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THE USE OF SAMPLING PROCEDURES AND ROLE THEORY IN SOCIOLOGICAL RESEARCH*

THIS paper is a consideration of the possibility of combining sampling procedures and role theory as methods of social research. It is one contention of this study that these are currently the chief centres of interest in sociological theorizing. The first, shared by several other sciences, is part of a broader interest in statistical procedures. The other is an integral part of the renewed interest in the study of social organization as such.

On the surface it might appear that these two interests are complementary in character, and that both would share in the attention of persons inclined to considerations of theory. Actually the situation is quite otherwise. Students in the two fields have remained isolated from each other to an eminent degree. The intent of this paper is to attempt a bridge between these two interests, by exploring the possibility of extending the techniques of sampling to the analysis of social organization.

The paper is divided into three parts. The first section is a very abbreviated introduction to some general features of sampling procedures and of the nature of social organization. The second part is a consideration of a specific study in each of the two fields. The final section deals with the application of both types of approach to a research situation.

One may start by considering some of the ways in which sampling procedures have been applied to the practical side of the social sciences. Academicians have played a minor role at this level, and in many instances are unaware of current developments. The things that come first to mind here are the spectacular achievements in such fields as public opinion polls. Although the results of these efforts contribute heavily to the daily news, it must be confessed that the contributions to scientific knowledge are relatively inconsequential. The gambler on election results can compute his odds a bit more effectively, and the soap salesman can decide which radio jingle makes his victim deodorant conscious. But it is difficult to detect any increment to scientific knowledge arising from such efforts.

Such applications are of decidedly lesser importance than the use of sampling procedures as a part of administrative surveys. In this latter field

*This paper was presented at the annual meeting of the Canadian Political Science Association in Vancouver on June 16, 1948. Generous acknowledgement is due my colleagues at McGill for their contributions and criticisms.

the results have been revolutionary. The monetary costs of surveys comparable to the decennial census have been cut to a tiny fraction of their former magnitude. At the same time the speed of such surveys has been enormously enhanced. Hence the administrator is enabled to use pilot and progress surveys as a guide to policy, and thus has a revolutionary advantage over his predecessor of a generation ago. In Canada and the United States the plans for the re-establishment, education, and rehabilitation of returned service personnel of the Second World War were prepared and carried out on the basis of sample surveys. There seems little doubt that the integration of these returned personnel has been remarkably successful when compared with the experience of World War I. It is equally clear that much of this success is attributable to the use of sampling procedures to obtain data to guide the decisions of the persons administering the programmes.

It is not only in the hands of the administrator, however, that sampling is making a substantial contribution. The effort to envisage administrative problems in terms of sampling theory frequently suggests the main directions along which analysis should proceed. An illustration will be given later in which such a formulation of an administrative problem serves to indicate the elements involved in its solution. In such cases statistics become not merely a tool for the administrator's adviser but also a research method for gaining new knowledge. In other words this approach belongs to the kit of the social scientist as well as that of the administrator. With these brief comments on three extensions of sampling we may turn to the second field of theoretical interest.

Earlier in this paper it was stated that persons with an interest in sampling theory had largely ignored the problem of social organization. The classical statement of the problem is found in the writings of men as widely scattered as Durkheim,[1] Pareto,[2] and Parsons.[3] These men shared the notion that the economists of their day had either neglected or oversimplified the nature of social organization, and they proceeded to marshal evidence to permit the viewing of society from a structural point of view. Of the economists who were influenced to reorient their thinking in this fashion, Veblen is the most conspicuous example. He slanted economics in the direction of institutional studies. However, his analysis of such problems as education[4] emphasized how successfully the idea of studying an institution can be extended to other parts of the social order. The orderly study of social organization has attracted the attention of a flock of sociologists; of these Everett Hughes,[5] formerly of McGill and now at the University of Chicago, has provided the most penetrating and readable analysis of the structure and function of social institutions.

[1]Emile Durkheim, *On the Division of Labor in Society* (New York, 1933), trans. by George Simpson.

[2]Vilfredo Pareto, *The Mind and Society* (New York, 1935).

[3]Talcott Parsons, *The Structure of Social Action* (New York, 1937).

[4]Thorstein Veblen, *The Higher Learning in America* (New York, 1918).

[5]R. E. Park, *Outline of the Principles of Sociology* (New York, 1939).

During the last decade there has been a considerable shift of attention from the study of formal institutions to that of informal organization. Garceau's book on the political organization of the American Medical Association[6] represents one of the more successful ventures in this direction. More recently there has emerged the widespread study of informal organization in industry. As an area of study, informal organization lacks the codes, constitutions, offices, and insignia of established institutions; however, its very nebulousness has acted as a spur and a challenge to the students who have developed an interest in this kind of phenomenon. At the present time very fruitful investigations are being carried on at the margin between specific formal institutions and the characteristic informal structures (cliques, friendship groups, and the like) which spring up within formal institutions. The studies of industry that have been oriented in this fashion have contributed significantly to the understanding of such phenomena as absenteeism, labour turnover, productivity, morale, and the development of spontaneous cooperation in the work situation.

The trail-blazing ventures in industrial studies were Roethlisberger and Dickson's *Management and the Worker*,[7] and the Yankee City series of monographs.[8] When viewed in retrospect these appear to have been inordinately expensive in terms of manpower and time, using dozens of workers over many years. Both of the above were avowedly pioneer investigations, and later efforts have improved the record. Under the stress of war, industrial studies along a broad front were carried on in relatively economical fashion. As example one might cite the investigation by Fox and Stone of absenteeism in the steel industry.[9] These men found very marked differences in absenteeism in three East Coast plants, and proceeded to organize research to seek an explanation. After following the conventional leads without success they hit on the hypothesis that high levels of absenteeism are correlated with low levels of competence among those plant executives who are in close contact with production workers. In other words, where executives showed hesitation and confusion about issuing orders and organizing new jobs, the workers found themselves working under emotional strain and reacted to the situation by staying away from the job temporarily. The three plants showed very wide differentials of competence as between the supervisory-executive staffs, and the rates of absenteeism were roughly proportional to these differences. In this case the research team started off with the prior hunch that the emotional relationships of workers and their supervisors represent an important focus of attention in the work situation; from this they moved to an early diagnosis of the causal factor in absenteeism.

[6]Oliver Garceau, *The Political Life of the American Medical Association* (Cambridge, Mass., 1941).

[7]F. J. Roethlisberger and W. J. Dickson, *Management and the Worker* (Cambridge, Mass., 1939).

[8]See especially W. L. Warner, *The Social System of the Modern Factory* (New Haven, Conn., 1947).

[9]J. B. Fox and J. F. Stone, *Absenteeism: Management's Problem* (Boston, Mass., 1943).

One might pause to comment here on the dependence of these later investigations on the pioneer studies. Initial exploratory studies have a distinctive function to perform. They serve to indicate the kinds of factors which may be operative in a problem situation. From such a perspective it is possible for more limited and precise studies to be made which locate a problem and diagnose it in fairly economical fashion. However, when one considers the amount of effort and interviewing involved in even the most economical of such studies it is obvious that the work of the sampling students is spectacularly cheap and speedy by comparison.

It is highly pertinent to enquire, therefore, whether the economies inhering in the sampling type of study cannot be achieved when studying social organization. I propose at this point to consider two concrete studies. One of these readily succumbs to a sampling approach, while the other proves highly resistant to such efforts. Comparison of the studies serves to emphasize some distinctive features of social organization.

The first problem has to do with discovering the distribution of goitre in a specified population. To clarify the nature of the problem envisaged, a digression is necessary in order to indicate three levels of approach to such a matter. At the lowest level of approach one can prepare a distribution of the cases of goitre in the total population, by means of complete enumeration, and thereby state the incidence of the ailment. At a higher level one can set oneself the problem of discovering the incidence of goitre by devising an efficient sample of the population which would provide the same type of knowledge, with a specifiable error, at, let us say, one-twentieth of the cost. At a still higher level one can investigate the incidence of goitre, by sampling methods, in such fashion that the determination of its incidence would at the same time indicate the related factors which are associated with the appearance of goitre. In the third case the statistical operation not only exposes the distribution of the ailment but actually offers leads for its control.

It is found that in a certain province there exists a substantial number of goitre cases, and these seem to be solidly located, that is to say, in a simply connected area with a moderately sharp boundary. The administrative problem is to define the boundary, having at one's disposal specified and limited sampling resources. Since the primary interest is not to discover the percentage of goitre cases within the province, a purely random allocation of the sample independently through the entire province is not the solution. The best way is to take a fraction, a third say, of the total amount of the sampling that is permitted by the resources available, and to spread this in a grid over the entire province. This will give a first approximation to the boundary. The second step would be to utilize another third of the sampling within the band which the first survey indicated as the boundary of the area suffering from goitre. This would provide a much closer approximation to the boundary. The third step would involve using the remainder of the sampling resources to work within the even narrower band obtained from the indications of the second survey. Along these lines of procedure will be

found the most accurate delineation of the boundary, working within the limits of the sampling resources available.

With respect to this little problem and the solution offered, three comments are necessary. In the first place, in order to delimit the boundaries, an economical sampling procedure is used which avoids the costs of an exhaustive survey. In the second place, a type of sequential sampling is used which locates the boundaries with maximum accuracy. And lastly, the progressive refinement of the samples focusses attention on the factors that are likely to be associated with goitre, such as drainage pattern, soil content, and so forth. In other words, the effort to devise a sample to define the boundaries of the goitre area actually drives the research worker in the direction of locating the chief analytical elements which are causally associated with goitre. In this problem are exemplified the three main characteristics of efficient sampling in social research: economical measurement of the phenomena to be studied, progressive delimitation of the area to be studied, and progressive delineation of the factors to be observed.

It seems appropriate before leaving this example to strip away one of the misconceptions widely held with respect to sampling. Much of the work done in sampling seems to assume that one can, acting on the basis of ignorance, extract dependable knowledge from a problematic situation. In the above example it seems clear that the elements of refined knowledge are derived from prior, though crude, knowledge. Thus, lacking any knowledge of the distribution of goitre except that it is located in a simply connected area, one secures only a coarse outline of the boundary. With the knowledge derived from the first sample one can devise a second sample that is vastly more efficient. From the standpoint of the knowledge derived from the first sample the efforts used on the elements lying outside the band are wasted. They provide no information whatever on the boundary. It is the knowledge concerning the area within the band which functions to define the boundary. It seems clear that, far from being a weapon for deriving knowledge out of ignorance, sampling depends for its efficacy on the ingenuity of the student in incorporating relevant previous knowledge in his selection of items.

The above example was drawn from the field of medical pathology. The next is selected from the field of social pathology, and deals with the organization of abortion in urban areas. To speak of "the organization of abortion" implies that urban communities develop specific social machinery to make abortion available.[10] Abortion was selected because it pin-points attention on the meaning of the term "social" in the expression "social organization." The term "social" has become so overladen with overtones of nicety and gentility that it appeared desirable to select an activity that seemed downright anti-social. In this way it is hoped to restrict attention to the structure within which the activity is organized, unencumbered by any hopes or prefer-

[10]If the reader doubts that this activity gets itself organized in Canadian cities, he is at liberty to ascribe to it an American location. If the proposition still leaves him incredulous the purpose will be served if he merely entertains the proposition as a hypothetical possibility.

ences as to the success of the activity studied. The point of emphasis is that it is the organized human machinery that is the focus of attention when the term "social organization" is used.

It is necessary to point out that one deals with different orders of fact when he turns from the study of goitre to that of abortion. In the first case he deals with data which exist in the form of hospital admissions, cases reported, operations performed, and the like. Hence the raw materials comprise the factual data. This order of fact lends itself admirably to statistical manipulation. The data in the second case have to be *constructed* from the statements which persons make about their own, and other's behaviour. The data are not immediately given by the raw materials. Such constructed data suffer three limitations. The available body of respondents will not possess all of the information about abortion. Thus gaps arise in the data. Some of the information will be withheld from the investigator because the persons with it refuse to divulge it. This involves further gaps in the data. Lastly some of the data will be withheld because respondents cannot reveal it, in the sense that scruples, convention, or shame drive the information out of the area of discussion.[11]

In studying abortion one could proceed as in the case of goitre and search for the pattern of distribution of cases. Much of the information available in the published literature is characterized by just such a conventional and statistical orientation. For example a recent textbook on the family[12] summarizes in three conclusions some of the relevant statistical knowledge. "(1) It is estimated that perhaps 90 percent of criminally induced abortions are performed by or on married women. (2) There is a tendency for the abortion rate to rise as the number of pregnancies increases. (3) The frequency of abortions increases as the family income increases."

Such statements indicate the conventional nature of the statistical categories used in this field. One would concede, of course, that there is interest and utility in relating the incidence of abortion to conjugal condition, size of family, and size of family income. However, one can hardly avoid the suspicion that it is because these categories lend themselves to ready mathematical analysis that they are chosen as the appropriate source of data with respect to abortion. They are the facts about abortion which are easy to count. It should be added here that one can scarcely expect official records to provide all the specific data needed for theoretical research. The currently collected statistics always represent an arbitrary selection from possible official sources. However, official series represent again a selection from the kinds of data that less restricted functionaries could gather. These considerations

[11]These limitations, it must be said, are distinctly relative limitations. The most prosaic studies suffer identical limitations, though to a lesser degree. In more conventional studies, however, either common sense or accepted theory determines what kinds of activity will be reported and used to construct the data. Since neither of these is readily available to aid in the study of abortion, the selection of data is rendered doubly difficult. Stated differently, one can identify data satisfactorily only if he already has some theory available.

[12]H. A. Bowman, *Marriage for Moderns* (New York and Toronto, 1942), p. 460.

indicate the necessity for seeking primary data on abortion by first hand contact with the persons involved.

Of course if you had never heard of induced abortion, or having heard of it know none of the functionaries involved in the activity, then you can do nothing. But suppose you find one of the types involved in its practice in a large city. You might wander across a druggist, say, who both sells abortifacients and sends enquirers to a clinic and draws a commission therefor. Once you find him you need only question him enough (provided you have his confidence, an important practical point) until you find out about the clinic with which he has business connexions. When, following this lead, you get to the clinic there will be several other leads—to patients, who can in turn give you information on other methods of getting abortions which they have had recourse to; to the shady doctors who work part-time in the place, who may do other kinds of work, of concern to the study of abortion, in the time they are not on duty in the clinic; to the operator of the place who will in all likelihood have his work here integrated with other activities in the field of crime. You may or may not, depending on the skill with which you ask the questions, in which both much tact and much knowledge of urban institutions will be involved, succeed in getting a complete picture of all the functionaries by simply following threads in such a fashion.

But once you are as far along in the job as the above implies you have another resource—a combination of simple logic and of elementary sociological theory. There will be many places where what we might call connective functionaries are plainly lacking and the system is therefore incomplete. If you asked for instance how the drug stores find out about the clinic in the first place and collect their commissions, the role of the salesmen would be revealed. If you asked why the police did not close it up you would find politicians and other go-betweens. If you asked what happened when something went wrong in the operations you would find out about private nursing facilities that were maintained, and semi-respectable doctors who were on the payroll and willing to make out death certificates.

One can conclude, from this brief synopsis, that the results of an exploratory study of abortion cannot be stated in terms of any statistical distribution; rather they need to be presented in terms of the very elaborate social machinery which is involved in the practice of abortion. The new knowledge from such a study primarily concerns neither the incidence of abortion nor the general attributes of persons involved in abortion but the specialized and differentiated behaviour of a number of key functionaries.

It is possible at this point to formulate some of the broad differences between the two types of studies, and to assess the applicability of statistical analysis to the two cases. The first problem concerns a *universe of attributes*, and each of the main elements in the problem can be restated as an attribute of a goitre case. Such a universe can be very conveniently considered in terms of distributions, and these lend themselves admirably to statistical manipulation. The abortion situation on the other hand represents, not a

universe of attributes, but a *system of social action.* In making the initial exploration of such a system it would be futile to sample at random for information. What is needed is something of the approach of the detective who is reconstructing a crime. The system which provides abortion is likely to involve a number of unsuspected components which are only realized by the faithful following out of clues. Once the investigator locates some of the main axes of behaviour he can use rational means to anticipate the possible connecting lines of activity, and guide his observation accordingly.[13]

The appropriate method of conceiving this system of action seems to be in terms of a theory of roles. It is obvious that these roles cannot be deduced by any process of formal logic but depend on empirical observation. Each of the roles comprises a complex pattern of behaviour, known in part to the actor of the role and in part to those with whom he interacts. These roles are reciprocal in nature, and do not operate in isolation; the roles are integrated, and comprise a system by virtue of the fact that each role is dependent on most of the others at certain points. Since the system is unplanned it is unlikely that any person within it will have knowledge of all the roles that comprise it. However, as noted earlier, the content of the roles is known in part to some of the participants; some of that knowledge they will reveal; some they will not, and some they can not. However, what the participants do not reveal they may on occasion betray. It is the function of the social researcher in the exploratory investigation to make as explicit as possible the system of roles which underlies the practice of abortion.

Probably enough has been said here to indicate why the usual statistical approach is inappropriate at this stage of investigation. Any distribution of the persons securing abortions, whether measured along a spatial axis or any other axis that the ingenuity of the researcher might design, would provide negligible information on the organization of abortion. The reason, of course, is that any causal interpretation of abortion as a social system is not traceable to the attributes of the persons securing abortions. Perhaps the only attribute that persons securing abortions have in common is the fact that they have contact with the form of social organization described earlier. On the other hand the securing of abortions is dependent on the operation of the social machinery that has just been outlined.

The significant comparisons of these two kinds of studies can now be summarized. Sampling procedures are manifestly appropriate for dealing with situations in which the units and their attributes are unambiguously defined. The attributes may be defined in terms of common sense, or in terms of

[13]The difficulties of investigation and the kinds of elements elucidated are by no means peculiar to the abortion situation—that is, one involving illegal and immoral sorts of conduct. They are, so to speak, highly general matters. One could anticipate the same difficulties and elements if he were studying the office of a reputable lawyer, a Russian collective farm, or the crew of a vessel. Where the study concerns an official institution one would start, of course, by considering the established functionaries who would advertise to some degree their activities. One would not, however, consider the task finished at that point, but would proceed to investigate the informal elements in the manner of the above.

commonly accepted theory. Under those two circumstances the researcher can pay chief attention to the distribution of attributes, to their relations and correlations, and to their stability. However, not all situations are structured in such a way as to permit this. In the distinctive research situation, that is the problematic situation, the unit or its attributes are in a state of flux, conatively, and through the process of research have a structure imposed on them.

Turning to the typical case of informal organization we are confronted with, not a system of attributes, but a system of action. It frequently happens of course that a system of action can be transformed into a system of attributes. This is precisely the situation in the case of the public opinion polls. In the conventional poll the action system is arbitrarily simplified to the point where action is limited to making the choice between voting for one or the other of a pair of candidates. Having simplified action to these lengths it is a simple matter to label one type of action Republican, and thenceforth the attribute Republican can be imputed to the person so voting. These attributes then become amenable to conventional statistical manipulation. Wherever action can be neatly polarized this sort of transformation is easily carried out.

However, when we turn to the less eviscerated systems of action found in situations involving informal organization, two formidable obstacles arise. In the first place the action is extremely complex. It no longer represents merely the two ends of a continuum. The roles of the persons involved in the kinds of informal organization discussed in this paper are too complex to be reduced to entities which can be counted simply. In the second place these roles are interrelated in reciprocal fashion to such an extent that a description of one involves related parts of other roles. Hence they can be isolated for mathematical analysis only at the cost of considerable distortion. There is probably no point in trying to reduce these roles to simpler components, or to equate them to combinations of attributes.

The final section of this paper is a brief attempt to consider the use of both approaches as applied to the same research situation. Up to this point the two approaches have been presented in contrasting fashion in order to emphasize distinctive features of each. It is now proposed to use them in complementary fashion on the same problem.

The selected problem emerges from an area of research which has had some preliminary consideration by a committee[14] of the Canadian Social Science Research Council. This committee attempted a reconnaissance of the range of problems which affect Canadian Indians on reservations. Although administrative in origin these problems are closely related to current theoretical interests. The problems have arisen in part from the limited success of the Indian Affairs Branch in trying to protect and control the affairs of Indians. The relative failure of many elements of policy suggests the existence of forms of social organization which to date have largely eluded the administrators.

[14]Under the chairmanship of Professor La Violette, McGill University.

Some field work has been done so far on the Caughnawaga Reserve in the Montreal area, in an effort to make explicit the social organization of this reservation.[15] Preliminary study of the reserve has suggested a variety of leads for intensive research. These leads indicate the apparent axes along which the reservation is organized. Three of the promising possibilities for further research follow.

In the first instance the reservation seems to be organized around hostility toward the federal government. This hostility is evident in the earliest contacts one makes on the reserve; it is abundantly documented in the letters written to the Indian Affairs Branch. One might presume that such hostility would be accompanied by a comradely identification with other minority groups like the French in Montreal. However the hostility toward the French is as strong as, if not stronger than, that toward the dominant British. Hostility appears to be one of the main axes along which life has become organized.

In the second place, and as far as economic activity is concerned, the main jobs attracting these Indians are in the steel construction industry, and more particularly the high jobs in bridge construction and the like. These jobs have a nomadic quality to them, requiring movement to some of the largest cities in the United States; they have a conspicuous element of danger in them; they offer large rewards in the form of high rates of pay. In a sense they are the industrialized equivalent of the Mohawk warrior's role. The semi-monopolistic concentration of Indians in these jobs seems to indicate a significant axis of prestige for the reserve population. Their work efforts along other lines, with the exception of the tourist trade, are negligible, though interestingly enough the very highest incomes on the reservation go to the purveyors of folk medicine.

In the third place the reserve displays a volcanic uneasiness. It is marked by recurrent disturbances of a minor riot character. These demonstrations produce no noticeable changes in the pattern of life of the reserve, but seem to indicate a basic lack of stability at some points in the social organization of the population. The uneasiness is shared by the religious leaders on the reservation, even though they have been in charge of religious matters since the organization of the reserve. The religious leaders seem unable either to control these spasmodic displays of violence or to predict how the Indians will react to a situation requiring collective action or collective decision making.

Each of the above axes represents an aspect of social organization of interest to administrators and research students. Each suggests a research problem requiring an appropriate set of research techniques. In undertaking research along these lines one can start by working with the recorded attributes of the reserve population, especially with those attributes which can be distributed geographically. This was the key to the goitre study, in which all of the attributes could be located in geographical space. It will be recalled that the study of abortion was not expressed in any such terms. We are forced by the nature of the data on abortion to conceive of such organization

[15]Unpublished report by Mr. Dan Lortie, field worker, McGill University.

in terms of *social space*, even though the axes of social space are considerably more complex than are those of geographical space. Parenthetically one might note that the only axes of social space that have a commonsense reference are those implied in the expressions "the Right" and "the Left," and in the status notion of higher and lower. Only by exploration can one determine which axes will be useful in describing the social space of the reservation.

The social organization of the Caughnawaga reserve can be partially stated by reference to geographical space. The legal boundaries are known, and the legal residents within those boundaries are readily distinguished. Also with respect to the Indian population of this reserve a wide range of attributes can be stated, such as age, sex, conjugal condition, and the other conventional categories. But not all of these attributes can be defined or estimated with equal accuracy. The distribution of occupations is more subject to error than are age and sex, while the estimate of the distribution of incomes would show even more error. It is totally impossible, on the other hand, to frame estimates of the prestige of jobs in steel construction as compared to jobs in the tourist trade. Nor could one estimate the status of current occupations as compared with those that the Indians followed traditionally. Occupational prestige and status are not attributes of the population; they belong within the framework of role theory. They are nodes of social organization. To secure knowledge about these phenomena one has recourse only to an interviewing technique.

Before attempting to gain knowledge by interviewing one would of course exploit to the full the facts provided by the attributes which can be enumerated. This frequently involves going beyond the series which the administrator or the official census provides. To secure these in economical fashion one would employ the relevant techniques of the sampling expert. Having estimated the attributes to the best of his ability one is in a position to start interviewing. At this point one may again use the methods of the sampling expert in order to select the subjects for interviewing. The advantages of this method of selecting subjects would have to be weighed against the danger that arbitrarily selected subjects may not discuss freely; a responsive member of a social system is considerably more useful than a highly representative one with whom *rapport* cannot be adequately established. By taking into account the attributes of the subjects, as an adjunct to interviewing, a double purpose is served. It aids, as was indicated, in the selection of subjects to interview, and it gives information on the potential roles that specified subjects may be acting. The attributes suggest the questions to be used in interviewing.

Discussion of the techniques of interviewing which are appropriate in this kind of study is omitted in order to devote attention to the central problem of role research. This is the problem of proceeding from the raw materials given in the interviews to the establishment of roles as scientific facts. In the beginning the roles are tentative constructs originating with the research student. The roles become facts in so far as they gain recognition by other

social scientists. The scientific status of such facts depends, here as elsewhere in science, on their acceptance by other students wishing to check the results. Roles are constructs of scientific investigation, and in order to be accepted they must pass the scrutiny of persons who share the interests and curiosity of the initial investigator. The construction of roles arises in the effort of the research student to analyse the content of his interview materials. Using a phrase of Max Weber the roles are ideal types which the social scientist constructs in order to achieve intellectual control over the complex materials of his interviews.

The roles suffer a pattern of tentative growth as the research student tests and checks them against new interview materials. One might say that he arrives at the constructs by a process of successive approximations. The first crude formulation is an effort to synthesize items of behaviour into a meaningful pattern. Thenceforth it passes through a double set of checks. In the first place the role is tested against the behaviour of other actors who are presumed to be playing an identical role. In so far as it fails to portray adequately the conduct of these later actors it requires modification. The role is also tested against the array of reciprocal roles with which it is interrelated. If it fails to articulate with these other roles it requires further modification. Meanwhile the reciprocal roles are undergoing comparable modification and reformulation. The roles of a social system are built up by such a series of successive approximations.

A role becomes a scientific fact when it can pass three tests. In the first place it must register stability when applied to actors in the new situations. Secondly, when integrated with other roles it must form an articulated, consistent system which provides an adequate interpretation of the raw materials with which the research student started. Lastly it must gain the acceptance of other scientists who use it to confront the same research situations.

In much research in the social sciences this process goes on in intuitive and/or unreflective fashion. Moreover there seems to be a tacit presumption that the content of interviews cannot be subjected to rigorous analysis of a controlled nature. The argument here is that an awareness of the process simplifies the work of the research student and reduces the areas of possible disagreement and misunderstanding between investigators.

The establishment of roles as scientific facts is an essential procedure in the discovery of social organization. As noted above the roles require extensive verification before they can be admitted as elements in social organization. Their verification by contemporary students would have to be supplemented by verification at later points in time. One of the main assumptions made concerning the nature of social organization is that it persists through time. Hence any significant role in a specific social organization would persist for succeeding students to document. Deviations in the role through time would be indexes of social change. There is a responsibility on the persons who organize research in social organization so to plan their programmes that they can be repeated at later intervals along controlled lines of organization. One might emphasize the necessity for careful specification of the attributes of

the population studied in order to permit comparative studies through time.

In the wake of these comments, and by way of conclusion, it is possible to point out some fundamental parallels between the two approaches to social investigation, the delineation of roles and the estimation of attributes by sampling procedures. In framing an estimate of a sampled attribute one passes through a set of stages, each of which provides a closer approximation of the magnitude in question. At each stage one includes more of the relevant information needed to select an efficient sample. This new information is provided by the results of each of the preceding samples. By such successive stages one moves from an initial crude approximation to a refined statement. The roles in a system of social action become defined by comparable stages. The first crude approximation represents the best that the student can do with the data available from a given set of interviews. Employing the role in tentative fashion he can proceed to interview further, guiding his interviews along the lines indicated by the axes of the role. Whatever new information comes to light through these succeeding interviews is then used to reconstruct the role, which can then be used for more refined interviewing.

By a series of successive approximations one abstracts from the raw data of observation the constructed roles which eventually can be the basis for predicting the actions of the members of a social system. Such prediction is the goal of role theory. Estimation is the comparable goal of sampling procedures.

A further parallel has to do with the phenomenon of error. The sampling procedures alluded to in this paper serve to keep the error of estimate small and also to permit one to state the range of error. In the case of role theory the successive approximations of the role are of the same order. The logical operations of the investigator in this case are equivalent to the mathematical operations of the statistician. The role never becomes a precise representation of a part of the system of action, but the error is reduced. It must of course be conceded that, at the present time, the operations of the role theorist lack the clarity and elegance of the mathematical operations of the statistician. On the other hand it may be suspected that the statistician sometimes works himself into the situation where his statement of error is the most precise and meaningful part of his total statement.

Error arises not only during these processes of abstraction but also from failure to discriminate at the point of observation. This type of error cannot be exorcised by any type of subsequent operation. It would seem, however, that successive samplings and successive interviews function in identical fashion to reduce the error arising from faulty discrimination at the point where enumeration or observation is going on.

This paper has argued that the people who call themselves sociologists have been accumulating a body of research techniques, somewhat unrelated, which deserve an effort at integration. They also have been developing a body of theory, concerning roles and social organization, which can be used to guide research. A critical appraisal of these assets may constitute a step toward more significant contributions to sociological research.

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