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Not so long ago the words "social engineer" were a term of opprobrium. They carried with them the suspicion that such a social scientist had somehow betrayed his vow of dispassionate objectivity and had sold his scientific heritage for a tasteless mess of popularity. This fastidious judgment was congenial to a stable society confident in the capacity of its established routines to cope with familiar tensions. It made sense also in a culture which had an unshaken belief in progress, rationality, and justice, and an optimistic faith that each new generation would automatically outdistance its predecessors. (7) As these assumptions no longer appear transparently self-evident, there emerge such pragmatic disciplines as disaster research, industrial sociology, military sociology, propaganda and communications research, and group dynamics—to mention only a few. Today, the growth of such organizations as HUMRO, RAND Corporation, The Air Forces Institute, and others, indicates the rapid transition to a more honorific and powerful place for the applied social sciences.

The applied social sciences have shifted for themselves, growing rapidly but in a trial-and-error fashion and with little assistance from the theorist. Traditionally, sociological theory has ministered to the needs of pure or basic researches, rather than to those of applied research. Indeed, the casual observer may almost think it a contradiction in terms to speak of a "methodology" of the applied social sciences. Yet the fact is that the applied social sciences are badly in want of such a methodology. For as a result of this deficiency, the very meaning and character of "applied social science" remain obscure and those concerned with it often reflexively reiterate received formulae.

A variety of dubious assumptions, some explicit and some tacit, are now commonly made concerning the nature of applied social science. Unless these

assumptions are given serious inspection they may become sacred items of faith rather than serving as useful guides to work. They can harden into a professional catechism which compulsively shapes future activities in the applied social sciences in ways that prematurely preclude lines of development which could prove fruitful. In the pages that follow several such assumptions will be subjected to re-examination. These are: (a) that an applied social science is one which applies the principles of pure or basic disciplines to practical problems; (b) that there is only one type of applied social science; (c) that applied social scientists cannot specify ends or values for their clients; (d) that resistance to the practical utilization of social science derives mainly from the inadequacy of present day research methods.

Social Science: Pure and Applied

To begin with the first assumption, it is all too commonly held that an applied sociology is "nothing but" the application of generalizations, developed by pure sociology, to concrete and practical cases. For example, in a seminar at Chicago University in 1937, A. R. Radcliffe-Brown commented: "There is . . . a very close relationship between theoretical natural science and applied natural science. Applied science is still science . . . it consists of propositions, but it consists essentially in the application of the knowledge which belongs to theoretical science to the practical problems which are met with in the application of the arts." (1) Fifteen years later essentially the same conception of applied anthropology was advanced by Darryl Forde at The International Symposium on Anthropology. (6) Russell Newman's paper on "Applied Anthropometry" (19) , at the same meeting, was prefaced with an approving reference to Webster's dictionary definition of applied science as "using and adapting abstract principles and theory in connection with concrete problems, especially with a utilitarian aim."

Though much reiterated, it would seem that this conception of applied social science is misleading if not inaccurate. There are in present day sociology few validated laws or broad generalizations; nonetheless, as the above comments indicate, there is a great acceleration of applied social science. There seems to be no close correlation, therefore, between the development of generalizations by the pure disciplines and the multiplication of opportunities for, and varieties of, applied sociology. The applied sciences cannot be fruitfully regarded as springing Athena-like from the furrowed brow of the pure disciplines. Any metaphor which conceives of applied social science as the offspring, and of the basic disciplines as parents, is misleading. It obscures the point that the applied sciences often contribute as much to pure science as they receive from it.

Perhaps the truth of the matter is that the applied social scientist presently makes use of the *concepts* rather than the generalized propositions of pure social

science. For example, anthropologists who have turned to applied endeavors often begin by asking themselves how the concept of "culture" can illuminate their particular problem. This would seem to be the point that George Foster makes in his account of research into Latin American health programs, when he comments, "The research problem was defined in the following general terms: how can the anthropological axiom—'in order to work with a people it is essential to understand their culture'—be translated into terms that would be meaningful to administrators." (7) In like manner, much of market research makes more use of the concept of "social class," to aid it in analyzing differential consuming habits, than it does of specific propositions about the behavior of social classes. Stated differently, applied social science seems to use "general orientations," which focus attention on patterns of behavior and belief that are systematically neglected by practical men, rather than using propositions which could generate specific hypotheses about this behavior. (15)

In the standard view of the relationship between applied and pure social science there is the tacit assumption that the development of the applied social sciences requires no special planning and theoretical analyses. It is assumed they possess no distinctive problems and that, with the maturation of the basic disciplines, all that will be required is to transfer their developments, like carrying bones from an old graveyard to a new one. It is in this vein that Goode and Hatt report that there is a "belief that science has best been able to achieve practical results when no goals other than those of science are considered. Those who hold this position maintain that if scientists are allowed to pursue problems dictated purely by theoretical concerns, the growth of science and hence the growth of its potential applications will be served." (9) The thought is scarcely entertained, however, that the applied and pure disciplines may have differences in their basic interests and thus in their very conceptual roots.

It is an open question whether all theoretical systems or conceptual schemes, in pure social science, have equal relevance and value for applied social science. An applied social science is above all concerned with the prediction and production of social and cultural change. As Thelen has suggested, an applied social science is a technology and, as such, requires "a set of principles useful to bring about change toward desired ends." (26) Eliot Chapple has, in fact, defined applied anthropology as "that aspect of anthropology which deals with the description of changes in human relations and in the isolation of the principles that control them. Perhaps it should also be emphasized that such a definition, by necessity, includes an examination of those factors which restrict the possibility of change in human organization." (3) There is little doubt that the central focus of all the applied social sciences is on the problem of social and cultural change.

In contrast, however, many of the current models of pure sociology have not developed an analysis of change, often having little or nothing to say about

this. Applied social science requires concepts enabling it to deal with change, while much of pure social science today is oriented to the analysis of stable structures in their equilibrium. (17) As a result, the objectives of applied social science often fail to articulate with, or derive little aid from, the models and concepts of pure social science. In this connection, there is a very instructive case in the work of Talcott Parsons, which reflects this disparity between the requirements of applied social science and current models of pure sociological theory. (20)

In Parsons' analysis of "The Problem of Controlled Institutional Change," a work in applied sociology, he attempts to develop a strategy for changing conquered Germany after World War II. In this article Parsons stresses the significance of "internal conflicts" in Germany as a tactical lever for the production of change. While the equilibrium model which Parsons normally uses in his pure theory ignores internal tensions, the problems of preparing a plan for changing German society apparently constrained Parsons to give this concept a much more salient position.

Moreover, in this same article much use is made of "class" concepts— e.g., in appraising the vulnerable position of the Junkers or in planning to modify the recruiting pattern of the German civil service—although such concepts are normally but little stressed in his pure equilibrium theory. There is, then, a strong suggestion in Parsons' work that the conceptual requirements of even his own efforts in applied sociology were not well served by his own model of pure theory.* It seems evident that the needs of an applied social science, which must above all cope with social change, are not met by all models of present-day pure theory. An applied social science cannot, therefore, be regarded as entailing the simple transfer of either the established propositions or the concepts of pure science to practical purposes. Even if a fully mature basic social science existed, the applied social sciences might still be handicapped if the former failed to be organized around concepts and models useful to the applied fields, and particularly if it failed to focus centrally on the problem of change.

The suspicion that the applied behavior sciences do suffer from this handicap grows stronger if attention is directed to one crucial case: namely, that what is probably the most successful of the applied psychologies, psychoanalysis, did not develop by way of transferring the established principles of pure academic

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*It needs to be pointed out, however, that Parsons' work on "The Problem of Controlled Institutional Change" was completed before the maturation of his equilibrium model. This, however, is not the case with respect to his interesting piece on " 'McCarthyism' and American Social Tension" (The Yale Review, Winter, 1955), which is also, I believe, vulnerable to a similar interpretation. Moreover, the former article on Germany was also clearly divergent from the pure voluntaristic model which Parsons had earlier formulated in his Structure of Social Action.

psychology to clinical problems. It seems instead to have been marked, from its very inception, by conceptual and theoretical innovations.*

Let there be no mistake about the meaning here: it is *not* being said that applied social sciences should not use or have not used the general principles and concepts of the basic disciplines. They may and have done so where they could. The actual relation between applied and basic social science is an empirical problem; we need many detailed case histories describing these relations as they have developed. Such researches, however, would be sorely misguided if they accepted the pat assumption now current concerning these relations as their guiding hypothesis. The following may instead be regarded as more favored hypotheses: (a) Applied social scientists are more likely to use the concepts than the generalized propositions of their basic discipline. (b) Not all concepts or theoretical models of pure social science are equally useful to applied social scientists. (c) Applied social scientists will more likely borrow from their basic disciplines those concepts and theoretical models which aid them in understanding or producing changes. (d) When the basic discipline does not provide theoretical systems or concepts aiding the applied social scientist to deal with change, the latter will develop these himself. (5) These new concepts will, in turn, exert pressure to produce modifications in the theories of the basic disciplines.

What implications follow from this analysis of the relations between pure and applied social science? Among others, it would seem that any discouragement of applied social science on the ground that it should not run too far ahead of pure science, and that its own development should await prior conceptual maturation of the pure sciences, is ill-advised. The applied social scientist cannot assume that theoretical guidance and aid will always derive from the efforts of the pure social scientist; he must be trained and prepared to make his own theoretical innovations. For unless he does so, his work may be in some ways impeded—even if it is in other ways aided—by the pure scientist, and especially by the latter's inclination to neglect the theory of social change.

One such theoretical innovation already attributable to applied behavioral scientists is the concept of "resistance to change." (8, 11) This is a concept which has derived largely from the work of the Freudians in psychology and the Marxians in sociology, both of them preeminently applied disciplines. Similarly, it is notable that the concept of "informal organization" emerged out of work in applied industrial sociology, where it was employed to account for

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*Psychoanalysis of course established its own pure theoretical model of substantive psychology, but this was based upon and largely derived from its applied clinical interests. As Freud sometimes stressed, his pure theory derived from his practical experience as a clinician.

resistances to industrial change.* Thus, in the Western Electric study, Roethlis-berger and Dickson comment that the social function of the informal organiza-tion among the "bank wiremen" served to "protect the group from outside interference by manifesting a strong resistance to change, or threat of change. . . ." (22)

Our analysis also has implications for the pure social scientist as well. Not only does it reinforce him in his efforts to develop a theory of social change, but it also specifically indicates one further way in which this can be done. It has been suggested that applied social scientists are constrained to develop concepts useful in the analysis of social change. It follows, then, that the pure social scientist may well derive some cues, for the formulation of a theory of change, by keeping abreast of and by making a close analysis of developments in applied fields. For by doing so, he may identify useful conceptual innovations which have "spontaneously" emerged there. Indeed, this already seems to have been done by Parsons, who has given a central place to the concept of "resis-tance to change" in his pure theory of social change. (21)

Engineering and Clinical Sociology

There is a second key assumption which seems to shape the growth of the applied social sciences. While it is never explicitly stated, it is nonetheless of considerable influence. This assumption seems to be that there is but one type of, or one model for, applied social science. In the pages that follow the suggestion will be made that there are at least two significantly different models available for applied social science, the "engineering" and the "clinical," and an attempt will be made to clarify a few of their underlying differences.

The distinction between an engineering and a clinical approach can be considered initially by inspecting a typical case, derived from my own experi-ence, of an engineering research in the social sciences. An industrial concern contracts with a "management consulting " firm to conduct an employee atti-tude survey among its own employees. The stated aims of this research are to determine whether employees are satisfied with their working conditions, hours, wages, or supervisors. By and large, the consulting firm consents to do this on the terms specified by the hiring company. In the end, the consultant conveys a report to the company which indicates the percentage of employees who are satisfied with their wages, their supervision, or their chances for promotion. Not uncommonly, this report may also include some recommendations for

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*I am, of course, aware that the concept of the "informal group" is now widely regarded as a "rediscovery" of the concept of the "primary group." This, however, overstates the continuity between the two concepts and fails to take as problematic the *differences* between the two, differences which are significant precisely in the context of an applied sociology.

changes in the company's labor relations policies. Usually, the company management invites the consultant to a discussion concerning the implications of these findings. Then, after a decent interval, the report may be quietly interred in that great graveyard of creativity, the filing room. Although crudely outlined, this is probably a representative history of the engineering type of applied social research. It is often with such a case in mind that people discuss the "gap between research and policy-making."

Notice that in the above example the consulting "engineer" has conceived and completed his assignment largely in terms formulated by his client. The consultant has failed to ask himself just why it was that the company management requested this survey in the first place; what kinds of problems produced a felt need for such a research among the company people; and will these problems persist even after the proposed survey is successfully completed according to management's prescriptions?

Many industrial sociologists would concur in believing that, underlying a request for an employee attitude survey, there usually exist a number of vaguely sensed tensions. For example, there has probably been some attenuation of informal communication between management and the worker. In short, the employee attitude survey may well serve as a functional equivalent for informal networks of communication which have deteriorated.

Such a survey, however, usually does little to alert the client to the existence of this underlying problem. Still less does the survey mend the ruptured informal channels, however much it supplies reliable data about employee attitudes. Indeed, the survey now makes it easier to continue operation *despite* the breakdown in informal organization. To that extent, then, the survey paradoxically preserves the very tensions which brought it into existence.

Again, an employee survey may also be used as a way of outflanking the union, by making it seem that management is better (because "scientifically") informed than the union leaders about the workers' feelings. In this case, one of the tensions promoting the research was a cleavage between management and the union. Here, once again, the tension is in no way mitigated by the use of the survey. If anything, the union feels increasingly threatened as a result of the research, and labor-management tensions are heightened rather than curtailed.

In contrast with these procedures, we may take a recent study in applied anthropology as a case which approximates, if it does not fully conform to, the clinical model. This is a project reported by Alan Holmberg which involved an Indian community in Peru, Hacienda Vicos. "When we first began to work at Vicos," writes Holmberg, "we soon discovered that one of the principal causes of in-group strife among the Indians was disagreements and fights over the ownership of cattle. . . . In view of this, it occurred to us—as it had apparently not occurred to the Indians—that one of the best ways in which to solve this problem would be to initiate a program of branding. This was suggested to the

Indian leaders who heartily agreed, as did the people themselves with whom we discussed this matter in a general assembly." (10)

Branding irons were then made and offers of assistance were advanced. At first few takers were found, whereupon the matter was again discussed with the Indian leaders. Only after the wealthier leaders themselves consented to have their own cattle branded did others follow suit. Finally, through this means, community disputes concerning ownership of cattle were eliminated.

Even from this brief account certain contrasts between the clinical and engineering models are already evident. Most importantly, the "clinicians" at Hacienda Vicos did not assume, as had the "engineers" in the management consulting firm, that their clients' own formulation of their problem could be taken at face value. Instead the clinicians took their clients' complaints and self-formulations as only one among a number of "symptoms" useful in helping them to arrive at their own diagnosis of the clients' problems. In the employee attitude study, the engineers studied what they were told to; at Hacienda Vicos, the clinicians made their own independent identification of the group's problems.

The "Value-Free" Assumption

Although this is only one difference between the engineers and clinicians, it is an extremely significant one. It is significant, above all, because it makes us re-examine one of the most cherished assumptions guiding work in the applied social sciences. This is the assumption that social science, pure or applied, cannot formulate and specify ends for its client group. Legitimated by references to the conceptions of a "value-free" social science, which were advanced by Max Weber and John Stuart Mill, many applied social scientists have claimed that all they can properly do is to study the diverse consequences of different policies, or to suggest efficient means for the realization of ends already specified by their client. (25)

The important questions concerning this assumption are pragmatic ones: To what extent does it truly describe the work of applied social scientists? To what extent does it provide clear and unambiguous directives for their actual operations? Is this assumption likely to be as congenial to engineers as to clinicians? There are many problems which the applied social scientist confronts for which this assumption, treated as a directive, provides no solutions. And there are many operations in which the applied social scientist engages which this assumption, treated as a description, does not accurately portray.

For example, in the event of employment by a client whose values differ from those of the group whom the applied scientist is asked to change, with whose values and to whose ends shall the scientist conform? If the work of industrial sociologists exhibits little uncertainty in this matter, the work of applied anthropologists employed by colonial governments evidences considerable

uneasiness and perplexity. (5) Furthermore, suppose the client does not know what his values are, or suppose he does not know in what priority to order his values? As sociologists very well know, this is a cultural condition which is very likely to give rise to all manner of tensions for the client. Is the applied scientist to deny assistance in these matters, to refuse to help his client formulate his values and goals, under the justification that his is a value-free science? And if he does aid his client in specifying his ends—as evidenced for example by the work at Hacienda Vicos—then is the scientist giving more than "lip service" to the postulate that he should not specify ends for his client?

Again, what of the client who pursues values which may be somewhat incompatible—e.g., desegregation vs. political stability? (28) Should not the applied social scientist somehow indicate that the client's own values may be somewhat incompatible and that this incompatibility may be generating tensions for him? And if the applied scientist does these things, is he not then influencing the values of his client group? If the postulate of a value-free social science is not an accurate description of what applied social scientists do, and, above all, if this postulate is not translatable into clear-cut, unambiguous, operational directives, facilitating the applied scientist's solution of his professional problems, then the postulate itself—if not operationally meaningless—would seem to be in need of consideration respecification. This is not to imply that the postulate, as presently formulated, is totally useless. For the postulate of a value-free social science may be most useful as an ideological mechanism. That is, it may successfully serve the social scientist as an instrument of status defense, deflecting the suspicions of client groups who fear that the social scientist wishes to impose his own values upon them and is a silent competitor for administrative power. (13)

In any event, engineers and clinicians among applied social scientists seem to differ with respect to their interpretation of the value-free postulate. The clinician is less likely to take his client's own values as given, and he establishes a relation with the client in which they may legitimately come up for re-examination in the light of their connection with the client's problems.

There are many other respects in which clinician and engineer apparently differ and, in the remaining space, only a few of these can be examined. It will have been noted that the "clinicians" at Hacienda Vicos carefully consulted with all who would be affected by their diagnosis and proposed remedy of that community's problems. In contrast, the management "engineers" conferred with only one segment of the group, namely, the top echelon; they did not consult with the workers.

One reason for this difference is the differing anticipations which clinicians and engineers have concerning client resistance to their findings, and their differing interpretations of the sources of this resistance. The engineer fatalistically assumes that resistance to his findings is not his legitimate problem and,

at worst, is due to the present deficiencies of his own research methods. He expects that inevitable improvements in research methods will sooner or later dissipate this resistance. (16) The clinician, however, assumes that findings produced by even the most perfect research technologies will continue to meet with resistance. He assumes that this resistance is his problem and that he has a responsibility for coping with it.

Assumptions Concerning Resistance

Without doubt inadequate research impairs the relations between applied social scientists and their clients, leading to many failures in the practical use of social science. But the client's resistance to social science findings is undoubtedly motivated by many considerations. Today no one is able to weight the various factors contributing to breakdowns in the scientist-client relationship. It is well known, however, that there are important cases where this breakdown cannot be attributed to the dereliction of the researcher or to the inadequacies of his research technology. This becomes evident when a research technology is employed in two comparable settings. In one case it is given successful application, and its findings are used by the client. In another very similar setting, however, this same research method will be employed but its findings are ignored and go unused. This seems to have been the case with personnel research which was successfully conducted and fully utilized by the Army Air Force during World War II, while the Navy made very little application of the personnel research which had been conducted for it. (23)

The experience of other applied disciplines also suggests, unfortunately, that the utilization of their findings is by no means entirely dependent upon their validity. It is noteworthy that physicians have sometimes been quite successful in securing acceptance of certain of their recommendations which were far from well validated and which, in fact, they themselves later rejected. For example, American doctors persuaded many parents to feed their infants on a rigorous and regular time schedule, say once every three hours, and even succeeded in diffusing this practice to certain parts of Latin America. Yet, later, the medical professions maintained that infants should be placed on a "demand schedule" and be fed as they wished. It seems evident that, in the case of personnel research, its scientific adequacy was not sufficient to secure its equal utilization in all cases, while the inadequacy of earlier infant feeding research was not sufficient to prevent its utilization.

Pure and applied scientists alike may be relied upon to improve their research technologies and, with this, the scope and reliability of their findings. By itself, however, this will not solve the utilization problem and will not automatically guarantee that these findings are successfully put to use. Applied social science does have to contend with a kind of client resistance which has nothing to do with the deficiencies of scientific research. As suggested by the

situation at Hacienda Vicos, clinicians, unlike engineers, fully anticipate and systematically prepare to cope with such client resistance.

They never suppose that client resistance is solely, or even mainly, reinforced by the researcher's ignorance or incompetence. It is clear, for example, that we do know a great deal about certain fields, for example, about criminology and penology, not to speak of ethnic discrimination and prejudice. Nonetheless, it also painfully clear that this knowledge is grudgingly put to use, if at all. Indeed, it may well be true, as some psychiatric clinicians avow, that the *nearer* the social scientist approaches to the nerve centers of his client's problems, the more resistant the client becomes.

There are many reasons for resistance to the findings of social research, other than those residing in the defects of the research itself. One reason may be, as the Freudians and others have insisted, that the client actually derives certain satisfactions or gains from his disturbances. As a result, he is not entirely and singlemindedly ready to accept knowledge which exerts pressure to remedy these problems. Another reason may be that the research itself may serve as one or another form of defense mechanism. In brief, the client sometimes undertakes a research so that he does *not* have to solve certain problems, and so that he need *not* change. In this case, the very conduct of research provides participation in a problem-solving ceremonial. It is a ritual particularly pleasing to the consciences of men reared in a rational tradition. Moreover, it provides a publicly evident token of the client's good faith and of his sincere interest in resolving the problem. But it does not inevitably entail the client's commitment to the conclusions of the research, or to the recommendations for change which may be proposed.

Kenneth Burke, a gifted sociologist who obstinately calls himself a literary critic, has termed this pattern of resistance the "Hamletic strategy." Named after the Great Procrastinator, this pattern of resistance is one in which the very preparations for action are transmuted into devices for postponing action. Nor is this always a matter of *unconscious* resistance. As Burke reminds us, "we may note how legislatures regularly adopt the 'Hamletic' strategy as a way to avoid embarrassing decisions. For if you would forestall a final vote on a measure, and would do so in the best 'scientific' spirit, you need but appoint a committee empowered to find more facts on the subject." (2)

In attempting to account for the resistance to social science findings and the failure to utilize them fully for practical purposes, some emphasis has recently been placed on the status of the social scientist, which is often lower than his client's. The point has been well made that "other things being equal, the amount of utilization is likely to increase with esteem for a science and its practitioners." (23) While this is undoubtedly correct, nonetheless it must be understood that the social scientist has a complex social role which involves much more than hierarchical qualities such as prestige, power, or class. This

role consists of a culturally standardized complex of expectations and definitions of function, which leads the social scientist to develop his relationships with clients in specific ways. To understand properly the failure to use social science findings, it would seem useful to examine not only the social scientist's prestige but the other aspects of his role as well, his role conceptions, and the resultant patterns of interaction with his client. It may be useful, therefore, to examine some of the differences between the clinical and engineering models, in terms of the varying role definitions which they entail.

The Engineering Model

Up to the present, the dominant role definitions of researcher and policy-maker, adopted by most sociologists, have been cast in the classic utilitarian mold. That is, the policy-maker defines his difficulties as deriving from inadequate knowledge. He formally operates on the assumption that, if he only had greater knowledge, his problems would capitulate. It is with this in mind, presumably, that he calls upon the applied sociologist. The policy-maker also tends to assume that the inadequacy of his knowledge is somehow accidental or a matter of neglect. He rarely entertains the dismaying thought that his very ignorance may be functional to him.

The applied sociologist who accepts such a definition of his client's role is more likely to conform to the engineering model and to define himself, in turn, as the bearer of facts and figures. He assumes that the client really wants to solve the problems of which he complains. The engineering sociologist recognizes, of course, that he has a job of "communication" to do. But the engineering sociologist is prone to regard this communication as well done if he reduces his report to fourteen-word sentences and mimeographs it neatly on multi-colored paper. As Wilbur Schramm puts it, "Utilization is sometimes thought of as a process of 'telling people'—writing better pamphlets, drawing better charts, making more and better teaching films, cranking up the transmitters of the mass media. This is clearly an inadequate picture." (23) Inadequate though it is, this is very much the way in which the engineers among the applied social scientists approach the problem of the utilization of social science. It is a fascinating anomaly that, while utilitarianism has been expunged from the *theories* of most sociologists, utilitarian assumptions such as those above still remain deeply embedded in their own role relations with clients. Their heads protrude into the twentieth century, but they shall remain among the half-born so long as their feet are still rooted in the nineteenth century.

The role conceptions of applied social scientists are, of course, still very much in flux and are taking new shapes as they are subjected to new client pressures and temptations. Unaware that the utilization process is, as Schramm calls it, a two-way hook-up, the engineers are particularly vulnerable to an

unwitting redefinition of their roles in ways which obliterate their professional distinctiveness and identity.

Thus one finds the "policy scientists" taking over whole the military language of their clients, or would-be clients, and talking, for example, about the need for "intelligence" rather than for information or data. (14) The general tone of their writing has the atmosphere of a military staff issuing urgent directives, mobilizing resources, and preparing for battle. Their rediscovery that ours is "one world" takes on the flavor of geopolitics; their insistence upon "time factors" is devoid of the humanism of the historian and has, instead, the perspective of the tactician. Their new self-images apparently emphasize tough-mindedness, worldliness, and realism, which are well oriented to the military crisis of our time and well adapted for interaction with a military elite. It is another and more doubtful matter, however, whether these new self-images of the engineering sociologists are equally valuable for the development of an independent and self-conscious social science, pure or applied.

The Clinical Model

A point has now been reached where some of the characteristics of the clinical model can be brought into sharper focus. There are a great variety of such characteristics which need to be clarified; here, however, the clinical model will only be considered as a social system, particularly as it is expressed in its distinctive role relations with clients. (12, 27) (a) From an engineering standpoint, the problems as formulated by the client are usually taken at face value; the engineer tends to assume that his client is willing to reveal the problems which actually beset him. The clinical sociologist, however, makes his own independent diagnosis of the client's problems. He assumes that the problems as formulated by the client may often have a defensive significance and may obscure, rather than reveal, the client's tensions. Not only does the clinician assume that the client may have some difficulty in formulating his own problems but he assumes, further, that such an inability may in some sense be motivated, and that the client is not entirely willing to have these problems explored or remedied. The clinician, therefore, does not take his client's formulations at their face value, any more than he does comments made by an ordinary interviewee; but he does use them as points of departure in locating the client's latent problems. As Emile Durkheim (who more than any other classical sociologist used a clinical model) remarked: ". . . a sick man faultily interprets the feelings that he experiences and most often attributes them to a cause which is not the true one. But these feelings, such as they are, have their interest, and the clinician notes them with great care and takes them seriously. They are an element in the diagnosis, and an important one. . . he is not indifferent as to where they are felt, when they began." (4)

(b) The engineer focusses largely on his relations with those from whom he secures the information necessary to fill his order. He is concerned, for example, about problems of sampling, questionnaire design, or interviewing technology largely as these affect his data collection from respondents, In contrast, the clinical sociologist takes his relationship with his *client* as seriously as he does his relations with interviewees. The clinician does not allow his relationship with his client to be governed by the all-too-common "come back and see me when you've done something" approach. He attempts to arrange his relationship with a client so as to secure the latter's consent to examine the underlying problems of his group.

(c) The engineering sociologist expects his findings to be accepted by his client, and particularly so if they have been acquired in conformity with the best canons of scientific research. The clinical sociologist, however, expects his clients to resist his findings, perhaps because "he that increaseth knowledge increaseth sorrow." The engineering sociologist assumes that his relationship with his client is regulated by the postulate that ignorance is evil, and knowledge power, and that men unequivocally prefer enlightenment to ignorance. Writing in what may be regarded as an engineering vein, E. A. Shils comments, "Truth is always useful to those who exercise power, regardless of whether they wish to share that truth with those over whom their power is exercised. . . ." (24) This is very dubious. Men in power are not merely technicians, concerned solely about the use of effective means to their ends; they are also politicians, committed to morally tinged precepts and symbols, and striving like all other men to maintain a decent self-image. (18) Truths which are inconsistent with their own self-images are demoralizing and thus, in this very real sense, by no means "useful" to them. By assuming that his client wishes to learn the truth, the engineering sociologist has confused an ethical imperative with a description of the learning process. When the applied sociologist recognizes that he has the problem of helping his client *learn* something, and when he recognizes that learning is not accomplished by fact-finding or "communication" techniques alone, then he is on his way to becoming a clinician. Unlike the engineer, the clinician seeks to identify the specific sources of the client's resistance to his findings and he attempts to develop and learn new skills enabling him to cope with his resistance.

It needs to be underscored that these are only a few of the differences between an engineering and clinical sociology. It should also be remembered that there has been a focus on their differences, and a resultant neglect of the similarities which they both share as applied sociologies. What has been attempted were approximate models of the clinical and engineering approaches; any given piece of applied sociology may therefore possess some characteristics of both models. Furthermore, despite this writer's interest in the clinical model, it should not be supposed that he sees no value in the engineering model and

no difficulties in the clinical. If the engineer lacks a sophisticated conception of the client relation and an adequate appreciation of the depth and meaning of client resistance, the clinician typically lacks a sophisticated conception of research design and technology. Moreover, one may well be concerned about the practical possibilities of securing client acceptance of the clinical model in relations with groups—as distinct from individuals—and particularly with large scale organizations. Undoubtedly there are important difficulties here, but as the work proceeding at the Tavistock Institute suggests, not insurmountable ones.

An applied sociology has much to learn from the clinical disciplines. It should not be assumed, however, as is so often done these days, that the only clinical discipline which can usefully serve as a concrete model is psychoanalysis. There is much to be learned from it, particularly if it is constantly borne in mind that psychoanalysis is an applied *psychology*. As sociologists we are interested only in borrowing elements which are properly applicable to the analysis of *groups*, or for the development of change-inducing relations with them.

Physical medicine itself, or bacteriology, to name only two other clinical disciplines, may be just as valuable as psychoanalysis for the development of a clinical sociology. What we happen to know best is not necessarily what we can best use. Nor should it be supposed that a clinical sociology is characterized primarily by the use of one or another therapeutic device, such as "consultative" or "nondirective" methods. Such devices are probably better suited to a clinical than an engineering sociology. The clinicians' basic commitment, however, is not to a particular therapeutic technique, but, rather, to a distinctive role definition. In short, a clinical discipline is not as such a psychological discipline, nor is it distinguished by a cultish commitment to any specific change-agent.

In fine, then, it has been proposed that applied sociology can profit by deliberately modeling itself, particularly its strategy of client relations, on the *several* clinical disciplines and by adapting them to its own needs. To do so effectively, however, it will have to examine reflectively and to codify systematically the elements of clinical activity in the variety of disciplines where they are presently employed. In this way, we may yet fashion a new branch of applied sociology, a clinical sociology which can aid in mending the rift between the policy maker and the social scientist and in helping groups in their time of trouble.

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