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CYBERNETICS

CIRCULAR CAUSAL AND FEEDBACK MECHANISMS IN BIOLOGICAL AND SOCIAL SYSTEMS

Transactions of the Seventh Conference
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JOSIAH MACY, JR. FOUNDATION CONFERENCE PROGRAM

FRANK FREMONT-SMITH

Medical Director

I WANT to tell you how happy we are to welcome you to this Eighth Conference on Cybernetics. For the benefit of the guests present may I take a few minutes to explain the nature and purposes of the Foundation's Conference Program.

You have been brought together to exchange ideas and experiences, data, and methods in an effort to further knowledge in this field. However, the Foundation is also interested in investigating the broad aspects of the problem of communication and integration. Experience gained from many research projects presented for consideration has led to the conviction that one of the greatest needs today is a reintegration of science, which at the present time is artificially fragmented by the isolation of the several disciplines or specialties. We feel that the setting up of physiological and—what is probably more important—psychological barriers between the several branches of science is seriously interfering with scientific progress. Although the fertility of the multiprofessional approach is recognized, adequate channels of interprofessional communication do not exist. The Conference Program hopes to encourage this reintegration.

Thirteen conferences are now in operation covering the following fields: aging, adrenal cortex, biological antioxidants, blood clotting and allied problems, connective tissues, cybernetics, factors regulating blood pressure, infancy and childhood, liver injury, metabolic interrelations, nerve impulse, problems of consciousness, and renal function. Each of these conference groups holds annual two-day meetings for a period of five years.

When a new conference is organized fifteen scientists are selected by the Chairman in consultation with the Foundation to be the original members. In this selection every effort is made to include representatives from all pertinent disciplines. For the purposes of promoting full and free participation of all members and guests attendance at any meeting is limited to a total of twenty-five.

In contradistinction to the usual scientific meeting we place

the emphasis upon discussion and not upon the presentation of formal papers. The introductory presentations at our conferences are merely the launching of the ship—the voyage is the important thing! The person opening a discussion is similar to the person who breaks the bottle of champagne over the bow of a new vessel. In other words we feel the heart of these meetings is the discussion. Even though everything said is taken down by the stenotypist you will be given opportunity to edit your remarks or delete any which you do not want to appear in the published transactions.

From our experience with conferences we have learned that if one desires to communicate successfully with another person, one cannot limit oneself merely to making statements *at* him, and to increasing the power of one's transmitting set when he does not understand. Some consideration of the receiving set is needed. One point which should be stressed is that between the disciplines there are real difficulties in communication—partly emotional and partly semantic. Emotionally some investigators accept only data derived from methods or disciplines with which they are familiar. On the semantic level the physical and biological sciences can understand each other without difficulty as can the medical, psychiatric, and social sciences. However, to bridge the gap between the physical and biological sciences on the one hand and the psychological and social sciences on the other is very difficult. Through the Conference Program this Foundation hopes to foster communication and reintegration and in the published transactions to give a clearer reproduction than now appears in the scientific literature of what takes place in the laboratory and what goes on in the minds of scientists.

This program is an experiment and you are part of the experiment. The success of the undertaking is measured entirely by what the participants gain from such an experience. We encourage your critique and hope continuously to improve our conference techniques.

INTRODUCTORY REMARKS

WARREN S. McCULLOCH

Chairman

THERE are two or three things that I wish to say. In the first place, several of you are new to the group. You may have a little difficulty with some of us as we speak. If you do, please do not hesitate to interrupt in order to make sure you understand what the speaker is saying.

Second, to those of you who are new to the group, we ask you to join in the discussion: we want you to do that right away; but don't feel you are going to be called on for a presentation until you know what we are like.

Sometimes we become agitated and interrupt a person too often, or we find that we are asking him questions about the sentence he is going to say next. When we let someone have the floor, we should permit him to have his say at once, interrupting only if we don't understand what he is saying.

Some presenters like to pause at times to ask if there is any discussion at a certain point. We encourage that, but it depends entirely upon the wishes of the man who is making the initial presentation. Others prefer to be interrupted only on the matter of understanding, preferring to discuss actual content after they have completed their statements.

I hope you all received a copy of the proposed agenda for the meeting, and if there are no objections we will start roughly according to that proposal. I don't believe that we will follow the schedule rigidly.

Dr. Gerard, will you open?

SOME OF THE PROBLEMS CONCERNING DIGITAL NOTIONS IN THE CENTRAL NERVOUS SYSTEM

RALPH W. GERARD
Department of Physiology, School of Medicine, University of Chicago

I SHOULD like to begin by saying, especially for the benefit of the newcomers, that this particular group is the most provocative one with which I am associated. I owe more new ideas and viewpoints to the meetings we have had over the past few years than to any other similar experience; our gatherings, therefore, have evoked some insights. The subject and the group have also provoked a tremendous amount of external interest, almost to the extent of a national fad. They have also prompted extensive articles in such well known scientific magazines as *Time*, *News-Week*, and *Life*. Some of these events have, in turn, led me to speak to you this morning.

It seems to me, in looking back over the history of this group, that we started our discussions and sessions in the "as if" spirit. Everyone was delighted to express any idea that came into his mind, whether it seemed silly or certain or merely a stimulating guess that would affect someone else. We explored possibilities for all sorts of "ifs." Then, rather sharply it seemed to me, we began to talk in an "is" idiom. We were saying much the same things, but now saying them as if they were so. I remembered a definition of pregnancy: "the result of taking seriously something poked at one in fun," and wondered if we had become pregnant and were in some danger of premature delivery.

Since this group has been the focus and fountainhead of thinking along these lines, we surely have a very real responsibility, both internally and externally. Internally, since we bring expertness in such varied fields, no one can be sure another's statements are facts or guesses unless the speaker is meticulous in labeling suggestions as such. Externally, our responsibility is even greater, since our statements and writings—which may extend beyond an immediate area of competence—should not give a spurious certainty to a credulous audience, be this audience the lay intelligentsia or that precious company of young physical scientists now finding the happy hunting ground in biology.

The language, experience, and ways of thought, say, of communication engineering, seem to be admirably adapted to make us recognize explicitly that the nerve impulse is not merely some physical-chemical event but a physical-chemical event carrying meaning. It is therefore a sign or a signal, as the case may be; and this is very important in physiological thinking. To use the best mathematical techniques and tools is obviously highly desirable. Everyone here would agree, however, that mathematics, being essentially tautological, cannot put into conceptual schemes something not there in the first place. Moreover, I doubt if anyone in this room believes for a moment that we have made even a majority of the necessary basic biological discoveries of how the nervous system works. We cannot safely build upon presently available biological knowledge rigorous conclusions about the nature of brain action with any confidence in their enduring validity. Overoptimism has appeared before in this very area. In the early 1800's a flood of mathematical articles based upon the teachings of phrenology and exploiting them quantitatively, issued from the best minds of the time. That material is now known only to such encyclopedic minds as that of Heinrich Klüver, who told me about this.

To take what is learned from working with calculating machines and communication systems, and to explore the use of these insights in interpreting the action of the brain, is admirable; but to say, as the public press says, that therefore these machines are brains, and that our brains are nothing but calculating machines, is presumptuous. One might as well say that the telescope is an eye, or that a bulldozer is a muscle.

This brings us to the more immediate problems, particularly that of digital and analogical mechanisms in the brain. We have spent much time discussing these two types of functioning, and probably all here will agree that both types of operation are involved in the brain; but perhaps I disagree with the majority in the relative emphasis put on the two kinds of mechanisms. I personally think that digital functioning is not overwhelmingly the more important of the two, as most of our discussions would seem to imply, and I want to present some evidence for this view.

In the first place, everyone agrees that chemical factors (metabolic, hormonal, and related) which influence the functioning of the brain are analogical, not digital. What is perhaps not fully recognized is the tremendously important role that these play not only in the abnormal but also in the perfectly normal functioning of the nervous system. The influence of carbon dioxide, of acidity,

of the sugar level, of the balance between sodium and potassium, of calcium and a trace of magnesium, and the influence of the thyroid hormone, the ketonic group, which is coming into prominence as influence on the nervous system, and the action of still other factors, such as temperature—these are not only theoretically possible, but, in extensively documented experimental analyses, are demonstrably great. Variation in them can produce or remove convulsions, hallucinations, voluntary control, consciousness itself.

Bateson: I am a little disoriented by the opposition between analogical and digital.

Gerard: I was going to say a few words about that shortly, but instead I shall explain now. The picture that I have of analogical and digital, owing to the expert tutelage that I have received here, primarily from John Von Neumann, is this: an analogical system is one in which one of two variables is continuous on the other, while in a digital system the variable is discontinuous and quantized. The prototype of the analogue is the slide rule, where a number is represented as a distance and there is continuity between greater distance and greater number. The digital system varies number by integers, as in moving from three to four, and the change, however small, is discontinuous. The prototype is the abacus, where the bead on one half of the wire is not counted at all, while that on the other half is counted as a full unit. The rheostat that dims or brightens a light continuously is analogical; the wall switch that snaps it on or off, digital. In the analogical system there are continuity relations; in the digital, discontinuity relations.

To return to the thesis: the chemical aspect of neural functioning is entirely analogical; there are continuities of concentration and consequence.

Second, much of the electrical action of the nervous system is analogical. The brain waves themselves, the spontaneous electrical rhythmic beats of individual neurons, particularly the well known alpha rhythm, are analogical. I am quite satisfied, and I think most neurophysiologists are also, that these represent a continuously variable potential, not the envelope of discontinuous spikes. Further, steady potential fields exist about the nervous system and have been shown by us and others to vary with the physiological state of the brain or, conversely, when varied artificially, to modify the physiological state of the brain. These fields are also analogical. I hope later to say more about the alpha-wave aspect of these.

Third, remember that the existence of a digital mechanism is of itself no particular guarantee that its digitalness has functional significance. The skeletal muscle fiber, even the whole heart, is as digital as anything in the nervous system, perhaps even more completely so—the all-or-none law, as it is called, applies to all—

Finally, in this group of considerations, I would emphasize that the synapse itself (and with that the nerve impulse) probably does not function digitally in a great many, perhaps in a great majority, of the cases in the central nervous system. This needs elaboration.

The point about digital and analogical and continuous and discontinuous relations can be developed further in this direction: the nerve impulse is digital in character, it has the all-or-none property. That is, if a stimulus is progressively increased in intensity nothing happens, as far as any propagated message down the nerve is concerned, until some further tiny increment in intensity of the stimulus sets off a full-sized nerve message. The response is all or none, a characteristic digital response. Closer examination shows what really is involved: after one region of the nerve fiber has been activated, the excitation which it in turn generates, and which then becomes the effective stimulus to the next region of the nerve fiber, is well above the threshold for the next region. In other words, when region *A* has been activated, by whatever artificially applied stimulus, it itself develops a stimulus intensity which is much greater than the minimal intensity necessary to activate region *B*. That is, in both engineering and physiological terminology, there exists a high factor of safety. The factor of safety in the nerve impulse and nerve metabolism, as several of us have estimated, is five or more; there is about five times as much electrical current generated by the active region of the nerve as is necessary to excite the next region which is to be activated. This region, in turn becoming active, generates five times as much stimulus as is needed for the next; so propagation, once started, is guaranteed. Even relatively large fluctuations in the condition of the nerve, in the response of one region or the threshold of the next, will not disturb this overimpelling drive to go forward.

Now let us examine the situation at synapses. One synapse that has been studied, by Bullock (1), a single giant fiber synapse in the invertebrate squid, has a safety factor of about three. Some vertebrate synapses also have safety factors well above one, for each presynaptic impulse crosses to the postsynaptic fiber with no problem of summation or the like. This is true, for example, for

the synapse from sensory neurons from muscle receptors to sensory paths running up the spinal cord—as reported here last year by Lloyd (2). Aside from such particular cases, the story for central synapses is one of safety factors below unity; and this means analogical functioning.

McCulloch: I am sorry, I did not understand it. Will you say it once more?

Gerard: The safety factor for excitation to cross synapses in the nervous system, in most cases studied (primarily in the spinal reflex group), is less than one. I am going to document that.

Von Neumann: That means?

Pitts: The single afferent will not fire.

McCulloch: O.K. Agreed.

Gerard: First, the general phenomenon you know as subliminal fringe: when one impulse arrives it may do nothing; another impulse, which also does nothing itself, combined with the first one will produce a discharge. This is just the point about which you were asking.

Fremont-Smith: They don't have to be simultaneous?

Gerard: They don't have to be simultaneous but probably they have to be close together.

Wiener: For that reason there is a rather short excitation period.

Gerard: There may be a zero combination period, but when other cells are involved there may be a very extensive combination period. I don't want to develop this line further because other evidence is much more direct, and I shall give three or four samples.

One is the nerve-muscle junction. This also is ordinarily digital in the vertebrate; one nerve impulse elicits one response of the muscle, and it does so even under conditions of fatigue, drug action, and many other disturbances. Yet in some vertebrate junctions, and in all those of many invertebrates, there is not a digital relation with a safety factor of more than one, and a variety of summation effects are necessary before responses occur.

Another case is the squid synapse, already mentioned, with a high safety factor. Even under slight fatigue, nothing more than would probably occur during ordinary physiological activity, the safety factor at that synapse drops to less than one. Repeated incoming impulses are required to fire it and, even more, the response becomes highly variable. Presynaptic impulses, repeated perfectly regularly, sometimes give tetanic outbursts, sometimes nothing at all, and irregular fluctuations between these extremes. The same sort of variability appears in artificial synapses and in

the nerve fiber. It is especially seen in invertebrate fibers, in which a given stimulus may lead either to a full propagated response or to none, but all show gradations of local changes. The stimulus produces local potential oscillations which may die out gradually or quickly or increment gradually or quickly, so that as long as 30 milliseconds after a seemingly ineffective stimulus (a fantastically long period for nerve), a discharge occurs.

The clearest and most important evidence of analogical behavior of synapses is implicit in the work Lloyd (2) reported here last year. You may remember that I was asked to comment on it at the time; but I was not smart enough to see at once some of the more far-reaching implications. Let me remind you of the phenomena: he was dealing with a particular spinal reflex, the muscle-stretch reflex, in which the afferent neuron connects directly with the motor one—a monosynaptic arc. The motor nerve response to sensory nerve stimulation involves transmission across a single synapse. The size of the efferent discharge is, of course, a function of the number of motor neurons that discharge in response to a given afferent stimulus. Two other nerves play upon that reflex center. One of them, when stimulated with or just before the main afferent, will greatly facilitate the motor response. The other will similarly inhibit the motor response. A standard shock to the primary afferent nerve, at a regular interval of a couple of seconds, gave a constant motor nerve response; and the effects of stimulating the other nerves were tested against this stable background. So far, all this is standard neurophysiology.

The important finding was that rapid stimulation (tetanizing) of any one of these impinging nerves tremendously exaggerated the effect of that particular nerve on the reflex arc. The normal afferent nerve, given the standard stimulus a second or more after a brief tetanus to it, would produce a manifold greater reflex response. The facilitative nerve would, similarly, be much more powerfully facilitative after it had been tetanized, and the inhibitory nerve would produce a much more profound inhibition. In each case the changed effect was limited to the particular afferent nerve that had been tetanized, reflex responses to the other nerves being unaltered. Other evidence showed that this effect of tetanization was produced in the incoming nerve fiber, not at the synapse; and the magnitude and timing of the effect was related to the positive afterpotential of the nerve impulse. In other words, the size of the electrical message going along the nerve to reach the synaptic system determined the number of synapses crossed. An increase of *10 per cent* in the electrical intensity of the nerve

impulse reaching the synaptic group led to an increase of *ten times* in the number of neurons that were effectively engaged and were stimulated to respond.

One interesting implication of this finding is that the mechanism of transmission at the synapse is electrical; but we don't want to go into that. Another one is that inhibition is not essentially different from excitation, as it should be on Eccles's theory; but that also is beside the point. The third implication is that these synapses are not acting digitally. If the situation at the synapse is such that a small variation in the incoming impulse, a 10 per cent fluctuation in one quantity associated with it, can determine whether one or five or ten or no synaptic units fire, the action is more nearly analogical and continuous than digital. The factor of safety is close to one, rather than the high value needed for true all-or-none discontinuity. Small variations in a nerve fiber, well within the physiological range, can determine whether or not a given impulse is effective.

Although it remains true that nerve impulses are atomic in character and that they move or don't move, I think it dangerous to go on from there and conclude that the functioning of the nervous system can be expressed essentially in terms of digital mechanisms of the all-or-none behavior of the units in the system and, particularly, of their connections. That does not for one moment mean that I don't believe digital functions are present, that nerve nets operate, or that the analysis of the properties of such nets is going to be useful. I am certain all of these are very important. I *am* saying that if we focus our attention too exclusively on the atomic aspects of the nervous system, we are likely to leave out an at least equally and perhaps more important aspect of the mechanisms of neural functioning.

I promised not to take over half an hour, therefore I shall stop at this point. Later I may say something about the several difficulties that arise in regarding the alpha rhythm as a scanning device to resolve problems of perception, and the other difficulties in resolving the problems of memory by recourse to reverberating circuits. I should like, however, to ask one question of the group. Do any of you know of definite experimental evidence that the reverberant circuits in the nervous system, which we all accept and use freely in our explanations, do actually exist? At least at the microlevel I can think of none. There is evidence, and it is quite conclusive, of long returning loops from one part of the nervous system to another; but if there is really decisive proof of interneuron circuits running round and round in a small area I hope someone will present it.

Wiener: May I refer to the *Life* and *Time* articles? I have not been able to prevent these reports, but I have tried to make the publications exercise restraint. I still do not believe that the use of the word "thinking" in them is entirely to be reprehended. I do not maintain for a moment that the detailed operation of the machine is too closely similar to the operation of the nervous system, but I do want to say that I am equally convinced, as I have said formerly and as I say more explicitly this morning, that the action of the nervous system is not purely digital. Processes like learning, and so forth, seem to me to involve what I spoke of last year, at least the possibility of "to whom it may concern" messages, messages that are not strictly channeled, that are probably hormonal. While I spoke of them as very possibly chemical, I don't want to exclude the possibility of their being to some extent nervous. I am definitely sure that they are. Where I think the working of the nervous system is at least digital is exactly where the speaker has said it is least digital; namely, in synaptic thresholds. I believe that the channeling of messages in the nervous system is extremely important. The nervous system is certainly not just a vague means of merely spreading messages in which the channels have nothing important to do. The channels are very important in the nervous system, but I think it is also clear that the determination of the thresholds of the synapse is something that is variable where we have no evidence at all of the principal factor, that is, a channeled factor.

I think that the freedom of constructing machines which are in part digital and in part analogical is a freedom which I profoundly believe to exist in the nervous system, and it represents, on the other hand, with humanly made machines, possibilities which we should take advantage of in the construction of the automaton. I have also felt that the computing machine, which has been an extremely important factor in the study of nervous transmission, is the best machine for the study of that type of behavior at present. It is not the numerical side of these computing machines that is most important for the nervous system, but the logical side of the digital machines. I feel that the machines we build in the future for a great many purposes should take advantage of nondigital ways of modifying the threshold of digital machines. I do not see any reasonable explanation for the learning process which does not take advantage of these things. In other words, I do not feel that there is the sharp antagonism between the different groups which appears on the surface. I believe we

have taken an important existing factor and studied it, but I see absolutely no reason not to believe that these other factors are present.

McCulloch: May I add one thing? I know that Ralph Gerard feels that it is perfectly certain that the alpha rhythm of the cortex is not analyzable into the responses of small components, that is, that it is not analyzable into a distribution of nervous impulses. I don't know that the evidence for his view is clearer than the evidence of a microscopic circuit actually reverberating. There are many cases in which we know of anatomical closed paths. To my mind it is quite conceivable that the alpha rhythm, as we record it, is nothing but an envelope of disturbances proceeding, let us say, over fine axonal ramifications and fine dendritic ramifications. The individual impulses under those circumstances would be below the noise level of our instruments for the most part. I don't see how this question can as yet be settled.

As to whether anyone has recorded the activity of a small reverberating circuit, I think the question can be answered most easily. If you look at various interpretations that have been put on the work of Lorente de No on the oculomotor system, where the question was first proposed, you have either to suppose that reverberation occurs within individual neurons in some way or that it occurs in a closed loop of those neurons. There does not seem to be a third possibility. I don't know if that makes too much difference to the question of whether or not a system is digital. The evidence in question is evidence from microelectrodes, which are only semimicro, placed in the oculomotor system at a time when a nystagmus has started up. There is a sequence of impulses, first from one group, then from another group of neurons, then back from the first, corresponding to the slow motion of the eyes and then a snap back. This persists for minutes in some cases after the end of the excitation. The only question is whether you are dealing with repetitive activity by the individual components or with a circular path going from component to component. You have to suppose that you have a reverberant process either within the individual components or else between them. It does not seem to me in either case that the question of whether or not it is digital is raised.

Von Neumann: I should like to formulate a "caveat." I certainly agree with the ideas that Professor Gerard expressed, but there seems to me to be a need of circumscribing some of the terms more precisely. It is very plausible, indeed, that the *underlying* mechanism of the nervous system may be best, although some-

what loosely, described as an analogical mechanism. An example from a different field which, however, should not be taken as implying too close a comparison, is this: an electrical computing machine is based on an electric current, which is an analogical concept. A detailed analysis of how a responding elementary unit of the machine (a vacuum tube or an electromechanical relay) stimulates another such unit, which is directly connected to it, shows that this transition of stimuli is a continuous transition. Similarly, between the state of the nerve cell with no message in it and the state of the cell with a message in it, there is a transition, which we like to treat conceptually as a sudden snapping; but in reality there are many intermediate shadings of stages between these two states, which exist only transiently and for short times, but which nevertheless exist. Thus, both for the man-made artifact as well as for the natural organ, which are supposed to exercise discrete switching actions, these "discrete actions" are in reality simulated on the background of continuous processes. The decisive property of a switching organ is that it is almost always found in one or the other of its two extreme discrete states, and spends only very little time transiently in the intermediate states that form the connecting continuum. Thus there is a combination of relatively fixed behavior first, then a rapid transition, then again a relatively fixed, though different, behavior. It is the combination and organization of a multiplicity of such organs which then produce digital behavior. To restate: the organs that we call digital are, in reality, continuous, but the main aspects of their behavior are rather indifferent to limited variations of the input stimuli. This requires in all cases some amplifying property in the organ, although the corresponding amplification factor is not always a very great one. All such organs must be suited to be connected to each other in large numbers, pyramided. Thus the question regarding the continuous or digital character relates to the main functional traits of large, reasonably self-contained parts of the entire organ, and it can only be decided by investigating the manner in which the typical functions are performed by larger segments of the organism, and not by analyzing the continuous functioning of parts of a unit or that of a single unit apart from its normal connections and its normal mode of operation.

It seems to me that we do not know at this moment to what extent coded messages are used in the nervous system. It certainly appears that other types of messages are used, too; hormonal messages, which have a "continuum" and not a "coded" character, play an important role and go to all parts of the body. Apart

from individual messages, certain sequences of messages might also have a coded character. It would also seem that the coded messages go through definite specialized pathways, while the hormonal continuous messages are normally messages at large. In any case, there seem to be very intricate interactions between these different systems. The last question that arises in this context is whether any of the coded ways in which messages are sent operate in any manner similar to our digital system. If I understand the evidence correctly, it is nonexistent in this regard.

Gerard: I agree.

Von Neumann: For neural messages transmitted by sequences of impulses, as far as we can localize the state of the transmitted information at all, it is encoded in the time rate of these impulses. If this is all there is to it, then it is a very imperfect digital system. As far as I know, however, nobody has so far investigated the next plausible vehicle of information: the correlations and time relationships that may exist between trains of impulses that pass through several neural channels concurrently. Therefore I do not think that one can claim to know anything conclusive about this subject at this moment. In the same sense, all statements regarding reverberating circuits, feedbacks which may be critical and are at or beyond the verge of oscillation under various conditions of observation, and the like, seem to me premature. In addition, even if they were valid, they would only apply to rather small parts of the total system.

Wiener: May I speak of the real distinction between the digital and the analogical situation? This is a comment on what Professor Von Neumann has said. Suppose that we take an ordinary slide rule. In the ordinary slide rule we have to get the precise position of the slider to give us a number. There is nothing to hold the slider in position. However, if we put little granulations in the slide rule and if we push it beyond one, it would have to slip into the next one. The moment we do that, we introduce a digital element. In other words, the digital element lies in the fact that the things to which we are referring are not precise positions but fields of attraction which impinge upon one another so that the field where there is any substantial indetermination as to whether the thing goes to one or the other is as small as possible. I will illustrate that by tossing a coin. Actually, if I toss a coin there is every possible position for the landing of the coin, a certain region where the coin stands on edge and one where it does not. That is the thing which makes the coin essentially a digital possibility. The

dynamic probability of the coin standing on edge is very small. In other words, we convert; in every analogical system we have a certain region that corresponds to a number in one way or another. In the digital systems these are made so that they consist of fields of attraction. We try to make the regions corresponding to the number, corresponding to the fields of attraction with indeterminate regions, as small as possible in between them so that the particle will develop itself in one position or another.

Gerard: May I pick up both of those comments and again say what I think the important point I was making to be? It is not in disagreement, of course, with either of the comments, but deals with the actual character of the synaptic mechanism. This is organized contrary to the assumption we have all been making, that it behaves discontinuously and would land, like the coin, on one side or the other; that the nerve impulse is clearly set up or is clearly not set up. Actually, there are gradations, as in non-Aristotelian logic, where a proposition can have shades of truth and falsehood.

Von Neumann: I should like to submit that the following is an acceptable equivalent of what you are saying: There has been a strong temptation to view the neuron as an elementary unit, in the sense in which computing elements, such as electromechanical relays or vacuum tubes, are being used within a computing machine. The entire behavior of a neuron can then be described by a few simple rules regulating the relationship between a moderate number of input and output stimuli. The available evidence, however, is not in favor of this. The individual neuron is probably already a rather complicated subunit, and a complete characterization of its response to stimuli, or, more precisely, to systems of stimuli, is a quite involved affair. There are some indications that one important trait among those that determine this response has rather loose and continuous characteristics; it is something like a general level of excitation. This is quite plausible a priori, especially for neurons which have many thousands of synapses on their surface, that is, many thousands of inputs. However, this does not exclude the possibility that there may be other important relations within the system of input stimuli, which determine other parts of the response, and that can be best described as coded relations between individual stimuli, or between intensity levels of various subgroups of stimuli.

Gerard: There may be coding factors involved.

McCulloch: May I interject some remarks that may help Von Neumann? In this case the motor neuron is the place on which

you get the greatest convergence of dissimilar signals, that is, signals from dissimilar sources. It is the point at which it matters least which neuron fires, because the muscle will add tensions. Consequently, if it is motor neuron A rather than motor neuron B in a given pool, you are all right. All you need to do is to determine roughly the number, and you will determine roughly the amplitude of that contraction of the muscle or the force of the contraction. When you go to input channels or ascending channels, you usually do not find one of these large fieldish types of organizations of termini but a tight grip of one neuron on another, so at the most either one or two contemporaneous impulses will fire it. If you look at the curve, let us say, for facilitation for the cells of the column of Clark, whose axons go up to the cerebellum, you don't find the motor-neuronlike performance but rather something that goes up very rapidly to a totality at a given number of fibers responding for increasing numbers of afferent fibers excited in a given muscle nerve.

Gerard: That is right. That is the case I mentioned with a factor of safety of more than one. But when you get up to the top brain again, we don't know which or how much of each kind of synapse is there.

Von Neumann: Isn't this the critical question: Which of these principles of organizations exits in the brain? We know very little about this.

McCulloch: We know much about it in some instances. I don't want to go into it now, but there are cases where the time has to be a matter of approximately 30 microseconds between impulses coming from two ears. That is rather an exact requirement of time that is precomputed before it is sent up to the cortex. Anything else of that sort may be done in a very small region of the brain stem or in the nerves as they come in. From there on, relayed impulses cannot possibly preserve phase relations accurately enough. Suppose you have sound impinging upon your ears. Thirty microseconds' difference between the time of the impulse starting in two ears is sufficient to give you direction. Is 30 microseconds correct?

Stroud: Right. For sharp transients a temporal difference of 30 microseconds is quite sufficient for you to get the bearing of the source of sound. This was a very old experiment performed in the last century. We have done it over and over again, and it always comes out 30 microseconds for a sharp transient and about 70 microseconds for rather smooth tone.

Von Neumann: This may nevertheless be analogical.

Wiener: It is an analogical mechanism that functions.

McCulloch: How is the mechanism going to transmit its information into other portions of the nervous system? A single click will do it, won't it?

Stroud: Yes, but in the sense that the center that is receiving it is receiving it over a very large number of neurons.

McCulloch: Oh, yes.

Stroud: Which have origins which are quite close together. I believe there is some evidence that as the impulse travels up the tympani there is a sort of compensatory lag in the neurons themselves which tends to make all arrive at the central point at about the same time.

Licklider: There is a suggestion.

Von Neumann: Many mechanisms exist which will tell you whether the distance in time of two consecutive systems is of the order of tenths of microseconds. You have to transform this into some statement of an intensity. This statement can then be transmitted at leisure.

McCulloch: Right.

Fremont-Smith: May I say a word here? It seems to me that there are a couple of points that could be made. Professor Gerard spoke about permanently valid conclusions. Of course I think we all agree that there is none, and that it is the basic system of science that all conclusions are impermanently valid. Similarly, the question of prematurity is relative. All statements are premature, but some of them are very much more so than others. When Dr. Von Neumann spoke about the lack of the atomic nature of the neuron, the lack of complete discreteness, it occurred to me that that also now enters our concept of the atom. Isn't it true that we have an entirely different concept?

Von Neumann: Forgive me.

Fremont-Smith: Am I wrong?

Von Neumann: No.

Fremont-Smith: We have a very different viewpoint of atomicity than we used to have. The nearer we approach knowledge on any topic, the more the concept of relativity has to be considered.

Von Neumann: What I meant was something less sophisticated.

Fremont-Smith: Correct me and put me in place if need be, but isn't it true that we are discussing the question of sameness and differences, and that if you add the words "with respect to," then part of the difficulty disappears? If you specify in what respect they are the same or are units, and in what respect they are merging—

Von Neumann: I mean the very practical operating question of whether in attempting to describe the function of the nervous system you reach simple pictures by assuming that the nerves are elementary units which are described simply, or whether it is preferable to assume that they (or that some of them, or the majority of them) are large distribution centers.

Fremont-Smith: With respect to what?

McCulloch: Behavior of the nervous system.

Fremont-Smith: Both might be preferable because behavior of the nervous system is multifold and in some respects it might be preferable to describe it in one way.

Pitts: It is possible to make very relevant statements on this particular question, because I believe that the part of the nervous system that Professor Gerard is talking about is precisely one where I believe there is reason for supposing that the relation between the two possible ways of describing its behavior should differ from the results to be expected from it. That is to say, in the lower level in the spinal cord midbrain, where primarily we are concerned with the mechanisms for maintenance of posture and the carrying on of motion, where we have to deal with continuous dynamic advance, it is necessary for the system to act as if it were analogous in the sense of having its ultimate input continuously variable, or variable as the output, no matter what happens in the lower levels. From what we know, the toes have a wasteful process; namely, the process does not code at any point. Certainly it does not on the simplest reflexes. At least it represents the intensity of muscle stretch or tension on muscle simply by the proportion of the total number of neurons which come from that source and which respond in this particular way. You have, particularly, the inverse phenomena in which it is wise as a simplification to describe a continuous variable by a discrete one by simply classifying its values into two classes. Here it is much more convenient to describe the behavior of a large collection of dyadic variables by simply describing their sum in the sense of giving all the really important information. In this particular case, that describes the nervous system perfectly well. I should consider it extremely unlikely a priori, in all parts of the nervous system that Professor Gerard was describing in particular, and certainly in the spinal cord, that there was any coding in significant degree except in the sense of one-to-one pathways upward.

Von Neumann: Is the evidence really cogent?

Pitts: This is perfectly good evidence. The mechanisms for maintenance of posture and motion, the reflexes, are operated on

an analogical basis which is constructed by summing digital elements.

Von Neumann: What is the evidence?

Gerard: That is a very good point and one well worth our consideration.

Pitts: The only way to get muscle contraction in different degrees is by exciting different portions of the neurons going to this.

Gerard: May I finish? The suggestion that perhaps—

Pitts: That is the last place we should expect to find coding mechanisms.

Gerard: One should look for different kinds of neural mechanisms in the cerebrum. But the fact is that so far our thinking about what goes on in the cerebrum has been predicated overwhelmingly upon the factual knowledge we have gained of other parts of the nervous system; and this does suggest some concrete experimentation. One should go after the cerebrum now and see if some of the behaviors which do hold for the cord are not present in the cerebrum. Such results would be very illuminating.

McCulloch: Hutchinson is next.

Hutchinson: I don't know whether I am injecting something frivolous or not, but if one takes the phylogenetic standpoint, starting from unicellular organisms and going upward to the vertebrates, it would seem a very extraordinary thing for the brain to evolve as a purely digital machine. It is likely to be digital on an analogical basis; and I think that where the analogical properties appear to crop out, they are very likely rather primitive. If you want to keep it digital, you must have an intracellular digital setup of the kind that has been suggested.

Pitts: My exact point. I suggested behaving like an analogical division on a digital basis, but it is perfectly true that the intracellular is behaving on a digital basis by analogical means. I think the digital mechanism was introduced later in the phylogenetic series, probably for the purpose of handling larger quantities of information.

Bateson: It would be a good thing to tidy up our vocabulary. We have the word "analogical," which is opposed to the word "digital." We also have the word "continuous," which is opposed to the word "discontinuous." And there is the word "coding," which is obscure to me. First of all, as I understand the sense in which "analogical" was introduced to this group by Dr. Von Neumann, a model plane in a wind tunnel would be an "analogical" device for making calculations about a real plane in the wind. Is that correct?

Wiener: Correct.

Von Neumann: It is correct.

Bateson: It seems to me that the analogical model might be continuous or discontinuous in its function.

Von Neumann: Is is very difficult to give precise definitions of this, although it has been tried repeatedly. Present use of the words "analogical" and "digital" in science is not completely uniform.

McCulloch: That is the trouble. Would you redefine it for him? I want to make that as crystal clear as we can.

Von Neumann: The wind tunnel, in attempting to determine forces of a particular kind upon an analogical model airplane, presupposes similarity in almost all details. It is quite otherwise for the differential analyzer, which is supposed to calculate the trajectory of a projectile. The parts of the analyzer look entirely different from any parts of the projectile. It is, nevertheless, analogical because the physical quantities of the true process are represented by continuous variables within the analyzer, for example, by coordinates or by velocity components of various parts, or by electrical potentials or current intensities, and so forth. This is clearly a much more sophisticated connection between the true physical process and its symbolization within the computing machine than the mere "scaling" in wind tunnels. All these devices have, nevertheless, a common trait: certain physical quantities that have continuous motions are represented by similarly continuous processes within the computing machine. Interrelationships are entirely different in a digital model.

To conclude, one must say that in almost all parts of physics the underlying reality is analogical, that is, the true physical variables are in almost all cases continuous, or equivalent to continuous descriptions. The digital procedure is usually a human artifact for the sake of description. Digital models, digital descriptions arise by treating quantities, some of which or all of which are continuous, by combinations of quantities of which each has only a small number of stable (and hence discrete) states—usually two or three—and where one tries to avoid intermediate states.

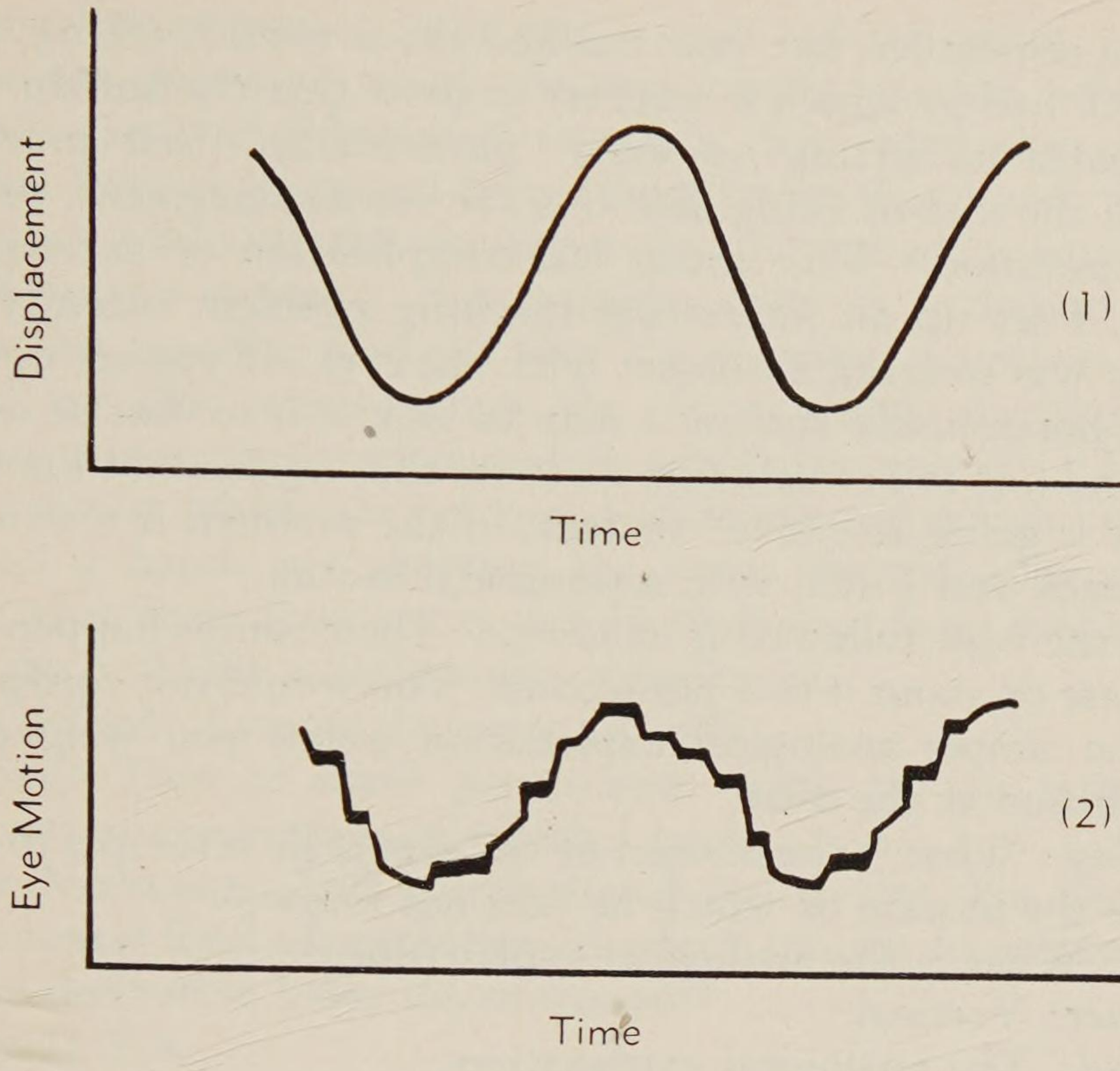
Wiener: I should like to say something that bears directly on this from the standpoint of the engineering problem; I am considering the question of automatic factoring in the automatic factory. Probably the best internal brains we can use for it will be digital models. I would not say purely digital, but what we now call digital computing machines. To work a chemical factory, for instance, we should have separate organ effectors. These separate organs will involve the stage in which analogical quantities

are converted into digital. This organ will read the thermometer, will have to convert this reading to, say, rotation of a shaft and then into a unit digit, a tenth digit, a hundredth digit, and so forth, for the machine to be able to take it up. Finally, at the end of the machine we will have an effector. This effector will be something that will turn a tap, let us say. This turning of the tap would be done by some machine which will take a series of digits, one of which will determine the place of the thing to within one-tenth, within one-hundredth, and so forth.

The point that I want to make is that the digital machine for analogical purposes is something that we are going to have to contemplate for the engineering applications of this idea. There is no reason to suppose that it does not happen in human-animal applications as well.

McCulloch: Stroud is next. Say a word about following a curve, will you?

Stroud: Dr. Gerard's anxiety about the fact that the general *Time*-reading public wants to change to some single, absolute explanatory principle makes us feel very uncomfortable for ever having said any thing about it. Personally, I refuse to feel guilty about the foolish mistakes that the general public makes in its limited ability to think or in its laziness. I know of no machine which is not both analogical and digital, and I know only two workable ways of dealing with them in my thoughts. I can treat them as analogical devices, and if this is a good approximation I am happy. I can treat them as digital, and if this approximation works I am happy. The devils are generally working somewhere in between, and I cannot understand how they work accurately. I should like to illustrate. This process of going from a digital device to an analogical to a digital device can go on in vertical lattices *ad nauseam*. You begin with the rather highly digital electron, conclude the next step with the rather analogical hard vacuum tube, use it as a "flip-flop," which is primarily a digital element, and so on. When you have gone through enough stages, what you are finally dealing with depends upon function. Either of the two approximations is confusing. An ordinary amplifier, if you put a signal in it at the right level, is an analogical device. If you use too much signal, it begins to clip off, with two states, a maximum plus value and maximum minus value, and goes from one stage to the other with the greatest rapidity. If you put in too little signal, you get noise from the shot effects, part of which are quantical effects arising out of the motion of individual electrons in the circuit elements.



(1) Actual motion of the target.
(2) First solution of tracking problem.
(3) Second (velocity) solution of tracking problem.
(4) Third (acceleration and velocity) solution.

FIGURE 1

If you remember, last year I talked about some tracking devices that NRL had by which it was easy to show that the human system was capable of setting up some "guesstimates" that practically involved the idea of being able to solve for displacement, velocity, and acceleration. With much less complication of external machinery, I set up an interesting tracking problem recently. The problem was to track an object with the eyes. If you set up a spot moving horizontally and ask a man to look at it so that he sees it as a spot and not as a streak, you discover that he does not know how the spot is going to move. In fact, in the problem it was moving slowly back and forth, with a sinusoidal motion.

First the man followed it in jumps. These jumps happen to fall at the rate of about 4 to 6 per second. You would not confuse that with the simple analogical explanation unless you were permanently biased at the start.

Bigelow: What is the motion of the eyes as he attempts to follow the spot the motion of which he does not know—

Pitts: What is the analogical explanation?

Wiener: Vertical.

Stroud: The analogical explanation.

Pitts: Is it a digital explanation?

Stroud: I am more inclined to the notion of analogical processes as the statistics of a large number of quantical events. You do not say that the man's eyes move continuously in pursuing a spot which is roughly following a course like that.

Pitts: By analogical and digital do you mean continuous and discontinuous? Many people mean merely that.

Stroud: You treat them as if these transition states did not exist. It is quite a good way of treating them. A little later this chap catches on to the solution of this simple sinusoidal motion and he begins to put in at the same rate little sections of line which are sloped, achieving a much closer approximation of the movement of the object. Here the eye is moving simply and continuously along a sequence of positions. The fits are not good, and you can still see the new little points at which the new constants are taken out and discontinuously posted to a gadget which is now quite obviously working as an analogical device.

Bigelow: Is the head fixed?

Stroud: The head is fixed. It is simple eye motion taken with a Dodge (eye-movement camera), with a cathode-ray-tube spot as the target. With still more learning he gets to put the curvature in here, until in the final process you have an almost perfect tracing of the ½ cycle curve, with a wobbly noise along it. You have

to use a fine source of light and a good optical system to differentiate the points at which the very minor changes (changes, if you like, in the differential equation describing this sinusoidal motion) have to be posted from time to time. For purely practical purposes I find that the statement that Pitts made is entirely true for all output systems. You consider them as analogical devices, and in this case you find out that the necessary constants that are needed for this operation are the things which are likely to be computed in groups and posted at particular intervals. There are other circuits which are not quite so simple and which involve an eye and a hand, yet showing the same essential characteristics except that their frequencies run in the range from two to three. They are probably a little more complicated.

McCulloch: Two or three per second?

Stroud: Two or three per second. This, by the way, is the fastest set of corrections of which I know.

Von Neumann: Did I understand you to say that there is an experimental level of resolution in which motion looks continuous but the derivative looks discontinuous?

Stroud: Yes.

Von Neumann: Is it published?

Stroud: No. We want to find out more about it. Besides, I get into trouble every time I try to publish something. I don't know how to do it. I try to get out of it and I also want to pursue the subject in greater detail for purely practical reasons. I must confess that I always keep the Navy's business in mind, and this thing has some immediate practical applications that I am very much interested in.

McCulloch: You are dealing with something that is possible, the correction every fifth of a second or third of a second?

Stroud: That is correct. In this system you can detect the difference because it is a vastly overpowered system and for some purposes can be considered independent of mass; its sharp jumps are very marked and easy to see.

Bigelow: The eye cannot move in infinite acceleration. What is the slope?

Stroud: I wish I had measured them. They are a full decimal order of magnitude greater than any of the slopes I have been imposing upon the eye as motion.

McCulloch: The eye can move far faster.

Stroud: As soon as I set up oscillatory motion which will get anything, even one-tenth or one-hundredth of the slope of the saccadic movements, I find that I have overcrowded the computer

that tries to post the computations and that it breaks down. It simply cannot follow, therefore the eye no longer attempts to solve the problem of motion. The saccadic motions are a whole order of motion faster than any organized effort to follow the motions of a target.

Licklider: I am reverting to Gerard's original problem. I want to say two things. The discussion first showed that there was general analogical continuous substratum to digital processes. Stroud brought out a whole hierarchy of analogues, and so forth. One of the extremely interesting points arrived at, not by getting too many hierarchies but by knowing that many of the things are of great interest to neurophysiologists and psychologists, too, was that there are processes in which we obviously have pretty much all or none of the impulses working as our basic elements. There are so many nerve fibers in a bundle, 25,000 in the auditory, a million in the optic nerve, that it seems inconceivable that each element of detail is important. We have rather the mean behavior of a system, including many parts, each part being perhaps digital. I want to see if I can get the attention back on that level of the problem. I also want to say that I think the time has come when some of us must really know what the distinction between analogical and digital is, besides that of continuous and discrete. Analogue and digit are not words that the ordinary person, even the intelligent person, holds up and says: These are opposite. I can conceive of digital system which is the digital process and the analogue of another digital process, and therefore really analogical. I need clarification. I wonder what the distinction is.

McCulloch: May we start again with the question that was on the floor, that is, the question on continuous and discrete?

Licklider: The question is simply this: We have been using the words "analogical" and "digital" to describe computers. To a lay man analogical and digital are not opposites in any very clear sense. We understand the distinction between continuous and discontinuous or between continuous and discrete. We understand roughly what an analogy is, but we would like explained to us here, to several of us and to many on the outside, in what sense these words are used in reference to the nervous system.

McCulloch: Dr. Pitts, will you tackle it?

Pitts: It strikes me, first of all, that we should speak of physical systems in general, not computers, because a computer is merely a special kind of machine assembled for a special purpose so that we can watch it and derive conclusions from it that we would not be able, perhaps, to find out for ourselves. The physical system in

general is a complex of variables which can be continuous or discrete and connected by various dynamic relations which cause the variables to change as time changes, a complex which can be altered and affected by external inputs. I say the variables can be either discontinuous or continuous. They are usually continuous in the physical systems, with the possible exception of electron spins, and so forth. I am sure we don't have to consider that here. In certain special cases, among which computing machines in the brain come most obviously into review, it is often possible to make a simplification when you try to calculate, account for, or consider the behavior of the system. Suppose that of the variables which compose a physical system and which are bound by various relations to various other variables, one is continuous; suppose that its effect on the change in all the other variables and the subsequent history of the system depends upon a single fact; namely, upon whether its values which may fall within a continuous range belong to one class or another class, or say, are less than B or no greater than B, and that that factor alone makes the difference to the rest of the system with respect to that variable, all you can say about it, all that matters for you is whether that partciular variable lies within one range or another range. Well, then, as far as we are concerned, in describing the system we can replace that variable by another one, not continuous at all but only capable of two values, say, zero in one range or zero in another, in any place you please. We can replace it by a discrete value whose knowledge would tell us all that is really important about the knowledge of that second variable from the point of view of the rest of the system. You see, whether it is possible to ignore the actual continuity of a physical variable in that sense depends upon the whole dynamic system and upon the relation between that particular variable and other ones connected with it. It does not depend upon whether it is in its own nature continuous or discrete. Therefore, continuous variables can be so ignored in the sense of affecting their neighbors only by virtue of their exceeding or not exceeding a certain quantity. The simplest example, of course, is the neuron, under certain conditions, at least. Others are the computing machines that are constructed almost wholly out of variables which are capable of finite number of states called digital computers. The ordinary desk computer is primarily an example of that kind and of the *other* kind where we try roughly to make the continuous variables ape the continuous variables of the physical system in which we are really interested. We also try to make the functional relations between them like the true laws of nature,

in which we want to calculate laws in true nature. So the whole system becomes a model in the sense of a true physical system. These are called analogical simply because there is a detailed analogy between the computed system and the computing one. Actually, the notion of digital or analogical has to do with any variable in any physical system in relation to the rest of them, that is, whether or not it may be regarded for practical purposes as a discrete variable. Simplified, I think that is the essence of the distinction.

McCulloch: Does this cover what you had in mind?

Savage: I think I might add a trifling gloss to it. The word "analogical" may suggest a little too strongly a computing device acting in analogy to the problem situation. Thus, for example, if a multiplication for a laundry bill be computed on a slide rule, the problem is purely digital. Yet the slide rule is properly called analogical precisely because it does not behave in analogy with this digital problem but rather in the essentially continuous fashion typical of an analogical computing device.

Pitts: In such a way continuity is essential.

Savage: That is right.

Wiener: Here is the important point: ordinarily in an analogical machine each digit goes down as we go along in the digits. We are performing a single measurement in which we really are adding the tens digit, the unit, the tens, thousands, and so forth, so that our smallest digit is corrupted by the error in our biggest digit. That is essentially a vicious way of handling things with precision. In the digital machine we make a deliberate effort to have a measurement without any particular degree of error, one which is a Yes or No measurement. In this particular case the probability of error in a particular measurement belongs to that measurement only and is not carried over to one of the others. That probability then can be reduced to a very low quantity. It is like the coin standing on edge. We know that it is either one thing or another. Now the point of the digital machine is that we get our precision of workmanship by extremely close estimates of each digit by itself and not in a situation in which a mismeasurement of one very large digit will corrupt a small digit together with it. To say that a thing is digital is to say that we use this technique of accuracy in the machine instead of the technique of accuracy which consists in extreme precision of the measurement, but in which the error of a big measurement is linearly combined with that of the small measurement and corrupts it.

Bigelow: I should like to say a few words on a very visceral

level. It is very easy to say things that are true about these words, but it is very difficult to say something which completely conveys the picture. The picture conveyed will be complete in the case of the mathematician, but I think that somebody ought to make the very platitudinous remark that it is impossible to conceive of a digital notion unless you have as a reference the notion of a continuous process by which you are defining your digit; that is to say, the slide rule has continuous length and it has on it numbers which are digital.

Wiener: Certainly.

Bigelow: The statement that "something is digital" implies that you have as a referent something else which is continuous. The second point I wish to make is that in the actual process of perceiving it is my frequent observation that the continuous property of things as they appear to our sensory organs constitutes an experience between one class of machine or one class of observing device and the outside world. Human beings see light as a continuous phenomenon, although they may be ignorant of its wave or composite nature. Actually, some evidence may indicate that you see a continuous phenomenon, but you may refer this to a digital system which is entirely artificial, but which you yourself produce as a means of interpreting a phenomenon in which you are not otherwise satisfied with your own methods of interpretation.

Thirdly, it does not seem to me enough to describe a digital process as being one in which there are two or more discrete levels in which you are only interested in saying whether you are at level *A* or level *B*. I think it is essential to point out that this involves a forbidden ground in between and an agreement never to assign any value whatsoever to that forbidden ground, with a few caveats on the side.

Finally, I think most people who think about digital machines also have in mind a definition of "coded." What is meant by the word "coded," which so far has not been cleared up in this meeting? I don't know that I can define it, but I believe there is this element to it: if you have two or more levels of a quantity, such as a voltage, or if you have two or more periods in time, and if you take the same event and assign to it different numerical values, for example, then you are in some sense coding. You are coding with reference to levels or with reference to segments in time sets. For example, a binary pip, at one moment in time in the computing machine, might equal 7 or 8. At another time it might equal 1. The only way you know 1 in one case, or 7 or 8 in another case, is by referring again to some outside reference system, so that

coding means in effect a technique aimed at gaining increased efficiency by having a simple signal possess different values when referred to a different referent.

Wiener: Yes.

McCulloch: You are next, Dr. Pitts.

Pitts: There is a certain difference between the brain and the computing machine, because in the brain there is not the possibility of variation that there is in the actual construction of computers. In the case of the brain you cannot alter the meaning of the signal coming in along a fiber in the optic nerve. It always means that a certain amount of light has struck a certain place.

Bigelow: Is that true?

Pitts: It is absolutely true.

Wiener: In combination with another signal it may mean a different thing.

Pitts: Connected with that particular place permanently.

Bigelow: If I see a green light on the corner of the street and I am driving a car, it certainly means one thing. If the light is at another place, it means something else.

Pitts: If a signal comes along, a given fiber is struck. A light strikes a given point in the retina under given conditions and you cannot contemplate change in the wiring, which is implicit at levels of that sort to increased efficiency.

McCulloch: In foveal regions they are soldered one to one.

Savage: Is Licklider satisfied with these answers to his questions?

Licklider: Is it then true that the word "analogues" applied to the context of the computer's brains, is not a very well-chosen word; that we can do quite well if we stick to the terms "discrete" and "continuous," and that when we talk about analogy we should use the ordinary word "analogy" to mean that we are trying to get substitution?

Bigelow: I should object to "substitution."

Licklider: I mean the object we are trying to compute, using "analogue" in the way it is used by most people and not in the way used by the computing machine.

Wiener: I think perhaps "discretely coded" would be good words for "digital."

Licklider: I think we could communicate better.

Savage: We have had this dichotomy with us for four or five years, Mr. Licklider. I think the word has worked fairly well on the whole. Most of us have been familiar with its meaning. There would be some friction for most of us in changing it now.

McCulloch: I should be happy to abandon the word except that

I don't see how any simple word like "continuous," as opposed to "discrete," would take the place of it. I think one would have to say, as Wiener suggested, "discretely coded" or "continuously coded." I think that is the chief obstacle.

Licklider: I did not think I would be successful in getting machine computers to use the word "conscious." That has been around a long time and now has to have a group to find out what that means.

Fremont-Smith: And they didn't.

Teuber: When Dr. Gerard spoke, he seemed to try to take us all the way back to the beginning. To me, he conveyed the idea that in the peripheral nervous system, we do have something that can be described—on some low level at least—in terms of discrete or digital functioning. No matter what we do to the nerve, we either set off a spike or we don't. It has been known of course, all along, that we can do all kinds of things short of setting off the spike—all sorts of things between firing and not firing, but the question is: Does it matter? We can raise the local potential by manipulating the environment of nerves. Still, whether we set off a spike is a matter of yes or no, zero or one.

Now the point is made by Dr. Gerard, if I understood him at all, was that the situation in the central nervous system might be quite different. Apparently, we need more than one afferent in order to set off an efferent, and we have many afferents converging on a single synapse. How many convergent afferents have to be active for transmission to occur depends upon the structure and state of the central nervous system in that region. But this again would not detract from the fact that we can only either set off the synapse or we don't. It may be all right to say that transmission in the central nervous system is not obligatory, but I don't see why this should make it impossible for us to use the "digital" analogy, if I might add to the confusion by coining this term.

To assume digital action is permissible as long as we remember that we are dealing with a model. The only justification for using the model is its heuristic value. It may turn out to be inapplicable to the central nervous system, but by finding out *why* it is inapplicable, we shall have discovered facts about the nervous system which we don't have in our hands at present.

What is it that we know now, precisely, that would make the "digital" model inapplicable? It cannot be the factor of convergence and non obligatory transmission in the central nervous system. I wished Mr. Pitts would have made that point, since this is the reasoning he has used for the past three years. I think I did

not quite understand Dr. Gerard's argument. I would rather have the discussion revert to him.

One more point: In the retina we have an interesting difference in neural structure. In the central region of the retina there is opportunity for one-to-one connections from cones through bipolars to ganglion cells: That does not mean that there are not ample opportunities for cross-talk through collaterals and horizontal cells, even in that central foveal region. Still, transmission can take place in one-to-one fashion. This is quite different from the periphery of the retina, which constitutes the bulk of the structure. There we have a tremendous amount of convergence.

McCulloch: About 200 to 1.

Teuber: Anywhere from 80 to perhaps 200 or more rods for each ganglion cell, again with reciprocal overlap in intermediate layers, and all the structural complexity characteristic of the central nervous system, as Polyak has shown. In that sense, the retina is a piece of cortex pushed out towards the outside world, rather than a peripheral end organ.

Now this anatomical difference between central and peripheral retina is there; the question is, does it make any functional sense? The usual interpretation is that optimal spatial discrimination has to be mediated by the central retina, where there is opportunity for one-to-one connections, and maximal light perception is mediated by the periphery where there is considerable convergence from rods onto ganglia; still, even if the anatomical difference were directly related to the hypothetical difference in function, both regions of the retina could work according to digital principles, as far as the firing-off of individual ganglia in the optic nerve is concerned. In sum, I didn't quite grasp Dr. Gerard's point.

Pitts: I want to chime in there to say exactly what you said, except that I should like to state in the form of a caricature what one of Dr. Gerard's arguments appeared to me to be. It may make clear to him what is disturbing me. His argument about Lloyd appeared to caricature roughly the following: we are trying to prove that the behavior of nerve systems is not completely descriptive of all-or-none impulses because Lloyd's experiment shows that the effect of an all-or-none impulse sent along given paths at the other end is altered by the all-or-none impulses that we may have sent previously along either the same or other fibers. Therefore the nervous system is not describable completely in terms of all-or-none impulses. That is the way the argument appeared to me, why it appeared to me not to have relevance. I think I must have mistaken its application.

Because presumably continuous processes are intervening at the other end, whereas all the all-or-none impulses sent along the tracts can affect the results of new ones we send in, therefore we cannot describe the behavior of the nervous system in terms of all-or-none impulses. But this does not seem to me to be cogent.

Savage: I take it your point is that digital machines behave that way, and that their response to digital stimuli does depend upon the past history of digital stimuli?

Pitts: The question whether or not there is an intervening variable makes little difference and is of no consequence.

McCulloch: I want Dr. Gerard's opinion at the conclusion of this.

Kubie: I want to consider the digital and analogical concepts at another level. Plain words lead to confusion when we do not know what we are trying to use them for. I cannot conceive of any measuring device, whether a machine nor not, that is not ultimately digital. If you measure, you count. If you are going to count, you must be able to recognize identical discrete units. But in science we often try to measure where we cannot even identify the units. Here we have to work by analogy.

Consider the clinical thermometer. Is a clinical thermometer a digital measuring machine or is it analogical? It is both; because if you think simply in terms of the temperature of the aperture into which you insert it, it is digital. If you think one step beyond that, in terms of the internal processes about which you are going to make some deductions on the basis of estimated quantitative changes in *un*isolated units, then it is analogical. Therefore, whether a machine is digital or analogical depends on the use to which the machine is put. As a measuring device, however, a machine must always be digital.

The reason this is so important to me is that in all our theories of human behavior the word "dynamic" implies a capacity to measure in an area where we cannot make the distinction between analogical and digital at all. I will have occasion to return to this later.

Klüver: I wonder what Dr. Gerard would have said if he had discussed these problems, not as a physiologist, but as a biochemist. For instance, does the Krebs carboxylic acid cycle involve analogical or digital mechanisms? In fact, is it particularly fruitful to consider it in terms of such a dichotomy? That is one point.

There is another point which might be stressed in connection with Dr. Teuber's remarks on the retina. Again and again the attempt has been made to determine the functional meaning of

the high degree of anatomical differentiation in the visual system or the functional significance of the point by point representation of the peripheral sensory surface in the cerebral cortex. Lashley once suggested that the difference between anatomical systems with little or no subordinate localization and systems with a high degree of internal specificity is related to a difference between nervous mechanisms regulating intensity of response and mechanisms involved in the regulation of spatial orientation [Lashley, K. S.: Functional determinants of cerebral localization. *Arch. Neurol. and Psychiat.*, **38**, *371* (1937)]. Many investigators have tried to relate the facts of spatial differentiation in the anatomy of the visual system to facts of visual functioning, for example, to the spatial differences of visual stimuli. More recently, in an anatomical study of thalamocortical connections, Lashley found that the differentiation within the anterior thalamic nuclei and their cortical fields was as precise as in other sensory systems. Since olfactory experience is lacking in spatial character, he found it difficult even to imagine an attribute of odor represented by an accurate detailed spatial reproduction of the surface of the olfactory bulb on the cortex [Lashley, K.S.: Thalamocortical connections of rat's brain. *J. Comp. Neurol.*, **75**, *67* (1941)]. If such a topographical arrangement is functionally meaningless in the olfactory system—so runs Lashley's argument—there is no reason to give it a functional interpretation in the visual or other systems unless that is done on nonanatomical grounds. Topographical arrangements may very well be "accidents" of the mechanism of embryonic development. To be sure, the situation is even more complex, since voices have recently been raised that the anterior nuclei of the thalamus are not concerned at all in mediating olfactory function and that they represent structures in an anatomical circuit concerned with emotions.

To return to the visual system, it may be argued that the eye and the subcortical and cortical structures of the visual sector of the central nervous system represent more than merely delightful opportunities for anatomical and electrophysiological researches. I may be forgiven for mentioning the old-fashioned but nowadays apparently somewhat radical idea that these structures have something to do with *seeing*. You would think that any investigator who really tries to relate brain mechanisms to visual behavior would be seriously concerned with the question of how nervous mechanisms as they present themselves on the basis of electrophysiological data are related to mechanisms as they are uncovered in investigating actual processes of seeing. Unfortunately, the visual

sector of the central nervous system appears to be an excellent device for achieving a remarkable degree of independence from the intensity and energy fluctuations on the retina. It is apparently for this reason that some years ago I was unable to find any evidence for electrophysiological correlates even of "primitive" visual functions operative in actual seeing [Klüver, H.: Functional significance of geniculo-striate system. *Biol. Symposia.*, 7, 253 (1942)].

This brings me to a third point. It seems to me that the discussion of the problem whether nervous system activity involves digital or analogical functioning or both has been chiefly concerned with a nervous system constructed by electrophysiologists and anatomists. I happen to be interested in the nervous system that is actually operative in behavior, let us say, in seeing. I find then, for instance, such facts as that a given line suddenly appears perceptually longer and that this increase in perceptual length is—and in another situation is not—associated with an increase in objective length. Or I find that increasing the length of a certain line transforms what appeared to me as chaos into the face of a devil or, if you wish, into five devils. I do not know how you propose to deal with the reality of polymorphic phenomena on a digital or an analogical basis. In fact, I do not even know how the factors governing the appearance or disintegration of even simple visual *Gestalten* are related to analogical or digital functioning or to what an extent, if any, an experimental analysis of such factors may benefit by digital or analogical models.

Teuber: May I correct one point? When I quoted Polyak's evidence on the retina, especially on the fovea, I did not mean to imply that I accepted Polyak's own interpretation of the anatomical arrangement. However, the fact remains that in the fovea we have this opportunity for discrete transmission in one-to-one fashion through the retinal layers, and from then on up, all the way up to the cortical retina, where there is orderly projection of the macula. Lashley has often made the point—and I quoted him here, about two years ago, I think—that the orderly anatomical arrangement in the visual system might be quite fortuitous, an embryological accident, so to speak. In the olfactory system there is also orderly projection; if one assumes that the anterior thalamic nuclei project olfactory activity to the cortex, then one has to be puzzled by the perfectly orderly spatial projection of these nuclei onto the cortex: What corresponds to space in olfactory experience? Lashley used this sort of argument to discredit the notion that spatial projection may have anything to do with experienced

space, and in that respect he is probably correct. Yet, orderly spatial projection might have a definite functional significance, all the same. For the olfactory system, matters begin to look different now. Since we first discussed this point, Adrian has picked up electrical activity from the exposed olfactory bulb in the rabbit, while the rabbit was stimulated with different odoriferous substances. Depending on the substance used you get an audibly different "orchestration" of the electrical activity, as picked up from the olfactory bulb and fed into a loudspeaker. These differences may depend on differences in diffusion gradients, different substances diffusing in different patterns over the entire expanse of the olfactory membrane. That means that some spatial ordering of activity may be of importance in the olfactory system; diffusion patterns may have to be mapped centrally in some fashion, even though in our own experience the result would not correspond to anything like visual space perception. Study of the structures will not lead us to the ultimate of experience; still, models constructed on the basis of present knowledge of neural structures suggest concrete hypotheses as to how function comes about. As long as the hypotheses are testable, and as long as we keep the obvious model-character of our notions in mind, these models are worth retaining till we have better ones.

Pitts: You mean position on the olfactory cortex essentially refers to position of the odoriferous substance on the olfactory membrane rather than on the quality of the odor?

Teuber: Intensity, and possibly quality, too. Rapidity and extent of diffusion over the olfactory membrane might give intensity of any one odor, but possibly different odors might give characteristically different diffusion patterns as such, or might be selectively absorbed. Different olfactory stimulations would lead to characteristically different space-time patterns of neural activity in the olfactory system.

Pitts: Space pattern?

McCulloch: Space coded information of some sort.

Teuber: Even though it does not give us a space experience.

McCulloch: That is right.

Wiener: I should expect a good deal of discrepancy between the possible coding of information in the olfactory system and actual coding, inasmuch as we are animals who are already on the downgrade as far as smell goes. Our smell sense is unquestionably largely a residual sense and cannot be expected to give us a true picture. You are lucky to find anatomical fossils and the connections there that no longer have the same physiological meaning that they had with euosmite animals.

Pitts: An animal with thirty light spots has not a good conception of visual space.

Licklider: I know which machines are called analogical and which are called digital. I don't think those terms make sufficient distinction and that is all there is to it. It won't help me to talk about differential analyzers. I know all about that.

Fremont-Smith: Good with respect to what?

Licklider: It confuses us in communication here. These names confuse people. They are bad names, and if other names communicate ideas they are good names.

Fremont-Smith: They are not good means of communication in this context.

Mead: It would help if we knew when this distinction was made in describing the machines, that is, if we knew the historical use of the term "analogical."

McCulloch: I don't know how old it is.

Wiener: I would put it at about 1940, when Bush's machine was already developed and when the rival machine, the differential analyzer, and the machines which were working on the principle of the desk machine electronically were being developed. That began to be an acute issue about 1940, and I doubt if you will find any clear distinction older than that.

McCulloch: They used to be called logical machines or analogical machines before the word "digital" appeared.

Hutchinson: Analogical, if I may use the word, the difference between the natural and real numbers, is hiding in the background all the time, but you must go back to the Greek mathematicians.

Wiener: If you want to say that in one case you are dealing with counting and in the other, with measuring, the concept of the machine goes back to the Greeks.

Hutchinson: That is a neat way of putting it.

Wiener: Yes.

Licklider: Continuous and discrete.

Gerard: May I speak now?

McCulloch: No, not now. I want to make one point here. There is no question in my mind that there are many continuous variables affecting the response of neurons. The question in my mind is different. If you will, for the moment accept as a distinction between analogical and digital, the question whether information be continuously coded or discretely coded. My question is whether these continuous variables, which are undoubtedly present in the nervous system, are conveying information or not; that

is, they may not be coding of any sort of information any more than the voltage which battery or power pack is delivering to your set, which may vary.

Pitts: Or they may be simply representing the effect of past all-or-none actions?

McCulloch: Yes.

Fremont-Smith: Or future, setting conditions for the future.

Pitts: Or they may have intervening actions precede the discrete and following one.

Bateson: There is a historic point that perhaps should be brought up; namely, that the continuous-discontinuous variable has appeared in many other places. I spent my childhood in an atmosphere of genetics in which to believe in "continuous" variations was immoral. I think there is a loading of affect around this dichotomy which is worth our considering. There was strong feeling in this room the night when Koehler talked to us and we had the battle about whether the central nervous system works discontinuously or, as Koehler maintained, by leakage between axons. The present argument seems to me to be the same battle.

Pitts: Whether better or worse, if insulating partitions were put between separate synapses.

Savage: The battle is whether that distinction is worth making or not. The most important question is that which Dr. Licklider said: Is the nomenclature confusing us, or is the nomenclature a promising one? That has always been a central issue here.

Fremont-Smith: With respect to what? It might be confusing in certain contexts and very helpful with others.

Savage: To be sure.

Wiener: That same issue did come up in the matter of genetics end; the continuous variables which were driven out with the pitchfork at one door came back. Are not characterizations as observed simply certain factors which combine with certain variables to give us the characterizations as observed? That is precisely the situation that has occurred here.

Hutchinson: However, you always use such situations in the mathematical handling of pure genetics.

Wiener: Use them both ways. The moment you begin to consider survival factors, you have to consider how these affect it.

Hutchinson: The continuous situation.

Licklider: I am afraid we disturbed the course of things by talking about this too much. We really ought to get back to Gerard's original problem. We will use the words as best we can.

McCulloch: All right, we have got about a half-hour before we stop for lunch.

Gerard: I shall try to discharge my duty to the group. Perhaps there will be some other points that will occupy us for another half-hour. Most of the comments made in the last few minutes seem to me very clearly to point up the critical issue, but I should like to go back just a bit and explain what I think has happened. Incidentally, I think this whole discussion has been extremely worth while to us even at this late date in our history.

Stroud used the example of the electron tube working on its characteristic or off its characteristic; on the characteristic one has continuous relations or an analogical behavior; off the characteristic it "flip-flops," Yes or No. Several others have pointed out that there are always these analogical factors in the functioning of any machine or system the ultimate output of which is effectively digital. Now most of the physiological evidence I gave was to the effect that in the nervous system, in the body of afferent receptors and efferent effectors, in the synapses, and perhaps even in the nerve fibers, operations are much more on the characteristic of the tube than we have usually assumed in our physiological thinking. It is operating, to use Pitts's term, in the forbidden region of the system. Now, just to the extent that that *is* where the system is operating, it is, not only by definition but also in terms of the interpretations of what it does and of the significance attributed to what it does, operating analogically.

Dr. Pitts made another critical point which was picked up by Dr. McCulloch and some of the others. He said that the essence of the digital machine was that, whatever happens inside, so far as the significance of its operation is concerned, all that matters is whether something is below or above a certain number in this category or that category. Whatever is happening as the wheels stir or the electrons shoot, what counts in terms of the functioning of the machine is its Yes or No answer. Dr. McCulloch pointed that up nicely by saying, "The question is, is coding done digitally or analogically?" My point, in emphasizing the actual functioning of the nervous system in the forbidden continuous region, is that much thinking about the nervous system and much of the theoretical interpretation of memory, learning, and many other things has been based upon its *not* functioning in this region, upon its working digitally, upon the all-or-none behavior of firing or not firing an impulse. What I am suggesting is that, although it is certainly true that impulses either do or do not fire through large

parts of the nervous system, this may not be the critical mechanism in its effective functioning. Just as has been said, in order to operate with a contiuum one breaks it down into units with which one can work; but that is just an incidental procedure, which could perhaps be avoided, and might have little relation to the ways the whole functions. So, first, we do have many continuous mechanisms operating in the nervous system and my feeling is, although I confess at once this is not established except by collateral evidence, that some of those continuous mechanisms have coding value and are critical to the functioning of the nervous system. I am further suggesting that, even though we find digital operation in the nervous system, this may not be the essential mechanism accounting for its behavior but may be incidental, to pick Teuber's nice term, to the orchestration.

I fear to use the word "*Gestalt*" at this point, let me use the term "envelope." Perhaps all through the brain, as is certainly the case in the periphery, the fact that discrete nerve impulses travel is not important. What is important is the total pattern of time intensity. No variation of the impulse, and whether messages go by discrete impulses or by some other mechanism which is not discrete, would essentially alter the total performance of the nervous system. That is stated as an extreme, and I ask how nearly it is valid. I have no doubt myself that in some cases in the nervous system the discreteness of the impulses, the digital behavior, is critical to effective functioning of the nervous system. I am also suggesting, however, that, to a much larger extent than has appeared in most of our discussions and indeed in the thinking of most people in the field, the fact that nerve impulses run discretely may be accidental. In the final determination of function, it is the total accumulation of impulses, here, there, and in the other place, which is critical. That is the initial difficulty.

Savage: One of the things Gerard's discussion has brought out is a concept which has not been given much emphasis in this group, largely because of our perhaps untoward tendency to classify all computers as digital or analogical. I refer to computational procedures in which random devices—gambling apparatus, to speak figuratively—play an essential part. As a matter of fact, such methods are currently quite fashionable in applied mathematical circles, where they are romantically referred to as the Monte Carlo method. In the nervous system there is such a multiplicity of similar elements that one can imagine that some computing is done by "games of chance" in which some of these elements are drawn upon at random, relying on statistical averages

for the appropriate effects. We have already come across this concept in our sessions, for example, in the treatment of clonus by Rosenblueth and Wiener. Statistical averages played an essential part in their hypothetical mechanism and in some of the phenomena Gerard has talked about today.

Wiener: May I say we have an example which plays right into your hands here. We are using shunt effect for analysis of the electric circuits in exactly that way. Excuse me! I want to pass that along because that was apropos.

Bigelow: A few minor points that Gerard mentioned: the possible existence of operation in the forbidden zone, needless to say, is a contradiction in somebody's terms. If a device operates in an in-between zone and if that is a meaningful behavior it seems to me one either has to throw out the term "forbidden" and admit that the zone is an acceptable one having a value, or else assume that there are as many values as you please and therefore as many zones as you please, and that therefore there is a continuum of zones, in which case the digital property really has vanished and you are talking about analogical concepts.

Gerard: Forbidden for one type of functioning.

Bigelow: It seems to me that most people who are approaching this Conference from a mathematical or machine side, as I do, would be happy to throw the following thing up in the air: What we mean by neurons are not cells as they are described in somebody's book on cell structure; we mean that the neural cell is exactly that part of the system which has the property of carrying out processes like computation, that is, the property of carrying out operations which are in fact digital. I think that actually the physicist would be willing to use this as a definition of what the nervous system is, all and everything that the system is, calling all else another system.

Gerard: I think the physiologists would be likely to say that that is just like a physicist.

Bigelow: There is one more point. As Dr. Savage has been saying so many times today, the useful aspect of the idea of the computing analogy and the computing aspect of the nervous system depends upon whether or not by exploring such analogies you can come to any new insight into what goes on, that is whether these notions contain useful, descriptive properties of what goes on. It is clearly true that sooner or later—and perhaps we are there now—we will reach a state where the business of describing some other point where computational properties of the nervous system are not like the model and will be advantageous and probably the best way we can explore it.

Gerard: That is a very good statement.

Pitts: There is a third between the two, because they are not opposite. The digital and analogical sorts of devices have been defined quite independently and are not logical opposites. We called them analogical because we think we meant roughly this: when we want to solve equations and construct a device to do it, often the way to do it is to construct a system. You can map variables in which we can make a one-to-one representation between the variables in the computer and the variables in the system obeying the equation, a representation such that the connections between them are the same equations that we want to solve. Beside that simple sort of analogy and direct relationship, we might also consider the possibility of a true sort of continuous coding where the mathematical corresponds between the machine and the original system which obeys given laws, although it is one-to-one, and therefore from the results of the computing machine we can calculate, we can find our answer. It nevertheless does not have such simple topology. We might not be able to map variable A in the first system into variable A_1 in the second, B in the first and B_1 in the second, and so forth, and the connections between A and B and the connections between A_1 and B_1. It might be topological where we map A and B together on A prime. If we were not interested so much in the accuracy of the measurement, something of that sort would really approach much more closely to true continuous coding in the strict sense of coding than what is ordinarily thought of in the case of analogical devices, where the coding is simply a strict one-to-one correspondence of a very simple kind of an order, where order is preserved and practicability and continuity are preserved, and so forth. You would really almost need that in the brain if it were to operate and do all the things it does with the truly confused mode of behavior, because the dynamical laws are not at your disposal, whether continuous or discrete.

Savage: Would you describe the sort of coding once more?

Pitts: Where the correspondence between the variables in the machine and the variables in the problem is not a one-to-one topological correspondence, it does not have simple continuity properties.

Wiener: That is easy to do.

Pitts: That is necessary in any brain that would operate on continuous principles simply because the equations that govern the relations between neurons or pools of neurons that are near, one to each other, are fixed and cannot be fixed around those

laws. They cannot be changed to suit the convenience of the problem.

Wiener: I am encountering something of this sort in the work I am doing in connection with prediction. If you are dealing with a discrete time series, the all-or-none sort of coding is a natural thing to do. However, in the presence of noise and a continuous time series, it is still possible to introduce functions in such a way that there is a hidden coding. The coding isn't given directly by the values. At one time it is a quasi. It has the appearance of being continuous, and yet you have discrete lumps of information which come up after a certain time but not all at once. There arises, for example, in this work that I am doing on nonlinear prediction—

Pitts: What a single value on the one side represents depends upon the whole course of values on the other side, so that the transformation is not a simple functional one?

Wiener: Yes.

Bateson: On the question whether or not the sorts of logic involved in an analogical computer would be essentially different from the sorts of logic involved in a digital computer, I don't know with what rigor Whitehead makes the point that the shift from arithmetical to algebra is the introduction of the "any" concept.

Wiener: Quantification.

Bateson: Arithmetic is quantification.

Wiener: In the logical sense.

McCulloch: Quantification in the logical sense.

Savage: It means any.

Pitts: It means every.

McCulloch: Introduction of pronouns.

Bateson: Introduction of pronouns in a sense. Is there difference of that order?

McCulloch: No. It is much more as if you shifted a problem, let us say, from the calculus of propositions to the calculus of relations—something of that sort—and it is a much greater shift.

Frank: May I ask a question that follows that? Is there any light or any understanding of how the transition or transformation from discrete to continuous takes place? Are they two utterly opposed processes? The second question is, is it conceivable that organisms which have had a very prolonged evolutionary history have developed a capacity for making that transformation from discrete to continuous that we are not yet capable of conceptualizing in language? That is a very important point. I get the impression that we are dealing with processes that we can approach

from the concept of discreteness or the concept of continuum and that it depends upon the way we phrase our problem which will appear to be more significant. I wonder if we have the same situation as that pointed out years ago by Eddington when he said that physics was classical on Monday, Wednesday, and Friday and quantum on Tuesday, Thursday, and Saturday. We are not confronted with irreconcilably opposed viewpoints when we realize that there are two ways of recording events which exhibit both discreteness and continuity.

McCulloch: Let us put it this way: as long as the probability of a state between our permitted states is great and has to be taken into account, we have still a flavor of the continuous. When the probability of the *Zwischen* state is zero or negligible, we think chiefly in other terms. That is, I think, purely a matter of practicality.

Wiener: I think it is entirely a matter of practicality whether we approximate a situation which neither corresponds to an absolute number of theoretical lumps nor to a complete continuum with all the derivatives and extremes by either means. That arises all the time in mathematics; it is the correct procedure and annihilates no theories.

Wiener: You simply do whichever is convenient.

Bigelow: We don't have to settle that question here, do we?

Wiener: No.

Frank: I hope not.

Wiener: I say that the whole habit of our thinking is to use the continuous where that is easiest and to use the discrete where the discrete is the easiest. Both of them represent abstractions that do not completely fit the situation as we see it. One thing that we cannot do is to take the full complexity of the world without simplification of methods. It is simply too complicated for us to grasp.

Fremont-Smith: Isn't it true of neurology today that the *Zwischen* zone is becoming more and more pertinent and that we really have to reexamine the all or noneness of the all or none?

McCulloch: A very much more peculiar thing has happened: we have begun to find parts of the nervous system in which a sufficient number of digital processes are lumped so that one can treat them as if they were continuous.

Wiener: Yes.

McCulloch: Mock continuous; that would occur, let us say, in such a thing as the spinal reflex.

Fremont-Smith: But if you go back to your neuron, it seems

to me one can and should—I brought this up in the Nerve Impulse Conference—challenge the use of the words "all or none." When I brought that up I got a violent reaction, which was what I expected from everybody present. It was as if I had suggested something unholy. It was said to me afterward that the challenging of the all or none was something that would be quite important about ten years from now. I thought that was an interesting comment. It seems to me that the center of our problem is the fact that we are basing the neuron on the all or none, while actually we have only relatively all or none.

Pitts: I think I neglected to make my point a moment ago. It was that there can be devices which are computing machines which are continuous without being analogous in the sense that the engineer assumes that the parts of the problem are analogous, the parts of the machine as well as the whole of the machine being analogous to the whole of the machine. If one tries—and it is worth while doing—to see how far one can endeavor to understand the nervous system on that basis, that is the way in which you would have to do it. I think that part of our difficulty is that we have been using terms as opposite which apparently are not logical opposites. We use them only because they are in the properties of the two.

Frank: Years ago Ned Huntington talked about the continuum in terms of the dense, discrete and continuous. Have we dropped that concept between discrete and continuous?

Pitts: I don't think we have. It is the very point. The nervous system treats the continuous by averaging many of the discretes.

Frank: As I suggested, there may be a biological process which we cannot conceptualize by our present-day concepts and language.

Fremont-Smith: Capillary flow is continuous and the heartbeat is intermittent; it seems we have a perfect example right there. You cannot take any point and decide when the shift from the intermittency of the heartbeat to the continuity of the capillary flow takes place.

Pitts: I think that is a very good point. I should like to go back to eye movements a bit, which appear to be discrete about following the sine curve in which the grade of continuity appears to be increased at successive steps. That really seems to be remarkable. The remarkable thing is, it is true in the best grade of following of the sine curve that the original nodes showed where there was discontinuity from the steps.

Stroud: I have been able, using a fine source of light, the con-

centrated arc lamp, to distinguish the discontinuities up to a fair amount of practice, but now I am confounded by the problem of increasing the resolution of my records. It is very easy to follow in the early stages of learning.

Pitts: Always in the same places?

Stroud: Roughly speaking, they are. The steps are nearly the same length. The frequencies are quite constant. I have a subject who can never get beyond the first step that is staged, yet others give smooth transitions from the start.

Wiener: The stepping is unchanged but the mechanism isn't.

Stroud: As though you had a set of one computer working with a good approximation of a continuous equation, which had a good constant and did not supply its own constants. These were changed at intervals to a better fitting set of constants.

Pitts: Those points in the first approximation where the step changes, are they always the same in the same person, are they always the same in number, or do they vary from time to time, or case to case?

Stroud: This I have not explored sufficiently. I can only say that over a forty-five-second period of record on a typical subject reasonably well settled to his task, to a first order of approximation they are quite smoothly repetitive at rates that fall in the range of four to six corrections per second.

Wiener: Trapezoid form, in simple zones, and so forth. I think that will convey what is happening.

Teuber: I don't understand the physical situation. Does that vary with the velocity of the target?

Stroud: I can tell you this: smooth following motions that do a good copy job even with a lot of practice are not possible at very high rates of periodicity. Half-cycle a second was the speed I chose to do most of the work at here. I got good copy. I can assure that at four cycles per second the whole system breaks up.

Marquis: It could also be too slow.

Stroud: It could, I am sure, be too slow for good following motions. I have not found out how slow it could be.

Wiener: This could be useful in detecting forged handwriting.

Stroud: Believe me, this is one of those lucky accidents where in getting into an argument and seeking to prove a point I fell on my feet and we got perfectly readable records the very first time I tried it. But the pressure of business has kept me from collecting more than something over twenty records on about ten or so individuals. It is a very preliminary experiment which, fortunately for me, is quite unambiguous from the start. It is just a fluke, if

you like. Incidentally, it is a bit late, but speaking of the mechanism which Dr. Savage suggested, I have been guilty of promulgating a theory of color sensitivity of cones which requires a somewhat similar sort of thing to take place in a neurological net. If you remember, in color vision for any subjective psychological color there is effective infinity of spectral distribution for each color. This is perhaps not too far afield from what Dr. Pitts was remarking of various kinds of mapping where you do not get the exact topological equivalents because for every point on the psychological color scale there is an infinity of spectra. In an attempt to imagine some reasonable neurological mechanism for color vision—some of the details of this will be published for those of you who have access to NRC Armed Forces Vision Committee proceedings—I used some theory of dielectric rod antennae. To come out with the psychological color, it was necessary to assume that some such process as Dr. Savage suggested was taking place, perhaps in the lateral connections that Dr. Teuber pointed out existed, as well as the direct one-to-one connections, in order to explain color vision. I don't know if it is of any use to any of you, but I have a holier-than-thou feeling because I escape all of this argument by considering these mechanisms not to be properties of some, to me, quite imaginary thing. There are reliable ways in which I may think about what I know, and therefore I find no difficulties if I can find a particular way of thinking about what I know that works. One of them is digital. One of them is analogical, and I suddenly realize that I was very liberal in using quite another one, the probabilistic that I spoke about. If I can think about what I know successfully I leave the rest of you to argue about the essentialness of these various mechanisms for the imaginary.

Klüver: I assume that your new color theory is based on investigations with spectral or, to use the psychological expression, "film colors."

Stroud: There are an infinite number of spectra for each psychological color.

Klüver: You are not talking about "surface colors," that is, the kind of colors that are seen when a surface reflects light in the presence of other surfaces.

Stroud: This is simply in the coding, the psychological experience of, for example, a lighted source.

Wiener: Have you been following the work that has been done by Professor Hardy of the Massachusetts Institute of Technology for printers with reproduction of colored pictures in printing?

Much of the work there is extremely relevant to this sort of thing.

Stroud: The entire practical system of specifying color for the printing and dyeing trade, the so-called "tristimulus" system, is based upon the statistics of large numbers of people in their responses to these various spectra and is a purely mechanical method of reclassifying physically measured spectra in the color equivalent with huge success.

Wiener: There is a probable developmental problem that is actually used to solve it.

Stroud: Modern photoradiometers are computers that are all thimblerigged to come out with the psychologically equivalent color.

Klüver: Deane B. Judd, at the National Bureau of Standards, has pointed out that practically all existing color theories refer to film or aperture colors with a dark surrounding field, but not to surface or object colors in an illuminated space [Judd, D. B.: Hue saturation and lightness of surface colors with chromatic illumination. *J. Opt. Soc. Am.*, **30**, *2* (1940)]. According to him, a surface color requires at least six variables in contrast to the three variables of a film color (hue, saturation, and brightness).

Stroud: This is a purely restricted notion of color.

Klüver: Practically all the colors we encounter in our environment are, of course, surface colors. In general we do not go around peeping into holes or looking at the clear sky or inspecting objects through Katz's reduction screen.

Wiener: This goes further than that.

Stroud: These things will report the analyzed spectral distribution and convert into acceptable equivalent color so that two entirely different spectra having the same equivalent representation will be psychologically indistinguishable.

Pitts: Let's get back to what we started with: the sine curves. What disturbs me is the secant, not the tangent, and there are only four or six in the whole business. So the secant differs considerably from the tangent at every point. Now how in the beginning do you project where you are, and in the next second, the second? How can you project to move along the secant in the sine wave rather than along the tangent?

Stroud: I can only assure you that I don't think the eye does anything of the sort. At first it merely moves to a new position in this possible row of positions. I drew a hump here in which time is drawn along this way and lateral motion is drawn vertically.

Pitts: That is what I supposed.

Stroud: The eye moves to a new position and stops in the first attempt to solve. It finds it is in a wrong position and moves to another position. It is still a wrong position; these saccadic movements are extremely fast. The eye is a vastly overpowered system. To a first approximation you can neglect its inertia. It soon discovers what it really wants is to have a moving point of view. So it sets up a moving point of view, starting with a rate, with a starting point of two constants.

Pitts: Our question is how at every node—let us so call the point where there is a chance of behavior nodes—it decides the velocity, knows where it is, and simply changes the velocity. It now knows it is going to proceed for the next period of time with a constant. How is it changed? It set the velocity apparently by the secant and not the tangent, not by the contemporary.

Stroud: It is a predictive cycle.

Pitts: Which apparently—

Stroud: The results of predictions come in discontinuously, but the thing acting is acting smoothly.

Pitts: It is very difficult to tell. It says nothing about the mechanism, because nothing ever changes and because you merely use the sine wave. Nothing ever changes, so you have no idea on what information it relies. It could rely upon information from very far back, since the sine wave was going on for a long time or simply relying on information in the last couple of cycles, or possibly on information in the last sixth of the second.

Stroud: Those are the things I want to find out.

Wiener: I have encountered precisely this in connection with prediction. You may have apparatus which works well on the sine curve but is going to show indecision when it comes to the angle. I think this should be studied.

Stroud: I can give you a little information about how long a simple sinusoidal motion has to go on before the eye gets good, since I happened to have data on the eye-hand circuit which was obtained elsewhere. Under these circumstances, if you have more than one cycle of harmonics the average subject will quickly enough get very excellent approximation of this in one or two preceding cycles.

Bigelow: What sort of screen persistence are you using on this?

Stroud: I was using the 11, which is a very short one, and I plan on using a 15 to remove any faint doubt that there is any possible persistence at all.

Pitts: If it operates as a linear prediction on that long-time base with things moving in an irregular way, it is going to do very badly.

Stroud: You have to remember that the third step comes in and begins to introduce acceleration.

Pitts: One or two cycles? You say that is enough to do it. That would be approximately a sixth or half a second?

Stroud: You are asking me about things I have not measured myself.

Pitts: What is the order of magnitude?

Stroud: The order of magnitude of 20 moments, something like that, for a good quick eye.

Pitts: That is about two seconds.

Stroud: I am guessing from another fellow's data using another circuit.

Pitts: I wanted the order of magnitude; linear prediction acting on a two-second time base would be very bad for the eye in general.

Licklider: Not affected time base.

Stroud: It has been shown in other tests that once such a solution breaks down, it does not break down slowly. It breaks down suddenly. For instance, if you are tracking a thing manually, all of a sudden the chap introduces a step function, you make the next prediction, and then your whole system of predictions of acceleration and velocity breaks up and you start off again more or less with the simple position in an attempt to get the following motion.

Pitts: What happens if the frequency drifts so slowly that it does not break up?

Stroud: These are things I hope to find out more about. Remember, I said these are preliminary experiments in which I fell on my feet.

Teuber: You know of Rademaker's and ter Braak's work on nystagmus? [Rademaker, G. G. J., and ter Braak, J. W. G., On the central mechanism of some optic reactions. *Brain*, **71**, *48* (1948)]. They had a rabbit look with one eye at a moving drum with black and white stripes. The eye that was looking at the striped drum was immobilized. Eye movements were recorded from the other eye, which was free to move, but was completely covered by an eggshell. The covered eye moved in the same direction as the stripes, which were rotated at constant velocity around the rabbit; that means the eye followed, even though it didn't know how well it followed, it couldn't judge.

Pitts: The first eye is immobilized . . .

Teuber: The first eye is completely immobilized. The surprising thing is that the rabbit does follow with the eye that gets no light. The other eye can see the light, but can't move. Rademaker and ter Braak have shown earlier that animals can get nystagmus when a single light moves in a totally dark room. That, too, does not fit into our theories of following movements and nystagmus. However, in the case of the rabbit in which one eye was covered, that covered eye moved much faster than under normal conditions. They state that it moved about sixteen times faster than the stripes which were rotating at constant angular velocity. There was following without tracking, without knowledge of whether you were on the target or not, but evidently with considerable overshooting. Still, you must assume some central mechanism for such optokinetic responses independent of any specific feedback. I think such mechanism should manifest itself in other tracking situations, even in the human. I'm bothered by the fact that you did not get any other oscillations than the one you described.

Stroud: That is one of those things I am very much interested in doing. I want to present, for example, a problem, watch it become a completely solved predictive problem, change the problem, and watch the breakup of the old solution. We are very much interested in intricate details. They have implications which I am not at liberty to discuss. Believe me, we will work this thing to the very bone before we are through with it.

McCulloch: Dr. Gerard, will you summarize briefly?

Savage: Then we go backward.

Frank: Let us continue.

McCulloch: Are you sure questions won't come up again? I think Von Foerster might like to quantize nervous activity at the level of the electron, the basic physical level. I know I should like to quantize at the level of neurons. I know Stroud had to quantize it at the level of the moment, something of the order of a second in order to match our data. We are next going to tackle speech efficiency. We are going to begin with Licklider, and I am going to ask you to show him the same courtesy that you showed to Ralph Gerard. Let us allow him to proceed without interruption except for questions of plain understanding.

THE MANNER IN WHICH AND EXTENT TO WHICH SPEECH CAN BE DISTORTED AND REMAIN INTELLIGIBLE

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I SHOULD like the blackboard, lantern slides, and permission to walk around a little bit, because I'll get stagnant if I stand in one place too long.

The discussion this morning makes it almost unnecessary to say what I have to say this afternoon, since my remarks will be directed toward the point that—even on the rather microscopic level on which I want to consider it—speech is highly redundant. You can talk a long time and say very little.

I should prefer to consider the problem of speech communication on a single level. It will be necessary, however, since I am interested in effects of noise and distortion (which are on one level) upon intelligibility (which is on another level), to consider the problem on two different levels. Noise is most easily thought of as some sort of a function of time or of frequency. Distortion is most easily thought of as a deformation of a function of time or of frequency. In describing noise and distortion, therefore, we will work with functions of time and frequency. In order to discuss intelligibility, on the other hand, it will be necessary to work with elements: phonemes, syllables, words, or sentences. Therefore we shall have to engage in an exercise in shuttling back and forth between the level of functions and the level of elements.

To facilitate our getting together on the kinds of noise and distortion I want to talk about, let us set up a simple paradigm. Let us write $f(t)$ for the speech wave, a temporal segment or sample of someone's speech, say the pressure variation in the air at the end of this piece of chalk or at the face of that microphone over there. Of course, f is a very complicated function of time t,

but we can think of it easily enough, and we can schematize it and represent it by a jiggly line like this:

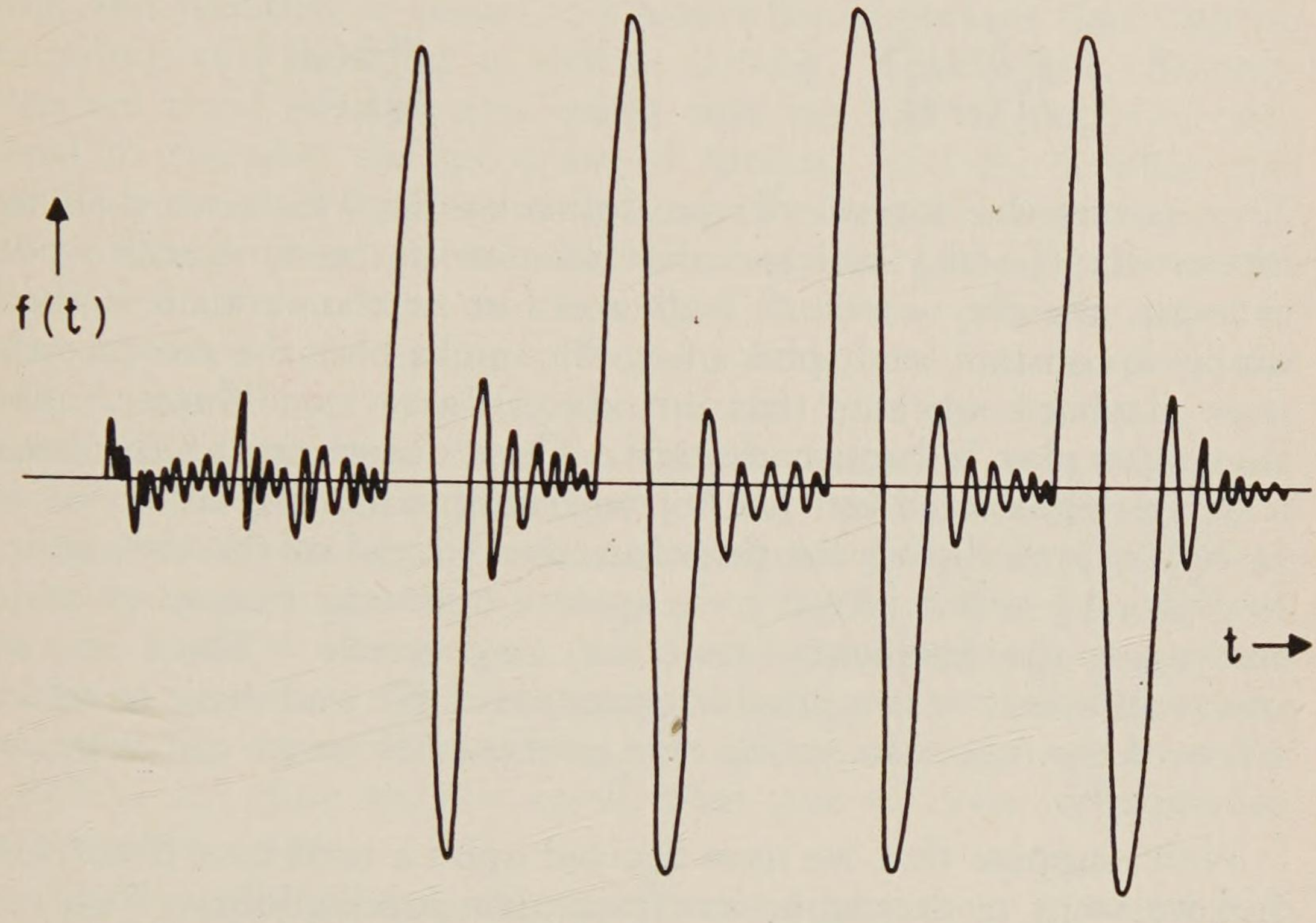


FIGURE 2. Schematic representation of speech wave, showing sound pressure as a function of time.

We shall use $f(t)$ as short for the speech wave form. Or, alternatively, we shall use $F(\omega)$, the Fourier transform of $f(t)$, which will be more convenient when we want to operate in the domain of frequency ω. [The complete spectrum $F(\omega)$ gives the amplitudes and phase angles of all the sinusoidal components into which the speech wave form can be analyzed, and therefore represents the same thing as $f(t)$.]

In addition to $f(t)$ and $F(\omega)$, it will be convenient to think of an arbitrary time function $g(t)$, different from $f(t)$, and the Fourier transform $G(\omega)$ of $g(t)$. Noise is represented by $g(t)$ and $G(\omega)$. By assuming various forms for these functions, we can run through the gamut of noises. When we inquire about effects of noises upon speech intelligibility, we are simply asking for a comparison of a listener's reactions to $f(t)$ and $f(t) + g(t)$, or, what is the same thing, to $F(\omega)$, and $F(\omega)$ superposed upon $G(\omega)$.

In order to handle distortion, we add to our notation by introducing the versatile operator h, which does whatever we want it to do to whatever we want to operate upon. Starting again with our $f(t)$ and $F(\omega)$, we can write:

$$f[h(t)] \qquad F[h(\omega)]$$
$$h[f(t)] \qquad h[F(\omega)]$$

That covers the domain of the distortions in which we shall be interested. $f[h(t)]$ indicates that we distort the time scale. For example, we play a record back too fast or too slow; h is then simply a constant multiplier of t. We might play the record on a poor playback machine that introduces "wow" or "flutter." Or we might play it back backward. These distortions of the time scale are represented by f$[h(t)]$ with simple interpretations of h. In $h[f(t)]$ we distort the time function instead of the time scale. In $F[h(\omega)]$ and $h\ [F(\omega)]$ we operate upon the frequency scale and upon the frequency function, respectively. There are of course infinitely many possible operations. We shall have to select a few of the more interesting ones, and even those we can examine only briefly.

Now, suppose that we have decided upon a noise or a distortion and we want to determine its effect upon intelligibility. You are probably all at least somewhat familiar with articulation or intelligibility tests. If you are not, it's immaterial; they are ridiculously simple. You could count the number of repetitions it takes to get a message across to a listener. You could determine how long it takes to transmit a certain amount of information. But in the great majority of intelligibility tests, at any rate in this country, you simply *(a)* have a talker read syllables, words, or sentences to a listener, *(b)* have the listener write down what he hears, and *(c)* after the test is over, find out what percentage of the words that were read were written down correctly (that is, correspond with the words on the talker's list). That percentage is called "per cent syllable (or word, or sentence) articulation (or intelligibility)." The relations among syllable, word, and sentence intelligibility are fairly well worked out. If you know what percentage of the syllables gets across from the talker to the listener, you can predict what percentage of words or sentences would get across.

Usually, of course, you conduct the test with a number of talkers and listeners (though often not a very large number, because the test is expensive and time-consuming). The mean or median

of the individual percentages is your measure of intelligibility.

Let us then start with noise. Of the many noises that have been studied, the most detrimental to intelligibility are: first, the babble of a number, say six or eight, of other people, all talking, laughing, and shouting at once. It is somewhat important that they be laughing and shouting as well as talking. That is one. Second, random noise (fluctuation noise) that has had its spectrum tailored to resemble the spectrum of speech. And third, what you get if you apply random noise to a vibrator, a Sonovox (one of those things they use to make musical instruments talk in the commercials you hear on the radio), hold the vibrator to your throat, move your lips and tongue a little, and then amplify the sound that comes out of your mouth. This third noise is very much like a whisper, but it has the advantage (if you are careful never entirely to close your lips) of not having any gaps in it.

The noises that are least detrimental to intelligibility are almost entirely nondetrimental. A sinusoid of 4,000 cycles, for example, can be made so intense that you cannot bear listening to it, yet it will not interfere with the understanding of even fairly weak speech. So, when we examine the effects of noise upon intelligibility, we must keep in mind what sort of noise it is that we are working with.

The favorite noise in the experimental laboratory is wide-band, continuous random noise. It is like the noise of Brownian motion, like the noise inherent in a resistance, but in practice it is usually derived from the fluctuations of current in a gas tube. This noise is uniform and continuous both in time and in frequency. These characteristics simplify the interpretation of its effects. It sounds like "ss-sh-hh-hh-hh."

If we add together some speech, $f(t)$, and some noise, $n(t)$, and measure the intelligibility of $f(t) + n(t)$, we find that the result depends most critically upon the intensity of the speech relative to the intensity of the noise. That (over a wide range of intensities) it is the ratio of the speech intensity to the noise intensity and not the absolute value of either is a simple but in my opinion very important fact. It means that the listener identifies the sounds of speech on the basis of characteristics that remain invariant despite wide variations of average intensity. Think how much wider our vocabulary of speech sounds would be if "ah" at one intensity level meant something different from "ah" a little louder. On the other hand, think how confusing it would be if the meaning changed abruptly whenever the acoustics of the room or the location of the talker changed slightly.

With the speech and random noise at the same intensity level, this level being measured in the frequency band 0 to 5,000 cycles per second, we get about 50 per cent word articulation. Varying the speech-to-noise ratio, we find that intelligibility changes in approximately the way shown by this curve:

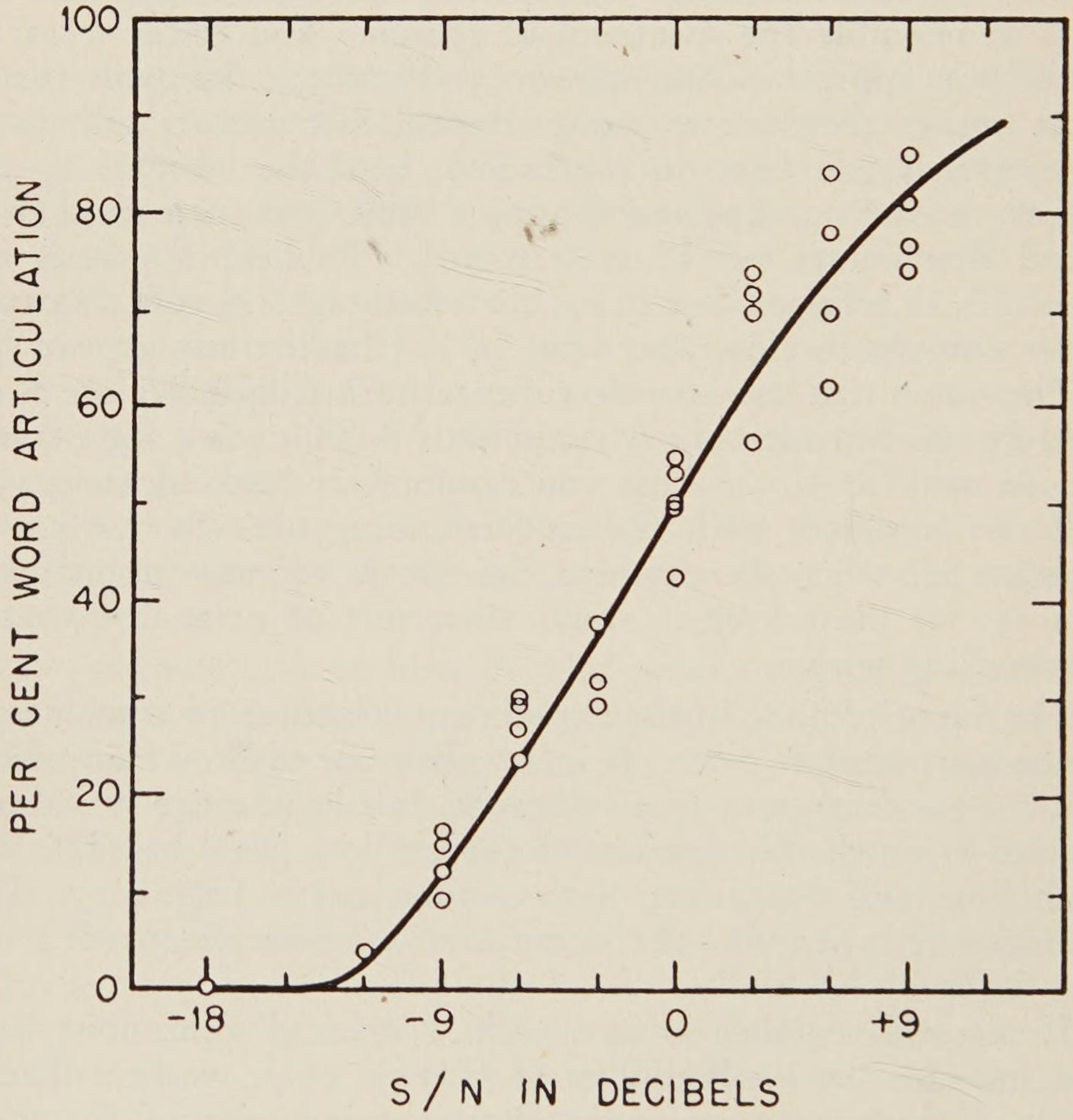


FIGURE 3. Word articulation for speech heard in the presence of noise, plotted as a function of the speech-to-noise ratio in decibels. The average speech level was held constant at approximately 90 db re 0.0002 microbar. [MILLER, G. A., and LICKLIDER, J. C. R.: *J. Acous. Soc. Am.*, **22,** *167, Fig. 9* (1950)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

The curve slopes about four words per decibel in the middle of the range and tails off a little at the lower end and quite a lot at the upper end. Now, if we fix upon a speech-to-noise ratio and

vary the levels of the speech and noise simultaneously, we find that intelligibility varies like this:

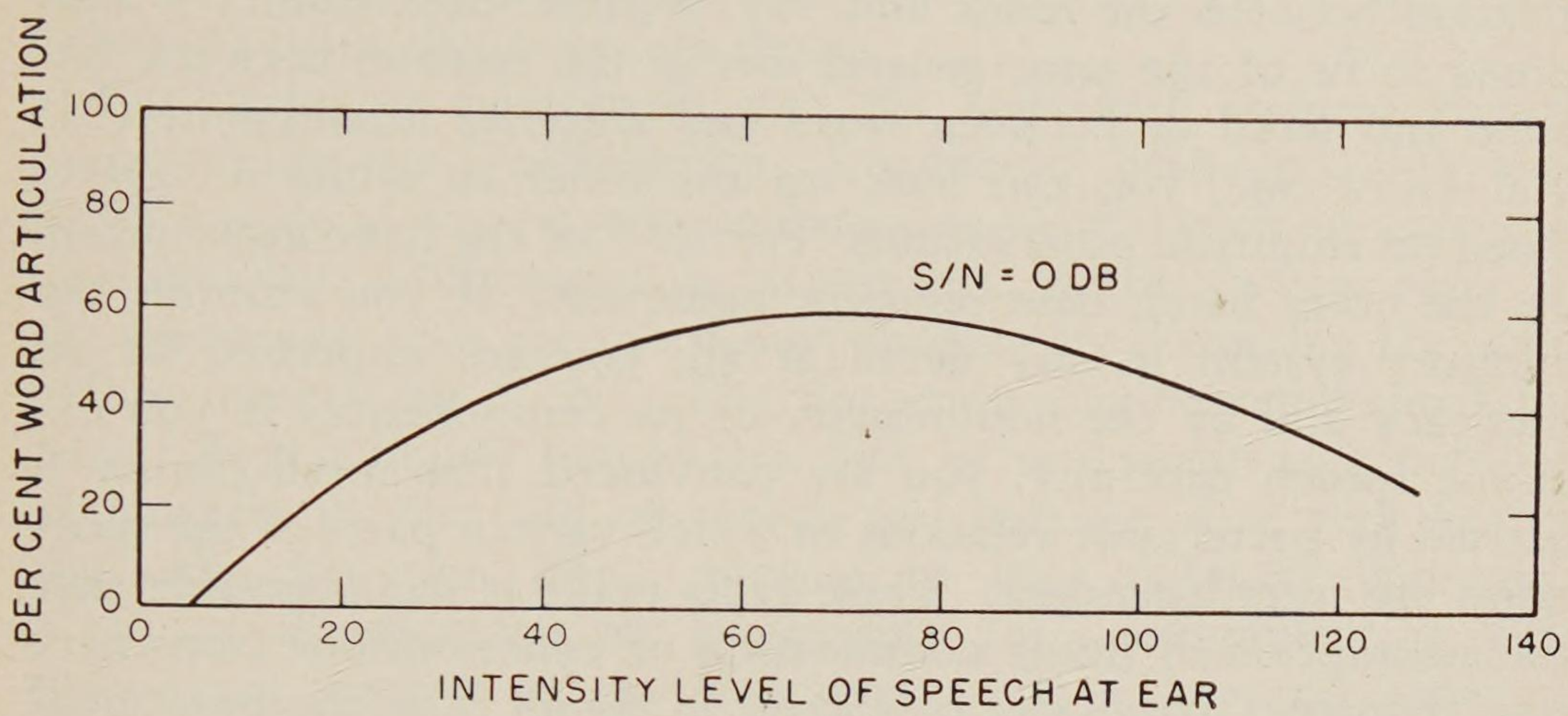


FIGURE 4. Word articulation at constant speech-to-noise ratio, plotted as a function of the intensity of the speech at the listener's ear. This figure is based in part upon the work of K. D. Kryter and in part upon a number of other observations.

The broad plateau covers the range in which only the ratio of signal to noise is important. If the speech gets too weak, some of the speech sounds become inaudible. If the speech gets too intense, the listeners either can't or won't listen (I think it is in large measure the latter).

So much for a rough map of the relations between intelligibility and the intensities of speech and random noise. We can now generalize a bit by bringing into the picture noises that are not uniform in frequency (noises we can produce by passing random noise through various frequency-selective networks), and we can uncover something a bit more fundamental about the listener's reaction to speech in the presence of noise. The easiest way to do these things is to describe a procedure for computing the intelligibility of speech in the presence of noise. The procedure is based on simplifying assumptions, of course, but it works rather well. And, to the extent that it yields correct results, it displays in convenient form some of the functional characteristics of speech perception.

The basic assumption of the procedure is that there exists an index, an index of articulation or intelligibility, that *(a)* bears a simple relation to syllable, word, or sentence intelligibility and

(b) is a linear sum of a number of parts, each of which is dependent upon the speech-to-noise ratio in a narrow interval of the frequency scale. I shall say no more about *(a)* than that the relation between the index and, say, syllable intelligibility is supposed to be of the same general sort as the relation between syllable and word or between word and sentence intelligibility. If you know one, you can look up the other in tables or curves based on empirical observations. Part *(b)* of the basic assumption, on the other hand, deserves some comment. If you examine the auditory system in any detail at all, you are impressed by its intricacy and by the nonlinearity of its components. If you examine speech carefully, you are convinced that intelligibility is carried by patterns or relations in which various parts of the spectrum are interdependent. These facts make it inconceivable that the assumption of linear combination of contributions from narrow frequency bands can be correct in detail. The computational procedure is only supposed, however, to yield results that are approximately correct in a long-run statistical sense. It is with that fact in mind that part *(b)* of the assumption should be interpreted.

Let us sketch roughly that part of variation of $F(\omega)$ that describes the distribution of speech power in frequency. This is $S(\omega)$, the power spectrum of the speech. On the same plot let us indicate $N(\omega)$, the power spectrum of the noise. It will simplify matters to assume without discussion that both the speech and the noise have continous spectra, and to define S and N in terms of power per cycle, i.e., power in a frequency band 1 cps wide, centered at $\omega/2n$ cps. Now the essence of the computational procedure is that the articulation index I is proportional to the integral of the logarithm of S/N over a *distorted* frequency scale. It will be necessary to say something about restrictions on the range of S/N and about the nature of the distortion of the frequency scale, but the main idea is, as the last sentence said, to add up contributions proportional to the ratio of speech to noise in each of a number of narrow frequency bands.

Making the speech more than 1,000 times as strong as the noise in any narrow frequency band adds little or nothing to intelligibility, so we set 1,000 as an arbitrary upper limit for S/N. If the speech is weaker than the noise, in any narrow frequency band, it contributes essentially nothing to intelligibility, so we set unity as an arbitrary lower limit for S/N. Furthermore we must restrict the absolute value of S by imposing an upper limit beyond which the auditory mechanism would be overloaded, and we must

restrict the absolute value of N by introducing a weak fictional noise to play the role of the absolute threshold of hearing. The basic function, then, is S'/N' where the primes indicate that we have made the necessary restrictions in range. S'/N' is shown as a dotted line in the preceding figure.

The very low frequencies and the very high frequencies are unimportant for intelligibility; frequencies between 1,000 and 2,500 cps do a disproportionate amount of the work of carrying intelligibility. Differences in importance of the various parts of the frequency scale are taken into account by distorting the scale before integrating S'/N'. The distortion can be represented by $p(\omega)$. It is a highly remarkable fact, or so it seems to a number of workers in the field of speech and hearing, that very nearly the same function $p(\omega)$ appears in four different contexts: $p'(\omega) = dp/d\omega$ gives the importance for intelligibility of each elementary interval of the frequency scale; it is proportional to the just audible increment in the frequency of a pure tone; it is proportional to the interval of frequency over which random noise is effective in masking a pure tone; and $p(\omega)$ is proportional to the subjective pitch of a sinusoid of angular frequency ω. Evidently, $p(\omega)$ is a reflection of something basic in the frequency-resolving mechanism of the auditory system.

The articulation index, then, is

$$A = k \int \log \left[S'(p)/N'(p) \right] dp \, ,$$

where k is the constant that makes $A = 1$ when the signal-to-noise ratio is always at least 1,000, or 30 db.

This formulation tells us a number of things about the perception of speech. It incorporates the fact, upon which we have remarked, that it is the ratio of speech intensity to noise intensity, measured in a narrow frequency band, that is the important quantity. It says that that quantity makes itself felt according to a logarithmic rule. And it tells us that the contributions of various frequency bands to over-all intelligibility are weighted in a way that is governed by the frequency-selective characteristic of the auditory system. It is noteworthy that characteristics of the auditory mechanism appear to be much more important than those of the speech signal in determining the weighting.

It will be of interest to many of you to note the parallel between the formula for articulation index and the formula for

amount of information,

$$H = WT \log_2 [(S+N)/N] .$$

It is possible that the index formulation may be improved by substituting $S+N$ for S (thus following the information formula) and dispensing with the restriction that S'/N' must not fall below unity. In any event, the parallel is sufficiently good to suggest that the listener is indeed receiving information when he understands speech.

Pitts: I have one question about the pitch-frequency relation. Is that such that it distorts the length of intervals very significantly at the extremes?

Licklider: It distorts them considerably. With respect to frequency, the pitch scale is neither linear nor logarithmic; it is halfway between.

Pitts: However, from the linear—

Licklider: Halfway from the linear to the logarithmic.

Pitts: In the range that is important?

Licklider: Yes, say from 100 cycles to 4,000.

Wiener: What does halfway mean? X plus S over N?

Licklider: Mathematicians are always doing that: taking me up on inexact statements. Subjectively, it would be about halfway between. I can quote you the terminals of the frequency bands which contribute equally to intelligibility, or better, which are judged equal in terms of subjective pitch. I can quote them pretty closely. We will see how many there are. Then we can see how many per cent. First, up to 160 cps. Then from 160 to about 400. Then 400 to 670, 670 to 1,000, 1,000 to 1,420, 1,420 to 1,900, 1,900 to 2,450, 2,450 to 3,100, 3,100 to 4,000, 4,000 to 5,100. You can see at about what rate we are going up in frequency. These bands are equally wide, according to the best available evidence, in terms of subjective pitch. In addition, they contribute approximately equally to intelligibility.

Pitts: The reason this is important is that it multiplies the S plus N over N factor in the quantity if you expressed it in frequency, and so expresses the difference from the behavior of linearality. I wondered how much difference was made by that. Here it was going to the difference, but then you rubbed it out.

Licklider: 160, about 400, 670, 1,000, 1,420, 1,900, 2,450, 3,100, 4,000, 5,100. I think, I am not sure, that from there it goes pretty far up, about 9,000 cycles. The relation is roughly linear with frequency to 1,000 cps, logarithmic beyond 1,000 cps.

Bigelow: This ought to do.

Licklider: I am sorry; I did not see what you were doing.

This, then, gives a rough orientation concerning effects of noise that is continuous both in time and in frequency. Perhaps I should not say "continuous" before so many mathematicians; it appears to be dangerous to say that a random function is continuous. (Pitts, here, says that random noise is continuous, all right, but not differentiable.) Anyway, the noise we have been talking about "covers" fairly uniformly both in time and in frequency. Let us see what happens when the noise is not continuous in time.

If we interrupt a noise, that is, turn it on and off intermittently, we can introduce temporal gaps. The possibility then exists that we can hear snatches of speech in the intervals when there is no noise. Let me draw for you, here, a graph:

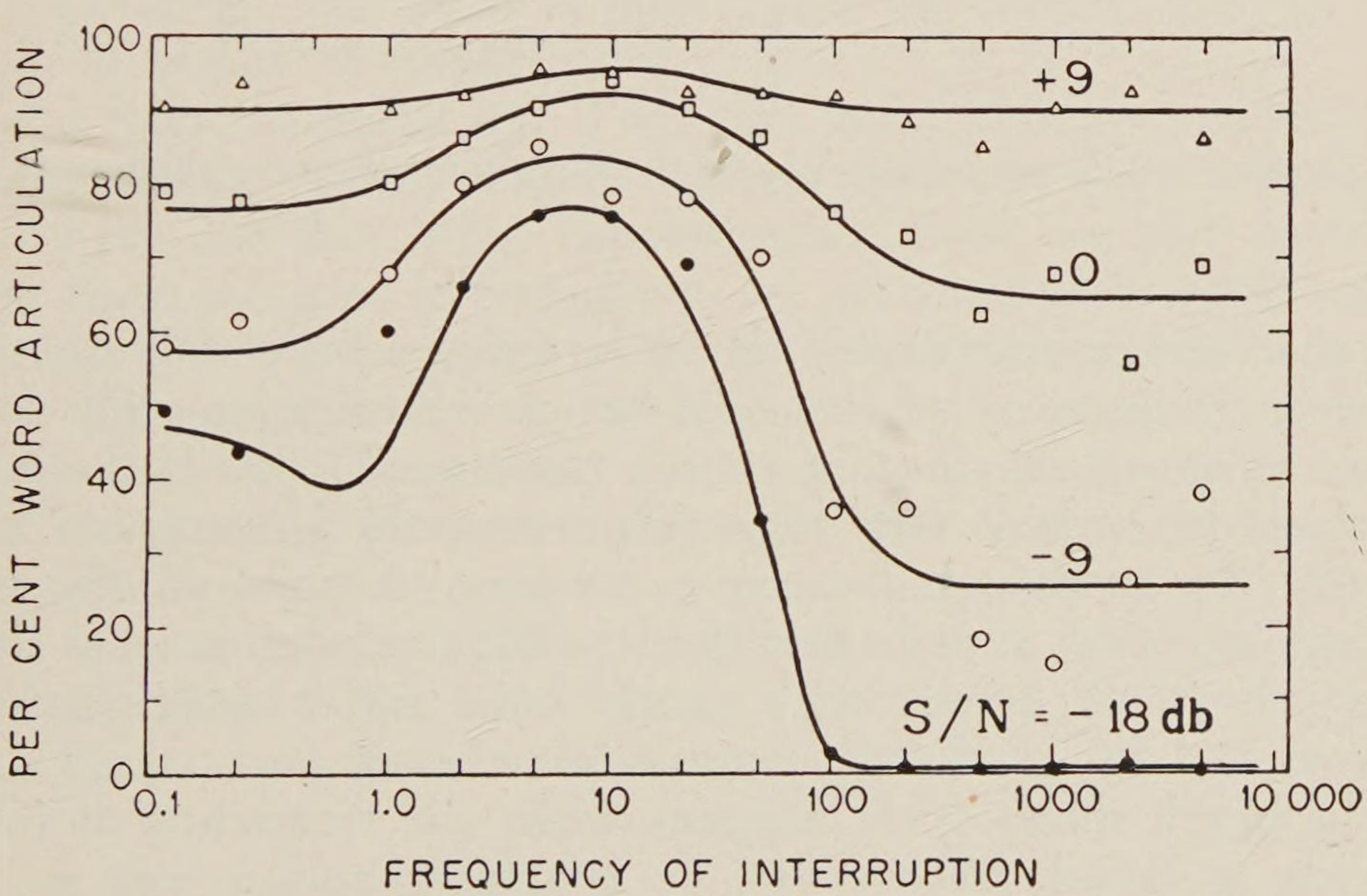


FIGURE 5. The masking of continuous speech by interrupted noise. Word articulation is plotted against the frequency of interruption of the noise, with the speech-to-noise ratio as the parameter. Noise-time fraction, 0.5 [MILLER, G. A., and LICKLIDER, J. C. R.: *J. Acous. Soc. Am.*, **22,** *167, Fig. 8* (1950)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

The ordinate is word intelligibility. The abscissa is the frequency of interruption. Let us examine first the curve for $S/N = -18$ db. (In this notation the intensity of the noise is the intensity

measured with the noise turned on steadily.) Now with a speech-to-noise ratio of −15 db (measured in the frequency band 0 to 5,000 cps), in which the noise is something less than 100 times as intense as the speech, you can understand just about nothing when the noise is on all the time. When it is on half the time and off half the time, it has a wave form like this:



FIGURE 6. Schematic illustration of the wave form of interrupted noise.

and the average intensity of the interrupted noise is only one-half the average intensity of the noise before interruption. The long-time average speech-to-noise ratio is therefore 15 db. If the interruptions occur at a very high frequency, the interrupted noise sounds just as though it were a continuous noise of the same average intensity, and we find that the intelligibility score is about what we should expect with steady noise and a speech-to-noise ratio of −15 db: practically no words are heard.

The graph shows what happens when the frequency of interruption is varied. Above 100 or 200 interruptions per second, the noise masks speech as effectively as if it were continuous. The ear cannot hear through gaps that are less than 1/200 or 1/400 second wide. John Stroud is looking at the peak at 10 interruptions per second, but let us first go to the other end of the graph. We see that if the interruptions are very infrequent, you blank out whole series of words, then pass whole series of words, and you come out with a score that is about one-half the score for speech in the absence of noise. Here that is about half of 100, or 50 per cent.

Now if we were using a weaker noise, intelligibility would follow a roughly parallel but higher curve (see graph). But we

already have the important thing, I think: the noise must be there effectively all the time if it is to mask speech. Or, conversely, the mechanism of speech perception is agile; it can piece together little bits of speech that get through between the bursts of noise.

Bigelow: Were these tests made on random words or on a text?

Licklider: These are random words.

Bigelow: No content?

Licklider: No.

Kubie: Random words or nonsense syllables?

Licklider: A vocabulary of 1,000 monosyllabic words was used for the tests. They were drawn at random from that vocabulary to form lists of 50 words each, and these were read off.

McCulloch: That peak is at 10 per second?

Licklider: At ten.

McCulloch: The peak in intelligibility?

Licklider: Almost identical results were obtained, in so far as the location of the peak is concerned, by simply turning the speech on and off at various rates.

But I want to hurry on. There are many things to cover, and I will probably have to subside before I am really quite willing to.

We have just seen what happens when there are gaps in time. When there are gaps in frequency, we get an entirely comparable effect. It can be explained with the aid of a very rough model of the frequency-resolving mechanism of the auditory system. We can think of the auditory system as containing a large number of overlapping frequency channels. The channel widths vary from a minimum of about 40 cycles to a maximum of perhaps 2,000. The channels handling frequences below 2,000 cps are all less than 100 cps in band width. The curve of band width versus center frequency is shown in Figure 7.

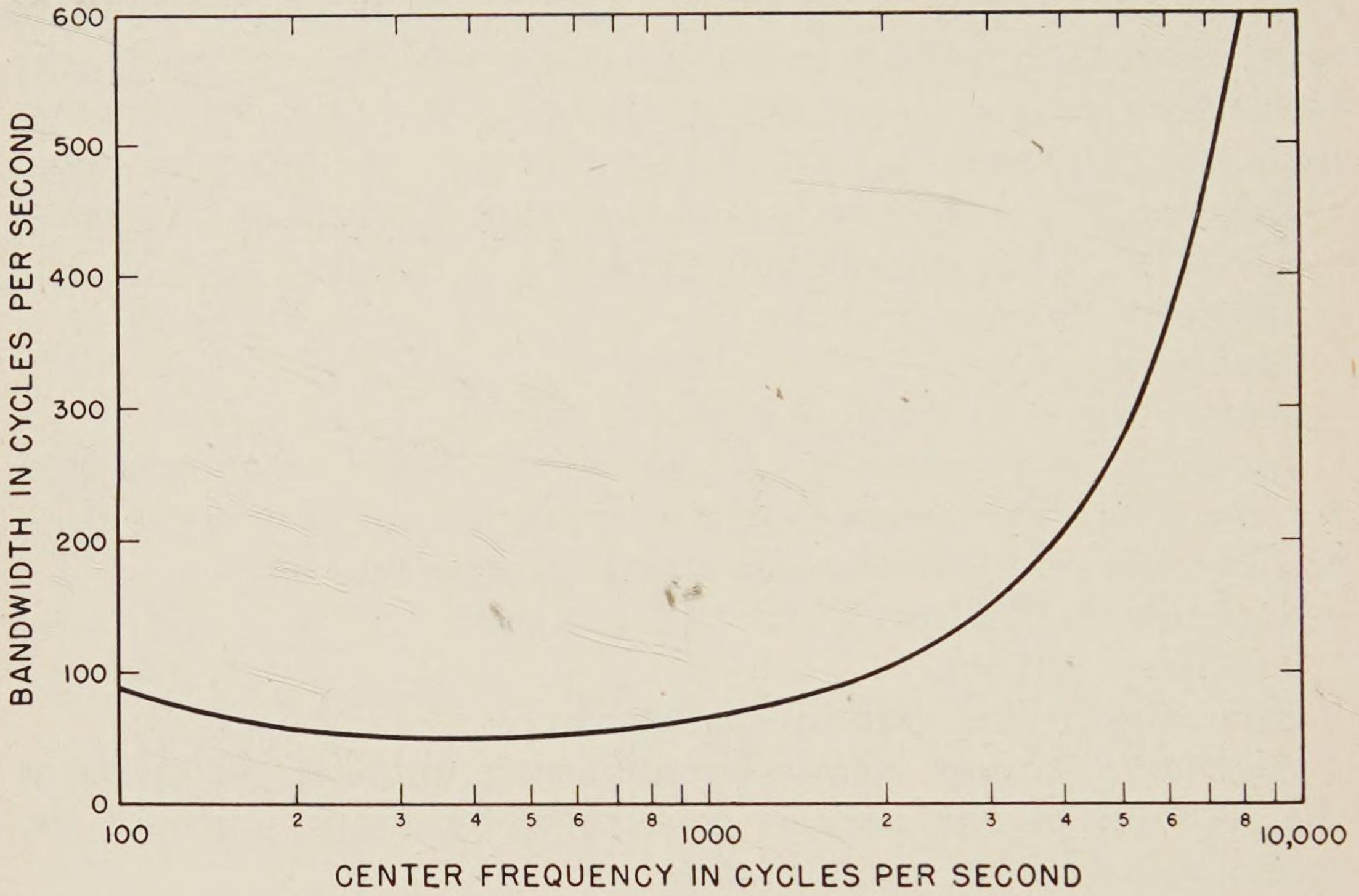


FIGURE 7. Band widths of the critical frequency bands as a function of their center frequencies. Based on data of Fletcher and of Hawkins and Stevens.

You may recognize this as the reciprocal of the function $p'(\omega)$ we talked about a while ago. Now if the speech components in one of the channels are to be masked, there must be noise in the channel. Therefore, any noise that has wide gaps in its spectrum will let some of the speech get through unmasked. This is the primary conclusion. I shall not spend time on the secondary one, which is the qualification that low-frequency noises do to some extent mask high-frequency signals.

An interesting situation arises when the interference consists of delta pulses—it doesn't matter whether I draw them in time or in frequency—spaced at equal intervals.

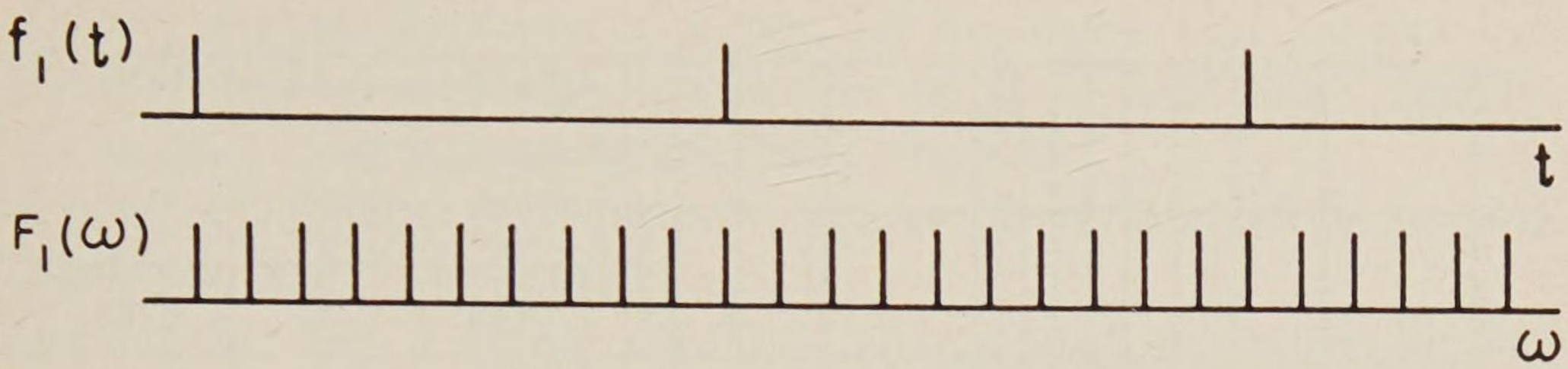


FIGURE 8. Schematic illustration of reciprocal relation between wave form and spectrum of a train of sharp pulses. When the gaps in the time function are closed, the gaps in the frequency function widen.

If the pulses are spaced at intervals of 1 second in time, they are spaced at intervals of 1 cps in frequency. If we decrease the time interval, we increase the frequency interval, and vice versa. If we want to mask speech with the pulses, therefore, we must make a compromise. We cannot have close spacing both in time and in frequency, as we should like; we have to sacrifice in one domain to gain in the other. The question, then, and I think it is a rather cute one, is what is the compromise that minimizes intelligibility?

Well, remember the time-gap graph. The rate of interruption had to be below 100 per second before much speech got through the time gaps. And remember the band widths of the auditory frequency channels. They were a little less than 100 cps wide in the important part of the frequency scale. These two facts suggest that 100 pulses per second should be about the best rate for masking speech. As the following graph indicates, that is approximately correct.

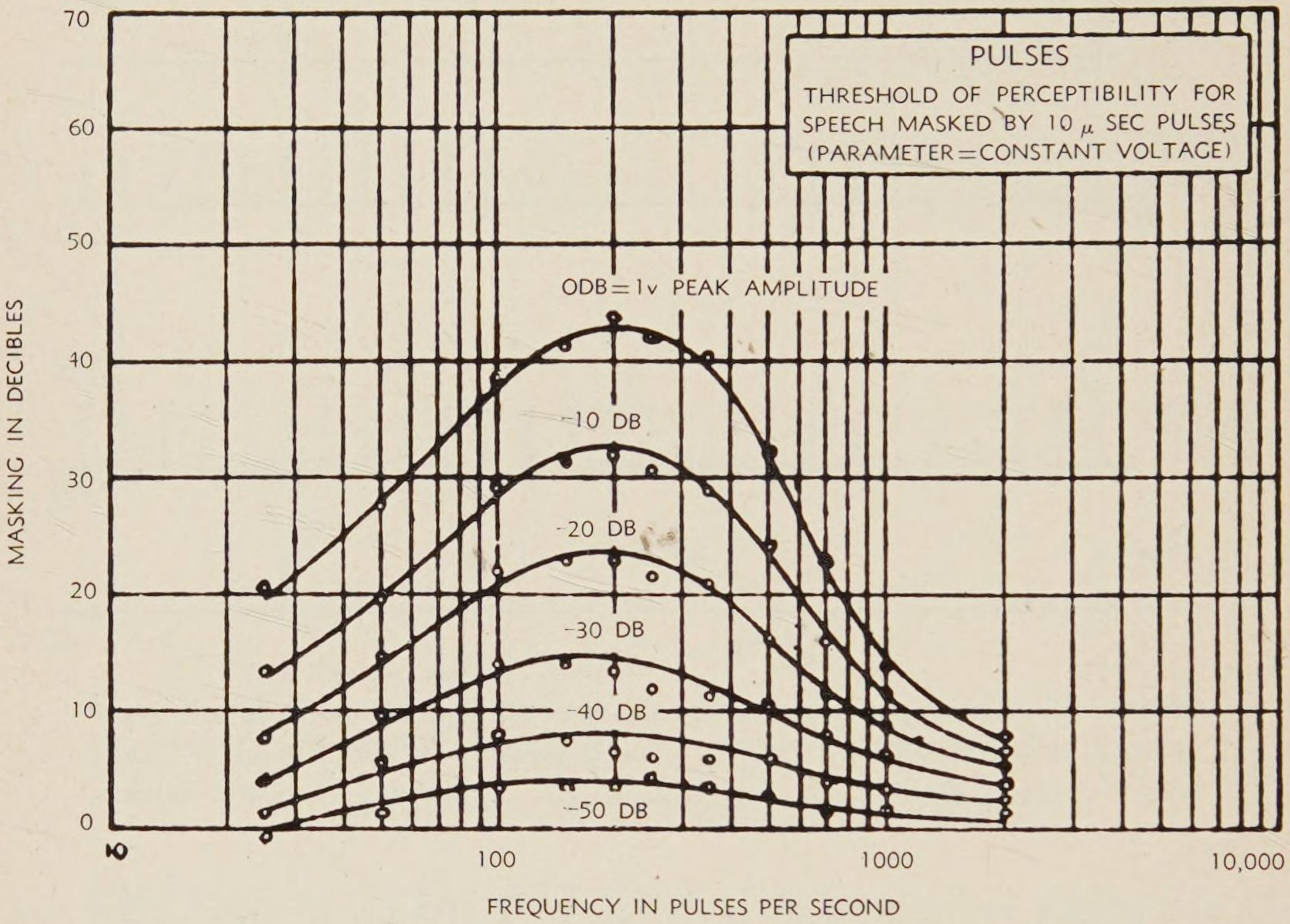


FIGURE 9. The masking of speech by trains of pulses having various repetition frequencies. [STEVENS, S. S., MILLER, J., and TRUSCOTT, I.: *J. Acous. Soc. Am.*, **18,** *418, Fig. 6* (1946)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

In a way, I am sorry that the critical figure comes out 100 instead of 10 per second, but, John [Stroud], I cannot help you here.

With that, so much for noise. Distortion is for me more interesting to talk about.

Kubie: May I ask one question? Do you encounter any subjects whom you have to eliminate because on scoring their reactions they deviate very sharply from your general group?

Licklider: No. I think I can give a categorical answer to that. But perhaps not: there are probably some aphasiacs, even bad ones, that might get through high school or college. It would not matter much if the subjects were a little deaf, really. Where there is noise, conductive deafness is not a handicap because the noise is attenuated along with the speech. You would have to except nerve-deafened people. Some people with nerve deafness are said to be completely confused by noise. Other exceptions, no.

Kubie: I was thinking in terms of auditory and visual types.

Licklider: I would say it is rather hard to find any "audives" or "visuals." There is no relationship there—

Kubie: Have you made any comparisons using random nonsense syllables rather than words?

Licklider: Yes, I have. In fact, many such comparisons have been made. You can find special sorts of stress that will break the rule down, but by and large the rule is approximately this: if we plot the articulation index I have mentioned along the abscissa and other measures of intelligibility along the ordinate, we have a way of transforming from any measure to any other.

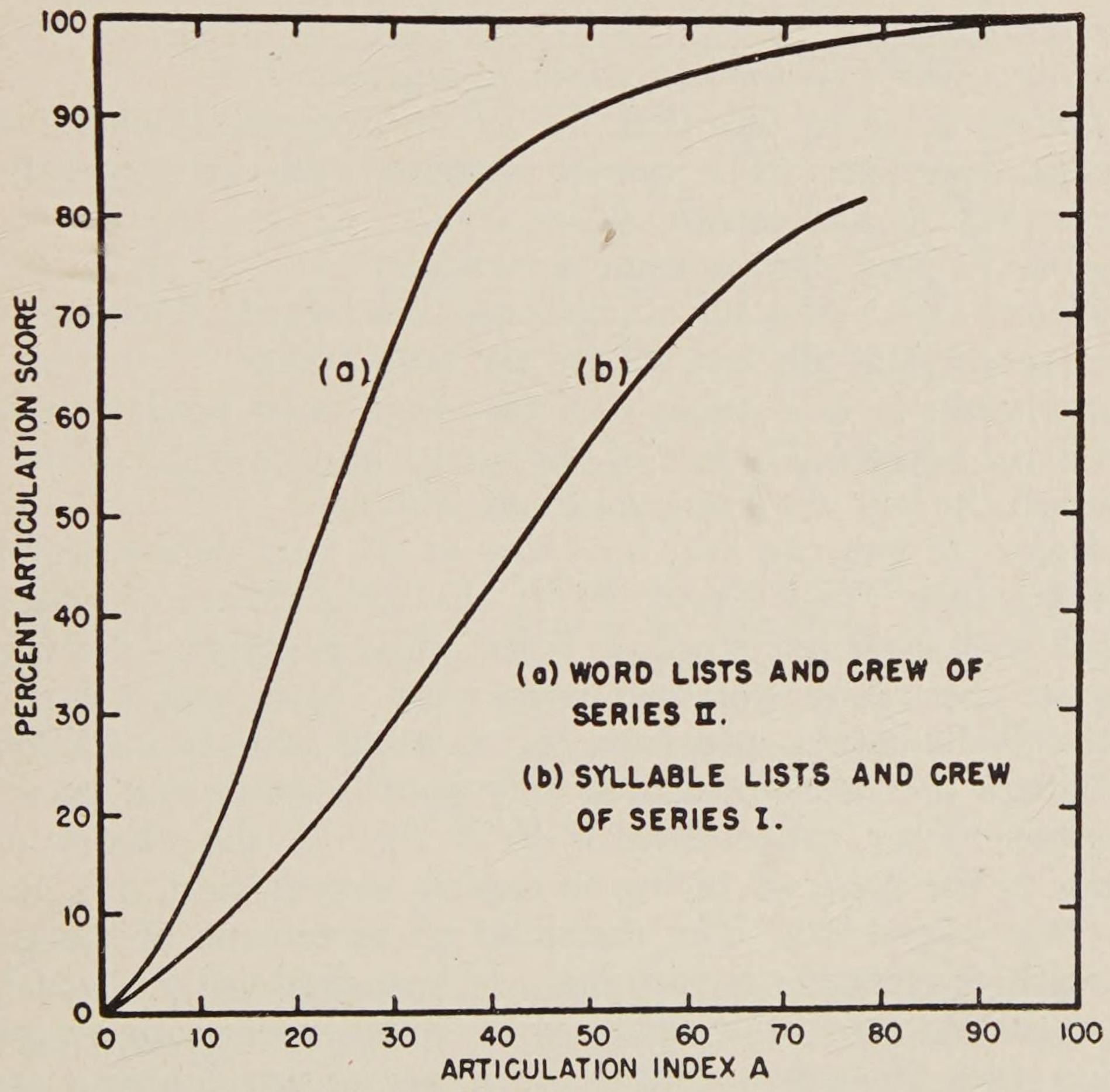


FIGURE 10. Experimentally determined relations between word and syllable articulation and the articulation index. [BERANEK, L. L.: *Proc. Instit. Radio Eng.*, **35,** *880, Fig. 11* (1947)]. Grateful acknowledgment is made to the Proceedings of the Institute of Radio Engineers for their permission to reproduce this figure.

The Bell Telephone Laboratories, who were the pioneers in this work, used nonsense syllables rather consistently. Others who have followed found it difficult to train listeners in the phonetic notation necessary for working with nonsense syllables, and they have worked largely with words removed from context.

Bigelow: What was the repetition rate of the words you used?

Licklider: As long as the period is 3 seconds or more, it does not matter very much. It is usually around 3, 4, or 5 seconds.

Bigelow: Between words?

Licklider: Between words. I did not elucidate the method of articulation testing at all. The words are imbedded in a carrier sentence to improve the dynamics—to make the talker read them more naturally. The talker says: "You will write beans now," or something of that sort. The key word, beans, is imbedded in the carrier sentence.

Bigelow: All this is for key words in sentences?

Licklider: It turns out that the carrier sentence makes practically no difference. The carrier sentence adds no meaning to the key word. It just gets the talker going.

Bigelow: It paces him in some sentences?

Licklider: Yes. We have tried the key words without any carrier sentences at all, and we get the same scores.

Hutchinson: If it is going at a very high noise level, how can he tell if the key word is part of the carrier sentence? How can he distinguish the key word in the carrier sentence?

Licklider: If you can hear anything at all, you always hear the carrier sentence. You expect it. If the same carrier sentence is repeated with each test word, as is the usual practice: "Now you will write this. Now you will write that. Now you will write the other," the carrier impresses you as being entirely intelligible, even though if it were presented only once it would not be at all intelligible. Your expectation is built up. If the theory ever develops to the point of trying to explain expectation, it is going to be very interesting. The threshold of agreement or disagreement with expectation is very low. If you diminish the vocabulary, so that instead of working with 1,000 words you are only working with 50, you find that the scores go up. There is more to it than getting some words right by chance.

We can distinguish between noise and distortion in this way: Both noise and distortion alter the form of the signal. In the case of noise, the discrepancy between the transmitted signal $f(t)$ and the received signal $f(t) + g(t)$ is simply $g(t)$, and $g(t)$ is in general unrelated to $f(t)$. In the case of distortion, on the other

hand, the discrepancy between the transmitted signal $f(t)$ and the received signal (say) $f[h(t)]$ is a function $H(t)$ that is in some respects like noise, but $H(t)$ is in general quite closely related to $f(t)$ because the operator h defines a definite operation. Noise and distortion are thus both deformations of the signal, but only in the case of distortion does the deformation depend upon the signal.

To take first the case schematized by $f[h(t)]$, where h is a constant, let me show you a slide.

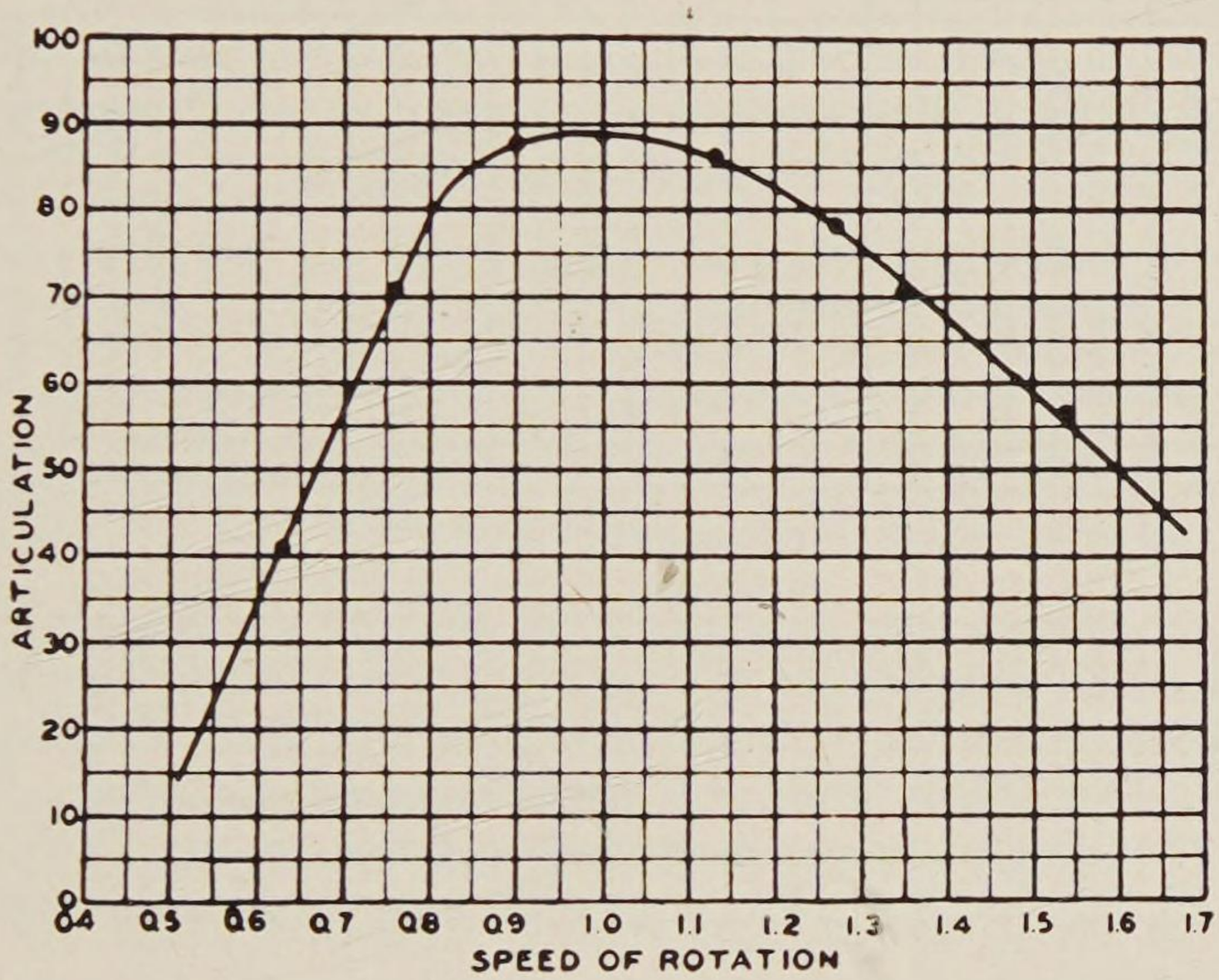


FIGURE 11. Effect upon articulation of speeding up and slowing down the passage of time or, what is the same thing, of multiplying all the component frequencies by a common factor. [FLETCHER, H.: *Speech and Hearing.* Bell Telephone Laboratories, Inc., New York, D. Van Nostrand Co., Inc., *Fig. 145* (1929)]. Grateful acknowledgment is made to the D. Van Nostrand Co., Inc. for their permission to reproduce this figure.

This shows what happens if you play a record too fast or too slow. In interpreting the graph, we can use a rough rule of thumb: if syllable articulation is over 50 per cent, a listener can get the gist of connected discourse. Applying this rule, we see that the auditory system has a fair tolerance for speeding up or slowing down the time scale.

Stroud: A little over one octave.

Licklider: If we consider our h to mean "add a constant" instead of "multiply by a constant," we get what we can call

the heterodyne effect. The influence on intelligibility is shown here:

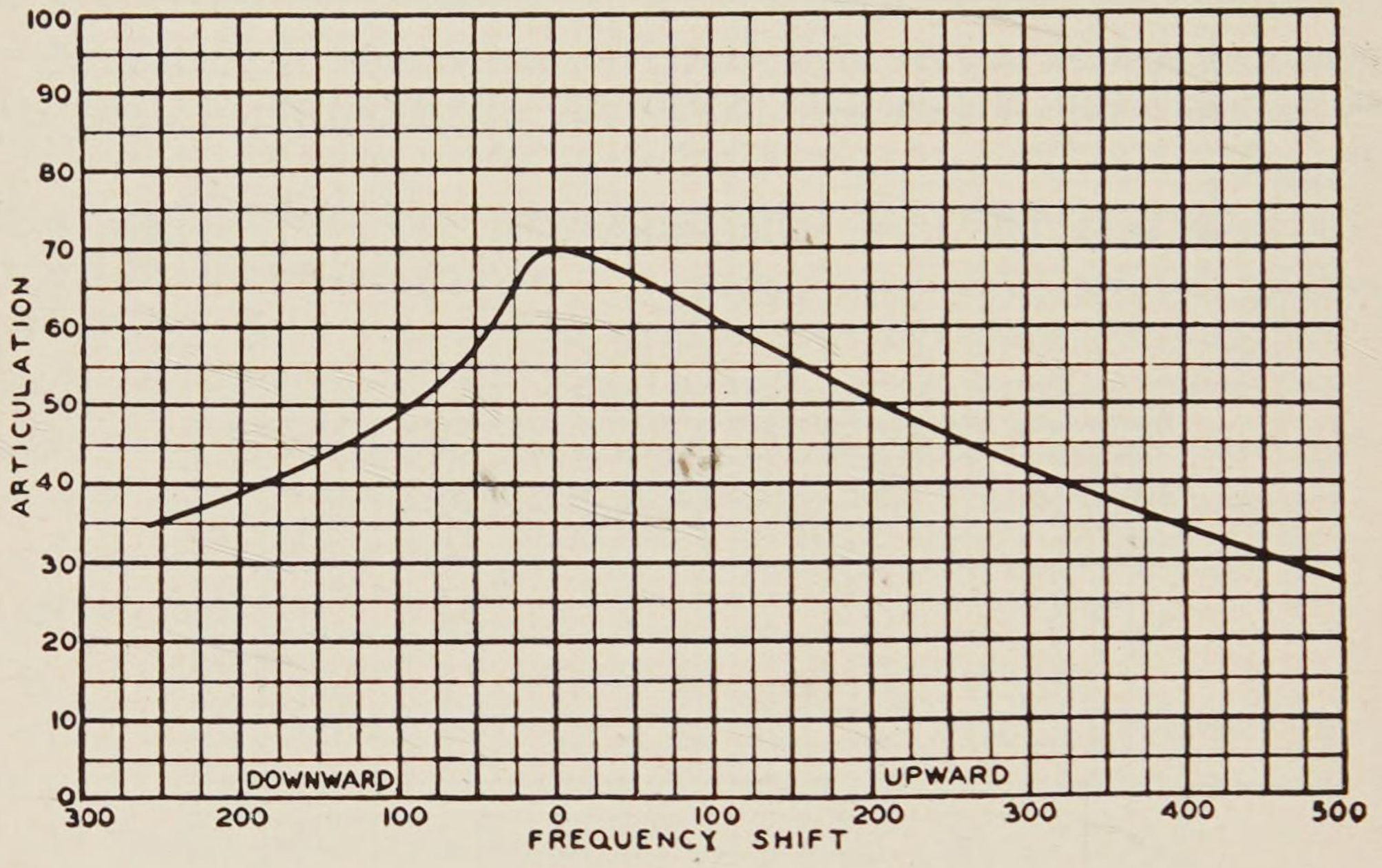


FIGURE 12. Effect upon articulation of shifting the frequencies of all the sinusoidal components a constant distance along the frequency scale. [FLETCHER, H.: *Speech and Hearing.* Bell Telephone Laboratories, Inc., New York, D. Van Nostrand Co., Inc., *Fig. 146* (1929)]. Grateful acknowledgment is made to the D. Van Nostrand Co., Inc. for their permission to reproduce this figure.

This distortion makes speech sound bad because it makes the harmonics of speech inharmonic. Nevertheless, we can tolerate a shift of perhaps 100 cps downward in frequency, and even a greater shift upward.

Savage: You slide the frequency spectrum along horizontally?

Licklider: I am sorry. I have gotten ahead of myself. I started talking about $f[h(t)]$, then switched over to $F[h(\omega)]$ without taking you in on the secret. Let me try to set things right.

We examined the effect of multiplying the time scale by a constant. This was the case $f[h(t)]$, h constant. I should have mentioned at that point that, of course, playing the record too slow by the factor h, which we did to stretch the time scale, had the effect of multiplying all the frequencies of the spectrum by $1/h$. The one set of observations therefore served for both $f[h(t)]$ and $F[h'(\omega)]$, $h = \text{constant} = 1/h'$. Then I should have said that if h means "add a constant," $f[h(t)]$ is simply $f(t)$ advanced or delayed in time. Advance and delay, of course,

do not affect the intelligibility of a single speech sample, though they might upset two-way conversation. The heterodyne effect that I introduced out of turn and without warning is the result of adding a constant to ω; it is in the domain of frequency the parallel of advancing or delaying the wave form in time. It involves shifting the spectrum along the frequency scale. As we have seen, the listener can tolerate a considerable amount of that kind of distortion, also. The speech sounds terrible, but the listener can understand what is said.

Going on to more complicated h's, we come to sinusoidal modulation of the time scale. This is the wow or flutter mentioned earlier. Actually, to get wow, I guess you have to modulate, not time t, but the rate at which time flows—but things are complicated enough as it is, and I shall simply report that work is going on now on effects of wow and flutter, and that although they annoy the listener they do not prevent his understanding the speech—not, at least, until they are so severe that they make time flow backward part of the time. Over in the frequency domain, complicated h's correspond to the inversions and garblings that are used in privacy and secrecy systems.

The effect appears when the transmitter and the receiver of a single side-band, suppressed-carrier, amplitude-modulation system are not tuned to the same frequency. The mistuning has the effect of sliding the spectrum along the frequency scale.

While we have the slide projector running, let us move along to distortions of the time function. In these distortions h operates upon $f(t)$. If h is simply a constant multiplier, it gives us linear amplification or attenuation. We have already noted that intelligibility is invariant over a wide intensity range. If h is a more general linear operator, a linear operator upon the past of $f(t)$, it gives us the whole class of frequency and phase distortions.

Perhaps the most interesting of the linear distortions is reverberation. To a first approximation, reverberation is due to echoes. Reflection and scattering from surfaces and objects set up a large number of more or less faithful replicas of the original sound, and these are all superposed at the ear of the listener. Different echoes are delayed by different amounts, so the listener actually receives a large number of inexact copies of the talker's speech, placed one on top of another in inexact register.

The effect of reverberation upon intelligibility is shown here as a function of the reverberation time.

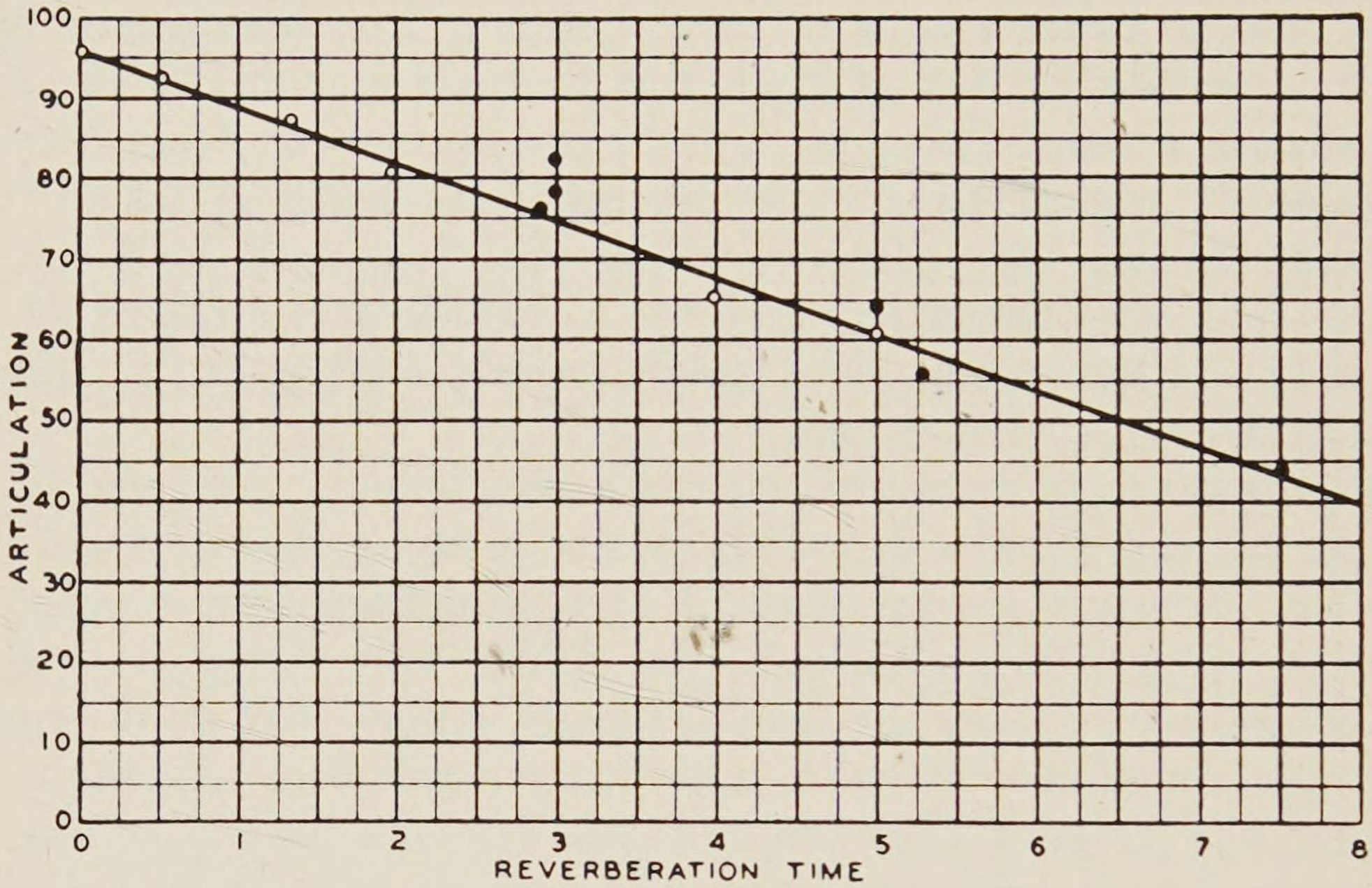


FIGURE 13. Effect of reverberation time upon articulation. [FLETCHER, H.: *Speech and Hearing.* Bell Telephone Laboratories, Inc., New York, D. Van Nostrand Co., Inc., *Fig. 147* (1929)]. Grateful acknowledgment is made to the D. Van Nostrand Co., Inc. for their permission to reproduce this figure.

The reverberation time is the length of time it takes the signal to decay 60 db when the source is turned off suddenly. For a reverberation time of 6 seconds—I think I am right about this—you have to have a tile room; in it, you get about 50 per cent syllable articulation. It is amazing to me that any mechanism can tolerate so much distortion, but it appears that the auditory system is able to pick out the first of a number of successive, overlapping presentations of a sound, to pay special attention to the first, and to ignore the rest. This primacy or precedence effect, as it is called, is important in sound localization as well as in speech perception.

Bateson: For some reason it seems to be able to do that with real speech in a room and not able to do it when listening to the same speech recorded on wire.

Stroud: If you stick your finger in one ear, you won't be able to do it in the room.

Licklider: If we now leave the lantern slides for a minute and get the lights back on, we see that we have gotten into frequency and phase distortion by letting a linear operator (e.g., a reverberant room) work on $f(t)$. In reverberation, we have frequency

and phase distortion in combination. Now it is possible, with the exercise of considerable ingenuity, to get frequency distortion without phase distortion and vice versa, but very little work along that line has been done. Some very important work has been done, however, in which it is reasonably clear that frequency distortion was the principal variable, and other work in which the same can be said about phase.

In one set of experiments, filters with sharp cutoffs were used *(a)* to pass the low-frequency components of speech while eliminating the high-frequency components, and *(b)* to pass the high-frequency components while eliminating the low-frequency components. The intelligibility of the components that were passed was measured as a function of the cutoff frequency. The results are shown here:

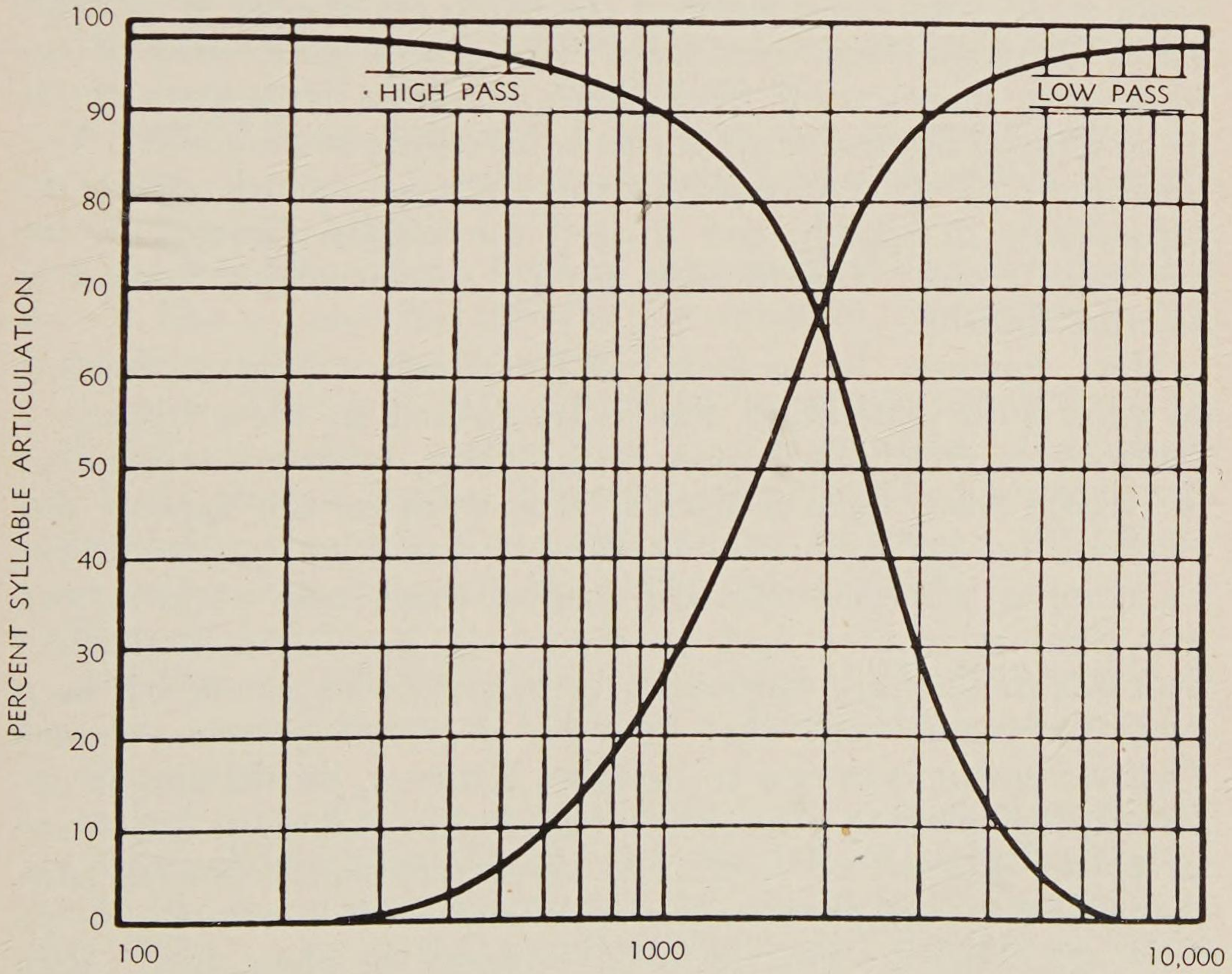


FIGURE 14. Functions relating syllable articulation to the cut-off frequencies of high-pass and low-pass filters. [MILLER, G. A., WIENER, F. M., and STEVENS, S. S.: *Transmission and Reception of Sounds Under Combat Conditions.* Washington, D. C., Office of Scientific Research and Development, *Fig. 18,* Chap. 7 (1946)]. Grateful acknowledgment is made to the Office of Scientific Research and Development for their permission to reproduce this figure.

The curve labeled "low pan" shows how intelligibility grows as we increase the cutoff frequency of a low-pass filter. The curve labeled "high pass" shows how it grows as we decrease the cutoff frequency of a high-pass filter. Note that the curves cross at about 66 per cent syllable articulation. This means that either the upper or the lower half of the speech spectrum is adequate for intelligible communication of connected discourse.

If the filter is a band-pass filter, eliminating the upper and the lower ends of the spectrum and passing the middle, the impairment of intelligibility is on the whole even still less marked. The speech components in the single octave between 1,000 and 2,000 cps will let you hear everything I have to say. Preliminary results obtained with a number of very narrow bands indicate that reasonable intelligibility can be obtained with considerably less than 1,000 cycles total band width if the bands are located in strategic places along the frequency scale. The general conclusion of the work with sharp-cutoff filters, then, is that a large part of the spectrum can be eliminated without destroying intelligibility. This conclusion points backward to my opening remark about the redundancy of speech, and toward the results obtained in the presence of noise, and forward toward similar effects I want to describe shortly.

Next, however, let us look for a moment at phase distortion, or, what is the same thing, time-delay distortion. Some frequency components are delayed more than others, with the result that the speech wave form is altered. The work on this type of distortion is not yet definitive. It is found that delaying parts of the spectrum is no worse than eliminating them, and we have seen that eliminating them had, on the whole, less detrimental effect than one might have supposed. Actually, the statement has been made for many years that "the ear does not perceive phase." That statement is shown to be false, however, by the simple experiment of playing a recording backward. Playing it backward leaves the pattern of amplitude versus frequency entirely unchanged; only the phase angles are changed. It is not—

Savage: That is a tour de force. Suppose the phase shift is only a few hundred degrees, does that really matter?

Licklider: It is quite true that you can shift any of the components up to a full cycle at its frequency and not hear the difference, provided the phase shift changes smoothly with frequency. That experiment was made by Van der Pol, who worked out a very neat phase shifter. If he did not vary the phases too fast (which is another matter), he could not tell that anything was

happening to the speech.

Wiener: That is not true of one, but it is true of quite independent shifts.

Licklider: What you say is true. The problem of phase has not been worked out definitively. Yet it is known—and there is the accumulation of many of incidental observations, and one experiment in which whole bands of speech were delayed—well, it is not at all certain that you could not work out a set of interrelated phase shifts that would have a marked effect upon speech intelligibility.

Bigelow: If you take frequencies close together and knock every alternative one by 180 degrees, the results are drastic. You can get cancellation, omega, omega plus epsilon. You knock them at 180 degrees and you get cancellation. There is no physical apparatus to do that.

Licklider: That is why I am wary of a negative conclusion about detrimental effects of phase distortion.

Wiener: I agree, but may I say that the following proves so? In vocoder work, if I understand rightly, you can divide a sound into frequency bands.

Licklider: How about holding that discussion for a minute? I should like to save for last the discussion of abstracting transformations of the speech wave.

Wiener: That will turn out to be essentially unrelated phase shifts. For unrelated reasons they are rather worse. They apply unrelated phase shifts, for different reasons, of fairly broad waves.

Licklider: One rule about the problem of phase: look at the time delays, not the phase shifts but the time delays.

Wiener: I agree.

Licklider: If the time delays are under 1/100 of a second, if you can keep them under 1/100 of a second, then you are really in no trouble.

At last, now, we have exhausted the linear operators, or at least we have talked about all the linear h's I want to talk about. I should like to consider, then, arbitrary, nonlinear h's, operating on $f(t)$. These give us the so-called "nonlinear" distortions, which have the effect of introducing frequency components that were not present in the original signal. Since I have talked too long already, I shall mention only the nonlinear distortion that is my particular favorite.

If we start off with the speech-wave $f(t)$ at the bottom of the diagram, and operate upon it by the h that is defined by one of the input-output characteristics (Figure 15), we get the wave

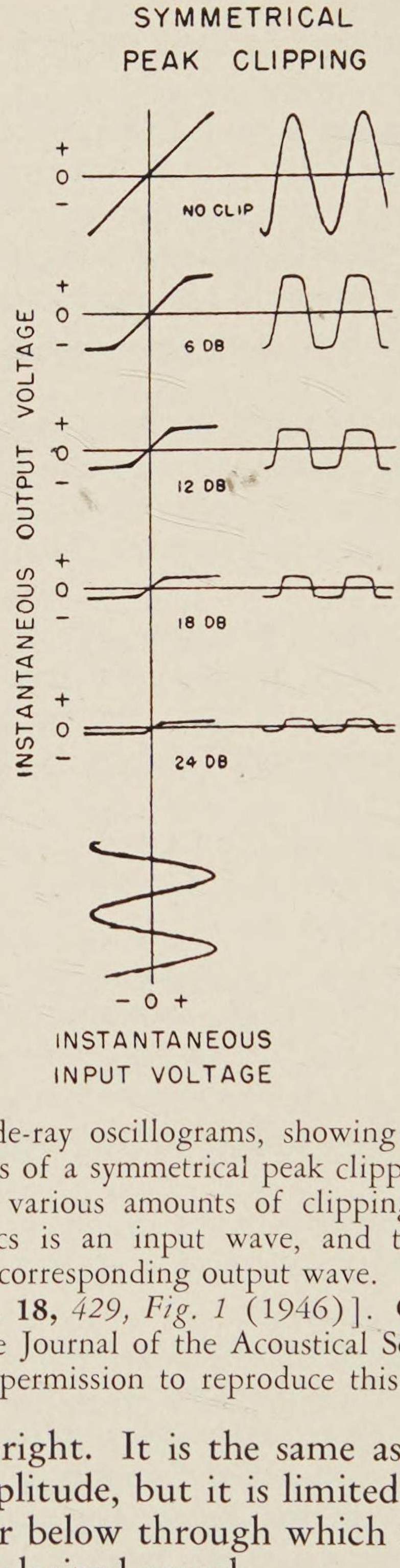


FIGURE 15. Cathode-ray oscillograms, showing instantaneous input-output characteristics of a symmetrical peak clipper. The several characteristics illustrate various amounts of clipping. Below the input-output characteristics is an input wave, and to the right of each characteristic is the corresponding output wave. [LICKLIDER, J. C. R.: *J. Acous. Soc. Am.*, **18,** *429, Fig. 1* (1946)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

form shown at the right. It is the same as $f(t)$ throughout the middle range of amplitude, but it is limited, as it were, by a ceiling above and a floor below through which it cannot pass.

Wiener: Rectangularized speech.

Licklider: Just peak-clipped speech. We have clipped the peaks off, both the upward peaks and the downward peaks. The rule is that if you do not reduce the level so that it gets below the noise, this clipping has no serious effect upon intelligibility. The easy way to perform the operation is to turn the gain up every time you reduce the amplitude by clipping.

Wiener: An interesting thing they have been doing at M.I.T. recently is to make the consonants of ordinary speech more intelligible.

Linklider: This shows how well acquainted with one another's work people are at M.I.T. I am doing work at M.I.T.

Wiener: One of the other boys told me of it on the basis of your work.

Savage: Have you heard the story?

Licklider: I am willing to listen.

Wiener: I think I got that from Levine.

Teuber: There is a certain paper by Licklider from a certain acoustic laboratory.

Wiener: I am sorry.

Teuber: It was in that, and he will tell us in a moment that clipped speech can be just as intelligible as ordinary speech, or am I misquoting the person who did the experiment?

Licklider: If we continue to lower the ceiling and to raise the floor, we get, in the limit, a sort of speech wave that has, as Professor Wiener implied when he said "rectangular speech," only two amplitude values. It is dichotomized.

Going from no clipping to infinite clipping, we find the effects upon intelligibility that are shown in this graph:

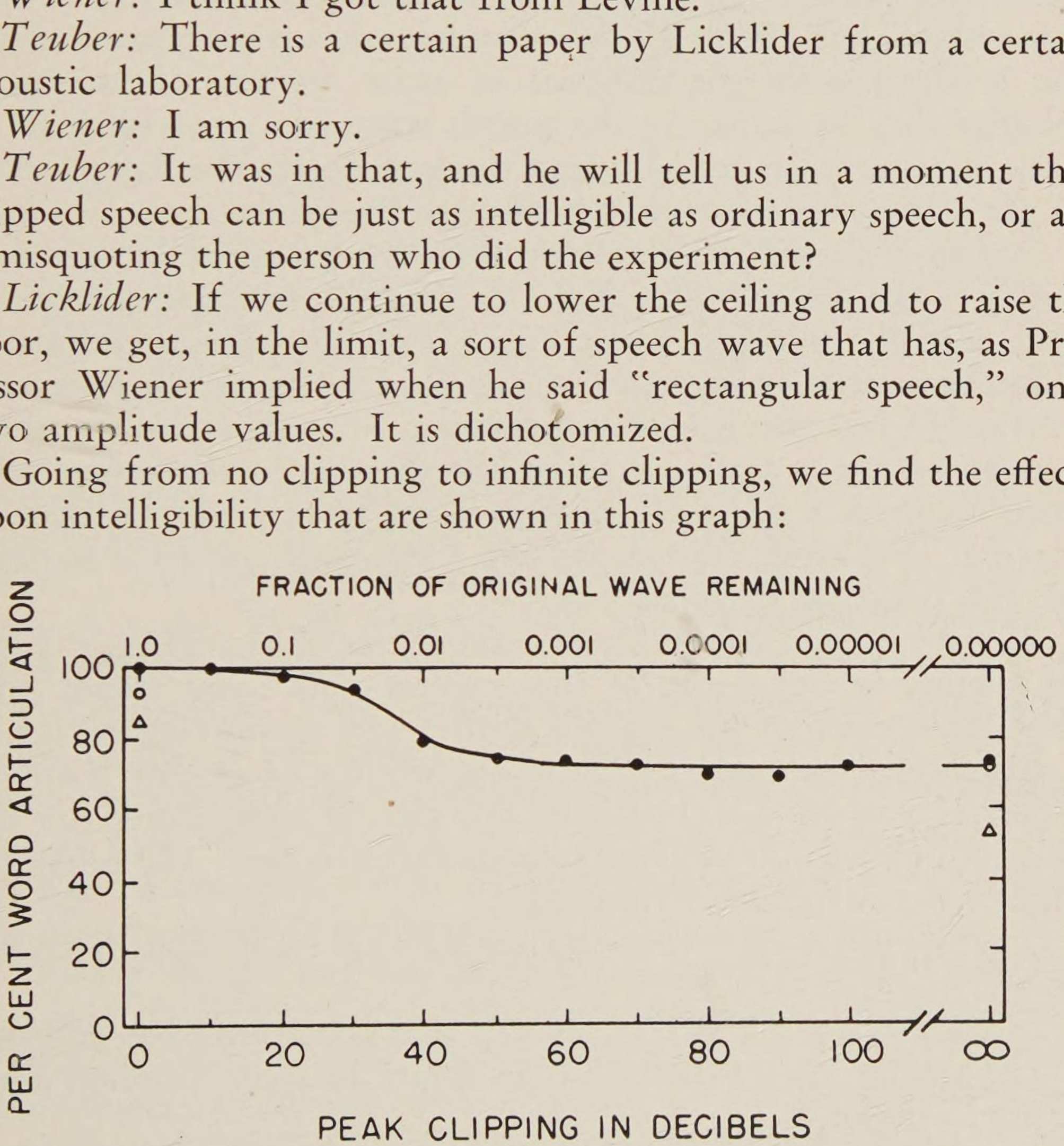


FIGURE 16. Effects of peak clipping upon the intelligibility of speech in quiet. The lower scale shows the amount of peak clipping in decibels; the upper scale indicates the fraction of the peak-to-peak amplitude of the original wave that remained after clipping and before reamplification. [LICKLIDER, J. C. R., BINDRA, D., and POLLACK, I.: *Am. J. Psychol.*, **61,** *1, Fig. 3* (1948)]. Grateful acknowledgement is made to the American Journal of Psychology for their permission to reproduce this figure.

Almost nothing happens to intelligibility until we reach 20 db peak clipping, which reduces the amplitude to 1/10 of its original value. Then the function drops gradually to about 70 per cent word articulation, and there it remains indefinitely. Calling the effect at the extreme right-hand side of the graph infinite peak clipping, we see that one understands about 70 per cent of discrete monosyllabic words in quiet. That is quite enough for intelligible conversation.

Now what Teuber suggested was this. If we are transmitting the speech over an amplitude-modulation radio system, or over any system that can handle amplitudes only up to a certain limiting level, then in the presence of noise we can obtain higher intelligibility by reducing the speech wave to a squarish form by peak clipping than by adjusting the gain to let the highest peaks of the original speech wave get through the system.

Perhaps I should mention that you do not have to hit the exact center of the speech wave. You can take any swath that is reasonably near the middle. There are in fact, an infinite number of different swaths or layers of the wave form that are reasonably intelligible, but this is hardly surprising, since there are an infinite number that are almost exactly like the one in the center. The main thing, as a practical matter, is not to miss any of the consonants. The weak consonants like *f* are easily missed if the clipper gets biased off the center axis.

Wiener: If you could displace the center of the thing, it would be roughly in the center of the energies of it. You would probably avoid most of the trouble with a sort of moving center.

Licklider: You need not go to the trouble of getting a moving center if you have a stationary one that is right along the time axis. All the sounds oscillate around the zero axis. The important thing is not to get away from that.

Savage: What does the stuff sound like? Can you describe it at all?

Licklider: You can certainly tell that something has happened to the speech. As we increase the amount of peak clipping, it goes about like this:

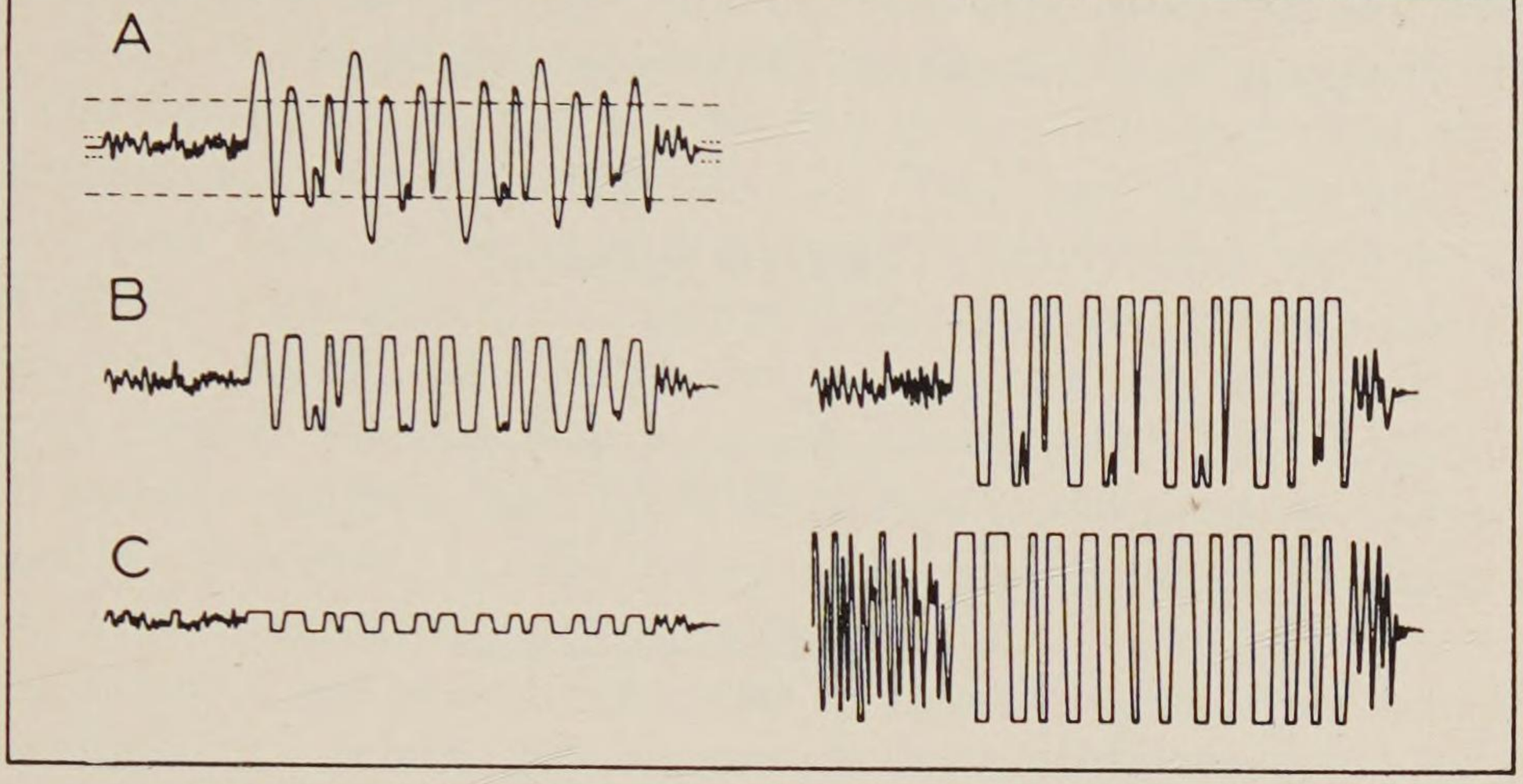


FIGURE 17. Diagram illustrating peak clipping. The waves of the word *Joe* are shown schematically at *A*. *B* shows what is left after 6 db peak clipping, i.e., after reduction to one-tenth the original amplitude. At the right, the waves of *B* and *C* are shown reamplified. [LICKLIDER, J. C. R., BINDRA, D., and POLLACK, I.: *Am. J. Psychol.*, **61,** *1, Fig. 2* (1948)]. Grateful acknowledgment is made to the American Journal of Psychology for their permission to reproduce this figure.

We clip half of the peak-to-peak amplitude away (6 db clipping); usually the listener detects no change. It may perhaps sound a little weaker. We turn the gain up and clip another 6 db (which makes 12 db peak clipping and eliminates three-quarters of the original waves); now it sounds as though there were sand in the apparatus. Then 24 db peak clipping; gravel. Infinite peak clipping makes the speech sound extremely bad, principally because the wave always has the same root-mean-square amplitude—just as high when you are not talking as when you are. The irregular rectangular waves between words sound like noise, and the noise is as strong as the speech.

Wiener: May I say one thing in this connection? They think of using the device for prosthesis. I just caught the constancy of the amplitudes, because there we are going to use electrical skin stimulation on different regions. Since we are going to work with constant voltage, with constant current density, it is very good that we don't have too variable a signal. This may actually be better from the standpoint of our prosthesis for the skin in ordinary sounds.

Licklider: Clipping assures that the speech comes through at a high level. A combination of clipping and what is known as compression—compression also smooths out variations of speech level—works well in hearing aids.

Let me mention that you do *not* want to do the opposite of peak clipping and introduce distortion like this:

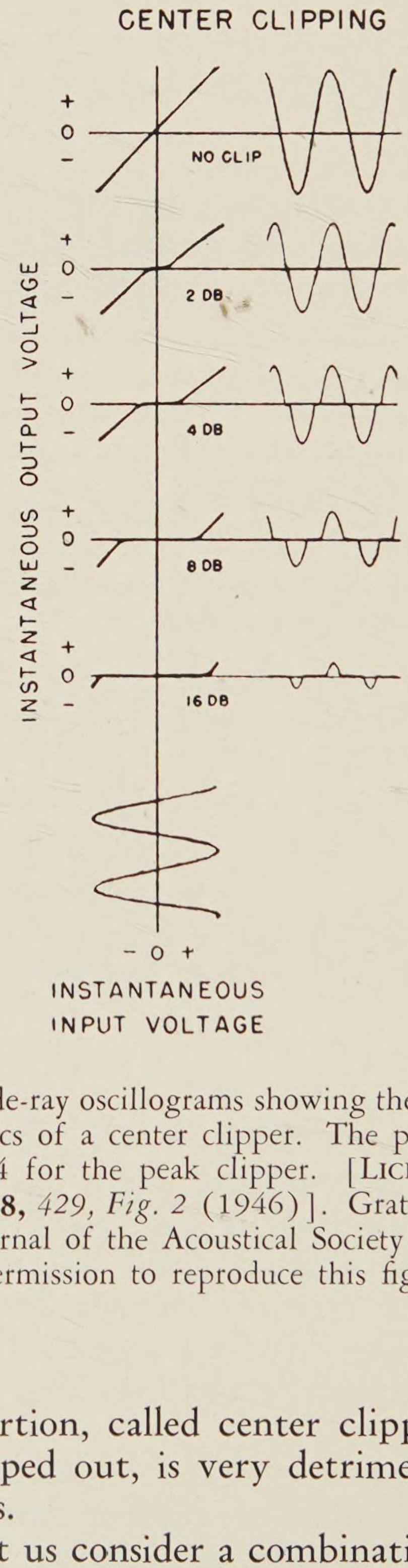


FIGURE 18. Cathode-ray oscillograms showing the instantaneous input-output characteristics of a center clipper. The plots are analogous to those of Figure 14 for the peak clipper. [LICKLIDER, J. C. R.: *J. Acous. Soc. Am.*, **18,** *429, Fig. 2* (1946)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

This form of distortion, called center clipping, since the center of the wave is clipped out, is very detrimental to intelligibility, even in small doses.

As a last item, let us consider a combination of linear and non-linear operations. A nice linear operation is differentiation with

respect to time. It turns out that the derivative of the speech wave form is just as intelligible as the original speech. Differentiate the derivative to get the second derivative, and now all the consonants are greatly emphasized. You can understand such speech; you can, in fact, understand it clearly and without unusual effort. Going in the opposite direction, we can take the time integral of speech, and then the integral of the integral. The integral sounds boomy but is quite intelligible. The double integral sounds muffled, and close attention is required to follow it. Evidently, however, intelligibility is highly resistant to differentiation and integration with respect to time.

The thing I am most interested in describing is what happens to intelligibility when you introduce both differentiation or integration and infinite peak clipping. The answer to that question is quite simple. If you integrate first and then clip, you lose intelligibility; the result is almost entirely unintelligible. But if you differentiate first and then clip, you come out with a square wave, as before, but the square wave gives you word articulation scores of about 90 per cent. It sounds terrible, but you can understand it clearly. Finally, if you introduce an integrator in cascade with the differentiator and clipper, so that you differentiate, then clip, then integrate, you obtain a wave that looks like the one in the second-to-last line of the figure, that sounds not at all as though it had at one point been an irregular series of rectangular waves, and that is no less intelligible than the result of differentiating and clipping without integrating. These observations are summarized in the following two figures:

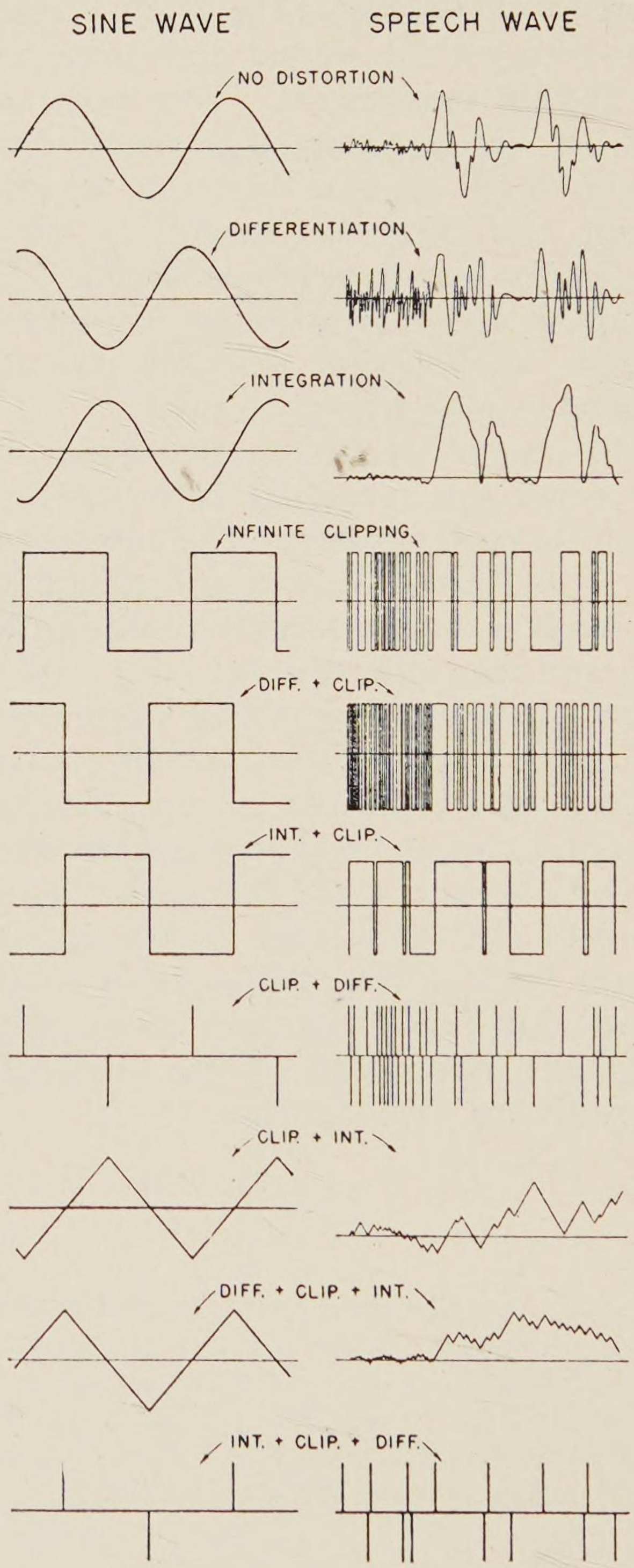


FIGURE 19. Schematic illustration of the effects of the distortions upon sine wave and upon speech waves. [LICKLIDER, J. C. R., and POLLACK, I.: *J. Acous. Soc. Am.*, **20,** *42, Fig. 3* (1948)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

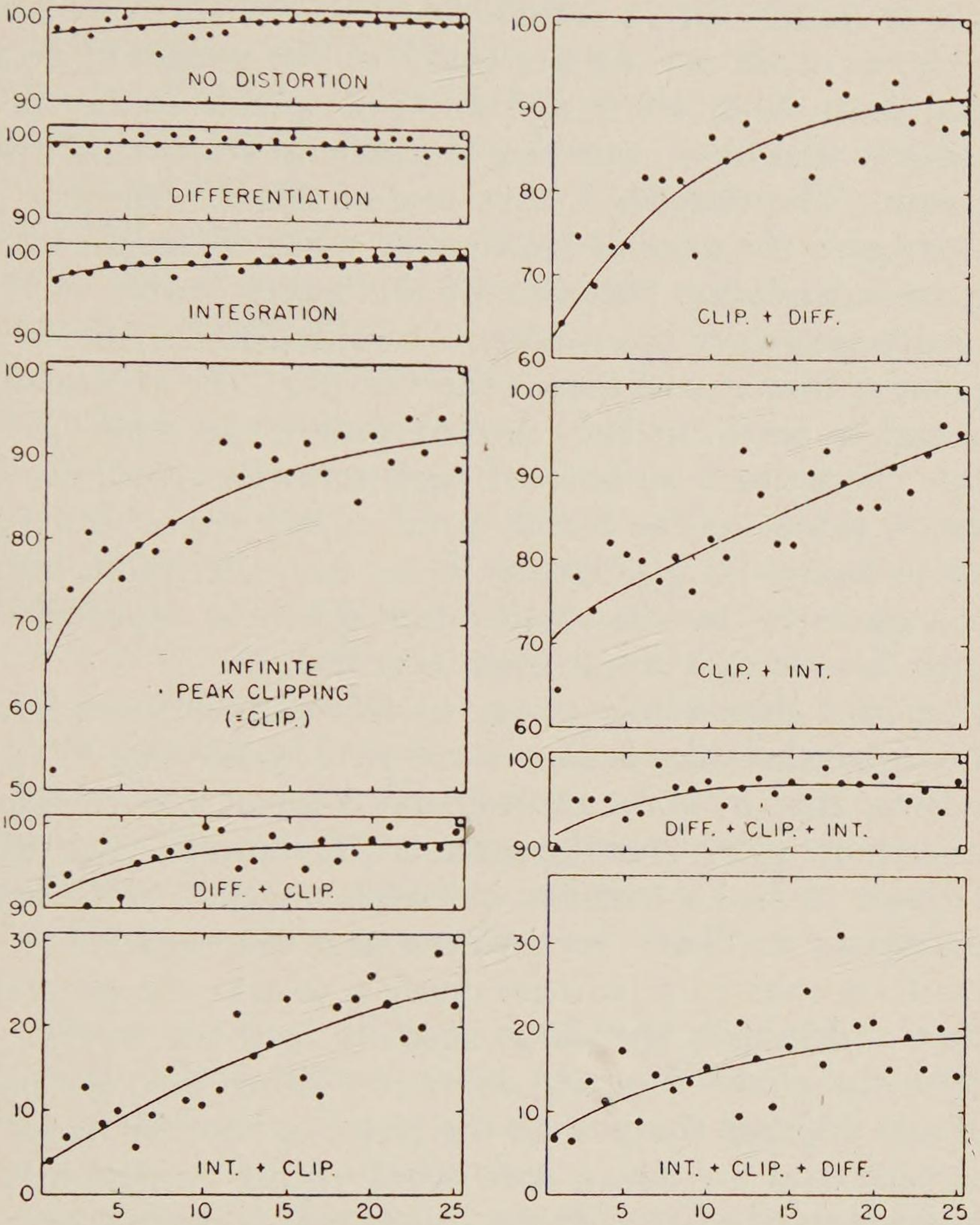


FIGURE 20. Showing the articulation scores for each of the 10 arrangements (see Figure 18) of the distorting circuits. [LICKLIDER, J. C. R., and POLLACK, I.: *J. Acous. Soc. Am.*, **20,** *42, Fig. 4* (1948)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

Wiener: Were you going to talk also about the separation of speech bands?

Licklider: I think that I must subside. In closing, let me say this: The next category of distortions that we should cover (or should have covered) is the category of abstracting transformations, transformations that intentionally throw away information,

but not the information that underlies intelligibility. Peak clipping is obviously such a transformation, but there are others. Perhaps the most interesting are based on the notion of retaining the general features of the distribution of speech energy in time and frequency, that is, in retaining the general features of the running spectrum. The vocoder is that sort of thing. Another possibility is to preserve the general features of what we might call the running autocorrelation function of the speech wave. A third possibility is to preserve the moments of the distribution of energy in frequency, that is, the mean, the variance, the skewness, the kurtosis, and so forth, of the running power spectrum of the speech wave. There are a number of such schemes afoot, most of them arising, I think, in the fertile mind of my friend Bill Huggins. I think it might be interesting if, in our discussion, some of you are particularly familiar with such schemes, especially with the vocoder, would talk about them further.

Wiener: I should like to say in this connection what the relation of phase to the vocoder work is. I think that is very important here; that is, you can interpret a lot of the vocoder work as a rather loose phase transformation. If you take your speech band and divide it into a number of bands, rectify, and then use it to modulate an oscillator somewhere near the middle of the band, you will see that what you are doing is to take the general envelope pattern and keep it the same; actually, you are doing a great violence to the phase. You are doing one thing with the phase in one band and another thing with the phase in another band. The only thing that you are doing is to work rather consistently with the phase in each band as a whole. Now that is what is actually being done with the vocoder, if I am not mistaken. It leads to admirable speech.

Licklider: It goes even further than that. A train of pulses is put into all the band-pass filters. These filters simply operate upon this impulse train to make the voiced or line-spectrum sounds.

Wiener: In other words, it is only an analogue that is preserved from each region. Everything else is mutilated terribly; you figured out how much of the information should be in from the frequency range, and the amplitude range preserved by this is less than one-tenth in many cases.

Licklider: I think Shannon has figures. It takes about 250 cycles to transmit vocoder signals, does it not?

Shannon: That can be done with 250, even less.

Licklider: It is interesting to look at it this way: the band width of normal speech is 7,000 or 8,000 cycles—even more than

that, if you want to include everything. It is possible, however, by picking the pass band from the center of the speech spectrum, to obtain intelligible communication with less than 1,000 cycles band width. Intelligible. It would not sound good, it would not be satisfactory for many purposes, but it would be intelligible with 1,000 cycles and no fancier transformation than that provided by a band pass filter. The vocoder brings it down to 250 cycles or less. But if you built a voice-operated typewriter, that is, an electronic stenographer, you could communicate through a band only 5 cycles wide, or probably even less. (This is assuming a reasonable signal-to-noise ratio, say 20 db.)

Wiener: This is just the reason why this hearing aid we are working on is so promising. Of course that is because obviously it would be impossible to do much on the skip if we had to get all the information of 7,000 cycles through. When you come to 250 cycles, there is nothing at all ridiculous in having, we will say, ten or even five recorders on different parts of the skin, each getting about 50 cycles' worth of information through. Fifty cycles isn't an awful lot. The skin can discriminate at that rate fairly well, and the result is that it is perfectly possible to build an automatic skin receiver for vibrations that one can be trained to read.

Licklider: Have you any recent data on the vocabularies of the people?

Wiener: The best thing we have done with that was a vocabulary of 12 words, a run of 80 with six errors. That is obviously not enough. But that simply shows there is enough basis for discrimination to warrant going much further.

There is one thing that I want to say about this from the general point of view of communication engineering, and that is that the measurements that Mr. Shannon and I have of an amount of infortion going over a line really need a certain comment. In view of this we have been measuring the maximum amount of information going over the line which is recoverable by any sort of terminal apparatus. Now that is very relevant if we have our terminal apparatus at our disposal. In the kind of situation we have here, concerning error with the skin receiver or anything else of the sort, the relevant amount of information is the amount that will get through both the transmission line and through what we may call the phonetic translation, translating oscillation into phonemes or their equivalent for the skin or any other sense. This information will give us no more information than we had in the beginning. There is still the maximum amount of information recoverable from this. In general, from the second law of dynamics, it would give us less information that we put in.

Licklider: It is probably of interest to set down some figures here. If you figure a speech-band width of 5,000 cycles and signal-to-noise ratio of 33 db (that is, 2,000 in terms of power), and this is at least tried for in commercial radio, then I think it appears that 100,000 bits of information can be transmitted through such a communication channel.

McCulloch: Per second?

Licklider: 100,000 per second. Now, if you speak at the rate of 10 phonemes per second and use a vocabulary of, say (to make it easy), 64 phonemes—we actually distinguish only 40 or 50, but 64 is 2^6—you generate 60 bits of information per second. This assumes that the phonemes are all equally probable—

Wiener: Yes.

Licklider: And, of course, they are not.

Wiener: Less than that.

Licklider: So the 60 is an upper limit. To get into round numbers, let us call it 50. From 100,000 down to 50. You see what sort of economy of communication facilities is possible in theory.

Wiener: There is also another translation.

Shannon: I have worked out the amount of compression possible with speech. If you made use of all statistics available, as far as I could estimate them, speech could be transmitted over a 20 db channel that is only about 2.5 cycles wide. You can compare with 20 db channels 10,000, if you have a 5,000 to 1 reduction.

Wiener: In the translation from the phonemes to semantic understanding, the words not only have to be heard but also understood, and again there is some further reduction. We really have another stage of translation that may either replace the one I have mentioned or be put on top of it, particularly when we observe animals or observe people, that is, the behavioral translation. In other words, we have what we consider the amount of information, the original theory being that we have the maximum amount of information; but as we have just said, the amount of information received and usable by the particular receiving apparatus is terribly pruned. The interesting thing is that this same sort of pruning not only comes up in speech; it also occurs with language in machines. There are many cases where we talk to a machine and listen to a machine. I am considering, for example, the remote-control substations for hydraulic power, where the power dispatcher has to get messages to the machine and where the machine has to inform the power dispatcher about significant facts. Now there again you have the same translation problem. You must distinguish the language as it goes over the line with the

amount of information going over the line, with the language as it exerts itself as behavior. There again you have the second law of thermodynamics cutting the information that you received; of course it is not good design to have it clipped too heavily, because it means a bad use of the line; but in most cases there is still probably a very appreciable amount of particulate information.

Licklider: Since we have talked about the vocoder, I think it might be of interest to report some very preliminary results of observations being made at M.I.T. now with a vocoder. It is actually an 8-channel arrangement that divides the speech spectrum into 7 channels and uses the eighth to keep track of the fundamental period of the voice. The apparatus is so set up that you can record the 8 signals (7 envelopes and 1 voice-pitch control signal) and play them back too fast or too slow. I believe that the observations are interesting in relation to Stroud's notion about how fast the brain can operate. The device lets you speed up speech without having all the frequencies go up, or down without having them drop. It is beginning to look as though you cannot increase the rate by more than a factor or two, or possibly three, over the rate of ordinary conversation, without—

Stroud: My best high is 2.5. You would be driving a man mercilessly. Two would be the maximum.

Licklider: You start to lose track of so many of the words that you cannot piece together—

Wiener: There is not time enough to get the memory working.

Licklider: You just don't run that fast.

Pitts: When you were discussing the effect on intelligibility of a sharp filter that cuts off below or above a certain amount, it appeared as if the upper curve were simply the lower curve subtracted from one?

Licklider: Oh, no! I mean to indicate that the two curves, low-pass and high-pass, intersect at the level of 70 per cent syllable articulation.

Pitts: Is it not also true that the other one is subtracted from the first, that is, in the sense in which you might then consider that the intelligibility is distributed additively in several ranges? You suggested that.

Licklider: On the logarithmic frequency scale the curves are, in the narrower sense, monotonic functions. I think, though, that you can find a scale (a distorted scale of frequency) on which the two curves would look symmetrical.

Pitts: Not if you plot them both in the same way.

Licklider: Perhaps within the limits of accuracy with which the

curves are determined. I am pretty sure you can.

Pitts: No, I mean the question is whether or not you can regard the intelligibility as being made up of additive components of different bands, suggesting roughly that that was nearly true when you had bands that were distributed over the spectrum in an irregular way, each one being narrow and spread out.

Licklider: It breaks down. Let us reduce the situation to a ridiculous extreme. Say that you take alternate one-cycle frequency bands; take all the odd cycles and compare their intelligibility with that of all the even cycles. Of course you cannot realize filters of that sort, but when you approach the situation represented by that extreme case, you get intelligibility that is too high, higher than the theory based on linear additivity of contributions from individual frequency bands says it should be.

Wiener: That whole subject is a fascinating one, and it explains why there is so much hope of working for the deaf. Has anyone tried to determine what the limits are for similar limits of compression for the eye for television and how much real information we use for intelligibility there?

Licklider: It probably should be said here that we have talked only about intelligibility. While there doubtless is in vision something comparable to intelligibility, it is not what people look for in television sets.

Wiener: I often wonder why people try to look at television.

Licklider: That defies analysis. One of the things that should be kept in mind while discussing effects (and especially non-effects) upon intelligibility is that all these distortions (I should perhaps except some phase distortions) impair speech "quality" considerably. It is amazing how sensitive the ear is in detecting slight changes in spectrum, wave form, or any other aspect of speech. The phase variation is the only one that can be carried to any considerable degree without producing a detectable effect. Intelligibility is unaffected, but differences can be heard. There are two separate questions: How finely can we discriminate, that is, how small are the changes we can detect? And, on the other hand, how strongly does our perception resist restructuring; how large are the changes that we can tolerate without losing the identity of the speech sounds and the meaning of the messages they convey?

McCulloch: Two or three want to speak here. Marquis wants to say something.

Marquis: I want to ask whether the so-called "electronic pencil" represents the same kind of compression of visual information when you have five scanning beams going across a line of print translated into electrical on-off signals in five channels?

Wiener: I think it does.

Licklider: Even better is the Telautograph. Since you are interested principally in contours, the Telautograph draws contours. It is not forced to cover the whole area of the paper. (The Telautograph is the machine they use in hotels, between the telephone switchboard and the desk, for example. You write at one end with a pencil, and another pencil at the other end of the line copies what you have written.) The Telautograph signals can be transmitted—I think William Tuller gave some precise figures once—in a fantastically small band width.

Wiener: There is one interesting thing in all this distortion. Have you ever tried to see whether you can completely destroy the listeners ability to hear music? Does anything in the way of music get through in these violent distortions?

Licklider: There are not any very good results on that. Most of the things are just casual observations.

Wiener: What are the observations?

Licklider: Ocarina music is vastly improved by clipping.

Teuber: But not violins or flutes, I suppose. Traditionally psycho-acousticians used to be very musical—people like Hornbostl, Stumpf. I don't know whether that tradition continues in New England. A musical person might take violent exception to your transformations. If we think of mere information value, whatever we get is redundant; we can safely reduce it to some iron ration.

Licklider: For understanding.

Teuber: Of a sort. It depends very much on your attitude. There are many situations where we might hear something, but we don't listen. All we notice, and care to notice, is whether someone is still talking or whether he has stopped. Then again, we can switch our attitudes and listen specifically to inflections, slight variations in pitch, overtones, or what not. Very fine differences can be detected; any of these transformations you mentioned can be detected by the listener, I presume, even though they might not grossly affect intelligibility. When you first published your observations, you put the emphasis in a different place. You spoke of equivalence of stimuli. You pointed out that the intelligibility of such distorted speech demonstrates the extent to which the nervous system is capable of disregarding differences, differences that are irrelevant to the organism at that moment. Certainly your data show how far we can go in extracting one and the same general pattern from vastly different physical configurations. To emphasize equivalence of stimuli would be more in keeping with my own bias. If you state the problem in terms of redundancy of informa-

tion, I'm afraid an important aspect of your work is overlooked.

Gerard: That points up something I have been very eager to ask. I don't know if you can answer it from all these modifications that you make. Have you come out with any fairly simple statement of just what the coding is for intelligible speech? What are the elements of matter that must be there? Are they few and simple?

Licklider: In so far as I have come to an opinion, I have come to one that has been held by several others. It is that, if not *the* most important thing, one of the most important things is the distribution of the energy in frequency and in time, the frequency and time analyses being made simultaneously. One way of saying it is this: look at the resonances of the speech spectrum (the places along the frequency scale where there are maxima of the power or energy spectrum), and follow the resonances along in time. Here, I know how to describe this. You probably are all familiar with the Bell Telephone Laboratories' work on visible speech. I brought along a slide showing one of the patterns.

Pitts: You think the results would be the same if you used Arabic words instead of English words?

Licklider: Why?

Pitts: Arabic has many more gutteral consonants and sounds back in the throat. I wonder if it is due to phonemes.

Licklider: The figure presents a representation of a sample of speech, "Unusual pictures." The plot is of this sort: the vertical scale is frequency plotted linearly. The horizontal scale is time, also plotted linearly. The darkness of the plot at a given spot represents the envelope amplitude of the output, at a particular moment, of a band-pass filter centered at a particular frequency. Figure 21. As you see, each of the sounds has a characteristic pat-

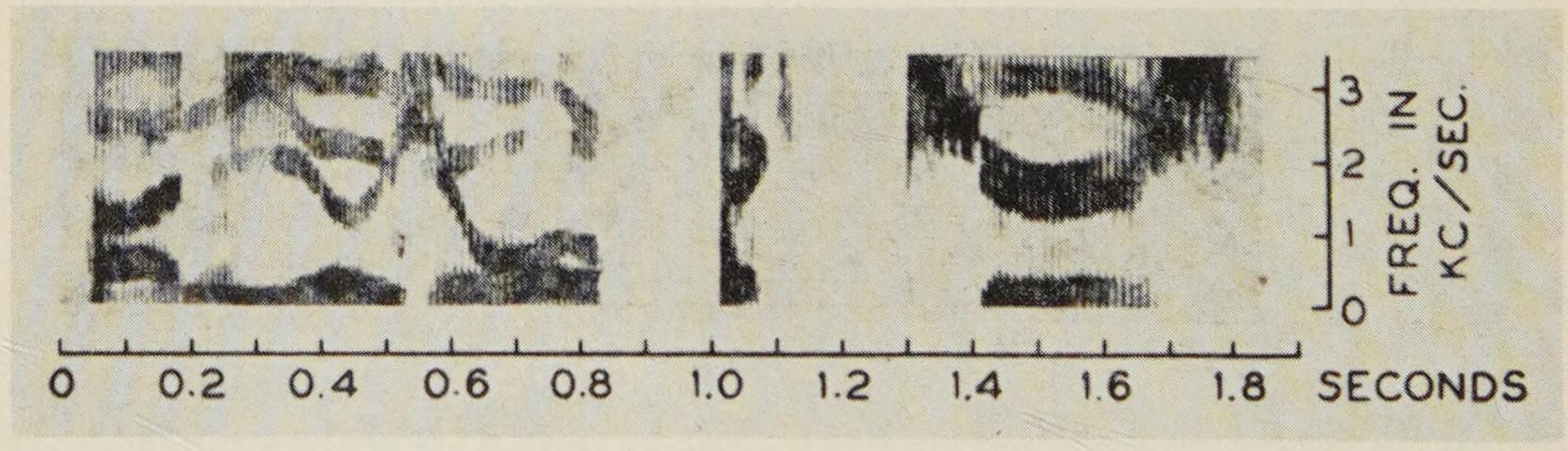


FIGURE 21. An intensity-frequency-time pattern. This visible speech pattern shows how the resonances or concentrations of energy along the frequency scale vary as the talker, saying "Unusual pictures," progresses from one speech sound to another. [POTTER, R. K., and PETERSON, G. E.: *J. Acous. Soc. Am.*, **20,** *528, Fig. 1* (1948)]. Grateful acknowledgment is made to the Journal of the Acoustical Society of America for their permission to reproduce this figure.

tern. In the vowel patterns there are concentrations of energy in frequency; these are called formants. You can see in the figure how the formants move along, sometimes swinging up in frequency, sometimes down. Now, one strongly supported opinion is that the important thing for intelligibility, the basic clue to the identity of a speech sound, is the pattern in frequency and time taken (in so far as frequency and time can be so taken) simultaneously.

I have the notion that, for the hearing of consonants, especially, there are other quite important features. The durations of consonant sounds, quite aside from considerations of frequency, are important, perhaps crucial in the case of some consonants. It is an interesting possibility that the auditory system either works simultaneously with two or more compromises for resolution in time and frequency, one part of the system resolving accurately in time and another resolving accurately in frequency, or that it skips nimbly from one compromise to another. I think that something of the sort is a definite possibility, but, as so often is the case, there are no really crucial data.

Stroud: If you make up a sound recording which is the same as listening to sounds in one ear, such as you get from this speech recorder, and it includes a minimum number of the multiple paths of transmission, it is possible to get a sense of the relative depth of the source's radial distance, or range, from a single-ear perception, whether you hear it over one or both ears, whereas it takes two ears to get the bearing. One ear will give you the range. Unless you have some precedence computer, it is difficult to imagine how you would get that information that *A* is in front of or behind *B*. That is not all you get. You get subjective enhancement of the loudness, although the physical intensity of the stimulus is unchanged between a single channel recording of a sound and a recording which includes the same sound pattern transmitted with the slight time delay over several paths. The physical volumes can be equated, but the one with the multiple paths which also gives you the sense of radial range is subjectively louder, although it may be equated to physical equivalent power.

McCulloch: Will someone say something about click loudness? I think that is one of the peculiar things that should be described.

Stroud: Last summer Dr. Garner at Johns Hopkins took several subjects. We have a little device which rather brutally clips out a short section of a sinusoid. If you clip it out very brutally, of course, you get not only the frequency from which you clip but many other frequencies. Under these circumstances Garner, clip-

ping brutally with naive subjects, found that a sample, perhaps a few milliseconds long, 2, 3, or 4, was quite a good sample, whereby a subject could estimate the loudness of the continuing sound, although he is using, in my terms, only about 2 per cent of the available information. A very short sample, 3 or 4 milliseconds long, was good for an estimate within a decible or two of the amplitude. How loud would it be if I heard it all the time? You have to be quite self-conscious about it. A little later I was working by a rather more difficult method, using filters which restricted the frequency components quite sharply, which meant that I could not make a nice square bundle of sounds, but that I had an envelope which rose and fell in almost probability shape distribution for sounds. However, I found that once you had the idea of what you were trying to discriminate, in this case a sample in which 2 or 3 of the waves passing through this filter reached their maximum amplitude at a thousand cycles, it was again a sufficient sample to estimate the amplitude of a constant signal. The method of estimation, by the way, was to set this same signal at a constant amplitude to the equilavent loudness.

McCulloch: Can you imitate the change in sound at all?

Stroud: Subjectively the sounds at short levels go *tick-tick-tick*, and you have a difficult time assigning any particular frequency to them. You can tell one tick is a little higher or lower than another tick, but to match for frequency is difficult. You miss by an enormous amount. As the samples grow a little longer without getting appreciably "click" louder, they go *tock-tock-tock*, then *beep-beep*, then *bleep-bleep*, and finally *e-e-e-e* and you have practically an exact replica of the sound. Apparently, whenever you fill up about 100 milliseconds of the sample, you have got about as good an estimate of the pitch as you will ever get, but you need to fill up only 3 or 4 milliseconds of it to get an estimate of how loud it is in the click sense.

Licklider: I think the repetition rate of the pips is an important parameter.

Stroud: You do your best in estimating the famous ten pips.

Teuber: Can you do it with single clicks?

Stroud: I suspect you can do single clicks with a great deal of practice. All you would do would be to smear your statistics out. That is all. I may be wrong there. Certainly if repeated, say ten times a second on these very short little bursts, it is not very difficult to make a good estimate with a sample a few seconds in length.

Werner: That brings out one point concerning the practice effect. Have you made any studies on training methods?

Licklider: Listeners or talkers?

Werner: Listeners. The practical problem involved here would be to see whether there are certain methods of training which are better than others and which would bring about a quicker adjustment to this type of verbal stimulation. As a psychologist I am particularly interested in two things: the practice effect dependent on training methods and the individual differences in the rate and kind of adjustment.

Licklider: During the war, especially when the military was picking people for fairly crucial jobs involving listening—the military had the notion, as you probably all know, that everything must go through many links of a chainlike communication system before it becomes really official—they were interested in the problem of training listeners. Quite a bit of practical cut-and-try work was done to find out how to train listeners, and talkers, too. As far as listening is concerned, the conclusion was this: the way to teach a person to listen is to provide high motivation (you can do that in a number of ways), to provide lots of speech for him to listen to, and to provide knowledge of results. (The listener must know whether he heard it correctly or not.) It is important, also, to verisimilitude. The test situation must be quite like the situation for which you are training. As a matter of fact, in articulation testing work it has been found that after much experience with a particular kind of speech and a particular communication system, the crew will become very proficient. The scores will be uniform. Then, when a somewhat new form of distortion is introduced, or when the speech material is changed a little, the scores drop and a new learning period begins. There is a considerable amount of transfer, of course, but the degree to which the learning is specific to a given communication system or to a given type of distortion is nevertheless impressive.

Werner: You are not speaking of testing a specific method.

Licklider: A new crew improves throughout a period of two or three months before it levels off on a plateau. Then the specific secondary improvements extend over intervals of perhaps a week or two.

Werner: You are obviously not concerned with the development and the testing of training methods. You present a person with a great variety of distortions and measure his ability to perceive.

Licklider: There has not been enough work done for me to say anything specific about that question. Actually, I was never personally active in the work on training; colleagues in the laboratory were doing it.

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McCulloch: Have you any idea what human variability is at the level they reach at the end of training?

Licklider: I know less about that than about their initial levels. Let me quantify variability in terms of percentage of words recorded correctly. I should say that if you run enough tests to get stable scores for individuals in a group of 20, there might very well be one person getting 25 per cent correct and another getting 75 per cent correct. The figures presuppose enough tests to rule out fluctuations of measurement; the range of individual differences is that great.

McCulloch: If you keep training that group, do they pull very close together as time goes on?

Licklider: What happens is this: as the scores get higher, they bunch closer together; but that fact is not significant, because the better listeners run into the ceiling of 100 per cent articulation. The thing to do is to introduce stress (in the form of noise or distortion) and thereby reduce the scores to the initial level. If you did that, I do not think that the spread of trained listeners would be much less great than the spread of untrained listeners. It might, but I do not think the effect would be very impressive.

McCulloch: They might? May I ask one more question? Is there any variation between those who can pick up syllables easily, those who can pick up words easily, or those who can pick up phrases or sentences?

Licklider: I don't know. I wish I did.

McCulloch: Does anybody have a measure?

Licklider: Perhaps the Bell Telephone Laboratories have.

Marquis: Didn't you find a lack of relationship between individual differences in this ability and intelligence?

Licklider: Very low, if any, correlation between the intelligibility test performance and performance in receiving code. There was an extremely low correlation, negative, perhaps —0.17.

Pitts: Significant.

Licklider: Just about.

Marquis: Code?

Licklider: Spoken words and telegraphic code.

Wiener: I think it might be.

Mead: If you talk about another kind of information, if you are trying to communicate the fact that somebody is angry, what order of distortion might be introduced to take the anger out of a message that otherwise will carry exactly the same words?

Licklider: I do not know. Peak limiting, peak clipping makes speech monotonous, and I would strongly suspect that some of the

clues to anger would be eliminated by clipping. But one thing seems to be quite true: even these emotional things are represented in a rather redundant fashion. The clues appear in amplitude and in time. The pace of the speech and its intensity are affected together. I think it very unlikely that you would lose the angry quality of speech by washing out, say, the amplitude variations (variations in the envelope of the speech wave).

Pitts: It is difficult to express resignation if you have to shout at someone.

Wiener: It would also be understandable in dead-pan tone. I do not think that this is good. I mean the tendency to convey emotion by limiting the time might be a very serious one.

Pitts: Time amplitude and possibly general drift in pitch from the beginning to the end of the phrase.

Mead: Perhaps the most essential point would be sincerity. If we are going further into this continual broadcasting between countries, or use of broadcasting in various sorts of general communication, what does and does not sound sincere might be something that you could analyze. Our commercial broadcasters are purposely insincere. They must do something. The question is whether that would be manipulatable.

Savage: You mean nobody could be that insincere without special training.

Wiener: I don't think so.

Pitts: It depends upon the person receiving. I imagine that to a Midwesterner anyone who speaks with an English accent sounds insincere automatically.

Mead: That is another kind of effect.

Licklider: The word "insincere" is used differently by different people.

Klüver: I should like to bring up a point which has fortunately or unfortunately no bearing on the international situation. In listening to talking parrots, you may discover that the speech of at least some parrots appears indistinguishable from that of the human teacher. My own observations are chiefly based on parakeets. After hearing some talking behind my back, I have again and again been obliged to turn around to determine whether the words or the sentence had been uttered by the parakeet or its human teacher. I am wondering whether this identity of auditory impressions merely represents some form of equivalence or whether the spectograms of the parakeet's and the teacher's speeches would be more or less the same.

Licklider: My guess is that if it sounds the same, the spectrogram

is going to be quite the same, but I do not know for certain.

Klüver: I think it is important to know whether such a striking "identity" of auditory impressions could be associated with widely different spectra.

I should like to return for a moment to the question of motivation mentioned by Dr. Licklider. Perhaps Dr. Werner will remember the old observations by Kalischer on the aphasia of parrots [Kalischer, O.: Das Grosshirn der Papageien in anatomischer und physiologischer Beziehung. *Abh. d. K. Preuss. Akad. d. Wiss.* (1905)]. Kalischer performed brain operations on talking parrots and discovered that he could produce speech disturbances by injuring the mesostriatum in the neighborhood of the Sylvian fissure. He made some very interesting observations on the restitution of speech in these aphasic parrots. After the operation, the parrots regained their vocabulary by themselves (first the rhythm, then the vowels, and finally the consonants) without ever being taught again. The birds behaved as if they were aware of errors and imperfections in their pronunciation and gave the impression of purposely trying to eliminate such defects by assiduously training themselves. At any rate, they were heard practicing the same words or even parts of words for hours at a time until their pronunciation was flawless again. As pointed out by Poetzl (Vienna), this is in striking contrast to the behavior of human aphasic patients. Apparently the "motivation" of an aphasic parrot in trying to regain normal speech is far stronger than that of a human aphasic.

Licklider: That is lack of cerebral cortex.

Klüver: I believe one of the more interesting things a psychoacoustic laboratory might do is to analyze the speech of normal and aphasic parrots.

McCulloch: If you wanted to make a Fourth of July oration, which of the modes of distorting speech would you necessarily eliminate?

Licklider: Certainly you would want to eliminate clipping. You might introduce a certain amount of "expansion." You can heighten the effect by increasing the dynamic range of speech artificially. I think you would want to avoid flutter. Flutter at a rate of 7 to 10 per second introduces a quality that suggests uncertainty on the part of the talker. Your bold, declamatory statement would fade into a question.

Pitts: Because the frequency changed also.

Licklider: It is rather hard to say. I think you want to keep the dynamics, that is, the rates of change of intensity, as pronounced

and definite as they are in the original speech.

McCulloch: Has anyone done this trick of taking a speech said perfectly flatly and then putting in these things that give dynamics to it, in order to see what happens?

Licklider: We tried blowing clipped speech back up by modulating the clipped wave with the envelope of the unclipped wave. That made it sound considerably more natural.

Bigelow: Does it change the intelligibility?

Licklider: Not much.

Fremont-Smith: In the sense of having more effect on it?

Licklider: Yes.

Fremont-Smith: Is it possible to make a friendly speech hostile, or vice versa, by building it up in a particular way after it has been clipped?

Licklider: That is a very interesting possibility. You might try, and this can be realized easily with the aid of electronics, having one person modulate another's words.

Gerard: The voice of Jacob in the words of Esau.

Fremont-Smith: Kukla, Fran, and Ollie on television. One character in that simply makes the sounds "Tui, tui, tui." Yet he carries on a conversation by modulations of "tui, tui, tui."

Licklider: One of the things we have been interested in doing but have not yet done is to start with random noise and see what we can put into it by such methods as imposing upon it the envelope of a speech wave.

Mead: When you say you can take the modulation of one person's speech, do you mean, for example, that you can take Hitler's speech, analyze it, and take Roosevelt's speech and analyze it, and then, in effect, have Roosevelt give Hitler's speech?

Pitts: That would be difficult because of language differences.

Licklider: Yes.

McCulloch: Not having listened to these test devices, I should like to ask a question about something that puzzles me. It is fairly clear that you can practically destroy everything that I have thought necessary to speech and still have speech that is speech. To what extent is that translation and to what extent is that still speech?

Wiener: It is still speech, sir, for this reason: it is understandable without special new training or with a minimum of new training.

Licklider: That can be qualified. I can present some figures. If we clip speech until all that is left is the series of rectangular waves we have mentioned, and present this to an articulation crew with no practice, we get scores of 60 words in 100. After perhaps a

month, they go up very nearly to 90 words in 100.

Wiener: But the point is that 60 is a conspicuous contribution.

Licklider: It is high enough for connected speech. As soon as you talk in sentences, say, "Pick up the yellow pencil," or, "Get me the pliers," it is possible to have the command obeyed immediately, that is, if the listener has any intention of obeying.

Wiener: Isn't the training that you need the kind that a man needs who is deaf, that is, the present-day hearing-aid kind?

Licklider: Perhaps less training than that involves.

Pitts: I wonder more and more how much the results of the degree in variance of given kinds of distortion depend upon the statistical distribution of the phonemes which are present in the thousand words which are possibilities and upon the degree to which different phonemes are fairly likely to be confused or present among them. I wonder if you might not get very different results if you added collections of words with quite different statistical distribution of phonemes in which there might be more or less confusing ones. As to the relative freedom from variants upon a given source of distortion, say, in language of different types, comparing Arabic with—

Wiener: Hawaiian would be the extreme.

Pitts:—with the consonants of almost any other language, would the results be different?

Teuber: When you put speech through transformations to make it unintelligible, I suppose you want to be sure that you can make your transformations in reverse and get the speech sample intelligible again. This is the principle of a secrecy system: you want to preserve potential intelligibility. But during the war, there also was the much simpler problem of "jamming"—destroying intelligibility by superimposition of some extraneous sounds. Maybe you are not free to tell us how it was really done, but I heard that one of the most effective ways of jamming speech was to superimpose on it some other speech—a jabber of voices—preferably in the same language. This sort of jamming is an experience we have here at times. Now I wonder does it have to be American speech for optimal jamming of American speech, or is it equally effective to use Chinese to jam American speech?

Licklider: I think that Chinese would interfere with American almost as much as American would.

Wiener: May I say there are really two jamming problems. In one you want to jam the other person from getting anything. If you take a transformation of speech that is meant to be gotten back from the apparatus, am I right that after a while anything

that you do to the speech to make it unintelligible where it is supposed to be recoverable by proper apparatus sooner or later is understood by the ordinary ear if the timing is not too badly changed?

Licklider: I am not so sure that that is true.

Fremont-Smith: Without changing from time to time, keeping one system constant?

Licklider: I cannot answer that. You could make the system complicated enough so that it would be unlearnable, I think. For example, you might take the speech spectrum, separate it into parts with the aid of filters, invert these parts, heterodyne them to other parts of the frequency scale, and then put it back together; and you might also change the recipe for the mixture at frequent intervals. Then I don't think anyone would learn to understand it.

Wiener: To go further, that is what one would have suspected in the beginning.

Shannon: During the war some of the secrecy systems that we developed used much scrambling of frequency bands. However, they were directly or indirectly understandable; therefore, it is obvious that you have to do a lot more than you think you do.

Pitts: Possibly anything you do that does not involve changing over too great time intervals would be penetrable or nearly so?

Licklider: One scheme that permits complete recovery of the speech is this: you send two signals in adjacent bands. One of the signals is speech entirely masked by noise. The other is the noise, the same noise that is used to mask the speech alone. At the receiving end you simply subtract one signal from the other, and out comes speech. There is no possibility of learning to listen through that, because the speech can be masked so thoroughly that it is dead and buried.

Wiener: You ferret it out, in other words?

Fremont-Smith: You cannot learn selectively to obliterate by virtue of attention?

Licklider: You cannot learn to hear speech through noise that is so strong that you miss by 10 decibels being able to hear the speech through it.

Gerard: This is a good chance for me to ask a question that has been on my mind. With your fully clipped speech, people at first get 60 per cent and improve to 90 per cent. Have you any evidence whether comments that would be pleasant to them are recognized more easily than those that are unpleasant; that is, is there any kind of affective factor in giving meaning?

Licklider: I don't know. There is, however, an observation that seems closely relevant. When speech is so badly distorted or masked that you cannot hear the words, then Freudian influences appear to exert themselves and to determine what is written down.

Gerard: That is, perhaps, another story.

Wiener: It is well known that people who are so deaf that they cannot follow conversation are not to be trusted not to hear things you don't want them to.

Licklider: That would make it very difficult to measure the thing you suggest.

Gerard: You still have right and wrong answers as your objective measure.

Fremont-Smith: Unpredictable, desirable commands.

Gerard: One so often hears the statement referred to that the deaf person hears what he wants to and does not hear what he doesn't want to. That would be a way of seeing whether it is so objectively.

Savage: That pertains to the deaf or the psychology of the people around the deaf?

Gerard: The psychology somewhere.

Fremont-Smith: It would be very important to do a hypnotic experiment on the problem of unhearable sound. I am not at all sure that you can say when sound is unhearable unless you do a hypnotic experiment. We do know that under hypnosis in the waking state, in the normal state, one actually perceives and records a mass of material which is unrecoverable except under hynosis. It seems to me that that might lead to interesting experiments.

Licklider: We once had a student who was interested in doing just that. He made the mistake, however, of doing the converse experiment first. I think he measured the masking of a tone by a hallucinated noise. The student was not supposed to know the literature, or enough about hearing to know how the experiment would come out if the noise were real, but the result he obtained with hallucinated noise was almost exactly what a real noise would have yielded. So we lost confidence in the whole thing.

Mead: I should like to get back to the question: Is this translation or isn't it? What is translation? If you take a very literate Slav who thinks there is something called *Slavic* and who speaks three of four Slavic languages, thinks of them as Slavic and believes firmly that there is something called Slavic, and that of course anybody who is erudite can speak them all, is he translating from one to the other or has he made a central point of reference

toward which his attitude varied? In contrast, if you took the average American who had never learned a foreign language, you could teach him two absolutely separate Slavic languages and he probably never would discover they had any stems in common. If you took two groups of people who had learned to handle these squared sound effects and told one of them that they were going to learn a completely new system of communicating English words which would be systematically transformed in a particular way involving translation, and if you told the other group of people that they were going to experiment with a lot of words which would involve a slight difficulty in hearing those same words, in one case would it be translation and in the other not? Can you define translation objectively instead of subjectively?

Wiener: I don't think that is possible. The experience you have spoken about is very interesting. I noticed when I was in Mexico, for example, that understanding a foreign language which is close to one's own depends much upon the degree of sophistication. The ability to understand a foreigner's mispronunciation varies greatly between an uneducated person and an educated one.

Pitts: Isn't that what the psychologist called the question of definition of stimulation?

Werner: Equivalence of stimuli.

Mead: Having questioned if this is translation, are we talking about stimulus?

Pitts: Every time we hear English we are in a certain sense translating. What we hear is somewhat different versions of the same word. The recognition of them as the same I suppose you could describe as an act of translation into the common exemplary, which is the Platonic idea we hold in our heads. Everybody's education is slightly different.

Wiener: A Spaniard who has never studied Italian can still follow Italian without studying it, but it is quite difficult for him to speak it if he has no idea that they might be related.

Stroud: I had a very curious experience in England. England has more varieties of English than we have. I had been there a week. I was rather naïve because I thought that *Pygmalion* was a wonderful play and I was sorry I was going to miss it. After I had been there about a week, I encountered two soldiers in a railroad station. One of them was trying to tell the other when the next train went to London. (Later, after many months of education, I finally realized that one was from Ayrshire and the other from Devonshire.) I had little difficulty in listening to either of them and I was wonderfully fascinated by the thick English

dialects. Brave Boy Scout that I was, I thought there was difficulty in hearing and began translating for them. Both listened to me. In no time at all we had the whole affair straight as to who was going on what train and where and when. A little later I discovered that all Englishmen understand two varieties of English: BBC and American movies. I, being an American, was perfectly listenable to by anybody in the British Isles. How do you sort that one out? Here were two people in need of a translation, yet I was not translating either form of English. It was still English to me.

McCulloch: That is just the point.

Wiener: My sister had a similar experience in a train between Bavaria and Hamburg. A woman from Rosenheim—she pronounced it "Roosenhame," and my sister had never studied Platt—had to translate between the two German women who could not understand each other.

Klüver: How far have you gone in analyzing the speech disturbances of neurological patients? For example, have you been interested in sensory aphasia?

Licklider: I have done nothing of that sort at all. There has been relatively little work of that kind, at least by people interested primarily in intelligibility.

McCulloch: May I put it a little differently? What puzzles me is what happens *when* something stops being stimulus equivalent and actually goes over to a process of translation. I don't know any objective way of testing it. I know, for example, my own difficulty in the Midwest was with the word "Umurukum." It was rather surprising. It was all right with the word "American," but there are many words whereof I simply didn't know which of two or three words a man meant. I used to try a literal process of translation in those cases, whereas ordinarily I don't. Having lost the distinction between the vowels, I would suddenly land at the word I didn't know and I would have to translate it. Does it mean this or does it mean that?

Licklider: This stimulus—

McCulloch: The name of the railroad station is one of the worst examples, that is, when they call off the name of the railroad station.

Hutchinson: I had a very curious experience three days ago. I was giving a lecture and I was talking about what I naïvely call "plants." One of the students was terribly worried and came to another member of the faculty and said that I was talking about something called "p-l-a-u-n-t-s," and that they seemed to be related to the vegetable kingdom.

Fremont-Smith: What was it?

Hutchinson: He got all around it but was just not able to hit it on the head, yet it isn't a very great transformation.

Fremont-Smith: Almost a selective blind spot.

Hutchinson: Perhaps he simply did not like the English way of doing things.

Mead: It is perhaps a question of framing. Some people frame all European languages together, so that the idea of learning any Indo-European language is regarded as just another language to be learned; the idea of learning Hawaiian or Japanese, however, would be regarded as something quite different. What is translation for one person is not translation for another.

Licklider: The distinction presupposes that you get inside the black box and see how the circuit works. If you set up a little theory about how one recognizes phonemes or how one understands words and sentences, it might run something like this: in the sensory channels of the nervous system, the process set up by stimulation takes the form of a multiple time series. This multiple time series is compared against each one of a large number of other multiple series which are stored in the nervous system. The incoming time series is "recognized" as being the same phoneme or word or phrase as the stored series with which it is most highly correlated. Doubtless the nervous system correlates series corresponding to words, phrases, or even sentences. But if you want to build an automatic, electromechanical recognizer of speech, you must work with phonemes. You bring in the phonemes, see which standard patterns give the best fits (after taking into account conditional probabilities that govern the expectation on the basis of what has already been received), and put each phoneme in the indicated category. That process we would not call translating. If then we put in distorted speech, and its phonemes fell into the proper categories, we would not call them translating, either.

McCulloch: Two procedures: let me make this perfectly clear. I am riding in a railroad train and the guard comes in and says "Fara." I have not the faintest idea what I am to look for except a "Fa" at the beginning and a "ra" at the end. I look out the window and see the name on the station. After I have done this two or three times, I don't translate. I just wait until I get the station and sound together. I don't translate and I can go out and say "Fara."

Werner: I believe Dr. McCulloch does not use the term "translation" in the conventional way.

McCulloch: I mean by it that I look around for a word among

words which is to stand for the same things that this stands for. The other way is to wait till you find the physical object or something outside language to tie it up with.

Werner: Let me make clear, by a visual example, the distinction between "translating" in the conventional sense and something which I would call "configurating." Suppose you are looking through a frosted window at an object. If you use the reduced cues and arrive at the perception of that object, for example, a tree, you are "configurating" and not "translating." On the other hand, a primitive man may be presented with a chair, though he has never seen one. No cues would enable him to understand the meaning of a chair. That can be achieved by showing him an object familiar to him which serves an analogous function, a low stool for instance. By equating the chair to something which already exists in his own system; namely, the low stool, he understands the chair: this is "translation." Verbal translation, then, is based on the finding of a word in my system equivalent to a word in another; configurating a verbal pattern involves the construction of words within the confines of one system.

Pitts: All the people in Licklider's experiment are exactly in the condition that you are in. The first time that the guard comes in and says, "Fara," they have not looked out the window. They have never had any opportunity of comparing what they hear with the original, and therefore it cannot possibly be translation because you cannot learn to translate unless you have opportunity for comparison; and never in any circumstances of any experiment do they have an opportunity for comparing what is truly said, what actually has been said, with what they hear. If they did, they might learn to translate, and everything would be wrong.

Gerard: The question of giving meaning initially.

Licklider: One qualification in all of these tests is that working with words of a limited set (the listener does not know the set at first; he gets a fair idea of what words are in it after prolonged testing), the listener tends to record only words he knows are in the set. If he hears what seems to be a word not in the test vocabulary, he tends, if he is not pressed for time in recording the words, to hold it in mind, think it over, and come up with the closest word in the set that he can think of.

McCulloch: He has to pick it from a thousand words?

Licklider: Limited context. If you reduce the number of words in the test vocabulary, the listener's ability to hit the right word increases considerably.

Gerard: I think this is a good time to do that little experiment.

McCulloch: Oh, imperception, standard jest phrase. Most of them know it.

In pine tar is
In oak none is
In mud eels are
In clay none are.

Gerard: Before you do anything else. That is a series of sounds and, with the rarest exception, people get no meaning at all out of it. If I give you a certain set, you will understand every word he says, and he will say it in the same way.

McCulloch:

In pine tar is
In oak none is
In mud eels are
In clay none are.

Gerard: Now suppose you know that a man was sent on a mission to find a supply of food and fuel; on his return he repeats:

McCulloch:

In pine tar is
In oak none is
In mud eels are
In clay none are.

Gerard: It is a remarkable example of the tremendous gap between any perception and the final giving to it of meaning. Here is a whole complex sentence.

Licklider: One thing is clear: until I got the clue, I could not remember the sounds. You cannot remember sounds you cannot put together into words, and you cannot try out various arrangements of the sounds you cannot remember.

McCulloch: There is a very great difference among people.

Fremont-Smith: That is true.

McCulloch: My eldest daughter is the only one in the family that has it. She can hear anything anyone says, and could do it as a small child. She could repeat the whole sequence of sounds with no understanding of what it was at all and babble it right back again.

Bigelow: I should like to raise a question concerning the effective utilization of information reaching an observer, say, the amount of information contained in a speech which is a little vague. It is clear that the amount of information which is conveyed in ordinary speech is far less than that of the channel used to communicate it, both the middle channel and others. The same thing is true also to some extent of visual communication. It is obvious if the channel is fully used, and if we remember all we

have heard, our memories would become loaded down in no time at all. It seems that there is a biological safety valve of some sort here.

Does anybody know that there are phenomena of recall in speech having to do with the unused odds of information similar to that of familiar recall of vision having to do with unused parts of the picture?

Fremont-Smith: Under hypnosis you can get them.

Frank: It is what you see in the "double take" in the movies, where the listener agrees, then repeats, then suddenly realizes the meaning of what he has heard and unthinkingly agreed to. This always excites laughter in the audience.

May I make this comment? The question raised about the difference between psychological stimulation and translation, it seems to me, might be illuminated by thinking about this: when we listen to words that we are accustomed to, we don't really listen. I think that occurs because most people don't hear what is said. What we do is to put a certain set of meanings into the sounds immediately; as long as we continue to put meanings in we can accept a wide range of stimulus equivalents. In the matter of translation, I wonder whether it is not a matter there of first identifying each word and then putting meaning into it. In that procedure there is an interpolated step.

It is very interesting to learn to read another language. I think most of you know that when you start, at first you translate each word very slowly and painfully. After you learn to read without translating, you do the same thing. You don't look at individual words. Isn't that part of the difference between the psychological equivalent and translation? It is the meaning, the point that Dr. Gerard and Dr. Werner were pointing out. That is important. We put the meaning into the situation almost without listening. In the other case we have to listen to the word, translate it and put the meaning into it.

Licklider: I think we really start to put the meaning in before we hear the word. That makes it possible to get the word.

Frank: Precisely!

Licklider: In other words, you would be able to understand nonsense sequences of words.

McCulloch: Apparently not greatly less intelligible. That was at the forty-fifth degree, and monosyllabic words pulled a little above it. It was only when you came to sentences that you began to fill up space in intelligibility.

Licklider: The words were considerably above. I may not have

done justice to the situation in my rough sketch.

McCulloch: May I ask one more thing with respect to .the unintentional auditory Rorschach, or whatever you intend to call it when you get unintelligible speech. Which is worse, mere noise, or the noise which begins to fall into the band width of speech? Which produces the most violent reaction?

Licklider: I think the thing that makes the effect show up most markedly is masking noise that almost but not quite masks the speech. Perhaps one of the formants is just audible, the others not at all. This structures the perception to the extent that the listener knows a particular vowel, or perhaps one of two or three possible vowels, is in the word, but he doesn't know at all what the word is.

Wiener: There is very much of that effect in my understanding the words in singing, particularly in a foreign language, if it is a language I know well. There is just enough masking there to require a very definite effort on my part to get them to come through.

Licklider: I was amazed how much better I could understand French when it was slowed down to half-speed on the speech stretcher. A person who speaks French fluently, who has no difficulty at all with full-speed French, thought that the effect of slowing it down to half-speed was horrible.

Fremont-Smith: It is very well known that when people are at international meetings, understanding is enormously increased if speech is slowed approximately to half-speed.

McCulloch: Yes.

Klüver: I believe you get the most perfect auditory Rorschach when listening to a parakeet imitating voices on the radio, and at the same time the voices of six or seven people talking, shouting and laughing in the same room. When I listened for the first time to such a performance—only the bird and I were present—I thought at first that I was merely listening to "bird talk" until I found myself again and again on the verge of recognizing meaningful words. It finally became clear to me that I was listening to something like an acoustic recording of a babble.

Bateson: Does it accurately reproduce a slice of babble or make babble?

Klüver: My guess is that such a bird occasionally reproduces a slice of babble accurately. In comparison with parakeets, we are apparently at a great disadvantage in listening to babble. For us acoustic events are practically always carriers of "meanings." A parakeet, however, does not seem to be plagued by the thousands

of meanings that keep us from "hearing" babble or hearing a sequence of acoustic events devoid of meaning.

Fremont-Smith: We cannot hear it when the parrot gives it back.

Klüver: One of the reasons that we cannot vocally reproduce babble or similar acoustic events is undoubtedly that we cannot even "hear" babble perfectly.

Fremont-Smith: How do we know?

Savage: Do we train the parrot to do that or hear a lot?

Klüver: The parakeets I am talking about have not been particularly trained to imitate babble, but they have been trained to imitate spoken words and sentences. Such birds must be isolated at the age of five weeks and kept from seeing or hearing other birds.

Savage: You must suppose that it does not need to hear the same sample to teach the same word.

Klüver: I am sure that the speech performances of "talking birds" and the stimuli eliciting such performances should be analyzed by objective methods. In the meantime, I consider a parakeet's imitation of babble as interesting stimulus material for an auditory Rorschach.

Teuber: Among people with brain injuries we sometimes run into individuals who have trouble in perceiving complex, or ambiguous configurations. Such a person may study a picture for quite some time before he can recognize it as you can. His retardation can be demonstrated by flashing the image on a screen. The exposure has to be abnormally long before he can recognize what it is. But there are always exceptions, and they are embarrassing for any simple theory as to why the man's perceptions seem to be slowed down after his brain injury.

During the war we had a prize case, a man who was injured by shrapnel which had lodged in his third ventricle. Of course, the shrapnel did some damage before it got there. He was very slow on the tachistoscope; when we projected pictures on a screen, they had to be exposed for as long as two seconds before he would attempt to identify them, and then he was often wrong. This was true for all sorts of material, even simple line patterns. Then, one day, we flashed a kodachrome showing his ward medical officer, a man about whom he held certain rather strong opinions at that time. On the picture, the officer was not even in uniform, but in civilian clothes holding one of his babies on his arm. We flashed the picture for 1/50 of a second, and immediately our patient called out the name of the officer with a long string of unprintable

Marine imprecations. The next moment he was quite slow again—as soon as we showed him different material.

Gerard: It bears on that question.

Teuber: Of course, it did not take him below the normal limit; his speed of recognition had just become temporarily normal, but not better than normal.

Licklider: There has recently been much experimental work involving tachistoscopic presentation of figures, work oriented toward the question you had in mind. The results first published suggested that things with high emotional content were perceived faster than neutral items. In more recent work the results have been analyzed in terms of the frequency of occurrence of the objects (words) presented. The conclusion is that the items with high probability are the ones that are rapidly perceived.

Gerard: I am not sure that you mean the words have emotional charge in one's past experience.

Wiener: With the image staying for a fraction of a second, the actual time of flashing is not the time of scanning, and it is possible to recognize things in the image during the period after which it is no longer there but their afterimage is. The point image, the seeing time is not the flashing time.

Werner: That is correct, though the problem of what one would call seeing time is a very intricate one. One of the reasons is that of qualitative differences in the level of "recognition." Flashed words may register "organically" without specific visual recognition. A number of experiments have shown that emotionally toned words flashed tachitoscopically may arouse a specific bodily reaction, as indicated by a psychogalvanic response; nevertheless the subject is unable to indicate the word flashed [McClary, R. A., and Lazarus, R. S.: Autonomic discrimination without awareness. *J. Person.* **18**, 171 (1949)].

This situation is probably very similar to auditory speech. If you want to analyze the way people hear word patterns with reduced cues, we might take into account different levels of hearing. This might have a bearing on training to hear distorted speech.

Licklider: Who were these people who responded to words they could not understand?

Werner: These are normal subjects and the words used are, for instance, nasty words.

Licklider: Why could they not understand them?

Werner: They would not consciously recognize these words.

Licklider: Normal people?

Werner: Normal people.

Pitts: Why not?

Licklider: Why could they not recognize the words?

Werner: Because they flashed on too quickly. We are talking about the tachistoscopic method.

Licklider: We are worried a little about the word "cannot" in that connection. Did not?

Werner: "Did" not recognize under these conditions.

Klüver: Concerning the time factor in emotions, some interesting data may be obtained by studying the reactions of monkeys to motion-picture films. I once used this technique in investigating the emotional behavior of monkeys belonging to different species [Klüver, H.: *Behavior Mechanisms in Monkeys.* Chicago: Univ. of Chicago Press (1933)]. The results were particularly enlightening in the case of a female Cebus monkey which responded in a variety of ways and always sat on my lap while looking at the films. The appearance of a python in one of the pictures always led to signs of extreme fear: she uttered various sounds, she defecated, urinated, and quickly disappeared into a corner of the laboratory from which the picture could not be seen. However, these fear reactions occurred only at certain speeds of projection. In other words, this technique may be used to demonstrate that emotional behavior is a function of a certain number of frames per second.

Stroud: Perhaps I am a little presumptuous here. Much of this discussion carries the implication that somehow, of necessity, a meaning comes from being presented with any stimulus pattern, and that somehow or other some necessary meaning should follow from it. The thing I wish to suggest is that any meaning is of the order of a hypothesis which can be tested on the basis of the available information. The length of time required to verify or reject or to examine a family of hypotheses is primarily a function of the number of alternatives, their degree or organization, and everything else. Thus, as Dr. Licklider pointed out, in a thousand-word vocabulary the probability, the amount of information required to accept or reject the hypothesis that this is a particular word out of the thousand, is considerably greater than if the number of alternatives is only fifty. I know that some time ago, and I am sure Dr. Klüver and others could verify it, a great deal of experimentation was done on the tachistoscope presentations of words. You were merely asked if it was a certain word or not. Under these circumstances very minimum amounts of information are required to accept or reject the typical.

Another experiment that comes to mind is the case of a very low-level presentation of the outline drawing of a banana.

Wiener: Oh, yes.

Stroud: If you present this to the subject, he does not see anything; but if you ask him in which orientation the banana is, his responses as to whether it is horizontal or vertical are well beyond the chance level. So in all of these considerations I prefer to look at the problem as one of the amount of information required to accept some hypothesis, as belonging to some hypothesis system, and to discard altogether the notion that any response in the central nervous system of the man necessarily follows from any particular stimulus configuration.

Frank: May we make one amendment? What I was suggesting was not a meaning as conveyed by the stimulus.

What I was trying to suggest for consideration was that the individual listening or hearing puts meaning into those situations out of his own past experience, which is a little different process.

Stroud: Let us put it this way: He will never test hypotheses, ordinarily, of which he is not here and now the master. To that extent you can say he puts the meaning there.

Frank: Yes.

Stroud: He will attempt to test and verify hypotheses of which he is the master on the basis of the configuration of the sensory input he has available, but nothing necessarily follows from any sensory input.

Frank: What I wanted to reemphasize was that the individual himself puts the meaning into those sensory inputs and that therefore we get a clue to the great diversity of what people hear out of the same speech.

Hutchinson: If he has many hypotheses, he will jam a lot more than if he is the master of only one or two.

Stroud: Yes, in a sense. There are other things about hypotheses, the flexibility of this indexing. A typical sort of situation in a training program is one in which you take some chap who is going to see a class of information that he never saw before, presented in a way he never saw before, and from which he is to draw conclusions he never thought of drawing. This is a very typical sort of Navy problem. Before he realizes it, we cram down his throat all the possible hypotheses he is expected to test. It is relatively easy to show he can test, but he tests at a slow pace because he is badly organized. As he becomes better organized, he can, with neither more nor less information, test with greater speed. When I say

"neither more nor less," I am neglecting the time dimension. He has less by the amount of lesser time that is required for him to test all these factors. It is necessary, then, to consider both the hypotheses and the familial organizations and their accessibility, and perhaps, in another sense, if difficult hypotheses, the length of time and the span of bits that he can hold while he is searching for the right hypothesis.

Frank: Does that agree with the conception that the individual receives every experience with a readiness or certain expectations?

Stroud: It is impossible for me to believe, now that you ask me, that at any given time you are not entertaining an expectation. This expectation may not be what one might call highly concrete and specific. It may be merely the expectation that I will continue to speak in English and not suddenly change into pidgin English and Chinese. If I did change, I dare say even if you understood Chinese, I would become unintelligible until you started taking down my speech in the new set of hypotheses.

Wiener: I have seen that happen very frequently, the change from a person who speaks perfectly correctly, to a person to whom even his own language would throw him completely off.

Klüver: Titchener insisted that it is the business of a psychologist not to commit "stimulus-errors." On the other hand, it may be argued that it is the business of psychology to determine all "stimulus-errors" that a psychologist or nonpsychologist possibly can commit in dealing with a certain sensory input. In many situations our chief interest is really in the hypotheses, categories or principles in terms of which a man or an animal reacts to certain stimuli or certain stimulus constellations. Heterogeneous stimuli may often be equivalent in eliciting the "same" response, and it is the task of the psychologist to specify the factors responsible for such an equivalence [Klüver, H.: The study of personality and the method of equivalent and nonequivalent stimuli. *Character and Personality*, **5**, *91* (1936)]. Obviously, sets of stimuli may be equivalent or nonequivalent for a thousand and one reasons. This does not relieve a psychologist of the obligation to determine, in a given situation, the particular factor or factors underlying particular forms of equivalence and nonequivalence.

Stroud: This gets to be quite practical. Somebody asks what is the man's acuity under certain conditions referring to visual, auditory, or other factors. I frankly come back at him, "You cannot say it that way." Given a man with a certain set of hypotheses indexed in a certain way, what is the minimum amount of

information he requires for a decision among these alternatives?

McCulloch: I should like to try one very simple experiment. This is a poem. Will anyone who recognizes it speak up?

Til mi no on mounful monfer
Fif is put an enty dreen
Fur they sow as dred as slummer
Wings ha na wha ha seen.

Only one word is from the original, the tenth word.

Wiener: The general rhythm is the same.

Pitts: The degree to which the stimulus is equivalent is largely conventional in terms of the language. You know in a foreign language how the field of stimulus expands visibly when you first begin to understand someone's speech. You usually can understand a small number of people whose pronunciation happens to be particularly clear. For one reason or another, then, the number of people whom you can understand gradually grows roughly according to the similarity of their articulation. The ways in which a given word may be mispronounced apparently are quite different, depending upon the word and depending upon the language; that is conventional, not phonetic. That, I imagine, is why, when apparently a parrot speaks a sentence imperfectly, it has such a sound, whereas when a man does, it is not the same field of equivalent with respect to the sentence.

Wiener: A parrot has no phonemes.

Pitts: He deforms words in a manner different from that of any possible speaker of the language.

Wiener: He has no phonemes.

Licklider: I should like to report just a couple of facts based on articulation-testing experience that bear out some of the things we have said. One of them is that when you have a test crew at work, the crew likes to entertain itself. The listeners will read magazines or books or even play cards if you let them, and they will be unhappy if you don't . We once tried to find out whether it mattered. Their scores were no lower when they read books and wrote the words down nonchalantly (getting several items behind, then catching up rapidly) than they were when we forced the listeners to devote full attention to the job. You can argue, of course, that they were irritated at us in the latter instance and therefore did not do as well as they could have done, but their performance was better than I could achieve when I worked as hard as I could.

Wiener: It has something to do with the similar situation of children studying with the radio on.

Licklider: I agree with Stroud about the hypotheses. I should emphasize, however, that the hypotheses are not on the level of awareness; they are tested automatically. After listening to recorded tests in which listeners heard the same sequence several times, they were tested to see how well they could predict what was coming. We presented four words in the repeatedly heard sequence, then blanked out the fifth. The listeners tried to guess or remember the word that followed the four they had heard. In this, they had almost no success. Next we mixed the words of the test lists to which the listeners had been exposed with an equal set of words of the same type, in fact, with other words drawn at random from the same source. The listeners could tell with high accuracy whether a word was from the old or from the new set. The listener learns the vocabulary beautifully, but not the sequence in which the words appear.

Werner: May I ask one very general question? Distorted language may be of two kinds: one in which there is a systematic change of which the subject, bye and bye, may become aware and to which he may become adapted, and another where there is no systematic change. Is the distortion you are dealing with of such kind that the subject will, so to speak, get adapted to the "key," as it were. The speed and the amount of recovery plausibly would depend on such an adaptation. I am interested in this angle because some time ago I studied the effect of systematic changes in melodic relationships on the appreciation of such relationships. I built what I called a micromelodic system where the physical intervals between the tones were reduced to about one-fifth, or less, of their conventional size, so that a complete scale spanning an octave would come within the range of one whole tone on the ordinary scale [Musical microscales and micromelodies. *J. Psychol.*, **10**, 149 (1940)].

We were able to train our subjects to recognize melodies within another range because we maintained a melodic system whereby the relationship between the intervals, though reduced, was identical with our ordinary diatonic scale. The question actually is whether you have any evidence that your subjects accepted the distortion as mere unsystematic changes or were able to adapt to a new frame of reference. In the latter case, psychologically, we are no longer dealing with distortion.

Licklider: As I defined "distortion," there had to be some relation preserved. I think it is probably true, as you suggest, that the distortions that retain the most systematic relations are the least detrimental distortions, but I am not sure that that is entirely

the reason. I cannot say. If you work with instantaneous sound pressures, any monotonic transformation is going to leave you with intelligible speech. But that isn't at all amazing when you consider that you can dichotomize the scale of instantaneous pressure and still have fair intelligibility.

Mead: Isn't it related to learning? In the different cases is it true that the more systematic the order of distortion, the more rapidly your test crew will learn to distinguish?

Licklider: Let me quote the result of an experiment, a preliminary result. You can measure the memory span for letters read in sequence from a book. The memory span for these letters is very great. The subject remembers the words the letters spell, of course, and the sentences the words form, so he remembers a lot of letters. Now a friend of mine, George Miller, thought it would be fun to test the memory span for letters with precisely the same conditional probability structure, but letters that did not make familiar words and sentences. He substituted one letter of the alphabet for another, choosing substitutes at random but always using the same substitute for any given letter. For the substitute sequences, the memory span was just about 6 or 8 letters. Mrs. Miller served as a subject. She practiced for about a month but could never improve her memory span for the substitute letters.

Mead: She never got the system. She never got the clue to the system.

Licklider: She was given the system. He told her what the substitutions were. It simply did not help. She could not go through the translation process fast enough to let the meaning of the translated symbols help.

Gerard: Do it in reverse. People may have intellectual information about a system, yet may have difficulty entering into it perceptually.

Pitts: It takes very little time to learn to read ordinary material in any particular substitution group. Suppose you make such a systematic transformation, it takes very little practice to be able to read it as if it were plain text. Certainly if you invert the alphabet, *A* to Z, and so forth, you learn that the regular combinations represent the common words, and after a while you read it very well.

Hutchinson: In the melodic system do the complements of the melody go over?

Werner: The melodic components go over but not the harmonic.

Hutchinson: I can see that. I wonder how people feel about

these strange new tunes. Do they feel that they are tossed in or that they are delightful?

Werner: They feel they are very delightful. I can tell you from my own experience.

Pitts: If a man who learned the system were presented with a form in the micromelodic system of a melodic ditty which you had some reason to suspect he would greatly like if it were presented in the ordinary system, would he find it similarly attractive there?

Werner: Yes. But I might add that maintaining the micromelodic system requires quite a bit of effort. Compared with our ordinary system the micromelodic system is more or less unstable.

Pitts: It is surprising that there should be any question.

Werner: The micromelodic system is unstable because it is in competition with the ordinary system with which we have been acquainted since childhood.

Pitts: Because we commonly assume that a number of relationships are essential.

McCulloch: For this evening's discussion Claude Shannon has the floor.

THE REDUNDANCY OF ENGLISH

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THE CHIEF subject I should like to discuss is a recently developed method of estimating the amount of redundancy in printed English. Before doing so, I wish to review briefly what we mean by redundancy. In communication engineering we regard information perhaps a little differently than some of the rest of you do. In particular, we are not at all interested in semantics or the meaning implications of information. Information for the communication engineer is something he transmits from one point to another as it is given to him, and it may not have any meaning at all. It might, for example, be a random sequence of digits, or it might be information for a guided missile or a television signal.

Carrying this idea along, we can idealize a communication system, from our point of view, as a series of boxes, as in Figure 22, of which I want to talk mainly about the first two. The first box is the information source. It is the thing which produces the messages to be transmitted. For communication work we abstract all properties of the messages except the statistical properties which turn out to be very important. The communication engineer can visualize his job as the transmission of the particular messages chosen by the information source to be sent to the receiving point. What the message *means* is of no importance to him; the thing that does have importance is the set of statistics with which it was chosen, the probabilities of various messages. In general, we are usually interested in messages that consist of a sequence of discrete symbols or symbols that at least can be reduced to that form by suitable approximation.

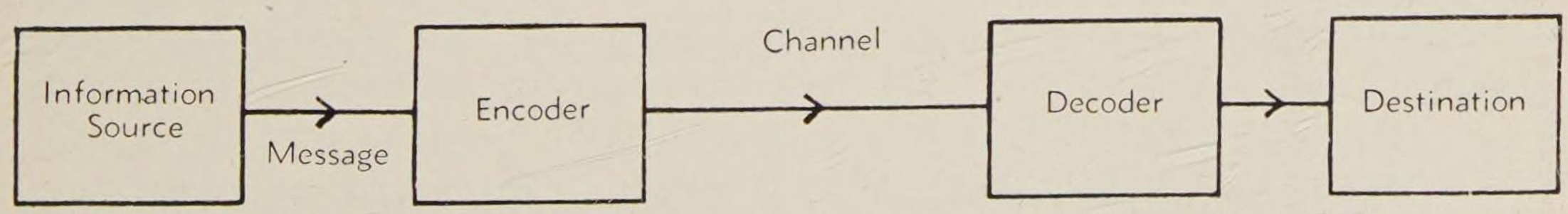


FIGURE 22

The second box is a coding device which translates the message into a form suitable for transmission to the receiving point, and the third box has the function of decoding it into its original form. Those two boxes are very important, because it is there that the communication engineer can make a saving by the choice of an efficient code. During the last few years a theory has been developed to solve the problem of finding efficient codes for various types of communication systems.

The redundancy is related to the extent to which it is possible to compress the language. I think I can explain that simply. A telegraph company uses commercial codes consisting of a few letters or numbers for common words and phrases. By translating the message into these codes you get an average compression. The encoded message is shorter, on the average, than the original. Although this is not the best way to compress, it is a start in the right direction. The redundancy is the measure of the extent to which it is possible to compress if the best possible code is used. It is assumed that you stay in the same alphabet, translating English into a twenty-six-letter alphabet. The amount that you shorten it, expressed as a percentage, is then the redundancy. If it is possible, by proper encoding, to reduce the length of English text 40 per cent, English then is 40 per cent redundant. The redundancy can be calculated in terms of probabilities associated with the language; the probabilities of the different letters, pairs of letters; probabilities of words, pairs of words; and so on. The formula for this calculation is related to the formula of entropy, as no doubt has appeared in these meetings before. Actually, to perform this calculation is quite a task. I was interested in calculating the redundancy of printed English. I started in by calculating it from the entropy formulas. What is actually done is to obtain the redundancy of artificial languages which are approximations to English. I pointed out that we represent an information source as a statistical process. In order to see what is involved in that representation, I constructed some approximations to English in which the statistics of English are introduced by easy stages. The following are examples of these approximations:

1. XFOML RXKHRJFFJUJ ZLPWCFWKCYJ FFJEYVKCQSGHYD

2. OCRO HLI RGWR NMIELWIS EU LL NBNESEBYA TH EEI

3. ON IE ANTSOUTINYS ARE T INCTORE ST BE S DEAMY
   ACHIN D ILONASIVE TUCOOWE AT TEASONARE FUSO

4. IN NO IST LAT WHEY CRATICT FROURE BIRS GROCID PONDENOME OF DEMONSTURES OF THE RETAGIN IS REGIACTIONA OF CRE.

5. REPRESENTING AND SPEEDILY IS AN GOOD APT OR COME CAN DIFFERENT NATURAL HERE HE THE A IN CAME THE TO OF TO EXPERT GRAY COME TO FURNISHES THE LINE MESSAGE HAD BE THESE.

6. THE HEAD AND IN FRONTAL ATTACK ON AN ENGLISH WRITER THAT THE CHARACTER OF THIS POINT IS THEREFORE ANOTHER METHOD FOR THE LETTERS THAT THE TIME OF WHO EVER TOLD THE PROBLEM FOR AN UNEXPECTED.

In the first approximation we don't introduce any statistics at all. The letters are chosen completely at random. The only property of English used lies in the fact that the letters are the twenty-seven letters of English, counting the space as an additional letter. Of course this produces merely a meaningless sequence of letters. The next step (2) is to introduce the probabilities of various letters. This is constructed essentially by putting all the letters of the alphabet in a hat, with more *E*'s than *Z*'s in proportion to their relative frequency, and then drawing out letters at random. If you introduce probabilities for pairs of letters, you get something like Approximation 3. It looks a bit more like English, since the vowel consonant alternation is beginning to appear. In Approximation 3 we begin to have a few words produced from the statistical process. Approximation 4 introduces the statistics of trigrams, that is, triplets of letters, and is again somewhat closer to English. Approximation 5 is based on choosing words according to their probabilities in normal English, and Approximation 6 introduces the transition probabilities between pairs of words. It is evident that Approximation 6 is quite close to normal English. The text makes sense over rather long stretches. These samples show that it is perhaps reasonable to represent English text as a time series produced by an involved stochastic process.

The redundancies of the languages 1 to 5 have been calculated. The first sample (1) is a random sequence and has zero redundancy. The second, involving letter frequences only, has a redundancy of 15 per cent. This language could be compressed 15 per cent by the use of suitable coding schemes. The next approximation (3), based on diagram structure, gives a redundancy of 29 per cent. Approximation 4, based on trigram structure, gives a redundancy of 36 per cent. These are all the tables that are available on the basis of letter frequencies. Although cryptographers

have tabulated frequencies of letters, digrams, and trigrams, so far as I know no one has obtained a complete table of quadrugram frequencies. However, there are tables of word frequencies in English which are quite extensive, and it is possible to calculate from them the amount of redundancy (Approximation 5) due to unequal probability of words. This came out to be 54 per cent, making a few incidental approximations; the tables were not complete and it was necessary to extrapolate them.

In this case the language is treated as though each word were a letter in a more elaborate language, and the redundancy is computed by the same formula. To compare with the other figures, it is reduced to the letter basis by dividing by the average number of letters in a word.

Pitts: Do other languages have the same frequency or the same degree of redundancy?

Shannon: I have not calculated them; but according to the work of Zipf,* who has calculated the frequency of words in various languages, for a large number of them the falling off of frequency against rank order of the word, plotted on log-log coordinates, is essentially a straight line. The probability of the nth-most probable word is essentially a constant over n for quite a large range of n:

$$pn = \frac{k}{n} .$$

Pitts: But presumably the constants are different for different languages?

Shannon: That I don't know. They are not vastly different in the examples which Zipf gave in his books, but the difference in constants would make some difference in the calculated redundancy. The equation $pn = \frac{k}{n}$ cannot hold indefinitely. It sums to infinity. If you go out to infinite words, it must tail off. That was one of the approximations involved here which makes the figure somewhat uncertain.

Teuber: Your probabilities are based on predicting from one letter to the next, or one word to the next?

McCulloch: No, upon the word.

Shannon: In the particular case (4) this refers to a language in which words are chosen independently of each other, but each

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*Zipf, G. K.: *Human Behavior and the Principle of Least Effort. An Introduction to Human Ecology.* Cambridge, Mass.: Addison Wesley Press, Inc. (1949).

has the probability that it has in English. "The" has a probability of .07 in the English language. So we have seven of them in a hat of 100.

Teuber: As you led up to it you said, first of all, you take the probability that any one letter will occur in that particular system, and then, given that letter, and the next one that occurs, you predict the one that follows immediately after that?

Shannon: Yes.

Teuber: Do you go further than that and say that any one letter will follow the one you took beforehand, regardless of what letter was in between?

Shannon: That is true in the calculation from 3. This is based on probabilities of groups of three letters. Number 4 goes on a new tack and starts over with the word as a new unit. The words are independently chosen. It is a better approximation to English than 3, since each group of letters forms a word, but the words don't hang together in sentences. At this point there seemed to be no way to go any further, because no one had tabulated any frequencies for pairs of words. Of course, such a table would be impractically large because of the enormous number of possible pairs of words. However, the thought occurs that every one of us who speaks a language has implicitly an enormous statistical knowledge of the structure of the language if we could only get it out. That is to say, we know what words follow other words; we know the standard clichés, grammar, and syntax of English. If it were possible, for example, to translate that information into some form adapted to numerical analysis, we could get a further estimate of the redundancy. It turns out that there is a way to do this.

The method is based on a prediction experiment. What you do is shown by the following typical experiment. Take a sample of English text:

(A) THERE IS NO REVERSE ON A MOTORCYCLE ...
(B) 1 1 1 5 1 1 2 1 1 2 1 1 1 5 1 1 7 1 1 1 2 1 3 2 1 2 2 7 1 1 1 1 4 1 1 1 1 ...

and it goes on from there. Take a subject who does not know what the text is, and ask him to guess the text letter by letter. As soon as he arrives at the correct letter, he is told so and goes on to guess the next letter of the text. In this case he guessed *T* as the first letter, which was right. We put down *1* because he guessed right on the first guess. He was also right on the first guess for *H* and *E*. For the letter *R* he guessed four wrong letters and

finally got it on the fifth. In general the numbers in the lower row represent the guesses at which he finally obtained the right answer. The figures may look surprising, starting off with three right guesses. It is actually very reasonable, since the most common initial letter is the *T*. The most common letter to follow that is *H* and the most common trigram is "the". In this particular sample, which went on for a total of 102 letters, the score obtained by this subject was as follows:

Right on guess	1	2	3	4	5	> 5
Occurrences	79	8	3	2	2	8
						Total, 102

The number of ones out of 102 letters is 79. He was right 79 times on first guess, that is, about 78 per cent of the time. He was right on the second guess eight times, three times on the third guess, and on four and five, twice each. He required more than five guesses only eight times out of 102. This is clearly a good score. It is more or less typical for literary English. The scores vary. With newspaper English scores are poorer, mainly because of the large number of proper names, which are rather unpredictable.

I should like to point out that in a certain sense we can consider the second line of such an experiment to be a "translation" of the first line into a new "language." The second line contains the same information as the first line. We have operated on the first line with a device, our predicting subject, and obtained the second. Now the crucial question is: Could we, knowing the second line, obtain the first by a suitable operation? I would say that this property is actually the central characteristic of a translation: it is possible to go from *A* to *B* and from *B* back to *A*, and nothing is lost either way. In the case at hand, it is possible to go from *B* back to *A*, at least conceptually, if we have an identical twin of the person who made the first record. When I say "identical," I mean a mathematically identical twin who will respond in exactly the same way in any given situation. Having the same information, he will make the same response. If we have available the line *B*, we ask this twin what the first letter is. He guesses it correctly because he guesses as the original subject guessed; and we know that is correct because it is the first guess he made. This process is continued, working through the text. At the letter *R*,

for example, we ask him to guess five times, and at the fifth guess we say that that is right.

Of course, we don't have available mathematically identical twins, but we do have mathematically identical computing machines. If you could mechanize a reasonably good predicting process in a computing machine, you could mechanize it a second time and have the second machine perform precisely the same prediction. It would be possible to construct a communication system based on this principle in which you sent as the signal the second line *B* from one point to another. This would be calculated by the computing machine which is doing the predicting. At the second point the second computer recovers the original text.

From the data in the second line *B*, it is possible to set upper and lower bounds for the entropy of English. There is a theorem on stochastic processes that the redundancy of a translation of a language is identical with that of the original, if it is a reversible translating process going from the first to the second. Consequently, an estimation of the redundancy of the line *B* gives an estimate of the redundancy of the original text, that is, of English. Line *B* is much easier to estimate than line *A*, since the probaabilities are more concentrated. The symbol 1 has a very high individual probability, and the symbols from 6 to 27 have very small probabilities.

Wiener: This is very interesting to me. In my prediction theory I start from a series with a correlation and I build a series which is equivalent; each is the past linear function of the past of the other. In addition, however, in my new series the choices are completely independent, whereas in my earlier series the series are partially dependent. In fact, the way I built the linear prediction theory was by the reduction of my dependent choices to independent choices. My method has some parallelisms to this. Excuse me for interrupting.

Shannon: That is perfectly cogent, I think. I was going to say in connection with this that the successive symbols in line *B* are not yet statistically independent, but they are much closer to independence than they are in the original text. It is approaching the sort of thing we are talking about, an uncorrelated time series.

Wiener: Yes.

Shannon: To continue the analysis: it is possible to estimate an upper bound and lower bound for the amount of redundancy for time series *B*. The two bounds for redundancy are based upon the frequencies of these various numbers in the second line. If we

carry out this experiment with a very long sample of text, we will obtain a good estimate of the frequencies of ones, two, threes, and so forth. Let these be q_1, q_2, q_3, respectively.

The lower bound for the redundancy is given by

$$R \geq \log_2 27 + \sum_{i=1}^{27} q_i \log_2 q_i .$$

This follows from the fact that if the line *B* were an uncorrelated time series, its redundancy would be that given by the right-hand member. Any correlation present increases the redundancy. Since the redundancy of line *B* equals that of line *A*, the result follows.

There is also an upper bound of a sort, but it is not quite as secure. It is given by

$$\log_2 27 - \sum_{i=1}^{27} (q_{i+1} - q_i) \log_2 i \geq R .$$

This is a provable upper bound if we assume the prediction to be ideal; that is, that the subject guesses first the most probable next letter, second the next most probable, and so forth. Actually, of course, human subjects will not guess quite this accurately, although in the actual experiments I believe they were close to ideal prediction. They were supplied with various statistical tables concerning English to aid in the prediction. All in all it is probably safe to use the upper bound given above.

Savage: Does the theory say that if a subject is an ideal predicter, his pattern of integers will be uncorrelated?

Pitts: Will he exhibit the maximum correlation?

Savage: No, I suspect not. Suppose, for example, that the subject guesses the first letter immediately; it takes him two guesses to think up the second letter and five to think up the third. Does that information imply anything about how many guesses the next letter is going to take him?

Shannon: Well, normally we don't start from the beginning of a sentence. Most of the actual experiments were done by giving a subject *N* letters of text, asking him to guess the next letter. This

was done 100 times with each value of N for 16 different values of N.

I am not sure I have really answered your question. At any point the subject knows the text up to this point, or at least he knows N letters of it. That will influence his next guess, because he will try to continue the test.

Savage: Will it influence how quickly he will? That will certainly influence the probability that his next guess will be right; there will be certain circumstances on which the guess is sure to be right.

Bigelow: Like in your previous example. After the fellow got the letter C in the word "motorcycle" he knew at that moment the whole word and got therefore every letter after one successful guess.

Savage: However, if you look simply at a sequence of integers generated by such a performance, do they have implications in probability for the next integer to be generated?

Shannon: I think they might if you were clever enough.

Savage: I see. There is no general theoretical reason why they might not?

Shannon: No, there isn't. There are cases in which you rather expect a series of right ones. By an analysis of this, you could say quite a bit, probability-wise, about the next numbers.

Wiener: There should be ways, I think, of sorting this out. That would be more absolute, like codings. I think there would be a reduction of the choices to completely independent choices. That could be worked out.

Shannon: Well, there certainly is in principle, if you allow the encoding of long sections of text into long sections of uncoded text, but as a practical matter I don't know how to get at it.

Wiener: To do it is difficult, because you would need more complete tables.

Pitts: Very extensive.

Wiener: Very extensive tables.

Shannon: When I evaluated the upper and lower bounds for redundancy from the experiment, the following results were obtained:

N	8	10	15	100
Lower bound for Dn	74%	75%	75%	93%
Upper bound for Dn	50%	57%	60%	72%

Dn is the redundancy owing to the statistical structure of English

extending over N letters of text.

Savage: Is this only some lower bound or the greatest lower bound?

Shannon: The only provable bound in case it were an ideal prediction but as I say, I suspect it still does bound the actual value because I think these people were close enough to ideal prediction too, so that the other things involved in this lower bound in this discrepancy more than compensate.

Savage: What mathematical properties of the new language do you use to introduce these bounds?

Shannon: The lower bound is rather trivial. It follows from the fact that the least redundancy possible with a certain set of letter frequencies would occur if they were independent. The upper bound is more difficult to prove. It involves showing how the ideal predictor would predict. An ideal predictor lines up the conditional probabilities in the order of decreasing magnitudes. You line those up and find the worst set of those that could occur. This will produce the highest possible value of entropy. Then you calculate this value in terms of the q_i.

Of course, these values are not only subject to the conditions given but also to statistical fluctuation, since the samples involve only 100 trials.

Pitts: That is, even where words are considered as being made of letters, where they are not treated ideographically?

Shannon: Everything is reduced to a letter basis.

Pitts: It might be quite different if one simply carried the parallel through with respect to ideographic words much as you could imagine carrying out the same sort of translation and the same sort of estimation?

Stroud: Didn't some joker do that? He exposed one-, two- and three-word samples and asked the subjects to guess the next word. He did it the other way around. He didn't ask them to predict the text. He simply reported the texts that were created by this method, placed certain other restrictions on it as to subject matter, and then gave you the samples as merely samples.

McCulloch: Miller. His name was Miller?

Licklider: He did do that sort of experiment.

Pitts: Have you carried through exactly the same procedure for words as a whole that you have for letters, and do you have bounds?

Shannon: In the first place, difficulties arise concerning the number of words there are in the language.

Pitts: You can ask a man for the first guess, second guess.

Shannon: Prediction of words as a whole is certainly possible, but it would take a long time to obtain a reasonable sample.

Pitts: There would be so many more words and letters that it would take much longer to guess the right one.

Licklider: There is a problem that bothers me. I am sure you have taken care of it, but I don't see quite how, in having the prediction made, compact prose is always generated.

Shannon: This is taken from standard text chosen at random out of a book chosen at random. It represents what a literary man would write. When he chooses an improbable letter, the subject usually must guess many times before he gets it right.

Brosin: I don't know the Zipf evidence. It is actually astonishing how the different languages, the different types of prose, literary, and so forth, follow the graph, the straight line.

McCulloch: Isn't the Zipf evidence for the newspaper the same as that for James Joyce's work?

Brosin: Yes.

Stroud: Both for James Joyce's work and for newspaper print. Joyce had a fantastic vocabulary, something like 15,000 words.

McCulloch: It seems the law holds approximately for Joyce as for newspapers.

Hutchinson: The number of moths of a given species plotted against the number of species containing that number of individuals, in the catch of a moth trap, or any similar statistics, behaves in very much the same way [Fisher, R. A., Corbet, A. S., and Williams, C. B.: The relationship between the number of species and the number of individuals in a random sample of an animal population. *J. Animal Ecology*, **12**, *42* (1943).] The constant can be used as an index of diversity of the population.

Licklider: My criticism was against the other method of doing it, not the one you used at all; it was against having the sample generated by presenting N items to a person and having him give you the $N+1$st.

Shannon: I don't think that a statistical sample of statistical letters is true.

McCulloch: May I ask whether it would be possible for you to look up, or to recognize among printed letters, queues to phonemes, and see what the frequency for phonemes, more constricted than the sequence of letters, really is?

Stroud: That is a very important question because of the multiplicity of representation.

McCulloch: Yes.

Shannon: I think it would be quite easy to do most of these

experiments with phonemes in place of letters. They go through rather rapidly after you get into the swing of it. I don't know that choosing phonemes would come as naturally, though, to the experimental subject as choosing letters.

McCulloch: I was thinking that one might link it with the intelligibility of speech of sentences as opposed to that of words, of that of words as opposed to that of nonsense syllables, and so forth. If you knew the sequence of phonemes represented by these letters, you might be able to link it to the intelligibility or to the increased intelligibility.

Licklider: Miller has completed some work on the learnability of speech that ties in with this analysis. The difficulty of learning a sample of synthetic prose is roughly proportional to its information content.

Teuber: By this logic, would baby talk be more predictable or less predictable than the talk of an adult?

Shannon: I think more predictable, if you are familiar with the baby.

Teuber: Do you know that linguists claim that baby talk is highly similar, that is, similarly patterned from baby to baby in different language systems, in terms of its phoneme structure? Wouldn't it be easier anyway to go from one phoneme to the next in predicting speech?

Stroud: In some of the latest orations from my youngest there is only one pair of phonemes.

Gerard: If you had taken simplified spelling, would you have decreased redundancy?

Shannon: Yes, that is right.

Licklider: Conversely, if you used the International Phonetic Alphabet to do the phonetics, you would find high redundancy.

Shannon: There is one other experiment that we performed in connection with this work which did not have much bearing on anything but which proved to be rather interesting. We asked a couple of subjects to predict in reverse, that is, to start in at the end of the sentence and to guess backward letter by letter. It turned out that the scores were almost as good as in prediction in the forward direction. The problem was much more difficult, however, from the psychological point of view. The subject was really tired out after he had worked through a sentence in reverse, whereas in going forward it is quite easy.

Gerard: As a matter of curiosity, how did the guesses go? Looking at those actual guess numbers, the first one is perfectly clear. Then you get to the *I*, and I suppose the first guess was an *I*.

The subject decided it was not *A*, so he guessed *I*. Now how did he get the *N* on the second guess? That I don't see.

Stroud: "There is a," "there is the," "there is no," are some of the most common progressions.

Bigelow: Assertionary denials.

Stroud: Either followed by the negative or by the article.

Shannon: I think *A*, *T*, and *N* would be the first three guesses; since "the" sounds a little strange in that construction, he probably guessed *A* and then *N*.

Savage: Then he has *O* for sure. He got space without the *T*.

Stroud: No, most probably, and not the most probable.

Pitts: Or *L* less.

Savage: No, reverse *EHT* there.

Hutchinson: Isn't five rather high?

Shannon: The *R* took 15 guesses, and one for the *E*.

Stroud: How does that appear compared to the probability of *R* as a first letter of a new word?

Shannon: I think this particular subject was not using tables and probably guessed his initial letters improperly. In later experiments, the ones I got these estimates with, the people were supplied with all the tables we had on the statistics of English and used them in any way in which they saw fit to aid their guessing.

Savage: How did you pick a book? At random?

Shannon: I just walked over to the shelf and chose one.

Savage: I would not call that random, would you?

Gerard: Unless you were blindfolded.

Savage: There is the danger that the book might be about engineering.

Bavelas: The book would be.

Klüver: I wonder whether Sievers' *Schallanalyse* is in some way related to the problems discussed here. The *Schallanalyse* or sound analysis of Sievers was concerned with "translating" auditory sequences as found in human speech into motor sequences [Sievers, E.: Ziele und Wege der Schallanalyse. Heidelberg: Carl Winter's Universitätsbuchhandlung (1924), pp. 65-111. Cf. also Vol. 35 of the *K. Sächs. Gesellschaft der Wiss., philol.-histor. Klasse.*] Sievers called attention to the fact that speech, no matter whether we are dealing with poetry or prose, tends to be accompanied by certain movements, postures, and tonus regulations. He held that the same is true for any written text, since all texts represent potential speech. He published numerous "curves" (Becking curves, time curves, and signal curves) supposedly involved in any form of auditory reproduction. These motor "curves," let us say,

a circle or the figure eight, may be produced by movements of the hands and arms while reciting, for instance, a poem. Sievers also made use of optical signals, such as brass figures lying before the speaker. These figures were supposed to influence auditory reproduction if viewed by the speaker while talking. However, the chief contention of the *Schallanalyse* was that out of all possible motor curves it is always only one particular curve that "goes with" a particular auditory sequence. It was said that the voice becomes inhibited if any attempt is made to produce curves that do not go with the poem or text in question.

Werner: The *Schallanalyse* is an extremely interesting but somewhat controversial method. Before Sievers, Rutz worked out a system of tones of melodic rhythm patterns.

Klüver: As I remember the story, Sievers really never succeeded in teaching his methods to others. The exception seems to have been a student who flunked all examinations, although he was extraordinarily gifted for *Schallanalyse* in Sievers' sense.

Pitts: I suspect philologists denied the acceptability of the method except for reasons in which discontinuity of the authorship came in.

Savage: G. U. Yule's efforts were bona fide and not mysterious. They were primarily based on English, and as I recall, in one or two cases on the Latin language, which he knows and is therefore able to deal with. He examines the frequency of occurrence of various obvious sorts of things, computes the average length of word, makes other like calculations, and finally makes a judgment based on standard statistical principles and his own very extensive statistical experience of whether the two works in question do or do not have style similar enough to be attributed to a common author.

Klüver: As far as Sievers' work is concerned, it rests on certain correlations between motor-kinesthetic and auditory phenomena. Sievers apparently was very gifted in "expressing" auditory sequences in kinesthetic patterns. He always insisted that only one type of motor curve was adequate whenever he recited or read a certain text. If he sensed that certain movements representing a particular motor curve were no longer adequate, his voice simply gave out until he replaced the old curve by one that "fitted." He was able in such a way to assign a particular motor curve or a sequence of different motor curves to a given auditory reproduction.

Werner: That's about it as I recall. Looking at a poetic line, Sievers transforms it into a kinesthetic pattern; if various other

lines are produced by the same author they will also fit the pattern. If then he comes to a part which does not go with the kinesthetic pattern, he infers that a foreign element has been introduced.

Pitts: What sort of kinesthetic patterns?

Werner: Though I do not remember very clearly, he contended that there were a restricted number of patterns. These patterns could be objectified by visual symbolic representations; a triangular pattern was one of them; a pendulum pattern was another.

Pitts: A particular phenomenon would be translated into the triangular?

Werner: Not quite. The visual representation is an aid for *Schallanalyse*. A poem has a certain rhythm which is repeated and which is represented by a certain visual signal, such as a triangle.

Pitts: How do you carry out the translation? It is not obvious how you would carry out the translation of the poem into the kinesthetic pattern.

Klüver: It looks as if the "translation" of a poem into specific kinesthetic patterns has remained a secret of Sievers.

Pitts: It is not an objective method?

Werner: No. Statistics have been applied, though, and they contend that these statistics bear them out.

Klüver: It may be argued that Sievers' "curves" were more than subjective motor-kinesthetic patterns. There seems to be some evidence that they represented objective indicators. If I recall correctly, Sievers was tested by some experimental psychologists who showed him, for example, a text he had not previously seen. This text had been written by two different authors. He read the text while at the same time producing the movements and curves that, according to him, "went with" the text, but then suddenly insisted that he could not go on reading. His voice gave out and he could continue only after having discovered the right kind of motor curve. The point thus located by Sievers was the point that actually separated the texts of two authors as previously determined by other philological methods.

Pitts: Nobody ever ascertained the rule by which he obtained this?

Klüver: William Stern used to say that he would give a doctor's degree in psychology to anybody who could throw light on the psychological mechanisms involved in Sievers' performances. Incidentally, I am sure that Sievers' own "explanations" do not suffice or are wrong. C. K. Ogden of Basic English fame told me just before the war that he had used the methods of *Schallanalyse*

to settle the old question of how the Romans really did pronounce Latin. In fact, I spent part of my last night in Europe making a gramophone record of Ogden's recitation of a Latin poem. And I took this application of Sievers' principles, that is, the gramophone record, to New York.

Pitts: Does this agree, on philological evidence, with the most powerful of the schools with respect to the actual pronunciation?

Klüver: I understand that the pronunciation of Latin as practiced by the Romans remains a controversial matter.

Werner: I may add that Sievers was one of the outstanding German philologists who wrote classic works on old-German grammar and texts. *Schallanalyse* was obviously a hobby for him at first which he later extended and included among his methods for textual analysis. Later he felt the *Schallanalyse* was much more satisfactory than the traditional philological methods.

Pitts: Philologists have explored more eccentric methods of argumentation that most people have.

Stroud: Every individual shows a reluctance to repeat himself in the use of a single word. Sometimes there is reluctance to place verbs too close to the noun, and there are other idiosyncrasies of sentence structure which are to some degree characteristic. I wonder if such landmarks could not be guessed at reasonably well with a fair length of text.

Pitts: The only trouble is that the serious questions are practically never concerned with two texts of any considerable length, whether by the same author or not. Usually both texts are very short.

Brosin: I don't know how relevant it is, but the Masserman-Bolk chromatic analyses of parts of speech in the Murray Thematic Apperception Tests with relevance to psychiatric reactions are an effort in this direction. I don't know how seriously you would consider this.

Pitts: You might decide the Shakespeare controversy in this fashion but probably very few others.

Brosin: I don't know how useful it is, but obsessed people use words in certain orders and quantities. Surely the scope of word patterns and, let us say, the kinesthetic rhythms of one type of schizophrenic, say a hebephrenic, will surely have distinctive patterns. Whether these are sufficient to be computed for absolute values, I don't know.

Mead: Milton Erickson has a whole series of texts taken down from different diagnostic psychiatric types. In these the clear formal properties of the language and the types of balance that recur can be distinguished. It would be such patterns, I think,

that you would have to deal with. You would have to use a good many abstractions, such as balance and types of repetition and inversion, in such an analysis.

Frank: Eliot Chapple, who has done the same thing at the Massachusetts General Hospital, has developed a machine for recording the pattern of speech.

Mead: But his records contain the interaction with another person within it. That would be the difference here, would it not?

Frank: Probably.

Mead: If you introduce two people into the picture, then Eliot Chapple's chronograph gives a very diagnostic speech pattern when one individual's responses to another's are analyzed.

McCulloch: May I ask one clumsy question? I don't see quite how to put it yet. Most of the things you have been going at, Mr. Shannon, have been on a small scale. Can you work out anything that would handle affairs as large as your ordinary grammatical units, phrases, clauses, sentences, and so on?

Shannon: I think the 100-letter approximation is beginning to bite into phrases. The person with 100 letters has a fraction of a sentence, perhaps a full sentence, to work on. He makes use of all that information, perhaps finds a key word 100 letters previous. More in the spirit of what you are asking, I feel there is a point at which this statistical approach is going to break down. It is very questionable to me that the very long-range structure of language can be represented by a statistical process, even that there is any meaning of speaking of the probabilities of sequences which are so rare that they never have occurred. Certainly the frequency concept of probability begins to weaken at some point. Also, when you are considering very long-range structure there are questions of whether the stochastic process is stationary or not. The process may not have the stationary properties that are implied by most of this analysis.

Teuber: Isn't it true—for long passages at least—that it is easier to make predictions after some preliminary acquaintance with the idiosyncrasies of a particular author, or even of a particular group of people with whom you have been in contact?

Pitts: Probably after the passages exceed a certain length, and if you are concerned with the question of deciding whether or not some particular person or anybody at all could possibly have said that. Your chances would be much better if you were to analyze on the basis of whether or not it is a kind of notion or idea that could have been expressed at that time, rather than on the basis of the statistics of a series of words in it.

Teuber: That could be based on a single experience. Just say the word "Gestalt" in this group, and you can just about predict what will happen and who will say what. You make your prediction in terms of past experience, but one previous exposure to that interchange of points of view will suffice. This is of course irrelevant to what Mr. Shannon is trying to do. For him, the important thing is to get rid of all idiosyncrasies.

Pitts: There you would find probabilities of notions of the man rather than of the symbols used to denote them. I don't see why this should be necessarily impossible a priori.

Bateson: I was thinking about the extraordinary difficulty of reconstructing stenotypic transcript. If a word like "ratio" becomes "ration," as it cannot do on the stenotype but may on the typewriter, it may be very hard to get back to the original alternative. Meaningful distortion has crept in where there has been a deformation.

Pitts: You can correct it astoundingly well, probably much better than you think on the basis of statistics about series of words. If you tried correcting it solely on the basis of the statistics of a possible series of words, and extended it, say, to ten-word sequences, even you would probably do it much worse.

McCulloch: If you can find anybody who knows the speech of the person in question and can imitate it, and if you make him read aloud the stenotypic notes, you can reconstruct the speech again and again.

Bateson: Have you ever tried it with a long text in which 10 per cent of the letters, say, are deleted at random? What percentage of letters do you have to delete before unintelligibility sets in?

Shannon: I tried a few experiments with a 27-letter alphabet, again with the space a letter, and found that you can reconstruct, say, about 70 per cent of the text when about 50 per cent of the letters are deleted at random. The trouble is that the random letters, the deletions, pile up in certain places. If you delete every other letter, you can do quite well with almost 50 per cent missing. You can delete all the vowels in a passage and have no difficulty in reconstructing it. Only very infrequently will you miss a word. Vowels constitute 40 per cent of the letters. You can also delete the spaces, which is another 20 per cent.

Frank: Is it correct to infer from your remarks that the tables you build up are predicated upon knowledge of the uniformities of English language on the part of your ideal informants, who have then interpreted the text with these deletions according to

the structure of English language and ideas? Is that the way it operates in your procedures?

Shannon: I would say that all of this work simplifies the complexity of English a great deal in that we say there is one kind of English and there is one set of statistics for English. Actually, English is really many different languages, each with different statistics. If a person knew who wrote the text he is predicting and was familiar with the author's habit patterns, he could certainly do better than if he were just taking it as blind English.

Hutchinson: Isn't it like the library catalogue which provides you an English book full of information by giving a single number that can be provided from Washington or New York?

Shannon: Provided you wanted to send that book; but suppose I write a book. That book is not in the catalogue, yet I want to send that; but you don't have any number for it.

Hutchinson: That is true.

Shannon: You should have numbers for all books and the one that might be written by this information source.

McCulloch: What is known about the frequency of the various parts of speech in the ordinary grammatical sense? To what extent can you guess what is coming next?

Shannon: I have not seen any tables on that, but it is possible to guess surprisingly well in this kind of experiment. When subjects obtain scores like 60 per cent right on the first guess, they must have known what word was coming.

Licklider: There is obviously a close relation between this and what Rudolph Flesch says in his little book, *The Art of Plain Talk*. The latter is on a less precise and more intuitive level, but I think it could be translated into terms of information and redundancy. Flesch says that if you really want to communicate with someone, you have to make your speech (and more especially your writing) even more redundant than it naturally is. You have to repeat two or three times, then say the same thing in different words. This gives us, perhaps, a very dismal outlook for verbal communication. I suppose it does not make the outlook for conferences like this very hopeful. They have to be long.

Marquis: There might be another implication of the same thing: for example, the best communication occurs if you say what the listener expects you to say.

Licklider: There should be an optimum degree of correlation between the talker and the listener. If the correlation is zero, the listener has no expectations and understands nothing. If the correlation is unity, he doesn't need to listen.

Mead: If you take a form of verbal communication like the one Harold Laswell uses, involving a vocabulary from about six different disciplines simultaneously in places where such usage is not expected, most people, while they may know all six vocabularies, will find following him exceedingly difficult. He has also adopted the device in his usual communications of interlarding endless redundant clauses, such as, "It is unnecessary to specify." One can, of course, learn to recognize these interpolations.

Pitts: You mean this is deliberate?

Mead: He does it all the time. I tried the experiment once of asking him to cut them out. After I had asked him to cut them out, I found it about impossible to listen.

Pitts: On the book page you can find the significant part of the sentence because it is padded with these things.

Mead: If you are used to the style, you knock these phrases out, and the pauses give you a chance to adjust to the shifts in vocabulary. If you get *him* to knock them out when you are trying to listen, then you realize how difficult it is to make these shifts all the time. This is the opposite of the point you were making about the condition in which a person is saying exactly what is expected. As you move away from expectancy, even in type of vocabulary, you need this padding which is not necessarily saying the thing over but just permitting a slight shift from one frame to another.

Pitts: He could probably speak more slowly and do nearly as well.

Gerard: Friends rarely finish sentences with each other, since one knows what is coming and picks it up.

McCulloch: Mr. Shannon, will you say a word about the assurance of getting a message across as it affects the redundancy? Have you any actual evidence on that?

Shannon: No, I don't have any numerical evidence. Do you mean into a human being or through a noisy communication channel?

McCulloch: Getting information through noise.

Pitts: Then the redundancy reduces automatically. By the way you calculate the redundancy it reduces if you have to put it through noise. I mean, the way you calculate information means that in case you put it through noise then repetition becomes less redundant than it would otherwise be.

McCulloch: That is right. Have you any quantitative work on it at all?

Shannon: We have some work, for example, done by Rice at the Bell Laboratories on White thermal type of noise and various

methods of encoding for transmission through it. Rice has developed formulae which show roughly how much delay is required in the encoding operation to introduce redundancy properly so as to overcome the effects of noise and enable correction of errors. It appears that a rather large delay is usually required if you wish to approach the ideal encoding with, say, one error in a hundred transmitted symbols.

Von Foerster: Is there knowledge of redundancy of different languages, or only of English?

Shannon: The only work I know of is in English.

Von Foerster: What do you expect for the other languages, the same figure or different ones?

Shannon: The Zipf curves suggest that the redundancy for other Indo-European languages may be of the same order as that of English.

Von Foerster: This has certainly something to do with the closely related grammars among the different Indo-European languages. The grammar of a language is probably more or less an expression of its structure. With respect to the redundancy of a language, it is certainly true that the more freedom of choice the grammar leaves the less redundant the language becomes. On the other hand, a language with an extremely highly developed grammar would be a language with a big redundancy. As for instance, mathematics or symbolic logic are languages with 100 per cent redundancy. I see here two tendencies operating against each other to develop the optimum of a language. The one tendency tries to decrease the redundancy in order to transmit as much information as possible; the other one tends to increase the redundancy by establishing a highly structural order within the language. That means we have to expect certain values for the redundancy in an optimized language. Perhaps the numbers of letters—or perhaps the number of phonemes—play a very important role in optimizing a language. I would like to remind at this point that the first attempts in writing are usually solved by an idiographic system which does not know any letter but has a symbol for every word. I am thinking of the old Maya texts, the hieroglyphics of the Egyptians or the Sumerian tables of the first period. During the development of writing it takes some considerable amount of time—or an accident—to recognize that a language can be split in smaller units than words, e.g., syllables or letters.

I have the feeling that there is a feedback between writing and speaking. After writing freed itself from the archaic rigidity of

idiographs and became more fluent and versatile due to the elastic letter system, I would expect a certain adaptation of the possibility of making words and making sentences. In other words, I believe there should be a connection between the redundancy of a language with respect to its word and with respects to its single letters. These figures must give a certain knowledge about the structure of a language.

Shannon: I am sure it does. I believe there are a large number of compromises in constructing a language, one of them being that the language ought to be pronounceable on reading it. This requires certain constraints about how the vowels and consonants separate each other. This already implies a certain amount of redundancy. I believe that there are many other desirable features that you require of a language which force the redundancy to be fairly high in order to satisfy these requirements.

Werner: Of course there is also the phonological aspect of language, which has been studied so extensively by phonologists and which enters into the problem of redundancy. Every language uses a limited number of diacritical phonic signs; certain combinations of sounds exist in one language but do not exist in another. The phonic unit *kn* does not occur in English but occurs in German, and so forth. Because languages differ in phonological structure, there is a qualitative and quantitative difference in redundancy between languages.

Pitts: In simple ways. You know, for example, that one language has common words on the average much longer than another. Then it will be more redundant in that sense. Thus, translation from English into Latin increases the length of the book. As a matter of fact, that is very clear in the Bible, in which the English translation is the shortest, except for the Chinese one. That, I think, almost certainly implies that the redundancy of English is less, on a syllabic basis, than that of most other languages.

If you are interested, however, in the capacity of language, in your sense you probably mean to a greater extent the redundancy on the basis of entire words taken as units.

If you were to take the article "the" and add four symbols to it, in one sense you may increase the redundancy. Another language that has articles may suffer more redundancy than a language that has none at all.

Teuber: Mightn't that possibly give us a definition of a "primitive" language? Some of the American Indian languages seem to us to "overqualify" all the time. They have to keep going for a

long time before they get where we would get in a few words. At the same time, would there be greater predictability of phonemes, on the basis of their sequence, that is, to be able to predict which phoneme follows which? I raised the question about predictability in baby talk for that same reason. If you have a very redundant language, you could argue that in that language one makes the same noises for a longer time, and one could soon tell what noise tends to follow what noise, so that there is greater predictability. I don't know whether that is so, and whether it would lead to a precise definition of "primitiveness" of a language.

Mead: I don't think you could do that. With a great knowledge of phonetic structure you might be able to construct an ideal, such as working on your ideal redundancy point, an ideal language of an ideal degree of primitiveness, especially using the child as a model. But with actual primitive languages you cannot do anything of the sort. You get extraordinary variations; therefore you would not be able to make any kind of sequence.

McCulloch: Some are extremely redundant and some are not?

Mead: Yes. Some are not.

Von Foerster: This situation might be illustrated by the problem of translation, where the same thought has to be expressed in different languages. It very often happens that a certain phrase, a poem or a thought can be beautifully expressed in one language and sounds impossible in another one.

For instance, the beauty and clearness of Aristotle in its own language becomes in German tedious and clumsy, whereas in English Aristotle regains his sharpness and conciseness. On the other hand, to read Goethe's *Faust* in French is almost ridiculous. But these two examples don't say anything against German in the first case or French in the second one. It seems to me that different languages are able to express things with different results. Isn't it so, that a language is a symbol for an intellectual world like any other human expression, architecture for instance? I am considering the Greek temples and the Gothic domes. Each of them is perfect in itself, serving the same purpose—and how different they are. I think word-redundancy is not a sufficient key to judge the value of a language.

Klüver: Dr. Von Foerster, it has been said that a German can understand Kant's *Critique of Pure Reason* only if he reads it in English. Obviously, an English translation makes it necessary to transform the long Kantian sentences into simple, short sentences.

Von Foerster: Yes, certainly. According to Pitts' statement about the Bible, Kant should have written his Critique in Chinese.

Savage: It should be emphasized again that Shannon has talked about redundancy at the presemantical level. Redundancy of printed speech really refers exactly neither to the spoken language nor to the very difficult problem of semantical redundancy. I think it would require special and difficult experimentation to measure the semantical redundancy of the language, though the experiment last reported by Shannon does bear on the subject to some extent in that the guesser knows English and is utilizing that knowledge in his guesses. But still, to isolate the semantical redundancy, to separate it from the phonemes would probably be very difficult. Yet it is the thing you are all talking about now. It is the thing that refers perhaps to what you would call the spirit or essence of the language.

Stroud: I wonder if you would care to consider such highly artificial languages, if you like, as symbolic logic, or to consider mathematical notations as being perhaps among the least redundant symbols that we have. My reason for bringing this up is really very simple. I planned at one time to use a sample of quite readable text by simply reading off the rules by which you extract the roots of the cubic equation. This can be read in English; and yet only those of you who are mathematicians would have recognized it for what it was, I am sure, and could have reproduced the equation from the instructions. A majority of us might have recognized that it was the process, but we could not have reproduced the process; some of us might not even have recognized it for what it was. I had thought of using this as an example that was perfectly pronounceable at the good standard elocution rate of one phoneme per tenth second, though insufficiently redundant to be absorbed by the listener. I hoped thereby to indicate that in this case that amount of information which I was trying to convey to the subject in question, lacking in many cases the full knowledge of the statistics involved, was too much for our abilities to handle.

Pitts: No, I don't think you can say in general that the artificial languages of symbolic logic express a minimum of redundancy. I suspect that anyone would agree with that who had read Bell's book on mathematical methods in biology in which he explains the elementary principles, or the principia mathematica.

Savage: I would say they are highly redundant. They risk nothing.

Pitts: Exactly.

Savage: Freedom from redundancy is a desideratum of mathematical and, to a somewhat lesser extent, of logical notation. It

is not, however, foremost among the desiderata. To appreciate this it is important to recall that redundancy means here the sort of statistical redundancy which has been defined, that is, the frequent use of long or otherwise awkward expressions. Thus, redundancy includes more than what we call redundancy in ordinary usage; namely, actually saying the same thing twice or using expressions that might well be dropped. The business of trying to make the frequent symbols simple, though more consciously pursued in mathematics than in ordinary speech, has been going on for a much shorter time.

Stroud: I shall have difficulty in believing this until I have seen fair samples of fairly long expositions in symbolic logic subject to this sort of analysis of the diagram, trigram type in which the knowledge of the person reading the material of the rules whereby these were established was ruled out. If you include the rules in a complete knowledge of them, hypothetically, at least, unless the person has been deliberately redundant, you should be able to reproduce the entire text once you got one-half of an equation of it.

Licklider: I think there is a possibility of studying the semantic aspects of language profitably in simple languages that grow up in special situations. I have some friends in the Human Resources Research Laboratories in Washington. They are very much interested in what they call Airplanese. This is the language of the control towers that get the airplanes down at airports, that control takeoffs, and so forth. They have kept track of the words and messages that pass between the towers and the planes. I remember this preliminary result: at an early stage they had 10,000 tokens, 10,000 words recorded; of these, 5,000 were either numerals or place names, such as Washington, Bolling, Andrews. The remaining 5,000 tokens included just 400 types. By any way of figuring, that is quite redundant. The most frequently occurring actions are landings and takeoffs, requests for wind directions, and so forth. The physical problems determine the messages. This suggests, since the semantic aspect of language has to do with the relation between the signs and their referents, that the signs are autocorrelated and redundant in large part because the world we live in is. When we get into routines, we find stretches of language that are extremely redundant. They are redundant because they are trying to describe actual situations that are redundant.

Pitts: Every natural law expresses a great redundancy in nature.

Licklider: That may be pursued farther in that direction. I think it is important to bring the referents into the picture. For example, the rate at which information flows over a Ground Con-

trol or Approach link is dependent upon the type of plane that is being controlled. When a jet plane comes in, the rate of direction goes up. When a training plane comes in, the rate goes down. The idea, then, is that you may be able to trace much of the structure of language to the structure of the actual situations in which language is used.

McCulloch: What comes in, then, is mere padding or actual repetitions of directions.

Licklider: In the G.C.A. example.

McCulloch: It sounds like an American radio advertisement. Is that the kind?

Licklider: The G.C.A. operator never goes off the air for any length of time during a talk-down. He talks, keeps talking; the idea is not to let the pilot get the notion that the radio link is dead. The pilot would be much distressed, flying blind, coming into the ground with nobody talking. If the operator is talking to a slow plane, he has to keep on saying the same thing over and over. It is extremely redundant. If the plane is fast, the situation changes rapidly enough for the operator to make every second or third phrase a new instruction.

Savage: Does it suffice to maintain some inanimate acoustical contact or is actual talking preferable?

Licklider: I have heard that the pilots don't like the operators to be overchatty. They would just as soon not have that.

Pitts: He might get a lick in for security to make perfectly sure he has been perfectly understood, since he has to—

Wiener: The direction might be repeated half a dozen times.

Gerard: Such as, "Keep coming."

Licklider: I think the G.C.A. operator does not talk about the wind direction. It is more like: "You are on course, you are on course, you are doing fine, you are on course; now two degrees left, two degrees left; the heading is so-and-so, the heading is so-and-so; hold that heading, hold that heading." It goes on at that rate; the number of words said is purposely high. Actually, the whole problem of coding in military communication comes into our picture. There are two rival philosophies. One says that you want to set up a restricted set of messages and enforce the restriction, hoping that an emergency does not arise for which it will not be adequate—

Stroud: And "hope" is the right word.

Licklider: The other says that you want people to use their heads about the phrasing of messages. I don't think there has been a decision between the two yet.

Bavelas: What may seem redundant from one point of view may not be redundant at all in terms of what one might call second-order information. I am reminded of a little study, done several years ago, in which college students were asked to tell what kinds of things people like themselves could do to help the war effort but which would very probably invite criticism from their neighbors. Also, they were asked to tell what people like themselves could do to help the war effort which would evoke praise from their neighbors. On the basis of that information, two leaflets were prepared. Both leaflets were purported to be statements by a public official urging college students to help the war effort and suggesting what they might do. One of them suggested only those activities which the interviewed group said would be criticized; the other leaflet suggested only those things which the interviewed group said would be praised. These leaflets were distributed to entirely new samples of college students. When these students were asked what they thought about them, it was clear that they attributed to the "public official" favorable or unfavorable characteristics, depending upon the extent to which the suggested behaviors were of the one kind or the other. In other words, the text not only bore information with respect to the activities in which the college student might take part but also information about the author.

Bateson: Communication about relationship between you and the other person.

Pitts: You could code them and send them all at the beginning very quickly.

Savage: I say you could not.

Pitts: Not communication of information. That is the important point, even of second-order information.

Frank: That is the difference between machine and man.

Pitts: If I could say you are alive in half an hour and continue to say it—

Savage: When you marry, tell your wife on the wedding morning, "I love you, darling; I love you eternally, no matter what I say or do from now on. I love you eternally, remember." Then you tell her again. If you never refer to the subject again, see what happens.

Pitts: That shows exactly that it is not proper to speak of it as a communication of second-order information in the same sense in which primary information is communicated, because the assumption is reduced to absurdity by exactly that remark.

Savage: She can't decode that.

Stroud: This concept of redundance abstracts the information from the date. If for any reason the date of origin is part of information, there isn't any such thing as a redundant signal, as near as I can make out.

Savage: I sympathize with Mr. Pitts. It is something of a tour de force to call these things information. The obvious differences, which are brought out by these examples as reassurance, emotional contact, and so forth, though there is communication in them, are connections between individuals which are not just transmission of statements of fact.

Bavelas: I think they are. If we could agree to define as information anything which changes probabilities or reduces uncertainties, such examples of changes in emotional security could be seen quite easily in this light. A change in emotional security could be defined as a change in the individual's subjective probabilities that he is or is not a certain kind of person or that he is or is not "loved."

McCulloch: Verily, verily I say unto you.

Bavelas: What I am saying is this: I want to avoid technical language, but if a man walks in and pats you on the back or winks at you across the table, this may be information in the very same sense that any other message is information if it reduces your uncertainty as to your present state among a possible number of states, your position in the group.

Frank: Can't you refine that by saying that this may consist of information if you want to call it such, but that primarily the communicated sign or signal or gesture keeps the recipient of your communication tuned to the meaning of the information that you want to convey? In a sense it is getting the person ready with the right expectation so that the information you want to convey will be accepted, received, and interpreted in the terms you want it to be.

Bavelas: That is one function, but there is also the function of informing his present state in, for instance, a group relationship.

Frank: Yes.

Bateson: In the straight telegraphic situation you have a whole series of conventional signs for, "Please repeat; I received the last word; talk louder," and so on. Now those are very simplified analogues of the thing you are talking about, aren't they?

Bavelas: Of other things too.

Bateson: I mean giving commands.

Bavelas: Let us suppose that I am a stranger in this group. As I sit here, a gentleman whom I don't know but whom I assume is

one of the leading members of the group smiles across the table at me. When I make a comment, he nods in agreement. Now it seems to me that his behavior bears information in the sense that it makes it possible for me to select certain ones from all the possible relations that I might conceive myself as having to this group.

Pitts: Certainly there is always an informative component in these emotional communications. I think our point was mostly that that is not all; very often it is not the essential part of it, nor is it the reason why people spend so much time at it as they do.

Frank: There is another aspect of the situation.

Pitts: It is the birthday telegram with us.

Frank: Often it is a reaffirmation, saying, "I mean," and then repeating in other words because the facial expression and response that you are watching on the other individual indicates that you must make another attempt at communication because you see you are not getting through. Therefore you restate it, reaffirm it, put it in another way while watching that individual, until you believe that you perhaps have made a communication. So I think that is another aspect, face-to-face conversation.

Gerard: It is about time to tell a story that has been on my mind for a while. Speaking about the stranger in the group makes it relevant. A guest spoke to a group that was intimately knit. One who preceded him said a few words and ended with, "72." Everybody roared. Another person said a few words, then, "29," and everybody roared.

The guest asked, "What is this number business?"

His neighbor said, "We have many jokes but we have told them so often that now we just use a number to tell a joke."

The guest thought he'd try it, and after a few words said, "63." The response was feeble. "What's the matter, isn't this a joke?"

"Oh, yes, that is one of our very best jokes, but you did not tell it well."

Pitts: Of course, in a certain sense there is much more information in this kind of communication than one would suppose. If a man tells his wife every morning for thirty years that he loves her, the declaration may convey very little information if she is in no real doubt. If he omits it, however, the omission gives her considerable information. You must consider the possibility of communication not occurring if it is not a message at all.

Stroud: Something that bothers me is the way in which we make wisecracks about this thing we call noise. For many very practical considerations it seems often very wise to consider the whole message as a message, and the noise, if it has any meaning at

all, merely as that portion of the message about which you do not wish to be informed. This has some very practical applications in that you have two ears: there is nothing to prevent you from hearing my voice as it comes to you from several paths, each of which repeats substantially the same pressure pattern, but with a slight time delay. From one point of view, if you say merely that it is sufficient for you to hear one version of this time-pressure pattern and not the other time-pressure patterns, then hearing with two ears is a highly redundant affair. If, however, you begin to find out that because people have two ears they hear these same pressure patterns in several different versions with slight modifications of amplitude and phase relations, you discover that they are able to take the range and bearing of the speaker with a considerable degree of accuracy. The information, then, was not redundant at all. It is perfectly true that they did not use the information to verify that I said "is" when I said "is," but they did use it for the very useful purpose of informing themselves about where the joker was who was talking to them.

Licklider: And what sort of room he is in.

Stroud: And where he is in the room, such questions as these.

Savage: None the less, it is highly redundant to answer these questions a thousand times over.

Stroud: They are highly redundant in the sense that your past experience tells you that you will not move without having some other source of information, or that the room will not change without some other comparison of information; but since in these matters one requires a very high degree of security, they are not over redundant if you want to attain high orders of probability in not making a mistake.

There is another case: Suppose I want to take an electrocardiogram on a man who is sick in bed in his own home. I try it, but what I get out on the trace is practically pure 60-cycle hum. This was not the information that I came for. I already knew that the local power company put out at that rate. However, it is a very practical stunt to be quite well informed about this, and, incidentally, independently informed. This I do by putting on an aerial that picks up this same information about the power system. In a certain sense this is rather stupid, but I can use this as a minus message in the same sense that Dr. Licklider meant when he was talking about transmitting the noise on one channel and the mixed message and noise on the other, then using the pure noise as a minus on the mixed signal, and coming out with nothing but the pure information. I use the information about the same self-

power supply as a minus message to the confusion I get from the patient and conclude with a fairly respectable electrocardiogram. So I often suspect that it might be considerably more profitable, at least in many contexts, not to be too quick to define what is the signal and what is the noise. I know that I am often very amply rewarded by stopping to find out what the stuff I call noise in a message is. Sometimes it turns out to be more valuable than I had implied when I said it was noise, when I said that the "not" signal I was interested in was noise.

Savage: Your mistake was in defining noise as part of the signal you were interested in. The distortion of speech that comes to you from the walls of the room is not in itself noise. It is distortion, that is to say, recoding.

Stroud: How, for example? I am sorry.

Licklider: Then you'd never know about it if there were really some 60-cycle stuff in the cardiogram.

Stroud: That is the difficulty. Perhaps the fellow with past experience with cardiographs would expect me to pick out the 60-cycle.

I study the presence of a message. This message is defined on an a priori basis in the presence of white noise. If this is realized in the experimental situation, do you know what my white-noise generators tell me? They tell me about random movement, charges in the gas tube, in the magnetic field. I must admit that in the vast majority of cases this information is nothing for me to be particularly concerned about, but I would remind you that this restriction upon what is a signal in a message, and what is a "not" signal is often highly arbitrary. Very frequently it is not stated, much to the detriment of the discussion that follows, even in your treatment of it.

Frank: May we go on from what you said earlier about noise to consider the situation that man and his mammalian ancestors grew up in a world of communication that consisted chiefly of noise? Everything was going on at once and producing a myriad of to-whom-it-may-concern messages. Man gradually evolved the ability to select and orient himself, as you say, with the two ears. Thus we get a conception of language as a codification of events which we have learned to pay attention to and to interpret in specific ways. Each culture has picked out what it pays attention to and what it will ignore among the innumerable messages from events. We think and speak in terms of selective awareness patterned by our cultural traditions, the *eidos* and *ethos* of each culture. Out of the noise patterns it is not *the* message that is received

but rather *a* message that our selective awareness, readiness, or expectation filters out and gives meaning to as a communication which may or may not be authentic or valid. In other words, we may be said to create the communication, each in his own image.

Stroud: I have a suspicion that all of our sensory inputs are capable of supplying us with a tremendous number of items of information, by the billion in the case of the eye, in very short periods of time. The limiting factor in the thing is the computer, and into that we are only able to insert a relatively small number.

Pitts: We discussed at length last time how the optic nerve is greatly reduced as compared to the retina. As a matter of fact there is an immense loss of information.

Stroud: We speak of these as losses, but I suspect they are not simple losses. They are well planned, programmed.

Pitts: It is done by the aid of natural laws generally.

Stroud: The fact that we are able to attend only to a limited number of bits per second in our abstraction leads us to quite unconscious and implicit statements as to what the signal and the "not" signal are in any given set of sensory inputs. It also leads to a similar arbitrariness in handling a set of information which we get over an instrumental, extrasense system, which most of these gadgets are.

Pitts: I don't think, in the sense of the exact theory of information which we have been discussing this evening, that there is any vagueness or ambiguity at all in what is meant by the message, in what is meant by the noise. It is perfectly true that in loose everyday use of the term there may be.

Savage: There is arbitrariness.

Stroud: Yes.

Savage: Consider a telephone. It seems to me that from the engineering point of view it is an arbitrary question whether the telephone is to communicate, say, the meaning of English spoken into it, or the affect—or whatever the psychological word is—of the English as well, whether it is to communicate not only the English spoken into it but also other sounds which happen to be occurring in the room. Practical considerations might dictate the discussion of a telephone from any of these points of view. It is therefore these practical considerations which would determine what is to be considered signal and what, noise.

I think Shannon ought to say a word now. His ideas have been discussed for half an hour, therefore I suppose he is interested in

expressing his own views about the discussion.

Shannon: There are several comments I want to make on the last point. I never have any trouble distinguishing signals from noise because I say, as a mathematician, that this is signal and that is noise. But there are, it seems to me, ambiguities that come in at the psychological level. If a person receives something over a telephone, part of which is useful to him and part of which is not, and you want to call the useful part the signal, that is hardly a mathematical problem. It involves too many psychological elements. There are very common cases in which there is a great mass of information going together. One part is information for *A* and another part is information for *B*. The information for *A* is noise for *B*, and conversely. In fact, this is the case in a radio system in which one person is listening to WOR and another to WNBC. You can also have situations in which there is joint information, something of this general nature. You can have a device with information going in at one point *A*, part of it coming out at *B*, and part of it coming out at *C*. It is possible to set up the device in such a way that it is not possible to transmit any information whatever from *A* to *B* alone or to *C* alone, but if two of these people get together and combine their information then you can transmit information from *A* to the pair of them. This shows that information is not always additive. In this case the information at *C* is essentially a key for the information at *B*, and vice versa. Neither is sufficient by itself. If two of them get together, they can combine and find out exactly what the input was.

Stroud: The ideal minus message case. I didn't mean to cast any shadows or doubts. I merely wish to make the point that Mr. Shannon is perfectly justified in being as arbitrary as he wishes. We who listen to him must always keep in mind that he has done so. Nothing that comes out of rigorous argument will be uncontaminated by the particular set of decisions that were made by him at the beginning, and it is rather dangerous at times to generalize. If we at any time relax our awareness of the way in which we originally defined the signal, we thereby automatically call all of the remainder of the received message the "not" signal and noise. This has many practical applications.

Licklider: It is probably dangerous to use this theory of information in fields for which it was not designed, but I think the danger will not keep people from using it. In psychology, at least in the psychology of communciation, it seems to fit with a fair approximation. When it occurs that the learnability of material is roughly proportional to the information content calculated

by the theory, I think it looks interesting. There may have to be modifications, of course. For example, I think that the human receiver of information gets more out of a message that is encoded into a broad vocabulary (an extensive set of symbols) and presented at a slow pace, than from a message, equal in information content, that is encoded into a restricted set of symbols and presented at a faster pace. Nevertheless, the elementary parts of the theory appear to be very useful. I say it may be dangerous to use them, but I don't think the danger will scare us off.

McCulloch: May I ask a question out of ignorance? I meant to ask it earlier in the day. Has any work been done on the number of simultaneous speeches that one can hear with noise, without noise, with distortion or without distortion?

Licklider: The only work I know is a preliminary effort, made a couple of years ago, to compare the intelligibility of two talkers talking at once with the intelligibilities of the same talkers talking separately. In one test the two signals were simply superposed. In another they were alternated at various rates. The word lists were read slowly enough so that the listeners had sufficient time to get down both words when there were two talkers going at once. None of the listeners was able to do as well, of course, in that case, but it came out between two-thirds and three-quarters as well. Thus, even though the talkers were trying to enunciate the test words simultaneously, there was only moderate interference. It turned out that one of the two talkers had a big advantage over the other: I was the one with the advantage, and I held it over a friend with a much better voice. It just happened that he had a slightly clipped manner of speech (New York State), and my Midwestern words began sooner and ended later than his words did. So the listeners heard me first and last, and presumably therefore better. We tried putting one talker's signal into one of the listener's ears, the other talker's signal into the other. The isolation thus provided did not help much.

McCulloch: How about alternation? You clipped and alternated?

Licklider: Not clip, but blank. An electronic switch turned on first one talker, then the other.

McCulloch: Regardless of rate of clipping?

Licklider: We tried only a few rates, ones that looked interesting. None of the ones we tried proved useful in separating the two talkers.

Teuber: In aphasics that have gotten practically well, one of the last symptoms of aphasia you can detect is a difficulty, on the

patient's part, to follow a dialogue between two people in the room, neither of whom is directly addressing the patient. He may grasp pretty complicated things if you talk to him directly.

Licklider: When you figure what the pattern of a pair of superposed vowel sounds must be like, it is really a little puzzling how the auditory system ever sorts the things out, especially if the two talkers are talking at almost the same pitch. There are, of course, a number of possible clues.

McCulloch: Have you tried different pitches, and so on?

Licklider: As I said, this was not a very elaborate enterprise. We don't know any more about it.

McCulloch: Has anyone any questions bearing directly on Shannon's theme?

Licklider: I have one. At Christmas time, here in New York, Shannon defined the concept of information—not just amount of information, but information itself. When I heard him, I said to myself: "That is wonderful. Why didn't I think of that?" It was simple and very clear. When I got back to Cambridge, however, I got a hint about why I hadn't thought of it. I couldn't even reproduce it. So I'd like to hear it again, and I think it would be of interest to all of us.

Shannon: Yes. The general idea is that we will have effectually defined "information" if we know when two information sources produce the same information. This is a common mathematical dodge and amounts to defining a concept by giving a group of operations which leave the concept to be defined invariantly. If we have a message, it is natural to say that any translation of the message, say into Morse code or into another language, contains the same information provided it is possible to translate uniquely each way. In general, then, we can define the information of a stochastic process to be that which is invariant under all reversible encoding or translating operations that may be applied to the messages produced by the process. In other words, we define the information as the equivalence class of all such translations obtained from a particular stochastic process. Physically we can think of a transducer which operates on the message to produce a translation of the message. If the transducer is reversible, its output contains the same information as the input. When information is defined in this way, you are led to consider information theory as an application of lattice theory.

Pitts: Can the transducer wait for infinite time before commencing its translation?

Shannon: There are actually two theories, depending on wheth-

er delays are allowed or not. The more general type of equivalence allows delays approaching infinity, while the restricted type demands that the translation and its inverse occur instantaneously, with no delay. Either type leads to a set of translations of a given information source, each containing the same information.

Licklider: The information is the group that is generated?

Shannon: Yes. Put it another way: It is that which is common to all elements of the group.

McCulloch: With your permission I am going to omit the presentation of semantics by Walter Pitts and hold him as a whip over our heads to keep us lined up semantically from that time on. He and I have agreed that this is probably the most effective way to make use of him. If we approach the semantic problem before we go into the problems of learning languages, he and I both feel it would be somewhat contentless, whereas after we get the problems of learning languages in the open I think it will be extremely useful to us. We shall continue then, as follows: we shall first ask Margaret Mead to give us a picture of how one learns languages if he does not know the languages of that family or the culture of the people, languages for which there is no dictionary. That is a situation in which an adult consciously attempts to break a code.

REFERENCES

1. Shannon, C. E., and Weaver, W.: *The Mathematical Theory of Communication.* Urbana: The University of Illinois Press 1949.
2. Shannon, C. E.: Prediction and entropy of printed English. *Bell System Technical Journal,* **30,** 50 (1951).

EXPERIENCE IN LEARNING PRIMITIVE LANGUAGES THROUGH THE USE OF LEARNING HIGH LEVEL LINGUISTIC ABSTRACTIONS

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I AM GOING to handle this primarily as a case history because work of this sort is so idiosyncratic that it is impossible to make general statements of how anthropologists work. From this general field of experience, I think that what would be most useful to this group is a description of what happens when an anthropologist who has had linguistic training, such as I had with Boaz, goes in equipped with the widest possible frame of reference to face a new language. We had systematically been broken of our Indo-European categories, though not with complete success. Boaz used to try to illustrate how bad it was to use Indo-European categories in taking a grammar of a non-Indo-European language, such as the early Spanish treatments of Aztec. But we learned Aztec faster than anything else, so the demonstration was not as complete as he meant it to be. Nevertheless we had, as students, systematic exposure to a variety of languages, and particularly American Indian languages, which broke down our Indo-European expectations about the way in which language is put together and widened our range of expectation. A general picture of the type of expectation given the student body may be got from Sapir (1).

What we learned was that any type of statement may be made in almost any way. Tense may be embodied in inflection; it may be handled merely as a particle. Almost any syntactic device may carry almost any type of information, or the same information may be simply relegated to vocabulary in one form or another. So you are given an enormous expectation to work within as you approach a new language. You also are given a very wide range of possible mechanisms. You know about suffixing, infixing, and prefixing. You know there are varieties of gender. I think perhaps the easiest illustration is recognition of what we call technically "formal gender," that gender can be divorced from sex, and that you may expect to find a language that will classify

round things, long things, sharp things, in groups, and handle them as we handle gender in Indo-European, with differentiation of pronouns and numerals. There are a whole series of expectations of that sort that one has. One also has some phonetic equipment. I, myself, have an abominable ear. I am really very bad at the phonetics, so that I will not be able to say anything very significant phonetically. I do not use phonetic notations when I am learning to speak a language. I use approximations out of the English alphabet, sometimes writing four or five letters one on top of another. I found that was an easier way to learn. I was concerned with learning to use the language. It is important to emphasize that my concern was to learn the language. I was not going in as a linguist to get a perfect record of the language, which would add to our knowledge of language, as such. I was going in as an anthropologist. I had wanted to work with the culture, and the native language was the only possible way of working in these cultures. There would sometimes be only four or five reasonably deculturated young adult males who had gone away to work for Europeans on plantations. With them I could communicate to some extent in pidgin English, but they were not the people I wanted to talk to; so while they could be used as a device to learn the language, it was still necessary to learn the language.

Another point that I think is significant is the type of ego involvement of this task. It is very different from the situation that arises when an educated European comes to the United States and wants to discuss things with his scientific colleagues.

I am going to talk primarily about New Guinea because it gives the most vivid picture of an area where we know nothing about what kinds of languages would be encountered in the interior. The European or American who speaks even a fraction of one of these unrecorded languages is regarded as a miraculous person by the native for the reason that no European ever has taken the trouble to learn them before. You do not want to discuss modern scientific thought with the native. The less you talk with the natives and the more you let them talk, the better it is. It is primarily teaching oneself to use a special kind of tool. You do have to establish some points of high specificity. You have to learn to ask questions perfectly. I spend a great deal of time learning to ask questions, mastering the appropriate interrogatory phrases for different things. You have to give commands accurately for your own self-preservation, so you get your dinner, things of that sort. You have to learn to make appropriate remarks; you have

to learn to give the signs of grief, pleasure, and other emotions which are verbalized in the society, the little or partially verbalized aspirations; you have to know when to sigh, when to give a sharp exclamation. If one goes into a strange society and can do these three things, ask a question accurately, give a command accurately, and gloom and exclaim and enthuse at the proper moments, most of the rest of what you have to do is to listen (2).

Wiener: Never use a period but a question mark or exclamation mark?

Mead: It does not matter so much in interaction. You actually don't, as a rule, turn to a native and say, "You have five clams." There is really no point in a declarative sentence. What is appropriate from you is an interrogative sentence, "How many clams have you?" a command, "Tell me more about that fishing trip," or an exclamation, "My, weren't you wonderful!" Those are the three positions in interaction.

Pitts: Position is very much more like the child learning the language in our own culture.

Mead: Yes. Dr. Wiener, one does make some declarative statements. One says: "I have no more yams." "I want no yams."

Wiener: Compare with other statements, with exclamation point or interrogation.

Mead: They are not primarily the important statements. They are the incidental statements. You work through the lingua franca. I have only once had the experience of having to learn the lingua franca at the same time that I was learning the native language. In other cases I have always had some lingua franca or English that I could use. In learning the Admiralty Islands Manus language, I had a schoolboy who understood a little English and spoke pidgin, which I did not yet know; using him as an interpreter, I had to work out the grammar of the local language. That isn't quite as bad as having to work out the lingua franca completely oneself. Pidgin English is a language which has Melanesian grammar with an extraordinary polygot vocabulary from all over the world, and you have to learn how to use a very large number of circumlocutions. You can get on with about three hundred words if you know how to circumlocute accurately. Therefore it is virtually an ideal language to acquire rapidly as a medium working with a real language. A relevant point that I didn't make yesterday is that pidgin is probably the most redundant language in the world. But its redundancy is functional and it is a substitute for the time that it would take any of the people speaking it to learn the other people's languages. You can describe

anything in the world in pidgin.

Pitts: Is this true of all pidgins?

Mead: I am talking about the pidgin English of the mandated territory (3). It is true of most pidgins that I know about, with the possible exception of Chinook, which I don't think we ought to discuss here.

Pitts: It might be a good case for vocabulary survey of philological elements necessary to say what a language of human beings must have. Of course logical languages are exactly all the same way.

Mead: On redundancy?

Pitts: That same principle: as few basic elements as possible and build out everything from them in philological combination.

Mead: Let us consider that one begins by knowing what to do. One wants to be able to listen and one wants to be able to interpolate often enough to see that the material that one listens to is what one wants to hear. One wants to be able to ask: "Who was that? Where were they going? Why were they there? What relationship were they to X?" One very rapidly begins to master the types of questions that it is possible to ask in the society. We wasted much time, for instance, in the village of Bajoeng Gede, trying to get people to answer the question: "Why did so-and-so take an offering to a particular temple?" because we thought they could answer it. It turned out to be a question they could not answer. The quicker you learn which areas are not articulate, the better. Ideally, the ideal method to do that—

Savage: When you say "could not answer," do you mean they could not conceive the answer?

Mead: They did not articulate to themselves the reasons why they did these things. Therefore the question was meaningless.

Pitts: You asked why they did?

Mead: Yes.

Pitts: You should have asked whether.

Mead: Why did somebody not do something? People in some cultures will give an explanation. The Samoan will tend to give an explanation in terms of physical defects. "Why did somebody not make a speech?" "Because he has a bent toe." The explanation has no reference to the event, but the explanation will be given (4). So one of the things that you want to avoid, if possible, is meaningless questions.

Werner: Is that because they want to give an answer, or is that because they are just confused?

Mead: Because they, the Samoans, have a general tendency to

refer aberrant behavior to obvious and trivial physical conditions.

Fremont-Smith: Rationalization in simplest form.

Mead: It is a statement about other people. It is not an attempt to explain away a motive.

Fremont-Smith: It is about other people as part of them.

Mead: It is an easy social way of doing it.

Kubie: We do the same thing. Why does somebody do something? The average person says, "Well, everybody does it." They think of that as an explanation.

Mead: This is another type. In learning a language, you make specific explorations of the interrogative structure of the language, rapidly, in as many kinds of interrogative statements as you can think of. This also gives you a picture of what it is going to be possible to ask, the things you get into trouble with if you try to discuss them.

To shift for a minute to what you actually do, there is one other point I learned when I first started to work on languages—to learn from children. This may look like a contradiction of going in at a very high level of abstraction based on a knowledge of possibilities derived from the linguistic structures of the world and at the same time learning from children, but it is much the quickest way for me to learn a language. I have done both, learned from adults and from children, in different circumstances. Children will repeat and repeat and repeat; adults will not. It is very often difficult to get an adult to say the same thing twice unless you have trained him to be a linguistic informant. He said it once, and there is not a very good reason why he should say it again. If you ask what he said, he says something else, whereas children are accustomed, in every society that I have worked in to practicing the language. They expect to practice and a child is able to comprehend that you are learning a language. In many of these societies the majority of the adults really do not comprehend the process of learning in another adult. You don't know the language or you do know the language. The minute that you learn to say six things correctly, they are apt to think that you can speak; they make very imperfect allowances for lack of knowledge. This, of course, is not true of someone whom you specifically train as a linguistic informant. But if you make mistakes with children that a child a year younger than they would make, they will criticize and correct it. They still have the frame of reference that says there are people who don't know how to talk their language correctly. I think that is the principle reason I learned from children, except that in any case I usually study

children. Little boys of eight to ten who are teaching other little boys how to behave are probably the brightest things you can find in most societies in the world. They seem to be the freest from other preoccupations. In learning from children, it is a matter of practice. You don't get your major points from children. You practice with children. To construct the grammar of a language is a question of getting a native who speaks a little pidgin. I am speaking here specifically for New Guinea. I never worked in an area of New Guinea where there was no pidgin. You get a native who speaks a little pidgin and you put him on a box. This is quite an important part of learning. You live in a house with chairs on which natives don't sit. There is the highly stylized European-white-versus-native situation in the home; that has to be maintained. You must take a linguistic informant from the many people sitting on the floor. You place one person on a box, which changes his relationship to you. He is then your vis-à-vis in a tutorial situation, in a context where the native is accustomed to being treated more or less as a subservient person. Then you simply start out with very simple questions, such as, "I see a dog."

Savage: What kind of a question is that?

Mead: You say: "Suppose you talk place. All right now, how's that you talk! 'Me lookim one fellow dog'." If I bowdlerize pidgin, it is close enough for you all to follow, isn't it? That would make it a useful device here. The native gives you a statement in unfamiliar phonetics; he may not say, "I see a dog." He may say any number of other things instead, because he has not gotten the idea yet. You have to allow for that. We assume that about 50 per cent of the time he does say what you asked him to say. Is that fair, Dr. Bateson?

Bateson: I think that 75 per cent of the time he would say, "You saw a dog."

Mead: Exactly. All those varieties enter; then you put in what he said with as accurate phonetic rendering as possible. That is an approximation. That is the place where your ear has to come in while you are trying to isolate the word for "dog." Once you have isolated it, you can mispronounce it within limit for the rest of your life with safety, but not before you have isolated it.

Pitts: You begin with questions about where, of the form where, when, and so forth?

Mead: You begin to give simple declarative statements that are variable, such as: "I see a dog; a woman; a dog goes; a woman goes"; but you don't know what tenses are going to be like.

Pitts: What do you ask: "Did you see a dog?"

Mead: No. You say, "You talk place." That is the pidgin English for "talk your own language."

Pitts: I see.

Mead: You say, "Suppose you talk place." "Place" is the phrase used in interpretation for "our village" in the European New Guinea world and the native's "You talk place. Now you talk: 'Me lookim one fellow dog'." Now perhaps he puts "dog" in that sentence. Perhaps he does not. Perhaps he says, "You said 'dog'." Perhaps he puts the pronoun in instead of the word "dog," and says the equivalent of "I see it," but you write down what he said.

Gerard: You would not use the ostensive definition of the word by pointing to a dog and saying, "Say that word?"

Mead: I usually go at the word and crack it.

Bateson: Finger.

Mead: If you pointed to the dog and asked for the word, as likely as not he would give you the word for finger. You have got to deal with whole statements. You could go around forever trying to get the word for house, dog, woman. You never would know whether you got woman or that particular woman's name, house or the word for the men's house. When we work with a culture, we start with wholes, then get the main pattern, and then work down toward details. You have got to get basic grammatical structure to know what the details are.

Let's assume that he did say, "I saw a dog"; you then ask him and you make a guess, an approximate guess of what in that sentence is "saw" or "looked at"; what is "dog." Then you work those through. You ask for "I saw a dog, a house, a man," all sorts of things.

Stroud: Twenty questions?

Mead: The principle of excluding possibilities. You may have missed something. In that language you may look at a dog with one verb and a woman with another. You may keep on getting replies in which there are still too many variations for you to tell which is the verb or the noun. That is just a question of excluding until you get it. Then you make a quick stab at all the possibilities.

I work on tense as soon as I get a notion of the nature of a verbal statement and a set of nouns that are identifiable, so that I am reasonably certain that this word is "woman" and this is "dog" in the discussion. Then, "Me lookim today." Pidgin handles tense entirely by words like "today," "yesterday," "tomorrow." "Me lookim yesterday, today, bye-and-bye me lookim," running through the tenses for the verb "to see," to get the notion how tense is going to work out. You start working on, "Me lookim,

you lookim, emi lookim," You will get "me" for "you" and "you" for "me," and so forth. But you aim in a very few days to have your basic grammar well enough to know how tense is handled, whether you have got inflection or whether you haven't, whether you are going to have to deal with modifiers that are related to the adjective or embodied in the noun, whether verbal ideas are expressed in the verb or by particles or by a complete change of stem, whether you are dealing with a language in which you have to deal with infixing, and so forth. I have never had to deal with a language that made one of these changes that has to be treated as a series of parentheses. There are languages in which the occurrence of one sound alters the sound pattern in subsequent words; it is like a change of sign with a series of parentheses, as in a mathematical statement. I have never had to crack one of those languages myself. We know that it is possible to do so, and that it is not too difficult to crack it, but it takes a little longer and it depends upon your ear more than some of the other languages do: You don't expect in the first two or three days to discover the refinements; also, you may be trapped in a point of change. For instance, I worked with a multiple-gender language which was changing from a multiple-gender language to a two-gender language. It was changing from a language in which there were a large number of phonetically differentiated genders, differentiated in the singular and plural, in which the pronouns, the adjectives, the numerals all fit the phonetic pattern of two-gender-language usage, animate and inanimate, breaking the phonetic congruences. Because the change was being made slowly, people still used both the old forms and the new forms. That sort of difficulty will catch you, and it may take you quite a while to solve it.

Wiener: General languages like Mantu.

Mead: Sometimes such genders are not meaningful, sometimes they are completely phonetically crystallized. You may have a pronominal pattern that fits with noun classes, but you no longer decide by analysis what that noun class meant. Sometimes you can.

Wiener: Would you call, by the way, the Chinese use of qualifiers a gender extension, *egoshim, ekul, dogya,* different words for the one piece?

Bateson: They are still related to genders.

Mead: We call them piece words in the analysis of Malay. They are related. In these New Guinea languages occasionally you have very complex structure. I worked with one that had thirteen

genders (5). Each gender has one to four phonetic peculiarities, and pronouns reflect these phonetic patterns in nominative, objective, possessive forms, and so forth. Now one form of Arapesh was once learned by a German missionary who never discovered there were genders. He had a perfectly good category to apply to the gender. He called it euphony. You have a word for dog which is *nubot*. *T* is the ending. When you say, "I saw a dog," you say, *Ya terut*, with *t* at the end of it. When a dog is the subject, the pronoun is *ta*. This phonetic correspondence runs all the way through the language and can be called euphony, so these missionaries learned to communicate with the natives without any recognition whatever that there was gender there. But I imagine it took them a great deal longer to communicate than it did me, because I had the possibility of multiple gender to work with. So you have your problem of cracking your essential grammar and then building vocabulary and building it fast enough. I build vocabulary entirely in terms of what I expect to need. Ideally, one would have thousands of pages of text that are to be taken down first. A linguist would first have made a study of the language. One would then know what vocabulary was relevant to the culture and what was not, without wasting time organizing the vocabulary along our lines. That is not possible when you have money for seven months and food for only five and you don't know whether the carriers will take any more food in.

Teuber: How important would it be in this situation to know something about possible differences between their tenses and ours? There is little chance of finding a one-to-one correspondence between their time structure and the three major tenses of our language.

Mead: You have got to know how tense is formed, otherwise you would be in great difficulty in no time.

Teuber: Suppose they have a half a dozen tenses or more, do you find that out in the first week?

Mead: Oh, yes, in the first week. You have got to get the mechanism for tense, for number, and so forth. You have got to know whether or not you are dealing with a system of pronouns which distinguishes sex.

Teuber: How do you do it? Do you do it on the basis of some general linguistics, looking for different ways of slicing up the time scale?

Pitts: You learn how to say things yourself, not how to understand them, apparently. You learn the translation of our tenses, even if the translation apparently involves just the use of adverbs

and not tenses. This is not learning to understand arbitrary sentences of theirs but being able to express arbitrary sentences that you wish to express in this form of question.

Mead: I learn to listen. I ask, "How do you say, 'I go; I went yesterday'." But you don't start with the intransitive, like that. You start with the transitive because you get the "dogs" and the "cats" in easier. But after you have got a little bit done and you are really sure this is, "I go," you can have, "I went yesterday, I went the day before yesterday, I went a long time ago, I was going but I didn't go," and so forth. You can range through the possibilities of tense and mood and conditional statements; you find something you didn't know was there at all.

Wiener: How general do you find true tenses and how general do you find aspects?

Mead: I don't know what that statement means.

Wiener: Ordinary tenses are placed in time with respect to the present. The aspects are placed in time with respect to other parts of the assertion.

Pitts: Roughly you have to try the statement with all possible adverbial conditions, don't you, in order to see which ones collect together on the common grammatical form?

Mead: You need to know, in Melanesian, for instance, whether there is a distinction between the incomplete act and the completed. That is essential.

Wiener: That is aspect.

Mead: That is exceedingly important. Suppose, for example, that the first day you are in a new village, your pidgin-speaking boy announces, "Mary (wife) belong Paleoa, emi die," You rush over expecting to find a corpse, but you find Paleoa's wife sitting up and talking. You turn to your boy and demand what in the world he meant, and he replies, "Em i no die finish, i die that's all." As you explore it, you discover new points. When you ask what is commoner, I don't think we know whether it is common to say, "My position in the past or my position in the future," or to work with aspects; you get all varieties. You don't know what you are going to get.

Wiener: Chinese has no tenses, only aspects.

Teuber: You check against sequences of actions observed in that particular context?

Mead: Yes.

Teuber: Then you try, not only to get what you can in the give and take of the language situation, but you try constantly to fit what you have learned, tentatively, to particular sequences of

action that you observe?

Mead: Then you start taking tests. Almost at once you start, just as soon as you can hear the language at all. Now Dr. Bateson would start with the text much earlier than I would. He has a much better ear. You would start with text almost immediately.

Bateson: Yes, but I start with substantives.

Pitts: Proper names?

Bateson: No, general substantives. "Man he fight him dog." Man hits dog. With a little vocabulary I then start telling a story which my informant is giving me back in native language. I tell the story in pidgin, phrase by phrase, and he gives it back in native language.

Pitts: You get into difficulty with the special tenses used for foreigners when you try to use the language in telling the story.

Bateson: The difficulty lies in special languages for foreigners or for castes.

Pitts: Then you might possibly encounter difficulty.

Bateson: And you are outside the caste system—an outcast.

Pitts: You would not learn how anything is said at all.

Mead: In Bali, Malay is the only appropriate language in which to talk to the foreigner. A Balinese can talk to you a long time in Balinese about the fact that neither of you can discuss anything because you don't speak Malay. You can get in that sort of situation, but there are always such possibilities. In Balinese we distinguished seventeen different degrees of gradations in the language, depending upon who is speaking to whom, or about whom. Such distinctions are a dreadful trap because sometimes you have no opportunity ever to hear the "rough" language spoken to you. There is no way in which you can get into a position that is low enough so that you can be spoken to as a low-caste child. Then you have to use an interpreter and get somebody on a lower level than the interpreter; you say something to the interpreter and he always answers you in high language, but he turns around and speaks to the other person in the low. Then you hear it. It is much harder to move in and speak a language that you never hear spoken to you. You always have to give an answer in a form different from the way the question was addressed to you. That is another complication.

Wiener: I am rather curious about that. To a minimum extent that does exist in the European languages and in Chinese. On the other hand, these different phrases of courtesy are being simplified, not only in most European languages but also in Chinese. Do you find a tendency for that sort of social up-and-down expression

to be fluid and to be changing while you are looking at it, as it were?

Mead: It is a changeable system; and if Europeans are entering the system, or if there is a breakdown of the social system, that is a part of the language that responds very rapidly. In Balinese "I" is one of the very crucial points; there are whole sequences of "I." In our mountain village there was an "I" you only used if you were born in the mountain village; it was also very low, familiar, and rough. There was a slightly higher "I" which was not quite as rough. You could use it in low-caste conversation to low caste if you were not born in the village. Then it went up the scale. The Balinese have been trying to get out of saying "I" by every known device for a long time.

Wiener: We are trying to get out of saying "I" or "you."

Mead: We are. "I" or "you" is a crucial point in the caste languages. Our Balinese interpreter, who was Dutch educated, used the Dutch "I" to his brother, who used it to him in order to avoid the situation; but he would also use the Malay "I," which is literally "your slave," but he did not know that. That was a way of avoiding the hierarchical relationship.

Pitts: Why is it that they avoid using pronouns only when the culture is changing? I suppose if there were a fixed system you would always know what part of the system to use.

Mead: There is Dr. Wiener's question: Do you find this a particularly fluid point? My answer was that when the culture is changing social organization, when new people are coming in, then it becomes exceedingly unstable.

Pitts: If you left the Balinese alone, you would not have trouble.

Mead: The Balinese have a composite culture, a caste system imposed on a large group of casteless persons. Sometimes a low caste tries to have the kind of cremation tower appropriate for a higher caste, and so forth. I imagine they have been playing with pronouns for thousands of years.

Wiener: It is almost impossible to guess a Swede's position from his outward dress and courtly manner.

Mead: In Bali you have a regular conversational form which is used until you find out the caste and status of each speaker, a form of courtesy between equals. You use this to people until you know who they are. You ask where they sit and they tell you. Then you know how to address them. Until you know that, you cannot really talk with them. If you are going in as an anthropologist, you also learn the vocabulary of technical areas rapidly. Kinship terms, for instance, you learn almost at once, because you cannot

operate in the society without a knowledge of the kinship structure; also, it may overlap the pronoun structure. You may, for instance, refer to your mother-in-law in the third person, as "they three," analogous to the royal "we." You may call a pregnant woman by a dual and a married couple without children "they three." Unless you learn those things very rapidly, you cannot understand what is happening. You have to enter those interpersonal situations for information before you can understand the next situation.

Pitts: At the stage we are at, do you translate, do you try to translate a folk tale which you have in your bag in the evening?

Bateson: I do.

Mead: Thanks, Dr. Bateson!

Pitts: You have no special tense for experiences?

Mead: A special tense for folk tales?

Wiener: Once upon a time.

Mead: Yes, once upon a time. In Samoan, for example, it is *ona . . . lea* with the predicative statement inserted. This form is also used for cooking recipes.

Pitts: That would not be useful for practical purposes.

Savage: You emphasize in the beginning, as anyone who talks about linguistics does, that one has to break the stereotypes of his own language—even of his whole language stock. What has never been clear to me is whether linguistics has discovered enough generalizations and principles so that you are not left with the feeling that anything whatsoever can happen. In approaching a grammatical question about a new language, can you say to yourself, "Well, only these few hundred things can happen, and by systematically dichotomizing, I will find the right one," or do you have to allow for the possibility that there will be a special tense referring to the future actions of men whose eyebrows happen to be of a certain color?

Mead: You still have to expect that unpredictable categories may occur.

Pitts: It is fixed. At least one man in the culture has learned pidgin English.

Mead: This much is fixed, that as far as we know, all human languages are learnable by other human beings. So far no has found a language that could not be learned, although there are languages which present great difficulties. Tchambuli is such a language. Only about 600 people could speak Tchambuli in 1933, and the peoples around did not attempt to speak it. This was a language in which the grammatical structure was in process of

simplification. Some of the boys who went away from Tchambuli to work refused to speak it when they returned; they insisted on speaking a neighboring language to their own parents. But Tchambuli is not as difficult as many American Indian languages. But most American Indian languages were expected to be difficult, whereas in New Guinea most languages are easy enough so that the average native expects to "hear" the languages around him to some extent. They make a distinction between "speaking" and "hearing." You "hear" the languages that are adjacent to you, and in border villages people sometimes speak two languages without knowing which one is primary. From the standpoint of other villages, this will be a village of which they say in pidgin, "they turn the talk," but from the standpoint of the people who are born and live there and speak both languages, they are not quite clear what they speak.

Now, Dr. Pitts, to get back where we were. You very quickly try to get a mass of material of some sort. Dr. Bateson gets it by telling a story and getting them to tell it back; it is also possible to get them to tell you a story or to get them to dictate a description of a quarrel that had just happened. I do this very early, if possible, because in this way I have some control of what has actually taken place in front of my eyes. Folk tales I hate. They are likely to be filled with archaic forms, pickled clichés not used in real life. They are not nearly as useful in getting on with life. Folk tale texts are the traditional linguistic method, but they are awfully poor, as a basis for field work. It is as if you were to try to talk to people entirely in terms of *The Idylls of the King* or the King James Version of the Bible. You meet natives who have learned English through the King James Version of the Bible. Unless you know the King James Version type of English well, it is pretty hard to understand. So I stay away from the archaic and turn to the living stuff. I turn it into interpersonal stuff, rapidly building vocabulary. I learn about two hundred words a day of a language and in about six weeks I have a workable vocabulary. How workable depends upon the culture. However, I can get around. One may know every word that relates to marriage and be helpless when it comes to funerals because the vocabulary is so specialized. On the other hand, it may be a language where all ceremonies have about the same form and are treated the same way, so by the time one can verbalize about people getting married one is also able to bury them.

Brosin: How do you motivate a translator to give you that amount?

Mead: You have to do two things. You pay them. This becomes a context in which it is correct for Europeans to pay natives. He becomes a work boy of sorts. And you have to get somebody who is interested. You have to move fast at all times. A linguistic informant has to be an intellectual. The intellectuals seek you out reasonably fast, too. They are bored, and the chance really to talk to somebody is offered them probably for the first time in their lives. They are usually natives who have never had a chance to talk enough, that is, interestingly enough. The first boy that you seat on a box may just sit there; he may get restless and start to wriggle and not stay very long. By that time what you are doing gets around, and other people come and stand about and listen. Somebody else comes into the picture, and finally you get your linguistic informant and he loves it. The main motivation is virtuosity.

For example, in Arapesh there are two ways of absorbing foreign words in the language. One is by looking at its structure roughly. You take the English word "butter," for instance. If the native has heard the word "butter" pronounced by an American, it then goes into the gender that ends with an intermediate "rl." The plural would then become *butigu*. If, on the other hand, he heard it pronounced with an Oxford accent and it becomes *buttah*, he can put it into another gender approximating this phonetic ending or he can say that the ending he hears is not like anything in his language. He can say, in effect, "Let us treat it like those words that are indeterminate." There is a gender which includes creatures of indeterminate sex. The word for "child" is in that gender, objects of indeterminate form where form is specified elsewhere. The plural forms of this gender are used for men-and-women, people, all the different things on this table, several kinds of fruit, and so forth. You can put a foreign word in this gender. We had two linguistic informants (6). One of them had a very formal mind and insisted that all foreign words belonged in the gender for foreign words, even though you could make them fit phonetically into other genders. But our principal linguistic informant had a lovely fluid phonetic flexibility. He would follow the sound around, work around it until he could fit the sound into the gender structure. Both were possible, and it was wonderful to argue about. So your linguistic informant is an intellectual who gets fascinated and to whom you teach the grammar as you learn it. Now sometimes you have to teach more than at other times; you teach such concepts as that of a *word*. You decide how you are going to write the language. If you are going

to write it, you are going to have to see something of the gender that ends in *t* and this *u*. *T* is an objective pronoun; you decide whether to handle it as a suffix or as a separate word. Once you have made up your mind on the subject, you then use the idea of a word, and if necessary of suffixes and prefixes. The Balinese when he wrote in his traditional script did not break it into words at all. But words are convenient for use and easier to think in. So, in the end, if you are using the concept of a word, you teach your linguistic informant in what size units you want him to answer. Sometimes there is a word for names of things; pidgin has the categories "big name," "lik-lik name"; "big name" is the class name and "lik-lik name" is the name for a particular type of object within a class. So the "big name" for birds, for instance, is "pidgeon." There will also be the name for a particular kind of bird, and then you may find another type of bird that is rather like a pidgeon but unnamed. You ask, "Now what name lik-lik name belong im," but there won't be a lik-lik name; instead it will be described as, "This fellow pidgeon i all the same brother belong." So you have categories, large name, small name, and "something that is related to something that is named, but we have not got a name for it." Now you work with that sort of loose net and gradually teach your informant how to give you rapidly what you need to crack a new sentence or a new piece of material that you have taken down.

Gerard: You indicated that you learned some 10,000 words in the first few weeks.

Mead: A good many of which I forget.

Gerard: How many words do you find it necessary on the average to know in order to break it up? That is a tremendous number of particular words.

Mead: That is entirely a question of the language.

Gerard: The number you need for the field.

Mead: Manus is a language completely analytical; there are no figures of speech, no sex gender ever, just the third-person pronoun, and very few adjectives. It is a cold, accurate, precise language of people that can count to 400,000 without any form of notation whatsover and do something with it in their heads. One figure of speech was all that we collected in nine months among the Manus. Such a language you learn very fast and can work with a very small vocabulary. It is analytical, not overconcrete. They don't have one name for the end of an object and another name for the end of another object and another name for the end point, middle piece, and things of that sort. Balinese, on

the other hand, has practically no abstract general words. If a boy is in front of you with a loaf of bread and a knife, whose task has been to cut bread with that knife every night for two years, and if you don't use the verb that means "to cut in even regular slices across the loaf," but instead use any of the other thirty or so words for cut, such as "to cut in irregularly," "to cut in asymmetric pieces sideways," he will look at you with absolute amazement and have no idea whatsover what you are saying. Balinese is a language in which you never learn to give enough commands correctly. You are dependent upon building a new vocabulary for every event. When we were there, we fortunately had an informant who could read and write. It is a most complicated society. He had to take the vocabulary down for each event. We could never have mastered the vocabulary in time. The range is from a small vocabulary to an enormous vocabulary that requires special techniques for building. If you have to have a complete vocabulary for canoe building, you don't learn that until you are ready to work on canoes; on the day you are ready to work on canoes, you sit down with somebody and work out the two hundred words you need for canoes. You draw diagrams, ask questions, get your models, write the words down, and then you forget them as fast as you can, because you cannot stand that much memorizing and still get on with the work.

What I do is to build myself a very large vocabulary of what I think I will need, which is not surrendering to the culture initially. I think I am going to need words that indicate size, for instance, or I want to know whether or not there is any way of indicating a comparative right away. That is an important thing to know. It is necessary to know what measures there are almost at once, a series of things of that sort. I need to know all the medical terminology I can get my hands on at once because the natives come in and ask for help.

Kubie: One question, Dr. Mead: It strikes me that this multiplication of names for the same object, depending upon the total relationship of the object, must mean that if that object appears in a myth or a legend or a dream, it in a sense represents all of those potential names. So that if you have two hundred names for a canoe and a Balinese dreams about a canoe, you have a hundred or two hundred starting points for his associations to the unit which he has visualized in his dream. Remember that only the visual image occurs in the dream.

Mead: I cannot get the visual image. When he tells me the dream, he has already made a selection. Now granting that that is secondary elaboration—

Pitts: Presumably it is as if we can use the wording to include canoe or possibly rowboat; you might say in a dream that you had dreamed about a canoe or dreamed about a small boat.

Mead: You work through these connections all the time. You are learning the language and the rest of the culture simultaneously (7). For instance, in Bali there is a word for what the witch does when she throws her hands up and back (demonstrating), *kapar,* which is also the word for a child who is startled by a fear of falling or for what a man does when he falls out of a tree—a gesture representative of fear. Every time you get a new word in the language, you follow it through. You don't simply ask what is the word for "good." You ask in pidgin and write down "good." But then you say, "Now what is the word 'good dog' or 'bad dog,' 'good man,' 'good woman,' and so forth," and then you try it on a whole set of connections to see. At first you try to fit things together; you listen; and the first time you hear that word in the new connection, you know. So you are continually operating between the categories you impose out of your need and the categories you hear out of what they do. They can do a lot of exasperatingly difficult things. As I said, the anthropologist has no ego involvement in talking to colleagues in polished scientific speech. But occasionally there is ego involvement of a different sort. Among the Iatmul, if a man has a quarrel with his wife, one of the things his sister's son can do is to stand outside the house and take the parts of the husband and wife, speaking first as one, then as the other—and this is a pretty problem for the recording anthropologist to unravel.

Savage: The man has a quarrel with his wife and the sister's son can do this?

Mead: The sister's son, who is in a very special kinship relationship to his mother's brothers, comes along, stands in front of the house, and starts speaking, first as the husband and then as the wife, from the extreme positions where he says that she is a something with whom he is never going to have anything to do again and he wishes she would take every article she possesses and leave, but that if she does not he is going to break them all, and so forth. Then he represents the wife answering in about the same tone of voice. This is all dramatized by the sister's son. Gradually, the speakers seem to approach each other; the quarrel gets milder. A few issues are dropped; others are intruded, and he reconciles them at the top of his voice, taking the positions of each. The first time you hear a thing like that, it is difficult to recognize in a strange language. You don't know what is happening unless you

have had the luck, before you hear one of those speeches, to have had it described to you by a native as a possibility. But without the knowledge of the possibility, if you suddenly hear a man speaking as a woman at the top of his voice outside—and it depends upon how much masculinity and femininity there is in the language—it is rather astounding. Iatmul has a feminine "thou," so you get a clue from that particular point. But different peculiar events of this kind occur simultaneously with your learning, and it is very much a question of luck how fast you learn. You may be stuck for quite a long time because you have not got the point.

There is just one other point that I want to make. I think it might go with child learning here. Cultures differ enormously in the extent to which their members think they have a language, and think about language, and therefore the learning of the child differs; that is one of the things that I experienced when I tried learning as a child. I once published the fact that I make the same mistakes that children do, which was true at the point that I published it; but I don't think it would be true of learning all languages. I make the same mistakes that children do in two different ways. I may make the mistakes of ignorance, of course, as they do. In Arapesh, every time that a child acquires a new noun it is acquired in a way that stresses the gender forms. So the child says: "What is this?" The word for *x* itself may end in a *t*, which would mean that the correct form for "one *x*" is *atut x*, and for "many *x*" would be *minahigi*. But the adult will answer by initially using the forms for "one" and for "many" which belong to the indeterminate gender which I discussed above, but using the correct plural form for the noun itself. So the first time the child hears the placing of the noun, by its plural, this is done in a way in which the usual gender correspondence is violated, and so the abstraction is conveyed to the child.

Savage: The parent does not teach the gender properly but the child knows automatically?

Mead: No, the child says, "What is this?" and the parent says the word for pencil in its accurate form but uses first the word for "one" which is limited to the nondefinitive gender. Then he says, "*Minihisi*" (many). The "one" and the "many" remain constant in the teaching situation and give the child the specialized statement.

Savage: The child is not confused by this irregularity? He knows this is the irregularity for teaching words?

Mead: The child learns the genders. The child learns that there are both the special forms and this *minihisi* form, which is any

kind of plural, just as we say to the child, "This is the singular, this is the plural." But we don't say, "This is the singular, this is the plural," to the child, as a rule, until it is five or six.

Wiener: There is a very interesing thing: the difference in teaching Indo-European languages with the gender where only in the teaching situation do you give the article to a great many words.

Mead: Yes, but another sort of parallel thing can happen, as in Manus, which is a very analytical, clear language on the whole. There are three particles in Manus indicating direction, up and away, down and toward, and something that can be defined roughly as "along." Children make terrific mistakes about whether now I am here or Warren is there. They do not know whether to say, "I go along to Warren," or, "I go away from myself toward Warren," or "Warren comes toward to me," or, "Warren comes along." There are three possibilities. Children make mistakes all the time, and they are concretely corrected, you see. So I would say, "I go up and away from myself toward Warren." They would say, "No, you go along to Warren." They wouldn't give any reason whatsoever. The child gets that correction. The next day however, if I am working and I am in a position like this, I may say something involving direction and again I may be corrected, but I will not know why. The children keep making mistakes, but by the time they are grown up they don't make the mistakes. The Manus don't know that they use these words because their conceptions are based upon a geographical model that is not conscious, that is, that the world is shaped like a saucer or like a platter. The village in which one lives is conceived of as lying along a long axis, as if it were at the bottom of a platter. If you are going along the bottom of the platter, you use the word "along." Otherwise you use the words "away from me up." There is no way for a child to learn the abstraction. When two people are going together, you use "toward me down" or "away from me up."" In this case the child's incorrect usage is because the teaching is arbitrary, because the form has not been articulated.

I think that is about enough, Dr. McCulloch.

McCulloch: I think that gives sufficient idea.

Wiener: I was going to say that that difficulty appears in many European languages, in Spanish, *agui, aqui,* and *aqua,* the three different forms of place. The confusion is the same as that in Dr. Mead's example, the near there and the far there.

Mead: Or you may have "near me." It is very common in

some of the Oceanic languages to specify, "near me, near you, and near him" in combination with "coming to your house" or "going to your house." In some languages these forms are articulate and teachable, or are implicit in a form of teaching which actually articularizes the form of the language. In others the adults merely correct, and the correction does not contain the generalization.

Kubie: We have similar uncertainties and overlapping forms, all of which carry slightly different emotional connotations, as, for example, the difference between saying, "Come to visit me," or "Drop by some time." This is quite like what you are describing.

Mead: Some of these expressions we tell children not to use, some we tell them to use in a different way. Sometimes we tell them a form is "common"; sometimes we tell them it is incorrect; sometimes we tell them it is bad grammar. We have different degrees of articulating class positions or formal position, and it makes a great difference to the manner in which the child learns the language. As far as we know at present, however, from the data we have, children in all cultures learn to speak at about the same time.

Pitts: We correct children by different methods in grammar or manners. In this case we would correct the child where it uses the grammar badly.

Mead: Calling it "grammar" when class is meant, or a class phrasing when grammar is meant, does not help much.

McCulloch: As to the learning of languages by children, you say it does not make much difference whether they teach or do not teach, particularly; that the child learns the language at about the same time?

Mead: Yes. Undoubtedly it learns it very differently, its sense of facility, its sense of being able to move around within linguistic forms. The Balinese don't really think that language is ever easy to use; they conceptualize with great difficulty. They think there are endless situations which cannot be tackled. The child learns in Balinese by hearing the parent put words in its mouth all the time. The mother speaks for a three-months-old baby and makes a speech to other people, thanks them, and uses the very best language she can use. The child always has a model presented for it by someone who is in fear of making a mistake. The child learns to talk, but it also learns this fright, this sense of paralysis if it doesn't know who the other person is, if it does not know who it is in respect to them. Even if it knows, it may make a mistake. Balinese varies every part of speech by caste or position, not only nouns and verbs but also participles. This sense of partial

paralysis survives all their life. You learn to talk but you learn to talk with a sense of near paralysis, you learn to talk with endless ifs, whereases, lests, and so forth.

Among the Mundugumor of New Guinea, children, little boys of three, four, and five years, are sent away as hostages to another tribe while a joint head-hunting party is being planned. The child knows that the people among whom he is a hostage will later be his enemies again, and one of his tasks is to learn the language and learn the roads. If he learns the language, later on this knowledge will be very valuable. Here the tone of voice is entirely hostile, and little children master the tone of voice of their host.

McCulloch: Is there any difference between the linguistic structure in those languages which are taught and those where the child just picks it up as best he can?

Mead: Not that I know of. We don't have the details of the learning of American Indian languages. We have not enough material on the learning of these languages to know to what extent the children learn in lumps where the adults virtually never analyze and simply deal in whole predicative statements. That is something which could be studied. It ought to be very valuable.

McCulloch: May I ask one more question? There is distinguishable in our civilization a prattling among children who cannot form words sufficiently to speak. The whole speech is opposed to the learning of particular words.

Mead: I would say there are two elements: individual differences in children—and some children will prattle under any circumstances—and the manner in which the society attempts to pin you down. The Manus *teach* language in the sense of getting the names of things and getting them very early. Older children teach younger children all the time. The minute the child can form words at all, nobody is interested in his making an imitation of a speech in nonsense syllables. They want him to be able to talk and to talk accurately. The culture can inhibit prattling by patterning the language teaching, especially if older children teach younger children. Manus children sit by the hour making other children count, making other children name their fingers and toes, going over and over the simplest form of learning. This has a very inhibiting effect on the prattler. In a society where nobody is very much interested in what the child is doing and does not care awfully when they learn, the prattler has a little more free range.

Teuber: Prattling and babbling are not the same. You do not

know how to determine which is prattling and which is babbling?

McCulloch: A Fourth of July address.

Teuber: Which comes later?

McCulloch: Which comes a little later.

Stroud: The kinds we invent in sixes and sevens, special private worlds.

Wiener: They cannot speak at all.

Stroud: My one-year-old daughter, with a very appreciative audience, will deliver an oration which consists of the syllable "da," but I would not for a moment hazard the faintest guess what it is about.

McCulloch: That is what I was referring to. That is the real prattler.

Teuber: I see.

Pitts: Actually you do have to make the assumption at the very beginning that there will be on the whole, and with reservations in different sentences about small common movable objects, a common sequence of the elements that you can isolate?

Mead: You do not have to make the assumption that people will isolate.

Pitts: If you asked for a collection of sentences, "I see a dog; you see a dog," and so on, and wrote them all down, there would be absolutely nothing in common with the sequences in phonemes; you would be absolutely at a loss.

Mead: You might get a good many sentences which from our point of view ought to have common isolatable elements, but they do not.

Pitts: Suppose you distinguished ten varieties of dogs but never combined them, you would find ten classes but it would be a small finite number.

Mead: It will be finite, we assume systematically. So you can practice. You find a common name for a dog or the name for a bitch, the name for a young dog, an old dog, a dog without teeth. You would assume at every point that it is crackable, that it is a code that is learnable. If you have a large enough amount of recorded material to cross-compare, plus a living person, as of course this sort of method requires, you can crack it.

Wiener: The thing that seems to me apparent is that the languages which belong to the professions and the trades will show an extraordinary specificity of words which are not learned by children but learned by adults. I am thinking of the old-fashioned Maritime varieties of the European languages where everything on shipboard has a name, but where the word "rope" is only used,

I believe, for the bucket rope, or something of that sort.

McCulloch: Tiller rope.

Wiener: You have "staff."

Bateson: That means attached rope.

McCulloch: Rope on shipboard only refers to the tiller rope.

Wiener: The rope with which you let a bucket down.

Teuber: That is a line.

Wiener: That is a line?

Stroud: Fasten the line to bucket and lower it over the side, is that about somewhere near it? Even the modern American Navy has a terrific hangover. I get confused aboard a big ship, in spite of the fact it looks much more like an electronics factory than like a ship.

Wiener: It is the very interesting case where you have the extraordinary variety of words for specific purposes, something like what you said about the Balinese. This is language only learned by adults, but mistakes are heavily reprimanded.

Mead: You also have children's languages in which every term is highly specialized, like our adolescent jargon. Mistakes are heavily penalized, and later the vocabulary is forgotten. I don't think it is a question of adult language but of the specialized languages which carry both the techniques and the ethos of the particular subject or aspect of a culture. What we try to do when we learn a language is to isolate only those elements we are going to need. We don't learn special vocabularies otherwise, and therefore we don't get confused by overspecificity that cuts oneself off from communication within the group.

Pitts: From the logical point of view, there is considerable restriction on the supposition that the language is based upon all names of objects in a sense.

McCulloch: All are.

Mead: All languages are, all known languages.

Pitts: Mathematical is not defined in functions of space. As a matter of fact, to express the notion of physical objects in physics is practically impossible.

Mead: We had a terrific argument with the Balinese informant on the word "container," about whether it was abstract or concrete.

Pitts: There is always the assumption that there exists a class of things which are invariant from sentence to sentence, which are names of classes constructed in some way, physical objects, people, and so forth.

Licklider: It would appear that the invariant is closely related

to perceptible constancy.

Pitts: There is a very simple way in which you can imagine any language based upon coordinates and properties of coordinates of, say, visual space.

Licklider: This is an excellent example. It could happen logically but does not happen naturally, and the reason is, as I say, that all human beings have in common a brain that works in a particular way.

Wiener: In modern physics the noun "ether" has been replaced by the verb "undulate."

Pitts: As a matter of fact, it would be very difficult to express in terms of physics a statement involving persons acting on objects, such as, a man kicking a dog, or something of that sort.

Mead: There is no known language that does not contain that order of statement, no known natural language.

Pitts: The notion of a person acting upon an object. Well, I am sure that is not the notion of a personal action.

Mead: It occurs in all languages.

Pitts: As something that a man does?

Mead: The general predictive statement in which you have a subject, an action, and specific circumstances—which may include an object—occurs in all known languages.

Pitts: Certain relations are not universal. To be able to say that so-and-so acts in a certain way on something else, where the form of acting of the two parties are entities of any kind that you like, is apparently always true in all natural languages.

Mead: It seems to be true in the real world.

Pitts: It is difficult to say. Suppose you have a movie film; you don't know if any person involved in the object has any idea, series of shapes, or not. You see how the shapes move. You don't know whether any action is involved. You see what I mean? You have colored shapes on the film. You see that the colors, the shapes changed. You can imagine all as amoebae in various ways. You don't know whether any action is involved.

Mead: You can say that too. You just said it. This does not alter the fact that all languages do deal with predictive statements.

Pitts: That is a very difficult concept.

Mead: Arbitrary.

Pitts: Suppose you have three patches of amoeboid color on a movie film in such a way that they changed shapes and relative positions, possibly their colors, too, in a more or less arbitrary sort of way, but more or less continually. Then say if all three of them were circular and one bounced from one to the other, you

would say an action had taken place, or that one had pushed the second toward the third. If they were all amoeboid, you would not say that action had taken place?

Werner: The direct perception of causal relations by means of moving balls is exactly what has been studied by Michotte in his work, "La Perception de la causalité," *Etudes psychologiques, VI,* Louvain (1946).

Pitts: That seems to be a fundamental notion.

Werner: In his set-up Michotte had, for instance, presented two balls, one moving toward the other. Ball 1 may reach Ball 2, Ball 2 may move away from Ball 1. This simple visual presentation gives rise to the perception of Ball 1 acting on Ball 2, which is an immediate and unverbalized perception of causality. Michotte had also studied the threshold of such a causality perception: if the movement is too slow, such action is not perceived. I think Michotte makes the statement that the experience of causal relationships is not just a problem of verbal conceptualization but occurs on a much more fundamental level—a primary perception.

Pitts: Fundamental ideas which are apparently common to all languages without being necesssary in any sense.

Bateson: I think it is necessary to say that. Well, for example, all people, as far as I know, have the notion of a person (which is not, on the whole, a starting-point notion) that is probably used for a very great deal of differentiation of objects, the notion of action, and so forth. You start from that level rather than from what is logically necessary in an abstract way.

Pitts: It is not a question of what you start from but of what is absolutely necessary. We are not concerned with what you start from in one sense of building out but with what we must have when we finish our construction.

Bateson: Those are two different questions.

Pitts: We can generally derive one from the other, making one more fundamental or the other more fundamental, but the important thing is that we have to end up with is our minimum start.

Bateson: Those two questions become confused.

Pitts: I am sure we should keep them separate.

McCulloch: I think we ought to start with the next presentation. Is there anything that has cropped up in the earlier period that we should get out of the way before we start?

Frank: Licklider and Shannon yesterday gave us some very significant aspects of the symbolic process—we didn't recognize it as such—wherein the individual invests those symbols with certain meanings and responds to them as symbols with meanings

he puts into them. Both papers, to my mind, significantly show the way in which symbols can be attenuated with the individual still responding appropriately. That is essentially the symbolic process, which is a circular process, as in all learning situations where we put meanings in the situation and then respond to what we have selectively perceived. Symbols then can become progressively attenuated.

Bateson: To illustrate: when the white man came to this country the Indians, in order to make war on him, seeing he had script, invented an ideographic script. It had a time period of about thirty to forty years, starting with beautiful drawings, say, of a bird with feathers. That script symbol degenerated to two lines representing a bird. The point, I think, is that as the recipient of communication learns the codification system in which the messages are being sent, it becomes possible to trim what appears to be a redundancy to a much simpler message form.

Wiener: That is almost universal, in alphabets and other scripts. Although the Chinese have done tremendous things in this system, the best example is the Egyptian hieroglyphics. The hieratic and the demotic script represent successive stages that can be followed in the degeneration of the script until we have something approximating confluent writing, the writing of the least words.

McCulloch: The next thing that we covered, in substance, was the manner in which an adult, fully equipped, with the highest level of abstraction, manages to learn a new language.

What we wish to hear next, and what we should hear next, I think, is the genetic approach. Will you begin, Dr. Werner?

ON THE DEVELOPMENT OF WORD MEANINGS

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I HAVE been assigned to the task of discussing the problem of ontogenetic development of language, as well as its breakdown in psychopathological stages such as schizophrenia. As to the latter, I am not expert enough to discuss it further than it pertains to genetic problems; and as to the former, I shall limit myself to discussing developmental problems of semantics within the framework of an experiment I have conducted with a group of children.

Most of you are accustomed to analyzing language in terms of normal adult language. In my discussion, however, I shall use as a starting point certain characteristics of nonnormal verbalized thinking, that of schizophrenics, which was originally described by Domarus (1) and rather recently reformulated by Arieti (2). To show the essential difference in thinking between the normal and schizophrenic, Arieti points to syllogistic thought structure, for instance, presented in the *modus barbara:* All men are mortal; Socrates is a man; Socrates is mortal. In other words, the reasoning presented by this syllogism is in terms of identifying the subject, Socrates, with the object, men, and drawing conclusions from this identity. This is the type of reasoning which characterizes normal logical thought. Arieti defines it as the principle of identical subjects. In contradistinction to this principle he speaks about the "paleological" principle of primitive, schizophrenic thinking. "Whereas the normal person accepts identity only upon the basis of identical subjects, the paleologician accepts identity based on identical predicates." Suppose that the following information is given to a schizophrenic: certain Indians are swift; certain stags are swift. From this the schizophrenic may conclude that Indians are stags. This conclusion, which to a normal person appears as a delusion, is reached because of the identity of the predicates in the two premises. This is a very interesting analysis; but I am doubtful whether the interpretation given here, that is, the interpretation in terms of predicative thinking, is final.

It seems to me that the analysis can be carried a step further. Arieti's statements seem to imply that logic is entirely independent of language. Before drawing any conclusions, however, as to what constitutes schizophrenic thought, we first have to inquire into the nature of the language used, the character of words and their meanings. We have now enough evidence that the connotations of words as the schizophrenic uses them are much less "lexicalized," much more "holophrastic," as I use the term (3). By holophrastic expression is understood the fact that words carry meanings far beyond the conventional connotation; they have an associative fringe, and therefore to state adequately what the meaning of a single word may be, one might have to use a whole sentence or even a paragraph. Coming back to the example of Arieti, illustrating so-called "predicative identity," the question arises whether or not this analysis is not too much in terms of that of the normal adult rather than that of the schizophrenic, whether it is adequate to talk about subject and predicate as if a sentence were analyzable into grammatically distinctive functioning words. I would rather assume that holophrastic grammar does not necessarily distinguish between a word and the linguistic context in which it stands. Consequently in the premise, "Certain stags are swift," "swift" in itself carries the connotation of the whole sentence, that is, "swift" is identified with staglike; and coming back to the premise, "Certain Indians are swift," it is readily conceived as "Certain Indians are staglike."

We can probably state with Arieti that the type of logics presented by his schizophrenics is a logic which is primitive with respect to the logics in the sense of syllogistic thinking. That would mean that in the development of childlike thought one should meet similar forms of thinking. Since we mentioned that at the basis of this type of schizophrenic thinking is probably a primordial word formation which we have termed holophrasis, one would expect to find these primordial thought processes directly related to the development of word meanings in the child. Instead of presenting to you a survey of this actual development, I offer results of a test that by its nature should throw light on the development of word meaning. The test consisted of a presentation of twelve sets of six sentences each. The child's task was to find the meaning of an artificial word which appeared in the six different verbal contexts; for example, the artificial word in the first set was "corplum," for which the correct translation would be "stick" or "piece of wood." This is the test (4):

WORD CONTEXT TEST

I. *corplum*
1. A corplum may be used for support.
2. Corplums may be used to close off an open place.
3. A corplum may be long or short, thick or thin, strong or weak.
4. A wet corplum does not burn.
5. You can make a corplum smooth with sandpaper.
6. The painter used a corplum to mix his paints.

II. *hudray*
1. If you eat well and sleep well, you will hudray.
2. Mrs. Smith wanted to hudray her family.
3. Jane had to hudray the cloth so that the dress would fit Mary.
4. You hudray what you know by reading and studying.
5. To hudray the number of children in the class, there must be enough chairs.
6. You must have enough space in the bookcase to hudray your library.

III. *contavish*
1. You can't fill anything with a contavish.
2. The more you take out of a contavish, the larger it gets.
3. Before the house is finished, the walls must have contavishes.
4. You cannot feel or touch a contavish.
5. A bottle has only one contavish.
6. John fell into a contavish in the road.

IV. *protema*
1. To protema a job, you must have patience.
2. If a job is hard, Harry does not protema it.
3. Philip asked John to help him protema his homework.
4. John cannot protema the problem because he does not understand it.
5. A child should try to protema his homework when it is only half done.
6. The painter could not protema the room because his brush broke.

V. *ashder*
1. A lazy man stops working when there is an ashder.
2. An ashder keeps you from doing what you want to do.
3. Mr. Brown said to Mr. Smith, "I don't think we should start with this work because there are ashders."
4. The way is clear if there are no ashders.
5. Before finishing the task, he had to get rid of a few ashders.
6. Jane had to turn back because there was an ashder in the path.

VI. *soldeve*
1. The dinner was good, but the fruit we ate was soldeve.
2. When we were driving in the evening, we did not feel safe because things on the road seemed to soldeve.
3. The older you get, the sooner you will begin to soldeve.
4. Most people like a blossoming plant better than a plant that is soldeve.
5. Putting the dress on the sunny lawn made the color of the cloth soldeve.
6. Because the windshield was frozen, things looked soldeve.

VII. *sackoy*

1. We all admire people who have much sackoy.
2. Have sackoy when you start to do a hard job.
3. When you have done something wrong and you are not afraid to tell the truth, you have sackoy.
4. A person who saves a baby from drowning has much sackoy.
5. Soldiers must have sackoy when they are on the battlefield.
6. You need sackoy to fight with a boy bigger than you.

VIII. *prignatus*

1. Boys sometimes prignatus their parents.
2. Mary did not know that Jane used to prignatus.
3. Mother said, "Jimmy you should never prignatus your own mother."
4. You may prignatus someone but you will not get away with it often.
5. A good man who tells the truth will never prignatus you.
6. If Bob prignatus somebody, he makes sure they don't find out.

IX. *bordick*

1. People with bordicks are often unhappy.
2. A person who has many bordicks is not well liked.
3. The plan to build a house was a bordick because it cost too much.
4. People talk about the bordicks of others and don't like to talk about their own.
5. A person has many bordicks because he doesn't listen to wise men.
6. Your work will not have a bordick if you are smart and work hard.

X. *lidber*

1. All the children will lidber at Mary's party.
2. The police did not allow the people to lidber on the street.
3. The people lidbered about the speaker when he finished his talk.
4. People lidber quickly when there is an accident.
5. The more flowers you lidber, the more you will have.
6. Jimmy lidbered stamps from all countries.

XI. *poskon*

1. You should try to give poskon to other people.
2. If you believe in poskon, you are a good person.
3. The children will like that teacher because she believes in poskon.
4. People will always be afraid when there is no poskon.
5. Some bad people do not like poskon because they don't want to be punished.
6. There is no poskon when a thief is not punished.

XII. *ontrave*

1. Ontrave sometimes keeps us from being unhappy.
2. If you ontrave a good mark, you must also work for it.
3. We ontrave good things to happen to us.
4. It is silly to ontrave things that are not possible.
5. Johnny ontraved that Mary would like him.
6. According to what the doctor said, the children could not ontrave that their mother would get well.

The children taking the test were between eight and one-half years and thirteen and one-half years. The way the child, going from one context to the other, forms an overall meaning reveals

processes which in many ways resemble the semantic processes which one finds in early childhood, under natural conditions, when the child begins to build up word meanings in varying contexts. What strikes us most—and this is something I shall discuss a little later—is the fact that the word meanings, conceived particularly by the younger children, appear to be more or less holophrastic, that is, in many cases the child does not distinguish between the meaning of the word and the meaning of the sentence. Though in other cases the child may distinguish between the word and the sentence, the child may still conceive of the word as containing a meaning which is so broad that in adult language one would have to use a whole phrase or a whole paragraph to define that word adequately. We may turn for a moment back to the early development of speech in the child. Here we find, and this has been stated over and over again, that the first meaningful utterances are extremely difficult to describe in terms of our grammar, that is, we cannot state the grammatical nature of the first meaningful sound patterns in terms of adult language structure. Psychologists have mentioned that such "early words" are not really words but represent sentences, and have called them "word sentences." I don't think this an adequate term. I prefer to use the term "holophrasis." If a child says, for instance, "chair," and means by that, "I want to be put on the chair," I should call this a holophrastic speech form, that is, a protoform out of which, later on, true words and true sentences emerge.

I may give you one example to show that there is an intimate relationship between the development of words as grammatical units and true sentence formation. I am choosing one of the examples presented by the French psychologist Guillaume. A child two years old understands the command, "Brush mama" (I use a brush and brush the mother); he also understands the command, "Brush papa." However, the command, "Brush hat," is not understood; the reason is that hat has a holophrastic meaning signifying, "Put the hat on the head." We see from this example that in order to develop sentences that articulate into words, the word has to have some circumscribed connotation which makes it possible to relate it to other such units. The development from holophrasis to lexical units is an important problem of speech development, and it is this problem which we are particularly studying in our test.

Coming back to our test, then, we find that the responses of the younger children abound with holophrastic expressions. For

instance, in series IX, sentence 4 reads, "People talk about the bordicks of others and don't like to talk about their own." The correct translation of "bordick" would be "faults." One child responds, "Well, 'bordick' means here that people talk about others and don't talk about themselves." The child conceives of the meaning of the artificial word as being identical with the sentence. We can test our interpretation by seeing how the child fits the meaning of "bordick" into sentence 1, "People with bordicks are often unhappy." The child fits his concept of "bordick" into that sentence in the following way, "People talk about others and don't talk about themselves and are often unhappy." To the question, "How does this fit?" the child has this to say, "Say this lady hears that another lady is talking about her, so she will get mad at her and that lady will get very unhappy." You can infer from this example that the child was identifying the meaning of "bordick" with sentence 2 and was pretty definite about it.

I should like to point out that this is but one form of conceptualization which indicates a lack of differentiation between word and sentence meaning. What I have called holophrastic gradient is another instance. Here a word is tied up so closely with its sentential surroundings that the latter are fused with the word meaning. For instance, series X, sentence 6, reads, "Jimmy lidbered stamps from all countries," which the child completes as follows: "Jimmy collected stamps from all the countries." "Collect stamps" becomes now an indivisible unit and is henceforth used as the meaning for the word "lidber." For sentence 2 the child states, "The police did not allow the people to collect stamps on the street."

Wiener: May I ask one question? I don't think this is altogether fair, because "collect" transitive and "collect" intransitive are treated as one word. A peculiar characteristic of the English language or of a limited group of languages is that there is this double use of a word as a transitive and intransitive. I do not believe there is any simple definition of a word which does not depend on a much more mature knowledge of the language than the child may be supposed to have, particularly in using the same word in a transitive and intransitive sense. Here "libder" does not represent a notion; it represents an actual word in the English language, and there is no good logical definition of the word.

Pitts: The word is used in English transitively and intransitively. I thought the word was "gather," which has the same general meaning. Transitive verbs may be used intransitively with very similar meaning in English.

rather than the unit which belongs to a thing, as it were, as your criterion.

Werner: Again I should like to point out that an adult has no difficulty in abstracting a verb from its object, whereas a child has. Since time is short, I should like to go on to demonstrate another aspect of the close connection of the verb with the sentential context in which it stands. This feature I have called "embeddedness in sentence." Let me first give you an example.

The child will, for instance, signify in series VIII, sentence 1, "Boys sometimes hit back at their parents." In sentence 2, "Mary does not know that Jane used to lie." In sentence 3,"Mother said, 'Jimmy, you should never holler at your mother.' " In other words, the child signifies in various sentences by using different words which have some common denominator. Now sometimes you will find that the child will express this common denominator in very general terms. If you press a child for what "prignatus" in this particular instance really means, the child says, "Not to be nice."

Kubie: Does this tendency to throw up a different explanation for each sentence arise more in certain series than in others?

Werner: On the whole, no. Of course the example just given is but one of many. There were quite a few children who responded similarly to this series.

Kubie: Don't you think that the fact that the word "prignatus" is pregnant with meaning for every child between the ages of eight to fifteen has something to do with it? You can't possibly get a child to react with a stimulus of that kind without mobilizing every process in him.

Werner: That might be so, but I don't feel qualified to elaborate on this question.

Mead: "Prignatus" is a wonderful word.

Kubie: Look at that context: "Boys sometimes prignatus their parents; Mary did not know that Jane used to prignatus; you should never prignatus your own mother."

Stroud: I wonder how that series happened to be constructed.

Wiener: There seems to be a good deal of constructive Rorschach in this.

Werner: There is undoubtedly a good deal of projection. We did not study the projective aspect in particular because we were more interested in the formal aspects. I should like to add that other words, such as "poskon" in series XI and "sackoy" in series VII, yielded a great many instances of this word embeddedness that I am trying to discuss.

I shall characterize the essential feature of this embeddedness. In trying to find a concept which would fit into the various sentences, the child is hindered because for each individual sentence the word meaning that he has offered is specialized to such a degree that it cannot be placed in another sentence. Though all these individual meanings have a common denominator, this common denominator cannot be placed as such into the individual sentences. This feature thus possesses two characteristics: 1. it is a concept which in a vague way is common to the specific word meanings used for the various test sentences, and 2. the individual word meanings are so specifically fitted into each of the sentences, and the final concept is so general, that the latter is incapable of replacing the single solutions. Therefore the common concept is, as I call it, "pluralized" in order to fit into the various sentences. We can speak here of "pluralization" as a protoprocess of true generalization.

I shall go on to talk of another aspect of the test, that is, the way the sentences are apprehended. We find here a lack of semantic articulation. As McCarthy (5) has shown, in early childhood sentences may be semantically, though not grammatically, complete. There is a tremendous development between one and a half years to nine years in the grammatical articulation and completeness of sentences, that is, from 80 per cent incomplete to 33 per cent in standardized situations.

McCulloch: Perhaps I have misunderstood. Do you mean that the child uses a grammatically incomplete sentence?

Werner: That is correct.

McCulloch: You do not mean that what he says is not for him a whole?

Werner: On the contrary. The study by McCarthy shows that children will use sentences in a structurally incomplete way, though as far as the semantic aspect of it is concerned it is complete. The child actually makes a semantically complete statement.

Pitts: What proportion of the statements made by adult human beings are grammatical sentences?

Stroud: I dare say if my own transcript was a sample, I must be very childish. I don't think I hit the ninth-year level as a rule.

Werner: You are modest.

Stroud: You did not see the transcript. It was really terrible.

Mead: It seems to me that the statement, "Children don't complete sentences," is a statement about written language, and not about adults speaking. Any stenotype record of almost anyone's speech, except those few individuals who are accustomed to mak-

ing formal presentations, perfectly rounded propositions, shows a very large proportion of unfinished sentences. Isn't much of this work that has been done on children's languages the comparing of children's spoken language with adult written language?

Wiener: That is true.

Stroud: It is in most of the cases. I had the occasion to dig up—

Werner: Unfortunately, as I said, we have no study on adults. I would very much like to have studies made on adults under comparable conditions.

Mead: Something of this sort was done by Theodora Abel, who recorded conversations of adults, comparing them with the results of Piaget's tests on children. The difference between children and adults virtually disappeared; she found incomplete sentences, imperfect sentences, conversation without reference to what the other person said. A large amount of the statements about children that have been made by Piaget were found to be just as applicable to adults.

Werner: I think Miss Abel used not highly educated adults.

Mead: Educated high school students working with paper work.

McCulloch: May I say that in the stenotype notes of our conferences Von Neumann almost never finishes a sentence and Wiener almost always does. I don't think you can hang much on the difference of the educational level.

Bateson: Except that Von Neumann is speaking a foreign language.

Wiener: Do you know if there was any substantial difference in the completeness of the sentences now and at the age of nine?

Werner: It would be excellent to study the speech habits of individuals during growth. There is a tendency among child psychologists to include more and more longitudinal studies that will give us a much more adequate picture of child growth. Only a few places have started such an ambitious enterprise; California, for instance, has.

It is a question of expense and time. Cross-sectional studies that we use today are, of course, crude in relation to what one might obtain from longitudinal records. Controversies, such as those Dr. Mead just mentioned between Abel and Piaget, will probably be ironed out when results from longitudinal studies become available. Everybody will agree that it would be absurd to assume mental development if there were no differences between children and adults.

Wiener: I should like also to call attention to the fact that the

finishing of sentences is not an easily definable thing. When a man with the New England habit of talk says, "Looks like rain today," is that a sentence or not? There is no "it." He would not say "it." "Think it is going to rain," things of that sort.

Mead: They are sentences.

Pitts: It is a sentence, certainly.

Werner: I should like to say that there is a definite method of approaching differences between adults and children. We will never find differences if we select atypical adult responses for comparison with the typical responses of children. In our language there exist many expressions which have the character of holophrasis or even of one-word sentences. But these don't occur typically in those standard situations such as were used in our comparative study between children and adults. An expression such as, "Oh, my God," said by adults in an emotional situation should not be used for comparison with the productions of children under circumstances of a relatively nonemotional nature.

Wiener: I also want to say—and I think this is along your lines, Dr. Mead—that the questions of what is a word, what is a sentence, differ by language. What is an isolated phrase?

Mead: The idea of sentence is a construction of our own particular linguistic form.

Wiener: Culture to word, too.

Mead: I shall give you an example that I think will show it up quite sharply even in our own society. One of the things that Americans who have studied in England say characteristically is, "I wish I could go back to Oxford and/or Cambridge and finish my sentences." I heard that a great many times, and I thought I would find out what it meant. I then listened to conversations among Cambridge men, and it seemed as if they never by any chance finished a sentence. A good conversation between two or three people who have gone to Cambridge together consists of one person saying three words, the next putting in another bit, the next putting in another bit, with nobody finishing his own sentence. A characteristic, on the other hand, of an American form of conversation, even among people who are accustomed to working together, is that of not listening to what the other person says except to find an opening. You wait for an acceptable end to what they are saying and then shoot your sentence in. The truth of the matter is that what those Americans meant was, "I wish I could go back to Cambridge, where people listened to what I said and where what they said felt like the end of my sentence."

Now under those two circumstances what is a sentence? A

sentence is an extraordinarily formal unit which we have devised in our handling of language. It is related, of course, to written language, to punctuation, and I don't think the question: What is a sentence? is at all meaningful except within a specific cultural context; it is not even meaningful between Englishmen and Americans, you might say, except in terms of written language. It is a formal but not very useful category.

Pitts: You might say a sentence is something that is capable of being true or false.

Mead: That is a predictive statement. A sentence is a device or form for writing language and punctuation.

I should like to go back, in just that connection, to the point you made a little earlier about the use of the word "collect." It seems to me that this is perfectly fair if what we are studying is the way a child learns English, because it is of the nature of English to have words both transitive and intransitive that have four or five meanings. Therefore, these combinations with the exception of two—may I please ask you what "hudray" is?

Savage: Increase.

Mead: You cannot use increase that way.

Kubie: Enlarge.

Mead: This is not a single word.

Savage: Not a single word?

Stroud: Increase, one grows.

Werner: It doesn't make any difference as long as the child uses words which are in the appropriate meaning sphere.

Mead: It does not need to be a single word?

Werner: We are only interested in determining if it is in the appropriate sphere; "increase," "enlarge," "get bigger," and so forth, would be all right.

Mead: You don't mind if it is not a single word?

Werner: I don't mind whether the child presents a solution in a word or in a phrase so long as we determine whether the meaning sphere is correctly reached or not. However, the question of a single word representing a sentence rather than a circumscribed meaning is something else again.

Mead: So you see, I think we can ask the question: What do children do in our society where we have an abstraction called a sentence and where we teach abstractions called sentences, where we teach punctuation in relation to sentences? We say: that is not a complete sentence. We send things back from the editorial offices because every sentence must have a verb unless it is an exclamatory sentence. Then it is a relevant inquiry to find out

at what age children complete sentences or at what high level people give up completing sentences. I think that is highly cultural. In Chinese the problem would probably be entirely different.

Werner: This brings up the extremely interesting problem of speech development in cultures where the language is of an entirely different type than ours. I wish some comparative studies would be made which would make it possible to generalize findings concerning speech development of the child.

Wiener: I also want to say something about the word "word." In Chinese you have an entirely different meaning. Take, for example, the word *shia,* which may mean "down," which may mean "beneath," which may mean "to lower," and so on. In Latin *faratultatuum* is one word, because it has been a local tradition in the Latin language to use pieces of different words for one word.

Kubie: We must also bear in mind the fact that in childhood there is a *pre*verbal language, and that the child acts out a great deal of his speech, only gradually substituting words in a sentence for activity. He does this only as he begins to inhibit activity, which then must be put into words. If the child takes a piece of bread and walks toward you with it, saying "Bread," he is saying to you, "I want to give you this piece of bread"; but he only *said* one word out of that sentence. There is a sentence-function in what he is doing, if you take in the totality of his behavior and language.

Pitts: Everything a man does is a language, anything he does is. That robs the concept of any usefulness.

Kubie: As we develop, we substitute for behavior more and more complex and subtle indices and codes, which become the fully adult elaborated language.

Wiener: Many things we would call words, I think, in Chinese, according to our way of thought, will be combinations of characters in Chinese.

Pitts: You can certainly say it is necessary for the concept of language. It should be a substitute for something else. I am not sure exactly what else.

McCulloch: Please be brief, because I should like to get back to the main presentation.

Teuber: I want to get back to what Dr. Werner said; I was bothered when we began to discuss the question whether "sentences" or "words" could have the same meaning in different linguistic contexts. If I understood Dr. Werner correctly, he

wanted to say that early sentences in the individual development of language are incomplete rather than unfinished. In the language of the psychopathologist, you are talking of some sort of agrammatism, or paragrammatism. I thought that was the thing you wanted to convey.

Werner: It is exactly this problem of lexicalization and grammatization as a developmental process that I had in mind.

Teuber: So it is not a question of an unfinished sentence left dangling in mid-air, a sentence that can be completed by anyone who listens, but rather a question of having parts of a sentence differently put together, and some parts left out.

Werner: The problem of incompleteness has some bearing on Mr. Kubie's question concerning the relation between gesture and language. He correctly points to the developmental feature which I would call increase in verbalization, that is, the fact that communication is increasingly based on verbal expression rather than on gesture, action signals, and so forth.

I'd like to go on now with my general presentation and say something about grammatization, which ties up with what we've been talking about. We find that the younger subjects in our test do not take the sentence as a closed and stable semantic unit. They may combine parts of one sentence meaning with another sentence meaning, a linguistic feature which I call sentence contamination. For instance, in the case of the word "hudray," one child did respond to the first sentence with "healthy": "If you eat well and sleep well, you'll be healthy." For the second sentence the child stated, "Mrs. Smith wanted her family to be healthy." In the fourth sentence the child stated, "You read about health and you know by reading and studying about health." Coming back to the second sentence, the child made this statement, "Mrs. Smith wanted her family to be healthy and to read books that teach you to be healthy. The book says, 'Brush your teeth three times a day,' and if you brush them only once you won't be healthy." The child finally comes to the conclusion that all these sentences deal with "health books." This illustrates well the semantic instability of sentences for the child. This makes possible the meaning assimilation of various sentences.

One of our very remarkable findings is the rather sudden drop of a great many more immature features of language behavior between the years ten and a half and eleven and a half, such as word-sentence fusion, sentence contamination, and the like. That is, after eleven and a half, very few of these grossly immature language characteristics were found.

Mead: Is it the same for boys and girls?

Werner: The same for boys and girls. The sudden drop holds only for the most immature features; there are other immature features where the drop is more gradual. In other words, mature lexicalization and grammatization of speech behavior on the whole show sudden emergence.

Bateson: Is it at that age that the child learns to separate the notion of lexiconization from other notions?

Werner: There are practically no incidents after that age level.

Mead: Do they all go to the same school or are they distributed?

Werner: Most of our subjects came from Brooklyn. We also studied children in Ann Arbor public schools.

Though we had some older children who still showed grossly immature characteristics indicating lack of lexicalization, their number was very small, and those children who did show these characteristics did so very infrequently. At the thirteen-year to fourteen-year age level, only 2 per cent of the responses showed these signs of immaturity compared with 33 per cent at the level of eight and a half to nine and a half years. At this higher level there was practically no lack of differentiation between word and sentence; there were very few of those word gradients that I mentioned; there was little of the instability of sentence meaning and sentence structure which leads to contamination of a part of one sentence with a part of another, and so forth. In other words, at ten and a half to eleven and a half years, something crucial in mental development must occur which is reflected in the sudden change in language behavior. I'm inclined to believe that this change is due basically to the understanding of the abstract nature of the test sentences or, generally speaking, to the acquisition of an abstract symbolic attitude. In order to arrive at correct solutions with a minimum of immature processes, one has to assume a hypothetical attitude removed from the concrete world of action. In speech behavior involved in everday situations, such an attitude may not be necessary. The child may be able to get along without his lack of this attitude being brought out. But this test, since it demands the abstract symbolic attitude, demonstrates by what it evokes from the child, the lack of this attitude. As is well known, the lack of abstract symbolic behavior is a feature occuring in certain psychopathological states, and I feel that a test like the present one would be valuable in testing immaturity or symbolic behavior in aphasiacs, mentally retarded cases, schizophrenics, and the like.

Pitts: It sounds very much as if the child had learned what a mental test was in between.

Werner: In order to solve the problem you have to know—

Pitts: What the riddle is.

Werner: Yes, you have to know that test sentences are removed from everyday speech.

Mead: Yes.

Werner: As I mentioned before, they are to be conceived as hypothetical statements. I should like to give you an example showing what a child might say. In series II, sentences 2 and 6 were considered by a child. Sentence 2: "Mrs. Smith wanted to feed (hudray) her family." Sentence 6: "You must have enough space to fix (hudray) your library." Coming back to sentence 2, the experimenter asked, "How does 'fix' fit here?" The child said: "No, it doesn't fit. Mrs. Smith wanted to feed her family. She couldn't fix her library at the same time while she is feeding her family." We see here that the child viewed these sentences as concrete parts of a realistic story. With such an attitude the child cannot solve a problem of this sort. You are, of course, right, Mr. Pitts; the child does not understand the nature of the test. However, it is the job of the psychologist to analyze further and find out the reasons causing this lack of understanding. Doing so, I believe we arrive at the conclusion that this lack is due to a concrete attitude toward language rather than a hypothetical one.

Wiener: Take a German child, or a child in any country where a good deal of teaching of foreign language is instilled, where familiarity with the use of grammar, and with test sentences for grammar, begins, say, at six. I cannot imagine the child not having an idea of what is meant by this.

Mead: I don't think it interferes if we keep it clear that these are American children in an American school system who are learning what the system will do to them, becoming test-wise. What we are studying here is at what age American children faced with this situation suddenly begin to adapt to it, and it seems to me that we may make a major generalization. What the children really say to themselves is, "This is something like a test in which a meaningless word has been inserted in a series of sentences, and I have got to find out what it means." At that point they stop doing all these other things which Professor Werner has described. They now correctly define the situation. If you took the same child with a high I.Q. at six and gave it the same test and said: "Somebody has made up a word. They have made up a word and have put it in many different sentences in such a way that you

have to look at each sentence, but do not guess what the word is until you have looked at them all," the child could then do it earlier.

Werner: Again I should say that without any doubt the mental age level or I.Q. has its bearing upon solving such a test adequately. But the developmental psychologist, as long as he does not act as a psychometrician, is not satisfied with stating such a relationship. Without going further into that problem, I should like to point out that mental age is only superficially defined by the overt achievement. Basically we have to define it by the underlying patterns of functions, attitudes, and so forth. We also have to deal with individual differences, influences of home enviroment, and again these influences are translated into the previously mentioned patterns of function; it is then the job of the developmental psychologist to determine the nature of these functions by adequate methods.

Pitts: I think that by eight and one-half the child has almost learned language completely. The language habits are practically the same as those of the adult—I mean the really important things he has always learned about the structure of language.

Werner: This may be true in a limited sense. As far as language as a tool for everyday communication is concerned, you are possibly correct to assume that there is little difference between the child and the adult; at least there is no evidence one way or the other. However, what we do learn from a test like this is that with respect to situations demanding abstract symbolic behavior, there is a fundamental difference between children at eight and a half and children at thirteen and a half.

Savage: We have to find out what "soldeve" is.

Werner: That is one of the meanings which is not a conventional English word.

I should like now to come back again to the problem with which I started; namely, the developmental nature of the word and its relation to forming concepts and logical relations between concepts. In other words, the problem here is the relationship existing between child language and child logics. As you recall, I started with Arieti's supposition that logics can be determined independent of language. Such an assumption is unwarranted. I should like to show you that if words are conceived holophrastically, the relationship between concepts, the logical inferences, and so on, will be basically influenced by it. I should like to illustrate this briefly by an example which reminds us of Arieti's examples of the paleologic inference of schizophrenics. In series III, sentence

3, a child had developed the concept "plaster" for "contavish," stating, "Before the house is finished the walls must have plaster" (contavishes). According to the child "plaster" also fits in sentence 5 ("A bottle has only one contavish"), in spite of his reponse, "A bottle has only one label."

Bateson: Good enough.

Werner: That fits very well, as the child explains it, because "plaster" is used to put on the "label." This is exactly what Arieti and Domarus have called predictive identification. However, if we examine more closely the reasons for this identification we find it is based on holophrastic word usage rather than on identification of circumscribed terms. The meaning of "contavish" is actually a global situation, "plaster + label," and in a particular sentence one or the other may be more emphasized but never really isolated.

McCulloch: I should like, in the next moment or two, to get a little more of a picture of how children learn language; I have asked Mr. Stroud if he would aid us. Then I shall ask Dr. Kubie to go on with the next topic.

THE DEVELOPMENT OF LANGUAGE IN EARLY CHILDHOOD

JOHN STROUD
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IN ORDER to give my talk as rapidly as possible, I show you at first a chart (Figure 23) with some data on it. This is a calendar for the approximate ages at which development in language takes place.

I had to develop an hypothesis on which to hang the descriptive facts. I was plain foolhardy. The notion begins very simply. If the child expresses anything symbolically, it must be its own experience. The development of the symbol action is language in its broadest sense; it must therefore reflect the developing organization of his own experience. In one point of view, what you see developing in the language of the child is perhaps like the developing leaves on the end of a tree. It is impossible to set up a rigorous scale to show how this goes on in the baby until the point about which Dr. Werner was speaking is reached. The reason for this is that what you are looking at is the outside of a considerable underlying development. The general notion was that it seemed reasonable to infer (I won't say from what, for time does not permit) that the newborn infant is roughly capable of distinguishing between *things* and *not things*, and has *feelings about*—

Werner: What are "not things?"

Stroud: Not things and things arise very simply. In the central nervous system the addresses of sensory endings are not entirely lost. Thus if a pattern is impressed upon some region, the bounds of the pattern roughly remain as bounds in their representation centrally. So that a *not thing* is that which is on the other side of the gradient. Not things for a child do not have any very elaborate or highly differentiated meaning. They are the raw given of his own experience, such as one might infer existed when he was born.

Werner: You mean what some would call background experience?

Stroud: What you call background experience might very likely

be included in the not thing, but I would certainly hesitate to say that the not thing of the child is at any such sophisticated level as that.

Fremont-Smith: Would you give an example for an adult of a not thing?

McCulloch: Grope your way in the harbor in a fog and you find things.

Stroud: If you wander in a fog at night, there are vague things and vague not things, and you are not even sure of your ability to define. It regresses, shall we say, to your childhood levels.

Kubie: The experience of darkness.

Mead: Aloneness.

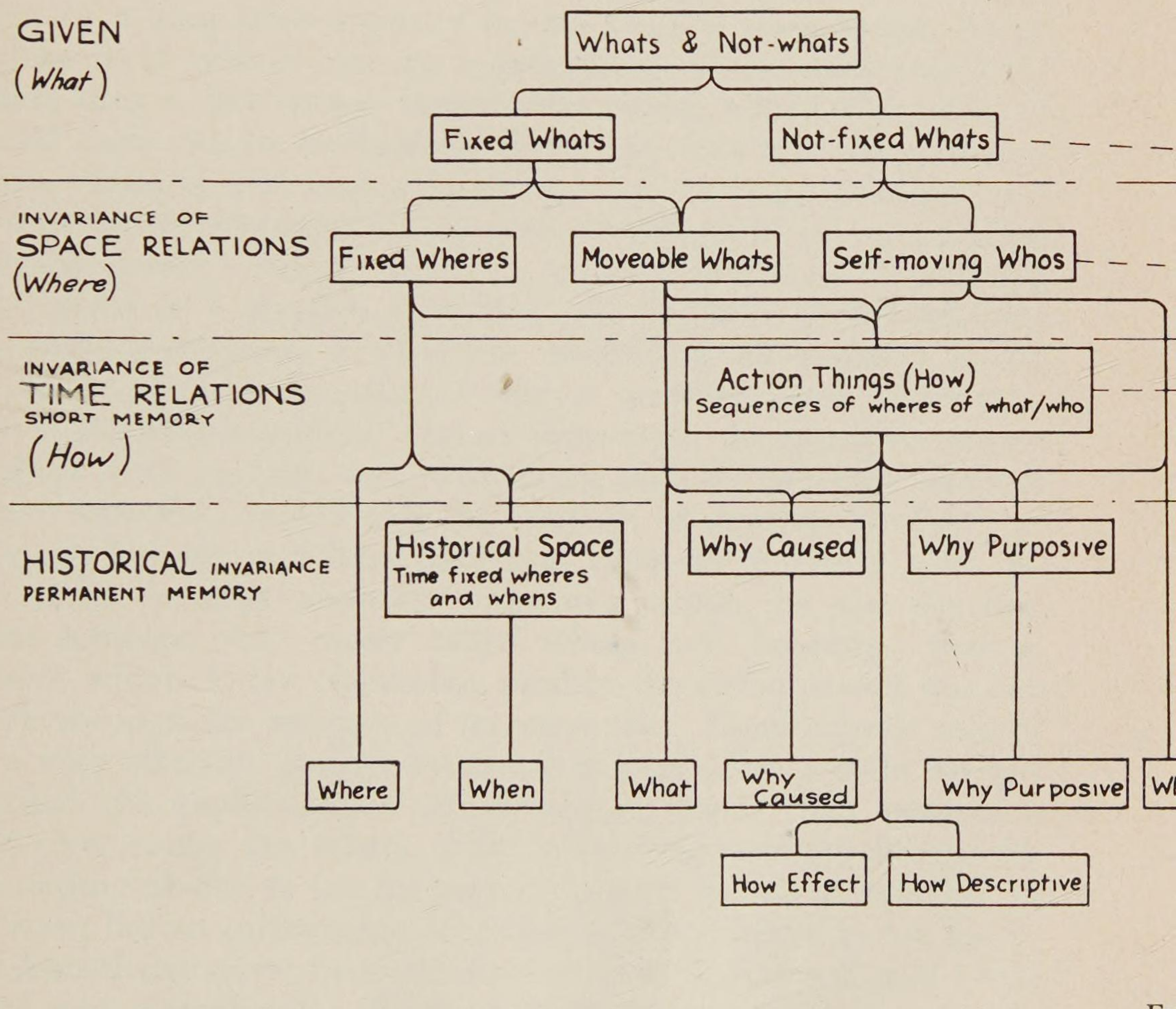
Stroud: Aloneness; the experience of something which might almost be a light in a very dark night, a light area in a dark area.

Fremont-Smith: One over a thing.

Stroud: Roughly one over a thing. It is rather difficult to believe, that is, I find it difficult to infer, that the newborn has any space. All that he has are things and not things; and looking on, we would say they change, but I doubt that they even change for a child right away—I mean, for a newborn infant. The only thing that you are reasonably sure about is that it can vocalize a simple dichotomy. It yells when it hurts vaguely and sleeps when it does not. There is vocal behavior at birth, and this particular vocal behavior you can follow, if you will note, on the chart. That is one behavior I did follow out well.

The first major transformation which gives any kind of invariant in this experience of the child occurs when it learns how to transform its experience so that it achieves the invariances of space; its former things and not things can now become fixed things and not fixed things, because after a child has learned the trick of transforming his central nervous system input to give invariance in space, there are still moving objects which will not assume fixed position in his recently and primitively created space of perception. This can again develop in such a way that he learns to move his body and to manipulate; the very fixed things become very primitive wheres; the manipulatable things become movable things. There is still a broad class of objects which do not remain fixed but are not manipulated; they are self-moving whos in the most primitive sense. Having managed this first-order transformation giving him his first set of invariants, the invariant relationships in space among unmoved things and fixed wheres, he has the possibility, with his developing short-term memory, to hold the sequences of wheres, of a movable who or what, and he develops

INFERRED DEVELOPING EXPERIENCE OF A CHILD



Fig

"action things," like go, run, hit. From such notions of a fixed sequence of wheres, which can be classified or described, from the notion of such a fixed sequence of words of a what, the most primitive notion of time develops. I would like to point out that whether you look at the wheres of the what clock hands or whether you concern yourself with the wheres of the maxima electric vector, in some very fancy oscillatory circuit, which will keep times in seven decimal places, it is still the means by which we define time.

At this stage the infant has action things. With the development of a long-term memory he can congeal these action things and be fixed wheres and the sequences of wheres associated with whats into a four-space framework within which the adult of stored facts can be established, that every fact was what, where, when. This, as I see it, is a very crude but practical primitive notion of the development of the child's handling of his own central nervous system. I don't say he is responsible for it. It is just a description of a process. It would take Solomon to tell whether his primitive action is effective, modifying his position in his world, or operates through the central nervous system of his parents and is thus symbolic. Out of these many actions some remain perhaps mere practice acts. Others are directly effective, and still others are effective through the medium of another life form or through himself at a later date, and these are symbolic acts. As the child develops adequate expressive jargon, he also develops what someone very nicely called whole body language, that is, actions which do not themselves modify the circumstance but can work through the medium of someone else. These become refined to a very effective gesture system at a time when a child has not reached the sophistication of developing more than perhaps a very few names for whats. His verbal language lags behind his developing ability to use his nervous system by many months. In between lies an intervening language which I regret to say has to my knowledge never been seriously studied. It had not as of 1946, after very considerable efforts of McCarthy to write her chapter in Carmichael. I have heard of none since.

As the child continues with his jargon, babble, inflections, gestures, and a few word names, these are soon reinforced by a few words for action things. It is dangerous to generalize from the data of one culture to another. I tried to abstract data that would be as general as possible. These words, as apart from the system of feeling expression, are added to, slowly, and never entirely supplant the original gesture language of the child.

The approximate developmental levels from mere things and not things and feelings to quite nice sorts of differentiations, as I tried to show, are by no means complete, but in spite of the relatively enormous number of factors which can influence the behavior of the child at any of the intervening stages, statistically this framework is still very largely held. The last major element of logical thinking, as we like to think about it, the manageability of tense, seems to appear at about three years. From this point on, in all the functional fundamentals, if not in all of the abstractions, a child has already acquired his first and fundamental verbal language, incomplete, inaccurate, and badly organized though it may be.

McCulloch: Let me say one more thing. If you read the older attempts to study the speech of children, you will see that we tend to attribute to a child's "why" our meaning of "why," to a child's "no" the adult meaning of "no," and so forth. What happens apparently is this—and I give the word "no" as an example—the word "no" has to be developed apparently rather early. A child can merely accept that which it wants, but it has to have some general form of negation. A general form of negation may at first simply take the form of shoving away; afterward it becomes "no." That goes on to develop later into both the "no" of logic and the "false" of logic. A child cries, "Mama" which has potency over mama so that mama comes; if a child says the word "Mama" and mama does not come, the child will say "No mama," which is the equivalent of the word "mama" being false. This is linguistically and semantically a higher level of problem than the "no" of not that object. It is already referring to the futility of the language. This kind of thing is altogether in the child in a state in which it does not submit to the interpretation which we later put on the word. The word "why" is the perfect example, because even in the adult the "why" is so confused again and again that you don't know what the adult means by it most of the time.

McCulloch: Dr. Kubie, will you speak next?

THE RELATIONSHIP OF SYMBOLIC FUNCTION IN LANGUAGE FORMATION AND IN NEUROSIS

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WHAT I have to say troubles me, partly because it renders what is already complicated even more complicated. Yet it must be in the picture as part of the totality that we are discussing and have been discussing for these last two days.

I want first to backtrack, so as to add to what has just been said. In 1934 I published a short paper, which this meeting led me to resurrect (1). It was called "Body Symbolization in the Development of Language" [*Psychoanalyt. Quart.*, *3*, *430* (1934)], and with your permission I will submit it to our chairman to be reproduced as part of our proceedings, since much of what I want to say is said there more concisely than I could possibly say it if I were to extemporize. The paper begins by pointing to Pavlov's evidence that new connections (conditionings) can be formed in the central nervous system only when the animal is not in a satiated state, and that consequently all processes of learning depend in some measure upon the existence of a state of craving. Then I pointed out that in various ways (which I will not go into here) analysis proves essentially the same point, that is, that it is only in the state of craving (of deprivation) that one establishes new connections and breaks up old ones. Next I point out that cravings arise in infancy and childhood in body tensions. Consequently it is inevitable that the child's first conceptual structure should begin around his own body, and that his first concepts must deal with the products and the apertures of his body, the needs and the feelings of his body.

Therefore to understand the growth of language we must begin by observing closely what the child wants, what parts of the body become involved in the process of wanting, and ultimately how he learns to represent in symbols these needs and the different parts of the body which are involved and associated with these needs. I emphasize this because my only modification of your

scheme is that it begins *too late*. It begins with the recognition of the phenomenology of the outside world. That cannot come until the infant has already realized a good deal of the internal experiences with which his life experience actually begins.

Stroud: I did not wish to indicate that at the thing-nothing level there was any inside or outside. You cannot get inside or outside until you get to the space transform. It is a meaningless thing.

Kubie: Let me quote from the paper I have referred to:

> Since the child's world begins inevitably with his own body and since the force which instigates the child to expand his knowledge is always the pressure of internal needs and tensions, since every new fact of experience which enters into our psychic life can make its entrance only by relating itself to that which is already present, then it follows that every new fact which is apperceived by the infant and child must somehow relate itself to bodily things. Schematically you can represent this by a series of steps. If this represents with any fidelity the process of expanding knowledge, it must also represent the process of expanding speech, expanding communicating measures, the meanings. There is at first a long period in which concepts are vague, broad, overlapping. With advancing years these concepts become discrete and distinct one from another. At some point during the early period they are represented by indefinite but meaningful gestures and movements and expressions. As time goes on they are represented by that symbolic code which we know as language.

That is all that I am going to quote. In the paper itself I give many illustrations of something which we all recognize in verse and story, and which we recognize in all the games we play with infants and children in which the parts of the body are named for external objects, animals, and so forth.

Now let me come back to what I had originally planned to say and which is going to make things seem to you even more complicated than they do already. We have to keep in mind the fact that the human organism has two symbolic functions and not one. One is language. The other is the neurosis.

To clarify my meaning, let me quote a few pages from an article now in press for the *United States Armed Forces Medcal Journal* and entitled "The Neurotic Potential, the Neurotic Process, and the Neurotic State":

> I want to make it clear that when we say that there are psychological processes of which we are not aware, these may mean either of two things. There are two kinds of unconscious processes, one of which is readily accessible to conscious self-inspection, the other not. Even the simplest activities of life, such as breathing, sucking, excreting, moving and crying, must be learned through repetition. All of these activities were originally random and explosive. Through repetition, however, they become economically organized toward goals. As they become fully learned, the act can be initiated simply by the contemplation of

the goal and we gradually become unaware of the intermediate steps which make up the act. This is the great economy that is achieved in learning by repetition. It is in this way that we learn to be able to walk without pondering each step, to talk without working out the movements by which we enunciate each word. It is in this way that the violinist and the juggler and the athlete learn complex chains of synergic movements. It is in this way that our thinking processes acquire Seven League Boots: i.e., the ability to leap over many intervening steps as we perform complex arithmetical processes, and the art of intuitive thinking. In each case the intermediate steps drop into the background and disappear from consciousness. Yet they remain accessible to conscious self-examination. They are in what William James would have called the "fringe of consciousness," or what Freud called the *pre*conscious (PCS) or the descriptive *sub*conscious, as contrasted with the *dynamic unconscious* (UCS).

The dynamic unconscious, then, is not merely a descriptive concept: it is an area of force or rather of whole constellations of forces in psychic life. Unconscious processes are constantly at work in our lives; but we cannot become aware of them by ordinary self-observation because they are hidden from us by active opposing forces within ourselves. Throughout life these UCS processes exercise a powerful influence on human behavior; and it is out of their influence that everything that is neurotic in human affairs has its origin. In this sense everything that we say and do and think and feel serves multiple functions and becomes a symbolic representation of both Conscious and Unconscious levels of psychological organization. From this we may go further and conclude that if the psychological conflicts of infancy and childhood could take place in the full light of consciousness, then the neurotic process would never be launched in human life.

Why we render psychological processes unconscious in this way is a question with which I will not try to deal on this occasion, except to say that it happens whenever we are unable to discharge internal tension because of opposing feelings of guilt and fear. This is when we render our conflict-borne tensions unconscious and then express them in the masking symptoms of the *Neurosis*.

THE SYMBOLIC PROCESS IN THE NEUROTIC POTENTIAL

The symbolic process which I have in mind includes the formal symbolism of dreams as a minor example, but is of far broader significance. The human being is capable of two related but differing types of symbolic processes. One gives him the ability to make abstract concepts of his experience, to represent those abstractions in symbols and thus to express and communicate his purposes, needs, thoughts, and feelings through behavior, gestures, sounds, words, and their written symbols. The other symbolic process is the one by which man expresses in disguised forms those psychological tensions which he is unable either to discharge or to face. The first is the symbolic process of self-expression through language; the second is the symbolic process of self-deception. In the developing infant and child these two symbolic processes have a common origin; and the ability to represent internal experiences in symbolic activity is the *sine qua non* equally of the neurotic process and of speech. Whether either symbolic process is possible to a significant degree among animals lower than man is far from clear: which is why it is not certain that the so called "experimental neurosis" in animals (actually an emotional disturbance which also occurs in human neuroses), is identical with the neurosis itself.

Between the two forms of symbolic representation there is a difference about

which we can be quite specific. The difference between the representational process in communication and the representational process in the neurosis is primarily the difference between using a symbol for an internal experience of which we are or can be aware, and using a symbol to express an internal experience of which we are unable to become conscious. The capacity to create and use symbols (or "transforms," as the engineers would say) is identical in both and is essential to both. The difference resides solely in the fact that the relationship of the symbol to the underlying psychological process is conscious in language, and unconscious in the neurosis.

Consequently, the roles of the symbolic representative of these two types of "unconscious" experience differ. In speech the symbol is like the salesman who represents a firm that is doing a legitimate business. Its salesmen save the heads of the firm much time and energy, because they do not have to visit every customer themselves; yet these principals are known to the customers and can always be reached by them. On the other hand, there is another kind of "salesman," i.e., the representative of a gang of criminals, or the secret agent of a foreign country. Even if he is captured, and even if it is known that he is the agent of criminals, he will not divulge their identity or whereabouts except under pressure, if at all. In the neurosis the relationship of the symbol to the inaccessible unconscious processes which it represents is of this nature. We must repeat, therefore, that if human beings were not able in the first place to abstract their psychological processes and in the second place to represent these abstractions symbolically, and if in the third place they were not able to render certain unacceptable psychological processes inaccessible to conscious introspection, there could be no such thing as a neurosis. Together, then, these three human capacities constitute the Neurotic Potential.

Thus human vulnerability to the Neurosis (i.e., the Neurotic Potential) arises out of our capacity for symbolic psychological function, without which there could be neither a neurosis nor a thinking process, but merely dreamlike sensory imagery, passive echoes of previous perceptions. Like the neurosis, planful action and speech require symbolic processes by means of which sensory imagery is taken apart and reassembled in new combinations.

Out of this matrix the *Neurotic Process* emerges gradually and progressively. It begins the first time that some complex psychological experience is hidden in such a way that all that is accessible to conscious introspection and all that shows to the world is the symbolic behavior which represents it. This representative or symbol or transform may be simple at first; but with the passage of time the same symbol can come to represent many hidden psychological states; and in turn there can be representatives of representatives, symbols of symbols of symbols; so that the complexity of such a linked chain of symbolic representatives can become very great indeed.

In speech, then, we use a symbol for something of which we are conscious. In the neurosis we use a symbol for something that we are unaware of; and the distinction between language and the neurosis is the difference between being conscious or unconscious of that which we are representing symbolically. The capacity to represent inner experience symbolically is necessary to both. But at some point a dichotomy occurs between the symbolic representation of that of which we are aware and the symbolic representa-

tion of those things of which we are unaware. There are many unanswered questions about this: for example, must everything that we represent symbolically at one time have been conscious? As far as I know, this is one of those issues about which people have biases but little evidence. It is certainly true, however, that everything that a child does, including his preverbal activities, can be used in either way; whether this is rocking, crawling, walking, running, eating, excreting, grunting, spitting, or throwing. All of these can be used in infancy and childhood, both as a direct expression of a conscious purpose or as an indirect expression of things of which the child is unaware.

How do we recognize the difference? Again in a simple and pragmatic fashion: by the automatic repetitiveness of the behavior when it is serving a purpose of which the child is unaware.

If we think of our own experiences with infants and children, we see this in the repetitive play of the child, and in his handling of his instinctual functions as well. Thus eating serves not merely the instinctual or biological need of the organism but also a vast superstructure of unconscious needs. This occurs both in the area which we call normal and in the pathological exaggerations such as compulsive and phobic eating; indeed, it occurs in all of the distortions of instinctual function that arise in childhood.

Fremont-Smith: Would you not agree that at a very early stage a given symbol may be used simultaneously for both conscious and unconscious?

Kubie: Not "may be" but *always* is. There is nothing that we do that does not serve both conscious and unconscious masters.

Pitts: Is not your conception of unconscious like the vermiform appendix, in that it performs no function and becomes diseased with extreme ease? It is there for no discernible purpose.

Kubie: I would not compare it to a vermiform appendix at all. Unconscious processes perform many functions, some of them constructive and useful, some of them destructive. Sometimes they are constructive, yet at the same time cost us a high price.

Pitts: That is the point. What are the constructive ones that are performed all the time? Roughly, what is the function?

Kubie: I am not a teleologist. I do not begin with that question. I begin with the question whether it exists and how it operates. That it exists is experimentally demonstrable. I will give you a demonstration in a moment. In fact, I will show you that it has very powerful influence in human life. That is also demonstrable. Let me give you two very simple examples. Both, so help me, are true stories.

One summer night I arrived in the Grand Central Station from the country and got in a taxicab and asked the driver to drive me home. My home is on Eighty-first Street between Fifth and Madison avenues. For those of you who are not entirely familiar with New York I will make a diagram. I am going up Madison Avenue this way. Here is Eighty-first Street. Eighty-first Street is a street in which traffic is bound this way (*indicating*), but down here, in the sixties, the driver starts to go over here (*indicating*), which would bring him into Eighty-first Street against the traffic. As he started to swing in this way, I said: "Hey, where are you going. I said Eighty-first Street. You will enter it against the traffic."

He said: "Right. I heard you say Eighty-first Street but here I have been thinking Eighty-second all the time. Now why do you think I was doing that?"

I said: "I don't know. Maybe you don't like the odd numbers."

He stopped the cab in the middle of the street, turned around with his eyes bulging out of his head, and said, "Jeezuz, how did you know? I have been betting on them all afternoon and lost my shirt." Then he proceeded and told me that he had been betting on the odd positions at the post in a bookie shop all afternoon, and that he had lost steadily; until now he could not go into Eighty-first Street and had had to go to Eighty-second instead.

This is interesting for many reasons. In the first place, there was no barrier, no resistance to insight. I only had to say that perhaps he did not like the odd numbers; he filled in the rest.

My second story concerns the airplane dream described in the paper to which I have alluded and which is appended to these remarks. Therefore I need not repeat it here.

Pitts: The whole style—the thing still does have, it seems to me, the peculiar flavor of the vermiform special organs which exist to be infected and have no function.

Fremont-Smith: Dr. Kubie did not explain what function it was performing. It performs a real and very important function.

Kubie: It was performing an important immediate function in the behavior of the taxi driver. He was ashamed to go home because his wife would ride him like the devil for playing the ponies. A whole human drama was behind this episode, yet not deeply buried. The other example led back to the girl's most traumatic experiences, and to the deepest problems of her life. She did not want to face these at all. She had had to bury the experiences, yet at the same time they had to be represented indirectly in some form that was going to get by.

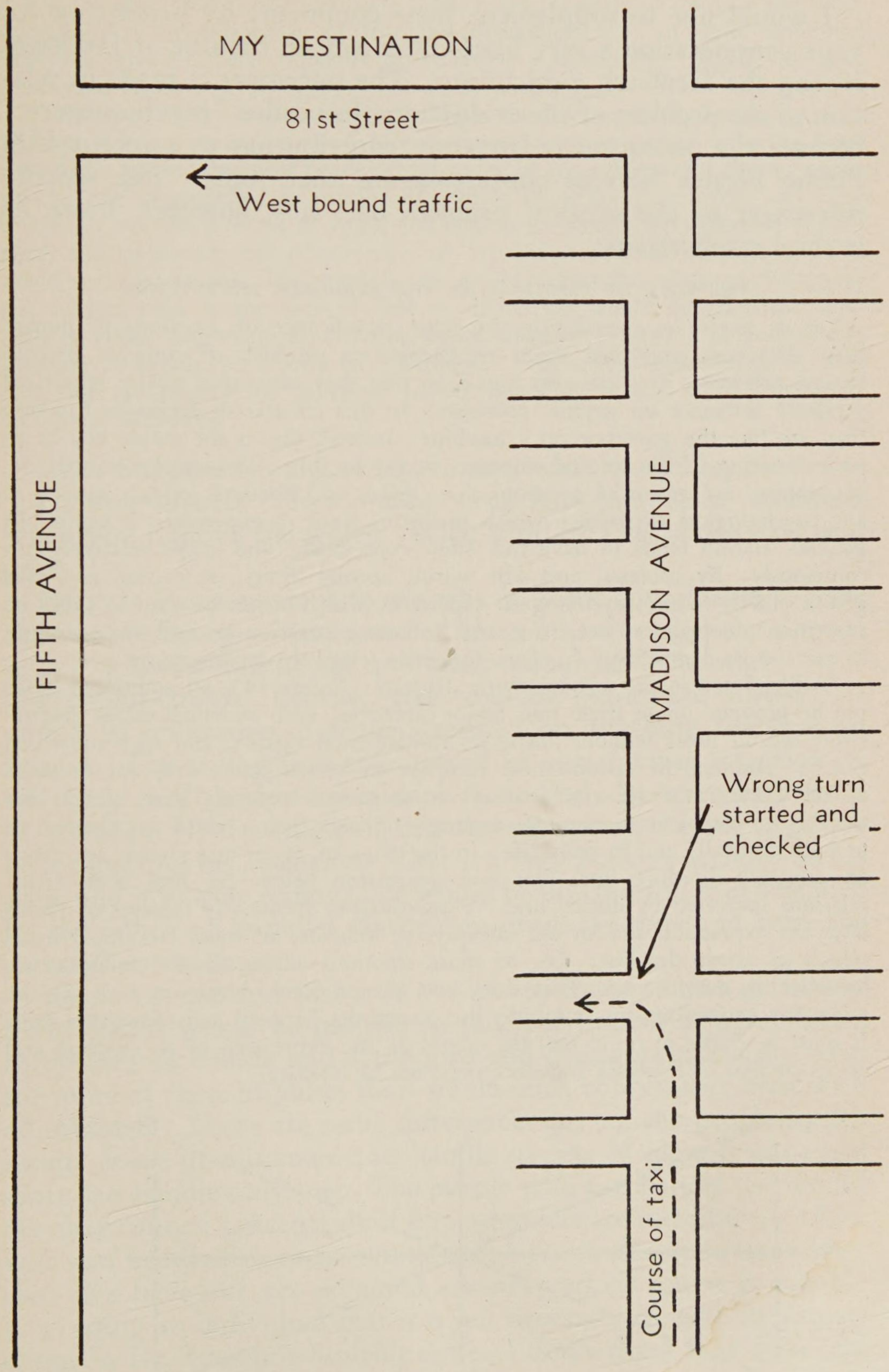

MY DESTINATION
81st Street
West bound traffic
FIFTH AVENUE
MADISON AVENUE
Wrong turn started and checked
Course of taxi

FIGURE 24

I would like to supplement these comments by presenting for your consideration a very brief statement on the role of *Emotions* among the feedback mechanisms. The statement is made in relation to the problem of the evaluation of so called "psychosurgery," because the quotation is from my contribution to a recent U.S. Public Health Service conference on that topic. The specific references to the surgical problem does not, however, lessen its general implications:

THE ROLE OF EMOTIONS IN THE FEEDBACK MECHANISMS

Let us begin by considering the basic significance of emotions in human life. Although emotional states are themselves products of complex psychological processes, they are also *causal,* in that they exercise a vitally important *feedback* influence on psychic processes. In this circular or feedback function they are like the governor on a machine. Indeed, this is the major key to an understanding of the role of emotions in psychic life. Thus under normal circumstances one group of emotions (i.e., anger and elation) lends a quality to any psychological experience which makes us want to experience it again. In general, elation tends to have this effect consciously, and anger relatively unconsciously. By contrast, and still within normal limits, depression and fear give a quality to any psychological experience which makes us want to avoid its repetition: depression exercising this influence consciously, and fear tending to exercise its dampening, feedback influence relatively unconsciously.

As illustrated in the accompanying diagram (Figure 24), all emotional states can be grouped under these four major categories, each of which varies qualitatively within itself without losing its fundamental quality, and each of which can be combined in various more complex emotional states with one or more of the others. Certain combinations occur more frequently than others; but there is no combination even of seemingly opposite pairs, which is unknown to us both clinically and in daily life. In the diagram, anger and elation are above an arbitrary dividing line, fear and depression below the line, with cross-relations indicated by dotted lines. Psychosurgery frequently releases or facilitates the expression and/or the intrapsychic influence of those emotional states which lie above the line: i.e., of those emotions whose circular influence on behavior is reenforcing. This does not always occur, however; nor can we make any generalizations as to why this sometimes happens and sometimes fails. It must be borne in mind that the effects on the expression of an emotion and on its circular or feedback influence need not be identical.

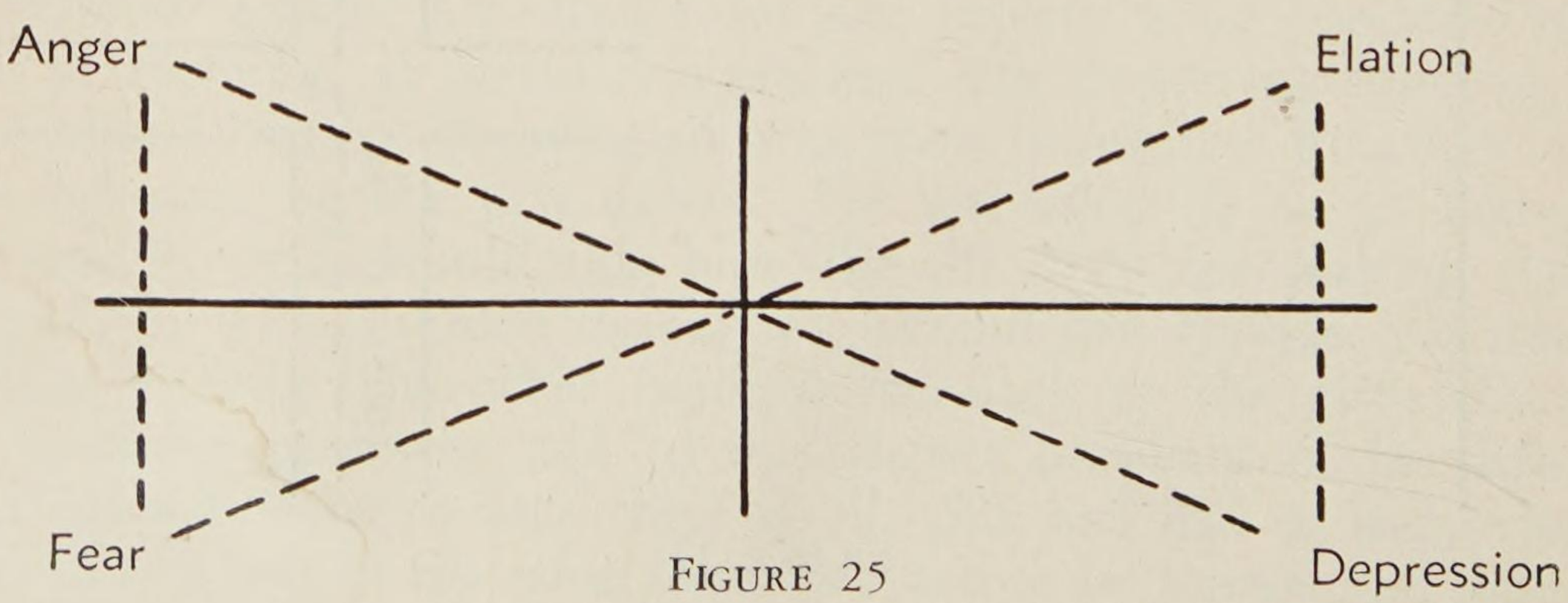


FIGURE 25

On the other hand, psychosurgery tends more regularly to decrease the influence and/or the expression of fear and depression (i.e., of those emotional processes whose circular influence on behavior is inhibitory). Again this generalization about psychosurgery is not an invariable rule.

From these facts we may draw one simple conclusion. If psychosurgery were attempted in a "normal" person (for instance, in an individual suffering from intractable pain), the most fundamental index of the influence of the operation on the role of emotions in psychic life would be to compare before and after operation the effectiveness of anger and elation in causing repetitions of experience, and conversely the effectiveness of depression and fear in causing us to avoid such repetitions. For example, we would expect that after psychosurgery the subject even in the face of fear or depression would be relatively complacent about repeating experiences which because of this fear and/or depression he would previously have avoided. This is the basic change to look for, and not merely the influence of the operation on the overt display of emotional fireworks.

Pitts: Suppose one did not have one, what would happen?

Fremont-Smith: That is like what happens to the rectangle when you remove the width. What kind of a rectangle have you got?

Pitts: It is not obvious to me why it is not conceivable to have the human being without one of these objects called the unconscious. How would he act and what would he do wrong?

Fremont-Smith: Let me give you an immediate answer. It would be impossible to focus attention on anything for even an instant. The only reason that any of us now focuses attention on anything at all is that unconsciously he excludes from his stream of attention all the inflow of impulses excepting the one to which he is directing his attention. That is a very positive activity on his part, and if it were removed he would be blurred and overwhelmed by the inflow of impulses, both from inside his body and from the external environment.

Mead: Let me add that it is possible to conceive of a culture so built that it would be possible to take the human being at birth, equip him to handle and symbolize and deal simultaneously with far more of these impulses than we do now, conceivably even with all of them. There are some differences among the people in this room. Some in this room can handle six sets of ideas at once and some can handle only one. The people who can handle one would say they cannot concentrate if five other sets are introduced. Those who can handle five or six would still have to eliminate something else. We have not yet invented a society which makes it possible so to equip an individual that it is not necessary to have an unconscious in Dr. Fremont-Smith's sense. I don't think it is quite our

sense. For your sake, Dr. Pitts, I could invent one.

Fremont-Smith: They merge. I think one could show, as Dr. Kubie said, that in the taxi driver there was very little that was buried. Everything was on the surface. The same thing is true, at the moment, of incoming impulses, for instance, the sound of the stenotypist's fingers, which you probably did not hear, but which, at this moment, you now can hear. That was not buried and could have been brought to your attention at any moment, and yet some active process excluded them so that you could listen or focus.

Pitts: The existence of that gives me no difficulty at all. It was that I had detected a difference between his concept and yours; it was about his that I had a queasy feeling, not about yours. I think that is inescapable.

Fremont-Smith: I think that you will find no dividing line between the two.

Pitts: He thinks there is.

Fremont-Smith: Let us ask.

Kubie: What do you think I think? Tell me what I think.

Pitts: The reason I think that you happen to believe this is that we talked about something more or less similar last time and I think you ended in the position that there was a substantial difference between your notion of the unconscious and that of the other people who had one roughly of that kind.

Kubie: What kind?

Pitts: In the sense that yours was somewhat more like an isolated box than theirs.

Kubie: Unfortunately the unconscious can become isolated indeed. If it did not, there could be no such thing as a neurosis; but unfortunately neuroses are ubiquitous. What I am trying to say is that along with the development of the capacity to represent things symbolically (which is necessary for all of our highest human functions), there has arisen for reasons which are not entirely clear, this dichotomy between things which are represented and of which we are conscious, things which are represented and which are on the fringes of consciousness (such as the taxi-driver example), and things which are so deeply buried that we cannot get at them without some special method of excavation.

Wiener: The point is, then, that the second case, that of the woman, was between the two. You did not dig for it, but it was suppressed.

Kubie: I dug for that by various indirect methods.

Wiener: Yes.

Fremont-Smith: But it came up partly spontaneously when the right time came, when life experience had changed.

Kubie: When her capacity to face certain problems had been enlarged. When the guilt feelings about those problems had been diminished.

McCulloch: Forgive me if I pull a little closer to the main theme. I am by no means clear in what sense a neurosis is part of the development of language in the sense of symbolism? Is it not possible for a neurosis to exist quite apart from symbolism? Is it of the essence of a neurosis that it is necessarily symbolic? Even if it is necessarily symbolic, what is the connection with the form of symbolism that we call language?

Marquis: May I add my question to that please? Has the communication that is added to the neurosis the same connection that the neurosis has?

Gerard: Could you have, conceivably, a social organization in which the neurosis did not develop because the subject consciously became aware of everything that you attempted to symbolize?

Kubie: We do not know. It is possible for me to conceive of a cultural attitude toward infancy and childhood which would attempt continuously and repeatedly to bring into the full light of consciousness all of those internal ethical struggles which under our present cultural pressure become unconscious. I can conceive of entirely different ways of bringing up children, the goal of which would be to minimize this dichotomy. I am not sure that it is conceivable to me that there would ever be a culture which could eliminate it completely. I repeat, however, that we do not know because it has never been tried.

Related to this is the question of when this dichotomy first arises. Here again it is difficult, to be sure. One sees a great deal of meaningful automatic activity in the preverbal life of infants which seems to evolve into early panics, nightmares, eating disturbances, bowel disturbances, and primitive neurotic episodes of childhood. Certainly it must begin early, yet I cannot answer when with precision. This is one of the more important questions of cultural development: What can we do to control, minimize, shape, eliminate this dichotomy?

McCulloch: Take them backward in time. You answered Dr. Gerard's question.

Mead: Communication.

Marquis: Does the neurotic symptom have the same relations as communication in general?

Kubie: This really relates to Dr. McCulloch's question. Again it

is hard to answer it, because certainly the functions are different. Every neurotic symptom serves multiple functions, among which is always some gesture toward the outside world, whether the symptom is a height phobia, a handwashing compulsion, or a conversion symptom. Whatever it may be, it is a way of telling the world that there are things I will do and things I will not do. Yet this is not its primary function. Its primary function is the effort to deal with certain internal tension states arising out of unconscious conflicts.

Marquis: There is one kind of language which is sometimes thought of as expressive, just emotional expression, which does not always have a communication function. It occurs when you are alone, and so forth. Is the neurosis that kind of behavior?

Kubie: That reverts to what I said before about emotional disturbances which can be induced experimentally in animals. In their fundamental nature and dynamics these are quite different from the disturbances of neuroses.

Klüver: I wonder whether all animal psychologists would agree with your statement that conditioning or learning is always associated with states of craving and occurs only in the absence of satiation. A research physicist once came to me with the idea of doing some work in the field of psychology. I suggested that he should pick a problem in which he could utilize his knowledge of physics. After a few days he came back with an indictment of all objective studies in the field of animal behavior. He felt that the studies done so far had merely produced a one-sided and misleading picture of the psychology of animals, since experimenters always had used hunger or the avoidance of punishment as motivating factors. When I finally inquired into the type of problem he wanted to work on, I discovered that he was interested in such problems as the psychology of a chimpanzee retiring with a full stomach for the night to its tree nest and taking a last look at its surroundings. He was wondering whether such a chimpanzee was capable of gaining new insights by attentively inspecting the landscape or even capable of enjoying the real beauty of an African sunset. I am not going to advocate the use of chimpanzee subject for studies in experimental aesthetics; I merely wish to point out that there is some evidence to the effect that a full stomach does not necessarily prevent conditioning or learning processes in animals. To be sure, even a determination of the properties of visual *Gestalten* involves more than an analysis of perceptual factors. It was Kurt Lewin who used to point out that you cannot analyze or specify such properties—no matter how

objective they are—if you turn your back to them. An objective psychology of visual *Gestalten* still depends on your having to turn around and taking a look at the *Gestalten.* Similarly, the fact that learning may involve nonperceptual or noncognitive factors does not necessarily mean that these factors must be identified with cravings, deprivation or an empty stomach.

In connection with Dr. Marquis' question, I should like to recall one of Buytendijk's studies. Incidentally, Buytendijk, who for many years was in charge of the largest physiological institute in Europe, has now become a psychologist and in a recent paper has analyzed "the first smile of the child" [Buytendijk, F. J. J.: Das erste Lächeln des Kindes. *Psyche*, 2, 57]. Since you strongly emphasized the role of internal needs and body tensions in development, I wonder, Dr. Kubie, how you would interpret the first smile of an infant. Is the first smile an epiphenomenon of bodily states or is it a smile at something or somebody?

Kubie: I am not sure that I quite understand the question.

Klüver: Suppose the infant is lying on its back perfectly relaxed and with its physiological needs attended to. Now it smiles for the first time. The question is whether this smile is merely a concomitant or function of bodily conditions or whether it is a phenomenon elicited by, or directed toward, some stimulus or some stimulus aspect of the external world.

Kubie: I should think there is a considerable probability that the first smile of the child is part of general random activity but that it becomes a method of communication with the grownups that tend to it. It is clear to anyone who has ever brought up children that it becomes a method of language very early.

Fremont-Smith: Identification comes in early there.

Savage: Dr. Kubie's interpretation, if taken literally, is testable anthropologically. If it is correct, there should be cultures—many of them—where a smile isn't a sign of pleasure, where it just means something else, or even more frequently, means nothing at all.

Pitts: It should not be very common, should not mean anything. Possibly babies always smile when happy.

Mead: They smile everywhere, but I don't believe you have to state it that way. If you have as random behavior in response to states, internally recognized states, either crying or smiling, the culture can change the meaning of either. A good many cultures exist that handle tears in a much more positive way than we do, and tears can be turned into expressions of pleasure later; smiles certainly can be turned into expressions of displeasure. In Bali there are a good many situations in which an adult's smiling is

regarded as virtually insane behavior. The person who smiles too much in Bali and the person who smiles at a stranger are considered insane. The expressive forms are modifiable by culture. I still think the data we have at present suggest that a smile occurs initially in states which are diffusely pleasant in some way, such as a full stomach. The smile seems to be a recognized phase, as suggested by Spitz's data on the child smiling at the masked human face [Spitz, R.: Emotional growth in the first year. *Child Study*, **14**, *68* (1947)]. That is probably some of the best material we have.

Wiener: A smile is quite different from what is expected on certain occasions of hostility.

Kubie: There is much hostile smiling in our culture, too.

Bateson: I suspect that the answer to Dr. Klüver's question, whether the smile is a symbolic statement about the outside or the inside, is that it very rapidly becomes both. And the fact that it becomes both is one of the roots of the unconscious component of language. What Dr. Kubie is really trying to say is that language is a double coding: both a statement about the outside and a statement about the inside. It is that doubleness which gives this conscious-unconscious quality to it.

McCulloch: May I come back to my question, because I think it is very closely related to Heinrich Klüver's? Is it true of the essence of a neurosis that the process is symbolic?

Kubie: Yes, it is of the essence of neurosis that the process is symbolic and that the subject does not know what it is symbolizing.

Teuber: To whom?

Von Foerster: To himself.

Fremont-Smith: That is part of this business. It is that the language is not only communication to the outside world but also to one's self; the neurosis is an interruption of communication to one's self as well as to the outside world.

Pitts: As a corollary, an animal neurosis, so called, and a human neurosis have nothing in common.

Mead: Dr. Kubie gave a definition of neurosis at the first or second meeting of this group that ought to be appropriate here. You said a neurosis was characterized by an inappropriate method of seeking a goal.

Kubie: I have never said that.

Mead: You have.

Fremont-Smith: You have neurotic amnesia for it.

Pitts: An irrelevant repetitive process.

Kubie: If, without knowing it, we seek unattainable goals, under any mask, then our conduct becomes both repetitive and inappropriate, but it is the unattainability of the unconscious goal that is essential. The repetitiveness and the inappropriateness are secondary attributes.

Pitts: He said irrelevantly repetitive process.

Mead: That he added also. That was the essence of it.

Pitts: He must know.

McCulloch: You say it is of its essence symbolic. Is that symbolism necessarily of the kind we call language?

Kubie: Well, Dr. McCulloch, in the neurosis the symbol has many forms. In obsessional states language plays an important role. In compulsive states action plays that role. In the phobias it takes a still different role. Actually, the use of language in the neurotic symbol is relatively unimportant. It is relatively unimportant in which form the neurosis presents itself. What I am saying is that the symbolic process which is used in language, and which is the *sine qua non* of language, is also the *sine qua non* of the neurosis, with the one additional factor that in language we do know by and large what we are trying to represent, whereas in the neurosis we do not.

Brosin: At the risk of oversimplifying this question, in direct answer to Dr. Marquis, I find the simplest solution or model is to regard the individual with the neurosis as being a complex, integrated series of operations in a matrix of other series of operations. The inappropriate ego functions may be directed toward two kinds of outer spheres of operations. One of these may not be physically present but may represent an inappropriate series of symbols of the other (inner) sphere.

Fremont-Smith: Or both at once.

Brosin: Or both at once.

Mead: You would include—

Brosin: In the language of the ego, id, superego model, the ego operations concern environmental operations or the instinctual-physiologic, or internalized, conscience. The ego has a galaxy of goals in the inner sphere of operations which may or may not be contiguous and relevant to the outer series.

McCulloch: Are those relations necessarily symbolic, or may they be direct?

Brosin: Let us recall the work both of semanticists and of philosophers on this question of the specific meaning of such operations. Until we define the detailed processes more clearly, I cannot answer the question. If you would set up examples and a

common notation that would make the question concrete in operational terms, I could venture an answer. We need a notation which is sufficiently neutral and free from emotion or older accrued meanings to permit formulating an experimental question which would elicit a meaningful answer.

Gerard: Perhaps the problem lies in what one means by symbolization.

Stroud: I would suggest that acts are the key. Acts, whether symbolic, irrelevant, or effective, that our subject is impelled to act. He is in a difficult situation. If he acts in such a way as to get out of it, we have no neurosis and no problem. If, however, he acts . . .

Fremont-Smith: How do you define "getting out of it," because there is the crux?

Stroud: That is precisely it. Getting out of the situation is to my way of thinking readily identified by the absence of the repetitiveness. If you got out of it, you would not repeat it. You can only say a man repeats it in this sense, because he is still in it now.

Kubie: I think it is important for us to recognize how closely related is the potentiality for neurosis to our highest symbolic functions. That is why I cling to this.

Fremont-Smith: This is the heart of the whole problem of this conference group right now. This is the thing I have been waiting for since I started this conference group: that we who think in mathematical, physical, and engineering terms would come to grips in a genuine way with the people who think and talk in symbolic unconscious terms. There is a tendency to intolerance on both sides which should be avoided. I suggested to our Chairman earlier that for the next meeting we should have the raw data from a hypnotic experience presented to the whole group for discussion. We are going to get that next time, because there are crucial phenomena dealing with human behavior which operate in the thinking of mathematicians; there are interactions between mathematicians and psychoanalysts with which we must come to grips if we are going to understand either the mathematical or emotional end of human behavior.

Gerard: Hear! Hear!

Pitts: It appeared to me that he was separating the unconscious from all normal processes. It seemed to me that if the unconscious was a valid process, it was related.

Fremont-Smith: I am sure you misunderstand him, because that is not his concept.

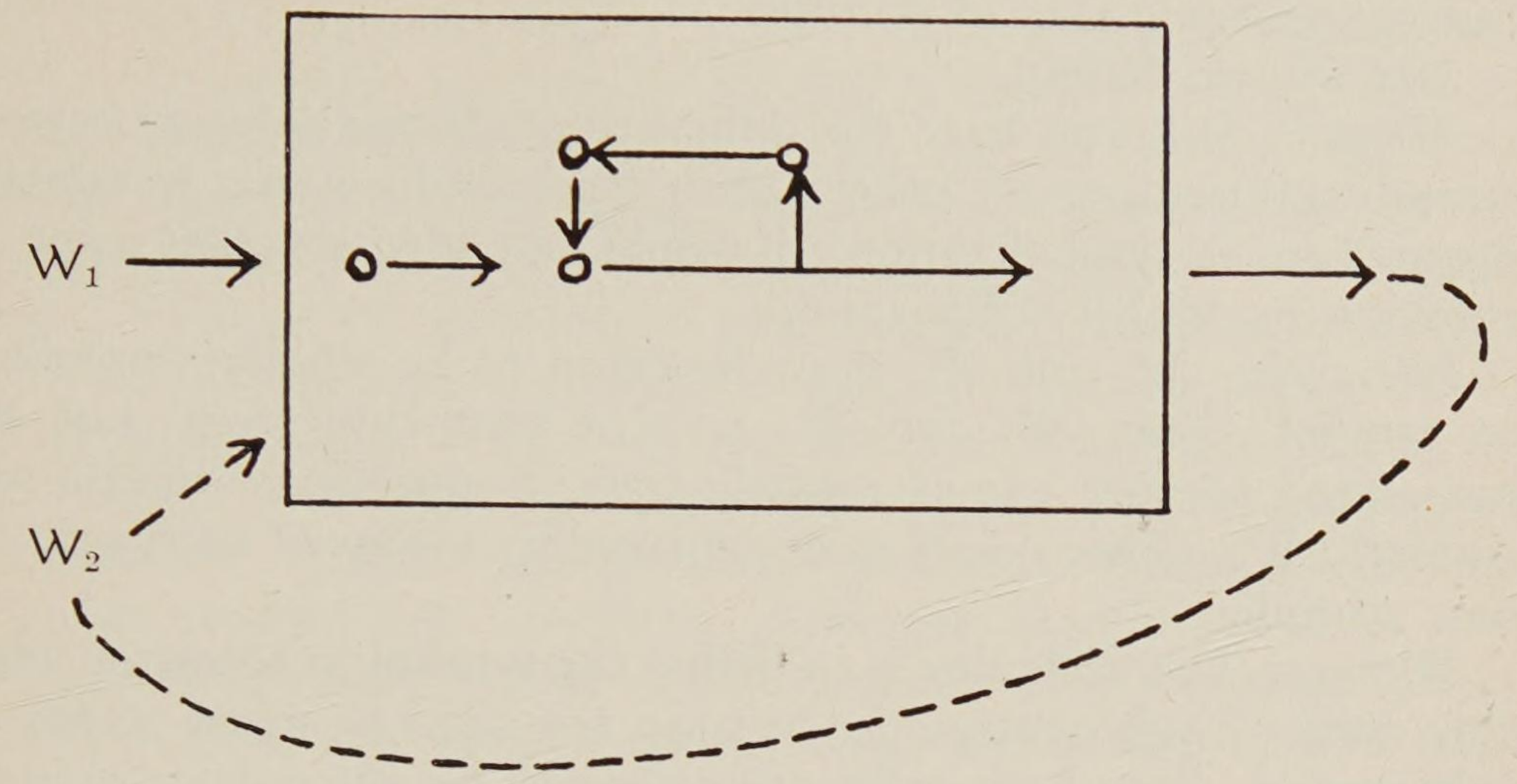


FIGURE 26

McCulloch: I should like to draw on the blackboard a diagram which I think will help to get rid of the difficulty which is standing in our way quite gratuitously. Consider an organism with receptors hooked through sundry circuits to effectors. Whether signals go straight through or reverberate within it, they evidently come out by way effectors into that same world where it has its receptors. If that is all that we suppose happens, I see no reason for calling the process symbolic, but when we consider two parts of the world, say W_1 and W_2, detected by this organism, and the central activities for these two associated so that for that organism W_1 comes to stand for W_2 at some later date, I would call it symbolic. If W_1 and W_2 have any common properties, W_1 can be used to signify W_2 to someone else or—

Stroud: Or to itself at a later date.

McCulloch: That is oversimplification. Every one of these is a distortion.

Fremont-Smith: You never have W_1, always W_2.

Stroud: Substitute W_1 for W_2. If I eat a melon, does the empty rind stand for the symbol of eating the melon?

McCulloch: For the basic notion of eating.

Stroud: I don't treat empty melon rind as melon. I do treat in many respects the word "melon" as a melon.

Pitts: It is a perfectly real, difficult stimulus equivalent, to begin with.

Werner: What about substitution?

Pitts: We must take him to have meant not stimulus directly but equivalent classes of stimuli.

McCulloch: Surely.

Werner: Still you have the difficulty of distinguishing between stimuli substituting for others, as in true conditioning, in contradistinction to symbolization. I would not identify that type of substitution with symbolization.

Pitts: No. W_1 and W_2 must be taken to be wholly equivalent of symbol. Since W_1 and W_2 are not class equivalent, but W_1 may stand for any one of a whole class of equivalent stimuli, and similarly W_2, either one may be replaced by a class of such equivalent stimuli.

Werner: The difficulty is to define the symbol in terms of what this type of substitution is, because we have so many types of substitution. You have to be quite clear about the particular type of substitution which you call symbol.

Pitts: Substitution which does not occur according to classical perspection, which is dealt with in Helmholtz's groups.

Fremont-Smith: Right.

Kubie: Let me give another example of what I mean. Perhaps this will make it clear. I think of an adult who was an X-ray man and an able and outstanding scientist. He got into serious trouble with the law because he was also a Peeping Tom. This brought him to treatment. As a child, he had had a severe eye-blinking tic. It was easy to work out that the eye-blinking tic had represented a defense against his own childish visual curiosity, which had been translated into a career as an X-ray man. Yet the childish needs which had become unconscious remained unsatisfied and finally broke through in the form of a Peeping Tom perversion that finally landed him in the hands of the law. That is a simple example of what I mean when I say that all of these things are essentially symbolic functions.

McCulloch: Would that be any more so than a goose that nurses a bright tin pan all summer by himself and makes love to it, and so forth?

Klüver: Since I have no definite opinions about neuroses in infants and children, I should like to speak about a baby monkey. I happen to know something about bringing up baby monkeys. The baby I am now referring to had a rather remarkable mother: namely, a Java monkey in which the connections between the temporal and the frontal lobes had been severed bilaterally without removing any cerebral tissue. Three years after the operation this monkey delivered a normal baby and within the next two

years had two more pregnancies at term. In all three instances the baby was found lying on the floor of the cage. The mother made no attempt to pick it up and definitely refused it even when offered. She showed no excitement when the baby was finally removed. There was, therefore, a complete absence of maternal behavior. It was one of the three babies of this monkey that I took to my apartment, where it spent the first months of its life. Its bedding was a pad of white cotton on which it used to lie contentedly, with its tail grasped in both hands, whenever its bodily needs had been attended to. One day it was taken back to the "hostile" world of the laboratory, where it exhibited at once marked fear reactions. Although it held on to its own tail it did not cease crying until it noticed a pad of white cotton. It ran to it immediately, squatted on it, and then produced what I believe to be the simian equivalent of a smile. In the world of this monkey baby, two things, grasping its own tail and a piece of white cotton, appeared to be essential for producing "social security" and peace. In fact, the same result could be achieved by reducing the pad of cotton to a few square inches or even less than one square inch of white material. The absence or removal of the white cotton always resulted in great anxiety. Unfortunately, I could not go on providing cotton through the following years. I wonder, Dr. Kubie, whether behavior deviations developing in such a monkey would have finally brought the animal in conflict with the "laws" of a respectable simian society.

Kubie: It may be. I am not denying the possibility of real neuroses in animals. I am just saying that I do not know. It is an even more difficult thing to appraise in animals than it is in humans.

Klüver: I have talked about cotton and Dr. Stroud has talked about "things" and "not things." We want to know the psychological structure of these things and not things in a child's world. These things are certainly not the things of our adult world: they have an entirely different "physiognomy." It is fortunate that we have Dr. Werner with us today, who has been concerned for many years with the psychological problems involved in "physiognomic characters." Some child psychologists have claimed that, long before an infant becomes aware that the mouth of the mother is red, her eyes blue or brown, and her hair blonde, it can recognize the face of the mother as being friendly or unfriendly. In fact, it has been claimed that our thresholds for recognizing or differentiating physiognomic characters such as friendly, gay, sad, melancholy, threatening, tempting, and so forth, are lower than

our visual acuity thresholds for brightnesses, colors, shapes, et cetera. The problem is, therefore, how a world divided into things and not things is related to a world differentiated on the basis of physiognomic characters. In the world of the baby monkey the white cotton was undoubtedly more than merely some material with a certain brightness, size, shape, and texture: it was imbued with certain physiognomic characters. And it seems to be the difficult business of psychology to deal with these ubiquitous and powerful determinants of behavior. I wish Dr. Werner, who is an expert in these matters, would comment on this.

Werner: I do not have very much to add to what Dr. Klüver said. The recognition of physical qualities, or what I have called physiognomic perception, is a primordial way of perceiving, definitely not restricted to human experience. Now some psychologists have termed a primordial physiognomic perception, such as the expression of friendliness in the behavior of an animal, as symbolic. I am a little bit puzzled by Dr. Kubie's use of the term "symbolic" without defining it, and particularly by his setting it off against expressive perception and behavior in the primordial sense. If a statement is made that symbolic behavior is essentially human, and therefore that neurosis, which presupposes symbolic behavior, is essentially human, such a statement hinges on a clear definition of what a symbol is.

McCulloch: Licklider, will you try to define what is meant by "symbol" in such a context?

Licklider: I think that, since one of the important things is to try to talk about neurosis in terms of language and symbols, in terms that are perhaps admissible both by the psychologist and by the mathematician, it might be all right for me to try—since I fall somewhere inbetween—to define "symbol." To do that, I should like to start off with the notion that any physical process, if broken up into small enough parts, can be represented by an arrangement of Dr. Shannon's black boxes, and that some of these processes have the property (he referred to it last night) that they lose very little, or no, information. In other words, you can reconstruct the predecessor if you can get your hands on the successor. What I am saying is that all processes can be described in terms of transformations, sometimes information-abstracting or information-losing transformations, sometimes information-preserving transformations. Symbols, I think, are the products of information-preserving transformations, products that have the special property of not looking like the things of which they are transforms.

Kubie: Would you accept one modification that there are two orders: one information preserving and one information burying?

Licklider: Would you elaborate "information burying" a little?

Kubie: The difference between the symbolic process in speech and in neurosis would lie precisely there.

Licklider: Let me say this: the predecessor can be recovered if you put your hands on the successor and if you know the rule of transformation. In the case of information-burying transformations, nobody knows the rule of transformation, unless it is the psychiatrist, who thinks he knows the rule of transformation. Of course, the rule may include the psychiatrist. I have said all I think I should say.

Fremont-Smith: When not conscious it is not easily accessible to translation and when it becomes conscious it becomes translatable. There are some things which are accessible to translation but it is not convenient to translate them, and there are other things which are inaccessible. Speaking at a different level, it might be convenient to translate, but you cannot do it well. It is at that level that we are making the distinction of the neurosis: even though it would be convenient to translate it you cannot; you have to behave because you cannot conveniently translate it when it would be convenient to do so. You are behaving in a neurotic way. Is that close?

Kubie: In that sense, therefore, the neurotic transform for communication cannot be used in the same way that the transforms of speech can be.

Licklider: It may unwittingly communicate to someone who knows the rule.

Teuber: That is the point I was trying to make with regard to symbols, by asking: "To whom?" I cannot think of a symbol without thinking of somebody.

Stroud: The neurotic family is ruled by the most neurotic member. That is the social communication function of these neurotic symbols. That is the next point I wanted to bring up.

Teuber: There is still another point that has been brought up during the last 48 hours, and that is that a symbol does not have to look like whatever it signifies. It is obvious that on Shannon's level it does not look like anything at all. Sometimes it is just a dot or a click or a pulse. It is not an eidolon as the ancients thought, an image which must resemble what it refers to, along some very obvious dimensions of similarity. In that sense I think we can find at least some precarious common ground if we are all agreed that a symbol does not have to look like the thing it symbolizes.

Brosin: I don't think the meeting ground is at all precarious once you explain it as well as you have. To reiterate an earlier statement about the importance of the object "to whom" the meaning of the symbol is directed, it may be useful to stress the fact that ego functions include a wide range both of direct and of more remote significance. An act such as throwing a plate during a quarrel may have multiple elements of both. Symbolic ego functions (receptive, integrating, executive, or motor) may be directed to objects in the outer environment or toward the more inaccessible areas in the unconscious. The reinforcement from the older emotional patterns may then furnish the impetus to produce or distort externalized symbol formation for a long time. The neurotic is solving his problems inadequately by symbols which are inappropriate, but these may often be easily understood by the layman in such everyday actions as apprehensive laughter, hostile smiling, ambivalent muscular gestures.

McCulloch: I want to ask Larry Kubie one more question. Then Larry Frank and Frank Fremont-Smith will speak. Is it of the essence of a neurosis that it is symbolic or is it of the essence of a neurosis that some information, whether it be symbolic or not, is in some manner inaccessible? The inaccessibility is the crucial item.

Kubie: Dr. McCulloch, sometimes in the course of argument you misstate my position a bit. What I have always said about it is that *if there were no unconscious processes, there could be no neuroses.* That is another way of saying what you just said: *if the information were accessible to the symbol, there would be no neurosis.* What is common to the neurosis and language is the transforming process, that is, the symbolic process. What is peculiar to the neurosis is the inaccessibility of the psychological state represented by the symbol.

McCulloch: Then my question is very much in order. I see no reason why the concept should in any sense be linguistic.

Mead: No.

Teuber: No.

McCulloch: If the symbol is no more symbolic than the nervous impulse is of the stimulus that evoked it.

Stroud: It is just possible that these symbol functions in neuroses operate for the person who looks at the neurotic but not for the neurotic himself.

McCulloch: Yes, that is the question: To whom is it accessible?

Frank: That reinforces a point I want to raise. It is too late now to discuss it, but we must give more attention to inner

speech. One gets the notion that inner speech is the way the neurotic maintains his idiomatic speech codes and that therapy consists of bringing the patient's inner speech into line with the actual world as directed by the therapist, so he won't go on giving a neurotic interpretation to events. Isn't that right?

Kubie: Right.

Frank: We have neglected in this discussion the important role of inner speech. We know very little about it and we should try to explore its operation so that we can discover more about the symbolic process and especially about language.

Fremont-Smith: That brings up the point I was going to make about "to whom," which is the crucial question here. It is not only to the individual or to the outside world, but also to which phase of the individual. I think that it is essential to our understanding of what we are talking about to accept the assumption that the individual has two or more phases, multiple phases, and that a thing may be accessible to one phase and not accessible to the other phase, or to two or three phases. There are dramatic examples of that in raw data, which I think can be demonstrated if necessary. The moment you bring that in, you have inner speech because inner speech "to whom" is involved. There is good evidence to show that inner speech, which can even be vocal, is to the other phase or to the other person, as we sometimes say. There may be two or more persons, so to speak, or phases of the person in which communication may be going in one direction but not freely in the other; and in order to understand what we are talking about I think we should realize that.

Bateson: I wonder whether we can clear up something about the question of "accessible to whom?" One of T. S. Eliot's poems, "Sweeney Among the Nightingales," makes a great deal of sense if you read it as a poem, but you cannot say what sense it makes. That poem has been analyzed by Collingwood in his *Principles of Art*, and he reconstructs, probably correctly, the objective situation which the poem is about. But this information does not add in any way to the impact of the poem.

Pitts: Excuse me! What objective situation?

Bateson: The context. I can't recall the poem in detail nor can I remember the analysis of it. I want to get the reference into the record. It is a situation in which Eliot is in the presence of certain people; there is a convent next door, and the life in the convent is visible from where he is, and so on.

Pitts: It is the authorization.

Bateson: The authorization?

McCulloch: It has nothing to do with the understandability of the poem.

Bateson: That is the case.

Pitts: It is possible, as Teuber said, that symbols need have nothing to do in these. They need not be at all like what they refer to. It is quite different with respect to complexes of them; that is, complex symbols which are made of symbols as parts will have a definite reference; they will have a certain kind of isomorphism to relations among the things for which they stand that will be a parallelism and a similarity. I think that occurs only when we get to the level of complex symbols. That is, a statement about a man and a dog has a correlation; namely, if you take the statement, "The man beat the dog," there is the man, a transitive relation, and the dog; there are three symbols following one another, one with the relation in the middle, an actual event to which it refers. You can identify the objects and relation, whereas single symbols may have no direct content of their own. The complex symbols may have content of their own, although elementary components may be arbitrary, like letters or simple digits.

Licklider: This takes us back to the conversation of yesterday morning. There may be isomorphism where there is not the perceptually obvious sort of relation that leads one to say, "Here is a clear parallel."

Teuber: It need not be point for point. It could turn out to be multiple.

Pitts: Symbol alone, as a matter of fact, conveys no information unless you suppose the implied statement that in such place there is the object designated by the symbol or that something like the information can be conveyed only by the whole statement, which is a complex of symbols.

Teuber: Given the system to which your symbol refers.

Kubie: Precisely. The little piece of white cotton could represent the security in your home for the little monkey.

Klüver: I wonder if your statement that the human organism has only two symbolic functions should perhaps be qualified. There are symbols, I believe, which have to do neither with language nor with the neuroses.

Kubie: The language of treatment and of the hypnagogic reverie which Silberer describes is the raw material out of which the dream comes. The language process of the dream is almost entirely in visual imagery (2).

Klüver: "Autosymbolic phenomena" of the type Silberer de-

scribed may also occur in dreams. At a time when I worked every day with Java monkeys and in spare moments thought about the principles of Kantian philosophy, I dreamed one night that I entered a room of my laboratory, in one corner of which I found a gunny sack standing against the wall. When I opened the sack, I saw that it was filled with Idaho potatoes. While I still looked at these potatoes, every potato turned into a Java monkey. It was clear to me in my dream at this moment that "the synthetical unity of the manifold in all possible intuitions" was directly in front of my eyes.

Pitts: How?

Klüver: In phenomena of this kind and in those described by Silberer, there seems to be a direct symbolic representation of thoughts.

Pitts: How would that go? I cannot see that.

Kubie: Kekule's dream in which he evolved the problem of the benzene ring.

Klüver: Kekule's own account of how he arrived at his cyclic formula has become very famous.

Kubie: Loewi had a similar experience.

Klüver: But since Kekule was a chemist and not a psychologist, I have wondered sometimes whether his own account of his discovery of the benzene ring should be accepted [Schultz, G.: Bericht über die Feier der Deutschen Chemischen Gesellschaft zu Ehren August Kekulé's. Deutsche Chemische Gesellschaft Berichte, **23**, *1265A*. Berlin (1890). Japp, F. R.: Kekulé Memorial Lecture. *J. Chem. Soc.*, **73**, *97* (1898)] One of the designs frequently observed in entoptic phenomena and in hypnagogic hallucinations is the "honeycomb" design with its hexagonal elements [Klüver, H.: Mechanisms of Hallucinations. Chapter X in *Studies in Personality*. New York: McGraw Hill (1942) p. 175]. This hexagonal pattern was seen entoptically by Purkinje, König, and many other observers. I have seen it on several occasions, not with closed eyes, but on the ceiling after awakening. It is highly probable that Kekule, who was used to watching "repeated visions" and configurations gamboling before his eyes, also saw hexagonal patterns either entoptically or in hypnagogic hallucinations. Unfortunately, my hypothesis that the benzene ring was psychologically derived from the hexagons of this well-known honeycomb design can never be checked. I should mention that hexagonal patterns also appear in mescaline hallucinations, in the visual phenomena of insulin hypoglycemia and when viewing flickering fields under certain conditions.

Stroud: How do you solve problems in sophomore calculus that way? I did it half one summer that way when I was asleep.

Fremont-Smith: Einstein got the formula while improvising at the piano and thinking about nothing whatsoever.

Mead: I think at some later stage the question Pitts raised will come back, that we might do something with the grammar of dreams, and do a great deal more with the primary process than what we have done so far. Kubie has given us several discussions of neurosis as we went along, and we had a little bit of Rorschach, but we have never discussed in any detail whatsoever the type of grammar, the logic of the primary process, and the way in which that type of grammar and logic are involved in the thinking in visual images and what kinds of speed can be made with visual symbols when they interact with the sort of thinking that I think Pitts calls normal.

Pitts: Rather the rhetoric of dreams.

Mead: You can call it that if you like.

Pitts: The rules of rhetoric of dreams.

Mead: Of artistic construction. We have had bits once in a while by Von Neumann. Wiener has given a little bit of dream but we never systematically discussed it. At some future time we will want to put those two kinds of thinking together much more than we have done so far.

Fremont-Smith: We will touch on it at the next meeting, when we have the one-to-one relationship of human beings and discuss it in the hypnotic phenomena. As a matter of fact, it is just in that area that Brenman is quite superb, I think, in discussing it. I do feel that this time we have come closer to a discussion in which there was a common denominator for every discipline here. I don't think we ever came as close to approaching that as we did this afternoon. And I think that that is the goal. We ought to have many common denominators for all the disciplines if we are going to reach our goal, but at least we had a common denominator this afternoon, and I think it is the first time we have had it. I feel very pleased. I must say I have been quite impatient for it, and others have been impatient for it. I really think we got closer to it today than we ever have before. I don't know if everybody else had any agreement or disagreement.

Savage: Just how do you mean that?

Fremont-Smith: I think we were all talking about what Larry Kubie brought up in a way that seemed to me to be intelligible simultaneously to most of the disciplines concerned. I don't mean to say wholly intelligible or wholly agreeable, but we are coming to grips with it.

I should like to remind you that in the earlier conferences these topics came up again and again, but in the earlier conferences they were effectively avoided. So much anxiety was aroused in the group when we began to talk about unconscious phenomena that we really never seriously brought our intellects to bear on the problem. This time I felt relatively little anxiety in the group; we tolerated each other and tolerated the subject before us, however difficult and evanescent it was.

McCulloch: I think we ought to close the meeting with a vote of thanks to the Macy Foundation.

Fremont-Smith: A vote of thanks to you and the rest.

McCulloch: And above all to Frank Fremont-Smith.

APPENDIX I

BODY SYMBOLIZATION AND THE DEVELOPMENT OF LANGUAGE

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PAVLOV HAS proved that no new reflex can be conditioned in a satiated animal, and that therefore all processes of learning depend upon the existence of a state of craving. Approaching the problem from another angle and by another method, psychoanalysis made the same discovery, proving that the acquisition of new knowledge depends upon the existence of a state of instinctual tension and deprivation. Since in infancy and childhood cravings arise in body tensions, it is inevitable that the child's thought world should begin with his body, and that his first concepts must deal with the parts, the products, the needs and the feelings of the body. In order to understand the growth of language one must observe closely what the child wants, what parts of the body become involved in the process of wanting, and ultimately how he learns to speak and think of the different parts of the body and of the desires and feelings associated with them.

These considerations may seem banal; yet they are far-reaching in their significance. Since the child's world begins inevitably with his body, and since the force which instigates the child to expand his knowledge is always the pressure of bodily desires, and since every new fact of experience which enters into psychic life can make its entrance only by relating itself to that which is already present, it follows that every new fact apperceived by the child must somehow relate itself to bodily things. Schematically the process can be represented as follows:

A	A′	A″ ..... etc.
(Body concepts)	(New data of the first order, related to A directly.)	(New data of the second order, related to A′ directly, but to A only through the mediation of A′.)

[Reprinted from *The Psychoanalytic Quarterly*, **3**, *430* (1934)]

If this represents with any degree of fidelity the process of expanding knowledge, it must also represent the process of expanding speech. It means that there is at first a long period in which concepts are vague, broad and overlapping; and that with advancing years these concepts become discrete and distinct. Schematically again one might represent the situation as in Figure 1. Therefore it is not surprising to find that in sleep, in a

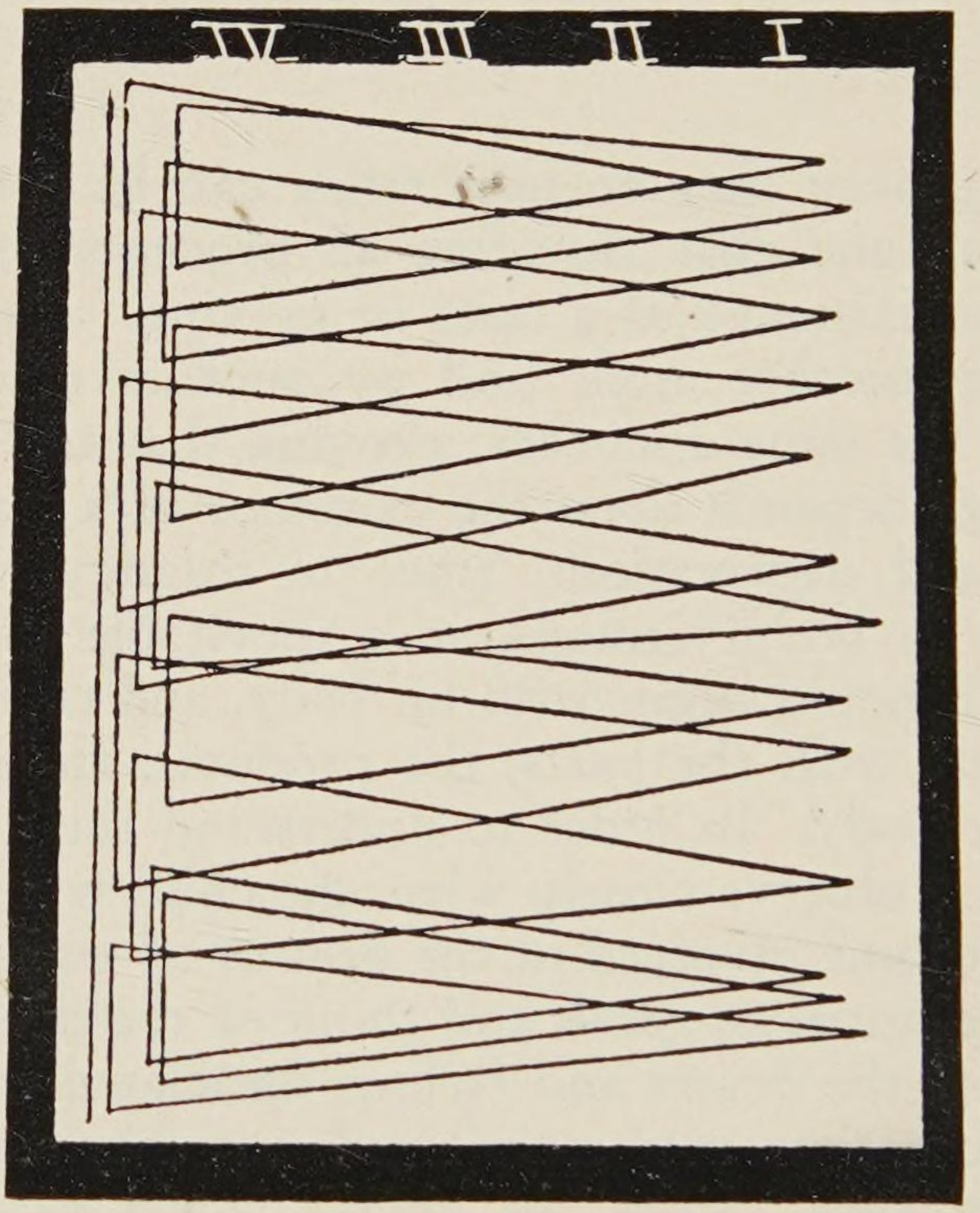


FIGURE 1

state of semidozing, and in delirium, we drop back from our topmost level of development, I, at which all concepts are completely separate one from another, to lower levels of imagery such as II, III, or IV, in which ideas and their related feelings fuse and interact. It is also clear then that some of the energy infusing speech derives not from level I alone, but from deeper, broader, more inclusive meanings.

In this paper I shall present a group of naïve and spontaneous examples of this type of "symbolic" language from children and from patients. There are several reasons for seeking examples of this process in the speech and behavior of very young children.

In the first place, under the conditions of modern education, the young child is becoming more articulate about his fantasies than was true in the past. We hesitate to check a child's naïve fantastic production, we are less heedless of what he is trying to express through these fantasies, and we no longer feel impelled to correct him at once with considerations of reality before making an effort to understand him. (It may be, of course, that modern educational methods err by going too far in this direction. Experience alone will settle that; but in the meantime one may profit by current errors to the extent of studying these childhood fantasies.) There is one further advantage to be derived from beginning with the spontaneous and naïve productions of childhood, namely, that by so doing one subjects to a fairly critical test many of the psychoanalytic interpretations of adult speech, dreams, and symptoms. If in earliest childhood one finds examples of the naïve use of similar symbolizations, where the interpretation is self-evident, and where the possibility of adult suggestion is excluded, it has the value of evidence in support of the psychoanalytic position.

In the following examples we shall meet one surprising fact. It will appear that children, at least in some phases, seem to develop certain individual and characteristic kinds of body-language. One child, for instance, for a time consistently used articles of furniture, familiar household shapes, buildings, etc., as representatives of the body; another child used clothing; a third child used animals. Still a fourth used machinery. We are not yet in a position to say whether this represents different stages of instinctual development, different age periods, the influence of sex, the effects of special interests on the part of the parents, or whether it correlates with different types of personality development, or with different neurotic structures. We can only record the observation tentatively and pose the problem for further investigation.

Let us turn to a very simple example: A little boy of seven, on a picnic in the woods, has a bowel movement which stands up straight on end in the underbrush. Pointing to it with evident amusement and satisfaction, he says, "Look, I made the Chrysler Building." One can share in his little joke without feeling that the analogy is too "far-fetched" or surprising. But then one asks whether such a joke uttered by a naïve child provides any hint as to what may be meant when a man in soberer and politer years dreams of making a tall building. Of course, it must not be overlooked that this is only *one* of the possible meanings of such a

dream element. The same little boy, three years before, had made drawings to represent each day of the week. One day would be a "black slide," another day would be an oven, and so on—each day being represented by some form or shape, mostly suggesting familiar household objects. This is a very complex type of fantasy, evidently related to its source only by a series of intervening steps which in the absence of careful analysis could not be made clear.

Another more complex, and more surprising, bit of symbolic speech and behavior was found in a girl of three and a half, who consistently used little animals as her idiom. The child had been reporting to her nurse and mother repeated dreams of a little mouse that got into her bed and tried to nip her. There had been no evidence of anxiety during the night, no undue restlessness, and no waking in terror as from a nightmare. The child's report, however, was uneasy, as though given half in jest and half in pursuit of reassurance. The little girl had frequently seen her older brother in his bath and had shown a direct interest in his body and in the difference between his genitals and her own. One morning, however, she burst into her parents' bedroom, just as her father emerged naked from the tub. She marched directly up to him and stared at his genital with unembarrassed attention for at least a minute. She then heaved a little sigh, smiled, and looked off thoughtfully into the distance with an arresting expression—which made her father ask, "What are you thinking of?" To this the child replied airily, "Oh, I was just looking at a little mouse." Here it seems an inescapable conclusion that the child was using both the image and the word-symbol for a mouse interchangeably with the word-symbol for a penis (a word which, incidentally, she knew quite well). Of course, the substitution of a little, furry, biting animal for a part of the body has its special significance.

A further and even more surprising example of the naïve use of an animal idiom is given in the following story. The same little girl, at about the same age, is standing on her father's shoulders looking down at him as she faces him and as he looks up at her. Suddenly she crouches, and quite deliberately presses her genital region against his face, so that he has to draw his head back in order not to participate in her little seduction. As he smiles up at her silently she suddenly says, "Would you like to see me naked?" The father parries with the counter question, "Why? Would you like me to?" to which the child replies, "Yes, but not when I'm asleep." Then to the amazement of her father,

she slides swiftly down to the ground and says, "*Come, let's play you're a snake.*" Here, perhaps, one is on more debatable ground; and yet the quick sequence of ideas, speech, and action almost makes one suspect that this three-year-old infant had been reading the text books. First there is the deliberate genital approach to the father; then the fantasy of exhibiting her body; then the faint flurry of anxiety as to what dangerous things might occur if this happened in her sleep; and the final resolution in a game in which the paternal partner of all of this play of instinctual trend is to assume the classical role of a snake.

To these anxiety laden fantasies one finds a sharp contrast in those of a little girl of five, whose habitual idiom was to a rather striking extent a language of clothing. It was characteristic of this child, for instance, that in her effort to solve the problem of the penis, she would put little pieces of chalk inside the trousers of her dolls in order to convert them into boys, and insisted on pinning a safety-pin to the front of her little shirt as a "pretend" substitute for the lacking organ. She called the pin her penis. (At her home psychoanalysis was held in highly critical doubt and any active suggestions to the child were scrupulously avoided.) This child had broken a shoe lace, and took a new lace and a shoe to her father so that he could replace the broken one. The repair completed, she moved reflectively into a room where her mother sat, and said to her, "Daddy took the shoe lace and put it in and out of my little hole, in and out, in and out." Since this was a term which the child ordinarily used to describe her urinary orifice, her mother looked up sharply and said, "What?" To which the child replied, indulgently, "Oh, I meant in and out of the little holes in my shoe."[1] Then she was silent for a moment and breathed a deep sigh, "I wish I were married." Here again one faces a perplexing phenomenon, the use by the child of such "far-fetched" objects as a shoe, a shoe lace, and the holes in the shoe to represent her concept of genital reproductive functions, and a thinly disguised fantasy of intercourse with her father. Again one must confess that how such symbolic representations are laid down, is, up to the present time, a mystery. That it cannot rest, to any very large extent, upon the basis of racial imprints of old experience, is proved by the fact that so often the objects used are shoes, automobiles, airplanes and the like, whose racial history can hardly be said to be a lengthy one.

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[1]Obscene jokes often use this method of simple emphasis upon an innocent word, thus attending to its sexual significance.

The fourth child cited above is the only son of an artist. In the atmosphere and daily life of his home machinery plays a negligible role; yet in the child's fantasies machinery is used rather strikingly to represent the body and its functions. At four this child had never had contact either with newborn babies or newborn animals. He had, however, asked the usual questions as to their origin. He was playing with an automobile jack one day, busily unscrewing all the bolts and taking it apart. His father came upon the scene and remonstrated, saying, "I wouldn't do that if I were you. You'll spoil it." Whereupon the child became quite excited, and protested, "No, Dad, no. I've got to. You see, if I can get way down into this hole, I will find a baby."

A year later the child was in a drawing class, in which the teacher was wont to call on the children for ideas before they sat down to draw. The youngster shouted out, "I've got an idea, Miss X.—It's night, a black, black night. There are two engines—on the same track—with great *big* headlights. They rush at each other and there's a wreck. I am going to draw a train wreck." "All right, Johnnie," said the teacher, "Go ahead." A few minutes later she strolled over to see the picture of the railroad wreck in the black night, and found that the child had drawn *a man and a woman.*

Food can also become for the child a representative of his bodily problems. There is, for instance, the case of the youngster of four who came to discuss a certain matter with her mother. She said that after she had touched her anal orifice, her finger smelled like chocolate. Here it is less surprising that the child had made a connection in her mind between feces and chocolate, on the basis of general appearance, color, suspected consistency and the like, than that the child distorted her real sensory impression and converted the actual odor from the anal orifice to conform to her preconceived expectation, namely, the smell of chocolate. She repressed, or denied to herself what she actually smelled, and in a sense hallucinated the expected smell. On this basis it is not difficult to understand how that child, when a trifle older, a little wiser in the ways of the world and a little more sensitive to the world's disgust, might reverse the process and refuse to eat chocolate because of the relationship which she had built up in her mind between chocolate and feces; and this even if, in the meantime, the connection had been rendered unconscious.

A colleague of mine actually had an opportunity to watch the development of a transient food-phobia in a boy of six. The birth of a baby had led this child to ask insistent questions, in answer

to which he had been told that a child was carried within the mother's body, and that it grew there as a result of the implantation of a "seed." Not very long after this the child was eating fresh peas, to which were attached unusually long points of implantation. After examining these with interest, the child turned to its nurse and said, "What are these?" The nurse replied, "They are seeds." The child looked startled, gulped once or twice, pushed his plate away, and for several months thereafter could not be induced to eat peas at all. An amusing contrast to this is a three-and-a-half-year-old girl, who ate grapes, and with each mouthful said proudly, "I eat the seeds, I like them." After repeating this boastfully several times, the child grew rather reflective, then suddenly turned to her father on whose knee she was sitting and said, "Aren't these the kind of seeds that Aunt Jane uses to make babies?" Here again one faces the child's persistent notion that conception occurs by the ingestion of something through the mouth, although in response to direct questions this had already been specifically denied. Nor again is it difficult to picture this child at a later date refusing food on the basis of conscious or unconscious fears of pregnancy, just as she, at this early and more fearless age, dramatized her desire to have a baby by proudly swallowing the grapeseeds.

These examples may stand alone as evidence that the indirect representation of those parts of the body which are connected with our emotional and vegetative functions occurs exceedingly early in the formation of language in the growing child. It would seem that the sharp focus and definition of concepts and of words is something which develops only later in life; that in the early years concepts, images, words and feelings overlap to an extraordinary extent, and become separated into independent entities only with advancing years. In the schematic diagram already shown (Figure 1), the bases of the triangles represent overlapping conceptual and verbal units with their concurrent emotional charges. With advancing years these units become more and more clearly defined; but in sleep and under stress in the neuroses and psychoses it is easy to see how the less isolated conceptual units of childhood are brought into play. Furthermore they stand as a fringe and background tonus behind all conscious adult thought and feeling—and it is into that well that one dips with the analytic technique of free association.

It is worth while pointing out that from nonanalytical sources somewhat similar conclusions have been forced upon most critical and objective observers. For instance, Piaget, on page 127 of his

book, *The Language and Thought of the Child* (1), describes a phenomenon which he calls "verbal syncretism," and in his description recognizes the use of symbolism in the speech of a child, that is, the use of apparently arbitrary imagery and loose analogy.

Occasionally, it falls to the lot of some observer to be presented with a ready-made experiment. In the observations on very young children this opportunity comes not infrequently. As the years go on, however, with the sharper definition of thought, feeling and concept, the opportunities become rare, and the example which proves the case becomes correspondingly more valuable. Such an opportunity occurred during the illness of a patient in the Henry Phipps Psychiatric Clinic of the Johns Hopkins Hospital in 1921. The patient was a gifted and attractive young woman of eighteen, in some ways unusually naïve, and formally educated according to quite old-fashioned ideas. She had literally never heard the name of Freud. Despite her conventional background, however, she had been subjected to certain very disturbing influences through the fact that her father, an alcoholic, on rare occasions had made erotic advances to his wife in the presence of the children, and through the fact that her brother had manic-depressive spells, in which he was sexually exhibitionistic. The patient's illness was a mild depression, which had been precipitated by her first proposal of marriage. Not long after her admission to the hospital she had a nightmare which was as disturbing to her as it was interesting to the physician. The dream was that she was walking along a narrow street whose high, grey buildings converged at one end. Suddenly an airplane appeared overhead and began to shower her with bombs. She repeated twice that it was a "funny looking airplane." In the airplane was a man whom she later recognized as her brother. In terror she ran up the narrowing street, and finally hid in a box at the end of it—a box which she again characterized as a "funny looking box." The emphasis which she laid upon the peculiar appearance of the airplane and the box led her physician to ask her to draw them. She began by making a long oblong shaft from the upper right-hand corner of the page. This, she said, was the fuselage of the airplane. Then as she looked at it, she said, "Oh, I know what was funny about it—the wings were round," and she proceeded to draw two circles at the upper end of this oblong. Then she added, "Oh, yes, the propeller was here in the rear," and she made a blur of scribbled lines at the rear of the plane. When completed, the drawing was an unmistakable phallus with testicles and pubic hair. The patient's repressive mecha-

nism was so strong, however, that her own drawing excited no comments from her and no recognition. Nothing was said to her except to ask her to draw the box. This she did in the lower left hand corner of the page, in the direction towards which the plane was pointing. She drew a triangular box, then hesitated a moment, and said, "Oh yes, and up here at the base was a funny little bit of a cover that didn't cover the whole box." Again she failed to realize that she had drawn the vaginal orifice with a clitoris. The completed drawing is shown in Figure 2. It is worth stressing that although when she had finished these drawings she still did not realize their nature. When shown the same drawings several months later, without any intervening interpretation, she recognized them at once. Not only had she dreamed of sexual objects in this form, but of sexual practices, as an attack, a showering with bombs (semen?), from which she retreats by going back inside a vagina—the box. Beside this dream one might place that of another patient who dreamed that he was about to take his first flight in an airplane; but just as the machine was about to leave the ground he awoke having a spontaneous seminal emission.

A third patient, a woman physician, while talking in the analytic session, suddenly had an image before her eyes of a penis and testicles—but as she spoke of this, they turned first into a

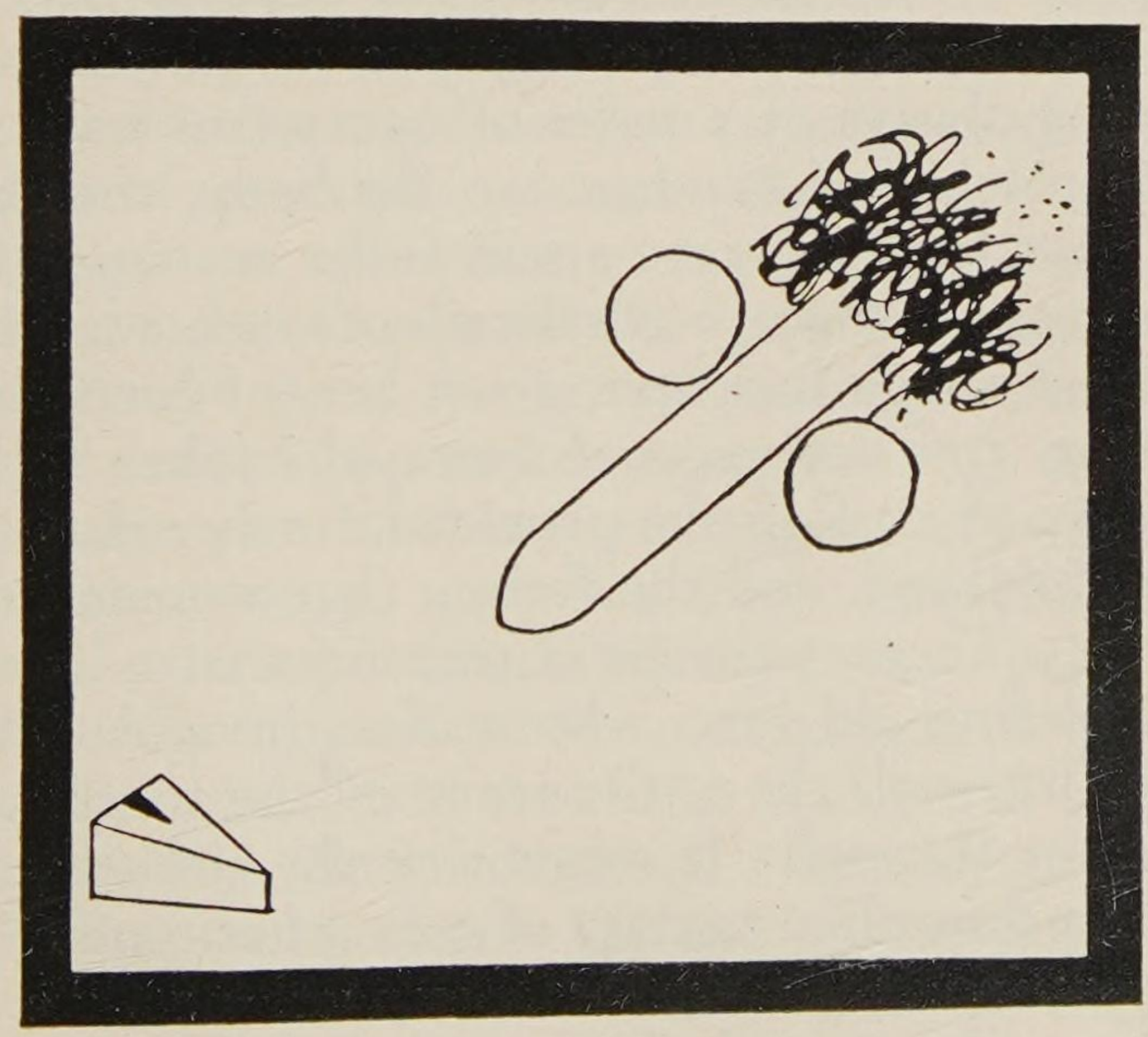


FIGURE 2

cannon on wheels, and then into a large scissors—i.e., the phallus became first a shooting and then a cutting weapon.

The next example concerns a young woman of seventeen, who was first seen in a mild elation, which necessitated her being sent to Bloomingdale Hospital. Before going to the hospital during one of several visits to the office, she rehearsed with rapid, flighty excitement and resentment a whole series of minor injuries to which she had been subjected in childhood—cuts, sprains and bruises, an accident in which her brothers accidentally broke her collar-bone at five, and finally a tonsillectomy at eight or nine. In a final burst of exasperation, she said, "*They took my tonsils out in the worst* PLACE *possible*," and then, correcting herself, said, "I mean in the worst *way* possible." Several months later, after her discharge from the hospital, recovered from her elation but in a mild depressive swing, she returned to the problem of early injuries, referring slowly and thoughtfully to the same series of accidents and operations. This time the meaning of her slip, and the significance which somehow or other had become attached to this tonsillectomy was made clear by the sequence of her thoughts over a series of days of analytic work. In the first place, her earliest years had been filled with an unhappy craving to have been born a boy like her two older brothers. Boys, she said, were her ideals; and as she said this, she described an image of an airplane taking off into the air; and a moment later she visualized a sudden image of a tiny ear of corn. The next day she brought an array of memories of childhood games of playing doctor, the game consisting always of a series of operations with knives, followed by the placing of bandages on the body, the chest and the arms. Two days later she gave a still fuller account of the tonsillectomy at the age of eight, with details of the inadequate warning and preparation which had been given her, of her final paroxysm of blind terror, and her sense of betrayal by her father. In the succeeding days of analysis she provided further data on her sense of injury in childhood, and the feeling that women in general are an injured lot. There were vivid memories of a wounded toad, and of a doddering old man who was an invalid. Then, finally, after another interval, the entire story of the tonsillectomy in its final form came through. It seems that she either had the actual experience or a dreamlike fantasy of seeing her tonsils immediately after the operation. She asked if they were not like little round red balls, about the size of olives, and if they were not like the material which is *underneath* the tongue. She added that she thought she had seen the hand of a man in a white coat holding

these round, red balls like olives, or like scooped-out pieces of watermelon, on a piece of gauze. Linked to it was a memory of her uncle, who was a physician, of how she kicked, of someone holding her legs down, and of terror almost to the point of desperation. Suddenly as she recounted these thoughts she broke off and said, "I just had a very disgusting thought—a disgusting name for a man's organs is balls. I thought of these, too, but I didn't want to mention them." We see, therefore, a long series of minor childhood injuries and accidents built into a fantasy of the damage that women suffer at the hands of men, and culminating in a tonsillectomy which in turn is linked directly to the idea of literal castration. Furthermore it is significant that this idea of castration contained the picture of two round red balls which were thought of as testicles and yet were made from material from *underneath* the tongue, so that the tongue takes its classical place as a representative of the phallus.

Again it is needless to say that one faces a perplexing phenomenon when one tries to understand how in this young woman's mind, totally unschooled or undirected into psychoanalytic ways of thinking or interpretation, such a spontaneous production of castration imagery in response to an experience of a tonsillectomy can have occurred. And yet, that it occurred is evident. (It should be stated that the young woman was undergoing analysis in a mild depressive swing in the hope that the analytic experience might forestall further episodes of elation or depression. The material just presented appeared spontaneously in her free associations during the course of the first two weeks of analysis, before interpretations of any kind had been offered her.)

Not long ago an opportunity occurred to observe a bodily dramatization of the same problem, again in a manner which left no room for doubt as to the interpretation of the phenomenon, although in this case not a word was spoken. It was necessary to perform a lumbar puncture on a young man of twenty-six. Some years earlier this young man had been subjected to the same procedure, and at the time had suffered intense pain. As a result, he was excessively apprehensive, and consented to the lumbar puncture only on condition that it be performed under a general anæsthetic. He was admitted to the Neurological Institute and the lumbar puncture was made under nitrous oxide-oxygen anæsthesia. As the equipment was wheeled into his room his terror mounted visibly, and he began to do a rather peculiar thing: although normally modest, despite the presence of the nurses he made quick impulsive gestures which would repeatedly expose his

genitals. Then, to our amazement, as he began to be affected by the anæsthetic, but before he was completely relaxed, he did exactly the reverse: that is, he reached out for all the bedclothes that he could grasp, for his bathrobe, for a pillow, for anything which his groping hands chanced to touch, and piled a protecting mountain of clothes and bedding over his genitals.

Again the question arises why an attack directed against one part of the body is deflected in the patient's mind in such a way that it apparently represents to him an attack upon his genitalia. Such a deflection seems extraordinarily uneconomical from the point of view of psychological tension and peace of mind. Without entering into a discussion of this problem, it is worth stressing that its solution may lie close to the heart of the problem of anxiety and its genesis.

The phenomenon of "displacement upward from below" is only a special instance of this phenomenon; and it might be better to speak of centrifugal and centripetal displacements, that is, of displacements away from or towards the instinctual zones. That displacement can occur in both directions is clear. The displacement centrifugally is readily explained, because it can be employed so readily to lessen tension and anxiety; but the occurrence of centripetal displacement, with its inevitable increase of anxiety, is a perplexing phenomenon.

SUMMARY

I. The basis for the symbolic representation of the body:—1. The growth of knowledge and the growth of language depend upon states of instinctual tension in the infant and child.—2. This tension is a body function.—3. The first learning therefore concerns itself almost entirely with bodily things, the child learns the parts, the products, the needs and feelings of the body, and so on.—4. All new knowledge must relate itself automatically to that already known.—5. Therefore all new knowledge must have special points of reference to bodily things.—6. Therefore as the outside world is apperceived, each new unit comes to have special significance with relation to various parts of the body,—i.e, representing parts of the body by analogy, at first consciously and later unconsciously.—7. This "body language" is used freely in early childhood, but later in life occurs chiefly in dreams, in dozing states, in delirious reactions, and in symptom productions.

II. The examples given indicate that the indirect or symbolic representation of the body can be classified into two general

types. There is one large group of representative objects which are drawn from the outside world: household objects and buildings, animals, machinery of various kinds, clothing, food, and so on. There is a suggestion that the type of symbol used may be correlated with different types of personality development, or with different neurotic structures. At any rate it is clear that this representation of the body by external objects is linked to the process of projection, and to the externalization of internal problems in the psychoses and psychoneuroses.

The second main form of representation is that in which one part of the body is substituted for another part of the body; and this can occur in either of two directions. Under certain conditions those parts of the body which are relatively slightly involved in any direct expression of instinctual yearnings can be substituted for parts of the body which are more intimately connected with emotional drives. And contrariwise, the opposite can occur; that is, those parts of the body which are directly involved in instinctual need and expression, may be substituted for the more indifferent zones of the body. In other words, the translation can be made either towards the instinctual zone or away from it. It is clear that in this translation of experience and feeling from one part of the body to another, one approaches closely the problem of anxiety, hysteria, and hypochondriasis.

BIBLIOGRAPHY

1. Piaget, J.: *The Language and Thought of the Child.* London: Kegan Paul, Trench, Trubner & Co., Ltd., 1926.

APPENDIX II

REFERENCES

From Dr. Gerard

1. BULLOCK, T. H.: Properties of a single synapse in the stellate ganglion of the squid. *J. Neurophysiol.*, **11,** *343* (1948).
2. LLOYD, D. P. C.: Post-tetanic potentiation of response in monosynaptic reflex pathways of the spinal cord. *J. Gen. Physiol.*, **33,** *147* (1949).

From Dr. Mead

1. SAPIR, E.: *Language, an Introduction to the Study of Speech.* New York: Harcourt Brace (1921).
2. MEAD, M.: Native languages as field work tools. *Amer. Anthro.*, **41,** *189* (1939).
3. BATESON, G.: Pidgin English and cross-cultural communications. *Tr. New York Acad. of Sciences,* Ser. 11, **6,** *137* (1944).
4. MEAD, M.: "Coming of age in Samoa" in *From the South Seas.* New York: Morrow (1939), p. 122.
5. FORTUNE, R. F.: *Arapesh, a Study of Papuan Language.* Publications of Amer. Ethnol. Soc., Vol. XIX, Locust Valley, New York: J. J. Augustin Co., Inc. (1942).
6. MEAD, M.: The Mountain Arapesh, American Museum of Natural History, *Anthro. Papers,* **40,** *237* (1947).
7. BATESON, G., and MEAD, M.: Balinese Character, New York Academy of Sciences (1942), plates 60-62.

From Dr. Werner

1. VON DOMARUS, E.: The specific laws of logic in schizophrenia, in KASANIN, J. S., *Language and Thought in Schizophrenia.* Berkeley: University of California Press (1944), p. 104.
2. ARIETI, S.: Special logic of schizophrenic and other types of autistic thought. *Psychiatry,* **11,** *325* (1948).
3. WERNER, H.: *Comparative Psychology of Mental Development.* rev. ed., Chicago: Follett (1948).
4. WERNER, H.: The test procedure and the results will be published in detail in the forthcoming monograph: Acquisition of Word Meanings. Mono. Soc. Res. Child Devel., Washington, D. C.
5. MCCARTHY, D.: Language development in children, in CARMICHAEL, L., *Manual of Child Psychology,* Boston: Wiley, ch. X (1946).

From Dr. Kubie

1. KUBIE, L. S.: Body symbolization and development of language. *Psychoanalyt. Quart.*, **3,** *430* (1934).
2. SILBERER, H.: Berichte uber eine Methode gewisse symbolische Halluzinations-Erscheinungen hervorzurufen und zu beobachten. *Jahrb. f. psychoanal. u. psychopathol. Forsch.*, **1,** *513* (1909).

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