

Communication Patterns in Problem-solving Groups

Alex Bavelas

Department of Economic and Social Science
Massachusetts Institute of Technology

I am going to describe two very simple experiments which are related and which I have chosen because I think they give a good picture of the way we are trying to get acquainted with our problem. I believe they give an idea of the motivation and the spirit of the work. They are not elegant experiments, and the work is in an exploratory stage. We are not striving for niceness of design but, rather, for a maximum of interplay between the experimenter and his material.

I should like to state the problem, describe the experiments, and then tell you about some of the notions from which we are trying to build a theoretical framework for understanding what is happening. The problem is this: If a task is of such a nature that it must be performed by a group rather than by a single individual, communication is usually necessary; but does it make any difference who may communicate with whom? In other words, if I draw circles to indicate people, and draw arrows between people (Figure 1) showing that a message may go from one to the other, does it make any difference, in a group situation, where and in what direction these arrows go? The experiments I shall describe deal entirely with a special set of such patterns which we call “connected groups.” By “connected group” we mean such a group that, taking any pair of individuals, it is possible, over some route, for the individuals that make that pair to exchange messages, i.e., the group shown in Figure 2 would not be a connected group; but the groups shown in Figures 3, 4, and 5 would be connected groups.

Of course, as long as there is an experimenter who imposes a task and who looks at the results later, I suppose we ought to indicate that he is present as a sink and a source. We feel, however, that the experiments were so conducted that so far as the subjects were concerned, they were operating without the experimenter in a sink-source role for them.

If you were a subject in one of the experiments that I shall describe, the first thing that would happen would be that I or some other member of the research group would appear in class about five minutes after the class had begun. I would give you a souped-up story about a research experiment that was being carried on and I would explain that it was supported by the Air Force, that it was quite important, that it had to do with communication, and that we would like some volunteers.

When you arrived at the laboratory, you would be asked to sit down and you would be presented with five cards. On each of those cards would appear five symbols—perhaps a cross, an asterisk, a square, a circle, and a triangle. Each set would be different. They would be so arranged that each symbol would appear on four of the five cards, but one symbol would appear on all five cards. You would be told to look at those five cards and tell as quickly as you could which symbol was common to the five cards. You would be asked to do this for several sets of cards.

Then we would say: “What you have just done is the experimental task. Instead of doing it yourself by being able to see all five cards, the task will be done by five people. You will be separated from the others,

and each of you will have one of the five cards. By writing notes to the others, you will determine as quickly as possible what the common symbol is.” Then you would be led to another room where you and the other four subjects would sit around a table. The table would have partitions built on it so that you couldn’t see the others. Each subject would have a little box full of cards of the same color as the cubicle in which he was sitting. The cards would be numbered in sequence. If you wanted to send a message you would write it on a card and put it into the slot that had the color of its destination. You would be instructed to keep all the messages you received; anything you sent out would have to be on cards of the color assigned to you.

There would be no restriction as to what you might write on the cards. You could write anything you pleased. As soon as you knew the common symbol, you would turn to a little box with six switches on it, each switch labeled with one of the six symbols, and you would press the appropriate switch. You could send the answer out to the others, too. These switches would operate a lightboard in another room on which the responses were counted. A single trial would end as soon as each man had a light on; in other words, it would end when each man had pressed a switch.

Savage: Does each individual know by looking at his own setup whom he can reach, and does he also know, presumably by having it explained to him, whom, the other participants can reach, or does he have to learn through participation this kind of information?

Bavelas: We began these experiments with the notion that we would show each group what the network was. After they had sat their places, they would look at a little diagram which would show the network; but we found that we couldn’t discover a way of doing that without biasing the performance of the group. We decided finally to tell the subjects nothing about the network, so that when each individual sat down at the table all he knew was that there were two slots that were open, that one was colored green and the other blue, and that when he sent a message in that slot it went to somebody designated by blue; but he didn’t know where that person was sitting, whether it was next to him or not.

Fremont-Smith: He has only two slots?

Bavelas: He may have one, two, three, or four, depending on the net, but each man knows what he has. Gradually, they discover what the network is, in some patterns. In other patterns they never quite know.

Kubie: Some men will have one or two slots open, and others will have three or four in the same trial?

Bavelas: They might; the net is imposed in the beginning.

Kubie: But they don’t all have the same number of open slots?

Bavelas: They might or might not.

von Bonin: He doesn’t know who is Red? He doesn’t know with whom Blue can communicate?

Bavelas: Not unless Blue tells him. Blue, of course, may tell him. This is a very interesting thing to watch.

Savage: How can Blue tell, if he doesn’t know where he is himself?

Bavelas: What frequently happens is that a man’s first message to the next man is, “I have a square, and I can send messages only to you.”

Bigelow: Does the curtain go up on everybody at the same moment?

Bavelas: You mean when we “upcover” the table?

Bigelow: Yes.

Bavelas: Yes. After they have taken their places, we uncover the table and say, “Are you ready for trial

No. 1?” They know what the task is, and we have described the table with pictures, and so on, to them, so they know what to do. They go right to work. There are rarely any mixups.

Hutchinson: Is there any effect due to psychological associations with the color names?

Bavelas: We don’t know of any.

Savage: Are the individuals permitted, or even encouraged, to keep notes and diagrams of their own? Does each have a blank sheet of paper?

Bavelas: They have blank paper and a number of pencils.

We chose three patterns of groups to experiment with; I shall report only on these three, although we have done work with some others. We picked one pattern, which is shown in Figure 3, because the topological properties in each place are the same; that is, any differences with respect to some of the things we were measuring and hoped to locate in pattern relations should be zero here. We hoped that it would give us some notion of how big a difference was a real difference. Another pattern chosen is the one shown in Figure 4. The last one is shown in Figure 5.

In discussing the results, I am not going to present numbers; rather, with words, I shall give you a general notion of what seemed to happen, and I shall mention only those things for which the numbers give us some reason to believe that the differences are significant.

First, of all, there is the matter of speed. We measured speed by counting the number of seconds between the beginning of the trial and the end of the trial. The pattern shown in Figure 3 turns out to be quite slow; the pattern shown in Figure 5 is quite fast; and the one shown in Figure 4 falls somewhere in between.

Fremont-Smith: Do you want to give us a rough idea of the duration of a trial?

Bavelas: Yes. Each group does fifteen trials. By the time they have done six, the picture with respect to time is fairly steady; that is, it doesn’t change much after that; in fact, it hardly changes at all. After they have reached this steadiness, the pattern shown in Figure 3 is doing a trial at about 60 to 75 seconds.

Pills: By the sixth trial they would be assumed to know the network. Is that correct? Or does it change? The fifteen trials are all run with the same connectivity?

Bavelas: Yes, and the group is never used twice.

Savage: So one group faces only one connectivity?

Bavelas: Yes.

Savage: And then they are presumed to learn the connectivity in the early trials, so that in the later ones there are typically no messages saying, “I can speak to So-and-So”?

Bavelas: That’s right.

Savage: And is it possible, at least legally possible, for them to form a conspiracy, as it were; that is, to agree on a way of getting the thing over with quickly?

Bavelas: Oh, yes, and of course, they do. They are working against time. They are told that other groups are doing this, that we are trying to find the best group, and that they are to be compared with those other groups.

Pills: Do you find it takes longer to learn one connectivity than another, in the sense of leveling off?

Bavelas: Some are never really well understood. In general, an individual in a net does not know what the net looks like, beyond two transmission links; along the shortest path, of course.

Mead: There is no illegality in giving people information?

Bavelas: No.

Mead: That is important to point out, I think.

Bavelas: There is no information that is barred.

Savage: When you say some are not learned at all, do you mean that, even among these three patterns, there are some that are not learned at all?

Bavelas: Yes. In the pattern shown in Figure 4, the individual at one end doesn't usually know what is at the other end.

Bigelow: Whereas in the pattern shown in Figure 3 he would know?

Bavelas: Yes, and in the pattern shown in Figure 5 he would know. We have tried other nets, but by and large it looks as though more than two links away they really don't know; they guess. If you plot the things they tell you, it is very hard to get a clear picture. The pattern shown in Figure 5 averages 20 to 40 seconds per trial. The pattern shown in Figure 4 lies between the other two. Another thing we tried to measure was errors. If we look at the errors made and average them for the nineteen groups and make a graph, we find that they, too, become remarkably stable after the fifth or sixth trial. The relative differences are the same between the patterns. The pattern shown in Figure 3 makes very many errors. It stabilizes at about 15 per cent. The pattern shown in Figure 5 stabilizes below 1 per cent. It makes very few errors. And the pattern shown in Figure 4 lies between 4 and 5 per cent when it settles down.

Savage: The group is never informed whether its last trials were correct, so it can't learn by experience to avoid errors?

Bavelas: No. The group may infer that it has made an error, but the experimenter never says, "You three made an error," or something of that sort.

Savage: Does the experimenter ever say, "You five made an error"?

Bavelas: No, he never does. But it is possible for them to know that they have made an error from evidence inside the net.

Savage: How could that happen? It would mean talking in the next trial about the last trial, which they presumably don't have time to do.

Bavelas: Well, it happens because an individual may throw his switch but not be able to correct it in time. However, he gets conclusive evidence just a moment later that it was wrong. Another thing in which we were interested was what happens with respect to the emergence of organization. We have looked at that in two ways. First of all, if you plot the frequency with which messages go from one place to another, do you get stability at all? In other words, does an operational pattern emerge? Do you find, for instance, in the pattern shown in Figure 5, that after a few trials messages just go into the center and the answer comes back out? Looking at it that way the pattern shown in Figure 3 never acquires anything you could say was a stable pattern for sending the messages around.

Bigelow: You mean stable within a single group or stable within group to group?

Bavelas: Within a single group. In other words, if you look at any one of the groups which operated in the "circle" pattern, you can't tell from one trial to the next who is likely to send, in which of the two directions he may send, or where the answer will occur first, and so on. What the individuals in these groups tend to do is to send the information they have in both directions as fast as they can, and sooner or later somebody gets all of it.

After the experiment is over, the subjects are interviewed, and one question that is asked them is, "Did your group have a leader?" Now, perhaps one of the most interesting findings as a result of this question is that nobody ever asks, "What do you mean by a leader?" Figure 6 gives the percentage of responses which indicate a leader by position in the pattern.

Fremont-Smith: No special attachment to color?

Bavelas: No.

Savage: It is relative to ego. Is that right? In the first pattern it is really perfectly symmetrical; and when you say there is such-and-such relative percent designated as leaders, they are so designated relative to ego, which is some place on the diagram?

Bavelas: Yes.

Kubie: When you say 8, 12, 4, and 6, you are starting from some one individual viewpoint. You are starting from the viewpoint of 4, 12, 8, or 6?

Bavelas: Well, each person who said there was a leader was asked who it was, and he said, "Red." We tabulate those.

Savage: Oh, you tabulate the color, not the position?

Bavelas: The color, yes.

Fremont-Smith: What happens if he says his own color?

Bavelas: We tabulate that color.

Fremont-Smith: But you make no distinction?

Bavelas: No.

Savage: Should you not, in dealing with that pattern, recognize that it is really perfectly symmetrical (except for such connotations as may be attached to color names) , and that therefore the position of the presumed leader relative to the individual who is speaking that is, the one I have been calling ego is of central importance? This general idea seems to me of primary sociological and psychological interest here, particularly in connection with highly symmetrical patterns.

Bavelas: You mean, we would like to know whether an individual in a given position is more likely to indicate one of the adjoining men as a leader?

Savage: Yes.

Fremont-Smith: Or himself.

Savage: Yes, or himself.

Fremont-Smith: It seems to me it would have a particular significance when he does indicate himself.

Kubie: But he doesn't know who is adjoining.

Bavelas: Of course he doesn't know who is adjoining him.

Fremont-Smith: But he does know himself.

Bavelas: Yes, he does know himself.

Fremont-Smith: Therefore, the man who chooses himself does something.

Bavelas: Oh, I can tell you that nobody chose himself.

Fremont-Smith: Nobody did? Ah!

Bavelas: Nobody did here, no.

Fremont-Smith: That is very interesting.

Savage: But he knows with whom he can communicate, that is, to whom he is, in a topological sense, adjacent.

Kubie: This is a spaceless experiment from the point of view of this subject.

Bavelas: You have a wire but you don't know where it goes.

Savage: But there is adjacency, in a sense. Those with whom you can immediately communicate may properly be called adjacent to you. Does he pick those as his leaders or the next two or—

Bavelas: I am sorry but I can't give you that analysis. I am sure I could dig it out very quickly.

Savage: I suggest that it may be important, especially in so symmetrical a situation.

Bavelas: Yes, I agree with you.

Pitts: Then the percentages you have depend wholly upon color, upon whatever associations a man may have with colors?

Bavelas: Well, I don't know that color really enters into the choice.

Mead: Oh, no.

Bavelas: I regard the differences among the positions in the pattern in Figure 3 as pretty much zero differences in the light of the differences one gets in the other patterns.

Pitts: You don't consider the differences of statistical significance at all?

Bavelas: I wouldn't say that that is important.

Gerard: But those aren't fixed figures. You have just given a random sample there.

Bavelas: They are pretty close to right, I think, for nineteen groups.

Rosenblueth: If they are right, they can only mean color selection. I don't see what else they could mean.

Savage: They could be just at random.

Bavelas: There is a question as to whether this difference really means anything.

Gerard: Put it this way: Are the twelve always in the same color? That is the point.

Bavelas: Of course not. I'm sorry. In tabulating the data, you have this difficulty, that the colors in position don't have any very good reference point unless you start with the ego point for reference. I am sorry I don't have that analysis. Color and position were deliberately—

Bigelow: Permuted?

Bavelas: Yes, permuted.

Savage: But, again, all that can be tabulated, The tabulations should be made with reference to position; thus you would be able to find out such things as that the man in the center frequently refuses to name himself, that those on the periphery invariably name the center. Does the man on one end of the line like configuration see himself as a leader, or does he see no leader? .Is it only the man in the center of that configuration who sees leaders, and so on?

Bavelas: I think the full answer to your question can be obtained. I am sorry I don't have it.

Hutchinson: Is there ever an individual present who is more interested in analyzing than in doing a job for you, who shows curiosity about this system and is a deviant from the standpoint of your experiment?

Bavelas: Well, after the experiment is over and the subjects have been interviewed, we let them sit around and talk if they wish. There is a great deal of curiosity and many ideas as to how the job could be done better.

Hutchinson: But nobody ever holds up the experiment by getting too interested in the formal properties?

Bavelas: This was never true when speed was the main incentive. When we ran other experiments and said, "We don't care about speed, but you must try to do the job in the fewest number of messages possible," the group spent a lot of time in the first two or three trials studying the thing out, and then performed much faster than the groups which were working against time.

To go on, we asked some questions in the interview with respect to things like this: "How well did you like the job you were doing? How good a group do you think this is? What kind of things in your opinion prevented the group from doing better?" In general, based on the answers to these questions, it is quite clear that the "circle" groups were quite happy with the way things were going. They were slow and they made a lot of errors, but they were quite satisfied. They would volunteer to come back and do many more trials. They would often hang around the laboratory to discuss the experiment and to tell us they could do better if they could try it again. In the pattern shown in Figure 5; except for the man in the center, the individuals were either quite aggressive or apathetic about the whole business. The evidence of this is very striking if you look at the messages they were writing. If you take the messages and give them to a number of individuals and say to them, "Can you pick out of this group of messages those which seem to you aggressive or which say derogatory things about other people, or those that are intended to stop the operation of the group?" They have no trouble picking them out. One individual, for instance, sends out information but sends it in Spanish; another tries to get his neighbor to play tick-tack-toe instead of doing the job; another tears up the messages he receives instead of saving them.

Bigelow: Do you ever get any smart-aleck situations? For example, as you were speaking, it occurred to me that if the subjects decided that the idea was to effect the quickest termination of the experiment, they could easily set it up to produce a trivial solution along this line: Send one message to every member of the group, telling everybody to take the first item on the card, then pull the lever for that; then take the second item on the card and pull the lever for that; take the third item on the card and pull the lever for that, and when you get a coincidence, you will ring the bell.

Bavelas: But you can have only one lever.

Bigelow: What I mean is this: Suppose everybody does follow such a procedure. You are going to tell them indirectly when they are right, so that they would be using you for a test device in the experiment.

Savage: But the rules are such that if each participant has thrown one lever, and one has thrown an incorrect lever, the group loses; that is, it is scored "incorrect" on that particular trial.

Bigelow: How about stopping the experiment? You said you stopped the experiment.

Bavelas: Yes, when the trial was over.

Bigelow: Don't they know that the trial is over when they get a correct answer?

Bavelas: No, when five answers have been given, the trial is over. You get the picture: those with more errors are happy, and those with fewer errors are quite unhappy. When you ask them how good a group this is, the individual in the center feels the group wasn't quite as good as it should have been, but the

others think it was all right.

Bigelow: Do you get a great deal more statistical variation in these centrally arranged groups than you do in the behavior of the ring?

Bavelas: That is a very interesting question. All of the error in the pattern shown in Figure 5 was contributed by two groups, which became confused, and apparently never got over it. This leads us to the second experiment—

Birch: Apparently there are two problems in this type of situation, when we begin to deal with the happiness-unhappiness dimension. On superficial examination, it appears that the individuals are happiest when they are (a) either unable to locate a leader in the group situation or (b) where they have a certain degree of probability of assuming that they are determining some kind of leader, whereas in your final situation they have a leader imposed on them, and this leader is imposed on them by the structure of the situation, something which did not arise out of their interrelations but out of your manipulations. Now, given that as the setup, a second point arises: which of these methods would be objectively most efficient in solving a problem? Apparently, from my point of view, I would think that this one central source as a collator would be the most efficient objective method for problem solution, and that people might be happy with such a situation if they were to be permitted to design such a setup for themselves and to select a central individual who would be doing their collating. In that sense they would have a leader who would be their representative and not an individual upon whom they were dependent. I wonder if you began to examine that feature of the relationship? It seems to me that this is somewhat contrived, for that reason.

Pitts: I should think that that could be controlled nicely in one very simple way; namely, one would combine the advantages of symmetry with the advantages with respect to the position of the participants and those of having freedom of communication by considering the case where everybody can communicate with everybody else. In that case the group can select its own leader if it pleases.

Birch: That's right.

Bavelas: We have run perhaps a half a dozen fully connected groups on this program. The result is complete chaos.

Birch: Are they happy?

Bavelas: They may take as much as a half-hour to do the problem. No, they get frustrated, and there is always a battle going on as to which channels should or shouldn't be used. They want to cut down on the number of channels and they don't have any good way of doing it—

Teuber: Before you turn to the second experiment—

Bavelas: Just one comment, first. If you look at the chain, there is no logical reason why all the messages should not go up to the top or why the answer should not come back down. It doesn't take any longer or any more messages than to have them come to the center and back out. As to the degree of imposition of leader, it is not quite as severe as it might appear to be. I know the board has been tilted by the imposition of the network. You are quite correct in that; but it isn't completely determined.

Teuber: Your speed score indicates how soon the experiment is over, doesn't it?

Bavelas: The average of trial times.

Birch: Does that mean successes or determination?

Bavelas: Either way; you get the same relative standing.

Teuber: But that score does not differentiate between the time it takes them to figure out their own group

structure and the time it takes them to arrive at a solution of the problem.

Bavelas: No, except that we know pretty well that the pattern is recognized and known by the second trial in the pattern shown in Figure 5, and by about the sixth or seventh trial in the pattern shown in Figure 4.

Birch: So these messages would be correlated so closely that it wouldn't matter?

Bavelas: No, because here they know the range. The stability in the message-sending doesn't emerge, though.

Gerard: Did you have any data on the abilities of these people, and, if so, did you get any correlation of abilities with what happens?

Bavelas: Ability?

Gerard: Abilities—plural.

Bavelas: Of the individual?

Gerard: Yes.

Bavelas: We don't think that is a factor because the problem is so low-level. We know it becomes a very important factor if the problem involves not only the collection of data with a very transparent operation to get the answer, but complexities as well.

Gerard: I thought it would have made some difference in the speed and the success with which they discovered the actual network. There was no tendency for a particularly able person to become the acknowledged leader, aside from his position?

Bavelas: Not for this kind of problem; there certainly was for more complicated problems.

Richards: There is a new preliteracy test that Dr. P.J. Rulon at the Harvard Graduate School of Education recently brought out that is almost the same as your cards. You might find that if you got down to testing on the preliteracy levels, you could find a relevance between the abilities of these participants and their position. It might be worth looking at.

Bavelas: I am very glad to know that.

Rosenblueth: The emphasis seems to be on the way the group is organized, but then you mentioned that you had tried other problems. Would those figures be very much altered? I would expect that they would be altered, depending on the nature of the problem.

Bavelas: Oh, yes.

Rosenblueth: I mean, you are testing two things. One is the way you organize your group, but the other one is the problem you give them.

Bavelas: Yes. As a matter of fact, it seems to me that the only useful concept here is a kind of task-net concept, and not that of a net which has properties without respect to the task. These things mean something together but not separately. It has been suggested to me that I should ask you to let me present this companion experiment very quickly, very briefly, so I can then get on with what kind of notions we have as to an explanation of what happened.

You remember that the "circle" group was slow, made a great many errors, but was quite happy. The "star" group shown in Figure 5 was a very fast group, with very few errors, but many of the members were unhappy. Now, two other types of things happened, which suggested the next experiment. If you remember, each subject had in his cubicle a box of switches so that he knew that the right answer must be one of those six. On his card he had five symbols, so he really knew it must be one of those five. Since

he was looking for a common symbol, he could just as well send the symbol that was not on his card as send the five that he had. It was just as good a way of getting the answer. But the task was so stated that it was more “natural” to send what one had rather than what one had not. This insight occurred frequently in each of these patterns. It was always acted upon in the “circle” group and never acted on in the “star” group. It never occurred to the individual in the center of this highly centralized star group that this other method could be used, I think, really, because he was too busy. You see, he was getting all the messages coming in, and he had to get the answer out as quickly as possible. At any rate, he did not get the idea. But the idea occurred as frequently in the other people of this group as it did in the people of the circle group. The messages sent in the star group give some rather amusing interchanges. An individual would say, “We ought to send what we have not.” The answer would come back, “We’re doing all right.” The suggestion would come again, and the answer would be, “Forget it,” and so on.

This interested us very much. Another thing that interested us was that in the star group there was a considerable volume of messages suggesting changes or new ways of doing things, new ways of organizing the group. These came from the peripheral members who, you remember, were not very happy with the way things were being done. These suggestions were not accepted by the center man as anything to be acted upon. Also, in one star group where the idea of sending what you have not occurred to two members simultaneously, they began sending this information in spite of, the central man’s resistance, and consequently they so fouled up his calculations in the center that the group made a great many errors.

It occurred to us, therefore, that perhaps we ought to study groups which had been permitted to “stabilize” and whose operation was then in some way disturbed, and to ask: Now, what happens? Is there a difference in the ease with which the different groups will adjust to the new circumstance?

The following experiment was done. All the conditions were like those in the one I described, but instead of showing the subjects a card with five symbols, a little mechanism fed them a box in which there were five marbles. They were colored red, green, white, blue, and so forth. The problem in this case was to find the common marble, since only one color was present in every box—every other color occurring in only four out of the five boxes. In this case, however, when the common color is known, the individual picks up the appropriate marble and drops it into a tube which delivers it to a counting mechanism. You will notice that in this case the error is irretrievable.

Each group in this experiment did thirty trials. The first fifteen trials compared roughly with those of the previous experiment. When the group reached the sixteenth trial they had a “shock.” When they opened the sixteenth box, the five marbles were there, but they were all mottled, a milky, mixed color. If one individual could have seen all five boxes, he could have easily picked the common marble. However, to do this by means of descriptions was another matter. The disturbance might be called “semantic” noise. What happens is interesting.

First of all, the errors per trial for both the circle and the star group increase sharply, and they reach about the same level. The star group goes on making about the same average number of errors throughout the remaining fifteen trials. The circle group comes down very sharply, and by the twentieth trial is doing as well under the new condition as it did under the old conditions.

Kluver: The leaderless group comes down first?

Bavelas: Yes. In the case of the circle group a common language developed; that is, a word is found which fits a marble, and it is used. In the star group no common language seems to develop. What happens in the star group, from what we can tell, is that the messages come in, and the man in the center assumes that the individuals he is working with are unreliable because they are describing the same marble, he feels, with different names. You see, the same marble may be called “ginger ale,” or “amber,” or “light brown,”

and so on. He just guesses at what must be the truth. He goes on, very often unaware of the errors that are being committed.

The feeling of the individuals with respect to the problem or the job remain the same as in the previous experiment. The percentage of leader emergence stands the same. What changes mainly is this picture of errors. If you make a study of the speed with which correct answers are turned out after the “shock,” the circle group is, in fact, faster than the star group.

Now, what I would like to do is to tell you a bit of how we are thinking about this problem theoretically, and here I expect to get some help from you. We started originally by looking at these patterns in terms of their topology. We asked the question, “How many ‘steps’ are required to get from any place to any other place in the same network along the shortest path there is? For the end man on a chain of five people (Figure 4), the number of steps along the shortest path to all the others is $4 + 3 + 2 + 1$. We add it up and say it is ten steps from the end to everyone else. We would do this for each man separately, add it all up, and say: “This chain group has a dispersion of 40, and the circle group has a dispersion of 30.” We felt that that number might bear some relation to the measures we were making of the group’s operation.

We went a little bit further. In the chain group (Figure 4) the distance from the next-to-the-end man to all the others was 7, whereas in the star group the distance from the center man to all others was 4. What we did was to make a fraction—in the first case $40/7$, and in the second case $32/4$ —and arrive at a number for the position which we called its relative centrality. We calculated one more number. If you take the most central position in the network and then take the difference between his centrality and the centrality of X (any other position), that difference we called the peripherality of x.

In other words, we asked this: First of all, how spread out is the group? who is closest to everybody? and how far away is anyone from the one who is closest to everybody? We have worked with a limited number of patterns, and it appears that if you take the peripherality index—how far a given individual is from the person who is closest to all—and plot against this index, the morale measurements we were able to make have a very good correspondence.

McCulloch: What sort of scatter do you get on your experimental results?

Bavelas: It is quite tight.

Pills: There are two possible curves you could plot: morale against distance from the most central person; and morale against the man’s own centrality.

Bavelas: Yes, Now, the correlation is much better with distance from the most central person than with one’s own centrality.

Pills: You mean it scatters much more if you plot it against his own centrality?

Bavelas: Yes.

Pills: The curve is the same in general shape, though?

Bavelas: The curve is the same in general shape, yes.

Savage: You say morale goes down if he is close to the central person?

Bavelas: No. The further away he is in distance from the central person, the lower the morale tends to be.

Savage: And yet in the star-shaped configuration, where everybody is proximal to a central person, morale was low in the periphery?

Bavelas: Yes, but the centrality of the central man in the star is $32/4$. The centrality of the rest is $32/7$. The

difference between these two is big. It is the biggest difference we have in these three groups. In other words, the more you centralize your net, the bigger will be the peripherality difference.

MacKay: Have you plotted any index of morale against the number of messages sent by each individual?

Bavelas: Yes, and that correlates too. And the number of messages sent, of course, correlates highly with where you are in the group. You see, in the chain the end man sends a message and then he has nothing else to do, he merely waits for the answer to come back.

MacKay: I was wondering which was the primary variable, and which might be merely dependent and more or less predictable from it.

Bavelas: I don't know. We have begun to think of the description of these nets in different terms. What we are doing now is something like this: We plot the topology of a net as a matrix. We put 1's where there is a link, and 0's where there is no direct link. Actually, the important numbers here are the 0's, not the 1's, because the 1's tell you that there may be a communication between those people, but the 0's tell you that there will not be. The 0 is a firm figure, but the 1 merely indicates that there is a probability that a message will pass along that link.

We are attempting now to describe the operation of a net in terms of the probability that there will be a message from A to B in a given time period, and in terms of the probability that an item of information at any point in this net will appear at another point in the net within a certain time. If we accept this way of describing a net, then we can play an interesting game, We play it on paper. This is a calculation, not an experiment.

We make these rules: first, that every person in the net will send a message at every moment of time; second, that he will send all he knows every time; third, that if he has more than one channel on which to send, he chooses a channel at random. The probabilities of choosing anyone channel are equal. The last rule is that a person cannot send the same message to the same person twice. If we play out this game on paper, we get almost exactly the operation one gets with an actual group. These rules for operating apparently give the same statistical picture that a group of subjects will.

Savage: Well, there is an ambiguity. If a person is excluded because of this "no repetition" rule, then the probabilities to be assigned to the remainder are the so-called conditional probabilities.

Bavelas: They are equally partitioned for the remainder.

Savage: Well, suppose his probabilities were already a third, a quarter, or whatever the figure may be.

Bavelas: No, they are always equally partitioned in this game we are playing.

Savage: Oh, it isn't the real probabilities that occur in the matrix?

Bavelas: No, this is a game, a game being played on a sheet of paper with a pencil and a random number table.

Savage: I, too, refer to such a game, but a more complicated one. Arbitrary probabilities might be assigned in advance, perhaps in the light of empirical work, but you are referring to the special case in which the probabilities are divided equally among the opportunities.

Bavelas: That's right.

Savage: I see.

Bigelow: Why do you rule out the duplication?

Bavelas: If you don't apply the duplication rule, you get some very nice curves, but they are not the curves

which the subjects give you. If you put this “against redundancy” sort of stipulation in, then you get a curve which is very, very close to what the subjects really give you.

Bigelow: Isn't there an objection to this on philosophical grounds, that you interfere with the randomness and hence put a bias on the results?

Bavelas: Well, perhaps it is not a good way to do it.

Bigelow: I am not saying that. I am only saying that if one tries to think of this in terms of a network in which, at certain points, the process is completely random, then perhaps you should allow the duplication.

Savage: It is not that random, Nobody is so foolish as to say the same thing to the same person twice. Only human beings do that.

Mead: This refers to human beings.

McCulloch: How about children? Might we not get a better fit for very young children if you leave out that requirement?

Bavelas: I don't know.

Pills: When you say that the message must contain all the man knows, does that include the fact that he has sent messages to a given person in the past, of a given sort, and received messages from other people?

Bavelas: No. You see; if he kept every message he received, including the stuff he started out with, in front of him, every message would have all of this on it.

Pills: I see.

Kubie: It seems to me there is one assumption here which you may have to make for mathematical purposes, but which is so contrary to the way in which human beings operate that it troubles me. That is the assumption that everything a human being knows is equally available and accessible to him at any particular moment. You are resting on an assumption which is contrary to fact, as the basis of operation of your experiment.

Bavelas: Well, by “everything he knows,” all I meant was what he knows about these five symbols.

Kubie: Yes, that is what I mean.

Bavelas: These are available to him.

Kubie: Not equally.

Bavelas: All I meant is that they are on a piece of paper in front of him and that he can copy them.

Pills: He might also send a random sample of the bits of information he has in front of him. He might have only one item of information exchanged at every stage.

Bavelas: Yes. If we take a group in which there are no restrictions placed upon linkage and which sends at each moment the very best message possible to exactly the right place, then the average amount of information across the entire net from moment to moment is given by a curve which is described by $\log_2 n$, “n” being the number of nodes.

Using the rules I first gave you and playing the game, you can, for any given pattern, find who gets the five items first, on the average. And when this person gets those five items, you can discover how much information is present at the other positions. This profile correlates very well with the emergence of the recognized leader and with morale. So this leads us to ask whether, for a group that is working in this laboratory situation, and who have a very specific task to perform, saying that So-and-So is a leader really

means that he is a person from whom information which can lead you to successful completion of the task regularly comes. In other words, the man recognized as leader is the man at the position where information accumulates most rapidly.

This line of thinking leads us to make the following general theory. An individual at any time has certain hypotheses that are very important to him, and some that are less important. He is interested in optimizing the probabilities that these hypotheses are correct—for instance that he is a successful person or that he is a loved person or that he is a good person. He does this primarily through information from other people. The sources of such information are very important to him.

Imagine a research group that has been working on a program, and imagine that they are very discouraged and that they feel they are failing. Now we observe the following phenomenon: a man walks in; a smile and a word from him and the picture changes. They don't feel so discouraged. They feel that they may succeed after all. What I would say has happened is that an information process has occurred by which the probabilities that certain hypotheses are correct have changed, and that—

Kubie: Why information probabilities?

Bavelas: Because I would like to define a change in probability as being an indication of an information process occurring.

Kubie: That is a circular assumption.

Bavelas: Yes, of course.

Kubie: But because this is a circular conference doesn't mean that all circular assumptions are valid.

Bavelas: Let me try to explain what I am about in this way: If I set up a situation in which a person must try to pick the one of eight boxes which has a \$10 bill in it, and the distribution of choices tends to be rectilinear, then I would say that he isn't getting any information with respect to this problem. But if I have a person look through a window at him and make faces, and this event changes the distribution, I say that, by definition, information has been received, and that I should be able to calculate how much was received. Now, I am beginning to think that what we call social needs are hypotheses, culture-given hypotheses. The individual who accepts the culture tries to make the probabilities of correctness of these hypotheses as good as he can. Many of the things that happen in a group process depend on how this information is being transmitted and received, whether an individual is below a tolerable level of uncertainty concerning these hypotheses. I can tolerate considerable uncertainty with respect to whether or not my dessert will be chocolate ice cream or vanilla because this hypothesis is not as important to me as some others; but I am not willing to accept that level of uncertainty for other things. The uncertainty concerning hypotheses of social relations between myself and others can be changed mainly—if I am a normal person—by information from others. In other words, an individual tries somehow to make the probability that certain hypotheses are correct as high as possible. Some of these cannot be made higher than a certain amount, but if they are high enough he behaves as though they were really so. I cross the street as though it were certain that I would reach the other side. I know that it is not certain; but when the uncertainty is great enough then I begin to behave “queerly.”

I am suggesting, therefore, that a good deal of the behavior one observes in organizations is not apart from the communication network that obtains in the organization. A person may well be in a situation where he gets no information about important hypotheses, although the leaders of the organization may feel they are giving him a great deal of information. The pamphlets written by the boss or by a personnel expert are distributed to the workers with their pay checks. These pamphlets, found in the gutter outside the plant, contain information—probably from the boss to himself—that is not of much value to the workers.

Now, I think I have talked enough to give you some feeling as to the direction in which we are trying to go theoretically, and I would be interested to hear what you think about all this.

Gerard: I would like to go back to your second experiment, if I may, and ask you two questions about it. Did you try the mottled marbles with the group without the preliminary runs on the clean-cut situation?

Bavelas: That is being done now. When a group starts with the mottled marbles, and then goes from the mottled marbles to the solid colored marbles, these marbles now may appear as very, very different. He is looking for really minute differences, which always exist, such as bubbles in the marbles or slight differences in size. He may, in effect, find he is alerted to search for differences.

Gerard: You are answering a somewhat different question from what I had in mind. Was the performance of the different types of networks with the mottled marbles used from the start the same as that when the mottled ones followed the solid-colored ones?

Bavelas: The difference is not as great, but they are in the same relative position.

Gerard: In other words, even without a predisposition, the central man does badly?

Bavelas: Yes.

Gerard: Is the variation between groups greater with the mottled marble problem as a function of the central man? In other words, I am again asking about this factor of ability .

Bavelas: Personal ability in the level of problems we have used does not seem to enter as a factor.

Gerard: Did some of your star groups do perfectly with the mottled marbles, for example?

Bavelas: None did.

Gerard: Never?

Bavelas: That's right.

Savage: I should like to call attention to a fact brought out by your later discussion; namely, that it is essential to your experiment that the individuals communicate serially. When one writes a note, he can't write it with carbon paper to everybody he is in a position to write to, but must take quite a bit of time writing to one after another. Though some real-life situations may be subject to such a limitation, the situation is often quite different. Consider, for example, the number of people to whom I can simultaneously transmit information simply by talking out loud. I suppose that an experiment parallel to yours, but permitting such simultaneous communication, might be rather dull, for in such a situation it would be sensible for a person simply to keep everyone with whom he could directly communicate up to date on all information which reached him, and the actual experiment might very well simulate such ideal behavior. Though this situation could not be expected to lead to so interesting a set of experiments as those on which you have been reporting, it may be a model for as many, or possibly even more, real-life situations.

While I have the floor, I should like to say that there is a chapter in modern statistics which I think is very intimately associated with communication within groups. I don't want to tell it now, but I do want to ask that if ten or fifteen minutes can be found at this conference, I be permitted to discuss this topic.

McCulloch: Can we hold that, then, until we come to a place where it might fit?

Savage: Any time you like. I am just asking now, while I think of it, for time to say it.

McCulloch: Right. I should like to hear from Shannon and from MacKay, if they will, as to the extent to which the notion of information, as you have used it, Bavelas, is the same as it is in their scheme of things, Will you take it, Shannon?

Shannon: Well, I don't see too close a connection between the notion of information as we use it in communication engineering and what you are doing here. I have a feeling that the problem here is not so much finding the best encoding of the symbols, because you do not have a limited amount of paper to write on, which corresponds to the engineering problem of encoding messages to use the smallest amount of channel, but, rather, the determination of the semantic question of what to send and to whom to send it. I don't see quite how you measure any of these things in terms of channel capacity, bits, and so on. I think you are in somewhat higher levels semantically than we who are dealing with the straight communication problem.

Bavelas: May I just give an example of how one of my students is trying to measure this? What he says he wants to do is this: He says that the group starts out with a probability of one-fifth, that anyone of the five symbols each individual has is the right one. Now, at the end of "X" minutes the experiment is halted, and he says to a participant, "Now, of these five symbols, which can you, with certainty, cross off the list?" He hopes in this way to get some indication of how much information has really been received.

Shannon: There was one remark you made which intrigued me a bit. You relate information to the change in probabilities. I gather by that you mean subjective probabilities, the probabilities a person estimates for such-an-such an event whether or not there is any valid reason for his estimate. From the communication point of view, the subjective probabilities do not enter at all. I wonder if we shouldn't somehow distinguish between valid information and information that the man thinks he has and acts on, perhaps, but which isn't based on any logical reasoning.

Bavelas: Of course, I am biased in this direction, because I think the change in the subjective probabilities is the thing that is worth talking about, if you are thinking about people at all. You know the demonstration that is frequently done in classrooms. You come to class and put a deck of cards on the table. You put a \$10 bill within the deck of cards and say, "Now, gentlemen, you are going to line up." Let's assume we have at least 52 people. "Walk by and take a card off the deck. You take the one that is on top when you get there. Now, will you indicate in a secret ballot which position you want in this line?" You get a distribution. Of course, nobody wants to be first or last. Most people want to be between 15th and 20th in line. Then you spend as much time as is necessary in proving to them that it makes no difference where they stand in line. After they are "convinced," you ask for another secret ballot, telling them that this ballot will determine where each one will be in the line. Well, how much do you think you will have shifted the distribution?

Savage: I don't think that is a fair example. If you had taken an example in which the information imparted had practical consequences, I would have been impressed. But here, you see, it has none. Suppose you argue yourself blue in the face that position in the line doesn't matter, then I say to myself: "Yes, perhaps it doesn't matter, but, after all, the guy might be wrong. If he's right, it follows that there is no harm in my playing my hunch; if he is wrong, I might much better play it." If you take a situation where the information has a practical consequence—for example, you could arrange one where the first place is actually best, but looks worst—then you will arouse a real conflict in the subject after instruction. He will say, "Gee, the guy says I should go first. It looks as though I should go last. Maybe he's got an argument." In such a situation you might shift somebody, but when you tell me that my prejudices are simply irrelevant, that they are superstition, and so on, then I might as well follow them, because it doesn't cost a thing to have the best both of your world and of mine.

Kluver: I have a question of some practical importance. Some years ago I came across a French book in the field of personnel research. After several hundred pages the author concluded that a man running an army or an industrial empire should never communicate or deal with more than five or six people. Any attempt to have personal contacts with more than six people would turn out to be a disaster either for army

or or industry. I wonder whether your work is ultimately going to throw light on such practical problems.

Rioch: Along the same line, Dr. Bavelas stated at a recent seminar that the size of the groups under study was limited to five, since with six or more the number of possible configurations became unwieldy. It occurred to me later that in team sports we subdivide teams of more than five members into three, four, or five subgroups; also, in other group activities we similarly deal with a small number of subgroups.

Gerard: What happens when you lecture to a class?

Rioch: You deal with just one, then. You are dealing with a class.

Gerard: You call that an individual, not a subgroup?

Rioch: When lecturing I find I usually pick out one, two, or three people in the class and talk to them, treating the rest of the class as a unit.

Gerard: I am not convinced.

MacKay: I wonder if I could come back to Bavelas's concept of information, because I don't agree with you, Shannon, that it is essentially different from yours. I think what is different is the type of ensemble with respect to which you measure it. Suppose we define "selective" information, which is the concept used by Shannon in communication theory, as that of which the function is to specify a selective operation: that is to say, you think of yourself, the receiver, as having prepared a filing cabinet of possibilities, and the receipt of information enables you to make a selection from it or, at any rate, to narrow down the probability-distribution over the possible commands that you have to give to your own selective mechanism. Then, I think Bavelas's results are a consistent illustration of this process, particularly when he introduces symbols differing from one another by nonsignificant characteristics, and then goes back to the original symbols. When the nonsignificant characteristics appear, their effect is to enlarge the space of possibilities out of which the receipt of information enables you, the receiver, to select. Originally, you started with a space with just five possibilities in it, or six possibilities; later, you had to widen your space of possibilities to include such things as mottling, and so on. Then, when you come back to the original symbols, the selective operation you are performing at first is still one on the larger space, until you learn that certain dimensions of it are "no longer being used," so to speak. I agree with Bavelas that it is the subjective probabilities that determine the ensemble from which we estimate amount of selective information here, but, with that proviso, I think it is the same concept as Shannon's. In communication you ask: "In the ensemble of expected messages, how rare is this one which has been selected?" In this problem, we ask: "In the ensemble of expected responses, or expected commands to respond, how rare is the response that I have made?"

Shannon: I would certainly agree that information is being transmitted here. What I was trying to say was that I don't feel that this group of people will tend necessarily to the most efficient way of doing it. This makes it difficult to apply any of the results of information theory. I would expect that they might settle—in fact, some of your experiments indicate that they do—on a means of communication other than the most efficient. For instance, they do very poorly on the connected graph, which surely should be better from the straight communication point of view, if it weren't for psychological factors.

MacKay: But I thought our question was whether, when Bavelas talks about "information," he means what you and I do. I think if we use the word "selective" to distinguish it, then we are all three talking about selective information.

Bavelas: I must admit that the concept I tried to describe is very attractive to me, but it always leads to a very puzzling conclusions. If I may do so, I will cite one.

The following experiment was done by some students. Five individuals were placed in separate cubicles without linkage one to another. They had no communication among themselves in terms of direct linkage. All the subjects could see was an indicator. They were told that the experimenter would cause a number to appear here, and that it would be some number from 0 to 25. All could see the number. Each of them was to write on a slip of paper a number from 0 to 5. The experimenter would collect the contributions and add them up. Then the sum of their contributions would appear, and they would hear him say: "Your target was 17. Your summation was 15. Try again." They also wrote down why they picked the number.

An interesting result here is that when the subjects are thus given the size and the sign of the error, they don't do as well as a group which is merely told, "You were wrong; try again." Certainly, it looks as though you are giving them more information when you tell them the size and the direction of the error; but if you look at the "real" array from which the choices are made, the array expands when you give them this additional "information." Without the size and sign of the error, the subjective array from which they select is smaller. Now, in the first variation, are they really being given more information?

Bigelow: No matter how much time they have, that is true?

Bavelas: There is no time limit at all.

Bigelow: Another point of the same sort: In the previous case, where you were discussing the precision of the process, it isn't clear to me under what circumstances the people were allowed to take as much time as they needed. For example, in the case where you had the ring, were they allowed to take as much time as they needed with mottled marbles? Also, were the people with the central situation ever given conditions where they could take as much time as they needed in order to build up the nomenclature?

Bavelas: No. In those experiments the instructions were: "Get it right. Get the right one as quickly as possible."

Bigelow: But has anything been done to explore what happens when you eliminated the necessity to do it quickly?

Bavelas: Only with the five symbols; in that case, I told you what the facts were. But you see in this last example what this may mean. The linkage has something to do with how much information the group can use. When the group was told only that they were wrong, they were selecting from a limited array—let us suppose the target was 17. A subject would reason, "If I put in 3 and the others do, too, it will make 15." Some subjects—most of them—put in 3 or 4. An occasional 2 was contributed, but 0, 1, and 5 were never selected. When they are told the size of the error and the direction of the error, however, the reasoning may go this way: "We're too low. We should go up. But everybody will think of that; therefore I should put in less than I did last time. But everybody will think of that, so maybe I should put in 5 to overcorrect." So you can see that the range from which choices are made has been increased.

Bigelow: No matter how long you give them? It is amazing that they don't realize that they should take the desired result "n" and divide it by 5, and come closer to it? There is no tendency to do that? If a person is above the "nth" portion, and the result is still too low, he certainly ought to stay where he is and let the other converge. Did they never learn that point?

Bavelas: But this is what they do. If the target is 17, they know there are five of them.

Teuber: Do you finally get a correct result?

Bavelas: Oh, yes.

Teuber: After how many trials?

Bavelas: Oh, the number of trials tends to get shorter; in other words, they do learn something. But it

might be three or four trials.

Teuber: Only three or four?

von Bonin: Suppose they all put in 5. All right, it will be 25. The experimenter says, “You have 25 instead of 17?”

Bavelas: Yes.

von Bonin: Do they undercorrect then? Do they all go down?

Bavelas: The difficulty is, you never get 25 if the target is 17.

von Bonin: Just by way of example—

Bavelas: What happens, very quickly, under either condition, is that some individuals, although they have information as to what the others are doing, begin to structure the group. They say, “My job is to do so-and-so, and somebody else is doing that.” They get very complicated hypotheses about how the group operates. They assume a role and give other people roles, almost as though they couldn’t stand the situation if it didn’t have some social structure.

Bigelow: Does anybody take the position that they should stay constant to allow others to adjust the small difference?

Bavelas: That is what some of them do. A subject may put in 3 ten times in a row, regardless of what the summation is. He won’t change. Even though the target is 16, and the sum comes out to be 15 five times in a row, he won’t change. He will say that his job is to stay constant because somebody else will adjust. Some subjects take the opposite attitude. They keep trying to compensate for what the others are doing.

MacKay: May I return to the question as to whether or not the error feedback procedure is giving information? The whole thing is completely consistent with the information picture. The general point is that the specification of more possibilities enlarges the space. That increases the number of bits necessary to perform a given selective operation, and so you would expect that your variance would go up if what you are dealing with is some kind of random process which is being modulated, as it were, in its probability peaks by the information that is coming in. I don’t see any paradox. What is happening, if you like, is that the information that is coming in is “structural” in its effect. It is “structural information;” that is to say, its effect is to increase the number of dimensions of the information space. Then, of course, you have a much larger number of possibilities, so you would expect the normal flux of information to have a proportionately smaller selective effect.

Savage: I don’t see it at all. You say if I tell people their error, that should naturally make them do worse than before, and you are not a bit surprised?

MacKay: Yes.

Bavelas: There is no linkage here, you see.

MacKay: Let’s put it this way: By naming something, you raise in the man’s mind a complex array of possibilities corresponding to what you have named. If you name the range of errors, you modify the structure of the filing cabinet from which the almost random mental process is making its selections, and it is not in the least surprising that the result is to increase the spread of the total. It is evidence, if you like, of the often ignored distinction between human thinking and human logic.

Gerard: Isn’t it much like what you said about the difficulty of selecting the clear-colored marbles after they have worked with the mottled ones, that they are looking for more difficulties?

Bavelas: Yes, the space has been enlarged.

Kubie: Because we so often fool ourselves that we are basing our choices on purely external criteria when in reality we are choosing in terms of our own unconscious processes, the probability of error increases as we go from random choice to unclear ad hoc hypotheses. This was proved in the selection of pilots for naval aviation before the last war. Attempts were made first to select pilots on the basis of a variety of theoretical assumptions, but it was found that choosing men at random worked at least as well as did these initial attempts to guide the choices in accordance with some pet theories.

Next I want to record my disagreement with Dr. Bavelas's first statement, that this is not an "elegant experiment." On the contrary, I think it is both beautiful and elegant. I derived from it the same pleasure that I get out of any precise and adroit and clear intellectual or physical activity.

Bavelas: Well, I can easily demonstrate that it is not precise.

Kubie: That may be. However, apart from either its beauty or its precision there are elements in it that bother me. In the first place, in addition to the point which I have just raised about the shift from random choice to ad hoc hypotheses, we must include among possible sources of error one which is so disturbing that I hate even to consider it. Even if it should turn out to be true, I frankly do not want to believe it. Nevertheless, it must be kept in mind. This is the issue of parapsychology, and whether any extrasensory perceptions exercise an influence from one individual to another in this experimental situation. Actually, it seemed to me that the experiment is so designed that it might even throw light on this problem.

Bavelas: Would this effect cease if you give them information on the size and direction of the errors?

Kubie: Yes, because then each individual becomes a restrictive individual when he tries to fit something to his individual hypothesis, whereas if he is working at random choice, if there is such a thing as parapsychological influence—which I hope there isn't, because life is complicated enough as it is—then we give him an opportunity, when all five are working at random, which he wouldn't have when he is obeying his little secret contrived hypothesis. I think an issue comes up in relation to the other one of these experiments.

Bavelas: In other words, we should measure the departure of the results of this group—without error information—from those derived from the tossing of five six-sided dies.

Kubie: Correct. My next point, of course, concerns the meaning of "meaningless." This is most uncertain. It has come up repeatedly in connection with color. We are all aware that color exerts emotional influences and has other subtler significance. But these emblems (for example, the asterisk and the quadrilateral figures) are also not devoid of meaning. In fact, this is demonstrated by the data presented here. I do not know to what extent these unconscious meanings influence the material; but the possibility must be kept in mind, in terms of your conclusions. Certainly, they will have different meanings to different people, to different sexes, and at different ages.

Bavelas: The thing is whether the effect we are trying to look at is overwhelmingly stronger.

Savage: This is one of those points of elegance you mentioned earlier. In so far as you have statistical sophistication about the experiment, you will remove the significance of color and of the emblems by suitable randomness and balancing. You have already indicated that some such precautions were taken, particularly with respect to color. With ingenuity and with circumstances at all favorable, you should be able to abolish utterly any of these effects. Strictly speaking, they cannot be really abolished, because they continue to be facts of life, but you can see to it that they cancel out, so that no apparently systematic results of the experiment, depend, for example, on the fact that some particular emblem is phallic, or anything like that.

Mead: That is fine for the experiment, but I think the important thing here is that we are going to take these experiments and use them in planning for organization in real life. In a sense, once we have been elegant and taken them all out of the experiment, the next elegant thing will be to put them in in such a way that we can learn something about them.

Savage: But you put in the ones in which you are interested. You will never be interested in real life in whatever phallic significance an asterisk may have.

Mead: I disagree. Have you ever had proofreaders on your hands?

Savage: If I say “never,” I mean “hardly ever.” Particular symbols for emblems which have been chosen for the experiment under discussion are largely accidental so far as any wish to explore the deep psychological significance of shapes is concerned, though that subject is also worthy of experimentation. I therefore reiterate that in this experiment you will want to remove the psychological effects of shapes by randomness and balance in design.

Kubie: May I put in just one point, to make it cohesive?

McCulloch: Go ahead.

Kubie: Keep in mind the meaningfulness of the whole experimental situation, that is, of the cubicle. To some people the cubicle would be of claustrophobic import; to others it would not be.

Kluver: In your experiments, Dr. Bavelas, the items with respect to which communication occurs are such items as circles, triangles, asterisks, marbles, and so forth; that is, items easily named and specified in English. But suppose you deal with such an item as “an irregularly shaped, wet, smooth object changing rhythmically in brightness.” perceptually, this is something very simple, since such an object is easily identified and recognized; to designate it unambiguously in English, however, requires telling a rather long story. Items with respect to which communication may occur often differ in “word-nearness” or “word-remoteness,” that is, in linguistic distance. I wonder, therefore, whether you have any data on how the mechanism of communication is influenced by systematically changing the linguistic distance of the various items entering the communication network.

Bavelas: We have done nothing on that point.

Rosenblueth: But there should be one thing you are trying to measure, to learn or to study with this experiment. I think that it is an extremely elegant experiment, that you correlate certain things, and that you get very nice correlations. But you don’t know what you are after. It seems to me that you have given a little too much importance to the notion of information in your judgment of the experiments. Solving the problem is not strictly a matter of conveying information. Here, the nature of the problem is obviously very important for the results that you get, from the data you gave us. You get quite opposite results with the different groups when you give them different problems, because you have introduced different things to solve. I can’t quite make out what this is giving you a measure of.

Bavelas: The question we have been trying to answer is simply this: Does it make a difference if you have groups that must communicate within the constraints of given patterns when all of the patterns under consideration are equally logical with respect to their adequacy for solving a task? Does the pattern itself have any psychological effects which cannot be explained on the basis of anything else but the fact that this pattern was imposed to start with? And does this imposition generate other psychological effects? The answer to these questions is very likely yes.

Rosenblueth: Given a specific task, yes, but what about different tasks? How can you generalize, and how can you evaluate what those different tasks have given you when they lead to quite opposite results?

Bavelas: That, we don't know. We know this, however, that if we change the nature of the task in such a way that certain individuals possess more information regarding that task, then the effects we get here can be explained only on the basis of where the information is at a given time. You see, what we have done here is to guarantee, because of the nature of the experimental task and the topology of the net, that information will accumulate at certain places more rapidly than at others. When you change the task, one thing that changes is the original disposition of relevant information. But again you can say, "What matters here is where is the greatest amount of relevant information and what is the path over which it must travel to get to the others."

McCulloch: Before we go on, there are now four people whose names I have on the fire for questions. I think Pitts's question should come first.

Pitts: It is purely a numerical one. In the case where information of error is provided, in the second experiment, you said that the groups, after a time, improved somewhat. How much was that? That is, in the case where: you have both the error and the target specified.

Bavelas: I can't give you exact numbers. There are two ways of looking at it. One is, how many tries—

Pills: That's what I mean.

Bavelas: —and the other is, what is the larger error?

Pills: I mean how many tries.

Bavelas: It tends to go down. How much, I don't know, but there is a general decrease of the number of tries on the average.

Pills: Does it ever fall below the number of tries taken by the group with only the target and no additional information about their own errors supplied to them?

Bavelas: Not consistently.

Pills: Never?

Bavelas: No.

Savage: That group also learns. There is learning in both cases; is that right?

Bavelas: Yes.

Teuber: I was trying to ask Dr. Rosenblueth's question; let me try it once more: In all these situations you have two things. Your group has to communicate; that is, they have to talk to each other, and they have to solve a problem. Now, to rephrase our question: What difference does it make if their problem is merely to talk to each other and to understand what they are saying, without using this communication process to solve a problem imposed upon them by the experimenter? I suppose you know the experiment that George Miller did with the Harvard articulation series (1). He has a triad, a group of three people, hooked up in different ways by telephone. With one hookup any person can talk to every other person, and they can reply to one another; with another hookup, they have to talk around the triangle, but cannot reverse the flow of communication, and things of that sort. He comes out, I believe, with results that are somewhat different from the situation in which the problem would be not just to understand what the message is, but to solve a problem on the basis of messages.

Bavelas: Yes, but, you see, it isn't so different, really, is it?

Teuber: That is what I am asking.

Bavelas: In an articulation test I think you have a list of words. Something comes over the wire, and that

is supposed to help you make a proper or correct choice. In those terms I don't think the problem is very different. This group also has a task which consists of selecting an item out of an array, and the means are signals from others.

Teuber: But isn't the outcome different?

Bavelas: Not very.

Teuber: The same connectivities, the same effects?

Bavelas: In general. He finds in that situation that the three or four patterns that he used cannot be distinguished until noise is introduced, and then they are distinguishable in their results.

Rioch: I was wondering, in this last experiment you described, if that had any relation at all to the class demonstration attributed to Raymond Pearl. He asked his class in statistics to guess the length of a line drawn on the blackboard, and showed that the average of the guesses was always closer to the "measured" length of the line than anyone person's guess.

Savage: I don't think that has any real relation at all, because it does not really seem to refer to psychology. If the line had been measured by an instrument such that human error could be considered negligible compared with instrumental error, the results would have been the same. We are dealing here with the phenomenon of statistical stability, the tendency of averages to be close to their expected values.

Bigelow: I should like to rub in a few points very hard that occasionally seem to have been missed in this group. We are a group of very mixed backgrounds, as everybody knows; and I should like to say that to me this experiment which has been described seems particularly elegant and has in it a drastic lesson for all of us, outside of the particular form in which it appears. The lesson, I think, is that in approaching a very complex field, what one does is to take a very simple model, which is almost always picked purely on an intuitive basis. The intuition may be good or bad, depending upon elements that nobody can judge, so far as I know. Then you set the model up, look at it a while, and sometimes you come to some concrete, although narrow and specific, facts. Now, these may constitute the most useful part of the experiment, rather than what you originally intended the experiment to suggest. That is sometimes useful in a different way, but the most definite and concrete part of the experiment may be the fact that it does or does not show information on one specific point.

Now, one of the questions which was directed to the speaker, I think, concerned the manner in which this experiment is related to the real world. Suppose there are affect values and relationships to the symbols, and so on. I think that it ought to be understood clearly that such questions are really irrelevant; that in making this measurement, the person who is making it is presumably not trying to say, "This is a model of the real world," when nobody can make a model of the real world. If you try to make a model which contains enough to describe what all of us feel when we see these cards, it is obviously impossible. But lo and behold, this, for example, tells us that human beings under certain circumstances are worse than the most incredibly bad computing machine you could possibly imagine. They cannot add 2 and 2, so to speak, under certain circumstances of duress. This is a concrete fact and, as far as I am concerned, it is a new one.

I think that the people in the social sciences, the people in the more difficult sciences, ought to realize the tremendously valuable lesson uncovering facts of this sort. It is from these that people in the cleaner and easier sciences have constructed fairly substantial theoretical buildings. To worry whether this is a matter of the real world is fine and has a purpose; but this experiment has shown some facts, and they are the really sound part of it. The facts are that people under certain circumstances apparently cannot do simple computation. The circumstances enormously affect their ability. This is, furthermore, a very good

illustration of the difference between what one thinks of as a computing mechanism and what one thinks of as a neurological process that goes on. The neurological processes are very well known to be much more elegant in some respects than a computing machine. On the other hand, they have these shortcomings of breakdown during duress. It is a very concrete and very definite point.

Pills: I have a suggestion which I think opens up all sorts of interesting possibilities. So far, in these experiments, you have imposed a common purpose on the group in the sense that the members of it are all trying to discover which is the common symbol on their list, and then transmit that information to all the other members. Now, suppose you change it in this way: offer a prize to the person who first discovers the correct answer in rank or order. Then you have all sorts of possibilities of coalitions forming against given people, and so forth. This situation has the great advantage that there exists a mathematical instrument for analyzing it.

Savage: It doesn't analyze it at all.

Bavelas: May I give an example of something in our own venture close to the game theory. We set up what we thought was, and what I still think is, an example of the classical duel situation. This is set up for two people; in order to get the "bugs" out of the machine, we intend it to be a game between two groups. Two individuals sit on opposite sides of a table. They are separated by a panel on which there is a big green light, which flashes on certain occasions which I shall explain in a moment. In addition, there are sixteen small bulbs across the panel. Under each bulb is a button which can be pushed. An electrode is attached to each individual's arm. The setting is the same for both subjects. At the start of the experiment, the subject is told that one of these sixteen buttons is the correct one, and if, he pushes the correct button he will shock the other person and also trip a relay so he himself can't be shocked. At the beginning of the experiment all the lights are on. Every five seconds, one of the lights goes off; therefore, if he waits long enough, he will know which button is the correct one; but the other fellow's chances are getting better, too. If one subject presses the wrong button, the big green light flashes, and the other subject knows he has missed.

We ran a series of trials under the condition that each man would have only two "shots"; we ran others with four "shots." We let each pair play long enough under one condition so that they really came very, very close to doing what the mathematical analysis gives as the best time to press the button. This interesting thing happens, however; if they have four shots to fire, the first shot—and we placed particular importance on that one for our measurement—is fired at the seventh or the eighth light, that is, when there are seven or eight lights remaining. If, however, you start this game, the same game, not with sixteen lights but with ten lights, they don't fire at seven or eight; they fire later, although, of course, the probabilities are the same when there are seven or eight lights left!

Pitts: Oh, yes. I would say the game theory in relation to a situation of that kind is roughly analogous to information theory in relation to your other case; namely, where, in both cases, the theory provides maxima or theoretical optima, without, of course, providing any mechanism for analyzing cases where people don't behave in that way; but if you do apply a rank order for the people who arrive at the correct answer first, you have a very interesting combination or conflict of motivation with information, since, of course, information from the other people is necessary for anyone to have the answer. I think that might possibly be important, while still retaining the great advantages of a controlled system where the communication can be analyzed very carefully.

Bavelas: It would be interesting, as a matter of fact, to set up groups in which, if everyone in the group gets the answer, there is a certain payoff to everyone, but with provision for an individual to get a private reward even if the group fails.

Pitts: Yes, make the individual find the correct answer himself first, which he must do, of course, by means of information from the other people.

Rioch: I want to say just one thing about this concept of motivation. I don't think we have any basis for using the term, because there is no differentiation made between "motivation" which is increased alertness and "motivation" which is deterioration of adequacy. We speak about "motivation" when we have a physical feeling, but such somatic feelings may represent anyone of several personal operations. There is at the present time no consistent operational definition of the term "motivation."

Bigelow: Can you tell us what the increase of adequacy is?

Rioch: If a person is faced with a difficult decision, for example, differentiating shades of color with the expectation of punishment, for example, electric shock, in case of error he may respond with being "alerted" or he may respond with "anxiety"—Kurt Goldstein's catastrophic reaction. The physical sensation of tension may be quite similar. Our concepts of motivation are clouded because they are not adequately differentiated from the conscious sensations or feelings which accompany the act.

Pitts: I expect that when an experimental psychologist uses "motivation," he is in essence defining it by the experimental arrangement he sets up to produce it in a given way, such as the \$10 bill or electric shock. He means the external situation which he has imposed, not the individual's reaction to it.

Rioch: But he doesn't measure it. That is just the point. He doesn't use methods which are operationally directed towards investigating this problem.

Bigelow: He tends to eliminate the abstraction "motivation" by utilizing the end result, by measuring the end result.

Pitts: Certainly, the experimental psychologist in general simply provides what he has reason to suppose is a desirable prospect or the chance of avoiding an undesirable one, and then simply defines motivation as a state produced in his subject by that, without considering that it may vary considerably, depending upon his other conditions.

Rioch: Yes, having defined his desirable and undesirable by what he thinks about it, not by what the rat thinks about it.

Bigelow: Aren't there plenty of examples of learning as a result of hunger?

Birch: Oh, yes.

Bigelow: Aren't these perfectly good continuous variable measurements in the sense that he is—

Birch: No, no.

Bigelow: To some extent?

Rioch: No.

Bigelow: To a considerable extent?

Rioch: No.

Birch: Well, the problem is a rather difficult one. Let us take some of the systematic attempts to study the quantitative variation in motivational states in animal learning or in animal performance—the Columbia studies in the early thirties, for example, that were done under the aegis of Warden, in Warden's laboratory (2) (3). Under those conditions you tried to compare such things as the relative strengths of maternal drive, sex drive, hunger need, and so forth, in the frequency and in the rate of the animal crossing of a grid, let us say. Warden found that he could get certain curves that would be related to the presumed intensity

of drive; actually, however, he was not discussing drive but deprivation, which is something else again. He was discussing what he was doing to the animal rather than the effect of what he was doing upon the animal, so that a number of problems arise.

For example, if you deprive an animal of water, what happens to its sensitivity to shock, independently of how many times it crosses the grid? In other words, the problem of sensory threshold and sensory threshold changes under conditions of deprivation as derived or as an ancillary effect of the condition of deprivation have never been studied; what has been assumed is that once you have deprived the animal of something, you have automatically a direct relation between this deprivation and the amount of motivation which it itself has. It does not follow. Therefore, you get a quantitative relationship between two abstractions that you have developed, rather than a quantitative relationship between two processes that may be occurring for the animal.

Rioch: As a matter of fact, what we need is a definition of motivation in terms of information theory, because the whole concept of motivation is in terms of energy mechanics.

Bigelow: You mean, there are no ways to eliminate secondary effects upon the animal itself, of any of the usual incentives, when varied quantitatively?

McCulloch: That is the awkward thing—

Bigelow: Is that true, sir?

McCulloch: —that motivations to and from turn out not to be added or subtracted but entirely haywire in relation.

Bigelow: But you might suspect that, for example, one might take the height a rat will jump over a fence one or two or four hours after food to get to a given target—

McCulloch: Well, in three hours, the rat is practically dead. The rat must eat oftener than that.

Bigelow: But there are no ways to eliminate these secondary effects in experiments?

Birch: You get peculiar reversals, too, which apparently indicate that what is happening is that a different qualitative process is becoming involved rather than that a nice homogeneous continuum is being expanded. For example, in one study of my own on the effect of differential food deprivation on the problems of behavior in chimpanzees, you find that you get a greater predicted effect for six-hour deprived groups than you do for the twelve-hour deprived groups. This was a function not of the time of deprivation but of the time of day that it was possible to introduce such a period of deprivation and still have the animals work. The twelve-hour period could be introduced only overnight, because the animals would not work at certain other times, in accordance with their own rhythms. They had a life history in which they had not been fed during the night, anyhow. Therefore, under those conditions, you had a noncontinuous relationship between the periods of deprivation and the effects of these removals upon the animals.

Gerard: I am sorry, but all this seems to me irrelevant to Bigelow's question, as the whole discussion is irrelevant to the paper. What you gentlemen are saying is that poor experiments have been done, not that the experiments can't be done properly, which is what he means.

McCulloch: I think they can't be done properly, and I am going to say it very simply. Under absolutely standardized conditions, you can have organisms that prefer A to B and B to C and C to A. The consequence is that the strength of motivation with respect to the three items is multidimensional and cannot be measured by anyone quantity. I think that some time we had better sit down with the problem of motivation, because it is a tremendously complicated one.

Gerard: I will agree with that, but that is still not the point Bigelow is making—whether you cannot get

graded, instead of Yes and No answers. I think these experiments are potentially designed to give graded answers.

Bigelow: It is not a question of whether they are easily separable but rather whether they are separable—

Rioch: They are separable on the basis of the frame of reference in which you are looking at them.

Bigelow: This is my question—

McCulloch: Will you hold it one second. There is a question of Shannon's still on the floor.

Shannon: Well, the remark was about the theory-of-games possibility. It seems to me that all of the experiments you did could be interpreted as games. The chief trouble is that they are not zero-sum two-person games, which is the simple case. Most of them are non-zero sum, because co-operative effort gains for everybody; also, they require more than two persons. This type of game is much more complicated in the sense that there can be several strategies, several systems of imputations among the various players. I think that perhaps what you are seeing emerge in some of these experiments are those patterns of imputations, particularly in the experiment where people were trying to match a given sum, and where one person would take on a certain role and another person a different role. The various ways that these roles could be assumed correspond to different good strategies for the game.

I also think that the poorer results you obtained when they were given the amount of error were really an indication of the irrationality of people, because in a game situation the additional information certainly could not hurt you if you were perfectly rational; that is, if you choose the best strategy, additional information can only make it better from the point of view of reducing errors, if you play the game by the most rational means. If you had five Von Neumanns sitting in your cubicles, the answer should be better or at least as good with the additional information.

Bavelas: You mean if there were five Von Neumanns playing the game, No. 1 would know what No. 2 would do?

Bigelow: Not by identity.

Shannon: Each one would choose the best strategy for this game, and if that involved making use of the additional information, that could only help them. If it didn't they would completely ignore it.

McCulloch: There is one question of Savage's. I believe, still on games. Do you want to bring it in now?

Savage: Yes. I do. When Pitts alluded to the possibility of doing a modified Bavelas experiment, which would be a many person zero sum game, as a check on Von Neumann's theory of such games (4), I tried to speak then and there. I wanted to say that so far as I have been able to understand. Von Neumann's theory simply doesn't make testable predictions about many person games. Though a lot of mathematical machinery is constructed in this connection, Von Neumann, neither in writing nor in conversation, seems to me to make at all clear what empirical consequences this machinery may suggest. The situation is totally different from that for two-person games, about which Von Neumann's writing suggests quite definite consequences. Thus, though such an experimenter as Bavelas can and does test the consequences of the two-person zero-sum theory. I think he would not even know what to look for in the case of many-person games.

While I am on the subject. I should also like to say a word about the experiment Bavelas did do, as a game. Shannon has said, and I think at least partly correctly, that it could be considered as an example of a non-zero-sum game; but as such it would not fall under Von Neumann's definition, because there are artificial constraints on the communication of players. Von Neumann would presumably prefer to consider it as a one-person game, in much the same way as he considers ordinary bridge as a two-person game,

neither player of which knows the whole of his own mind-is schizophrenic, so to speak.

The really interesting aspect of the experiment, though, is its departure from the Von Neumann concept. Thus, the team is not permitted to get together before the whole experiment, armed with foreknowledge about its general nature. In contrast, suppose five of us, having heard Bavelas's lecture, were to go up to the Massachusetts Institute of Technology on the train to participate in this experiment. We would sit in the parlor car and discuss how we should work, what codes and shorthands we should use, and in general make agreements as to how to behave in the face of the various contingencies which might arise. We might then go up there and beat the pants off Bavelas. But the experiment as it is actually done carefully precludes all such prearrangements.

Therefore, interesting and invigorating though Von Neumann's theory is in many contexts, it seems here to have been dragged across our path in two different directions as a red herring.

Bigelow: If time urgency were eliminated, though, wouldn't this be possible? If they were allowed all the time they wanted to devise a strategy, then this communication with pieces of paper would do that.

Savage: Then you might say that early parts of the game should not be considered as a game; they should be considered in the light of the prearrangements made on the hypothetical trip up to MIT.

Bigelow: There, I agree with you.

Savage: Where there is a long breaking-in period, the participants could begin playing the Von Neumann game, which, as I have said, ought to be considered as a one-person one. But, of course, a one-person Von Neumann game is rather dull.

Pitts: That is exactly the reason why I suggested introducing the conflict of motivations, because, in effect, it does become a real game on the assumption that everybody knows the structure, which you could easily make, but has a motive different from the others.

Savage: But that's a poor game.

Pitts: I know; it's terrible. In the first place, you can say that the theory of games naturally cannot make any empirical predictions as to what any group of people playing a game will actually do. Being mathematical theory, you can't have any empirical consequences directly; but it does provide certain extreme possibilities which you can list; then you can inquire whether the actual behavior of the group shows a tendency to approximate those, and, so to speak, provide you with reference points and a mode of characterizing particular sorts of situations.

Gerard: I want to go back to a comment that Bigelow made. He stated that, to him, the most important consequence of this research was the demonstration that human beings under certain conditions calculated less well than the most primitive calculating machine. It worried me at the time and is still worrying me, and I am going to ask Bavelas if he agrees with that statement. The reason, it seems to me, why it is not a correct interpretation is that when one starts with a rational series or set of operating conditions, as in the paper-solving of these problems, one finds that the subjects do precisely the same thing that happens when the problem is being solved in reality. In other words, I wonder whether it isn't a matter of the people doing the best that is possible with the information available at any time? Would you accept the conclusion that Bigelow drew, that human beings are worse than the calculating machine?

Bavelas: In these circumstances I would, because in the experiment where you have the individuals in a circle, it can be demonstrated that the problem can be done in just three transmissions; but we've never had a group do it even under specific requests to attempt it.

Savage: But six machines can't do it in three transmission movements; one machine could. It doesn't

seem to me the individuals here are guilty of miscalculation. They are guilty of international warfare or something like that; that is, they are guilty of not getting together into the best combines and subteams.

Rosenblueth: Given the same instructions that you gave your five people, I suspect five machines would not do it any better.

Bigelow: I certainly do not. I mean, if you give them more information than they need, the machines will certainly not do worse as a result; but the people did.

Rosenblueth: I mean, give them the same information.

Bigelow: This is a case where excessive information would produce a negative result.

Rosenblueth: Excessive information of the wrong sort.

MacKay: It seems to me that this is a two-level problem, and that is why we are arguing. There is, first, the problem of formulation. You see, these people are locked in, and the problem is formulated only in vague terms, and they've got to be able to develop for themselves a model of their situation in terms of which they may draw their deductions. Second, there is the question of drawing the deduction. If you put a man in the situation in which the problem is so formulated that the question is as simple as, "What is 2 and 2?"—if that is simple—then he will make his deductions as reliably as the computing machine. But the first problem here is essentially one of abstracting a structure from a pattern of experience, isn't it? and any child is better equipped than a normal computer for that job. It is possible that a suitable machine could be made to do better than a human being at both tasks, but it must meet a criterion of performance different from that of deductive ability. If the mechanism operating here is some kind of random search process in the mind or the mechanism of the individuals concerned, then we should make comparisons not with the ideal logical mechanism to deduce logical sequences, but rather with the ideal random mechanism to induce structure.

Bigelow: Well, it certainly starts off with a random input in the sense that you never know to whom you are going to talk. But beyond that, the question would be whether these people, on the basis of information that they have at any given time, make choices which are less than optimum. Each individual mayor may not make optimum choices with the information he has. Now, I would guess that the evidence here is strongly that they do, and that an individual computing machine, given any information—they may start out with random search and then increase the information in a certain flow pattern which is not exactly the same in each case—would make inferences from these which are far more reliable than those of human beings.

MacKay: Yes, I think the importance of Shannon's point is that we should ask ourselves with what type of mechanism it is fair to compare a human performer. In attempting to make sense of Bavelas's results, where the subject has to decide whether to fire on the fifth or the seventh light, our analysis will be quite different according to whether we assume the performer to be designed as a logical calculator or as a mechanism adapted to the kinds of situation that confront a human being. If we ask whether a human being is as good as a machine, or whether an action is "unreasonable," the answer will depend on which data and assumptions we want our ideal mechanism to use. Psychologically, it appears that the subject treats the array of lights as a uniform assembly and picks something near the middle as a typical point in it. He is presumably using a random mechanism whose chief law is, "Thou shalt show no bias."

Bigelow: Surely, but that is the point, that here is a striking example where the human being is not behaving like a computing mechanism.

MacKay: I am sorry, but I think it is a case in which the human being is behaving, certainly not like a normal computing mechanism, but like a random-search-computing artefact, if you like.

Bigelow: Fine, but then it is not a computing mechanism in the ordinary sense.

MacKay: Not like the conventional type of computer, no; but not necessarily inferior (or superior) to it. It has to be ready for a wide range of quite different tasks.

Pitts: The computing machine can certainly be designed to carry out processes of random search in a way which minimizes the number of expected steps to acquire the desired information, and certainly such a computing machine would do better than a group of human beings. It could not do any worse.

MacKay: Yes. The question is, first, whether, in the present problem, there is anything paradoxical, and, second, whether we are justified in comparing the performance of the human operator in this situation with that of an optimum computing machine, which might not have nearly as good an all-around performance.

Gerard: We are all talking about simple computing machines, not rather complicated ones.

Fremont-Smith: It is evident how intrigued we are with the problem that comes up again and again: whether the computing machine would do better or worse than the human being. I think that the degree to which that intrigues us is one of the most interesting things.

Savage: That used to be our title subject. We used to be a seminar or meeting on the subject of computing machines and we naturally do revert to the theme of the analogy between computing machines and human and social behavior, because at one time, at any rate, it was one of our most important theses, that we might find something fruitful in that analogy—not that the machines would be like people, because they were made by people—after all, we don't expect a neon lamp to be like a person because it is made by a person—rather, we had at that time some hope, of some of us had some hope, and it was widely advertised, that computing machines would be like people because they did so many human things. It seemed to us worth while to examine these analogies, especially to discover, if ever we could, limitations to the analogy .

We have had, throughout the existence of the group, the important problem of seeing if there is anything that people do, that can be precisely stated, that may not be done by a machine. We have always known that that hope was in some sense chimerical because there is the famous theorem of Turing, which we used to hear about at every meeting, to the effect that if you could only tell precisely what you want, you could make a machine that would do it. And yet we have continued to hope in spite of this theorem; which in a sense should end all our hopes that we might discover some kind of behavior which does not deserve to be called mechanical. This has not prevented us from simultaneously hoping the opposite, so we have repeatedly sought mechanical analogues to the various aspects of human behavior. But it is by no subconscious accident that we revert to a theme which, as I say, used to be incorporated in the very title of our group.

Rioch: It is interesting to note the historical concepts of this. Hanns Sachs wrote a paper, "Why the Delay in Civilization," making the point that the machine operationally represents a magnified part of the human being, and that the building of a machine is copying the human being over a narrow range of the human capacities but multiplying the human capacities in that particular narrow range. He described the tremendous resistance to making machines previous to the time of the Renaissance. This anxiety is represented in all sorts of way, both in terms of the danger of copying the human body and in terms of the danger of dissecting or experimenting with the human body. The Koran, for example, forbids the copying of the human body, and a good Mohammedan has no portraits.

In another very interesting scientific revolution, which occurred at the end of the nineteenth century, we have the curious situation of the machine coming between the observer and the phenomenon. The manometer comes between the physiologist and the blood pressure, and the camera comes between the

astronomer and the star; along with that, we get a great deal of anxiety about the machine. The exact verbalized form that it takes is different from that which it took in the Renaissance, but the general problem is still there. However, I think it would be a horrible comment upon the people who build computing machines if they couldn't build one which, over a narrow range, was better than the human being over the same limited range.

Pitts: That is perfectly true in principle. I think we have made a wrong comparison, really, one which is unfair to the human being. We have compared the human being to a machine which is designed to perform a particular purpose, and naturally the machine will perform that purpose as well as it is possible in principle to do so. We really should make an analogy between a machine designed to perform a particular purpose and one designed for a purpose different from the one we are considering.

Teuber: You could rephrase that and say that the machine might do better in any individual situation, but an organism of the complexity of man might still answer with relative adequacy to a much wider range of situations than any presently constructed machine. I think Bavelas's concern was not so much that, but the question whether we can use these particular models, at this point, as a measure of rationality, or as a measure of optimum performance against which we can measure the actual performance of the group. Or, if the group tends towards optimum performance, how can we use these mechanisms and mathematical theories in predicting how a group will act under certain simplified conditions? I think that is a somewhat different approach.

Gerard: Along the same lines, it seems to me that, although I may not recognize a good deal of unconscious anxiety as to whether we are or are not better than our machines (which may lead to a preoccupation with it), the conscious basis for preoccupation is with the extent to which we can get useful clues from studying the machines, as to how our brains or our social groups work; and—

Teuber: Exactly.

Gerard: —that is not a matter of saying, “It is,” or, “It isn't,” but of saying, “In what respects is it?”

Mead: May I make one point more? Dr. Bigelow made the point about the experiment that is limited and simple but gives us some clues on the relation between the natural and the social sciences. I think we had a very nice illustration of both uses here. When Bavelas produced the patterns, Lawrence Kubie showed points that might be relevant. Now, it is perfectly true that you can take those out; you can set up an experiment and randomize them so that they are not relevant. Part of what you said, Dr. Savage, was fine, that you should have different experiments to study these deep psychological relevances as a step to doing something else. You also said, “Who cares about an asterisk?” I believe. Weren't you the one who said that?

Savage: I did, and I said I was misinterpreted.

Mead: That is the point which I think is dangerous. We have to distinguish here between the fact that you can do experiments, you can simplify them, you can make nice tight experiments, and you can use information about the unconscious to be sure that you have randomized the right elements; but if you carry that over into real life, with a statement that a thing like an asterisk is of very little importance, and then make an estimate about the amount of error that you will get in proof readers in a year in the world today, with the amount of printing that there is, you would make an enormous error. We keep vacillating between the two.

Bigelow: The danger point is where you make use of such a specific conclusion from an experiment.

Mead: But that is one of the difficulties that comes between the groups, that when people introduce “real-life” they are only introducing “real-life” to make the experiment tighter; say, you had better randomize

this, or you had better take this into account, but don't generalize from the experiment in such a way as to say, "Who cares about an asterisk?"

Savage: Let me defend myself about the asterisk, if I may. I said what you said I did, and you criticized it rightly. What I should like to have said is this: Who will pretend that the six symbols which happen to enter into this experiment are the best six to study with respect to their psychological import? The six are accidental and, in that sense, no one cares about them, although, of course, one or more of them might turn out to be important in some psychological context.