

## **Master's Series on Field Research**

A series of interviews with major figures in field research conducted in the early 1980s  
by Peter Blanck

### **Transcript of an interview with Freed Bales**



Burton Blatt Institute  
SYRACUSE UNIVERSITY

**Peter Blanck:** I thought we'd start with--just to get rolling-- a general description of some of the critical questions you faced in doing field research: what do you think good field research is, and what kind of field research-- we'll build into--how that developed SYMLOG, so, you're on.

**Freed Bales:** Yes. Well, I fell into field research, I suppose, in so far as I got there at all, without any education that might have helped in any way. Early in my graduate career, I formed an interest in small groups. And I was interested in the effect of context on social behavior, and, in particular, the effect of membership in a group, and the effects of social interaction. In fact, I wrote a masters thesis on the concept situation, which was current then in the field theory of Lewin, but also was persisting in sociology from earlier work of the Chicago school, Thomas and Szene, Szenesky. And so I wanted to do some kind of research that got into the field and dealt with the effects of context realistically.

Almost by accident, I got interested in Alcoholics Anonymous, and so, without any real preparation in field work, I located the local group, and obtained permission to attend the meetings and became a kind of friend of the group. And got along famously for quite a while; meeting with the members afterward for coffee, and going home immediately and writing up everything I could remember as to what people said and did. That was the important part of it. But, I had the idea that I wanted to observe interaction in the group. And, as you know, an Alcoholics Anonymous meeting is a little bit of a religious exercise, and I proved to be a little obtrusive taking down my little notes on my little piece of paper. And the steering committee sooner or later called me into the back room and said "Look, you're making the members nervous with your observations, and would you mind stopping." So I said, "Of course." But, I saw at that point that I was not going to be able to study what I really was interested in, which was the interaction of members. I thought that somehow the interaction of members with each other was important in helping to change attitudes to deal with the addiction, in short. That was the reason for my interest in it. And so that was a major frustration, in a way; I mishandled the situation. I was so eager to take my observations that I ruined the opportunity. So, I suppose that left a kind of traumatic gap in my thinking, and I tried I guess, I've been trying for the rest of my career, really--to work out methods of observation which are practical in the field and are under adverse conditions.

So, when I got back from that field trip, as it were, and considered what to do next; I decided to write my thesis on something else. And eventually I came to Harvard as an Assistant Professor after I had obtained my degree, and gone away for a while. And Sam Stouffer, the head of the laboratory at that time--and from the University of Chicago, by the way-- said "What do you want to do, your research?" And I thought, and said "Well, I'd like a room, an observation room, where I can study groups." So he said "All right, we'll build that." So they did build the first observation room on the third floor of Emerson Hall, and I started to observe groups. Actually, prior to the finishing of that room, I observed a clinical conference group. It was Bruner, Smith, White, Shelley Korchin, and a number of other psychologists, at the time, who were studying personality and political opinion at the old Harvard Psychological Clinic on Plympton

Street. And, so I sat in on those meetings, again I'm afraid, with a rather obtrusive piece of apparatus. I knew that I wanted to take observations in time sequence, so I had Henry Gerbrans, our shop man, build me a machine that would pull a paper tape along. The tape--since I needed many, many categories, and didn't know exactly which ones I wanted, actually, I started with around eighty or ninety--it was an eighteen inch wide paper tape, and it pulled this paper tape along beside the list of categories, so that I could write down who was behaving in what way, toward whom, and when. I also thought I needed a record of each minute and a reminder as to when important non-verbal behavior occurred. So, Henry Gerbrans fixed a noisy device, which clanged down on the paper every three minutes, I think, and put a marker across it.

I've always wondered, ever since that time, how that group managed to put up with that obtrusive noise [laughter]. George Homans and I were sitting at the other end of the room. George Homans was interested in studying the same group, and he was getting his start in actual research at that time also. So we were making a complete sound recording of everything also, for his purposes. So, we observed there together for about a year. And that was the period during which I began the development of the categories, which later turned out to be interactional process analysis. But, when the observation room was completed in Emerson Hall, sometime later, I began the observation of groups there. The studies I made there were not really experimental studies. They were more, I would say, controlled observational studies. These were not groups in the field; in fact, I have never really studied groups in the field. But, I studied such factors as: the effects of group size; and type of problem; and so on, which involved free observation. I shouldn't say free observation, I should say controlled observation using interactional process analysis to study freely interacting groups working, of a given size, or working on a given kind of task. So they were more or less naturalistic.

**Peter Blanck:** Okay, we're about at interaction process analysis, so, if you want to give us some background on that?

**Freed Bales:** Yes. Well, the version of observation that I had worked out for studying the interaction in Alcoholics' Anonymous was really the first cut at interaction process analysis. And, it was a notation of a series of kinds of group roles, as I had it conceived then, built partly on earlier sociological work, Thrasher's study of the gang and others. And he talked about different kinds of roles in the boys' gangs in Chicago. And I had the idea, "Gee, there must be certain kinds of roles that regularly appear in informal groups," and so I was attempting to test that idea, or refine that idea. And, I felt that somehow the role of the sponsor and the role of the alcoholic, who he took under his sponsorship, was psychologically important in dealing with the addiction. So, I really carried on that same line of reasoning when I started to observe the group at the Harvard Psychological Clinic. But, I beefed it up with descriptions of a lot more kinds of behavior; it was not centered around group roles as such, but types of social behavior.

I was also inspired in part by the work of Durrkeim and other sociologists, who were getting, who had gotten good results from studying the rates of occurrence of social phenomena. Durrkeim studied suicide, for example, in that fashion and in the early days

of sociology, that was something we all studied, Durkheim's famous work on suicide, and differential rates. I thought, well, why can't we do that for small groups. We have no records at all, no idea at all of how frequently different common types of behavior occur: how frequently do people agree, for example, on the average; how frequently do they disagree; what are the normal frequencies of a whole series of different kinds of common social responses. So I was attempting, in interactional process analysis, to develop an instrument for the compiling of social statistics about small groups. I realized pretty soon that you could not deal with eighty-seven categories. Perhaps you'd be able to think of that many categories; but at any rate, you would never get rates because some things occur so infrequently. So, I started grouping the categories together, and working it down. And eventually, got down to twelve categories, which are now very well known where they were published in this little book--now, a kind of piece of ancient history almost, Interaction Process Analysis. And, this book is cited very frequently in all kinds of other works, many of them having to do with practical work with groups.

Actually, it's cited twenty or thirty times as often as it's used, I'm sure. It takes a certain amount of training to learn to do this, and actually, it also requires rather special conditions of observation; you have to be able to sit and observe what happens, with good visibility, and you have to write quite rapidly. There are twenty scores a minute, or such a matter. Normal interaction--as fast as you can classify it, including the non-verbal behavior, and so on occurs at a rate of something like twenty acts per minute shifting back and forth. So it almost required a kind of, well, it certainly required a special role made for the observer in the group. And it was greatly facilitated by an observational room, where the observer would be out of sight and would not be so obtrusive. Other machines were developed to carry the paper tape, by now quite a lot smaller, about eight inches wide, and taking observations on a category list of twelve categories. The old interest, in agreement and disagreement and giving suggestions and so on, was preserved in that twelve. And the list looked so nice and symmetrical and logical that and you could print it on one page--and I think that's responsible for its wide dissemination.

But, so far as I know, it was the first category list; the first attempt to build a classification of social behavior that was appropriate to the description of every kind of behavior you could think of. That is, every kind of group and situation, and which would give rates with enough facts in each category to make statistics practical. So it was the first general purpose set of observational categories, so far as I know. There were other earlier ones made for special purposes; for example, in non-directive counseling, and in other studies of psychotherapy, lists have been developed. And, in fact, Lewin, Lippitt, and White, in their famous study on authoritarian, democratic, and laissez faire group atmospheres, had developed quite an extensive list of categories, which they use in observing these boys' groups. But, they didn't attempt to standardize it. In fact, Lewin and his followers, although they needed an observational system, I think, never really developed one. I can remember visiting Lewin's shop at MIT one time, and talking about interaction--the observation of interaction. And Lewin said "Yes, we really need to standardize our observers; we have not done that yet." So, he was very favorable in principle to doing it. But in fact, the Gestalt psychologist field

theorists didn't carry on with any systematic building of instruments. And I think that was one of the one of the reasons for the fact that the field theory approach tended to dwindle in the following ten, fifteen, twenty years after its beginning in the work of Lewin. The work that I've done, in the observation of interaction, has grown and developed through that long period.

I was not, in fact, thinking specifically of field theory. But, I think, the outcome of that work represented in my most recent book called SYMLOG, this one here, is, in fact, a kind of systemization of field theory. The observations of behavior--that are taken in terms of this system, and the observations of content, and the classification of content--are taken in terms of the position of an act in a theoretical space, a social, psychological space, of possible types of behavior. And that includes non-verbal behavior as well as overt behavior. It includes a classification of the types of things people talk about. That classification includes reference to: the self, and to the other, and to the group presently interacting to the situation; to society in a more general sense; and, finally, a kind of residual category of psychologically significant remarks with a kind of double meaning. I call it "fantasy," but you could call "symbolic references," or something else. And so, various kinds of images of self, other, and the situation, are reflected in the system; plus value statements, pro and con, about the kinds of behavior that one feels should be expected from these various kinds of images.

**Peter Blanck:** Could we back up for just a minute? I think a description of what SYMLOG stands for is needed, and how this developed from interaction process analysis.

**Freed Bales:** Right. SYMLOG is an acronym, which stands for a System, that's the S-Y, for the Multiple Level, that's the M-L, Observation of Groups, and that's the O-G. It also suggests a log of some kind that is a serial record of something or other, actually behavior, and the S-Y-M suggests symbols somehow. So, there is a kind of connotation of a running record of symbolic content and behavior which constitutes a kind of log. So, it's the grown-up, developed version of interaction process analysis. And was made with the same aspiration, really, in mind, which is to provide a general purpose system for observing and obtaining social statistics about behavior under various kinds of contextual conditions.

Social psychology has always been concerned with the social context of behavior. That's practically the definition of it. But, as of this time, we still have no, very good, classification or way of treating situations, of describing situations. We can imagine a bewildering variety of situations in a concrete sense. But how does one study, or classify, or describe situations in a more systematic sense that enables us to make comparisons between one situation, or one class of situations, to another class of situations. The psychological space, which is the background of SYMLOG, and which, to my mind, is a modern version of field theory, more or less in a direct continuation of Lewin's aspiration. That space is capable of forming the framework for the description of behavior images, value statements, and situations.

**Peter Blanck:** Could you talk about your three-dimensional space in a little more detail?

**Freed Bales:** Surely. Interaction process analysis was used for many, any studies of groups in the laboratory that I described in Emerson Hall, and I developed norms for groups of different sizes and so on. And, eventually began to study the effects of personality differences of individuals and value differences between them, attitudinal differences, and began to construct instruments for the description of values and value positions. In those days, I used case-discussion as the primary task of groups in so far as I standardized the task. I standardized it around a series of human-relations cases, actually by ancestry, coming from a course in the Business School in an earlier era, called Administrative Practices. Case analysis was the mode of study there. And, I developed, in fact I took over a course, which was developed in the Faculty of Arts and Sciences based upon the course on administrative practices in the Business School; that course was concerned with the analysis of cases.

It was started in 1946, actually, I think, by Dean Donovan of the Business School at that time. And, eventually passed into my hands in 1954, or there about, after I had developed interaction process analysis, and had developed a very great interest in the way in which groups change over time, or what is now called Group Development. So, I was not only interested in this course, but I took the task which was used in that course, the task of case analysis, as a basic kind of task to study, in a naturalistic way, in the development of observational methods. So, now to get around to the question of the origin of the SYMLOG space, which you asked a moment ago. I added more and more bells and whistles to the observational system, to the pretest battery, and eventually, one of my younger colleagues, Arthur Couch and I, made a huge factor-analytic study of a number of groups. About sixty subjects rotated through some number of groups which I have forgotten at the moment. But, extensive personality studies were made of these people at the Harvard Psychological Clinic. And we gave them extensive tests on values, and they rated each other on everything we could think of, and we took interaction process-analysis observations of their interactive social behavior. And we also observed and recorded the kinds of value statements that people made to each other; they were in favor of this, that, or the other kind of action in the case.

Many of these cases involved some kind of conflict as to what the person in the administrative position should do, and a kind of dilemma between an authoritarian kind of solution and some other kind of solution. So, the students who were the subjects in these groups got pretty steamed up about some of these issues. And I observed there the thing which captured my interest, and which, in a way, I've worked on ever since. I now call it polarization in the group; it's the development of a kind of conflict, or quarrel. In the first instance, about a written case, what I was observing, but what I observed that impressed me was that at a certain point in the discussion, when the emotional feeling got to a certain level; people began to identify each other as actors in the case. Whoever was defending the person in the administrative position who should take some kind of authoritarian action was regarded by the other members in the group practically as if he were that person. So, they took on the role of the persons in the case unwittingly. And viewed from the other side of the mirror, it was an astonishing phenomenon because the

amount of feeling generated seemed very disproportionate to the description on the piece of paper which they had been given. They were given a fictitious situation or one that was essentially fictitious, about people who meant nothing to them. But, before they were through, they were completely divided, in many instances, and treating each other as if they were actually involved in the situation. And this kind of event captured my interest because it was a graphic demonstration of the effect of context on social behavior. So, I was interested in what kinds of personality factors tended to lead people to take certain positions and what kinds of value conflicts arose in groups and, in fact, in social situations generally.

And so, in addition to working on a classification of interactive behavior I worked on a classification of kinds of values and kinds of value conflict. There's a large background of that in the social science literature also. Values had been studied from various points of view, but no general classification had yet gained currency. So, all of these things sort of converged on a large factor analytic study which Arthur Couch and I did in 1957, I think. And the amount of data produced was staggering. The number of variables was in the hundreds; that would be strongly disapproved these days in any experimental study; you're not supposed to have more variables than you have subjects. Anybody knows that, but we had lots more. And, it should be regarded, I think, as a kind of case study. The volume of data was so great that I spent about . . . well, from 1957 until 1963, I think, figuring out what we had. I stared at those numbers, I trekked my way through that wilderness in fifty-seven different ways, and, finally, I had the beatific vision. I saw how it fitted together, I thought; I still think so, incidentally. It was a vision of a three-dimensional space within which one could describe behavior, values, types of things talked about, even situations. And that three-dimensional space really has to be described repeatedly because the terms which apply to the position of acts of behavior in the space are different from the terms that apply to values in different positions in the space. But, I'll give the description for our purposes here, in terms of behavior. It's really pretty simple, in terms of intuitive perception.

I think everybody recognizes these dimensions of behavior and can learn to use them quite easily. There's a dimension of "Dominance to Submission;" and a whole lot of different kinds of behavior which have a dominant flavor, and others which have more or less a submissive flavor. And, I think of that dimension, as many people do, informally as kind of a vertical dimension of higher, lower, dominant, submissive, and so on. That's the first dimension which comes out usually quite strongly in observational studies, not so strongly in other kinds of studies, which have also given insight into this three-dimensional space. But, I think here, particularly, of the studies of Osgood, and what he called the "Semantic Space." In his studies of the semantic connotation of concepts, the most prominent dimension, typically, is what he calls the "Evaluative Dimension," a dimension extending from good to bad in some rough sense or other. A dimension which I call, with regard to behavior, "Friendly Versus Unfriendly." And, I think there are many different terms one can use to describe this difference. But I think we all recognize it quite easily. What the factor analytic studies showed was that these two dimensions are orthogonal to each other, generally speaking. That is, from a knowledge that a person is dominant you can't-- if you have only that piece of knowledge alone--you

can't conclude whether or not the behavior appears positive or negative or neutral; the ordinary meaning of "orthogonality" in factor analysis. There's a third dimension, less clear, hardly appearing at all in personality studies, but which appears strongly in value studies. And, that's a dimension which goes from, in terms of behavior, behavior which is task oriented, or productivity oriented, or instrumentally oriented, or carefully controlled, to behavior which is, from the point of view of management, or of leadership, usually, which appears to be unproductive, or sometimes deviant, or sometimes goofing off, or sometimes has emotional, primarily emotional. The general term I have for this is "Emotionally Expressive," which means that in the processes which give rise to the behavior, the level of affect, or of emotion, or of feeling, rises to a point where it takes precedence over the careful instrumental control; the means-end calculation also goes into behavior. So, there's a kind of balancing back and forth in many situations between the careful instrumental control of the behavior and the behavior which is expressive of a preexisting emotional state.

Instrumentally behavior is typically planful oriented toward the achievement of some future goal. Emotional behavior tends not to be planful, but simply an attempt to release, or to give vent, or to express an existing emotional feeling. In this sense, it sort of looks backward. So, instrumentally controlled behavior looks forward; emotional behavior sort of is present oriented and backward in time. This is a rough description of the space, and I think you'll see from the description I've given, that it has reference to various kinds of behavior, social behavior. We can think of any act of social behavior as existing somewhere in this three-dimensional space. It also turns out that when you think of all the values that you can or generalized attitudes, that people have, and do factor analyses of these dropping out those things on, which most everybody agrees, for example, that it's good to have plenty to eat, and a good night's sleep is desirable for most everybody and so on when you get down to the question of "On what kinds of values do people disagree, or polarize?" It turns out that they arrange themselves in the same three-dimensional space, the same one, or a cognate one. Perhaps it's not quite clear. So, that there is a kind of value which sanctions and approves every kind of social behavior that occurs. Then, when you go to classifications of non-verbal behavior, which are based upon emotional states or feelings, and analyze those as another psychologist, Albert Merabian has done, you find again the three-dimensional space. There's a kind of feeling or emotional state or the control of emotion, which corresponds to behavior in each of these directions. If you ask people to rate each other's behavior, or describe each other's behavior, and collect the terms they use and how there're related to each other, perceptions of behavior, again, a three-dimensional space.

Well, I've talked a certain amount about the space, probably too much, but, in any case, I was busy all the time during the 60's and the 70's observing the behavior in a course, the same course that I mentioned before, a course now called "Group Psychology Laboratory." It's a group made up mostly of undergraduate student's and they come with the agreement to study their own behavior. And there's no specification of what they should talk about, we don't any longer talk about specific written cases; we dropped that in the mid 60's, something like that. I was busy, meanwhile, winding all the theoretical insights I thought I had into the observation of students, and I had the students observing



themselves. That is from the total group of maybe sixteen or twenty students, a small observation team of four or five would be abstracted each week. And they would observe, using the current observational system, and prepare a feedback report for a Friday session.

The group has run on that pattern for quite a number of years. And I've gradually refined and perfected the methods of observation and feedback in that context. It has some resemblance to field research, in that, there's a lot of freedom in what may occur, both in terms of behavior and terms of what is talked about, and so on. But it's also strongly controlled in terms of physical location where it takes place, all in the laboratory down the hall here, and in terms of the desire to produce systematic data and to use it in helping students to understand themselves as individuals and to understand group dynamics. So, it's been used to teach, to give students something, and I have regulated everything I have done in terms of it's service to the client, as it were, rather than in terms of it's service to science. I've not done experiments, in short, and I have avoided doing things which had no value for the students; so, the educational value has been paramount.

In this sense, it's been very much like applied work in general, I think. In applied work, typically, the persons you're working with need to feel that they're getting something out of it, and otherwise, why should they be doing it. So, the upshot of it has been the development of, I think, quite a practical and theoretically strong system, which can be used for the study of all kinds of groups and situations in other field settings. I've described all this so far in observational terms, with the implication that the behavior is observed act by act. That's not practical in lots of field situations, but it is typically it is always practical for an observer, a single participant observer, if he has permission to be there at all, to systematize his observations at the end, and SYMLOG has been geared to that kind of self-debriefing, which will give systematic information about what the researcher has observed in the field setting. In many situations, it's also very practical for individuals to describe their perceptions of the behavior of each other, and this has turned out to be extremely valuable because a number of them added together will give you a very reliable and valid picture of the behavior, of the kind you would get from act to act observation. But, on the other hand, when you examine the observations of perceptions of each individual separately, they diverge from each other in psychologically significant ways. So, you can use the divergences and biases in perceptions from the group average as extremely valuable diagnostic information for purposes of research, and also, perhaps even more important, for purposes of feedback to the participants because, typically, they are fascinated to see the general result of the work they have done describing each other. And also to see how they vary from each other.

This is prime motivation in the field employment of this system. And, it's also the prime motivation of the students in the group psychology course. I think, if there is one practical finding--that's I think, very general, and applicable in the field--it is that if you have a good system for feedback of information to people about their own behavior and attitudes, they are typically very interested and will cooperate. SYMLOG is being used

in applied settings fairly extensively now. It's being used for the study of top management groups. For example, there's a many different consulting firm in Boston, which uses it. One has been set up in New York with the explicit purpose of using it in top management consulting, and it's now starting in Paris for the same purpose. It appears to be practical for use in business settings because the categories, even though they originated in an academic setting, the descriptive terms were chosen to be very general and applicable in all kinds of situations. So, that appears to be developing rapidly. It's also used in a number of business school courses, both here at Harvard and more than one other place, for the study of the work teams, and for team building. The students in the group psychology course use it to make case studies of their own families that's the most easily accessible group for most students they can also use it for studying their roommate group, and they do, or group in some work setting. But most of them make studies of their own families. So we have had a lot of experience in family studies done by students. There are a few family therapists using it. And one student at Boston College has made extensive case studies of families in the field not a great number of case studies, but intensive studies of a few families, eight or ten or twelve, something like that--with feedback of the results to the family, with very good results. That is, he appeared to learn a lot, and the families liked the procedure and were willing to, in fact, wished to go further with it.

**Freed Bales:** Well Peter, here we are in the group-observation room, in which I've done many years of work. And you see that we have special props all around this huge circular table. And rooms without any discernible corners, so that nobody has to sit in the corner. We have video apparatus in the ceiling here, which we don't use very much actually, because we prefer to use the real thing whenever we can. And, in the other room, on the other side of this one-way mirror, as you know, there's an amphitheater where up to twenty, twenty-four observers can sit. So, it's a room for the study of one group, interacting group in this room, by a group of observers in the other room. It looks like my research has gone from no complication to extreme complication, in terms of difficult facilities required.

In a way, that's true, but I'm happy to say that that isn't the only way it's gone. I mentioned before that it's possible for a single participant observer, with no equipment whatsoever, or with not even a paper and a pencil to write things down, can make observations systematically and can debrief himself afterward. And, that I regard as a real triumph of the system, and perhaps the solution of the trauma which I suffered at the hands of Alcoholics' Anonymous, where I had to take my notes visibly. Now it's possible to work in an applied setting and record everything that you can see and hear with a systematic method, in addition to whatever other notes you may take, so that you can make studies of groups comparatively with enough reliability to make it practical. If you want more reliability, all you have to do is add more observers, and that of course is what we do in this room.

Among other things in this interesting setting, you'll see posted around this, all the around behind us, what we call field diagrams. Each one of these is the record of a particular group meeting, and you'll notice that each one of them is inscribed with a

polarization, a couple of large circles, which isolate certain portions of space, and those circles are placed so as to include the subgroups which would, taking the which were performing the kinds of behavior described in these portions of space. Every one is different; every meeting gets off on a different foot. And, so the polarization that develops, or the argument, or the kind of overwhelming emotional mood that develops, gets reflected in a particular kind of polarization or unification. Some of these are not polarized, but show, practically, the whole group unified in a given mood. In spite of the fact that there is a different kind of mood for every meeting, or a different polarization. There are also chronic tendencies which show up in the ratings that we have students make of each other at mid-term and at the end; these are the more kind of permanent social positions that people have.

These diagrams are interesting to the students, fascinating, and they're the same kind of diagrams that you can use in the field with any group that you happen to be studying. It's the main method of feedback. And, the dimensions are very easy to explain, the friendly versus unfriendly, the task oriented versus the emotionally expressive. And people recognize, almost intuitively, the fact that the diagram portrays the relationships as they see them. So, they can talk about what's up on the board, what's in the diagram, instead of directly confronting each other. And that seems to unlock their ability to do a lot of analysis of the group that is otherwise prevented.

**Peter Blanck:** You've actually done a SYMLOG kit . . . [coughing] . . . associate which researchers can use as well--which is sort of a self-contained diagramming kit. Maybe you could talk a little bit about

**Freed Bales:** Yes. Yes, the SYMLOG Kit is simple a little description of how a group may make a self study, containing all the forms necessary to make diagrams of this kind. So, it's sort of the final step in making this method, seated as it is in a lot of social and personality psychology and clinical psychology and so on, available to groups with practically no technical experience. A group that is motivated to do so, can make a useful study without technical help at all.

**Peter Blanck:** Now, I want to shift gears a little, and talk about your general views on future interdisciplinary approaches coming from a Psych. and Soc. Rel. Department that has gone through many changes, and many different influences. If you could describe how, historically, that has influenced your views on the future of interdisciplinary approaches. We'll talk about it for a couple of minutes.

**Freed Bales:** Well, I am indelibly imprinted, I think, with the point of view of the Department of Social Relations, which is that it's necessary in the study of actual situations, actual groups, real interaction, to take a great many factors into account. Not only psychology is necessary, social psychology, but also knowledge of personality, clinical psychology, sociology, and knowledge of politics and political science; that is the political aspects of relationships. And, certainly, in many of the business settings which viewers of this film will be interested in; you have to take other factors into account, the technological factors.

And I'm glad to say that organization development, originating as it did, more or less in the study of small groups, training groups, in the Lewin tradition, is now fanning out, as far as I can tell, to a much more interdisciplinary approach, which takes the technical factors into account, and the nature of the task in other respects and financial factors, and so on. Which seems obvious enough that you need to take those into account if you really want to understand what is going on. You may start at a given level, studying one of these things, but they tend to be all highly interconnected. And, if you mean to make a significant difference in an organization over time, you probably have to take up a multidisciplinary approach, and work in a great many facets of the organization. I think organization grew out of the, organization development grew out of the realization that it was not enough to extract managers from the plant and send them off for leadership training or something in a T-group they got back and were inundated. So, the researchers and trainers said "Well, let's go into the organization, and work with it as it is, in a kind of custom-made fashion." That's what organization development is.

But, now I think it's being realized that that is not enough either, that you have to take technical and other aspects of the organization into account that are not within the competence of psychologists, sort of, sociologists.

**Peter Blanck:** Okay. One final question which I ask everybody, and that is, I simply end by saying what do you find most fun about research, why do you enjoy doing it, and what is most interesting to you, and what do you want to do in the future?

**Freed Bales:** Well, I love working with people, actually, because it has that emotional zing to it, and it gives me pleasure to feel, as I sometimes do, that I understand enough about it, to deal with the practical situation, and don't have to depend upon out-of-control experimental situations in order to find that my generalizations can be brought to life. So, there is some kind of ego involvement that I have in being able to deal with real situations in real time that gives me satisfaction, when I'm able to do it. But, I'm also extremely interested in the integration of social psychology, and I believe that the time is ripe for a better integration, that we now have, into a field theory.

I think I'm on track, of what is involved in that kind of field theory, represented in what I've said about SYMLOG, represented in part. And, what I intend to do in the future is to concentrate more on the theoretical integration of the experimental literature with the practical necessities arising out of applied research. I feel that social psychology is not going to solve its problems by dealing with experimental situations alone. We need a much better interplay between experimental and field approaches. And we need, also, people who are working hard, both to skim the cream from the generalizations that applied social scientists are able to make, working in the field, practitioners' know-how, translating that into theoretical social psychology, comparing the findings from family therapy with families or the generalizations and concepts from organization development, with the findings from various kinds of psychotherapy, group psychotherapy, and so on.

These are all part of the same discipline, theoretically social psychology; but, we've not really taken advantage of the practical knowledge that exists. And, we have not translated our experimental findings realistically into field settings, where the context is really extremely important. Many of our generalizations are extremely fragile and anchored to a particular experimental situation. And, we have to do a lot of work in discovering where those generalizations are developed, and where they are not, because they are certainly very fragile in terms of contextual differences. I think we're going to have to work, not only in business contexts, which are of great interest presently and to me, but also in a lot of different organizational contexts, different institutional contexts. All the professional schools of the university have some beginnings in social psychology. I think the social psychology that is produced in business schools is probably the most highly developed. But, there is a branch of applied social psychology appropriate to medical schools, to law schools, to schools of government, and so on. The task of social psychology--as I see it, now, works toward integration is to fan out and make themselves useful in these various different professional settings, institutional settings, and to learn how the findings from those areas can be integrated into a general theoretical social psychology.