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THE ROLE OF APPLIED SOCIAL SCIENCE IN THE FORMATION OF POLICY: A RESEARCH MEMORANDUM*

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1. **Rationale of the Inquiry.** Although the application of social science to practical problems of policy and action is still in its early stages, a large body of experience has been accumulated. Social science *has* been applied, in diverse spheres and with diverse results. The experience is there, but it has not been systematically reviewed and codified. Consequently, no one knows the present status of applied social science or, more importantly, its potentialities.

Social scientists have been so busy examining the behavior of others that they have largely neglected the study of their own situation, problems and behavior. Foundations, government and commercial enterprises have been so concerned with research directed toward pressing problems that they have failed to take systematic inventory of the achievements and potentialities represented by this large body of researches. The hobo and the saleslady have been singled out for close study, but not the social science expert. Sociological monographs document the problems and perforance of the professional thief and the professional beggar but not the problems and performance of the professional social scientist. Yet it would seem that clarity might well begin at home.

Quite apart from the direct intellectual merits of the problem, the most varied groups have a stake in an analysis of the present and potential role of applied social science in American society. Most prominently, social scientists themselves stand to gain by such inquiry. Perhaps owing to the absence of any systematic appraisal of their role, social scientists are sometimes beset with exaggerated doubts and harassed by exaggerated claims concerning their contributions to solutions for the problems of our day. The actual workaday relations between basic and applied social science must for them be largely matters of opinion, sometimes well-founded, at other times not, simply because these relations have not been made the object of systematic investigation.

Foundations and other philanthropic agencies engaged in endowing social

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science research have their stake in the inquiry as well. For until the *actual*, not the *supposed* or *ideal*, relations between basic and applied research are clarified, policies governing a program of endowed research must be based on rule-of-thumb experience. Yet it would seem the most elementary rule of intelligent administration to examine, from time to time, the consequences of diverse decisions. Are there types of applied research in social science which fructify basic theory? Do other types of applied research deflect scientific talent from fundamental inquiries into theory and methodology? Under which conditions does there occur a fruitful reciprocity between applied and basic research? A preliminary inventory may not succeed in providing circumstantial answers to these questions, but it can scarcely fail to lighten the fog of ignorance which, one must admit, now settles about the role of applied social science.

This inventory promises much the same returns for the maker of policy in government, business, and industry. To a large and growing but precisely unknown extent, applied social science does find a place in the world of practical decision. Much experience therefore exists, but this experience has not been codified. What are the obstacles to the effective utilization of applied social science? For which types of practical problems is the introduction of applied social science presently pointless and for which is it prerequisite to the formation of intelligent policy? Are there circumstances in which men of affairs have a direct stake in endowing basic research rather than calling for immediate applications of pre-existing knowledge? After all, the decision to utilize or to forego applied social science is itself a matter of policy and it would seem useful to have this policy based on available, though presently uncoordinated, information.

It is long since time, finally, for the intelligent layman who does himself not directly utilize applied social science to learn something of this current in contemporary life. His preconceptions of social science may range from unshakeable scepticism to equally ill-founded fetishism. In either case, how is he to arrive at an appropriate opinion? He is subjected to varied propagandas. One day he is told by seemingly unimpeachable authority that social science is merely gobbledegook. The next, he hears from other authorities that science alone, including its social divisions, can build the road to salvation. His choices are thereby limited. He may remain in a state of suspended judgment, which, in the present instance, may be only an euphemism for a state of confusion. Or he may cast his vote for one or another conflicting authority and emerge with clear and erroneous images of the present-day role of applied social science.

No preliminary inquiry can satisfy the diverse interests of social scientists, foundations, policy-makers in government and business, and the men-in-the-street. But it can manifestly gather up presently scattered materials, coordinate these and seek to lay a basis for an instructive appraisal of applied social science. Through a review of cases in point, it should provide some new perspectives on

(1) the achievements of applied social science;

(2) the conditions limiting and making for these achievements;

(3) the scientific (*i.e.*, theoretic and methodological) by-products of research in applied social science.

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2. Scope of the Inquiry. We shall center our attention upon the disciplines commonly regarded as comprising the field of "human relations," namely, anthropology, psychology, social psychology, and sociology. For as it well-known, a given practical problem ordinarily requires the *collaborative* researches of several social sciences. And it is precisely these four disciplines among the social sciences which have most consistently entered into collaborative research. This has in part resulted in a system of interlocking theory and implications for policy. The marked tendency toward coordination of these disciplines should therefore facilitate an immediate focusing of the inquiry.[1]

A second delimitation should be noted. Although all applied social science research involves *advice* (recommendations for policy), not all advice on social policy is based on *research*. An analogy with medicine may help clarify the distinction. In medicine, advice may be based on anything from sheer empiricism to systematic applied research, thus:

(1) *empiricism:* the experienced herb-doctor finds empirically that cinchona bark (for some unknown reason) is a specific antidote for malaria and advises his patients accordingly;

(2) *standardized therapies derived from previous cumulative medical research:* the physician treats well-identified cases of malaria with standarized quinine treatment;

(3) *advice based on specific researches oriented toward new problems:* investigating a high rate of malaria in a specified area to identify the local factors which must be brought under control, and advising alternative modes of treatment.

In matters of social policy, advice may likewise be based on anything from empiricism to systematic applied research, thus:

(1) *empiricism:* Henry Ford finds in 1914 that he can pay the then-extraordinary minimum wage of five dollars a day for unskilled labor, with consequent rise in output and profit;

(2) *standardized practices derived from previously cumulated research:* "scientific" wage policies based on taylorism, etc.

(3) *advice based on specific researches:* detailed analyses of an industrial plant to determine the "most effective" wage policy.

The passing analogy with medicine suggests a further point. In contrast to medical advice, advice on social policy appears most often based on rule-of-thumb experience and only infrequently on generalized knowledge or on specific researches directed toward the problems in hand. Moreover, it may not always be possible to identify the lines between "sheer empiricism" and "cumulative scientific knowledge" in the realm of social policy. But this presents no large difficulty. Since the proposed inquiry will be centered on *research* in applied social science, the role of the social science expert who proffers advice out of his general fund of knowledge will receive only secondary attention.

[1] In our projected inquiry, the materials will be further delimited and confined to a few substantive *fields of application*, of which the following are merely illustrative: Diverse problems of community planning, practices in personnel selection, propaganda for diverse purposes, advertising, democratic participation in political or organizational life, reduction of race hostilities, planned migration, inter-cultural relations. Limiting the inquiry provisionally to the study of applied research in *a relatively small number of diverse fields* should provide a broad range of experience and yet permit a fairly intensive study within each field.

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3. **Orientation of the Inquiry.** Ultimately, cases of policy-oriented and action-oriented research must be collected and analyzed to determine the problems involved in the formulation of the research and its application. Among these materials are documentary sources (correspondence, memoranda, drafts, minutes of conferences, reports, etc.) and interviews with the researchers and policy-makers (interviews aimed to clarify and elaborate the impressions gained from the documentary data).

Commonly underlying the occasional published accounts of the role of applied social science has been the presupposition that the applied research itself has been entirely adequate to the occasion, and that the "essential problem" has been one of persuading the policy-maker to utilize these adequate results. In other words, the intellectual adequacy of the research does not typically come into question, but merely the organizational and interpersonal problems of "selling" the research. This emphasis is understandable enough. These accounts have usually been written by social scientists, and it is to be expected that they would be more sensitive to the inadequacies of the policy-maker and his organization than to the possible inadequacies of the research. This orientation cannot be adopted here. Instead, we shall distinguish the two distinct, though interrelated, types of problems attending the utilization of policy-oriented social research:

(1) *interpersonal and organizational problems:* stemming from the relations between the research worker and the 'clientele' (operating agency, administrator, etc.):

(2) *scientific problems:* involving the difficulty of developing scientific researches adequate to the practical demands of the situation.

Although these types of problems are closely interrelated, and although we shall want to consider these interrelations, they should not be telescoped into one set of problems with the result that their distinctive aspects are lost to view. Interrelation should not be mistaken for identity. The sources of the organizational and scientific problems are different; the available means for coping with them are different; and their consequences for the development of applied social science are different.

Throughout our discussion, therefore, we shall attempt consistently to distinguish between the broadly "organizational" and the broadly "scientific" problems involved in the utilization of social science research.

4. **The Cultural Context.** The repute of applied social science, as of any other intellectual resource, is in part a product of its accomplishments. This is an interlocking system, in which social status and utilization interact endlessly. Not only does utilization affect esteem but esteem also affects utilization. The higher the social standing of a discipline, the more likely it will recruit able talents, the greater the measure of its financial support, and the greater its actual accomplishments. And closing the circle, the greater its utilization, the higher, ordinarily, its social standing. Even a cursory examination of the history of medicine or of physics will suggest the same pattern of interplay between cultural evaluations of the discipline, intellectual development of the discipline and utilization of the findings.

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The cultural context of evaluation, therefore, has a basic place in any analysis of the utilization of applied social science. And here, we find ourselves limited by an impressive gap in available data. What are the prevailing evaluations of social science? How do they differ among various groups and strata in the population? And how have they been changing in the course of time? Manifestly, we do not know. No systematic inquiries into the cultural evaluations of social science have been made.

In the absence of the facts, we must speculate on the prevailing public images of applied social science and on the determinants of these images. All this is premised on the view that these prevailing images in part determine the extent to which policy-oriented research in social science is sought, by whom it is sought, and the purposes for which it is sought.

Of the numerous dimensions which may be found in public images of social science, only a few can be itemized and fewer still, briefly discussed. Experience suggests at least the following dimensions of these images:

Objectivity: ranging from the view that social science is merely private opinion masquerading as science to the faith in its rigorously objective quality;

Adequacy: ranging from belief in its unmitigated futility to belief in social science as the means of social salvation;

Political relevance: ranging from belief in its inherently 'subversive' nature to belief that democracy can function adequately only if social science data are at hand;

"Costs": ranging from the naive view that scientific results can be obtained with little expense (of time and funds) to the view that usable results are so costly as to be 'uneconomic'.

Other possible aspects of prevailing images will readily come to mind, but these may suffice to set the problem. Of these, I should like to consider the first two in brief outline.

The Dimension of Objectivity. We do not know the frequency of these images ranging from the view that social researches can be (and have been) "used to prove almost anything" to the view that they are wholly objective, uncontaminated by the researcher's predilections.

The fact that clients often, perhaps typically, publicize the findings of applied social science only when these are in accord with their own interests probably helps spread this belief in the unobjective nature of this research. Thus, the *New York Times* has seized upon the curious coincidence between the interests of clients and social science findings to conclude, in effect, that the winds of social science bloweth where they listeth. When an applied economist files a research report for the CIO which differs basically in its findings from a comparable report filed by experts of the NAM, the *Times* not only stresses the discrepancies, but notes that, oddly enough, the disparate findings coincide with the rival economic positions of the sponsors. Competing interest-groups attack and counterattack with their own social science researches. This is not merely a problem of "who shall decide when doctors disagree?" Since they are ostensibly based on research, the disagreements may activate a disbelief in the objectivity of applied social research *in general*. The specific instance may be

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generalized with consequent deterioration of the status of the disciplines involved.

The difficulty of distinguishing between 'genuine' and 'spurious' social science research further supports this scepticism of objectivity. The layman (often including the administrator and potential client for research) cannot always discriminate between the "research" which has all the outward trappings of rigorous investigation (sampling, design, controls, etc.) but which is defective in basic respects, and the genuinely disciplined investigation. The outward appearance is mistaken for the reality: "all social researches look alike" to many laymen.

Since careless, undisciplined, irresponsible "research" may promise larger returns at less expense, there may be a tendency for "bad research to drive out good research." And when these spurious investigations are tested in the crucible of experience, the resulting disappointment may lead to a repudiation of social science in general.

The Dimension of Adequacy. There are apparently some enthusiasts who would seek in social science knowledge the vade mecum to a scientifically-planned and altogether desirable world. There are others who view applied social science as only an elaboration of the obvious, and who therefore consider it entirely dispensable as a basis for policy and action. Still others hold that social research is adequate when it deals with picayune problems and inadequate when it deals with "significant" problems. Here again, more information on the diverse images of adequacy and the comparative frequency of those images would be of value in helping to shape the future of applied social research.

Obviously, existing social science knowledge may be sufficient to deal with certain types of practical problems and wholly inadequate to deal with others. Thus, specific types of market researches may quite typically satisfy the needs of clients, whereas researches on, say, propaganda may prove typically unsatisfactory. The demands now made of applied social scientists may far outrun the *present* capacity and equipment of social science knowledge. As long as there is no roughly established inventory of our present knowledge such that laymen and scientists alike may have some approximate idea of applied researches which are and which are not promising for policy-decisions, this will continue to provide a flow of disappointment and a consequent devaluation of the adequacy of applied social research *in general*. It is unwise to permit exaggerated public images of the immediately attainable achievements of applied social science to go unchecked.[1a]

Reacting against *under*-estimates of the potentialities of applied social science, social scientists themselves may inadvertently supply exaggerated conceptions of what is now possible. Such propaganda for applied social science may boomerang and produce the excessive expectations which lead to subsequent

[1a] This general observation now takes on added force since the subsequent public reactions to the election forecasts on November 2, 1948 by the major polling organizations. It remains to be seen if the reaction against empiristic polling forecasts is generalized to the discredit of social science.

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disillusionment and popular reaction against the use of social science to any degree.

The preceding examples only touch upon the probably rich array of public images of applied social science. At this point, we can be confident only of two things. First, that we do not have adequate information on the range and comparative frequencies of these images and second, that this information would be useful. The NORC poll on the social status of occupations has some suggestive findings on the comparative status of some types of social scientists (economist, sociologist, psychologist, etc.). Apart from limited material of this sort, literally no systematic data exist on prevailing conceptions of social science in general and of applied social science in particular. Interestingly enough, the very social scientists engaged in studying standarized images of ethnic and racial groups, labor unions, business, *etc.*, have not yet begun studies of prevailing images of themselves.

There is plainly a need for an "applied social research on applied social research" to ferret out the public images of social science, particularly among makers of policy in government, labor and business.

The proposed inquiry may suggest appropriate lines of action. The role of the expert always includes an important fiduciary component. Laymen must be placed in a position where they can count on the responsible exercise of specialized competence by experts. Yet in contrast to the medical and legal professions, applied social scientists have not explored these problems of their own professional group. If the proliferation of irresponsible agencies of social research, for example, is found to be a major source of unfavorable and unrealistic images of applied social science generally, this may lead to recommendations for the regulation of these agencies.

5. **The Organizational Context.** The problems of utilizing applied social science research in policy-formation probably differ according to both the social position of the research agency and the client (or sponsor).[2] Each type of research agency may have diverse types of clients and each type of client may utilize diverse types of agencies.

To obtain a systematic picture of the various structures of social relations between researcher and clientele, we have only to cross-classify the two variables of research agency and of clients (as in the following specimen classification).

Each of the type-relations generated by this cross-classification can be readily identified. Thus, row A, column 1, "academic research agencies" with a governmental agency as client would include, for example, the contracted research conducted by the University of Michigan for the Treasury Department; row C, column 1 would include the Division of Agricultural Surveys in the Department of Agriculture; A-3, the Hawthorne studies conducted for the Western Electric Company by the Graduate School of Business Administration at Harvard; and so on.

Starting with some such classification, it should be possible to determine,

[2] The importance of this was long since recognized by Walter Lippmann, in his perceptive chapters on the potential role of the social science expert. *Public Opinion* (New York: Macmillan, 1922), Chapters 25, 26.

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through comparative analysis, the distinctive problems, procedures and effects upon research of these several structures of social relations between researchers and clients. Do these structures characteristically differ, for example, with respect to the role of the researcher in defining the problem, in the types of research problem at the focus of attention, in the type of interaction between researcher and client, in the relevance of the research for policy and action, in the degree to which the research findings are utilized for policy purposes, in the methodological and theoretical by-products of the research, etc.

This would provide a beginning toward systematically analyzing the part played in the formulation and utilization of applied social research by the organizational context. Thus, it may be found that when the organization of a client is itself the object of study (*e.g.* a corporation, governmental bureau or division, etc.), the research findings are more likely to be taken as a basis of policy when the research is done by 'independent' outside agencies than by a research staff

Synopsis of Social Structures of Researcher and Client

TYPE OF RESEARCH AGENCY	TYPES OF CLIENTELE				
	(1) Govern-ment Agency	(2) Founda-tions	(3) Business Corpora-tions	(4) Welfare Agencies	(5) Etc.
I. Research agencies independent of operating agency A. 'academic' (endowed in part or whole) B. 'commercial' (dependent on research income)	A-1		A-3		
II. Research agencies incorporated in operating agency C. permanent research staff D. *ad hoc* research staff (for limited period)	C-1				

which is itself part of the organization. Or, it may be found that applied social research for welfare agencies tends to have fewer methodological by-products than research for, say, business corporations.

It is in any case necessary to explore the assumption that the problems of making social research applicable will vary according to the organizational contexts. And to test this assumption, it is necessary to have some working classification of these contexts.

6. **The Situational Context.** There appears to be no literature which collates the types of situations leading to the decision to conduct a research in applied social science. Which occasions call applied research into being? And how do these different types of situations affect the nature of the research and its utilization?

The conventional picture of how this comes to pass is clear enough: a "problem" arises and the research worker, as a professional solver of problems, is asked

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to discover a solution. But who originally perceives the problem? Is it invariably the practical man of affairs, or, at times, the social scientist himself? And which types of "problems" are subjected to applied research and which are characteristically dealt with, without recourse to research? What are the functions of the research as conceived by the sponsor? And how does all this relate to the utilization and development of applied social science?

No systematic inventory of situational contexts is attempted here, but at least several can be identified. We can first consider the situations in which the need for an applied research is initially perceived by policy-makers or by social scientists.

Functions of Research Originated by Policy Makers

1. *Individuals or organizations confront the problem of 'influencing' or 'persuading' others to a given course of action.*

They seek 'objective data' to aid in persuasion. *E.g.*, an advertising agency has a research conducted in the hope of convincing a client of the greater effectiveness of their advertising program over alternatives proposed by rival agencies; a pressure group sponsors an applied research to obtain data in support of proposed legislation; a corporation vice-president solicits a research in defense of his policies as against those advocated by another vice-president; a group of public-spirited citizens advocate a research on racial segregation to demonstrate the dangers of segregation to the general public, *etc.*

Since the chief function of these researches is persuasion, they are perhaps more subject to the tendency to have the research findings exploited for propagandistic aims. In these instances, the research findings are not likely to be subjected to the test of experience. They serve primarily to lend support to pre-determined courses of action.

2. *Individuals or organizations confront problems requiring action by them, and find that they do not have sufficient information for 'intelligent' action.*

An industrial plant is repeatedly strike-bound; it tries a variety of expedients which are unsuccessful, and then turns to a research to suggest new alternatives.

Under which conditions is social science research sought? How does the pattern of action-oriented research differ from the pattern of persuasion-oriented research?

3. *Individuals or organizations wish to delay action to the point where the pressure for action from others is eliminated.*

In such contexts, the applied research is intended not to lead to action, but to preclude it. The function of the research is to allay criticism of inaction. Public officials not infrequently authorize a "thorough study" of a problem on which they do not wish to take action.

In different situations, then, the policy-maker may utilize applied research for quite different functions. We have mentioned three broad functions—persuasion, action, inaction. It is, of course, important to learn how each of these affects the nature of the research.

Functions of Research Originated by Social Scientists

1. *Social scientists may seek to sensitize policy-makers to new types of achievable*

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goals. Some applied research has its origins in the work of the academic social scientist. He may detect what he considers a "practical problem" which has not yet been so identified by the maker of policy. In these instances, it is the first task of the research worker *to create* a practical problem for the policy-maker.

For what is a "practical problem"? It represents a gap between aspiration and achievement, and holds out a challenge for closing this gap. If a policy-maker has certain aspirations which are moderately well met, he, of course, perceives no "practical problem." But, the social scientist may at times detect the possibility of at once heightening or extending these aspirations and of realizing the new goals. This requires him to serve as a gadfly, stinging contented policy-makers into a state of discontent by widening their horizons, by introducing new criteria of the achievable and by orienting applied research toward ways of reaching these new goals. Thus,

> the manager of a housing community may feel that it is running smoothly and well. He experiences no acute 'problem'. Rents are paid promptly, tenant turnover is low, few complaints reach him. An inquiring social scientist may find that there is little organized community life in the housing development and that the level of residents' satisfaction is less than it could be, if specific provision were made for community organization. In effect, the researches of the social scientist are here aimed at introducing new, and more demanding, criteria of a "satisfactory state of affairs," of extending the goals of the housing manager.

A major function of research emanating from social science circles, then, may be to establish new goals and bench-marks of the attainable.

2. *Social scientists may seek to sensitize policy-makers to more effective means of reaching established goals*. In much the same fashion, administrators may assume that their organization is operating at a satisfactory level of effectiveness. The social scientist may discover more effective instrumentalities for approximating present goals. The task here is one of modifying criteria of effectiveness of ways and means. Thus, output in an industrial plant may be judged satisfactory by the policy-maker. Further inquiry may show that this is at the expense of a rigorous regimen which puts considerable strain upon the workforce. Alternative methods may produce the same high level of output without exacting this price of workers. It is altogether likely, as these casual instances suggest, that the modification of criteria of effectiveness of ways and means will characteristically involve a modification of goals, as well. The pattern is the same in both types of instance: sensitizing policy-makers to a wider range of realizable potentialities.

Practical problems are many-faceted. They can be examined from the perspectives of several different disciplines. Increasingly, policy-makers have been weaned from the naive view that a practical problem is invariably in the orbit of one specialized body of science. High labor turnover, for example, is no longer automatically assumed to be in the province of "applied economics." Psychology and sociology may find partial determinants of rates of labor turnover in the human relations and social organization of the plant, or in the inadequacies of the local community from which the workforce is drawn. On what grounds,

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then, does the policy-maker select certain disciplines rather than others as most appropriate for studying the problems at hand?

This question introduces several considerations which can only be mentioned here. It points to the fact that for many, if not most, practical problems demanding applied research, collaboration among several disciplines is required. It suggests the role of the specialized research worker himself in acquainting the policy-maker with the need for such collaboration. It points to the major organizational and scientific problems of providing for collaboration between the several applied social scientists. (The experience of the Tennessee Valley Authority should be especially instructive in this connection.) And, anticipating a later section of this discussion, it suggests that *a major function of applied research is to provide occasions and pressures for inter-disciplinary investigations and for the development of a theoretic system of "basic social science,"* rather than discrete bodies of uncoordinated specialized theory.

7. Defining the Practical Problems and the Research Problems. 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 a contemplated research. This initial clarification of the *practical* problem, therefore, is the first crucial step in applied social science.

Some types of unwitting mis-statement of the practical problem by the client can be itemized here. Further inquiry will undoubtedly disclose others.

Over-Specification of the Problem. The policy-maker often assumes that he has precisely identified his particular problem and comes to the researcher with a specific request for research. But this may be premature specification. The researcher has the task of ascertaining the *central* pragmatic problem rather than passively accepting its initial specifications by the policy-maker.

Thus, a Jewish "defense agency" requests a research to determine which of alternative types of mass propaganda will probably be most effective in curbing anti-Semitism. This does not represent the *prime* objective which is "reduction of anti-Semitism." The policy-maker has prematurely included in his statement of the problem a specification of *means* as well as the end-in-view. The expert re-defines the practical problem. On the basis of previous researches, he indicates that deep-seated prejudices are not markedly vulnerable to propaganda campaigns. The problem becomes re-formulated: it is no longer an inquiry into efficiencies of alternative propaganda, but the comparative efficiency of a given propaganda campaign and of inter-religious voluntary organizations.

Over-Generalization of the Problem. Or, the maker of policy may assume that he has sufficiently stated his problem when he indicates his general objective. He may seek fuller participation of the rank-and-file in a labor union or reduction of race tensions or increase of college attendance. But each of these general objectives may be approached through very different types of procedures, requiring different types of research.

When the policy-maker over-specifies his practical problem, the expert must clarify by searching out the prime objective, thus often re-defining the problem.

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When the policy-maker over-generalizes his practical problem, the expert must clarify by searching out the various alternative instrumentalities, and determine the consequences of each of these.

8. **The Framework of Values in Definition of Problems.** *Value Framework of the Policy-Maker.* We assume that the policy-maker always has a set of values, tacit or explicit, and that this places limits upon the scope and nature of the applied research directed toward his problem. These "value constants" circumscribe the alternative lines of action to be investigated. It is the task of the researcher to search out these values in order to know in advance the limits set upon the investigation by the policy-maker's values. (This is not only an ethical task but also a technical task. If it is true that the policy-maker always assumes certain features of his problem-situation as *given*, as *constant*, as items which he would not under any circumstances consider modifying, this at once limits the range and type of research which will be done with his support, thus affecting the social scientist's decision to undertake the research.)

Thus, policy-oriented research is requested on ways and means of improving morale of Negro workers in an industrial plant. The constant assumed by the policy-maker: continued segregation of jobs, sanitary facilities, etc.
Policy-oriented research is requested on means for increasing sales of a product. The constant assumed by the policy-maker: no change in the product itself.

These value-constants are probably of limited types. Two major types are noted here:

Objective factors of the situation shall remain unchanged, while attitudes toward the situation are modified (e.g., not changing objective fact of segregation but modifying Negro workers' morale; not changing product, but increasing sales; research may show that the proportion of Negroes and whites in an interracial housing project must be administratively stabilized if it is not to become an all-Negro project, but the policy-maker rejects this research conclusion since it implies a "quota system" which offends his values; etc.).
Objective factors in the situation shall be changed, but no arrangements are to be made to modify attitudes. (E.g., eliminating racial segregation in a housing community but not providing for ways and means for local acceptance of this change.)

Value Framework of the Research Worker. The research worker also has his values, tacit or explicit, which affect his definition of the problem, the lines of investigation which seem to him most fruitful, the alternative policies to be explored, etc. These values can be detected by determining the researcher's self image of his role:

As a technician, he will accept alternative proposals for policy as a basis for research, providing only that these alternatives be technically amenable to research.
Since it is feasible to test symbolic ("psychological") measures for improving the morale of Negro workers without eliminating segregation, the technician finds this definition of the problem adequate and *confines* himself accordingly.
The researcher is asked to determine how a given radio program can reach a larger audience; since this is a feasible problem, he searches out strategic listening periods, etc. and is content to accept the policy-maker's constant of increasing audience without exploring effects upon audience-size of changing the program content.

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As a "socially-oriented" scientist, he will explore only those policy-alternatives which do not violate his own values.
He not only includes in his study symbolic means of improving worker morale (*e.g.* symbolic awards for performance, recreation groups, etc.) but also 'realistic' changes in situation (modified wage-policies, etc.)

Study of the actual role played by the values of policy-maker and researcher in the formulation of the research should help carry this question from the exclusively ethical context to that of the impact of values upon the relevance, scope and utility of the research itself.

9. **The Economic Framework of the Research.** *Whether "applied" or "pure," empirical research in social science is costly in time and money.* But the economics of empirical research may affect the patterns of applied research and of basic research in quite different fashion. To be sure, the applied and the basic research alike may have a fixed budget and a definite deadline. But this is not to say that the degree to which and the ways in which these affect the research are alike in the two instances.

The Tempo of Applied Research and of Policy-Decisions. The tempo of policy-decisions and action is often much more rapid than the tempo of applied research. Since action cannot always wait upon the completion of a research, varying degrees of urgency in decision affect research in various ways.

When there is great pressure for almost immediate decision, the *research* expert comes to be converted into the expert *advisor*. The policy-maker draws upon the cumulative knowledge of the expert and foregoes an actual research. At this extreme, urgency is lethal for research, though not necessarily for other social utilities.

When there is need for decision at a definite, but more distant occasion, a research may be designed to supply appropriate information. But since the 'key' problems cannot be adequately investigated within this limited period, the research is necessarily confined to 'practicable' though secondary problems. Furthermore, it becomes evident that data other than those needed for the immediate problem-in-hand may be expeditiously collected at the same time. But since this would prolong the period of field-work, these collateral materials are not included. The potential theoretic usefulness of the research is thus further circumscribed. As the research proceeds, fresh implications, not closely related to the present practical problem, are sensed by the research worker. These provocative clues, barely crystallized and wholly unformulated, are lost to view as the researcher bends to his immediate task of meeting the unalterable deadline. How often does the researcher return to the materials, after the deadline has been met (or not met), in an effort to recapture the fresh perceptions experienced during the research?

Occasionally, policy-oriented research escapes any marked time pressure. Certain types of data are periodically needed in order to take appropriate action. Or the research staff itself may at times reduce the pressure by anticipating future needs for decision, or by planning continuous researches in given areas so that the time required for new researches on a specific problem in that area is somewhat reduced.

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The Costs of Applied Research. Just as the pressure for immediate decision tends to eliminate research in favor of the considered judgments of expert advisors, so does the pressure to reduce costs. The comparative expensiveness of certain investigations leads to the substitution of advice for research. It would be useful to determine the grounds for opinions on the amount of money which can be justifiably expended for research on a given problem. How often is a given appropriation made first and the research tailored to fit this budget, and how often does the researcher plan the seemingly most appropriate research, and then have the estimated budget accepted? How does this differ as between researches in applied and basic social science? Since there exists no social bookkeeping for determining the "economic value" of *basic* research, the criteria for allocating funds to basic social science cannot be narrowly "economic" in character. But what of applied researches? Are the economic returns of specific applied researches typically estimated by sponsors or clients? And do these economic calculations determine their appropriations for research? Is there a tendency for applied researches to be diverted to peripheral problems when it is clear that research on the central problems-in-hand would be "too costly?" And since costs are inevitably increased by following up purely scientific leads developed in the research—leads which can have no value for the immediate practical problem—does this practice not limit the 'non-practical' by-products of applied research?

10. **Types of Research Problems in Applied Social Science.** Just as we suggested that patterns of applied social research may differ according to the organizational context (see Section 5), so we know that they will differ according to the type of problem in hand. The most fruitful bases for classifying these problems are by no means clear. From several possible classifications, one developed by the Columbia University Bureau of Applied Social Research is here presented for discussion.

Research Problems Classified According to Practical Purpose

1. *Diagnostic:* Determining whether action is required. Magnitude and extent of problem; changes and trends since last appraisal of situation (*e.g.* changes in level of race tensions); differentials in affected groups, areas, institutions.

2. *Prognostic:* Forecasting trends to plan for future needs. Predicting behavior of individuals and groups from stated intentions (post-war plans of demobilized soldiers; people's disposition of liquid assets); predicting needs by trend analysis and other hypothetical means (unemployment, wage, price trends from business-cycle analysis; predicting housing needs by analysis of birth and marriage rates and trends in size of family).

3. *Differential Prognosis:* Determining choice between alternative policies, (*e.g.* public reaction to rent control or rationing).

4. *Evaluative:* Appraising effectiveness of action program (assessing effectiveness of information and propaganda campaigns; of Emergency Maternal and Infant Care program in reducing infant and maternal mortality).

5. *General Background Data:* Of general utility or serving diverse purposes (*e.g.*, censuses of population, housing, business, manufacturing).

6. *"Educative" Research:* Informing publics upon pertinent data and particularly countering misconceptions.

"Strategic Fact-Finding:" this involves the systematic assembling of descriptive data

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pertinent for popular conceptions and controversial beliefs. Thus, facts pertinent to stereotypes: "labor-leaders-are-foreign" stereotype confronted with facts on place of birth of labor leaders; "United States-remains-the-land-of-increasing-personal-opportunity" conception confronted with periodic data on social mobility; "you-don't-need-a-college-education-to-get-ahead" conception confronted with data on correlations between education and income, occupation, etc.

To assess the current and potential role of applied social science, it is necessary to note the scope and scale of the practical problems with which it has dealt. These might range from broad, generic problems (generalized means of reducing crime, race hostilities) to highly circumscribed problems in a specific setting (the comparative effectiveness of two propaganda campaigns). It may develop that the extremes represent the least promising sectors of applied social science research. With the excessively large problem only failure can presently be reported and with the excessively limited problem, the results are often trivial. It would be important to identify the *stragetic, intermediate range of problems*, namely, those which have generalized theoretical and practical significance, but which are not too large in scope to be subjected to disciplined research.[3]

11. **Scientific Gaps between Research and Policy.** Several of the circumstances which seemingly make for applied researches *not* affecting policy have been considered. The values of the policy-maker, questions of time-and-cost, inadequacies in the formulation of the problem, etc., conduce to discrepancies between research-based recommendations and actual policies. As suggested previously, these gaps are of two interrelated types,—the "scientific" and the "organizational and interpersonal." Since each type raises distinct problems for the research worker, it is advisable to consider them separately.

The Research Is Not Adequately Focused on the Practical Problem (cf. section 7). When the research worker inadvertently accepts the 'overly-specified' or 'over-generalized' statement of his problem by the policy-maker, the resulting research will ultimately be found partly irrelevant to the actual problems of decision by the client. Alternative lines of action which have *not* been explored by the research may come to the later attention of the policy-maker and he will conclude that the choice between the explored alternatives is spurious.

Concrete Forecasts are Contingent upon Uncontrolled Conditions. Many, if not most, applied researches involve forecasts. *These concrete forecasts in applied science differ significantly from abstract predictions in basic science.*

Basic research typically deals with 'abstract predictions,' *i.e.* with predictions in which a large number of "other factors" are, conveniently enough, assumed to remain constant. The prediction will of course include a statement of the conditions under which the predicted consequences will probably occur, *Ceteris paribus* is an indispensable concept in basic research.

[3] Further observations on this point are presented by S. A. Stouffer, "The Strategy of the Social Sciences," address before the Harvard Graduate Forum, April 20, 1948 and by R. K. Merton, "Discussion of 'The Position of Sociological Theory,'" *American Sociological Review*, April 1948, 13, 164–168.

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In applied research, *ceteris paribus* is often an embarrassing obstacle—for what if the "other factors" do not remain constant? As a matter of well-known fact, the research worker in applied research is not permitted the luxury of the *supposition* that other pertinent factors will remain equal. If action is to be based on his findings, he must indicate whether relevant "other factors" *will* remain constant. And since they typically will not, he has the further large task of assessing the changes in these factors and their effect upon contemplated action.

In short, applied research requires the greatly complicated study of the interaction of many interrelated factors comprising the *concrete situation*. The research cannot be confined entirely to the interplay of a severely limited number of variables under severely limited conditions.

This requirement of applied research has several consequences:

(a) Every applied research must include some speculative inquiry into the role of diverse factors which can only be roughly assessed, not meticulously studied.

(b) The validity of the concrete forecast depends upon the degree of (non-compensated) error in *any* phase of the total inquiry. The weakest links in the chain of applied research may typically consist of the *estimates* of contingent conditions under which the investigated variables will *in fact* operate.

(c) To this degree, the recommendations for policy do not flow directly and exclusively from the *research*. Recommendations are the product of the research *and* the estimates of contingent conditions, these estimates not being of the same order of probability or precision as the more abstract interrelations examined in the research itself.

(d) Such contingencies make for indeterminacy of the recommendations derived from the research and thus create a gap between research and policy.

Diverse Utility of Samples for Different Types of Practical Problems. Though this is not peculiar to *applied* social research, it should be noted that adequate samples are not readily obtained in the study of certain types of problems. Public opinion and market researches typically sample aggregates of individuals and the findings can be readily extrapolated to the universes which have been sampled. But much greater difficulties are encountered in other spheres—for example, in studies of social organization. The units here are *not* individuals, but *organized aggregates of interrelated individuals*. And since the study of *one* such unit is ordinarily a major research enterprise, this leaves open the question to which the *single* unit under examination is representative of the universe of organized units. Thus, recommendations for policy based upon the detailed study of one bi-racial housing community may not be adopted in other such communities because the policy-maker feels that the communities are significantly different.

Further clarification is needed of the problems in which sampling problems can be met through available procedures and those in which the research, though involving hundreds or thousands of individuals, is essentially a case-study of one social unit. We have further to determine when a case-study is and is not regarded as an adequate basis for shaping policy.

12. **Interpersonal and Organizational Gaps between Research and Policy.** Other sections of the memorandum have touched upon some possible sources

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of gaps between research and policy which are interpersonal and organizational rather than strictly intellectual in character. There are others, some of which are here tentatively identified.

The Framework of Values Precludes Examination of Some Practicable Courses of Action (*cf. section 8*). It appears that practicable policy alternatives are not explored because they run counter to the values of the policy-maker or the research worker. (Thus, determining the most stable proportion of Negroes and whites in a bi-racial community may be rejected since it implies an objectionable 'quota-system.') In some cases, it is precisely the policy thus eliminated which most fully meets the requirements of the practical situation. Since these are ruled out, the resultant alternatives deriving from the research may be of dubious utility, and the research eventuates in inaction.

The Economic Framework May Lead to the Premature Conclusion of a Research (*cf. section 9*). It is evident that limitations of time-and-funds at times condemn an applied research to practical futility. In most investigations, there emerge alternative lines of inquiry which are not followed through simply because of budgetary fiat. In such cases, it often happens that the research findings may not be entirely adequate to arrive at the most appropriate recommendations for action. The gap between research and action could only be closed or narrowed by following up the emerging implications.

Attitudes of the Policy-Maker toward Risk-Bearing. Policy-makers differ in their attitudes toward the taking of risks. No matter how circumstantial and meticulous the research, there is an element of risk in following the recommendations which seem to flow from the research. The policy-maker may be more willing to take the risks involved in decisions based on his past experience than risks found in research-based recommendations. The applied scientist may be more often willing to support certain policies than the policy-maker, since it is the latter who takes ultimate responsibility for the decision.

In some instances, a given research, however competent, may seem too slender a basis for running the large risk. Thus, a bank or insurance company may hesitate to invest in an inter-racial housing development despite researches which suggest that the resulting problems can probably be 'managed.' The economic investment is large; deep-seated public attitudes are involved; once made, the decision cannot be easily modified. In such instances, it would not be expected that research, however sound intellectually, will appreciably modify prevailing policies. Correlatively, when risks are more limited—*e.g.*, the decision to introduce a new personnel selection policy or a new advertising campaign—a far-from-conclusive research may affect a decision.

Lack of Continuing Communication between Policy-Maker and Research-Staff. Once mentioned, this need not be elaborated. The problem is generally recognized and it is likely that data bearing on this problem are abundantly available.

Status of Researcher Vis-a-Vis Operating Agency. It is possible that the quality of the research does not completely determine its use: the status of the research worker may play a large part. Systematic inquiry into this possibility is indicated.

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The foregoing account is far from exhaustive. It does, however, suggest leads for determining how and why applied research does or does not provide a direct mandate for policy and does or does not eventuate in policy-formation. A key set of problems centers in the determinants of this leap from research to practice.

13. **Theory and Applied Social Science.** Everyone who has read a textbook on scientific method knows the ideally constructed relations between scientific theory and applied research. Basic theory embraces key concepts (variables and constants), postulates, theorems and laws. Applied science consists simply in ascertaining (a) the variables relevant to the problem in hand, (b) the values of the variables and (c) in accordance with previous knowledge, setting forth the uniform relationships between these variables.

It will be instructive to discover how often this ideal pattern actually occurs in the application of social science. We anticipate finding that it is the exceptional rather than the typical pattern. In one sense, a major objective of our proposed inquiry is to account for the discrepancies and coincidences between the "ideal pattern" and the "actual pattern" of relations between basic and applied social science.

In the present section, we confine ourselves to some remarks on the role of preliminary conceptualization in applied research. There is no danger that this will be mistaken for a comprehensive discussion.

Conceptualization at Work: the "Overlooked Variable." Perhaps the most striking role of conceptualization in applied social research is its transformation of practical problems by introducing concepts which refer to variables *overlooked* in the common-sense view of the policy-maker. At times, the concept leads to a statement of the problem diametrically opposed to that of the policy-maker.

Types of frequently overlooked variables will be ascertained through further inquiry, but a few can be set forth now.

Concept of the definition of the situation. Not all policy-makers have the practice of viewing policies from the perspective of others affected by the policy. As a result, they periodically find their decisions leading to a train of unanticipated, and often undesired, consequences.

Case: The policy-maker in the field of colonial administration may seek to "educate" the "native" by building and staffing schools for him. He, the administrator, sees this as beneficent activity. Education is a positive value, and he is making education available to the native. He is subsequently shocked by a nativistic reaction; the "ungrateful" natives rebel against this policy. The expert introduces the concept of definition of the situation and of culture differences. He indicates that western education, defined as an asset by the administrator in terms of his cultural values is defined by the natives as a device for cutting their children off from their traditional tribal values. The key concept brought to bear upon the problem by the expert is the different definition of the "same" situation by members of different cultural groups.

Case: illustrating *a variable below the level of awareness of the administrator*. An industrial manager hopes to achieve better employee morale and higher output by introducing a general rise in wages. He is disturbed when the expected results do not occur. The expert

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approaches the problem with a clarifying conception: wage *differentials* are of central concern to workers. Previous low morale had been a product of differentials conceived as unfair' by some groups of workers; the general rise in wages did not change the differentials.

The concept of a social system. Naive common-sense seldom thinks in terms of total systems of interrelated variables. Behavior is construed as a series of isolated events. Yet many of the untoward consequences of policy-decisions stem from the interaction between variables in a system.

As Wesley Mitchell has remarked in this general connection: "When some change in existing arrangements is proposed, our minds (i.e., of the economist) fasten immediately upon the effects this change will have upon other factors directly or indirectly, immediately or after a time: we think also about how these consequences will react upon the initial change .... *Obvious as the concept of the interdependence of all economic activities seems to us, it is not part of the working equipment of many lawyers, business men, or engineers,* if the able and patriotic dollar-a-year men I have collaborated with are a fair sample."

The Theoretic By-Products of Applied Research.[4]

In passing, we note two major relations between applied research and theory.

Applied Research Tests the Assumptions Underlying Theory. As noted earlier (section 11), basic research includes certain assumptions (*ceteris paribus*) in its abstract formulation of a problem. Since applied research is conceived as a basis for action, and since action must always occur in a *concrete* situation and not under abstractly envisaged conditions, the applied researcher is continuously engaged, *nolens volens,* in testing the assumptions contained in basic theory. This is perhaps a key function of applied research.

Immediate Pragmatic Success Postpones Theoretic Analysis. Not infrequently, applied research leads to an empirical finding which may be at once successfully applied, although the finding itself is not "understood" (*i.e.*, located) in theoretical terms. Thus, it may be found that provision for several rest periods in an industrial plant reduces labor-turnover, raises employee morale, *etc.* The plant manager who finds that this program "works" may see no occasion for further research. If the research worker is not theoretically sensitized, he, too, may be content with this "successful" application of an empirical finding. The fact remains that he has not yet identified the critical variable in this result: was it that rest-periods reduced fatigue? Or was it, possibly, that the degree of managerial concern with employees' problems (as symbolized by the rest-pauses) was the decisive variable? Or, again, was it the part played by employee representatives in arriving at the decision regarding rest-periods—in short, the manner in which this policy was introduced—that proved basic? Unless the crucial theoretical variable in the concrete practice of rest-periods can be identified, there is no basis for assuming that the same results will be obtained on other occasions. It will be of interest to learn if such practical successes tend to vitiate the continuance of inquiry until the theoretically significant findings

[4] Since I have provisionally discussed this in a paper presented to the American Sociological Society in 1946, I shall not comment on it further at this time. "The Bearing of Empirical Research upon the Development of Social Theory," *American Sociological Review*, October 1948, 13, 505–515. More study of actual cases is required to outline the conditions under which theoretic derive from applied research in social science.

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have been extracted from the empirical results.[4a] It is at least possible that specific practical successes may invite theoretical failures.[5]

14. **Methodology and Applied Social Science.** Just as with theory, so with the logic of procedure. The skeletonized version of relations between methodology and applied research found in textbooks is logically impeccable, but not always descriptive of what actually occurs. It will be necessary to review cases in point to determine the respects in which the ideal and actual patterns coincide or differ.

Without attempting any systematic discussion, we raise several questions which require study.

To what extent does the applied scientist's thorough familiarity with certain types of procedures and relative lack of familiarity with others, predetermine the design of the applied research? Do such predispositions toward procedures sometimes deflect attention from more appropriate though less well-known procedures?

Do applied researches more often call for quantitative treatment than do 'pure' researches? Is the policy-maker's concern with "how much" and "when" a prod to quantification? What are some of the scientific consequences of this pressure for quantification?

For which types of practical problems has non-quantified case study proved most appropriate?

One has the impression that the practical demands laid upon the applied research worker result in a continuing pressure for improvement of methods. The development of sampling procedure in social science, for example, appears to have been markedly advanced by applied researches in public opinion, market studies, etc.

[4a] Since this was written, the 1948 election forecasts have provided the most dramatic recent instance of the danger of operating with wholly empirical and theoretically ungrounded uniformities. In previous national elections it had been found that virtually no net shift in vote-intentions occurred during the last weeks of the political campaign. The extrapolation of this empirical pattern to the 1948 campaign by the polling organizations led to now familiar and unfortunate consequences.

[5] Since this was first written, James Bryant Conant has set forth apposite remarks (in his presidential address before the A.A.A.S., December 28, 1947): "To my mind we need to analyze the present situation, not by attempting to classify the various sciences and their subdivisions into pure and applied science, but by examining closely each separate undertaking. I have suggested in a paper on 'Science and the Practical Arts' that we need to inquire as to the *degree of empiricism* now present in any branch of science. The cases I quoted as examples were classical optics and chemotherapy. In the former *the conceptual scheme employed has wide validity, the degree of empiricism is very low. In the latter the concepts are few and of limited application,* progress toward a new drug is still very much of a 'cut and dry' affair, *the degree of empiricism is high* . . . . I should like to suggest that unless progress is made in reducing the degree of empiricism in any area, the rate of advance of the practical arts connected with that area will be relatively slow and highly capricious." (underscoring supplied). There remainds for us the question of the factors which make for the retention of a high degree of empiricism in much of applied social science, and it is in connection with this question that we make the suggestions found in the text at this point.

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We should like to learn whether the applied researcher is subjected to a greater variety of rigorous criticsm by diverse 'interested parties,' leading him perhaps to search out increasingly effective tools of analysis.

An inventory of methodologic by-products of applied social science would be similarly instructive.

In any event, the reciprocal relations between theory and methodology on the one hand, and applied social science on the other should constitute a major focus of inquiry.

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