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THE NEW REVOLUTION IN POLITICAL SCIENCE *

by David Easton

A new revolution is under way in American political science. The last revolution — behavioralism — has scarcely been completed before it has been overtaken by the increasing social and political crises of our time. The weight of these crises is being felt within our discipline in the form of a new conflict in the throes of which we now find ourselves. This new and latest challenge is directed against a developing behavioral orthodoxy. This challenge I shall call the postbehavioral revolution.

The initial impulse of this revolution is just being felt. Its battle cries are *relevance* and *action*. Its objects of criticism are the disciplines, the professions and the universities. It is still too young to be described definitively. Yet we cannot treat it as a passing phenomenon, as a kind of accident of history that will somehow fade away and leave us very much as we were before. Rather it appears to be a specific and important episode in the history of our discipline, if not in all of the social sciences. It behooves us to examine this revolution closely for its possible place in the continuing evolution of political science. Does it represent a threat to the discipline, one that will divert us from our long history in the search for reliable understanding of politics? Or is it just one more change that will enhance our capacity to find such knowledge?

NATURE OF THE POST-BEHAVIORAL REVOLUTION

The essence of the post-behavioral revolution is not hard to identify. It consists of a deep dissatisfaction with that kind of political research and teaching that is striving to convert the study of politics into a more rigorously scientific discipline modelled on the methodology of the natural sciences. Although the post-behavioral revolution may have all the appearances of just another reaction to behavioralism, it is in fact notably different. Hitherto resistance to the incorporation of scientific method has come in the form of an appeal to the past — to classical political science such as natural law or to the more loosely conceived non-methodology of traditional research. Behavioralism was viewed as a threat to the status quo; classicism and traditionalism were responses calculated to preserve some part of what had been, by denying the very possibility of a science of politics.

The post-behavioral revolution is, however, future oriented. It does not especially seek to return to some golden age of political research or to conserve or even to destroy a particular methodological approach. It does not require an adherent to deny the possibility of discovering testable generalizations about human behavior. It seeks rather to propel political science in new directions. In much the same way, behavioralism in the 50's, by adopting a new technology, sought to add to rather than to deny heritage. This new development is then a genuine revolution, not a reaction, a becoming, not a preservation, a reform, not a counter-reformation.

Post-behavioralism is both a movement, that is, an aggregate of people, and an intellectual tendency. As a movement it has many of the diffuse, unstable, even prickly qualities that the behavioral revolution itself once had in its own youth. It would be a serious mistake, indeed a grave injustice, to confuse this broad,

* Tekst van het 'presidential adres' uitgesproken door David Easton voorzitter van de American Political Science Association op 5 september 1969.

inchoate movement with any organized group either inside or outside the profession. Nor ought we to attribute any special political color to post-behavioralists in the aggregate. They range widely, from conservatism to the active left. Nor has this movement any particular methodological commitments. It embraces rigorous scientists as well as dedicated classicists. Neither does it appeal to any one age group alone. Its adherents include all the generations, from young graduate students to older members of the profession. This whole improbable diversity — political, methodological, and generational — is bound together by one sentiment alone, a deep discontent with the direction of contemporary political research.

Even though today the organized cleavages within our profession are writing most of the dramatic scenarios, in the end these cleavages may prove to be the least interesting part of what is happening. What will undoubtedly have far deeper meaning for us is the broader intellectual tendency that provides the environment within which current divisions have taken shape. It is on the purely intellectual components of post-behavioralism, therefore, that I shall focus.

New as post-behavioralism is, the tenets of its faith have already emerged clearly enough to be identifiable. They form what could be called a Credo of Relevance. I would describe the tenets of this post-behavioral credo as follows:

1 Substance must precede technique. If one *must* be sacrificed for the other — and this need not always be so — it is more important to be relevant and meaningful for contemporary urgent social problems than to be sophisticated in the tools of investigation. For the aphorism of science that it is better to be wrong than vague, post-behavioralism would substitute a new dictum, that it is better to be vague than non-relevantly precise.

2 Behavioral science conceals an ideology of empirical conservatism. To confine oneself exclusively to the description and analysis of facts is to hamper the understanding of these same facts in their broadest context. As a result empirical political science must lend its support to the maintenance of the very factual conditions it explores. It unwittingly purveys an ideology of social conservatism tempered by modest incremental change.

3 Behavioral research must lose touch with reality. The heart of behavioral inquiry is abstraction and analysis and this serves to conceal the brute realities of politics. The task of post-behavioralism is to break the barriers of silence that behavioral language necessarily has created and to help political science reach out to the real needs of mankind in a time of crisis.

4 Research about and constructive development of values are inextinguishable parts of study of politics. Science cannot be and never has been evaluatively neutral despite protestations to the contrary. Hence to understand the limits of our knowledge we need to be aware of the value premises on which it stands and the alternatives for which this knowledge could be used.

5 Members of a learned discipline bear the responsibilities of all intellectuals. The intellectuals' historical role has been and must be to protect the humane values of civilization. This is their unique task and obligation. Without this they become mere technicians, mechanics for tinkering with society. They thereby abandon the special privileges they have come to claim for themselves in academia, such as freedom of inquiry and quasi-extraterritorial protection from the onslaughts of society.

6 To know is to bear the responsibility for acting and to act is to engage in reshaping society. The intellectual as scientist bears the special obligation to put his knowledge to work. Contemplative science was a product of the nineteenth century when a broader moral agreement was shared. Action science of necessity reflects the contemporary conflict in society over ideals and this must

permeate and color the whole research enterprise itself.

7 If the intellectual has the obligation to implement his knowledge, those organizations composed of intellectuals — the professional associations — and the universities themselves cannot stand apart from the struggles of the day. Politicization of the professions is inescapable as well as desirable.

No one post-behavioralist would share all these views. I have presented only a distillation of the maximal image. It represents perhaps a Weberian ideal type of the challenges to behavioralism. As such the credo brings out most of the salient features of the post-behavioral revolution as it appears to be taking shape today.

SHIFTING IMAGES OF SCIENCE

What has this developing new image of political science to offer us? In the United States behavioralism has without doubt represented the dominant approach in the last decade. Will post-behavioralism destroy the undeniable gains of the behavioral revolution or is post-behavioralism only a valuable addition that can and should be incorporated into our practices?

One thing is clear. In a rapidly changing world surely political science alone cannot claim to have completed its development. Only on the assumption that behavioral political science has said the last word about what makes for adequate research and an appropriate discipline can we automatically read out of court any proposals for change.

The history of the various theoretical sciences, like physics and chemistry, reveals that every discipline rests on certain fundamental assumptions. It is a captive of what has been described as a research paradigm.¹ Over the years political science has been no less prone to develop models of what constitutes a good discipline or adequate research and these models have undergone marked transformations.

The behavioral model of this century has been but the last in a long chain. It has shifted the balance of concern from prescription, ethical inquiry, and action to description, explanation, and verification. Behavioralism has justified this shift on the grounds that without the accumulation of reliable knowledge, the means for the achievement of goals would be so uncertain as to convert action into a futile game. The growing success of the scientific enterprise in political science cannot be denied.

New conditions of the modern world, however, force us to reconsider our image of what we want to be. Scientific progress is slow, and however more reliable our limited knowledge about politics has become in the last fifty years, social crises of unforeseen proportions are upon us. Fear of the nuclear bomb, mounting internal cleavages in the United States in which civil war and authoritarian rule have become frightening possibilities, an undeclared war in Vietnam that violates the moral conscience of the world, these are continuing conditions entirely unpredicted by political science, behavioral or otherwise. The search for an answer as to how we as political scientists have proved so disappointingly ineffectual in anticipating the world of the 1960's has contributed significantly to the birth of the post-behavioral revolution.

In this perspective the legitimacy of raising doubts about the adequacy or relevance of political science in the contemporary world of crises cannot be questioned. We can join the post-behavioral movement at least in asking: Must

¹ T. S. Kuhn, *The Structure of Scientific Revolutions*, (Chicago: University of Chicago Press, 1962).

we be committed eternally to an unchanging image of the discipline, behavioral or otherwise? Is it not incumbent on us to take account of changing conditions and to be ready and willing to reconsider old images and modify them to the extent deemed necessary? Must political science continue to do what it has been doing over the last few decades, in the hope that some 'normal' period will one day return in which time will be on the side of those who seek to develop a more reliable understanding of political processes?

The negative answer that many individuals from all generations of political scientists are giving is clear. One of the probable underlying reasons for this answer we can readily understand. Mankind today is working under the pressure of time. Time is no longer on our side. This in itself is a frightening new event in world affairs. An apocalyptic weapon, an equally devastating population explosion, dangerous pollution of the environment, and in the United States severe internal dissension of racial and economic origin, all move in the same direction. They move toward increasing social conflict and deepening fears and anxieties about the future, not of a generation or of a nation, but of the human race itself. Confronting this cataclysmic possibility is a knowledge of the enormous wealth and technical resources currently available in a few favored regions of the world, the spectacular rate of increase in man's material inventiveness and technology, and the rich potential just on the horizon for understanding social and political processes. The agony of the present social crisis is this contrast between our desperate condition and our visible promise, if we but had the time.

In the face of a human situation such as this, the post-behavioral movement in political science (and in the other social sciences simultaneously) is presenting us with a new image of our discipline and the obligations of our profession. It pleads for more relevant research. It pleads for an orientation to the world that will encourage political scientists, even in their professional capacity, to prescribe and to act so as to improve political life according to humane criteria.

We can respond by refusing to budge, much as the classicists and traditionalists once did in the face of the onslaught of the behavioralists. Or we can recognize the need for change and explore the best ways reconstructing our conception of our discipline and of the related professional institutions of which we are part. It is the second course that I propose we consider.

THE IDEAL COMMITMENTS OF POLITICAL SCIENCE

A decision to contemplate revising the image of our discipline and profession places the political scientist in a strange and difficult predicament. Fierce pressures are building up for solutions to immediate problems. Yet the nature of basic research is to shift the focus away from current concerns and to delay the application of knowledge until we are more secure about its reliability.

This dilemma of contemporary political science is perhaps best revealed in the ideal commitments of behavioralism. For example, according to the behavioral image of science, those very epistemological characteristics of political research to which the post-behavioralists so strongly object would seem to be unavoidable, indeed, highly desirable. Post-behavioralism deplors what it views as technical excesses in research. Yet no one could possibly deny that technical adequacy is vital. Without it the whole evolution of empirical science in all fields of knowledge in the last two thousand years would have been in vain. Despite some post-behavioral objections to scientific abstractness and remoteness from the world of common sense by its nature science must deal with abstractions.

No science could by itself cope with the whole reality as it is interpreted by

the politician. Only by analysis, by chopping the world up into manageable units of inquiry, by precision achieved through measurement wherever possible, can political science meet the continuing need of a complex, post-industrial society for more reliable knowledge. Even to appeal to science to discard abstract theory and models as the test of relevance for research and to put in their place the social urgency of problems, is to ask it to sacrifice those criteria which have proved most successful in developing reliable understanding.

Furthermore, it appears that the use of the methods of behavioral science favors the very kind of sociological position for the political scientist to which post-behavioralism so strenuously objects. These methods help to protect the professional scientist from the pressures of society for quick answers to urgent if complicated practical problems. The history of the natural sciences shows us how slowly basic research moves. The overshadowing new ideas in the natural sciences — Newtonian mechanics, Darwinian evolution, Einstein's relativity, or modern cybernetics — come infrequently, on a time scale of centuries. But during the intervals between new ideas, great or small, science seeks to work out their implications with a passion for details, even if research seems to lead away from the practical, obvious problems of the day. These seemingly remote, often minute details, about scales, indices, specialized techniques for collecting and analyzing data and the like, these details are the building blocks of the edifice in which more reliable understanding occurs. What is true about the slow pace of basic research in the natural sciences and its remoteness we can expect to apply with equal force to the social sciences. Indeed in social research we even have difficulty in agreeing on the great discoveries, so undeveloped are our criteria of adequacy.

In addition, even if the political scientist begins with an immediate social problem, as he so often does, in the process of investigation he will be likely to restate the problem in more researchable terms. This reconceptualization usually leads him back to the very kind of fundamentals that appear irrelevant to initial practical concerns.

The ideology of pure basic research and its success in the better developed sciences in providing a reliable base of knowledge have seemed to justify this research strategy, slow and painstaking as it is. In helping to protect scholarship from the daily pressures of society for quick and ready answers, this ideology has freed science to pursue truth in the best way it knows how.

This same concern for generalized, verifiable understanding has forced social scientists to discriminate with extreme care about what we can and cannot do with our premises and tools. We can describe, explain, and understand but we cannot prescribe ethical goals. The value questions is thus set aside, not because we consider it inconsequential but only because we see it as unresponsive to the tools useful in analyzing and explaining the empirical world.

These then are some of the normal ideal commitments of science: technical proficiency in the search for reliable knowledge, the pursuit of basic understanding with its necessary divorce from practical concerns, and the exclusion of value specification as beyond the competence of science. It is these ideals that behavioral research in political science has sought to import into the discipline.

NEW STRATEGIES FOR SCIENCE

Today these traditional ideals of science are confronted with a set of social conditions which have no historical precedent. This extraordinary circumstance has created the predicament in which behavioral research now finds itself. It derives from the fact that we are confronted with a new and shortened time scale in the course of human events, one in which the future may need to

be discounted more heavily than ever before. For many, nuclear war or civil strife, with authoritarianism as a credible outcome, are clear and present dangers, to be counted in decades at the most. For many, without immediate and concentrated attention to the urgent issues of the present day, we may have no future worth contemplating, however uncertain our findings or inadequate our tools. How then can behavioral research, with its acknowledged glacial pace and apparent remoteness, hope to meet the demands now being placed upon our discipline?

For some post-behavioralists, the fear of physical and political self-destruction has led to the abandonment of science altogether. For them science is simply incapable of measuring up to contemporary needs. Others, who have always considered science to be inherently defective, now feel justified in their convictions. But for those post-behavioralists who continue to place their hopes in modern behavioral science, the current crisis poses the issue about the wisdom of continuing our commitment to a 'normal' strategy of scientific research. These kinds of post-behavioralists, have been driven to conclude that we have no alternative but to make our research more relevant. For them we can do so only by devoting all our professional energies to research, prescription and action with regard to the immediate issues of the day. In short, we are asked to revise our selfimage by postponing the demands of slow-moving basic research and by acting in our professional capacity so as to put whatever knowledge we have to immediate use.

For all of us this plea poses some critical questions. Even in the face of the social crises of our time, do we really need to subordinate the long-run objectives of the scientific enterprise to the undeniably urgent problems of the day? Is there any other way in which we can cope with this transparent need for practical relevance? And if so, can we hope to retain those conditions of theoretical autonomy, precision, and relative insulation for political science so vital if we are to continue to be able to add to our capital stock of basic understanding?

I would argue that we do not need to abandon the historical objectives of basic science. There is a strategy that will enable us to respond to the abnormal urgency of the present crises and yet preserve these traditions. By adopting this course, post-behavioralism need not be considered a threat to behavioral research but only an extension of it necessary for coping with the unusual problems of the present epoch.

To appreciate the strategy implied, we must remember one thing. Even if it is arguable that the time scale in terms of which we must think has been greatly shortened, mere projection cannot fully persuade us that the future needs to be counted in decades, not centuries. What little solace we may get from it, we know that our intuitions have been wrong in the past. We may still have centuries rather than only decades ahead of us.

This realistic possibility suggests that we ought to pursue an optimizing strategy in which there is some apportionment of resources for the long run as against the short run, just in case we are not in fact all dead. The cost of devoting our efforts exclusively to shortrun crises is far too high. It might easily assure that if we do in fact survive the present crises, the failure to continue to add to our capital accumulation of basic social knowledge will see us tragically unprepared for even greater crises in the more distant future. We will then have lost every chance to prevent the self-annihilation of mankind or the collapse of those political institutions we cherish.

Is there any sensible way in which we can provide for some satisfactory use of our resources without distracting excessively from the attention and altered research orientations that the major issues of the country and the world require? It is to this question that those of us who still have some hope that we may

survive the certain and greater crises of the near future ought to be devoting some of our energies. Various courses of action are possible, and we need to consider them as they apply to the discipline as well as to the profession.

THE DISCIPLINE

Basic vs. applied research. For the discipline, the post-behavioral revolution suggests the appropriateness of revising our ideal image at least as it has been incorporated into behavioralism. It is vital to continue to recognize the part that basic research ought to play. But in the allocation of financial and human resources we must also consciously recognize that a shift in emphasis must occur at once to take into account the critical times in which we live.

In terms of any ideal distribution of our efforts, basic research ought to command a disproportionate share. Although socially useful results from such research are usually a long time in coming, they are in the end more dependable. But under the inescapable pressure of current crises the emphasis needs to be reversed. A far larger part of our resources must be devoted to immediate short-run concerns. We need to accept the validity of addressing ourselves directly to the problems of the day to obtain quick, short-run answers with the tools and generalizations currently available, however inadequate they may be. We can no longer take the ideal scientific stance of behavioralism that because of the limitations of our understanding, application is premature and must await future basic research.²

In truth this proposal represents less of a shift in our practices than a change in our ideological posture. The behavioral revolution has never been fully understood or absorbed into the discipline; we are still grappling with its meaning. Any casual inspection of ongoing research would reveal that, regardless of any ideal apportionment, at no time has pure research really consumed more than a very small fraction of the resources of the discipline. We have been only too ready to advise federal, state and local agencies on immediate issues and political parties and candidates about their campaigns. It is just that with the behavioral revolution the ideals of the discipline as incorporated in research ideology were beginning to change. This new image legitimated the kind of basic research, the pay-off of which might not be immediately apparent, but the future promise of which was thought to be considerable. Today we need to temper our behavioral image of the discipline so that in these critical times we no longer see it as commanding us to devote most of our efforts to the discovery of demonstrable basic truths about politics. We will need to obtain more of our satisfactions from seeking immediate answers to immediate problems.

This kind of shift in disciplinary focus will call urgently for the systematic examination of the tasks involved in transforming our limited knowledge today into a form far more consumable for purposes of political action. Certain difficulties stand in the way of applying our knowledge. In the first place, contemporary social problems far outrun the capacity of political science alone or in concert with the other social sciences to solve them. Our basic knowledge is itself limited. What little we have is not necessarily directly applicable to practical issues.

In the second place, like medieval medicine, we may still be at the stage in which we are letting blood in the hope of curing the patient. Because of our low capacity for sorting out the complex causal connections between our advice

² See D. Easton, *The Political System*, (New York: Knopf, 1953), p. 78 ff.

and its social consequences, we have little assurance that we may not be doing more harm than good. Some efforts are currently under way to correct this situation. In the broadening quest for social indicators we are inventing techniques for isolating the outcomes of policy outputs³ and for comparing these consequences with the presumed policy goals.⁴ Thereby we shall have a measure of the effects of our intervention in the social processes. But the success of these efforts lies some distance in the future.

In the third place, political science alone is unable to propose solutions to social problems; these normally involve matters that call upon the specialized knowledge and skills of other social scientists. Yet seldom do policy makers seek the collective advice of comprehensive teams of social scientists.

These and many other difficulties have stood in the way of the application of our knowledge to specific situations. They have contributed to the low academic esteem of applied science, in comparison at least with basic research. Past efforts at application have experienced too little success to attract the best minds of the day.⁵ In temporarily modifying the immediate priorities of the discipline, we will need to devise ways for elevating the self-conscious development of applied knowledge, inappropriately called social engineering, to the respectability that behavioralism has succeeded in acquiring for basic research.

To assign all of our research resources to the present, however, as some post-behavioralists seem to be suggesting, would be to discount the future far too heavily. We need to keep alive and active the legitimate long-range interests of all science. Social problem solving is not totally inconsistent with this objective. The line between pure and applied research is often very fine. Those of us who choose to adopt the long-run point of view, optimistically expecting the survival of mankind, will find much from which to profit in the research undertaken by those concerned with applied problems. Yet this cannot relieve us of the need to continue to devote specific attention to basic problems in the discipline — to the reconceptualization of our significant variables, to the continuing search for adequate units of political analysis, to the exploration of alternative partial and general theories and models about the operation of various types of systems, and to our basic methodological assumptions and technical requirements. Admittedly these persisting concerns often lead us far from the practical issues of the day. Yet without attending to these basic problems we cannot hope to add to our store of reliable knowledge, and thereby to prepare ourselves for equally critical political crises in the more distant future.

Value premises and research interests. In addition to suggesting this temporary reallocation of our resources as between basic and applied research, we need to become increasingly aware of the fact that basic research is not without its own substantive deficiencies. This is the message underlying the constant post-behavioral complaint that our research is not relevant. It is argued that excessive preoccupation with techniques and with factual description has distracted us from the significant questions about the operation of the American democratic system in particular. We have learned a great deal about this system but all

³ For the difference between outcomes and outputs see D. Easton, *A Systems Analysis of Political Life*, (New York: Wiley, 1965) p. 351.

⁴ For the literature on social indicators see R. A. Bauer (ed.), *Social Indicators*, (Cambridge: M. I. T. Press, 1965); 'Social Goals and Indicators for American Society', *Annals of the American Academy of Political and Social Sciences*, vols. 371 (May, 1967) and 373 (September, 1967).

⁵ See H. W. Rieken, 'Social and Contemporary Social Problems', *23 Items*, (1969) 1-6.

within a value framework that accepts the ongoing practices as essentially satisfactory and at most subject only to the need for incremental improvements. As a discipline we have proved incapable of escaping a commitment to our own political system. This research myopia, the post-behavioralists argue, has discouraged us from posing the right questions for discovering the basic forces that shape the making and execution of authoritative decisions.

Here the post-behavioralists are alerting us, once again, to what has been repeatedly revealed over the years, by Marx, Weber, and Mannheim, among others, namely, that all research, whether pure or applied, of necessity rests on certain value assumptions. Yet the myth that research can be value free or neutral dies hard. We have continued to develop our discipline as though the subjects we select for research, the variables we choose to investigate, the data we collect, and the interpretations we generate, have all some extraordinary pristine purity, unsullied by the kinds of value premises to which we subscribe, consciously or otherwise. We do not consistently ask the question, central to the sociology of knowledge: To what extent are our errors, omissions, and interpretations better explained by reference to our normative presuppositions than to ignorance, technical inadequacy, lack of insight, absence of appropriate data, and the like? Behavioralists have indeed failed to insist, with the same fervor we have applied to our technological innovations, that our operating values be brought forward for self-conscious examination and that their impact on research be assessed.

Today the hazards of neglecting our normative presuppositions are all too apparent. There can be little doubt that political science as an enterprise has failed to anticipate the crisis that are upon us. One index of this is perhaps that in the decade from 1958 to 1968, the *American Political Science Review* published only 3 articles on the urban crisis; 4 on racial conflicts; 1 on poverty; 2 on civil disobedience; and 2 on violence in the United States.⁶

In some considerable measure we have also worn collective blinders that have prevented us from recognizing other major problems facing our discipline. For example, how can we account for the failure of the current pluralist interpretations of democracy to identify, understand, and anticipate the kinds of domestic needs and wants that began to express themselves as political demands during the 1960's? How can we account for our neglect of the way in which the distribution of power within the system prevents measures from being in sufficient degree and time to escape the resort to violence in the expression of demands, a condition that threatens to bring about the deepest crisis of political authority that the United States has ever suffered? How can we account for the difficulty that political science as a discipline has in avoiding a commitment to the basic assumptions of national policy, both at home and abroad, so that in the end, collectively we have appeared more as apologists of succeeding governmental interpretations of American interests than as objective analysts of national policy and its consequences? Finally, in even so recent a major research as political socialization, how can we account for the natural, effortless way in which inquiry has sought to reveal the contributions of preadult political learning to the stability of systems, virtually ignoring the equally significant function of socialization in bringing about political change?⁷

There is no single explanation for the narrow vision of our discipline. We can, however, at least go so far as to offer this hypothesis: Whatever the reasons,

⁶ This undoubtedly reflects only the few articles on this subject submitted for publication rather than any editorial predisposition.

⁷ See D. Easton and J. Dennis, *Children in the Political Systems Origins of Political Legitimacy*, (New York: McGraw-Hill, 1969), chapter 2.

the failure to broaden the vision of our basic research may well be due in good part to a continuing hesitation to question our normative premises and to examine the extent to which these premises determine the selection of problems and their ultimate interpretations.

Creative speculation. How are we to make those serious efforts necessary to break out of the bonds imposed on basic research itself by ongoing value frameworks? How are we to create those conditions that will help us to pose fundamental questions about the operation of political systems, that will lead us to pose those 'outrageous hypotheses' about which Robert Lynd once chided us?⁸ A new awakening to the part that our value commitments and other social influences play in limiting the range of our basic research may partly correct the errors of our ways. But this moral self-scrutiny may not be enough. If we are to transcend our own cultural and methodological biases, such self-awareness can carry us only part of the way. We may need to take stronger measures and find additional help by returning to an older tradition in political research but in a thoroughly modern way.

Many years ago, in *The Political System* I argued for the urgent need to reconsider our approach to value theory at the same time as we began the equally critical task of constructing empirical theory.⁹ The latter task is now under way in our discipline. The first one, creative construction of political alternatives, has yet to begin.

To enrich their own understanding and to give broader meaning to their own social reality, the great political theorists of the past found it useful to construct new and often radically different conceptions of future possible kinds of political relationships. By formulating such broad, speculative alternatives to the here and now we too can begin to understand better the deficiencies of our own political systems and to explore adequate avenues of change that are so desperately needed. This, I would argue, must now be considered part of the task and responsibility of science if it is to retain its relevance for the contemporary world. Those philosophies that seek to revive classical natural law and that reject the possibility of a science of man have thereby forfeited their opportunity and put in question their fitness to undertake this creative task of theory. We require boldly speculative theorizing that is prepared to build upon rather than to reject the findings of contemporary behavioral science itself and that is prepared to contemplate the implications of these findings for political life, in the light of alternative, articulate value frameworks.

The significance for political science of this kind of creative speculation cannot be overestimated. For those who seek to understand how political systems operate, such speculation provides alternative perspectives from which to determine the salience of the problems they choose for research and analysis. If we take seriously the conclusions of the sociologists of knowledge, then our scientific output is very much shaped by the ethical perspectives we hold. In that event, by failing to encourage within the discipline creative speculation about political alternatives in the largest sense, we cannot help but imprison ourselves within the limitations of the ongoing value framework. As that framework begins to lose its relevance for the problems of society, its system-maintenance commitments must blind us to urgent questions emerging even for the immediate future.

And this is precisely what has happened to political science. Both our philo-

⁸ R. S. Lynd, *Knowledge for What?* (Princeton: Princeton University Press, 1939).

⁹ D. Easton, *The Political System*, chapters 9 and 10.

sophers and our scientists have failed to reconstruct our value frameworks in any relevant sense and to test them by creatively contemplating new kinds of political systems that might better meet the needs of a post-industrial, cybernetic society. A new set of ethical perspectives woven around this theme might sensitize us to a whole range of new kinds of basic political problems worth investigating. It might also point up the significance of inquiry into these problems with new or radically modified types of relevant empirical theories. Thereby we could perhaps be freed from that occupational myopia brought about by excessive attention to the facts as they are. We would perhaps be less prone to stumble into the pitfall of 'empirical conservatism'¹⁰ or commitment to system-maintenance perspectives, of which political science has with justice been accused,¹¹ by post-behavioralists, among others.

In these several ways then does our discipline need reordering. Basic research needs to be maintained as an investment for the future. But even its priorities need to be rearranged in the light of a better understanding of its own value assumptions. Applied, action-oriented research requires more systematic attention than ever before. We need greater awareness of the limits that our value premises have imposed on our research. And on the solid foundation of knowledge constructed by behavioral research, alternative possible rearrangements of our political relationship need to be seriously contemplated.

THE PROFESSIONAL AND THE USE OF KNOWLEDGE

Not only our discipline, however, but our profession needs restructuring to bring it into harmony with the changing conceptions of social science. Our discipline refers to our intellectual enterprise; our profession, to the trained and expert scholars who participate in the discipline. Post-behavioralism suggests that behavioral commitments create not only a discipline but a profession that shows a declining relevance to the political world around it.

The behavioral image of the profession. Two basic reasons account for this decline, it is in effect argued. First, professionalization of the discipline in behavioral terms has nourished an image of political science in which knowledge and action have been carefully separated and compartmentalized.¹² As scientists possessed of special skills, we see ourselves as purveyors of something called professional expertise. Our task as experts is to offer advice about means only, not about the purposes to which our knowledge might be put. As the well-worn adage puts it, we are on tap, not on top.

In fact, as post-behavioralism correctly asserts, the expert has never lived by this rule. In the discipline, as we have already noted, behavioral inquiry has not been able to attain any real measure of ethical neutrality. This has had serious consequences for basic research. In the profession too, the critics point out, ethical neutrality is no less spurious. In the application of his knowledge the political scientist explicitly or unwittingly accepts the value premises of those he serves. His posture of neutrality has the added consequence of undermining his will or capacity to challenge the broader purposes to which his knowledge is put.

A second reason accounts for the decline of professional relevance. Here post-

¹⁰ H. Marcuse, *One-Dimensional Man*, (Boston: Beacon Press, 1964), chapter 4.

¹¹ See C. A. McCoy and J. Playford (eds.), *Apolitical Politics* (New York: Crowell, 1967) and D. Easton, *The Political System*, chapters 2 and 11.

¹² See especially T. Rozak (ed.), *The Dissenting Academy* (New York: Random House, 1968), Introduction.

behavioralism breaks sharply with the prevailing professional paradigm about the moral relationship between research and action. In the behavioral interpretation, the possession of knowledge imposes no special obligation on the political scientist to put his knowledge to use in the service of society. He remains free to choose whether or not he ought to step outside his engagement for this purpose. This laissez faire attitude towards political engagement has been an accepted moral premise of the profession. It has permitted if not encouraged withdrawal from political strife. Knowledge is divorced from action.

For post-behavioralism, however, the line between pure research and service begins to fade. Knowledge brings an awareness of alternatives and their consequences. This opportunity for rational choice imposes special obligations on the knower. The political scientist as a professional is the knower *par excellence*. It is therefore immoral for him not to act on his knowledge. In holding that to know is to bear a responsibility for acting, post-behavioralism joins a venerable tradition inherited from such diverse sources as Greek classical philosophy, Karl Marx, John Dewey, and modern existentialism.

Criteria for the use of knowledge. The implications of this post-behavioral shift in the image of the professional's role in society are considerable. If the political scientist is to evaluate the uses to which his knowledge is being put and if he is himself to bring his knowledge to bear on social issues, what criteria are to guide his choices? Here post-behavioralism returns to the humanist conception of the intellectual as the guardian of those civilized, humane values known to most men. It is incumbent on the professional to see to it that all society, not just a privileged part, benefits from his expertise. His obligations are met only if he takes into account the broadest spectrum of interests in society.

Many post-behavioralists scrutinize the activities of scholars in recent years and conclude that the talents of political scientists have been put in the service largely of the elites in society — in government, business, the military and voluntary organizations. The professional is seen as having little communication and contact with those who characteristically benefit least from the fruits of modern industrial society — the racial and economic minorities, the unrepresented publics at home, and the colonial masses abroad. These are the groups least able to command the resources of expertise for which political science stands. The social responsibility of the political science expert is to rectify the imbalance.

In this post-behavioral view, the application of expert knowledge in the service of social reform becomes competitive with the pursuit of knowledge for its own sake. Reform becomes inseparable from knowledge.

Clearly there is in birth a new image of the professional, one in which science is not necessarily denied its place but in which the scientist is no longer free to divorce the life of the mind from the life of social action. Weber's differentiation between the vocation of the scientist and that of the politician no longer wholly suffices.

This new image leads to the politicization of the profession. If the individual professional is called upon to utilize his knowledge on behalf of society, those collectivities of experts that we call the professional associations are themselves equally culpable if in their corporate capacity they fail to challenge the purposes to which their expertise may be put or if they fail to act when their knowledge warns them of danger. Herein lie the moral and intellectual roots of the constant pressure on the professional associations to take positions on public issues about which their competence may give them special knowledge.

The politicization of the profession. This post-behavioral tendency to politicize the professional associations has met with great resistance. Objection arises less

from principled argument than from the practical fear that our professional associations will no longer be able to fulfill their normal scientific purposes. Let us grant the plausibility of this practical consideration. Even so, do we need to reject entirely the new moral image being developed by post-behavioralism? One fact is clear. The crisis of our times spares no group, not even the social sciences. The pressures to utilize all of our resources in critically evaluating goals as well as in providing effective means are too great to be denied. For increasing numbers of us it is no longer practical or morally tolerable to stand on the political sidelines when our expertise alerts us to disaster.

In accepting this new (but ancient) obligation of the intellectual, however, we need to recognize that the professional political scientist may engage in three distinguishable kinds of activity. These are teaching and research on the one hand and practical politics on the other. Somewhere between these the political scientist acts as a consultant and an adviser. Each of these kinds of activity — as a scholar, politician, and consultant — shapes and influences the other. Is it feasible to construct a single organization that will serve the collective purposes of the profession for facilitating all three of these kinds of activities? It seems highly unlikely. Can we provide some sensible division of labor among different organizations that will permit the fullest expression for all those activities into which these critical times are pressing the professional political scientist? This seems possible.

We can conceive of some professional organizations being devoted largely to that kind of action that helps to add to our store of basic knowledge and that eases communication among ourselves and among succeeding generations of political scientists. These we already have in our professional associations. They are designed to aid both teaching and research. We can, however, also conceive of other types of professional organizations that would be concerned with structuring the application of our expertise to ongoing critical social problems. This kind of organization we do not yet have in political science, or, for that matter, in the social sciences as a whole.

But here if we consider the matter only as political scientists we create insurmountable difficulties for ourselves. Social problems do not come neatly packaged as economic, psychological, political and the like. Our crises arise out of troubles that involve all aspects of human behavior. Our professional associations are oriented towards the disciplines, and these are analytic fields. Of necessity they piece up reality into specialties that have meaning largely for the pursuit of fundamental understanding. For purposes of setting goals and determining means for solving social problems, however, we need to draw the discipline together again into a single organization, one that can mobilize the resources of all the social sciences and bring them to a focus on specific issues.

To this end it is time that we accept our special responsibility as students of politics. We must take the initiative by calling for the establishment for a Federation of Social Scientists, a proposal that has already been advanced by one of our colleagues.¹³ The tasks of such a Federation would be to identify the major issues of the day, clarify objectives, evaluate action taken by others, study and propose alternative solutions, and press these vigorously in the political sphere.

Without collectively politicizing ourselves in this way, by the very act of standing by while the problems of the world continue to increase in numbers and intensity, we thereby uncritically acquiesce in prevailing policies. We in fact adopt a political position. By acting collectively in our professional capa-

¹³ David Singer of the Mental Health Research Institute, University of Michigan, Ann Arbor, Michigan, in personal correspondence.

cities through a Federation of Social Scientists, we will have an opportunity to justify our policies intellectually and morally. Thereby we may begin to satisfy our growing sense of political responsibility in an age of crisis. At the same time we shall be able to preserve our historic institutions, the professional associations, for the continuing pursuit of fundamental knowledge.

Such a Federation would fail in its responsibilities, however, if it became merely an echo of national goals, an instrument of official policy, or a bland critic of things as they are. If Mannheim is correct in describing the intellectual as the least rooted of all social groups, the professional social scientist ought to view himself as committed to the broadest of humane values. These need to be the touchstone that he brings to bear on social issues. Yet many barriers block the way. Of these identification with the goals and interests of one's nation is prominent. Political scientists have still to escape the crippling effects for scholarship of unwitting commitment to national goals and perspectives. Just as science as a set of disciplines has pretensions to being international in scope, so the social scientist himself needs to be denationalized. Some day, like the ideal international civil servant, the professional social scientist too may be permitted to achieve maximum freedom from national commitments by being obliged to carry an international passport and to conduct himself accordingly.

For the profession, therefore, the emerging post-behavioral phase is encouraging the development of a new norm of behavior. It sees policy engagement as a social responsibility of the intellectual, whatever the institutional form through which this may be expressed. Some day it may also require the release of the social scientist from bondage to the unique needs and objectives of his own national political system.

It is clear that changing times require radical rethinking of what we are and what we want to be both as a discipline and as a profession. Post-behavioralism is a pervasive intellectual tendency today that reveals a major effort to do just this. Its very pervasiveness prevents it from becoming the possession of any one group or of any one political ideology. It extends behavioral methods and techniques by seeking to make their substantive implications more cogent for the problems of our times. Post-behavioralism stands, therefore, as the most recent contribution to our collective heritage. For that very reason, as an intellectual tendency it is not the threat and danger that some seem to fear. Rather, in the broad historical perspectives of our discipline, the post-behavioral revolution represents an opportunity for necessary change. We may choose to take advantage of it, reject it or modify it. But to ignore it is impossible. It is a challenge to re-examine fearlessly the premises of our research and the purposes of our calling.