One of these threads, let us say, from the needle, is the derivative. The shuttle which moves across catching the other thread and fixing it is the stretched, extended form of signal. So we have this constant interweaving of the clipped form, from one source, with the stretched form, from the other, which forms are then interwoven into a permanent and durable seam of congruence and contingency, provided that their coincidence is greater than chance expectation. That is why I call this a knot of probability, because what the making of the seam depends upon is precisely the measure of probability that these two series of events from different sources are connected, that one may imply the other.

In order to learn more about this measure of probability, about the odds of association, I have devised an electronic model, an instrument which can receive two streams of signals. Some criterion of the likelihood that two series of events will be regularly and frequently associated with one another has thus been provided.

Other questions that come to mind are how the model works, and more cogently, how the brain or an animal builds up a notion of probability, how it begins to reckon the odds, and how it learns what the odds are in favor of significance, in favor of two events being significantly connected.

Consider an experience that is common but that is happening for the first time. We are aware of a flash of light outside; a few seconds later, we hear thunder. The first time it happens we may think that someone turned on a light and someone hit a drum. If the two things happen regularly, however, we begin to build up a significant idea of a thunderstorm. We then think that when there is lightning, there is thunder and we may generalize in this way. From a statistic one may conjecture, but should not assert, a cause. Yet we may be tempted to go a step further, and say, that lightning causes thunder, unless we are metaphysical or mystical and prefer to say that lightning and thunder have a common cause, which we may call Zeus or electricity, according to the age we live in. We may well ask again what the criterion of significance incorporated in the brain is. But we know the answer already: for tentative action, the same as you would ask for in a preliminary scientific observation, about 20 to one against chance; for definitive action, about 100 to one; for "certainty," 1,000 to one, and so on.

SEMANTIC INFORMATION AND ITS MEASURES

YEHOSHUA BAR-HILLEL
Department of Philosophy
Hebrew University
Jerusalem, Israel

The following presentation will be confined mainly to the theory of semantic information as it was developed recently by Dr. Carnap and myself (1). Also it will be concerned with the relation of this concept to the concept of amount of information, as developed by C. E. Shannon (2), and an attempt will be made to show that the two are not in competition. The presentation will occasionally touch upon the relationship between thermodynamical entropy and amount of information, emphasizing both the positive connections and the nonidentity; and it will also touch upon the distinction between statistical probability and inductive probability.

Information Theory, in the sense in which this expression is used in the United States, namely, Theory of Information Transmission, is not generally spoken of as dealing with the content of the messages transmitted, and hence it is regarded as a discipline having no connection with semantics (3). (At a previous conference, Dr. MacKay (4) pointed out to this group that, in British usage, Information Theory has a different connotation.)

However, few could resist falling into the semantical trap laid by the use of the word "information" and not assume sooner or later that this field of interest is applicable to semantics and does reveal certain aspects of meaning. Claude Shannon is, to my knowledge, the only major information theoretician who consistently refrained from drawing illicit inferences.

The theory, an outline of which is presented here, is meant overtly and exclusively to deal with the concept of semantic information conveyed by a statement, or the semantic content of a statement, and various measures for this concept.

"Ma," for instance, is a basic statement signifying that *a* is male. All the statements of our language consist of the twelve basic statements and the statements formed out of them with the help of the connectives.

Our specimen language is extremely poor. What it loses thereby will, I hope, be outbalanced by the perspicuity of the applicability of the rather abstract concepts to be introduced presently.

An example of a state-description in our language is as follows: Since such a state-description denotes a possible state of the universe dealt with in this language, it tells us, for each individual, whether this individual is male or female, young or old. It consists, consequently, altogether of a conjunction of six basic statements. "Ma & Ya & Fb & Yb & Mc & Oc," for instance, is a state-description telling that *a* is male and young, *b* female and young, *c* male and old. This conjunction describes our universe completely. As soon as I add a basic statement which is not identical with any of the six components of this conjunction, this basic statement can only be the negation of one of these components so that the resulting conjunction will be self-contradictory. We conclude, then, that any statement which logically implies, or is stronger than, a state-description is self-contradictory. In this sense, a state-description is a strongest synthetic statement in its language.

We take as the *explicatum* for the information conveyed by a given statement the class of the state-descriptions excluded by that statement. Each statement in our specimen language excludes, then, none, one, or more (up to 2^6, or 64, which is the number of all state-descriptions) of the state-descriptions. An analytic, or logically true, statement, such as *"Ma v Fa,"* excludes none; a self-contradictory, or logically false, statement, such as *"Ma & Fa,"* excludes all; a synthetic, or logically indeterminate, statement excludes some, but not all, state-descriptions. An analytic statement has minimum content, and a self-contradictory statement maximum content. This is not surprising. A self-contradictory statement tells too much, it excludes too much, and is incompatible with any state of the universe, whereas an analytic statement excludes nothing whatsoever and is compatible with everything.

A state-description itself, as a strongest synthetic statement, excludes all other state-descriptions. We shall deal somewhat later with its counterpart, a weakest synthetic statement, that excludes just one state-description.

Please notice that we have so far explicated only the notion of information itself. In a prior meeting, Shannon told this group that he did not define the concept of information itself and was not interested in it. He wanted to define only the concept of amount-of-information, with undeletable hyphens. To my knowledge he never uses the notion of information as such in any essential way, nor is there any reason why a communication engineer should do so. We offer an explication for information itself, in its semantical sense, for whatever it is worth. Whether *Cont* is a good *explicatum*, you may decide for yourselves.

Our next problem is now to define *measures of content* to serve as *explicata* for amount-of-information, in the semantical sense. So long as we are talking only about content itself, the most we can say is that a certain statement has a larger content than another one, and this in case that the class of state-descriptions excluded by the first statement includes the class of state-descriptions excluded by the second one as a proper part. Thus we would say that

$$\text{Cont}(\text{“Ma”}) \supset \text{Cont}(\text{“Ma v Yb”}),$$

since the class of state-descriptions excluded by "Ma" (there are 32 of them) contains the class of state-descriptions excluded by "Ma v Yb" (there are 16 of them) as a proper part. But if two contents are exclusive, i.e., have no member in common, or overlapping, i.e., have some but not all members in common, we can say no more about the statements whose contents they are, although we certainly would like to be able to say, and justify, that "*Ma*" has a larger content, conveys more information, than "*Mb v Yb.*"

In order to do just this, we must go over to the stage of talking about measures of content, to define – as the mathematicians would say – a measure-function ranging over the set of contents. Fortunately, we do not have to start from the beginning. Carnap (6) has developed a rather extensive theory of measure-functions ranging over, not, to wit, contents, but something very similar to them, namely, what he calls *ranges*. (The range of a statement is the class of those state-descriptions that logically imply that statement.) Those measure-functions, which he calls *m-functions*, are meant to explicate the presystematic concept of *logical* or *inductive probability.*

Now, since a measure-theory of ranges has already been developed, we can clearly make full use of it and define our content measure-functions on the basis of Carnap's m-functions. For each such m-function, a corresponding function can be defined in some way that will measure the content of any given statement.

Let m be a measure-function of ranges. What kind of function of m shall we take as a measure for contents? We have a multiplicity of choices. But all choices will have to fulfill the following condition that seems clearly indicated for any adequate explication of amount of information: *the greater the logical probability of a statement, the smaller its content measure.* The fulfilment of this inverse relationship will be the guiding requirement for our choices.

The mathematically simplest relationship that fulfills this requirement is the complement to 1. Let $m(i)$ be the logical probability of the statement i. Then $1 - m(i)$ can be taken as a plausible measure for the content of i. We call this measure simply the *content measure* of i and denote it by "*cont*(i)" with a lower case "*c*," in distinction to the upper-case "*Cont*" standing for content. The formal definition is, then,

$$\text{cont}(i) =_{\text{df}} 1 - \text{m}(i).$$

It can easily be seen that *cont* fulfills, in addition to the aforementioned condition of adequacy, other requirements of an adequate explication of amount of information, *but not all.* It fails to fulfill a certain requirement of additivity, the counterpart of which plays a great role in Shannon's theory. This requirement, in our case, would be that the content measure of two statements that are *inductively independent* – meaning thereby roughly that the logical probability of either statement should not be changed by being given the other statement – should be equal to the sum of the content measures of each of them taken separately. But, it is not the case, in general, that if i and j are inductively independent, then $\text{cont}(i\&j) = \text{cont}(i) + \text{cont}(j)$. There is, indeed, an additivity theorem for *cont,* but the condition under which it holds, is not that i and j should be inductively independent but rather that they should be *content-exclusive,* i.e., that there should be no state-description excluded by both *i* and *j*. This additivity condition makes sense though it is at odds with the more customary one of inductive independence. It makes sense to say that the content measure of the conjunction of two statements should be equal to the sum of their content measures if, and only if, they are content-exclusive. Then *cont* has certain plausible properties though it lacks certain other properties which are equally plausible. Since, however, no concept can have both these plausible properties simultaneously, we are led to the idea – and there are many other arguments pointing in the same direction – that we do not have in our mind *one* clearcut, unique, presystematic concept of amount of information but at least two of them (and both still in the semantic dimension). This is not so strange. On the contrary, it is a rather common phenomenon that two related but different concepts are regarded as being identical although contradictory properties are required for their *explicata*.

Though *cont* seems to be a very natural and simple systematic correlate for the presystematic concept of amount of semantical information, neither it nor, what is perhaps somewhat more surprising, its statistical counterpart have been discussed much so far. On second thought, however, it is perhaps not difficult to account for this neglect. It seems that in communication engineering, the requirement of additivity under the statistical counterpart of inductive independence is much more important and practical than such a requirement under the counterpart of content exclusiveness, and it is doubtful whether this condition makes sense there at all. Another reason will be given further on in the presentation.

At this point, it will probably be of some help to present to you a pair of content-exclusive statements in our specimen-language:

"Ma" and "Ma ⊃ Fb."

You can easily see that the contents of these two statements do not overlap if you transform the second statement, according to the rules of the ordinary propositional calculus, into "Fa ∨ Fb." More generally, any two statements, one of which is an implication statement having the other as its antecedent, are content-exclusive.

I already mentioned that we chose the roundabout way of introducing the concept of content measure *via* the *m*-functions only because an extensive theory of these functions stood at our disposal. For those unacquainted with that theory, it would probably have been pedagogically wiser to base the whole thing not on state-descriptions but on their "dual," which we call *content-elements.* You get the "dual" of a state-description if you replace the "&"-signs by "v"-signs and each capital letter by its complement. The dual of the state-description, for instance, I mentioned before for illustrative purposes, is:

$$\text{Fa v Oa v Mb v Ob v Fc v Yc.}$$

Just as the state-descriptions are the strongest synthetic statements, so the content-elements are the weakest synthetic statements. I could then have defined the content of i as the class of all content-elements logically implied by i. I could then finally have defined $cont(i)$ as a measure-function of contents dually analogous to Carnap's definition of m-function.

Since *cont* is not additive under inductive independence, we need another *explicatum* for amount of information that will have this property and will assign to "*Ma* & *Yb*," for instance, an information measure that is equal to the sum of the information measures of "*Ma*" and "*Yb*" since these two statements are inductively independent for any adequate m-function I can think of. Being given the information that *a* is male, the logical probability of *b*'s being young should not be affected. This seems rather obvious. Under a certain normalization, the information-measure of each of these two statements turns out to be *one* (bit). We would then like the information measure of their conjunction to be 2. But this does not hold for *cont.* The *cont*-value of the conjunction is smaller than the *cont*-value of the components, since these two statements are certainly not content-exclusive: They both imply, for instance, the statement "*Ma v Yb.*" Since a synthetic statement is logically implied by both statements, their contents cannot be exclusive.

Insisting on additiveness on condition of inductive independence, we obtain another set of measures for amount of information which we call this time *information measures* and denote by "*inf.*" We can define "*inf*" either with the help of "*cont*" as

$$\text{inf(i)} =_{\text{df}} \log_2 \frac{1}{\text{cont(i)}}$$

or else directly on the basis of *m* as

$$\text{inf}(\text{i}) =_{\text{df}} \log_2 \frac{1}{\text{m}(\text{i})} \ (= \log_2 \text{m}(\text{i})),$$

which immediately recalls the standard definition given by many people, though not by Shannon himself, for the amount of information carried by a single signal *i*. We have only to replace "$m(i)$" by "$p(i)$" for this purpose.

It can easily be shown that *inf*, as defined by either of these definitions, fulfills the above-mentioned requirement.

There is another requirement which causes dissension among people looking for an explication of amount of information. Some would insist that the amount of information of *any* two statements should always be at most equal to the sum of the amounts of information of these statements; others would not want to commit themselves on this point. It can be shown that *cont* fulfills this requirement, i.e., that for any *i* and *j*,

$$\text{cont}(\text{i\&j}) \leqq \text{cont}(\text{i}) + \text{cont}(\text{j}),$$

whereas between *inf* (*i&j*) and the sum of *inf*(*i*) and *inf*(*j*) all three possible relationships of magnitude may subsist.

One standard objection, to be found among those who believe that an explication of the semantical sense of information is of little or no use, is that the concept of information, as ordinarily used, is essentially subjective. The statement, "Johnny is hungry," carries no information for someone who already knows that Johnny is hungry, a moderate amount of information for someone who is ignorant about Johnny's state in this respect, and a very large amount of information for him who believes that Johnny is not hungry.

In analogy to the situation with respect to probability — and this analogy is, of course, no accident in view of the close relation between probability and amount of information — this objection must be split up into two quite different parts, only one of which points to an essential subjectiveness. This part is only the relatively trivial one in which the same statement can be said to carry different amounts of information even for two people with the same beliefs. Admitting this, we admit no more than we were ready to do long before, namely, that there is a concept of pragmatical information which is badly in need of explication. But the other part, based upon the fact that different people may have different sets of beliefs, need not necessarily be interpreted as pointing to *subjectiveness* but can better be interpreted as pointing toward *relativity,* toward the fact that the same statement might carry different informations, *objectively* different semantic informations, *relative* to other statements, taken as objective evidence. With regard to our illustrations, we would prefer to formulate the situation by saying that the statement, "Johnny is hungry," carries different informations when taken relative to the statement, "Johnny is hungry," as evidence, when taken absolutely, and when taken relative to the statement, "Johnny is not hungry," as evidence. This points only to the necessity of distinguishing clearly between the information carried by a statement absolutely, taken by itself, and the information carried by it relative to other statements. This we do, of course, and distinguish *Cont* (i/j) – the content of i relative to, or given, or on the evidence of, or simply on, j – from *Cont*(i), *cont*(i/j) from *cont*(i), *inf*(i/j) from *inf*(i). The definitions look alike, that of *inf*(i/j) is, for instance, as you would probably predict,

$$\text{inf}(i/j) =_{df} \text{inf}(i\&j) - \text{inf}(j).$$

Denoting any analytic statement by "t," we find that

$$\text{inf}(i/t) = \text{inf}(i),$$

an equation that would have allowed us to define *inf*(i) in terms of *inf*(i/j), i.e., the absolute information measure in terms of the relative information measure, instead of the other way round.

Here the question arises: What are the relations of the relative content and information measures to the relative, or conditional, probability measures? Defining $m(i/j)$, i.e., the inductive probability of i given j, in the customary fashion as

$$m(i/j) =_{df} \frac{m(i\&j)}{m(j)}$$

we arrive at the remarkable result that

$$\text{inf}(i/j) = \log_2 m(i/j),$$

in complete analogy to

$$\text{inf}(i) = -\log_2 m(i).$$

There is no corresponding theorem for *cont,* i.e., it is not the case that

$$\text{cont}(i/j) = 1 - m(i/j).$$

This is then the other reason, intimated previously, for the preference given in communication theory, and everywhere else, to the correlate of *inf.* It seems that many authors take it for granted, more or less, that the amount of information of *i* relative to *j* should be the same function of the probability of *i* given *j* as the absolute amount of information of *i* of the absolute probability of *i.* The fulfillment of this requirement leads indeed to a log type of function.

I claimed previously that the fact that both *cont* and *inf* exhibit plausible properties indicates that the presystematic concept of amount of semantic information is ambiguous and apparently an amalgam of at least two different concepts, one of which is explicated by *cont,* the other by *inf.* (In addition to this, the vital distinction between the absolute and the relative concepts is often not made, thereby still increasing the confusion.) To prove this once more: A moment ago, a property of *inf* was pointed out which many regard as essential for an amount of information function. I am going to point now at a property that characterizes *cont* but not *inf,* which is considered by many as a *desideratum* for an amount of information function. When I asked people what they regard as the appropriate relation between the absolute amount of information of a given statement *i* and its amount of information relative to any *j*, most of them were very positive that no increase in the evidence should increase the amount of information, though it might not necessarily decrease it. It can, however, easily be shown that whereas indeed

$$\text{cont}(i/j) \leqq \text{cont}(i),$$

the corresponding statement for *inf* does not hold.

I shall show this for our specimen language. For any adequate *m*-function, *m*(*"Ma"*/*"Fb"*) will be less than *m*(*"Ma"*), since, according to the *principle of instantial relevance,* the instance *"Fb"* is negatively relevant to *"Ma."* Therefore $-\log_2$ *m*(*"Ma"*/*"Fb"*) will be greater than $-\log_2$ *m*(*"Ma"*), hence

$$\text{inf}(\text{"Ma"}/\text{"Fb"}) > \text{inf}(\text{"Ma"}), \text{ q.e.d.}$$

It is perhaps not too far from the point if we regard *cont* as a measure of the *substantial aspect* of a piece of information, and *inf* as a measure of its *surprise value.* When the census taker learns that *Ma*, when he first comes to the hamlet, he learns that the universe he is interested in is not in any of certain 32 states out of the 64 states it could possibly have been in. If this is the second thing he learns, having learned first thing that *Fb*, the substantial increase of his knowledge about that universe is less, since "*Ma*" tells him now only that the universe is not in any of 16 states out of the 32 it could still have been in, these 16 states forming a subclass of the class of 32 states, in the first case. But though his knowledge increases less substantially, he is (or should be) now more surprised than he was in the first case. Knowing nothing, he expects *a* to be *M* as much as *F*, but having observed that *b* is *F* first, he then expects *a* to be *F* rather than *M* – it is important to remember that this is *all* he knows, in our fictitious situations; he knows nothing about attraction of sexes and all the other many things we know and which are relevant to the census – and is therefore rightfully surprised when *a* turns out to be *M* after all.

By illustrating in psychologistic terms, I am afraid I created more puzzles than enlightenment. In order not to fall into the trap laid thereby, it might perhaps be preferable to speak of *inf* as a measure of (objective) *unexpectedness* rather than of surprise.

Though *inf* is a monotonic transform of *cont*, it is not a linear transform. Consequently, not only are the *cont*-values and *inf*-values in general different, but so are the *cont* and *inf* ratios. It will, therefore, in general make a difference whether, in a given practical situation, one is going to use the *cont* or *inf* measure.

This can be illustrated by means of the following story, inspired by the title of a book by Agatha Christie entitled *Bridge Murder Case.* There was a bridge party in A's villa, with B, C, D, and E participating; A was the host and only kibitzed. When the last rubber was finished and the guests were looking for A to take leave of him, they found him murdered in the garden. Everyone of the four players had been the dummy at one time or another and had left the room for refreshments. Each one had, on the available evidence, an equal opportunity for murdering A. A reward was promised to those who could forward information leading to the identification of the murderer.

A day later, X came and produced evidence sufficient to prove that B could not have been the murderer. The next day, Y showed, to the district attorney's satisfaction, that C was innocent. The following day, Z did the same for D. Whereupon E was duly convicted and electrocuted.

The problem now for the district attorney was how the reward should be distributed; he had to adopt some numerical proportion. He could evaluate the information given by the three informants according to the absolute *cont* value of their statements, or according to their absolute *inf* value, or according to their measures, relative to the information he received, or according to no explicit function whatsoever. I believe that American law does not require him to justify his method of distribution.

I asked six friends at MIT how they would have handled the situation. I received six different answers. One, a newcomer to MIT, with little previous contact with Information Theory, would have distributed the reward equally between X, Y, and Z. Another claimed that the information supplied by Y was worth more than that supplied by X, since X's testimony excluded one suspect out of four, whereas Y's testimony eliminated one out of three, and similarly for Z. He was in favor of distributing the reward according to the proportion $1/4 : 1/3 : 1/2$. A third agreed with the second's evaluation of the situation but argued for a distribution of the reward according to a logarithmic scale. A fourth wanted to give all of it to Z, since he alone achieved the identification of the murderer. A fifth, an Iranian, was sure that if the story had happened in his country some years ago, the attorney would have kept the reward for himself, which is probably exaggerated; and I have forgotten what the sixth had to say.

The story could be usefully elaborated in many different directions. I shall not do this here, but I hope I have made one point clear: A numerical measure of information content is of interest not only for science but even for everyday situations.

The next concept we introduce is the *estimate of the amount of information* conveyed by a statement. Consider, for instance, that we are about to perform an experiment with so many possible outcomes and that these outcomes form what we call an *exhaustive system,* so that one and only one outcome must occur. Under these conditions, it makes sense to ask: What is the amount of information which the outcome of this experiment can be expected to carry?

Let H be the exhaustive system, and $h_1, h_2, \ldots, h_n$ the n possible outcomes. Let $p_1, p_2, \ldots, p_n$ be their respective logical probabilities with $\sum_i p_i = 1$. We define now *the estimate of the amount of information carried by* (the members of) *H* as the weighted average of the information carried by each h_i, with the probabilities serving as the weights. In symbols:

$$\text{est}(\text{in},H) =_{Df} \sum_i p_i \times \text{in}(i)$$

(where "in" stands for any amount-of-information function). For the specific *inf* function, we have

$$\text{est}(\text{inf},H) = -\sum_i p_i \times \log_2 p_i.$$

If there are, for instance, 16 possible outcomes and these outcomes are equiprobable, then each of these outcomes will carry four units of information according to *inf,* and the expectation-value will then, of course, be also four. But if the probabilities are not equal, the estimate will be less than four, with the difference depending upon the specific distribution.

We then go on to define even more complex notions like *amount of specification and estimate of amount of specification.* However, I shall not go into a discussion of them at this time. May I point out, however, that the whole theory now takes on an aspect which should be very familiar to those who are acquainted with current (communicational) Information Theory. The task of thoroughly comparing these two theories is still before us, though many preliminary attempts have been made. Pointing out that they deal with entirely different subject matters in a manner that shows very far reaching formal analogies is a good start but certainly not all that could and must be said. But no more will be said at this time.

Many of you are acquainted with the considerations, not to say speculations, that accompany another formal analogy subsisting between the basic formulas of Information Theory and mechanical statistics, and the concepts of entropy and negentropy have popped up in the discussions of this group more than once. Some of our best thinkers have expressed the view that this analogy is much more than just any analogy, and statements identifying thermodynamics and communicational information theory, requiring a revision of the second principle of thermodynamics, and even statements identifying thermodynamics with logic have been made recently. I believe that these declarations were more than an attempt to explode old-fashioned ways of thinking and to force people to go deeper into the foundations of thermodynamics than they did so far. But, perhaps for the first time in my life, I find myself on the side of the scientific conservatives. I find it utterly unacceptable that the concept of physical entropy, hence an empirical concept, should be identified with the concept of amount of semantic information, which is a logical concept, and this in spite of the recent attacks by excellent logicians on the logical-empirical dichotomy (7).

I think that my initial attitude will carry more weight if it is shown what might have brought about this identification, in addition to the formal analogy. One source of this mistake lies in the fact that many physicists are accustomed to say that entropy — physical, thermodynamical entropy — is a concept that depends upon the state of knowledge of the observer, so that if someone is going to compute the entropy of a given system, for example, a container of gas, he will have to do this relative to his state of knowledge. If this were so, then it turns out that the entropy concept in physics is becoming a psychological concept, which is at least as disturbing as saying that it is becoming a logical concept (and probably behind this latter statement). This is, of course, not the first time that physical functions have been described as being dependent on the state of the observer, including his state of knowledge. It has even become a kind of fashion, probably originating with certain expositions of relativity theory, to introduce everywhere such dependencies. However, this talk about a physical function being dependent upon the state of knowledge of the observer can hardly be taken seriously. It is probably no more than what Carnap has termed "qualified psychologism," i.e., formulations from which the psychological terminology can be eliminated without loss. It may be of some didactical importance to present certain arguments in terms of the observer, but one can do without. The introduction of the observer is only a *façon de parler*.

Not being a physicist, what I am about to say now in conclusion may well be pure nonsense, but I feel that it is necessary to say this in order to provoke clarification of the prevailing situation by those who are more qualified to do so than I.

As I see it, the entropy of a system is a determinate quantity. However, being fallible human beings, we are unable to determine this quantity, at least in general. If the outcome of some action of ours is a function of the entropy of a system, then we would like to act on our knowledge of the *true* value of this quantity. However, all we can do is to act on our *estimate* of this value. This is utterly trivial and no different from, for example, the case of the length of a table. This quantity is, of course, uniquely determinate, though in order to act, we have to rely on estimates of this length derived, for instance, from certain measurements through averaging operations. Now, of course, estimates are relative to the available evidence, hence, in a sense, to the state of knowledge of the estimator. Someone's estimate of the entropy of a given system depends upon his state of knowledge. I would urge not to formulate this situation by saying that the entropy of the system depends upon his state of knowledge. The treatment of estimate functions of entropies, of lengths, or of any other quantities, belongs to (inductive) logic, but this does not mean, of course, that the treatment of entropies belongs to logic.

MEANING IN LANGUAGE AND HOW IT IS ACQUIRED

YUEN REN CHAO
Department of Oriental Languages
University of California

I COME HERE as a linguist, and by a circular route, from a long distance away. I was born and brought up in an old country, with very early linguistic experiences of various sorts. As a child, I had to move from place to place because my family traveled a great deal. As most of you know, the dialects in China sometimes vary so much that they are really entirely different languages. Before I came in contact with foreign languages, I had to learn to read and write in the ancient style, as was the custom, and also in different dialects, so in that way I became very conscious of language. At the turn of the century children were still required to read all the classics, even though they did not know what they meant. However, later on, suddenly everything began to take on meaning. That gave me the experience of "understanding" without knowing the exact meaning of the language. It took on an autonomous meaning, rather like music. That is one of the points I hope to develop later in this presentation.

Then I came into contact with foreign languages, and as soon as I had the opportunity I went abroad to the New World, planning to study electrical engineering; however, on the way, I met a friend who had been in the New World, and he explained to me the difference between pure science and applied science. This influenced me to study pure science, so by the time I arrived in the New World, I had changed to mathematics and physics. I found physics too specialized a subject to satisfy me, so in my graduate work I went into technical philosophy. However, because the use of words was so important in my work, I decided that I should resume the study of language. Therefore, by a circuitous route, I had arrived back where I started.

I returned to the Old World and spent 20 or 30 years in studying and recording the languages and dialects there, sometimes even trying to change the language and writing of the country. That is how I got into the study of language, and I have remained there ever since. I have very little to do with physiological or medical studies, but I feel at home with those of you here who work in those fields, because, as Dr. Norbert Wiener said in his autobiography, "Chao married a charming Chinese woman doctor" (1).

What I have to say may be, in part, a repetition of your previous discussions. We have already had a very rigorous and eloquent presentation of the meaning, from the point of view of strict semantics, of language. I shall start by saying that I am speaking of the general problems of meaning in natural languages; then, later, I shall take up the pragmatic aspect of that meaning. Perhaps the various approaches to language, and to meaning of language, are mutually complementary. If we wish to be rigorous and clear, and be certain of each step we take, then we can say very little. On the other hand, if we wish to speak of what we are really interested in, taking all factors into account, then what we say will be not only less certain as to truth, but perhaps even less clear as to content.

I shall try to take an intermediate point of view. Perhaps the everyday work of a linguist is somewhere between the very rigorous methodology of semantics on the one hand, and the full-blooded concrete study of the speaking human on the other. The speaking organism is studied by psychology and psychiatry, but among linguists there is a comparatively recent tendency to lean toward the easier, neater, cleaner, more formal aspect of language, and to leave out that which is more interesting and concrete. I am probably representative of the majority of linguists in that 95 per cent of my work has to do with formal linguistics, the dry description of the stuff of language; only 5 per cent has to do with the more meaty, interesting, and human side. I will say also that, at least as far as I am concerned, the 5 per cent of my professional work is somewhat amateurish, and what I have to say about the meaning and pragmatic aspects of natural languages will have to be judged on its merits.

I shall divide the discussion into three parts. First, I shall say something about the acquisition of language, especially with regard to meaning. Secondly, I wish to say something about the continuity of form and meaning: the gradual shifting line between the stuff of language itself, and the meaning, in whatever sense you take it, of the form. Thirdly, I should like to present to you an idea which I call the nonplasticity of form, and I shall tell you what that means. I make that contradiction in terms intentionally.

1.

There are already two excellent papers on the acquisition of language in the Transactions of the Seventh Conference: "On the Development of Word Meanings" by Heinz Werner (2) and "The Development of Language in Early Childhood" by John Stroud (3). Therefore, I shall not say very much on what has been dealt with in those papers.

As to the acquisition of meaning of linguistic forms: When adults, who have forgotten how they learned meaning in their own language, learn a foreign language, they usually remember some of that process. If we wish to know what "chat" means in French, we are told it means "cat." But the other day my granddaughter asked me, "How does an English-speaking child learn what 'cat' means?" That is a difficult thing for me to answer in the vocabulary that she has.

That example, perhaps, is not so very difficult; in things like cats and dogs it is easy enough to tell by what is known as "ostensive definition." They may be pointed out. However, as Dr. Mead has said, if we point at a cat and ask, "What is that?" we may be told that it is a "finger." In fact, that is how the etymology for the classical form of the Chinese word for "self" came about. The graph for that is a picture of the nose, by ostensive definition.

Now, as meanings become less and less clearly related to easily delineated parts of the child's world, it becomes more and more difficult to locate and point at them, so the child learns very slowly and uncertainly during this process, and makes mistakes constantly. He gets into habits that diverge from the general usage of the people around him, and so has to be constantly corrected. We tell a child, "You don't write pictures, you draw them; no, you don't sing a story, you tell a story." Or you say, "Yes, that is a tomato, too; it's a yellow tomato."

Jesperson (4) told a story of a child who wanted to have some peace in the house. The adults around him could not understand what he wanted until, after a while, it began to occur to the father that a few days earlier there had been company in the house and they had been served beer. The little child had insisted on having some and had been refused; so after a while, the father had said, "All right; let's have some peace in the house!" That was an ostensive definition, which was given without enough varying circumstances to narrow it down so as to agree with the usage of the adult language. So, out of this "big, buzzing, blooming confusion," and out of "things" and "not things" – in Stroud's paper these latter terms were used – in a child's early life, it takes a lot of trial, error, and focusing before the child can isolate and recognize approximately recurring features and items of his world, and then relate them as to form and meaning in ways that agree with those of the people around him.

Now, from the point of view of complete system building, a rather different point of view from the rigorous language system as described earlier by Dr. Bar-Hillel, one might like to imagine that all linguistic forms could be related in some vast systems of multilingual, bilingual, and monolingual dictionaries, equating one thing to another. That would leave only a relatively small list of basic forms with primary meanings, and these could be merely defined ostensively, perhaps in a central museum of meanings. I imagine such a museum would have a Hall of Colors, a Hall of Gestalten, a Hall of Smells, and then, in a central court, it would have a Hall of Time where it would be shown what it is to be before, after, or simultaneous. Yes, right and left – there's something that will tax the ingenuity of the curator or even the director of the museum. How can one define that if the visitor insists that he cannot understand what that means? Martin Gardner (5) has recently written on this subject.

I started this museum of ostensive definition idea as something incidental; a kind of side show. What I wished to do was to set up a straw man and knock him down. In the actual process of learning the meaning of language, we do not have such superhuman power as to know all the primitive meanings embodied in these fundamental ostensive definitions, or all the combinations and deductions so that we can know the rest of the language or languages involved. Actually, ostensive definition goes on all the time, whether in agreement with previous experience of an individual, or in disagreement, and what a child does in acquiring meaning, so as to agree in the main with adult usage, is constant correction. This means constant denial, in some respect, of previous ostensive definitions.

So, from this point of view, perhaps we could comment on one of the points brought up earlier as to whether, if we say 17 times 19 is 323, it gives any information or not. From the point of view of the actual learning of the meaning of numbers, it does give a lot of information, because we have a lot of experience with 17 and 19, but perhaps not so much with 323; still, it does not come exclusively from the basic postulates for arithmetic. We certainly have many other ways of contact with the number 323. Likewise, in the example of the statement that the three medians of a triangle meet at one point, if we stick to the initial Euclidean postulates as all that are necessary, it seems to give us no new information. But in practice, if we draw a triangle and three medians with ruler and compass, they often do not quite meet in the same point, and by revising the drawing, we try to make them meet in one point. The result is that we make better medians. Of course in geometry, as it is written, we do not define medians that way. It is the common practice to treat certain parts as belonging to the postulated parts, and other parts as theorems, although the division is quite flexible. But in practice, one is just as meaningful, or just as useful, as the other.

That is typical of the way one learns meaning in language. One learns it without regard to independence of elements, independence of initial ostensive definitions, and without regard to consistency. The actual meaning one finally arrives at is a composite photograph – and composite photographs are always blurred – of the various associations one has met in life, so that ostensive definition is really going on all the time. The acquirement of meaning is a process of change in the individual, and that change, in the case of normal growth, will stabilize in approximate conformity with the society around one.

The society around one is, however, also subject to change, if at a much slower rate. The acquirement and change of meaning in the child's language parallels to some extent the same processes in history and may be compared with ontogenetic recapitulation. The earliest uses of language by a child or mankind tend to lump together emotional outlet, influencing of action, and factual observation, all three in an undifferentiated way. This undifferentiated use of language seems to be the state of affairs as found among the so-called primitive peoples and in early literatures.

Usually one thinks of a child as uttering sounds which are an expression of some emotional state; then the expressing of wants comes rather earlier than factual comment, or reporting on observations of the world. But sometimes the opposite seems to be the case. In my granddaughter's speech, the word "water" was used in commenting on the presence of water months before she discovered, to her great surprise and satisfaction, that the mentioning of the word could bring about the thing itself. In Helen Keller's account (6) of her memory of the first word she learned as a word, the word "water" was also understood in a somewhat cognitive sense, though very much charged with feeling.

I do not need to say much about the shift of scope in meaning which occurs in the child's language, as it does in the history of the language. I shall only note one factor in the change of meaning which may be of relevance to our discussion in that it has to do with quantitative information theory, namely, the matter of frequency of occurrence. In consonance with the idea in information theory that the occurrence of a frequent item out of a list of possible items gives less information than that of a rarer one, a very common word or phrase or any linguistic form "means" less than an unusual one, whether it is for the purpose of simple information, literary appreciation, or for influencing action. That is why basic English, no matter how skilfully composed, always seems to taste so insipid and that is why poets search for less hackneyed words and expressions in order to obtain fresher effects. For words charged with meaning and significance fade with use and have to be replaced by newer ones until they in turn have to be replaced.

2.

My next topic is the continuity of form and meaning, or to use Mr. Bateson's terminology, the continuity of communication and metacommunication.

In my very unmathematical treatment of this subject, I should say that two points make a dichotomy and three points a *continuum.* Most descriptive linguists today, counting myself as one, would agree, whether they are in psychological theory behaviorists or not, that language is a type of social behavior. Linguists as a class are less ready to regard meaning also as behavior or something reducible to behavior, but I suppose everyone will agree that meaning is context in some sense; that is an expression used as early as 1909 in E. B. Titchener's (7) work on the psychology of thought processes. And he was a typical introspective psychologist.

The word "context" can have various graded senses. If it is linguistic context, such as context in the use of pronouns, then of course it is already form, but in most cases it is the social context, or at least the context of nonlinguistic items in the speaker's experience.

These things called nonlinguistic are not simply nonlinguistic. I think of them as having various gradations of linguistic status, and have classified them as follows: Zero grade, or I, is form, apparently without meaning. II is some arbitrary association of form and meaning. III is form, and some kind of behavior which is not usually regarded as linguistic form, but which I believe can be treated in the same way as we treat linguistic forms. IV is my own particular interest, i.e., stylistic elements; roughly speaking, it includes gesture, intonation, voice quality, and so on. V is a certain subtle, less tangible situation, which seems to be the meaning of the form, but often not verbalized. Finally, VI is the literal meaning, the core of the meaning of the form.

I shall take up the first instance of form, which apparently has no meaning. We have all probably had the experience of reading through a page while daydreaming about something else, and then, suddenly realizing that we did not understand what we had read, we have gone back for the meaning. Here is an experience of what seems to be pure form in the use of language. The example I mentioned earlier, of learning by heart all the classics without knowing what they mean, seems to me to have at least the emphasis on the learning of it; that is, one begins to learn the form. In such a case, language is an experience rather like listening to music; you enjoy it, but it does not necessarily have a further reference. There is a famous essayist (8) who was proud that when he read books he did not attempt to understand them too thoroughly because he was enjoying just the form of the language.

Secondly, we have the form, and also some arbitrary association, which seems to be its meaning but really has nothing to do with the use of that language, so far as the socially sanctioned usage is concerned. There is probably a medical or psychological term for the experience we have just before going to sleep, when various things pass through the mind, and somehow one thing seems to mean something else. And yet afterward, in recalling it, we see that they have actually nothing to do with each other. A fine example of that was mentioned by Dr. Klüver (9): his dream about a sack of Idaho potatoes was a perfect illustration of "the synthetical unity of the manifold in all possible intuitions."

I now come to the third item: the continuity of form and overt behavior. Piaget (10), in reporting his observations of the behavior of children, reports that children use language as behavior in the classroom, for instance, along with other behavior. The two interchange or alternate, and the children do not necessarily use them for the purpose of communication. In one of the experiments, a child, whom he calls the "explainer," is told a story, or is given an explanation of some mechanism or physical object. The child is then asked to explain it to another child. Even though it is often not successful as an explanation, and the listening child may not really understand, yet very often it works out as a game consisting of gestures and speech, or one alternating with the other, and in that way the second child learns.

Another example would be the comparison between the early days of the sound movie and the later form. When it first became possible to put speech into a movie, there was speech going on all the time, as though a continuous stream throughout the story were considered necessary. But it was soon realized that a movie is just another kind of mixture, and that if the artistic purpose of the film is to be taken into account, the two media should be integrated into one articulate whole.

In language teaching, the conventional method is to give the forms in the foreign language, and then equate them to words, phrases, and sentences in the language of the learner. Contrasted with this is the so-called direct method, which tries to articulate the speech with what is going on; it has less to do with ostensive definitions of terms than with the actual using of the language in various situations, very much as a child learns language. One of the forms of this method, called the Coué Method, consists of demonstrations performed by the teacher, such as this: "I am taking this piece of chalk; I am writing on the blackboard; I turn around; I sit down," and so forth. It gets a little monotonous, with the limited situations in the classroom, but the reason why it is effective is because it exemplifies the actual condition under which language and behavior merge into each other when language is actually used.

Under this heading, I might mention the meaning of certain things, half remembered, in the form of some behavior attitude not clearly verbalized. I wish to make a telephone call, and the telephone is around the corner to my right, but something interrupts me. Then, after the interruption is over, I remember that I have to do something on this side, but the verbalization, or whatever other linguistic form it takes, of the making of that call is gone, and what remains is not specific enough to direct me to do exactly what I wished to do. I still remember the direction, and I have some tension in my muscles, but that seems to be all that is left of the meaning of that intended telephone call.

Now I come to the fourth category, that of "stylistic elements." This is not primarily in the sense of literary style, but the sense of those elements of language other than the distinctive units usually represented in writing in letters, or in other forms of writing. They are intonation, dynamics, over-all loudness, rhythm, voice quality, gesture, and, last but not least, diction, i.e., the frequency distribution of the kinds of words that we choose. The last mentioned is in fact the primary, though not the only, element of literary style. There are two approaches to the treatment of such elements: One is to regard them as just elements of language. One studies them, lists them, finds out what they mean, and perhaps symbolizes and teaches them.

For they have recognizable recurring patterns, with either conventionalized or physiologically natural meanings.

The other approach is to put such elements on a separate level, because they are more complicated. They are usually outside the normal list of sounds of the language; sometimes we do not have to deal with them, and, in reading a book, we do not find them spelled out. Henry Lee Smith* and George Trager (11) have regarded the study of at least some of these aspects as part of metalinguistics. As Dr. Fremont-Smith has said, here is an example where there is a rather serious conflict in the use of terms, because metalinguistics in this sense does not mean the study of any metalanguage. But we shall not discuss the merits and disadvantages of either usage.

In the stylistic sense, metalinguistics is sometimes concerned with the language being used, and comments upon it; but most of the time it has nothing to do with the language. It has really more to do with the behavior of the speaker or the speaking situation. One says:

"Good (mid rising) night (low-to-mid rising)!"
on parting; but one says to the hostess:

"Good (low) night (mid-high-low-mid double circumflex)!" meaning "Please listen to me; excuse me for interrupting, but I am leaving."

It is a historical accident that we have one part of language – to be sure, a very important part – recorded in writing, whereas most of the other parts are not recorded; however, they can be. A while ago, I was talking with Dr. Savage about the recording of expression in the dialogue of a play. That has actually been done. H. H. Davies' play, *The Mollusc* (12), has been transcribed by Dorothée Palmer in the International Phonetic Alphabet – but that is not the point, it could have been in ordinary orthography – with all the expression marks, mostly intonation marks, according to the system of Harold E. Palmer. That gives one specific interpretation in which the play could be given. Of course actors may not like it; they

* Smith, H. L., Jr.: *An Outline of Metalinguistics.* (Unpublished data)

might say, "I should rather read it some other way." On the other hand, consider a similar kind of recording, the matter of musical notation. Formerly, a composer wrote out the figured bass and let the organist do what he liked; there were few or no expression marks. But the modern practice is to write out even the exact time values of grace notes, and make very detailed expression marks, dynamic and otherwise, for all that is part of the music. The question is: Is the composer supposed to do all this? How much is the composer expected to compose, and how much is the playwright expected to write? In the case of the transactions of this conference, how much are we expected to include? It is a relevant question, because in some instances, one cannot understand the meaning for lack of the necessary significant or distinctive linguistic elements.

I am inclined to agree that the two usages of the term "metalanguage," on the one hand, and "metalinguistics" on the other, overlap in certain situations, but I do not believe that they are completely of the same scope. I can also cite examples where there would be shifting from one to another. If we say, "*I* do not believe it," we have the ordinary sense, plus the metalinguistic element in the Smith and Trager sense, of the voice qualifier, with a contrasting stress on "I." But if we are translating into French, we cannot say, "*Je* ne le crois pas," because we would be changing it to something that is not French; we would have to say, "Moi, je ne le crois pas," which consists of changing only the ordinary items of the phonemes and morphemes of the sentence, without using any special element that would be called metalinguistic. There are other cases of that kind. Take the intonation which H. E. Palmer calls "the swan," because the intonation curve looks like a swan, giving a concessive implication. "It's good (mid-high-low-mid)" means that it is good, but there are some other objections to it. That, of course, is metalinguistic in the sense that it is put on top of the predicate "good." But if I try to translate that into Chinese, it would take the form, "Good is good," meaning, "As for being good, it is good (but as for something else, it may not be)"; so that if I were building that sort of equivalence into a translating machine, I should have to build in it: "Input, swan intonation: output, predicates, verb 'to be,' repeat predicate," etc., to respond to that situation. Thus, on the one hand, we have the ordinary as well as metalinguistic elements, but in the other language it is just rephrasing of the material.

I should like to go on to the fifth heading of less tangible situations, as shown in the example Mr. Bateson gave of the hand stretching out from the frame,* and as shown still more vividly in Dr. McCulloch's example† of the ground crew calling in the pilot. The fact that the pilot is talking continually is behavior which may be interpreted, in addition to what he is saying (which may be nonsense or unimportant things). The pilot knows that he is talking, but he does not think of this as a coded message, but rather as routine aviation procedure. The fact that he does that would place it under metalinguistics in the Smith and Trager sense. Because it is about the language, it would also be a metalanguage, but perhaps not in Mr. Bateson's sense, because what the pilot says may be nonsense or unimportant. He is not talking *about* it; he knows that he is on the beam for the next second or two, so it may not be relevant; it may not be metalinguistic in the other sense.

I shall give another example, which I have observed in more than one child. There is a scene; a mother scolds or punishes a child, and the child cries. Then, after a while, the mother comments on something else, quite unrelated. From the behavioral situation, the child learns that that incident is closed. After some experience with that sort of thing, the child will begin to test the situation by asking questions, or asking for things, meanwhile watching how the mother

* Let us consider a picture showing a man holding a glass of whiskey. There is a frame around him, and the frame is there as part of the advertiser's message, to attract and focus the attention on that which is to be the figure as opposed to the ground in the message. The frame, then, appears to be, "Oyeh, oyeh!" a listening exclamation, a command; "attend to my message about this whiskey." Then it becomes apparent that the frame is being used in other ways; this device is very common in liquor advertisements. For example, the hand that is holding the whiskey will be projecting from the picture outside the frame, in the style of trumboleic pictures, in which the frame, which suggested the unreality and limitation of the picture, is now used as a counterstatement, so that an unreal reality can be attributed to the picture by transcending the frame, and so on. The messages of these various components — the picture, the hand, the frame — are all we have of messages about each other.

†When a plane is coming into an airport, the pilot is "talked in," with a steady stream of conversation. It is the business of that conversation to say, "I am in contact with you, I am ready to pick up whatever you want," and so on and so forth, so that the man knows his line of communication is open. The statement that the line of communication is currently open is not the message that is conveyed by the speech as speech. That speech as speech is saying, "You're on the beam, pull a little more to the right, stay with it," that sort of thing. That is the official concept of it. But the steadiness of the stream of this is the assertion. "This line of communication is open." In a sense, this is a remark concerning the channel or concerning the language.

reacts. Then the mother, knowing from experience that this is a test, will or will not change the subject, as she sees fit. This intentional manipulation, which hitherto has been part of the total behavioral situation, will become part of the language, so it would be a metalinguistic element which is a little more subtle than intonation or stress patterns.

Another example which is similar to the ground-to-plane talk is this: When I back my car, I say to my wife or daughter, "Is there anyone behind?" or, "Is there a car behind?" and they will say, "No," and stop. Then I will say, "Keep saying 'No car, no people; no car, no people,' until I have backed out." That is quite parallel to the talk between the ground and the pilot. It is not only the message itself, but the fact that it is continually being sent that is an important message. In developing this technic for backing a car, I did not know about the aviation technic; it began merely as behavior between people. Neither I, nor those who took part in this behavior, treated it as a language symbol, but after a while it developed as a symbol for those who co-operated in using it.

I think my item VI, the literal meaning of linguistic forms, is something of an anticlimax. Since we have already discussed it under the general idea of ostensive definition, I shall not spend any more time on it. I shall go on to the third aspect of the pragmatic language, which is here more formal than pragmatic, because I am more concerned with forms; namely, the nonplasticity of forms. If you do not like that apparent contradiction, you may speak of the autonomy of forms, or perhaps even the primacy of forms, because by "form" I mean the stuff of language itself.

3.

Forms have a way of being themselves and going their own ways. They are made by speakers according to the nature of their speech and hearing organs. They have been adapted by communities through the activity of individuals who have exercised their vocal organs in ways that are recognized and accepted by others of the same community. They are forms which have been used more or less effectively in their original application or have been adapted to other activities; there is no reason why they should not be well adapted to the purpose of the interpersonal and intrapersonal communication. However, it is highly improbable that the habits of formation of sound sequences should have any simple or systematic relationship to the rest of the life of the individual in the community.

As I said, the processes of natural formation, conventional design, learning and transmission, and perhaps also of forgetting of linguistic forms have their own special ways. They have their physical and physiological conditions. They have a strong resistance to any attempt to mold or change them at will. They are not wholly plastic in the ordinary sense. There are strong cultural traditions for keeping a language as it is and allowing only very slow changes, especially in the larger structure of words and sentences, and there seem to be also strong physiological, noncultural conditions of the human organism, which put rather narrow limits on the possibilities of linguistic elements and their manner of combining.

Most languages in the world can conveniently be analyzed into phonemes and morphemes. The morpheme is the smallest unit of one or more phonemes that has a meaning, and the majority of morphemes have one or two syllables.

Many linguists have tried to put both phonemics and morphology under one uniform method of analysis by treating both on a distributional basis, that is, according to the manner in which elements of various sizes (phonemes and morphemes) typically occur and recur. While a morpheme defined in distributional terms is a very different sort of thing, intensionally, from a morpheme defined as the minimum unit with meaning, there is no reason why it should be incompatible with it, extensionally, in actual application to any given language. In practice the old definition is still the only one used by practicing linguists, including those who develop the new approach. Harris (13) has made an analysis of the morpheme on a purely distributional basis. For a different but more rigorous than the traditional treatment of the morpheme see Hockett's manual of phonology (14).

In Hawaiian, there is a fish called a *homohomonukunukuapua*, and you cannot analyze that into smaller meaningful units. But at the same time, in that same language, there is another fish which is called ø.* In inflected languages, however, accidences or elements of inflection are morphemes, even though they are often less than a syllable. Expressive or stylistic elements will usually spread over more than one syllable, and over a phrase or a whole utterance if they are included under morphemes, but on the whole they are of medium length. In some languages we have morphemes which do not consist of continuous rows of elements, but groups of, for example, three consonants, with spaces filled in by vowels which do not belong to that morpheme and which may indicate something else. I would not call such forms rare, but it is certain that they are to be found in only a minority of the well-known languages.

By and large, the size of the morpheme is less than one, or two syllables, or, roughly, four or five phonemes. As to the number of phonemes in a language, most languages have a moderately small number. The list rarely falls below ten, and rarely even approaches 80, so that, speaking roughly again, the number of phonemes in a language is of the order of two to the fifth power, or five bits of information. This would give a total of over 20 bits to each morpheme. If any phoneme could combine with any other in any order, it would yield a possible vocabulary of the order of millions. Actually, however, the succession of phonemes is so limited to characteristic patterns that the total number is always only a very small fraction of the possible number. In other words there is great redundancy, in the informational sense, in the actual use of phonemes in a language. This has been clearly demonstrated in detail by Cherry, Halle, and Jakobson (15), who have analyzed Russian, with a typical repertory of 42 phonemes, not by just counting the phonemes, but by going into the subphonemic distinctive features.

If we consider the vocabulary of basic English, it is supposed to have a list of 850 words, but in this case it is necessary to put "words" in quotes, because the system of counting is quite special; for example, we count "what" as the neuter gender of "who." That would be an example of one word. It is by that method of scoring that we arrive at the number of 850. It is really a good deal more than one thousand by one of the more usual ways of counting words.

---

* This short-named fish was mentioned at a public lecture that I attended but I have not as yet been able to find further reference to it in any publication.

Again, if we count the number of syllables in Mandarin Chinese, it comes to 1,279 syllables, counting the difference in tone, because tone is a part of the phonemic constituents. This is apart from the question of intonation, because intonation in Mandarin is something else than tone.

This in no way suggests that linguistic forms are quite flexible and plastic, because it shows that languages tend on the whole to take meaningful units of a certain size and shape, no matter how we make use of them. Wherever there are marked deviations from this average condition, leaving such exceptional cases as *homohomonukunukuapua* and *ø,* there is usually a tendency to bring it back to this approximate average, shall we say, by negative feedback of some sort. A morpheme must not be too large, and if there are too many phonemes in an unanalyzed whole, the speaker tends to analyze it anyway, and that is one of the causes of folk etymology. There is too long a string of sounds, and we wish to put meaning into parts of them. If they are similar to some morphemes already in the vocabulary, we break it up accordingly.

On the other hand, if the natural historical wear and tear of distinctive sounds have lowered the informational capacity of any linguistic item, resulting in coalescence of words originally distinct, then there is another compensatory change. When the romance word for "bee" was worn down to a single vowel *"e,"* it dropped out of the French language and a modified word was used in its place. Of course such things are not consciously done on the part of the speakers in any way, but rather indirectly through the choice or preference for certain forms to others. We have a situation like that in the change from ancient to modern Chinese. In the ancient Chinese of 601 A. D., there were 3877 distinguishable syllables, each of which was, on the whole, the size of single morphemes. When the distinctions had been worn down and reduced to modern Mandarin, of 1279 syllables, what happened at the same time of the change was that we had a great number of dissyllables, which, although analyzable historically and meaningfully on the part of the educated, are spoken by the illiterate as unanalyzed wholes, so that we come back to a number much larger than 1,000.

I have been speaking of the size of morphemes as one of the most interesting of the cases of the nonplasticity of forms, but that is not the only aspect of language that is nonplastic. Conditions of transmission and of reception of signals and codification will affect the morphemes too, and a change of method of written records will also change the style. The style of pen strokes and drawing, in fact the whole system of writing, can be affected according to whether one uses a stylus, brush, pen, pencil, or stenotype. As I was putting down these notes in Chinese, I quite unconsciously changed the order of the items, which is rather symptomatic of this phenomenon. I had at first the order: "lead writing-instrument, steel writing-instrument" and so on. That is the usual order. But when I came to typing the list in English, it became "pen, pencil," because "pen" is the simpler unit and "pencil" is "pen" with something added to it, and just because of the mechanics of the thing itself, it changed the very order of the terms without my knowing what I had done.

A more extreme example of the autonomy of forms going their own ways is the divergence of literary Chinese, which leans heavily on visual differentiation of homophonous characters, from the spoken language, which must satisfy the requirement of auditory intelligibility. I have constructed a story in literary Chinese, consisting of only the syllable *shih* (in four tones, to be sure) pronounced 106 times and therefore not auditorily intelligible, and yet, in characters:

施氏食獅史

石室詩士施氏嗜獅誓食十獅氏時時適市
視獅十時氏適市適十碩獅適市是時氏視是
十獅恃十石矢勢使是十獅逝世氏拾是十獅
屍適石室石室濕氏使侍試拭石室石室拭氏
始試食是十獅屍食時始識是十碩獅屍實十
碩石獅屍是時氏始識是實事實試釋是事

it is as clear and idiomatic as any other prose telling the story of Mr. Shih eating lions:

> "Stone house poet Mr. Shih was fond of lions and resolved to eat ten lions. The gentleman from time to time went to the market to look for lions. When, at ten o'clock, he went to the market, it happened that ten big* lions went to the market. Thereupon, the gentleman looked at the ten lions and, relying on the *momenta* of ten stone arrows, caused the ten lions to depart from this world. The gentleman picked up the lions' bodies and went to the stone house. The stone house was wet and he made the servant try and wipe the stone house. The stone house having been wiped, the gentleman began to try to eat the ten lions' bodies. When he ate them, he began to realize that those ten big lions' bodies were really ten big stone lions' bodies. Now he began to understand that this was really the fact of the case. Try and explain this matter."

The nonplasticity of forms makes it easy for communication between persons with the same or similar forms of schemas, and it makes it difficult between persons who have different forms, such as classical and modern Chinese. In the Transactions of the Eighth Conference on Cybernetics, Donald MacKay (16), in his paper on symbols, made a distinction between communication by what he called prefabricated representations, on the one hand, and the theory of scientific information, the designing and learning of new representations, on the other. When we come to the meaning of natural languages, it is largely in the form of prefabricated representation, at least so far as communication between adults is concerned. But even in the case of communication between children, we find that at a certain age in acquiring prefabrications, they tend to follow a recalcitrant tendency of keeping to what they already have.

I should like to quote from Piaget (17) again on that point. He says, in *The Language and Thought of the Child*, "It is not paradox to say that at this level" – that is, between the ages of 7 and 8 – "understanding between children occurs only in so far as there is contact between two identical mental schemas already existing in each child. In other words, when the explainer and his listener have had at the time of the experiment common preoccupations and ideas, then each word of the explainer is understood, because

* The word for "big" has an alternate pronunciation *shuo*[4], if it is of any help.

it fits into a schema already existing and well defined within the listener's mind. In all other cases, the explainer talks to the empty air. He has not, like the adult, the art of seeking and finding in the other's mind some basis on which to build anew." That is MacKay's second problem. But it is interesting to see that Piaget adds a footnote to this discussion, which seems somewhat germane to our discussion here. He says that Nicolas Roubakine (18) came to an analogous conclusion in his studies on adult understanding in reading. He showed that when reading each other's writings, adults of different mental types do not understand each other. But we adults of different mental types do make honest efforts, as we have been doing, in the building of new representations, and in the explication of prefabricated representations. So, in order to conclude my remarks on a positive note, I will make a correction in what I said at the beginning about the complementarity of rigor and content: If you wish to be precise and accurate, you cannot say much, and if you wish to say a great deal, you cannot be clear and precise; as though there were a parameter there that had to be constant, and which we could not do anything about. I feel that, with our efforts to approach from both sides and the efforts to break or interpret our code, we have slightly increased the value of that parameter. For a parameter is a constant which is variable. Even if the product of quantity and accuracy must remain less than or equal to an upper limit, I visualize it as a parameter which had one value yesterday, and has another value nearer to the upper limit today. Dare one hope that that upper limit is perfect mutual understanding among all the disciplines?

APPENDIX I: SUMMARY OF THE POINTS OF AGREEMENT REACHED IN THE PREVIOUS NINE CONFERENCES ON CYBERNETICS*

WARREN S. McCULLOCH
Research Laboratory of Electronics
Massachusetts Institute of Technology
Cambridge, Mass.

EINSTEIN ONCE DEFINED truth as an agreement obtained by taking into account observations, their relations, and the relations of the observers. In his case, the observations were the coincidences of signals at points in frames of reference; their relations were matters of space and time in those frames; his observers were reduced to what Helmholtz called a *locus observandi,* devoid of prejudices and imagination; and the only relations he had to consider among them were their relative positions, motions, and accelerations. The truth he had in mind is a picture of the world upon which all observers can agree, because it is expressed in a manner invariant under the transformations required to represent the relations of the observers. It is a paradigm for what "scientific agreement" may mean.

Unfortunately for us, our data could not be so simply defined. It has been gathered by extremely dissimilar methods, by observers biased by disparate endowment and training, and related to one another only through a babel of laboratory slangs and technical jargons. Our most notable agreement is that we have learned to know one another a bit better, and to fight fair in our shirt sleeves. That sounds democratic, or better, anarchistic, as you have twice reminded me. Aside from the tautologies of theory, and the authority of unique access by personal observation of a fact in question, our consensus has never been unanimous. Even had it been so, I see no reason why God should have agreed with us. For we have

* This material was distributed to the participants in advance of the Tenth Conference on Cybernetics.

been very ambitious in seeking those notions which prevade all purposive behavior and all understanding of our world: I mean the mechanistic basis of teleology and the flow of information through machines and men. In our own eyes we stand convicted of gross ignorance and worse, of theoretical incompetence.

Our meetings began chiefly because Norbert Wiener and his friends in mathematics, communication engineering, and physiology, had shown the applicability of the notions of inverse feedback to all problems of regulation, homeostasis, and goal-directed activity from steam engines to human societies. Our early sessions were largely devoted to getting these notions clear in our heads, and to discovering how to employ them in our dissimilar fields. Between sessions many of us made observations and experiments inspired by them, but we generally found it difficult to collect sufficient appropriate data in the 6 months between meetings. At the end of the first five sessions, of which there are no published transactions, we elected to meet but once a year, keeping our group together as nearly as possible, replacing a few who were lost to us, and inviting a few speakers to help us where help was needed most.

By the time we made this change, we had already discovered that what was crucial in all problems of negative feedback in any servo system was not the energy returned but the information about the outcome of the action to date. Our theme shifted slowly and inevitably to a field where Norbert Wiener and his friends still were the presiding genii. It became clear that every signal had two aspects: one physical, the other mental, formal, or logical. This turned our attention to computing machinery, to the storage of information as negative entropy. Here belong questions of coding, of languages and their structures, of how they are learned and how they are understood, including the theme of this, our last meeting, in which we expect to range from the most formal aspects of semantics, to its most contental contact with the world about us. For all our sakes I wish Wiener were still with us, but I understand that he is at present happily immersed in the clear and serene domain of relativity.

To refresh our memories and inform our guests, let me recapitulate, in logical rather than chronological order, the topics we have considered, and on which I believe the majority of us have been of one mind, to the limit of our ability to understand the evidence or the theory. You may find the consensus more frequently in my statement than in our published transactions. I am compelled to watch your faces, and to guess, before I let you have the floor, whether you will speak to the point or not, and from which side of the fence. With malice aforethought I have given the malcontent the floor, because he disagreed, or doubted, however unreasonably. Before I knew you so well this happened by accident, but as time went on, and we learned one another's languages, I learned that it was the best way to keep our wits on their toes. Our guests have been remarkably good sports, but the transcriptions of our discussions inevitably sometimes result in misunderstandings and altercations instead of agreement. Of those who understood and agreed the transaction reveals nary a trace.

Feedback was defined as an alteration of input by output; gain was defined as ratio of output to input; feedback was said to be negative or inverse if the return decreased the output, say by subtracting from the input. The same term, inverse or negative feedback, was used for a similar effect but dissimilar mechanism, wherein the return decreased the gain. The transmission of signals requires time, and gain depends on frequency; consequently, circuits inverse for some frequencies may be regenerative for others. All become regenerative when gain exceeds one. Regeneration tends to extreme deviation or to schizogenic oscillation, unless gain decreases as the amplitude of the signal increases. Inverse feedback determines some state to be sought by the system, for it returns the system to that state by an amount which increases with the deviation from that state. Servomechanisms are devices in which the state to be sought by the system is determined by signals sent to that system from some other source. These notions were applied to machines, including the steam engine and its governor, to the steering engines of ships, to well-regulated power packs, telephonic repeaters, self-tuning radios, automatic gun-pointing machinery, etc., and thereafter to living systems. Homeostasis was first considered in terms of reflexive mechanisms, in which change initiated in some part of the body caused disturbances, including nervous impulses, which were reflected eventually to that part of the body where they arose, and there stopped or reversed the processes that had given rise to them. Similar regulatory circuits entirely within the central nervous system were found to resemble the automatic volume control of commercial radios. Appetitive behavior was described as inverse feedback over a loop, part of which lay within the organism, part in the environment. When a target or a goal could be indicated, a description of appetitive behavior was found to be couched in the same terms as that for self-steering torpedos and self-training guns, whether these devices emitted signals reflected by their targets, or merely depended upon signals emitted by the target to readjust subsequent behavior to the outcome of previous behavior so as to minimize its error. Wiener drew a most illuminating comparison between the cerebellum and the control devices of gun turrets, modern winches, and cranes. The function of the cerebellum and of the controls of those machines is, in each case, to precompute the orders necessary for servomechanisms, and to bring to rest, at a preassigned position, a mass that has been put in motion which otherwise, for inertial reasons, would fall short of, or overshoot, the mark. These notions have served to guide subsequent neurophysiological research in the functional organization of the nervous system for the control of position and motion, some carried out in my laboratory in Chicago, and others by Wiener, Pitts, and Rosenblueth in the Institute of Cardiology in Mexico City, as well as by our friends in other laboratories. The general organization was found to consist of multiple closed loops of control, but the circuit action was extremely nonlinear, and consequently not amenable to any general simple mathematical analysis in terms of the Fourier Theory. Generally, multiple loops, severally stable by inverse feedback, may be unstable in conjunction, but the system can be stabilized by adding a portion of each of the returns and subtracting the sum from one or more of the servos. Such a system was found in the central nervous system by Setchenow in 1865, and rediscovered by Magoun. A group of us is studying the detail of its multiple afferents and its mode of affecting all reflexive activity; we shall use destructive lesion, and shall stimulate and map sources and sinks in various parts of the nervous system by methods presented at the last meeting. With failure of inhibitory signals or increased gain, the stretch reflex becomes regenerative, producing a rise in tone and a series of contradictions known as *clonus*. This has been elegantly analyzed, quantitatively, by Rosenblueth, Pitts, and Wiener, as described at our conference. Moreover they were able to demonstrate that the pool of relays of the so-called monosynaptic arc showed two numerous groups of relays, and a third less numerous, as judged by the random distribution of thresholds around three maxima. It will be years before we have fully exploited these notions.

Closed loops within the central nervous system — first suggested by Kubie as a substitute for undiscoverable motor activity proposed by the behaviorists to explain thinking in terms of reflexes, and by Ranson to account for homeostatic processes within the central nervous system, and independently discovered and demonstrated in the case of nystagmus by Dr. Rafael Lorente de Nó — were mentioned as possibly accounting for transitory memories by McCulloch and Pitts, who indicated that they were logically sufficient, but physiologically improbable, as an explanation for all forms of memory. Livingston has suggested that such mechanisms might account for causalgic symptoms after blocking or removal of perverted peripheral circuits which had been rendered regenerative by some trauma resulting in streams of impulses over small afferent neurons appreciated as burning pain. Kubie had proposed that the core of every neurosis was a reiterative process in some closed loop.

I have summarized and presented to the Royal Society of Medicine evidence along all of these lines, with much more obtained from many varieties of intervention in causalgia. It is clear that the notions of feedback are the appropriate ones for the understanding of the normal function and diseases of the structures in question. Since that time, Dr. Galarvardin of Lyons, studying patients with auditory hallucinosis accompanied by muscular activity of mouth, tongue, and larynx, has had removed, bilaterally, the postcentral somesthetic area for the face. The consequent disappearance of the hallucinosis had lasted 18 months when I last heard from him. This brings one symptom of a clearly organic psychosis into line with the findings on those obsessive compulsives who have been at least temporarily helped by frontal lobotomy, in that the central pathways of some reverberative process within the brain have been partially interrupted.

Again in terms of these notions, we have been able to make sense out of some aspects of what the psychologists have called goal-directed activity, and our attention has been duly called to the asymmetry of advance and escape, for in the former, the object sought is kept near the center of the receptive field of the sense organs, and behavior duly modified to approach it, whereas, in escape, learning along these lines cannot occur, and the behavior may easily become stereotyped. The most complex situations we have heard discussed are the stabilities engendered by inverse feedback in social structures of isolated communities reported principally by social anthropologists. Their devices have been extremely elaborate, depending, in some cases, on many interwoven loops. They seem to have utilized elaborate forms of distinctions and rules with respect to kinship, forms of address, hazing, bullying, praise, blame, and even rituals with respect to eating. Examples from ecology and from the behavior of anthills have extended these notions of inverse feedback.

Our members interested in economics and the polling of public opinion made use of these notions to explain fluctuations of the market, the banter leading to fight in roosters and boys, and the armament races initiating wars. In such circular systems, it becomes difficult to detect the causal relations. Wiener handled this by pointing out that it was possible to detect causality in the statistical sense by auto- and intercorrelations with lag, in those situations in which correlation was not perfect between the time series of the related component events, and explained how, with such devices, optimum predictions could be obtained. He doubted the applicability of this method to social problems, because of the shortness of our runs of the time series of information concerning human behavior. In these terms we discussed how a fielder catches a ball and a cat a mouse.

The question of conflict between motives was then raised by the psychiatrists, who, like psychologists, would like to have some common measure of value among human desires, comparable to what economists believe they have in the doctrine of marginal utilities and price in an open market. Kubie raised the question of the urgency of dissimilar ends, beginning with the need for moderate temperature, air, drink, food, sleep, and sex, the most urgent need resulting in the simplest response, and the least urgent allowing elaborate play. I indicated that an organism endowed with six neurons, constituting three chains of inverse feedback, and interrelated either by the requirement of summation or inhibitory links, was sufficiently complicated to exhibit the value anamoly, and if organization were left to chance would do so half the time. That is, given A and B it would prefer A; given B and C it would prefer B; but given C and A it would prefer C. A similar question was raised concerning dominance in the pecking order of chickens, but there was no adequate data as to the number of circles in coops of given numbers to settle the question. By this time we had become so weary of far-flung uses of the notion of feedback that we agreed to try to drop the subject for the rest of the conference.

Two interesting digressions appeared at this point: The first concerned cardiac flutter, which appears as a propagated disturbance running around the periphery of an area it cannot cross and it therefore cannot stop itself, whereas fibrillation appears as a disturbance which wanders over changing paths determined from moment to moment by shifts of threshold produced by previous activities at those points. Its mathematical analysis was indicated but not presented to the group. Second, Pitts presented a theory of disturbances in random nets, such perhaps as the cerebral cortex, in which it was possible to find a value around which to perturb the activity; namely, that probability of a signal in a neuron is equal to the probability of a signal in the neurons that are afferent to it.

Moreover, we had all come to realize that for problems of feedback, energy was the wrong thing to consider. The crucial variable was clearly information.

We began by considering computers as "analogue," if the magnitude of some continuous variable like voltage, pressure, or length were made proportional to a number entering into a computation; and as "digital" if they were a set of stable values (at least two) separated by regions of instability, and the number was represented by the configuration of the stable state of one or more components. Analogue devices showed tendencies for errors to appear in the least significant place, but were limited by precision of manufacture and could not be combined to secure additional places. Digital devices might show errors in any place (a limitation inherent in all positional nomenclatures), never required extreme accuracy, and could always be combined to secure another place, at the same price per place as previously. When components are relays, the digital devices sharpen the signal at every repetition. We considered Turing's universal machine as a "model" for brains, employing Pitts' and McCulloch's calculus for activity in nervous nets. It uses the calculus of propositions of the *Principia Mathematica*, subscripted for the time of occurrence of an impulse of a given neuron. We demonstrated the equivalence of all general Turing machines, and how they could be designed to answer any nonparadoxical question which could be put to them in an unambiguous manner. We considered the far-flung conclusions that followed here from Goedel's arithmetizing logic. It became clear that having ideas required circuits capable of computing invariants under the necessary groups of transformation, that is, reverberant activity preserving the form of its afferent, initiated at one time, or inverse feedback leading some figure of input by some path to a canonical presentation out of its many legitimate ones. Gestalt notions led only to multiplications of particulars with distortions attributed to "cortical fields" in which currents are conserved, though in nature there are sources and sinks, and although the areas of cortex they are said to pervade are anatomically discontinuous. The discrete action of nervous components was considered the only way in which they could normally function to handle the amount of information transmitted through them. Gross disturbances of function (epilepsy, etc.) were seen to be accompanied by gross fluctuations affecting most of the neurons in a given area in much the same way, thus producing a loss of information. Emotions were considered as expressions of some overrunning of parts of the computer, producing somewhat fixed responses to diffuse and variable inputs, as if in a Turing machine the computed value of an operand ceased to affect subsequent operations. Wiener proposed that by glandular means emotions might broadcast a "to whom it may concern" message, causing items to be locked in, or remembered. It was suggested that the best way to find out what an unknown machine did was to feed it a random input; clearly it had to be random in tems of the aspects of the input that the machine could discriminate. This was likened to the Rorschach Test, and its auditory equivalent, and it was noted that the gibberish produced by free association was apt to cause the psychiatrist to project his own difficulties on his patient.

Three kinds of the storage called memory were discussed at length: The first, active reverberation, such as in the acoustic tank, was recognized as responsible for nystagmus and the only storage left in presbyophrenia. J. Z. Young made use of the same notion to describe the residual memory after the destruction of the main memory organ of the octopus. The second type of storage has been found only in the octopus, where it occupies a separate structure with well defined and separate access and egress. The organ itself is composed of a host of small cells; the nature of its synapses is not yet well known. This is the storage that has excited theoretical physicists because of the immense number of bits retained by it. Von Foerster computed its size from access-time times access-channels against mean half-life of the trace, and Stroud from the number of snapshots one-tenth second each at, for example, a thousand bits per frame. Figures are in rough agreement that it lies between 10^{13} and 10^{15}. Instead of declining asymptotically to zero, a few per cent of the items are retained forever. Von Foerster has proposed mechanisms to account for this, requiring *circa* 0.02 watt; the brain is a 24-watt organ. Access to this store is probably not by simple addresses sought seriatim. Recall seems to rest on a process locating items by their contents. This was discussed by Von Neumann concerning similarities, and by Klüver concerning stimulus equivalences, but both had apparently noted retention of eidetic fragments, a topic which should be gone into much more thoroughly hereafter. Peculiarities of this kind of storage in man seem to be that the contents are a series of snapshots, each devoid of motion; they are accessible in the order of filing, not in the reverse order; there is a delay of about a minute between the making of the trace, and the time when it is first accessible; and finally, a snapshot too similar to the one before it may upset the process. These traces cannot be simply localized; each bit is an alteration of synapses effective somewhere in a net, and the alteration is not confined to some one junction. The third type of storage seems to behave more like the growth-with-use characteristic of muscle, and shows fatigue on too frequent testing. Shurrager apparently has evidence that it can occur at a monosynaptic reflex level, and changes with use have been seen where the vagus contacts ganglionic cells in the frog's auricle, but there is no evidence that such a change persists anywhere in the central nervous system, and there is no anatomical evidence that it ever happens there. Some change of organization with use does occur perhaps as suggested by Ashby. The organization of the visual cortex with use is a case in point. When congenital cataracts were removed, the difficulty with vision was in part found to be attributable to antithetical organization of these mechanisms by impulses from elsewhere in the central nervous system.

"Traffic jams" of brains become increasingly probable with increase in volume, for the number of long distance connections cannot be expanded to keep pace with the number of relays to be connected, except by increasing cable-space disproportionately. It was suggested that potentiation, described by Lloyd, may serve to lock in lines temporarily on a basis of their previous use; this would resemble a scheme proposed in Holland for more efficient use of limited telephonic facilities. Repetitive firing of cerebrospinal neurons leaving the cell body and dendrites largely depolarized when axons were hyperpolarized (P2 after-potential) would thus account for that component of facilitation marked by surface negativity and depth positivity of the cerebral cortex, as a matter of lowered threshold, with increased voltage of volley delivered to cord. The same mechanism would account for the stiffening up of the Parkinsonian patient.

Considerable time was spent discussing the way in which the actual flow of information determined the structure of groups, and discussing the way in which command moved from moment to moment to that place in the net where most information necessary for action was concentrated. In parallel computing machines, including brains, when one part is busy or damaged, another will serve for the same computation. This requires that the whole machine be tended by some part of the machine which can switch the problems to them. Such a machine might give correct answers when most of it was out of commission. What appears on one side as the problem of redundancy of neurons and channels composed of them, appears on the other as the problem of securing infallible performance from fallible components. Von Neumann's last work on this score, delivered at a conference on the West Coast, is titled "*Probabilistic Logic.*"

With respect to language, as second only to vision as a source of information to brains, and all important in human communication, not to mention psychoanalysis, it was generally admitted that we almost never say anything unless we wish someone to do something about it. Apart from this general hortatory aspect, language contains a few signs such as "hum," "um hum," "unh unh," and "huh," that are specifically so, but otherwise contentless. The question arises whether the logical particle comes from the signs used by dogs and small children. It was generally agreed, as stated by Dr. Mead, that when all definitions must be ostensive, as in learning a language where no possibility of translation exists, e.g., as a child, or as a newcomer to an island of an alien tongue, it is best to learn from children because they will repeat indefinitely. Learning was first defined as an alteration of transition probabilities. Speech, broken into phonemes, distinguished, according to Jakobson, by a few decisions between opposites, poorly represented at best in the conventional spelling of English, and studied by Licklider's method of chopping and distorting it to an incredible extent, retains its intelligibility when little is left beyond an indication of when pressure waves cross the axis, and even this is enormously redundant. One point, returning to ten snapshots per second, is the peculiarity of speech to remain intelligible when each tenth of a second is half speech and half noise many decibels louder. Total amount of information conveyed by speech is probably not more than ten bits per second, though it takes a thousand bits per second to produce a sound indistinguishable from it. Shannon's work on redundancy of English in reducing amount of information conveyed per symbol was studied from the position he shares with Wiener, that information is negative entropy. The recipient has a set of entities to match the signals he is intended to receive, and the signal causes him to make the selection. This selective information was found to be comparable to MacKay's logon information but not to his metron information, the point being that the entropic cost of a metron of information goes up as the square of the number of metrons, rather than as the numbers.

We have considered Zipf's law — that the number of kinds of any given rarity is proportional to the square of the rarity — but I do not think we are satisfied either as to the validity of the law, the basis of the exceptions, or the universe it presupposes. Finally, we have proposed to look into the amount of information conferred upon us by our genes, and have tried to straighten out for ourselves those difficulties that have arisen because of confusion of the level of discourse. It is my hope that by the time this session is over, we shall have agreed to use very sparingly the terms "quantity of information" and "negentropy."

REFERENCES

INTRODUCTORY REMARKS

1. LEIBNITZ, G. W.: *The Philosophical Writings of Leibnitz.* Translated by C. R. Morris. New York, Dutton (Everyman's Library), 1934.
2. ARISTOTLE: *De Anima. The Works of Aristotle.* Vol. III. W. D. Ross, Editor. New York, Oxford (Clarendon Press), 1931.
3. HUME, D.: *Treatise of Human Nature.* New York, Dutton (Everyman's Library), 1911.
4. KANT, I.: *Critique of Pure Reason.* Translated by J. Meiklejohn. New York, Dutton (Everyman's Library), 1934.
5. LAYCOCK, T.: *Mind and Brain.* 2nd ed., 2 vols. New York, Appleton, 1869.
6. BUTLER, S.: *Unconscious Memory.* rev. ed. New York, Dutton, 1910.
7. ———: *Erewhon.* Press, Canterbury, N. Z. 1863.
8. PITTS, W., and MCCULLOCH, W. S.: How we know universals. The perception of auditory and visual forms. *Bull. Math. Biophys.* 9, *127 (1947).*
9. LASHLEY, K. S.: Functional interpretation of anatomic patterns. *A Research Nerv. & Ment. Dis. Proc.* 30, *529 (1952).*
10. SHOLL, D. A., and UTTLEY, A. M.: Pattern discrimination and the visual cortex. *Nature* 171, *387 (1953).*

SEMANTIC INFORMATION AND ITS MEASURES

1. CARNAP, R., and BAR-HILLEL, Y.: *An Outline of a Theory of Semantic Information.* Technical Report No. 247, Research Laboratory of Electronics, Cambridge, Mass., Massachusetts Institute of Technology, 1952.
2. SHANNON, C. E., and WEAVER, W.: *Mathematical Theory of Communication.* Urbana, Univ. Illinois Press, 1949.
3. CHERRY, E. C.: A history of the theory of information. *Proc. Inst. Elect. Engnrs.* 98, 383 *(1951).*
4. MACKAY, D.: In search of basic symbols. *Cybernetics.* H. Von Foerster, Editor, Trans. Eighth Conf. New York, Josiah Macy, Jr. Foundation, 1952 (p. 181).

5. KEMENY, J. G.: A logical measure function. *J. Symb. Logic* 18, *289 (1953).*

6. CARNAP, R.: *Logical Foundations of Probability.* Chicago, Univ. Chicago Press, 1950.

7. QUINE, W. V. O.: *From a Logical Point of View,* Cambridge, Mass., Harvard Univ. Press, 1953.

MEANING IN LANGUAGE AND HOW IT IS ACQUIRED

1. WIENER, N.: *Ex-Prodigy.* New York, Simon & Schuster, 1953 (p. 299).

2. WERNER, H.: On the development of word meanings. *Cybernetics.* H. Von Foerster, Editor. Trans. Seventh Conf. New York, Josiah Macy, Jr. Foundation, 1951 (p. 187).

3. STROUD, J.: The development of language in early childhood. *Cybernetics.* H. Von Foerster, Editor. Trans. Seventh Conf. New York, Josiah Macy, Jr. Foundation, 1951 (p. 205).

4. JESPERSON, O.: *Language; Its Nature, Development, and Origin.* London, Allen & Unwin, and New York, Holt, 1922 (p. 116).

5. GARDNER, M.: Is nature ambidextrous? *Philosophy & Phenomenological Res.* 13, *200 (1952).*

6. KELLER, H. A.: *The Story of My Life.* New York, Doubleday, 1903 (p. 23).

7. TITCHENER, E. B.: *Lectures on the Experimental Psychology of the Thought Processes.* New York, Macmillan, 1909 (p. 175).

8. T'AO, CH'IEN (365-427 A. D.): Wu-liu-Hsien-sheng Chuan (Biography of the Gentlemen of the Five Willows) *Collected Works,* 1910.

9. KUBIE, L. S.: The relationship of symbolic function in language formation and in neurosis. *Cybernetics.* H. Von Foerster, Editor. Trans. Seventh Conf. New York, Josiah Macy, Jr. Foundation, 1951 (p. 233).

10. PIAGET, J.: Types and stages in the conversation of children between the ages of four and seven. *The Language and Thought of the Child.* 2nd ed. New York, Harcourt, 1932 (p. 50).

11. TRAGER, G. L.: The field of linguistics. *Studies in Linguistics: Occasional Papers No. 1.* Norman, Okla., Battenburg Press, 1949.

12. DAVIES, H. H.: *The Mullusc,* Annotated phonetic edition with tone-marks by Dorothee Palmer. Cambridge, Heffer, and New York, Appleton, 1929.

13. HARRIS, Z. S.: From phoneme to morpheme. *Language* 31, *190 (1955).*

14. Hockett, C. F.: *A Manual of Phonology.* Baltimore, 1955 (p. 15).

15. Cherry, E. C., Hallie, M., and Jakobson, R.: Toward the logical description of languages in their phonetic aspect. *Language* 29, *34 (1953).*

16. MacKay, D. M.: In search of basic symbols. *Cybernetics.* H. Von Foerster, Editor. Trans. Eighth Conf. New York, Josiah Macy, Jr. Foundation, 1952 (p. 182).

17. Piaget, J.: Understanding and verbal explanation between children of the same age between the years of six and seven. *The Language and Thought of the Child.* 2nd ed., New York, Harcourt, 1932 (p. 120).

18. Ferrière, M. A.: La psychologie bibliologique. D'après les documents et les travaux de Nicolas Roubakine. *Arch. de Psych.* 16, *101 (1917).*

CUMULATIVE INDEX FOR THE TRANSACTIONS OF THE CONFERENCES 1949-1953

N