SOME OF THE PROBLEMS CONCERNING DIGITAL NOTIONS IN THE CENTRAL NERVOUS SYSTEM [11]

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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. | [12]

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 sys-
[13] tem. 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
[14] 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 [15]
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.
[16] 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
[17] 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. | [18]

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
[19] 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* mech-
[20] anism 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. [21]

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
[22] 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
[23] 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. | [24]

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
[25] 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
[26] 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? | [27]

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: It 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.

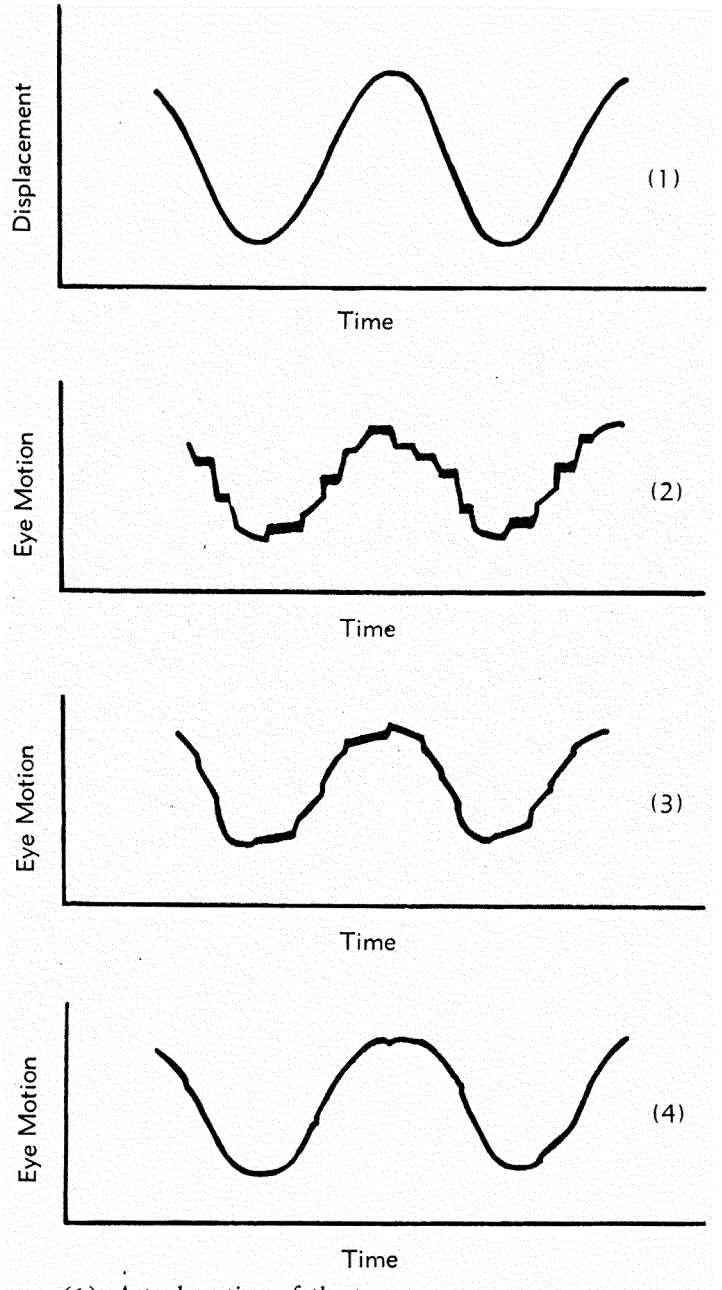
Wiener: I should like to say something that bears directly on this from the standpoint of the engineering problem; I am considering the question of automatic factoring in the automatic factory. Probably the best internal brains we can use for it will be digital models. I would not say purely digital, but what we now call digital computing machines. To work a chemical factory, for instance, we should have separate organ
[28] 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
[29] of the motion of individual electrons in the circuit elements. | [Figure 1] |

[30] 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



(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

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
[31] 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 [32]
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 [33]
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 particular 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
[34] 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 [35] 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 [36] 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.

[37] 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. [38]
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 [39]
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
[40] 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 [41] 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 [42] 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
[43] 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 [44]
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. | [45]

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,
[46] 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 continuum 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 [47]
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. | [48]

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
[49] 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 [50] 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 [51] 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.

[52] 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 [53]
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 [54]
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.

[55] 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[s] 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. | [56]

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 … | [57]

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.