

THE SOCIOLOGY OF EMPIRICAL SOCIAL RESEARCH *

PAUL F. LAZARSFELD

Columbia University

This presidential address deals with the interrelation between the organization of social research and methodology. Five general points are made: (1) empirical research requires a specific kind of organization, namely, institutes; (2) induced sensitivity to methodology can be fruitful for general sociological analysis; (3) the contemporary scene in social research must be understood in appropriate historical context; (4) today's social research institutes raise important organizational problems and have broad implications both for the teaching of sociology and for university administration; and (5) the substantive work these institutes are carrying out needs to, and soon will, undergo considerable broadening.

To choose a topic for a presidential address is a rather frightening experience. More irrevocable than marriage, more self-revealing than a dream, it forces one to assign priorities to a variety of interests which have long remained lazily undecided. Between the time when the American Sociological Society was organized in 1906 and the first world war, its presidents were more fortunate. Elected for two years, they could give two addresses. One was usually devoted to a specific sociological problem that concerned them, and the other to a kind of state of the union message, in which they discussed matters currently of concern to our profession at large. At first, I thought I would have to make a choice between the two types. Recently I completed a preliminary survey of organized research in this country wherein I investigated where we stand with respect to our research centers, social relations laboratories, our bureaus of applied social research. This seemed to me an urgent professional problem and a good

topic for tonight's discussion. But I also had a theoretical candidate. You all know the old saying: those who can, do; those who cannot, teach; and those who have nothing to teach, become methodologists. I always felt that this is an unfair misunderstanding of methodology, and tonight's occasion seemed an opportunity for clarification. I finally decided to combine the two topics and to center my remarks on the interrelation between the organization of social research and methodology. This involves the following five points:

1. Empirical research requires a specific kind of organization which I shall call "institutes." These institutes in turn generate a bent of mind, a way of reflecting on research procedures which I shall call "methodology."

2. Such induced sensitivity to methodology can be fruitful for general sociological analysis in areas far afield from what we think of as empirical studies.

3. To understand the contemporary scene it is necessary to provide some facts and raise additional questions on the history of empirical social research in Europe, as well as in the United States.

4. Today's social research institutes in this country are a very recent development, raise interesting organizational problems of their own, and have broad implications for the teaching of sociology and, perhaps, even for the future of our university administration in general.

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5. The substantive work these institutes carry out needs to, and soon will, undergo a considerable broadening which I shall specify at the end of my remarks.

There is not time enough to elaborate any of these points in detail. Consequently, I shall occasionally use, as a device for speedier communication, illustrations taken from my own academic career. I have repeatedly advocated that sociologists should give accounts of the way their interests and writings actually develop and I am ready, therefore, to take my own prescription and trace back some of the things you know I stand for. In a way, this is an effort to draw generalizations from a single case. And my first point is indeed best introduced by a personal reminiscence.

THE METHODOLOGY—INSTITUTE SYNDROME

When I joined the staff of the University of Vienna, some thirty-five years ago, one of my first assignments was to review a large body of data on the occupational choices of young people. It was easy to see the regularities of their choices. They were linked with social stratification, and permitted one to interpret age differences in terms of a general theory of adolescence. The available studies also contained many tabulations of the *reasons* for the choices. But this material was contradictory; no sense could be made of it. My attention shifted to the problem of what was wrong with the mode of investigation. And here I noticed an ambiguity in the question "why". Some youngsters answered in terms of the influences to which they had been subjected, while others talked about the attractive features of the jobs under consideration. Still others referred to broader personal goals which they hoped would be served by a particular occupation. An investigator's lack of skill in the art of asking "why" led to meaningless statistical results.

The study was originally inspired by a program of research on adolescents laid out by my Viennese teacher, Charlotte Buehler. But, in my own biography, it led to a sequence of studies on choices: how could one find out why people bought one product rather than another, why they voted the way they did, why they listened to certain radio programs, and so forth. Ever since, I have

continued to search for sound ways for making empirical studies of action. When can one use retrospective interviews? When is it better to use panel studies (repeated observations of people in the process of choice)? When are decisions best understood by considering the social context—the school, the factory or other organizations—within which the choice was made? And in all this, of course, I find myself puzzled by a theory of action where no one ever acts, and by modern mathematical decision theory where people act on probabilities of future events and utilities of outcomes, but where no one ever asks from which social and experiential background these estimates arise.

A program for the empirical study of action required a staff of collaborators trained to collect and analyze data whenever a research opportunity offered itself. I obtained permission from my academic superiors to create, in Vienna, a research center very similar to the kind of American institutes I shall discuss presently. It antedates, as far as I know, all such university institutions in this country except the one at the University of North Carolina created by Howard Odum. Now, supervising even a small research staff makes one acutely aware of the differences between various elements of a research operation and of the need to integrate them into a final product. Some assistants are best at detailed interviews, others are gifted in the handling of statistical tables, still others are especially good at searching for possible contributions from existing literature. The different roles must be made explicit; each has to know what is expected of him and how his task is related to the work of the others. Thus, staff instruction quickly turns into methodological explication. Maintaining the intellectual standards of an institute is tantamount to codifying empirical social research as an autonomous intellectual world.

But this is not the end of the story. When one is responsible for directing research, abstract sociological issues turn into down-to-earth challenges. It is not enough to develop constructive typologies; one must decide under which type a particular person or group actually should be classified. One cannot just ponder over the nature of causality; one must give concrete evidence as to why a certain election was won or lost.

At this point the tables are often turned. The research operation can provide the model which helps to clarify and unify problems that arise in spheres of inquiry far removed from empirical social research in its narrower sense. And this is my second point: Methodology can often give aid to social theory.

RELATIONS BETWEEN METHODOLOGY AND SOCIAL THEORY

Permit me to consider with you one example of the relation between methodology and social theory. Many of you, I am sure, are acquainted with the notion of an attribute space. It starts with the observation that objects can be described along a number of dimensions. Think, for example, of an IBM card on which people are described by sex, race, education, etc. In such a space, regions can be combined to form typologies. Thus sex and employment status permit many combinations; but for certain purposes it makes sense to distinguish just three; men, irrespective of their work, and women according to whether they are housewives or work outside.

This reduction of a combinatorial system of attributes to a smaller number of types has a counterpart which I have called a *substruction*. Beginning with a typology, or simply a list of objects, we ask ourselves in what way they have originated from an attribute space. The linguists do that today. They take the basic sounds of languages—the phonemes—and look for the minimum number of attributes of which these phonemes are combinations. Such a “binary description” of language leads to characteristics such as nasal/oral, strident/mellow, tense/lax. Any real language occupies only certain regions of the attribute space and leaves others empty.

In empirical social research we come across this substruction whenever we wish systematically to classify people or groups according to a proposed typology. My first encounter with this problem occurred when I worked with Erich Fromm on a study of authority in European families. He had distinguished four types: complete authority, simple authority, lack of authority, and rebellion. In order to use his ideas for an

empirical study, we had to introduce criteria or, more specifically, questionnaire items along two dimensions: the degree of authority the parents wanted to exercise and how much of it their children accepted. Each of the two dimensions was divided into three levels, giving nine combinations. Seven of them were easily reduced to Fromm's types, but two of them forced us to acknowledge a fifth type: families in which the children wanted more authority than the parents were inclined to impose. Substruction helped us discover a new type.

The relation between typologies and attribute spaces will be obvious to anyone who has converted people into questionnaires and finally into cross-tabulations. But what matters most now is the way such a formal observation clarifies more general sociological issues. You will remember that Max Weber gave ten criteria for a pure bureaucracy. We cannot deal tonight with a ten dimensional space, so suppose we arbitrarily select two of the ten criteria. Each officer has a clearly defined sphere of competence and he is appointed on the basis of technical qualification. This gives a two dimensional space you can visualize easily as a traditional system of x-y coordinates drawn on a piece of paper. Actual organizations will be points in this system according to the degree to which they exhibit the two characteristics, which we shall assume have been scaled from 0 to 5. And as a free gift we now know what an ideal type is: it is the region in the upper right corner, around the point with the coordinates 5/5.

How about the diagonally opposite point, the one with the coordinates 0/0? No one, as far as I know, has worked out in detail what a non-bureaucracy looks like. But in another area the relation of these two points is very familiar. I refer to Toennies' “Gemeinschaft and Gesellschaft,” Durkheim's “Société Mécanique and Organique,” Becker's “Sacred and Secular Society,” and Redfield's “Folk and Urban Continuum.” The most seminal effort to provide a substruction for this typology is Parsons' “Pattern Variables.” For the sake of simplicity, let us take two of the many dimensions which have been proposed for this typology—say, isolation and social homogeneity. Assume that they are somehow measured and entered

respectively on the two axes. Then the pure folk society is at 0/0 and the pure urban mass society is diagonally opposite at 5/5. Now a good model is supposed to generate more ideas than were put into it to begin with, and this one does. Our two corner points can be connected by any number of lines that can vary in shape and length. They turn out to be the paths along which the transition from the traditional to the modern social system can be found. I suggest to you an instructive parlor game. Take your favorite theory of social change—telling how one aspect of society affects other social dimensions—and translate it into lines connecting points in an attribute space. While you will not obtain an empirical answer, you will be helped by the clearer formulation of problems and by seeing unexpected connections between possible solutions.

I hope the example I have given has helped to back up the second item in the five point plan which guides me tonight. The technical and organizational nature of empirical social research leads to formal ideas, to distinctions and interconnections relevant for many sociological pursuits well beyond the realm of strictly empirical research. My position is akin to the kind of sociology of knowledge which Marxists employ when they stress that new tools of production are often reflected in new ways of intellectual analysis. I look at empirical research as an activity which is especially conducive to x-raying the anatomy, the basic logical framework of general social inquiry. This is, of course, not the only way to look at the situation. One could focus on the content of the empirical studies produced in recent years, and my concluding remarks are devoted to this substantive aspect. But let me elaborate for a moment on the context in which I see my formal emphasis—formal both in intellectual and in institutional terms. I have always been most curious about the *process* of production, the *structure* of a piece of work, the *way* people reach a specific intellectual goal. As an amateur musician, I find my enjoyment of music considerably enhanced if an expert explains the theoretical structure of a quartet. Knowing little about *belles lettres*, I am indebted to the “new criticism” because its internal analysis of a piece of writing opens an experience to which I would not otherwise

have access. This interest in “explications” was reinforced during my student days. It was in that period that the theory of relativity had come to the fore. We were greatly impressed by the fact that it came about not just through substantive findings, but also through the conceptual clarification of basic notions. I remember vividly the delight in discovering that it is not obviously clear what is meant when one says that two events, one on the sun and one on earth, occur “simultaneously.” I should add that reading a mathematical paper reinforces this tendency. Hours are spent on one page, trying first to guess what the author is driving at, then why he is concerned with this objective, and, finally, the understanding of his proof. (Proofs are usually presented in a direction opposite from the way in which a theorem was originally discovered.)

In my teaching, I try to convey this mood to students in various ways. We read empirical research closely and try to reconstruct how the author was led from one step to the next: what data he might have inspected but not reported; how the order of his final presentation might have developed from an originally vague and quite different imagery. Often an hour is spent just on analyzing a table of contents. It comes very close to what the French call “*explication de texte*,” a training which gives them such great expressive strength. Dilthey’s notion of hermeneutics, his general principles for interpreting philosophical systems, is echoed in this effort to make students understand that writing a term paper and publishing a book have more in common than they suppose. I have no evidence for the educational value of this methodological approach, but this does not keep me from being very convinced of its merits.

I now want to turn to some institutional problems and consequences which are indigenous to the way empirical studies are typically set up in contemporary American universities. This is more easily explained if I first insert some remarks on the history of social research.

A NOTE ON HISTORY OF SOCIAL RESEARCH

There are two leading facts in this history. First, its origins lay in early modern Europe

(it may be dated as far back as the Seventeenth Century), but in Europe it failed to develop as a regular branch of professional sociology. Second, in the United States, where it was destined to flourish, it existed long before it found organizational setting in our universities. Permit me to digress sufficiently to explain some of the remarkable circumstances these bare generalizations cover.

A series of studies now under way at Columbia University shows that practically all modern empirical techniques—our Latin American friends sometimes summarize them as Yankee Sociology—were developed in Europe. Sampling methods were derived as a sequence to Booth's survey of life and labor in London. Factor analysis was invented by the Englishman, Spearman. Family research, with special emphasis on quantification, came of age with the French mineralogist, LePlay. Gabriel Tarde advocated attitude measurement and communications research. (Looking at the contemporary French scene, one might well speak of his posthumous victory in his epic battle with Durkheim.) The idea of applying mathematical models to voting was elaborately worked out by Condorcet during the French Revolution. His contemporaries, Laplace and Lavoisier, conducted social surveys for the Revolutionary Government, and their student, the Belgian, Quetelet, finally and firmly established empirical social research under the title of "*physique sociale*." He did this, incidentally, to the great regret of Comte, who claimed that he had invented the term and now had to substitute for it a much less desirable linguistic concoction, to wit, sociology. In Italy, during the first part of our century, Niceforo developed clear ideas on the use of measurement in social and psychological problems, brilliantly summarized in his book on the measurement of progress. The Germans could claim a number of founding fathers: Max Weber was periodically enthusiastic about quantification, making many computations himself; Toennies invented a correlation coefficient of his own; and, during Easter vacations, von Wiese regularly took his students to villages so that they could see his concepts about social relations acted out in peasant families.

And yet, before 1933, nowhere in Western

Europe, did empirical research acquire prestige, a home in universities, financial support, textbooks, or enough devotees to form what I should like to call a critical mass: the number of people sufficient to maintain each other's interest by providing a reciprocal reference group. What accounts for this discontinuity in European sociology? All of these countries have today a large, albeit somewhat ambivalent, interest in empirical research, but why is it now experienced as an American invasion rather than what it is in reality: a revival of an autonomous European development? I do not know. Perhaps the ravages of two wars and the intervening fascist period kept western European sociology from taking the "operational jump" for which it was ready; or perhaps structural features of university life or of the general intellectual climate in Europe made it necessary for the breakthrough to come in a new country. Only a very careful analysis of the material published around 1930, here and in Western Europe, could give an answer.

In the United States another historical problem is puzzling. We know that concern with underprivileged groups led to various fact-finding efforts here, such as the work of the American Social Science Association around 1870, and the survey movement which began at the turn of the century and was later supported by the Russell Sage Foundation. But why did it take so long for the universities to find their proper place in this broad trend? The question will not be fully answered but the issue is well illustrated by the efforts of the University of Chicago to develop means by which ameliorative activities in the community and the research interests of academia could join forces. The facts I shall summarize have been assembled by Mr. Vernon Dibble as part of the Columbia University history program mentioned before.

You all know of Albion Small, the founder of the *American Journal of Sociology*, and one of our early presidents. He began his chairmanship of the Department of Sociology at the University of Chicago in 1893. One of the professors in this Department, a former minister, was appointed because of his knowledge, based on previous activities, of the needs of the Chicago community. I

refer to Charles R. Henderson, who, indeed, lived up to everyone's expectations. He wrote manuals for social workers on how to collect information that would help advance social legislation; he organized networks of social informants; he trained students who later became prominent in their own right. But, at that time in Chicago, it was assumed that the role of the University was to help the community solve its problems; the sociologist was not to carry out research himself. This was not a meaningful division of labor, however, and Henderson was soon forced to collect and analyze his own data. And yet the University structure had made no provision for this turn of affairs. Consequently, one day, Henderson, in a mood of desperation, wrote Albion Small that he just had to have an assistant at \$100 a month. He listed ten arguments in favor of this revolutionary idea. As a typical illustration let me quote his argument No. 8:

My department of study suffers unjustly in comparison with those of the physical sciences, with their costly equipment and corps of permanent assistants. I do not hope to be put on an equality with them, nor do I wish for them any diminution of equipment, but I want a little chance to demonstrate what can be done for the science of human welfare and furtherance of the higher life, with even a meagre supply of help.

The typewritten letter, dated February 1902, has a handwritten postscript: "Since writing the above I have learned that a similar arrangement to this proposal has been successfully tried at Columbia by Professor Giddings."

In March, Small wrote a three page letter to President Harper supporting Henderson's request as "part of a large program which we are all feeling that it is time to work out." This large program was empirical sociological research which would entitle "the University of Chicago to the leading place in that subject in the world, at least until some of the European universities shall realize the readjustment of interests that is going on."

Henderson obtained his assistant, but Small did not realize his great design. In 1914, a committee wrote a sixty page report on the need for a "bureau of social research" in Chicago. The bureau was not visualized

as a University activity. The plan was sponsored by the City Club of Chicago and signed by a committee of four, consisting of three businessmen and George Herbert Mead. As far as I can tell, it was never implemented.

In 1922, Small was willing to join forces with a man with whom he shared nothing but an institutional conviction. The then dean of the Business School at Chicago proposed a central research institute, ignoring all departmental divisions. In a letter to the president, Small not only approved the idea but stated that "It makes my heart bleed to fear that our own social science group will miss its birthright by failure to qualify for the opportunity." And he added his own version of how the system would work:

There should be "genuine commissions of inquiry" with a hierarchical order of work: graduate students would do the "assorting of materials and of organizing them in accordance with the findings of their more experienced seniors." But, so that this not become mere routine, "regular sessions of the seniors would be held with the graduate students present for thrashing out all the questions of principle involved." He wanted a cumulative continuity of such work. "The minutes of each inquiry, properly filed and indexed in the archives of the institution, would form an object lesson in the methodology of that type of inquiry and would be permanently instructive, both as to mistakes to be avoided in subsequent inquiries, and as to methods which proved to be useful. All this in addition to the substantive results of the investigation."

But even in 1922, the University was not ready for such a radical step. As a matter of fact, at about the same time, they refused a similar institute which Merriam had proposed for political science.

But, short of this, Small made great strides. He succeeded, often in the face of great resistance, in appointing men like Robert Park who introduced the guided dissertation into the Department of Sociology. Until then, the doctoral candidate followed the German pattern; he chose his topic, wrote his thesis in solitary confinement, and presented the final product to a professor who judged its intellectual merits. In Chicago in the 1920's the dissertation became part of a general program and was carried out in close contact with the sponsor. This was facilitated by Beardsley Ruml who deliberately used the Spellman Fund to make

empirical research a regular part of the graduate curriculum.

That was the time when the sociological work being done at Chicago was prominent in American Sociology. But, after the initiative of the great pioneers like Park, Burgess, Thomas, and Ogburn, this dominance waned. It is my guess that a more formal organization for social research would have extended the influence of these great Chicago leaders even after other graduate schools began to make their bid.

Small always stressed that empirical research did not concern him personally, but that the future of sociology as a discipline depended upon the discovery of an appropriate institutional form for its exercise. It is interesting, incidentally, that this part of his work is nowhere mentioned in the many papers written about him. And yet, it is to his great credit, as a sociologist, that he sensed something which has since been documented by historical investigation. Turning points in higher education have often hinged on some institutional innovation. The medieval universities became permanent institutions once Paris established the disputation as a way of training students. The humanist revolution revolved around the scrutiny of classical texts. The idea of the modern university began with the Berlin seminar, a group of students who did more than just listen, who, in fact, also conducted their own research under the guidance of a master. And the contemporary sciences—of nature as well as of society—required the laboratory.

It took fifty years before we began to face this problem realistically in sociology, and much research will be needed to clarify what delayed and what finally led to the social research institute becoming imbedded in the university structure. Among the early handicaps one can easily think of are reasons like these: too few graduate students to form teams with division of labor; lack of seniors who had themselves risen from the ranks of empirical research; and, of course, lack of funds. But only monographic studies of places like North Carolina, Wisconsin, and Columbia will bring light onto the issue. Let me now turn to a review of the contemporary scene.

THE SOCIAL RESEARCH INSTITUTE IN
THE AMERICAN UNIVERSITY

There are about one hundred universities in the United States which today give at least ten PhDs in all fields combined, including the natural sciences. Slightly less than two-thirds of these institutions have made arrangements to carry out social research. We draw our definition rather liberally: it can be either a unit specifically attached to a Department of Sociology; or an interdepartmental setup including the Department of Sociology; or, finally, a fairly permanent project to which at least one sociologist is attached, even though the Department does not participate officially. The programs of these agencies are either *specialized*, or they cover a broad range of topics and are, so to say, *general purpose* units. The former outnumber the latter by more than two to one. The "general purpose" units, in turn, divide rather evenly into those which are *autonomous*, in the sense that they develop their own programs, and those which see themselves mainly as *facilitating* the research activities of individual faculty members. The distinction is somewhat difficult to make because academic tradition favors the rhetoric of facilitation, while the inner dynamics of such institutes press towards increasing autonomy and self-direction.

You are all aware of the controversies which have grown up around these institutes. On the positive side, we may note the following. They provide technical training to graduate students who are empirically inclined; the projects give students opportunities for closer contact with senior sociologists; the data collected for practical purposes furnish material for dissertations through more detailed study, or what is sometimes called secondary analysis; the members of a Department with an effective institute can give substance to their lectures with an enviable array of actual data; skills of intellectual cooperation and division of labor are developed; chances for early publications by younger sociologists are enhanced.

On the other side of the debate, the argument goes about as follows. Students who receive most of their training on organized

projects become one-sided; instead of developing interests of their own, they become mercenaries of their employers; where institutes become influential, important sociological problems are neglected because they do not lend themselves to study by the "research machinery"; people who work best on their own find themselves without support and are regarded as outsiders.

The situation, as I see it, is promising but confused. We allow these institutes to develop without giving them permanent support, without integrating them into the general university structure, without even really knowing what is going on outside our immediate academic environment. As a bare minimum it is imperative that a more detailed study of the current situation be carried out. This would hopefully lead to recommendations for university administrators, for members of our own Association, and for all others concerned with the basic problem of how the avalanche of empirical social research can be fitted into current educational activities without having careless institutional improvisations destroy important traditional values or hinder creative new developments. True, we have no perfect formula for incorporating institutes into our graduate education. But pluralism is not the same as anarchy, and it is anarchy with which we are faced at the moment. Some form of permanent core support, assimilation of teaching and of institute positions, a better planned division of the students' time between lectures and project research, a closer supervision of institute activities by educational officers, more explicit infusion of social theory into the work of the institutes—all this waits for a systematic discussion and for a document which may perform the service which the Flexner report rendered to medical education fifty years ago.

In such a report the role of the institute director will have to figure prominently. Let me place him in a broader framework. We are confronted, nowadays, in our universities, with a serious problem which can be classified as an "academic power vacuum." When graduate education in this country began, no one doubted that the university president was an important figure. Gilman at Johns Hopkins and White at Cornell were intellectual as well as administrative leaders.

Stanley Hall at Clark was impressive both as a president and as a psychologist. Inversely, individual professors were deeply involved in organizational innovation. John W. Burgess forced the creation of a graduate faculty upon the Columbia trustees. In his autobiography he describes movingly what this meant to him as a teacher and scholar. Silliman sacrificed his private fortune to establish a physical laboratory in his home and finally convinced the trustees at Yale that natural sciences were not a spiritual threat to young Americans.

Today, however, we witness a dangerous divergence: academic freedom is more and more interpreted in such a way as to keep the administration out of any truly academic affairs; and the faculty, in turn, has come to consider administration beneath its dignity. But educational innovations are, by definition, intellectual as well as administrative tasks. And, so, they have fallen into a no-man's land: the President and his staff wait for the faculty to take the initiative; the professors on their side consider that such matters would take time away from their true scholarly pursuits. As a result, many of our universities have a dangerously low level of institutional development.

One institutional consequence of research institutes is that they inevitably train men who are able and willing to combine intellectual and administrative leadership. An institute director, even if his unit only facilitates faculty research, must train a staff able to advise on important research functions. It is not impossible that, on specific topics, the collective experience of the institute staff exceeds the skills of the individual faculty member. One who has lived with scores of questionnaires can help write a better questionnaire on a subject matter in which he is not expert. Having helped to dig up documents and sources of data on many subjects makes for greater efficiency even on a topic not previously treated. In an autonomous unit this is even more pronounced. Here the staff carries out a self-contained work schedule. A hierarchy is needed, proceeding often from assistants to project supervisors, to program director, and, finally, to the director himself. The latter is at least responsible for reports and publications. But the director is also concerned

with maintaining what is sometimes called the "image" of his operation. Its prestige, its attraction for staff and students, and its appeal for support are self-generated, not derived only from the reputation of the teaching departments. The professional staff sees its future career closely bound up with the destiny of the unit, a fact which sometimes makes for challenging problems in human organization.

At the same time, the director must develop the coordinating skills so necessary in a modern university. Often the place of his unit in the organization chart is not well defined. The novelty of the whole idea makes for instability and requires considerable institutional creativity. And, finally, we should frankly face the fact that in our system of higher education the matching of budgetary funds with substantive intellectual interests is a characteristic and enduring problem. The institute director knows the skills and interests of the faculty members, and he brings men and money together. This is not badly described as the role of "idea broker." Often he will have to work hard to obtain funds for a more unusual research idea suggested to him; at other times a possible grant looks so attractive that he will try to discover, among some of his faculty colleagues, what he would diplomatically call a "latent interest."

I am afraid this is not the appropriate forum for reforming university presidents. But I can at least try to convince some of you that directing a research institute is no more in conflict with scholarly work than is teaching. The director is faced with a variety of research problems which permit him to try out his intellectual taste and skills, while the individual scholar might find himself committed to a study prematurely chosen. The multitude of data passing through the director's hands considerably broadens his experience. Staff conferences provide a unique sounding board for new ideas. Even negotiations for grants open vistas into other worlds which a sociologist can turn to great advantage in his own work. Undoubtedly not every personality type is suited for this role, and even the right type of man needs proper training. But the opportunities for self-expression and for intellectual growth are considerable, and sociologists, in particu-

lar, should not be misled by the prevalent stereotype of administration.

I have now sketched out my main theme: empirical social research tends toward an organizational form of work which has two consequences: on an intellectual level, it forces one to be explicit about the work in hand. This, in turn, leads to a methodological awareness which radiates ideas into general fields of social inquiry. On an institutional level, institutes are, in themselves, a highly interesting innovation. They affect the organization and curriculum of departments of sociology, and they focus attention on the broader problem of what I have called the power vacuum in American universities.

This brings me to my last point. To what does all this empirical research add up? In a way, this is related to the main theme of this convention: "the uses of sociology." First, a reminder: until 1937 our annual meetings always had a central theme; then a resolution was adopted that because the field had become too diversified, this practice was no longer possible. By now, it would seem that diversification has reached such a point that the annual meetings should perhaps try to review common denominators, one by one. In any case, I used the feeble authority of the President and persuaded the Council that the problem of utilization is an urgent one. Actually, it would be better to talk of a utility spectrum. At the one end, you have the idea, most clearly represented by contemporary Soviet opinion, that the only justified use of social research is the advancement of social revolution. Having grown up in an exciting and constructive period of socialist optimism, I have never quite lost my hope for radical social change. But I do not believe that empirical social research of the type we are discussing tonight can contribute much to it.

At the other end of the spectrum, one finds utility in the narrowest sense. This includes studies for government agencies, for business firms, labor unions or other voluntary organizations which pay for them in the expectation that they will advance their purposes. But do they really help? As you know, we hope to publish the main contributions of this convention in a volume under some title such as "Applied Sociology." From watching the period of preparation

for this program, I gather that a kind of curvilinear relation exists. The greatest difficulty in providing concrete examples comes at the two extremes of the utilization spectrum: the exponents of basic social change and the people who want guidance for immediate policy and action are most often disappointed.

Within this continuum many points could be singled out for discussion. Having little time left, I want to select two of them, one because the record of the past is impressive, and the other because the needs of the future are urgent.

ACHIEVEMENTS AND HOPES OF EMPIRICAL SOCIAL RESEARCH

It has been said that the most sociologists can hope for at the moment are theories of the middle range. My colleague who coined this phrase gave his presidential address on multiple discoveries. So he will not mind if I report that fifty years ago, during the early phases of the Chicago school, the hope was expressed that their field studies would contribute to "intermediate scientific truth." And, indeed, empirical social research has proved most useful in serving this function.

Let me bring in at this point a final reminiscence. When I began to conduct studies of consumers as part of my Austrian program of research on choice, I often had to defend their scientific dignity. On one occasion, I pointed out that a series of such studies permitted important generalizations, and chose, as an example, the notion of the "proletarian consumer." Comparing him with his middle class counterpart, I described him as:

. . . less psychologically mobile, less active, more inhibited in his behavior. The radius of stores he considers for possible purchases is smaller. He buys more often at the same store. His food habits are more rigid and less subject to seasonal variations. As part of this reduction in effective scope the interest in other than the most essential details is lost; requirements in regard to quality, appearance and other features of merchandise are the less specific and frequent the more we deal with consumers from low social strata.

Notice that this is a summary of a large number of studies, no one of which, in its own right, is very interesting. But, together,

they led to the notion of effective scope. This concept became subsequently useful in many ways—be it to distinguish between local and cosmopolitan roles, between lower and better educated social strata, or just between people whose radius of interests could be small or large. Stouffer's notion of relative deprivation was similarly developed from a variety of seemingly unconnected attitude surveys. Many other examples could be given to show the possible contributions of empirical studies, however narrow, to theories of the middle range. As a matter of fact, this is almost implied in the very idea of mediating between descriptive data and higher order generalizations. Inversely, there probably would not be much theory of the middle range without the steady supply of specific studies, a growing proportion of which comes from various social research institutes.

At another segment along the utility spectrum we meet the question whether major social improvements have been facilitated by the available techniques and the existing organizational forms of empirical social research. I do not refer to the continuous efforts to improve recognized trouble spots such as delinquency or racial discrimination. The issue is rather whether it is possible to do what Robert S. Lynd once called research for the future—studies which are generated by a sociological analysis of unrecognized social needs. We have tried for years to clarify this challenging idea at Columbia University. When Allen Barton became Director of the Bureau of Applied Social Research at Columbia, he put some order into our collective thinking by developing types of studies which would satisfy Lynd's criterion. Let me give you two examples from his list. One type can be called the investigation of "*positive deviant cases*." We take it for granted that certain types of situations usually take unfavorable turns. And yet sometimes exceptions occur: local or regional elections in which a good candidate wins in spite of the fact that his adversary has the power of the machine on his side; a really independent small town newspaper which survives in spite of opposition by the "interests"; a faculty successfully resisting infringements on academic freedom; escape of youngsters from temporary associations with criminal gangs. While the content of

these examples varies from case to case, they all converge on the central task of finding generalizations centered on the problem of how to stem an undesirable social drift. It is muckraking in reverse.

Another type of study can be called the *pretesting of new social ideas*. A new notion of creative reform—especially if it has just been formulated—often needs studies to check on its assumptions and to perfect its design, partly to improve its feasibility and partly to facilitate its public acceptance. At the present moment, for example, it appears that structural unemployment can be solved only by a large-scale relocation of workers. The idea often meets great resistance from local commercial, church, and union interests. Some legislative aid for specialized re-training even precludes the use of federal funds for relocation. In addition, many workers themselves seem to resist relocation, although the extent and the relative weight and interconnection of all this is by no means known. Parallel to the need for pertinent sociological and psychological data, one must ask whether, from an economic and a technical point of view, it is easier to move people or to move factories.

The mere size of such studies might be enough to outgrow the capacity of a single research group. They require an interweaving of quantitative and qualitative technique, of simultaneous research on individual and organizational levels, together with some historical analysis. Furthermore, such inquiries are time consuming. But there is nothing prohibitive in itself about such an extension of current research practices. And yet the fact that it has not been done is by no means accidental. Having been director of an institute for some years, I cannot avoid a feeling of regret. It is a great temptation to undertake a study for which funds are avail-

able, on topics which require the kind of skills one has developed in one's staff, under sponsorship which promises continuity. My generation has had to worry a great deal about the mere survival of our organizations and their acceptance by university administrations. The new generation of directors, whom we have trained, work under better conditions. They should have the courage to strike out in the direction of some of the more complex areas that I have just tried to exemplify. I seriously hope that they will take the flames, and not the ashes, from the fires we have kindled.

As I come to the end of my remarks I become aware that the difficulty of writing a presidential address is equalled only by the prospect of leaving the rostrum and hearing one's friends say that it was all very interesting, but the main themes were not quite clear, that the examples might have been more pointed, the organization of the points was somewhat confusing.

It is a little like the case of the man who asks for a divorce and gives, as his reason, the fact that his wife talks and talks and talks. The judge asks what she talks about and the man replies: "Your honor, that's just the problem, she doesn't say." My sympathies tonight are naturally with the woman. There are situations where one wants to express ideas that can hardly be communicated to others who have not undergone comparable experience. I have done my best to say what I am talking about. It is my conviction that, as time goes on, a growing number of sociologists will meet the problems and situations to which I have been exposed. If some of them find that, in retrospect, my observations make sense and that, as they face a concrete decision, some of my suggestions prove of help, then the purpose of this address will have been achieved.