Research for What?

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My intention is to discuss some of the difficulties and obstacles encountered in research undertaken specifically to guide practice, that is, research for application. This is not only a concern of the Society, but, in my opinion, one of our major contemporary tasks. If what I have to say sounds familiar, even repetitious, I hope you will bear with me.

Until recently we lived in a "planned society," not planned in the sense of deliberately drawn blue prints and specifications, but "planned" and directed, in the sense that everyone was guided, as if by an unseen hand, of tradition. In our historically developed laws and institutions, our familiar personal roles and relations, everyone knew what he was expected to do and not do, how to act as a responsible citizen in the various activities and relations of individual and group living. We did not always act responsibly, but we were rarely in doubt what we should do and not do, and we knew what to expect and what was expected of us by others.

Moreover, until recently the criteria for making decisions, for formulating programs and directives in government, in industry, business, education—all the varied organized activities of our social order, these criteria were fairly clear and applicable to the familiar situations we had to cope with.

Today we are confronted in almost every area of living with questions we cannot answer by invoking these familiar precedents; with situations we cannot manage by the customary formulas; with conflicts, and often with acute disorders, which we cannot compose or resolve in the terms of our traditional expectations and practices. Moreover, we are being compelled to develop new patterns, new designs for living in an urban, industrialized society, where we are all perplexed and often baffled by the unevenness of change.

While it may sound grandiose, yet I believe we can, and must, say that we face the immense task of renewing our culture and reorienting our social order so that we can live in the new climate of opinion now emerging and can begin more fully to benefit from the marvellous new resources of technology which are now creating many of our difficulties. We face the necessity of creating an industrial civilization, utilizing the growing resources of science and technology for human conservation. A fundamental need in this situation is for research to cope with these new situations and opportunities. At present we are, to use the familiar phrase, backing into the future, trying to deal with these new problems in terms of older assumptions and perceptions. We cling to the ideas, patterns, and
practices of the past as our guides for dealing with the present and utilizing the suddenly revealed potentialities of the future. With the great prestige of scientific approaches to problems and the enormous benefits we have already gained from applied science and technology, many are looking to social research for help in dealing with our many exigent choices and the social problems we face today.

Thus it may be assumed that social research will be invoked ever more frequently and extensively by individuals, organizations, public and private groups who hope and expect that investigation will provide dependable guides for action and useful criteria for decisions, where they can no longer rely upon precedents nor resolve their difficulties by familiar practices. In addition many investigations will be financed by governmental and foundation grants for studies which are expected to further improvements in one or more fields of human living.

Social research and application therefore presents a topic for prolonged reflection, some aspects of which I will try to discuss in the hope that members of the Society will continue these explorations and develop increasingly fruitful approaches to the urgent problems we now face. We are concerned here not only with the more specific questions of how social research aimed at application may be formulated and carried on, but with some ethical questions of considerable import, as I see it.

I.

The first point we might consider is the different approaches exhibited by the researcher and by the practitioner. As Merton has observed:

Experience suggests that the policy maker seldom formulates his practical problem in terms sufficiently precise to permit the researcher to design an appropriate investigation. Characteristically the problem is so stated as to result in the possibility of the researcher being seriously misled as to the "basic" aspects of the problem which gives rise to the contemplated research. This initial clarification of the practical problem, therefore, is the first crucial step in applied social science.\(^1\)

Redl, in discussing the need for research on delinquency, has pointed out:

The practitioner—by whom I mean all those who deal directly with delinquents (including teachers and parents) has an old complaint. He is ready to pay tribute to the long-range usefulness of our fancy formulations, terminology and curves. He politely admits the research expert probably produces a lot that is important. His com-

plaint is that the research expert does not answer the questions he asks.\textsuperscript{2}

In this article Redl indicates how the practitioner is so often frustrated when he turns to research for help. Redl emphasizes that in the delinquency field few researchers have had much actual contacts with delinquents or had the kind of experience that the teacher in the slum area, the leader in a settlement or youth center, or the matron in a detention home has had in face to face relations with delinquents. He says that the researcher when invited to study delinquency asks, “What could we use our research tools on?” instead of asking, “What do we need to know about youth in order to tackle delinquency?”

Here we may interpret Merton’s term, “appropriate investigation,” as meaning an investigation that permits the researcher to utilize his familiar research tools upon a professional problem which will be considered by his discipline as relevant and important and which will make a contribution to that discipline. In practice this often means that the prestige of certain problems and the respectability of accepted methods become more important than the exigent questions raised by the practitioner. This is especially to be noted in those disciplines where there is a well developed “pecking order,” so that to deviate from the approved problems and customary procedures may be professionally hazardous.

Thus, Merton’s statement: “This initial clarification of the practical problem . . . is the first crucial step in applied social research,” often means that the researcher more or less ignores or rejects the practitioner’s question. Instead he formulates a professional problem which may be studied by using the practical situation as a source of data for such an investigation. Perhaps, as Redl suggests, the researcher does not know enough about the actual situation to be able to formulate a problem more relevant to practical needs.

Everyone will agree that Merton is justified in saying the practitioner has not, in many cases, asked the question he wants answered, often stating it too specifically or too generally, or failing to recognize the actual problem.\textsuperscript{3} Also we know from long experience that the direct, head-on approach to a difficulty may be futile while a round-about, indirect approach, such as basic research, may be very fruitful. I am not raising any question of the validity of such “appropriate investigations” but of their relevance and responsiveness to the practitioners’ need for guidance.

The legislator, administrator, or policy maker is confronted with a complicated situation, often a conflicted situation calling for decisions involving people and their activities. Or he faces the need for a new program or operational procedure. Or an individual faces a difficulty, social


\textsuperscript{3} Frank, L. K. Society as the Patient. New Brunswick: Rutgers University Press, 1948. See especially the chapters on “Social Problems” and “What is Social Order.”
task, or group activity and seeks some guidance for coping with that task. If the researcher undertakes to help him, accepts funds for the study, and then focuses on a professional problem which may or may not have much relevance to the need for help, we may well ask: research for what? There is no obligation upon social scientists to study such practical problems. They are free to pursue whatever problems appear to them worth studying, with whatever methods they prefer. They are not compelled to seek or accept governmental or foundation grants designated for specific purposes; but when they do, what obligations do they accept or incur? This raises an ethical question that needs to be more fully discussed when time and occasion permit.

There is a growing expectation that social research can be helpful in our daily lives, can contribute to the resolution of the many difficulties and conflicts we face today. This expectation has been confirmed by research in physics and chemistry and medical sciences which over the years has been immensely productive. Accordingly many people believe that social research can and should be invoked to help us in dealing with our many human problems, expecting that the results of social research can be immediately applied, but forgetting that the basic research in physics, chemistry, and medical science become operational only through engineering and clinical medicine.

Perhaps one of our difficulties is that we have no professionally-trained personnel who can translate social research into practice as do engineers and physicians. According to this view, we should go on patiently researching in the social sciences and hopefully awaiting the emergence of such trained professional workers as we see appearing in public administration and city planning, who will be able to utilize social research and apply it productively. This Society was organized for just such a purpose and has been offering research materials to practitioners since it began.

II.

But there is another approach which may be more effective, through a reformulation of our problems of applied research, an enterprise for which there is increasing support from recent scientific explorations. Indeed such reformulation, far from being a mere compromise with necessity, is in line with some of the recent developments in modern scientific thinking, as I will try to show. Scientific research, until recently, has been guided by the analytic tradition; to study any situation or event we must analyze it into its various components and investigate the relation between pairs of variables in an adequate sample. I need not elaborate upon these procedures, which are familiar to you and which have been highly productive, except to say that they are not adequate for the study of total situations and organized wholes, and it does not seem that further and further analysis will help in such problems.
Writing on research in meteorology, Goody remarks:

Nearly all the phenomena (meteorological) which he studies seem to be closely interlocked: in other words, he is dealing with a tight feed-back system. With such a system the familiar laboratory procedure of isolating and controlling one factor at a time is useless, for the essence of the problem lies in the complex of interlocking phenomena.  

Analysis does not solve but avoids the problem of complexity, as Ashby has pointed out recently:

We are beginning to see that complexity in major degree is not an insuperable barrier to knowledge, provided it is tackled in the right way. The way not to tackle such a system is by analysis, for this process gives us only a vast number of separate parts or items of information, the results of whose interactions no one can predict. If we take such a system to pieces, we find that we cannot reassemble it. What is the best strategy is not yet clear, but many of us are convinced that suitable and practicable strategies exist and that they are now in process of being discovered.

This questioning of the analytic procedure is not a repudiation of the method as such, but of its relevance and appropriateness in studies of organized, complex wholes. This appears in a statement by McKay:

If I say that an electric advertising sign is ‘nothing-but’ a certain array of lamps and wires, I may mean one of two things: (1) I may mean that an electrician could make a complete catalogue of all that is there, and have nothing left over, without mentioning ‘the advertisement.’ This is true. (2) Or I may mean that since there is nothing left over from the electrician’s account, there isn’t really an advertisement there at all. This is the error of reductionism. It consists in confusing exhaustiveness with exclusiveness. The electrician’s account is exhaustive, at least in the sense that a perfect replica could be constructed from it. But the electrician’s account and the advertiser’s account of ‘all that is there’ are not mutually exclusive. The advertisement is not something to be fitted into a gap in the electrician’s account. It is something that we find when we start all over again to describe what is there in another complementary language.

Perhaps we can accept this principle of complementarity and agree that the accepted analytic methods offer one kind of approach, but we

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need another approach which will lead, not to progressive reductionism, but to a grasp of what Warren Weaver has aptly called "organized complexities."

In his paper with that title, Weaver discussed the development of science from the earlier study of two or more variables, revealing as Schroedinger has put it, "order in disorder," to the present day, where we are beginning to face the problem of organized complexities. As Weaver stated it:

These problems (of organized complexity)—and a wide range of similar problems in the biological, medical, psychological, economic and political science—are just too complicated to yield to the old nineteenth century techniques which were so dramatically successful on two, three, or four variable problems of simplicity. These new problems, moreover, cannot be handled with the statistical techniques so effective in describing average behavior in problems of disorganized complexity.

These new problems, and the future of the world depends on many of them, require science to make a third great advance, an advance that must be even greater than the nineteenth century conquest of problems of simplicity or the twentieth century victory over problems of disorganized complexity. Science must, over the next fifty years, learn to deal with these problems of organized complexity.

Let us consider what we may gain if we focus on problems of organized complexity in our social research, especially when undertaken for practical use. We would cease fractionating such wholes into a series of variables to be studied seriatim as in most of our present studies. We could address ourselves more directly to some of the urgent problems such as human growth and personality development, mental health, delinquency, family living, the many and varied difficulties of our disorderly social life and international relations, which cannot be dealt with piecemeal.

You may ask how we can state these problems of organized complexity. As we are increasingly realizing, we need, not hypotheses about the relation of two variables or suspected causal relations (appropriate for analytic studies), but rather some conceptual models which are not explorations or generalizations, but scientific approaches. The purpose of a model has been well described by McKay:

A theoretical model of the type we have been discussing is intended to serve as a tool of research. We can think of it as a kind of template which we construct on some hypothetical principles, and then

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hold up against the real thing in order that the discrepancies be-
tween the two may yield fresh information.

This in turn should enable us to modify the template in some re-
spect, after which a fresh comparison may yield further informa-
tion, and so on. The model, as it were, 'subtracts out' at each stage
what we think we understand, so that what is not yet understood is
revealed more clearly.

You will see at once that we shall judge a good model for this pur-
pose not so much by the success with which it imitates or predicts,
but rather by the clarity with which its failures enable us to infer
what to modify next. To be sure, our aim is to approximate more
and more to the real thing. But we may easily be misled into an ap-
proximation process that doesn’t converge. It may even pay us to
discard one model for another which offers us fewer numerical pre-
dictions to start with, if the first model shows signs of requiring one
or more additional hypotheses for each phenomenon it encounters!

Our second criterion of a good research model is that it should not
only function normally like the brain, but also it should go wrong in
the same ways.

It is possible that some mathematicians may feel that no theory is
worthy of the name until it has produced some equations. But I
want (if I dare) to emphasize that what we wish to emphasize in
such multi-dimensional problems would be very little illuminated
even if we could produce a gigantic equation relating all the vari-
ables that we should never measure.

Understanding, here as always, means knowing so far as possible
what to expect in given circumstances and what to infer when the
expected does not happen.\textsuperscript{10}

The foregoing is especially relevant to the social sciences, because we
need a model of how a social order operates and especially how it breaks
down, goes awry, and fails to operate as expected—a phenomenon which
is so frequent, nowadays, that some might even question the term social
order. Also it may be pointed out that prediction as the criterion of sci-
entific endeavor is appropriate only in the study of convergent events,
which, as Langmuir has stated, average out and can be viewed in terms of
cause and effect\textsuperscript{11}, as distinguished from divergent events.

III.

Specifically we need a more adequate model for the social-cultural
field which will recognize, as did Kurt Lewin, that the field is a way of


conceptualizing the organized complexity of organism-personality and the total "surround" (Sherrington's term), as exhibited in a culture and a social order. For such a field concept we need to think in terms of circular, reciprocal relations and feed backs, both positive and negative, through which the component members of the field participate in and thereby create the field of the whole, which field in turn regulates and patterns their individual activities. This is a circular, reciprocal relation, not a serial cause and effect, stimulus and response relation. The term "field forces" may be abandoned because it too often implies some serial causal action and obscures this circular activity. Also we should recognize that each measurement in that field is relative to all the other measurements in that field and to the observer and also to the specific time when they were made. Because we can count and measure human behavior we assume that such measurements are similar or equivalent to measurements of actual physical events, whereas they are usually recordings of symbols and symbolic activities. Such symbols derive their significance, not from their actual size, shape, frequency, or other quantitative dimensions, but from their meaning to those who use them and respond to them; hence the most precise measurements, if taken literally, may be misleading.

The classical social sciences have long approximated to a field concept in their use of the term "system"—such as an economic system, political system, social system, and also social structure. But these concepts of system have, until recently, been treated as referring to actual systems or mechanisms, located somewhere out in space like the ether, operating through large scale forces, above and beyond human control or interference, essentially a Newtonian concept. Moreover each of the social sciences assumes a separate, independent system—economic, political, sociological—which operates with little or no relation to the other "systems." Indeed each of these social sciences assumes a different kind of human nature for those living in its system, and explains human behavior in its own terms. Thus it is difficult, if not impossible, to find any relation between the economic, political, sociological, and psychological findings, although it is obvious that they are derived from observation of the same human actors living in the same social order and sharing largely the same cultural traditions. Cultural anthropology has largely escaped this fractionation of human behavior by focusing on the web of behavior and viewing a whole culture as a design for living.

An adequate concept of the social-cultural field would enable us to

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12 Einstein, Albert, and Infeld, Leopold. *The Evolution of Physics*, p. 259: "It needed great scientific imagination to realize that it is not the charges nor the particles, but the field in the space between charges and the particles, which is essential for the description of physical events. . . . The theory of relativity arises from the field problem. . . . The contradictions and inconsistencies of the old theories force us to ascribe new properties to the time-space continuum, to the scene of all events in our physical world."

escape from this fractionation of social order into a number of discrete, unrelated "systems" and "forces." Moreover we could dispel much of the present confusion arising from reifying the data of the several social sciences into seeming entities, such as prices, wages, rents, votes, marriages, divorces, crimes, etc. Each of these data is treated as if it was an entity, independent of human behavior, and then we invoke the idea of special economic, political, and social "forces" to explain how these entities change.

The human actors, whose behavior in all these forms and patterns constitutes social order, have been parcelled out among the different social sciences, each of which has focused (consequently ignoring all else) on one set of specific symbols and rituals in which that behavior is revealed, such as prices, wages, credit, or votes, marriages, etc. These selected and partial observations of human behavior are then used as data for each of the separate social sciences, independent of all other data. During the war the government asked many social scientists to make studies for different governmental departments and programs. These investigators for the most part undertook studies and prepared directives based on one set of data—economic, political, sociological, psychological, legal, ignoring the far reaching consequences of their recommendations because they were largely unaware of how their unilateral directives would affect the activities reflected in the data of other social sciences.

This situation in social science is not unlike that in physical science about 1800, when many observations and even measurements had been made on heat, light, electricity, magnetism, and chemical activity. Each of these sets of observations was reified into an entity—thus heat was conceived as a substance, electricity a fluid, light a corpuscle, etc., and for each a separate theory was sought. Not until these various supposed entities or substances were recognized as different ways in which energy was propagated through different media and recorded by different instruments—not until this was recognized could physical science be unified and able to group the basic physical processes in operation.

We await a similar unification of social science which may come from a recognition that the various kinds of social data are all generated by human behavior operating in a social-cultural field and using the language, rituals and symbols, roles, laws, and institutions which pattern that behavior and give it orderliness. We also await a unified theory of human behavior which will embrace all the different aspects and manifestations of human behavior in the cultural-social field. At this point we might ask what is the role of social psychology in this awaited reformulation of social sciences? Will social psychology, as the latest development in the social science disciplines, undertake the formulation of a more unified, social-field concept and a more comprehensive approach to human beh-

behavior. Surely we cannot go on assuming that when a person is engaged in economic activity, political, or sociological, he is not exhibiting social behavior; or is social psychology content to deal only with what the classical social disciplines ignore or leave out?

In his discussion of *Psychology and the Design of Machines*, Taylor points out the methodological advances that have come from working on problems of machine design, especially the gain in the grasp of problems when the psychologist learns to deal with the *total system of man*, and to use theoretical models that are relevant and appropriate for such a system. He also remarks on the gains to psychology with the breakdown of the barriers between the psychological sciences and the physical sciences. Similar gains may be achieved when we break down the barriers between psychology and the other social sciences and begin to study the *system of individual-social order*, using models adequate for such an organized complexity.

A field concept or model for social research should deal with this system of individual-social order and recognize that, as an organized complexity, a social order has form, configuration, Gestalt, a structure or organization, as contrasted with random disorderly events which must be sampled for study and which exhibit only statistical order. This organization persists as such, over varying periods of time, as shown in the life cycle of the human organism-personality, or the prolonged, continuous existence of a culture and a social order.

There are obstacles or resistances to this kind of thinking as well as prevailing scientific fashion. John E. Anderson has recently asked: "How much does the present vogue of hypothesis research force problems and analyses into the determination of two variable relations?" Likewise Kantor has remarked: "Variable intervention is simply a technique for loading the organism with internal principles and powers."

It may not be irrelevant to regard intervening variables, so frequently invoked today, as a device for avoiding the recognition of the behaving organism as an organized complexity, so that research may continue to study the relation of two variables.

IV.

Fundamental in the study of social order is the development of what we may call a "communication model." The organized complexity of social order, like that of the organism, operates through a communication network, and hence any model of social order must recognize the com-

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munication process, which, may I emphasize, involves more than is to be found in information theory and includes the whole range of symbolic behavior revealed in circular reciprocal or resonating relations. Wiener has stated the situation in this way:

The existence of social science is based on the ability to treat a social group as an organization and not as an agglomeration. Communication is the cement that makes organization. Communication alone enables a group to think together, to see together, and to act together. All sociology requires the understanding of communication.

What is true for the unity of people is equally true for the individual integrity of each person. The various elements which make up each personality are in continual communication with each other and affect each other through central mechanisms which themselves have the nature of communication. (Quoted by Karl W. Deutsch.\(^\text{18}\))

A communication model frees us from the specificity of previous conceptions of determinate relationship, such as cause and effect or stimulus and response, and enables us to recognize that symbolic messages trigger the responses which are patterned and expressed according to the life experience of the respondent in a cultural and social order, his present state and his expectations. Thus we may understand how the great diversity and variations in human behavior become regulated, channelled, ordered, by using the prescribed symbols, such as language, the rituals and roles necessary for communication in a social order. What we call human relations are essentially communication in interpersonal relations or in group and social relations, using the whole array of private and social symbols for such communications.\(^\text{19}\)

Once we construct such a model of an organized whole, we can deal with the problem of persistence with change, and the maintenance of a dynamic stability or homeostasis. It is the formal communication network of a culture and a social order, largely symbolic, which continues and persists, while the individual component members vary in their behavior and are replaced by the oncoming generations. Likewise in an organism the different constituent cells, tissues, fluids and their chemical components are being continually replaced, but the organism persists, grows, develops, and ages. Moreover, through these communication processes, what we call organization arises and operates as a self-regulating, self-repairing process.

Thus in our conception of social order as an organized complexity we should recognize that it is, not a closed system as we have previously thought in our scientific studies, but an open system, as Bertalanffy has


emphasized, wherein we observe equifinality as he has termed it. We may now substitute the idea of process for mechanism and for cause and effect or stimulus and response, and begin to see that the same process, operating at different times and places, in different contexts, may produce different products, while dissimilar processes may produce equivalent products. This may be one of the crucial differences between mechanism and organized complexity. Organized complexities share the same organizing and communication processes and produce equivalents which we have often failed to recognize as such.

V.

Another requisite for an adequate model for social order is the concept of purposive behavior. Unless we assume that human behavior may be purposive, there seems little need or justification for research aimed at application. Purposive behavior is now being reformulated so that it no longer implies vitalistic assumptions. As Rosenbluth, Wiener, and Bigelow have proposed, the problem of purposive behavior can now be stated rigorously in terms, they suggest, of feed backs. Ashby has also proposed a conception of self-regulating, purposive organizations which operate through part functions, step-functions and achieve ultra-stability. He has also emphasized the necessity of considering the organism and the environment as a system which attains equilibrium by dynamic processes, a conception essentially of a field situation and an open system. Thus Ashby has said:

There cannot be a proper theory of the brain until there is a proper theory of the environment as well. I think the psychology of the environment will have to be given as much thought as the psychology of the nerve net.

A similar statement has been made by Taylor:

The inseparability of the behavior of living organisms from that of the physical environment with which they are in dynamic interaction argues against maintaining separate sciences and construct languages, one for the environment, the other for that which is environed.

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VI.

All this, you say, may be interesting but purely speculative. What is the connection with social research for application? I would again emphasize that the administrator, legislator, policy maker is confronted with organized, complex situations and people engaged in all their life activities and strivings. He cannot take over the findings of analytic research, no matter how great the confidence the investigator asserts in his findings, and apply these partial results as such to actual people and situations. A major contribution by social research as suggested earlier would be the formulation of some new models of social order to replace the traditional Newtonian models of separate economic, political, social “systems” or mechanisms.

We need today a number of new social inventions which might be developed once we could escape from the old concepts and assumptions so that we were free to think of social order in more dynamic terms, with the use of conceptual models. Practical problems indicate that the long-accepted models or theoretical formulations as given by tradition are no longer adequate.

Kurt Lewin’s idea of action research was of a dynamic approach to a whole situation with the hope that people’s behavior therein could be altered for the better.

If we accept the proposal by Dewey and Bentley that we give up the classical concept of knowledge as a third substance and recognize the transactional process of knowing, established by the knower with the known, or to be known, we see that research for application should be productive of new ways of relating to situations, by giving the practitioner, not bits of abstract knowledge, but rather new and better ways of knowing what he has to deal with, of relating himself to situations. How he will attempt to deal with a situation is governed by new perceptions of that situation which can be altered by research.25

Assuming then that we should approach social research for application in terms of organized complexities for which we can design various models, what methodologies can we use? The mixed team offers one possibility which, as you all know, has certain difficulties arising largely from the fierce loyalty of each member of the team to his own discipline and to its assumptions and methods. Also each member of the mixed team is usually concerned only with the problems his discipline says are important and considers the others as insignificant. But only a mixed team can observe adequately the multi-dimensionality of a social problem, and their total efforts are essential to any comprehensive grasp of the organized complexity they face.

The mixed team approach has been increasingly productive in operations research, as shown notably by the British experiences during the war.

At the Conference on Philosophy and Operations Research, as reported in *Science*,26 there was general acceptance of the technique of model building as a fruitful tool for investigation. Operations research was described as "scientific method applied to action problems—a virile newcomer in the scientific spectrum."

Also, in much of current research for the Air Force and the military and naval organizations, mixed teams are studying complex situations such as "weapon systems" involving the interrelated use of many different weapons and specialized personnel in one unified operation. In industry today most of the theoretical as well as technical advances are made by mixed teams each member of which brings to the study his specialized knowledge for dealing with one or more of the dimensions of the problem; thus he contributes to the solution what his specialized training alone can reveal or suggest. A mixed team, when focused on a common problem, operates like an orchestra where each instrument does not compete but contributes to the performance of the whole composition. We urgently need mixed team research in social science so that we can deal with the multi-dimensional tasks we face today.

If we cannot now prescribe the methods for research on organized complexities, let us remember that the statement of the problem is the challenging scientific task.

Einstein has said:

There is no inductive method which could lead to the fundamental concepts of physics. Failure to understand this fact constitutes the basic philosophical error of so many investigators of the nineteenth century. . . . We now realize with special clarity how much in error are those theorists who believe that theory comes inductively from experience.27

As we learn to think in these new terms and begin to recognize these problems, we will see that we are in much the same situation as the physicists when they were confronted with the new conception of the atom as no longer a hard, inert, indivisible particle, but rather as a highly-organized configuration of nucleus and electrons which travel in orbits with angular spins and "jumps" that are revealed through emission of various radiations in quanta. Once they recognized this new problem they were stimulated to devise appropriate methods for its study. I expect that the social sciences will sooner or later follow this example of imaginative, creative invention of new techniques and procedures, as Ashby has forecast.

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Perhaps we can devise new and appropriate methods if we will focus on the situation or difficulty, as in operations research, instead of relying so much on the assumptions and formulations of our discipline, especially since these offer little help in approaching organized complexities.

What the practitioner seeks is not merely a presentation of what exists or is occurring, or what trends may be revealed, no matter how precisely these are measured or correlated. Rather, he needs a plan of action, a strategy for dealing with situations so that desired ends may be attained through a kind of action research which will help people to change their ideas, expectations, and behavior. This is developing in various fields of professional work, notably public health, urban and regional planning, and the varied enterprises known as technical assistance for underdeveloped areas. While much of the research in these fields is of the classical analytic type, increasingly the focus is on creation of operating programs, reconstruction of large areas, reorganization of groups or social orders. The public health worker, the planner, the technical advisor are striving to deal with all the dimensions of a problem and to devise plans, programs, and procedures which are more or less articulated into an operating whole, just as the physician must deal with a living person as contrasted with the anatomist or physiologist who can dissect his subject.

One of the most urgent needs for research is in urban and regional planning, which requires the close collaboration of all the social science disciplines since comprehensive planning must embrace and plan for all the diverse but interrelated, indeed interlocked, activities in a social order. Here the concept of organized complexity becomes especially fruitful, and the need for a series of conceptual models is essential to guide the planner in designing a whole neighborhood, community, urban renewal, regional development, or the industrialization of an underdeveloped area. Planning cannot ignore or neglect any of the multi-dimensions of human living in a social order, if it is to provide for new communities and areas in which people can live.

We should recognize here the social research carried on by business and financial organizations, by advertising agencies, which embraces more than economic questions. These are action studies focused on the behavior of people, often aiming at manipulating their feelings, their actions, and their relations (as recently revealed in *The Hidden Persuaders*). We should reflect seriously on this situation where the major efforts, in terms of personnel, time, and money, for altering our social order are being carried on, not by universities or government, but by private enterprise. Perhaps the attraction to this work is not solely the larger salaries paid by business but the opportunity to work on fresh and stimulating problems in concert with other disciplines, as we see in the mixed team research departments of many business firms.

If academic members of the scientific disciplines lament and deplore some aspects of this work, as many do, perhaps the inability or unwilling-
ness of the universities to undertake research except as now defined by their discipline may be at least partly responsible. If universities are not responsive to the need for unified social research, where will we find the help we seek?

But as suggested earlier, we need not consider action research as a concession to practical needs, to be undertaken with apologies. Rather in the light of recent scientific advances, action research may be recognized as offering fruitful possibilities for formulating a new kind of problem, which is as valid and promising, if not more so, than the familiar nineteenth century concepts and methods so highly esteemed today. When thus approached, we see that action research means studying the organized complexities presented by human behavior in a cultural-social field, devising new concepts and models and inventing new methodologies which will enable the investigator to grapple with these new problems in all their multi-dimensionality.

Through such action research the social sciences may achieve the kind of unification which the physical and biological sciences have attained. They may become abreast of the more advanced sciences, not by following earlier nineteenth century assumptions and methods (which the advanced sciences have largely relinquished), but by creative, imaginative explorations. As Weaver has said, the next great advance in science will come through the study of organized complexities, the solution of which, as he said, may largely govern the future of the world. Is it too much to expect the social sciences to make this great advance, especially when the need is so urgent and the eventual rewards will be so great?

VII.

Finally let me ask, is it absurd to think of a social order as essentially a conceptual model which the members of that social order are continually trying to attain in living, are always revising and improving, testing its adequacy by its failure to exhibit the desired qualities, the hoped-for order and effectiveness in providing a way of living as expressed in their philosophy, religion, and art? If we conceive of a culture and a social order as that which is sought but never fully attained, then we will realize that each member of a social order, especially the administrator, legislator, and practitioner, operates with such a conceptual model of our human symbolic world by which his life career and practice are guided. Moreover, it seems clear that advances in a social order, as in a culture, occur when the members of that group accept a new or different model which they attempt to realize in daily living, as Margaret Mead has found in New Guinea.\textsuperscript{28}

If we pursue this idea we will see that perhaps the most valuable contribution social science could make is to provide new models of social order which will guide the activities and expectations, not only of citizen-members, but more especially of the administrators, policy makers, legislators, planners, technicians, and the various helping professions. Let me remind you that the classical social sciences had their major impact upon social order through the theoretical models they offered, as notably in economics by Adam Smith, Ricardo, and their successors, Locke, Hume, and their followers in political science, by Comte and later sociologists. Their models were never proved, nor disproved. They were accepted as explanations of why and how economic, political, and sociological affairs occurred, or should occur. It seems clear that their theories served primarily to pattern people's conceptions and perceptions of their social order—they provided theoretical models, norms, ideals which people attempted to actualize in all their individual activities and relations, in legislation, administration, in organizations and procedures, much as the law operates both as a guide and as a corrective to group approved conduct.

Today with the passing of the old order we must, as I have said, attempt to create an industrial civilization for which we need an adequate model to guide the innumerable decisions and plans being made by administrators, legislators, policy makers, engineers, planners, and indeed everyone who must make decisions and choices.

My query, research for what, is essentially a series of questions, of suggested leads to their answers and a plea for more imagination in social research and, let me emphasize, for more responsibility on the part of investigators who undertake action research or research for practice to deal with the problems they are expected to study.

Let me add a statement that may indicate my own personal orientation.

Democracy implies more than freedom of action, speech and belief, more than voting and representative government, precious as are those hard-won rights. A democratic social order is one which conducts a continual assay or evaluation of all its laws and institutions, its patterns of conduct and relation, its varied forms of communication and other symbolic processes in terms of their relevance to, and congruity with, our enduring goal values—our belief in the worth of the individual human

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personality and our conviction of the primacy of human dignity, begin-
ning at birth. This is the focus and the major purpose of social research,
as I see it, and it emphasizes the necessity for bold, courageous, scientific
imagination.

Board, Washington, 1943 (out of print). See also Lynd, Robert S. *Knowledge for