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DISILLUSION AND REGENERATION: THE QUEST FOR A DISCIPLINE*

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A discipline, at least to the initiated, is known more by the questions it asks than by the answers that it provides. For questions indicate goals or aspirations that answers may not reach. At certain periods, however, a field of knowledge may be more conspicuously characterized by the controversies that occur among those who work in it. When these take the form of debates over the adequacy of particular answers, as determined by agreed, even though sometimes imprecise, criteria of assessment, they are unlikely to be highly prominent, except for the immediate participants. When, on the other hand, such controversies extend to the standing of the questions asked and place in dispute the means of appraising answers, it becomes obvious to all that something is happening that has implications for the entire field. Clearly the discipline is undergoing redefinition or at least an attempt at redefinition that may sharply alter its meaning. If the criteria for admitting questions and for validating answers are changed, the discipline is changed. Inescapably, therefore, controversies of this sort involve participants and observers in considerable numbers.

The pertinence of these observations to the field of political science in the past decade or so needs no elaboration, I am sure. It may be less obvious, however, that this situation in the field has a history that extends back a good many years. A view of that history may both shed useful light on our current controversies and perhaps help us to assess their implications.

At the very outset I am willfully going to commit the sin of parochialism by confining my remarks primarily to the discipline in the United States. In justification I would argue that the problems of political science are, if only because of the number of practitioners involved, chiefly problems of American political science. In the period on which I shall focus, beginning roughly two decades before the turn of the century, the influences upon the men who were developing the American discipline did not have their origin solely in the United States, but their responses to these influences were highly distinctive. These responses, moreover, set the stage for the controversies that have had their center in the United States, though their margins have extended far beyond this country.

In thinking about the contemporary development of political science I find particularly suggestive the notion of the paradigm, which is one of the two key concepts in Thomas S. Kuhn's *The Structure of Scientific Revolutions.*[1] By this term he means a common set of beliefs, constituting a kind of open-ended model that more or less explicitly defines the legitimate problems and methods of a research field, the working elements of what he calls "normal science." The developmental pattern of a mature science, he argues, is a movement from one generally accepted view of a research field to a successor. Prior to the emergence of such a research consensus, a field—such as physical optics before Newton—is characterized rather by a number of competing schools, not necessarily "unscientific" but bearing at most a loose resemblance to one another.

Kuhn is concerned, of course, with the physical sciences, but he explicitly does not rule out the possibility that comparable patterns of development characterize the social sciences. He leaves open the question of whether any parts of the social sciences have yet acquired para-

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[1] Chicago, University of Chicago Press, 1962.

digms or ever will. He would not, however, deny that many, if not all, of those fields display most of the features of the pre- or non-paradigmatic stage of inquiry.

In the formative years of political science in the United States, in the decades around the turn of the century, the field did not have a paradigm, nor has it acquired one since. Unquestionably the absence of such an agreed model has influenced the pace and pattern of change in the field, for a crucial feature of a true paradigm is its precision. This gives form and direction to "normal science." More important for the birth of scientific revolutions, precision in the paradigm permits the investigator to know when something is wrong, *i.e.*, contrary to what should be expected, and to see the need for paradigm change. In the absence of a paradigm and hence of a relatively precise means of knowing where, if not why, an existing conception is deficient, dissatisfactions with existing formulations in a field are not eliminated. Rather they may be multiplied. But they are likely to lack the coherence and the sharpness of focus that can be provided by a true paradigm.

I think it is accurate to argue, nevertheless, that something loosely analogous to a paradigm characterized American political science for at least the half-century running from sometime in the 1880s into the 1930s. In order not to distort Kuhn's provocative conception, this should be thought of as simply an implicit though fairly general agreement on what to do and how to proceed in the field. Because the matters in agreement were vague and the terms applicable to them loose and imprecise, they permitted a product diverse both in quality and in intent. Despite this diversity, however, the works of most political scientists had a number of qualities in common that serve to identify the nature of the general agreement and the form and character of the discipline.

The political science that emerged about the turn of the century gradually displaced and was in considerable part a revolt against an older tradition of preceptive and scholastic formalism. The new trend was toward contemporaneity and "facts," in the name of "realism" and of "science." Its early stirrings can be illustrated by Woodrow Wilson's *Congressional Government* (1885) but interestingly not by his later treatise on *The State*, which was clearly of the older mold. More than by Wilson or by any American, the note of change was struck by James Bryce in his *American Commonwealth* (1888). The trend that it signified lasted well into the 1930s, but it reached something of a peak about the turn of the century as part of the general pursuit of "realism" that was characteristic of the Progressive Movement in literature, in journalism, and in most of the emerging social sciences.

At the risk of distortion and over-simplification, I shall propose six closely related features of this predominant agreement that profoundly marked the discipline. That mark helps to put our current quandaries and controversies in instructive context. These six features, stated for the moment without elaboration, were: (1) an unconcern with political systems as such, including the American system, which amounted in most cases to taking their properties and requirements for granted; (2) an unexamined and mostly implicit conception of political change and development that was blandly optimistic and unreflectively reformist; (3) an almost total neglect of theory in any meaningful sense of the term; (4) a consequent enthusiasm for a conception of "science" that rarely went beyond raw empiricism; (5) a strongly parochial preoccupation with things American that stunted the development of an effective comparative method; and (6) the establishment of a confining commitment to concrete description.

Lack of concern for the political system as such meant, first of all, that the discipline worked within implicit and common-sense assumptions about the requirements of the system, its boundaries, and the articulation of its elements. Yet these assumptions essentially fixed many of the questions that the discipline could ask and set the limits on the answers that could be supplied, whether in analytical or in prescriptive terms. This in turn meant that particular segments of the system could be described and prescribed for without reference to implications for or effects upon the system as a whole. This tendency, it seems to me, is at the heart of the negative connotations recently attached to the label "institutionalist." The target of such criticism can hardly be the study of institutions, since one can scarcely imagine a political science in which institutions, as persistent patterns of political action, would not be a proper and central focus of concern. Criticism rather bears on treating an institution in its own terms, at face value, and without reference to other portions of the inclusive scheme or to the functions of a particular pattern within the system.

Unconcern with the system as such meant that political scientists, and commentators generally, assumed in effect a kind of atomistic non-system. Preoccupation with isolated particulars and a commitment to the concrete and the "practical" led so far away from concern with the political system as a whole that it was virtually assumed out of existence. The field in consequence was condemned to working within the conventional, legalistic conceptions of the system, since it lacked the means of dealing with it, conceptually and empirically, in any other terms.

Closely and quite logically associated with this unconcern for the characteristics of the system was a view of political change or development that optimistically and uncritically assumed an inevitable progress toward democracy and the rule of law, provided only that enlightenment through education and the public channels of communication was sensibly provided for. The apparent facts of the relevant political world—Western Europe, the Americas except for some trouble spots south of the border, the self-governing British Dominions, China, and Japan—did not, at least until the 1920s, sharply contradict this bland view. In any case, the conception was essentially taken for granted. This did not inhibit, rather it encouraged, an enthusiasm for particularistic reform. Unconcern about the political system permitted championing proposals such as the direct primary without attention to their ramifying effects, and the assumption of democratic progress confidently justified the effort. An uncritical preoccupation with reform relied upon a conception of linear democratic development that seemed likely to terminate in a withering away of politics and the realization of an immanent harmony among enlightened and right-minded men.

It is not remarkable that this unconcern with the political system and with a critical view of political change should have carried with it, especially among American political scientists, an indifference to theory as an element having anything but a conventional, ornamental utility for the field. Theory in any systematic sense was for the most part peripheral, not in the main stream of the discipline. Albert Bushnell Hart was not much exaggerating the temper of his profession when he observed in 1907, "The most distinctive American theory of government is not to theorize."[2] This attitude was in part an over-reaction to the abstract formalism of an earlier day, but it also followed logically from the unexamined assumption that the system provided its own theory and that the task before the profession was to facilitate the inevitable flowering of democracy. Small wonder if the textbooks through which students in this period were introduced to political science (via American government) changed their implicit conceptual positions from chapter to chapter, from page to page, and even from paragraph to paragraph. A less obvious consequence of this indifference to theory was that the sub-fields of political science tended to develop in increasing isolation. They had nothing in common except that many of them dealt with American phenomena and that all of them talked about government in some way. Without theory, politics tended to become defined as itself an isolated sub-field, rather than as the process permeating every phenomenon studied by the discipline.

The discipline in these years also tended to take its "science" seriously, but the type of science, almost inevitably, was a rank, nontheoretical empiricism. The dominant commitment was to the collection of facts, in many cases almost for their own sake. This preoccupation may have been one influence toward developing, especially in the 1920s, new devices for the collection and analysis of data—primitive population samples, interviews, word-association tests, and so on. But the climate of the field was such that the facts normally were assumed to speak for themselves. Empirical description, usually in association with uncritical reformist inferences, was a hallmark of the field.

An understandable consequence of the new realism was that political science became increasingly parochial, primarily engrossed in things American and, almost inescapably, in the minutiae of American phenomena. A natural result of this and related commitments was that the discipline did little to develop comparison as a basic component of investigation. One of the saving features of the older formalist persuasion was that it had displayed at least a rudimentary interest in comparative analysis. After the succession of the new realism, however, what was known as "comparative government" largely involved a description of the institutions and structures of particular European governmental schemes. The comparative element rarely went beyond legalistic distinctions and literary gestures.

Finally and ironically, all five of these factors combined to insure that, in rejecting confinement by the abstract formalism of the earlier period, the new realism adopted instead an equally confining and in some ways even more rigid mode of concrete description. Without an explicit concern for political systems as such, without an interest in the patterns and directions of political change, without some com-

[2] Albert Bushnell Hart, "The Growth of American Theories of Popular Government," this Review, Vol. 1 (August, 1907), p. 560.

mitment to theory, and with a compulsion to raw empiricism and parochial concerns, the narrowness of this political science was virtually inescapable.

This brief and perhaps unfairly simplified sketch is not presented in a spirit of condescension. If it emphasizes shortcomings, moreover, that is not because I assume that the period produced no gains. Given the temper of the American scene in the Age of Reform, the state of the means of communication, the paucity of data, and the limited manpower available to the discipline, the emergence of these characteristics was natural if not inevitable. Within the limits that it accepted, the gains that the field made were not trifling ones.

This summary is offered rather to indicate that once the experiences of political scientists, whether as professionals or as citizens, began to raise questions that fell outside the implicit general agreement on what to do and how to proceed in the field, none of the features of this largely implicit consensus could long go unquestioned. As such non-congruent experiences multiplied, the positions assumed by the discipline became successively untenable and a search for alternatives inescapable.

No purpose would be served by attempting to pinpoint a time at which experiences inconsistent with the general agreement began to occur with disturbing frequency; the choice would be arbitrary and the effort meaningless. The process in any case was one of cumulation. Thus the immediate results of World War I probably were mixed in their impact. The dissolution of the Hapsburg monarchy, the formal institutions of the successor states, the establishment of the Weimar Republic, and the launching of the League of Nations presented no immediate inconsistencies. Many political scientists, in fact, found it easy to attach to the League and to other international organizations both the uncritical optimism of the professional consensus and the analytical assumptions associated with it. On the other hand, the Bolshevik Revolution, with its apparent and apparently successful rejection of these assumptions, may have had a more disturbing effect. Also the retrospective views of the politics of the War, illustrated in a rash of semi-popular books on propaganda and reflected in Walter Lippmann's essays on public opinion, indicated some degree of disillusion. Probably more disturbing were the Fascist coup in Italy and the later Nazi take-over in Germany, with their open and effective repudiation of the expectations and practices that underlay the implicit agreements of the profession. The associated political instabilities and counter-currents of change throughout Europe, especially in Spain and France, and the bankruptcy of the Third Republic in 1940, liquidated the intellectual utility of the rootless, untheoretic concern for descriptive detail that hobbled what was known as "comparative government," but they almost as obviously undermined the assumptions and the conventional imperatives of much of the rest of the field.

An order of experience different but of comparable consequence for the profession occurred during the New Deal and World War II periods as academic political scientists in unprecedented numbers were transplanted to Washington. The impact of such first-hand experience with problems and processes at the level of the national government cannot be calculated, of course, but one cannot doubt that it was substantial. This re-shuffling, moreover, probably was important not merely for the element of direct confrontation with national government but also for a confrontation of a different sort—with the practitioners of other social science disciplines. As political scientists found themselves in mutually instructive collaboration with men from other fields, they were almost compelled to reckon with the utilities that these disciplines offered, not only for coming to grips with questions of immediate urgency but also for dealing with questions more strictly professional in character. These influences undoubtedly were reciprocal, but the political scientists' side of the transaction is the one that concerns us here.

Finally, at least two sorts of development in the decades since World War II have contributed to the dissolution of the established professional consensus. One was the drastically altered character of world politics after Potsdam. As "over there" moved here, the need for a systematic concern with the processes of international relations and with political and national security strategy became compelling. The reality of nuclear weapons and the politico-military implications of operations in space could not be grasped, if they were to be understood at all, by the assumptions and concepts that had appeared viable in a world based on Geneva. A second such development was the break-up of the colonial system, the emergence of new nations or national entities, and the awakening of older ones, which revealed the inadequacy of a disciplinary posture that was essentially parochial, that took the nature of the political system for granted, and that lacked an adequate and explicit view of political change. The appearance of the new nations demanded not only some sophistication on these counts but also tools of analysis equal to the task of a genuine comparative method. The conventional descriptive language of "comparative government" was weak enough in dealing with the relatively homogeneous and generally stable institutions of the Western European tradition; it was for the most part less than useless for coming to grips with the differences, the novelties, and the fluidities of the new national systems.

Although it took experiences such as these and their cumulative effects to bring the discipline as a whole to perceive the inadequacies of the prevailing consensus, a succession of individual dissents antedated all or all but the most recent of them, and these were the seeds of counter-tendency. One thinks, for example, of Graham Wallas, whose *Human Nature in Politics* (1908), for all its over-enthusiasm about psychology's potential for political science, was an eloquent and, at least on this side of the Atlantic, influential rejection of institutional description for its own sake. You will expect me to mention the almost unclassifiable Arthur F. Bentley, who was in but not wholly of the Age of Reform. If Bentley has been misunderstood in the 1950s and 1960s, his work at least has emerged from forty years of almost complete neglect. The Bentley revival, moreover, was itself symptomatic of the widespread readiness within the profession—largely absent in 1908—to question the conventional guiding agreements. Comparable, if less explicit and less radical dissent can be seen in at least some of the work of other scholars in the same generation, including Beard, Goodnow, and Lowell.

One must certainly include, moreover, the work and the career of Charles E. Merriam, who for more than twenty years was the prophet of reconstruction in political science. The "Chicago school" that he created in the years before World War II was not strictly a school. It had no dogmas and no orthodoxy except a restless skepticism concerning the adequacy of prevailing conceptions of the discipline and a corresponding receptivity to the unconventional. Among the most distinctive products of those years, however, were several that demonstrated a reach for the coherence of political phenomena. For example, political systems, their properties and conditions of change, were, then as now, a central focus of Harold Lasswell's work. Of the same order was Frederick L. Schuman's *International Politics*, which, when it appeared in 1933, broke dramatically with the rootless moralizing and mechanical description then prevailing and foreshadowed new patterns of analysis in that growing sub-field.

There were individual stirrings elsewhere in the 1930s, as in the work of Pendleton Herring, then and since a pacemaker for his students and colleagues. But restlessness within the field as a whole did not develop until after 1945, when the effects of the full range of inconsistent experiences began to appear.

Given the looseness and especially the lack of precision in the prevailing implicit agreement on what to do and how to proceed in the field, its weakening and gradual dissolution were bound to be followed by a confusion of competing and divergent, if not incompatible, views of the appropriate questions to be asked and the proper methods to be used. How long that state of affairs is likely to exist is anyone's guess. At least three possibilities suggest themselves. First, it may be that the discipline is so uninsulated from its environment that it will have to wait for a broad intellectual or social movement to give it implicit coherence, as the Progressive Movement seems to have done in the formative years. If this should be so, the wait, I suspect, will be a long one. A second possibility is that segments, at least, of the discipline have grown self-conscious enough to supply their own momentum and their own modes of coherence. These may then develop as increasingly divergent and separate schools, with little in common except some raw data and possibly, but not necessarily, a departmental label. Or, third, most of the discipline may have acquired a degree of self-awareness sufficient to permit it to set the outlines of what to do, if not altogether how to proceed, without total dependence on dominant currents of thought in the environment and without the widening cleavages in both conception and procedure that the second possibility would involve.

A case can be made for each of these, and at this stage it is impossible to say with confidence which of them, or of a number of variant patterns, will in fact develop. As an inveterate optimist, however, I am disposed to bet on something like the third possibility, and I should indicate my reasons for the wager as well as for my preference.

The first and most basic reason is the emergence—perhaps more accurately re-emergence of an explicit interest in the political system. I do not refer here to a particular kind of conception but much more broadly to a renewed awareness of the simple but important assumption that the phenomena of politics in any sphere are interrelated in persistent or recurrent patterns. More important than the awareness itself is that it has involved explicit examination of the relations among things political—efforts to specify forms or types of systems, the elements involved in them, the factors associated with alterations in systems, and probable implications of these forms and mutations for the strength of specified values. This constitutes, of course, a revival of interest in one of the classic problems in the study of politics, and it is significant for just that reason. For, if a new agreement is to emerge on what political science is about, it will not come, I am certain, from a conception that does not permit the discipline, at least in principle, to deal with such large, important, and classic problems.

This revival thus seems to point in the right direction, but its implications go beyond that advantage. As the political system becomes a central focus of inquiry, guided by careful theory—of which more in a moment—the isolation of sub-fields within the discipline should begin to break down. National and international systems are not identical, of course, but the questions they invite are not wholly dissimilar, nor are the terms in which they may be analyzed. Explorations at each level may be expected to have increasing relevance for the other. And the same may be said of subnational systems, the area last and least directly affected by the experiences that disrupted the once-prevailing agreement within the discipline. The centrality of system considerations also will make less permissible the analysis of an institutional segment or process without reference to an explicit conception of the system to which it relates. Finally, an attempt to deal seriously with the problem of the political system necessarily involves reconceptualizing, re-casting the language of inter-system comparison. This reconceptualization was clearly anticipated a dozen years ago when the Social Science Research Council set up the Committee on Comparative Politics. The committee was expected to concentrate on the developing nations, but the larger objective was to contrive ways of looking comparatively at whole systems in terms of variations in attributes common in some degree to all political systems, whatever their stage of development.

A second reason for betting on the possibility of a new disciplinary consensus is the revival of interest in theory. This development fundamentally was a predictable consequence of the break-up of the discipline's nontheoretical consensus. But it has been aided in significant measure in recent years by the increased accessibility of data in a number of sub-fields. No discipline, especially among the social sciences, has good data readily available in sufficient amounts. But when data are not easily secured, even in modest quantities, it is likely that an enormous proportion of research time will go into mere collection. In such circumstances, moreover, it is not astonishing that much professional work, even in the absence of an anti-theoretical bias, is reportorial, journalistic, and non-theoretical. Thus one has the impression that the specialties that today are least engaged in renewed theoretical concerns are those that must scramble the hardest for data. "Kremlinology" as an alternative to theory is in part traceable, I suspect, to scarcity of data.

However this may be, a renewed emphasis on theory of all kinds seems to have occurred in recent years. I emphasize "all kinds" because, as might have been expected, it seems evident that the theory chorus is less polyphonous than cacophonous. Discord seems more prominent than harmony even within what some see as the solid ranks of "the new political science." Nevertheless, the renewed concern for theory seems clear, and I cite as illustration an impression that the majority of textbooks in all parts of the field today show a degree of care, at least about consistency of assumption and coherence of expository framework, that was rare thirty years ago. This care indicates nothing about the theoretical adequacy of these efforts, but it does suggest a more sophisticated level of expectation.

In noting a revived interest in theory I refer to the creation, development, or application of theory and to "theorizing," or the readiness to draw inferences from a set of data to the class of events to which they belong. I am not here discussing the analysis and explication of documents in the history of political thought, on which I want to comment in another connection. What I want to emphasize at this point is a growing self-consciousness and fruitful awareness of the necessary conjunction of theory and empirical investigation. Such awareness involves accepting the elementary point that an investigator never has a choice of whether to use theory or not, that the facts never speak for themselves but only through the assumptions and concepts that control their selection and analysis, and that in consequence the choice is only between implicit, internally inconsistent, and hopelessly inadequate theory, on the one hand, and explicit, logically defensible, and reasonably adequate theory, on the other.

I do not anticipate that an awakened respect for theory will eliminate a division of labor in the field based on differences of disposition, training, and aptitude. Nor do I suggest that every reported research will or should display its theoretical structure prominently. We shall continue to have and should wish to have more political scientists whose talents and sense of strategy lead them to work on and from conceptual schemes and formal models of broad reach.

We shall also continue to have and to need those who, like our great colleague V. O. Key, in full awareness of their broad assumptions, prefer to work more inductively. The good-humored skepticism that helped to make Key so creative also made him brilliantly at home with his data and healthily uneasy when his sentences got much beyond them. He denied that he was a theorist, but he was one, though he was indisposed to work with higher abstractions or to embrace formal systems for which he saw no early possiblity of empirical test. His *Southern Politics,* for example, represented and probably more than any other single work encouraged the restless searching that has marked the field since 1945. The South he wrote about has all but disappeared less than two decades after the book was published. What is left? An indispensable record for the historian, but for the political scientist much more: a structure of propositions and inferences —many of them tucked away in footnotes— that have theoretical pertinence well beyond the South or the United States. One day, perhaps, someone will do what Key would have been too modest to do, namely, extract that structure from its contemporary setting and show the strength that sensitive empirical inquiry can bring to theory.

For the predictable future both modes of theorizing will be needed, and from time to time both may be profitably cultivated by the same man. In practice, though not in principle, it may not be possible to develop general models, to say nothing of hypothetico-deductive theories, with much systematizing or predictive power. Should that be the case, we should be compelled to rely upon more restricted theories of inferential origin. But even if the obstacles to general theory can be surmounted, the more restricted type is likely still to be necessary, at least to stimulate and in part to test the more inclusive. Theory of both sorts, moreover, like the revived awareness of political systems, should contribute to reducing the insulation of sub-fields and perhaps to their re-casting. It may even be possible at some future point for a sub-field to be known by the problems it addresses itself to rather than by the piece of real estate it focuses upon or the institution it investigates.

Finally, my third reason for counting on a new consensus within the discipline is what I would call a recommitment to the goal of science. Although it is at least partially implied by a revived concern for the political system and for theory, I am less confident of this tendency than of the other two and I can hardly be unaware that it is considerably more controversial. No topic in the last two decades has provoked more fruitless discussion or has more consistently produced false, or at least wholly unnecessary, oppositions. I would not touch the issue but for a desire to bury it rather than to raise it, although I probably cannot expect that a death certificate will be widely recognized or that my mortuary services will be generally welcomed.

Close to the heart of the matter, however particular issues may be phrased, is the question of definition. It is possible to define the requirements of "science" and "scientific method" in such categoric terms that nothing in the study of politics now or at any projected future time is or is likely to be eligible for the label. But, to quote from an essay by Ernest Nagel on which I shall rely heavily, such an effort also leads " . . . to the unenlightening result that apparently none but a few branches of physical inquiry merit the honorific designation."[3] Thus, to specify that "science" requires the hypothetico-deductive procedures and the integrated form of systematic explanation exemplified by the science of mechanics or that it necessitates the use of a particular set of techniques regardless of the type of inquiry is probably to deny that the discipline can be scientific or at best to confine it to problems of the most trivial character.

It is not necessary, however, so to restrict the definition or even to espouse that form of science as a goal or an ideal, and I do not see the recommitment to which I refer as involving such a definition or necessarily such an espousal. If one accepts Nagel's characterization that " . . . the sciences seek to discover and to formulate in general terms the conditions under which events of various sorts occur, the statements of such determining conditions being the explanations of the corresponding happenings";[4] and if one further agrees that "The practice of scientific method is the persistent critique of arguments, in the light of tried canons for judging the reliability of the procedures by which evidential data are obtained, and for assessing the probative force of the evidence on which the conclusions are based";[5] then the recommitment in the discipline becomes sensible and, at least presumptively, manageable.

[3] Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York, Harcourt, Brace and World, 1961), p. 449.

[4] *Ibid.*, p. 4.

[5] *Ibid.*, p. 13.

Science so conceived requires generality of statement but not in a specified degree, nor does it require a particular level of precision, or a limited set of techniques. It does not assure the truth of every conclusion that it reaches or the absence of bias deriving, for example, from the value commitments of the investigator. It does not suggest that a precise line divides knowledge or beliefs that can be labeled "common sense" from knowledge that claims to be "scientific," and it does not, finally, assert that only knowledge so derived can or should be admissible as part of the discipline—a point to which I shall return. Any science does aspire to explanations of classes of events, explanations subject to the controls of empirical evidence and with sufficient systematic power at least to place its findings beyond complete invalidation by the day's events. These are aspirations, as I see it, that lie close to the heart of the restlessness that has characterized the field for at least two decades.

It is recommitment to science in this broad sense that I see as an essential part of a new consensus in the discipline. A more restricted conception, corresponding to the rigorous requirements of mechanics, for example, almost certainly cannot be fruitful in any foreseeable future and will certainly have the consequence of fostering increasingly divergent schools. Without, however, a commitment to a science somewhat more openly conceived, the empirical work of the discipline will not progress, will not move cumulatively toward explanation, toward establishing relations of dependence between events and conditions. If the empirical work of the discipline does not move, moreover, the discipline will not.

A new agreement on what the discipline is up to, resting on a broad conception of the scientific enterprise, as well as on the two tendencies previously identified, is likely to permit movement and the expectation of progress in the field. As Kuhn has argued in a somewhat different connection, the absence of such movement and of the associated expectation probably lies behind the recurrent definitional debates about the "science" of the contemporary social sciences. "What would permit X field to move ahead?" is a question more fundamental than "Is it a science?" He suggests, and I think soundly, that such debates are less frequent among economists, not because they know what science is, but because they have achieved "consensus about their past and present accomplishments"; hence it is economics rather than "science" on which they agree.[6]

But why be concerned about a fresh consensus in the field, especially one that derives from the three elements discussed here? Why not welcome a diversity of competing and increasingly separated schools? Surely the temper of the times and the state of the technology are such that someone will exploit the potentialities of science in the analysis of political events and actions; if not "political scientists," then a school, or perhaps a set of scholars who do not carry the label "political science" at all. What difference would that make? Perhaps none at all, but I suggest we should not concede the point without reflection.

Even one who may be committed to the potential of the scientific enterprise in this field must grant that he cannot know how far that effort in practice will be able to go and must seriously entertain the possibility that in fact the distance may be fairly short. If " . . . it is any man's guess whether a comprehensive social theory of . . . [a scientific sort] is destined to remain permanently as a logical but unrealized possibility,"[7] as much probably must be granted concerning such a political theory. Only a bigot would perhaps deny that significant progress recently has been made in explaining and developing a theory of electoral behavior, and only the most skeptical would reject the likelihood that other sub-areas—perhaps the comparative analysis of public policy—could make similar gains. But one would be imprudent to project these accomplishments indefinitely. The practical obstacles may be too great. For example, it may not be possible to secure enough cases, enough standardized and precise observations, to make any reasonably reliable scientific statements about political systems, though the discipline must talk about systems.

From these cautionary observations I would draw two inferences. First, if the prospect of a comprehensive *social* theory lies so far in the future as to be a matter of guessing, then theory in the *political* realm, however comprehensive, for at least that long is likely to remain distinctively political. It will work primarily from data and concepts rooted in its more restricted area, and it will not appear merely as a special case of a more comprehensive theory of society. Second, if the reach of reasonably reliable science in the area of politics may for a long time and perhaps indefinitely be shorter than many might hope, then for so long the discipline will have to place some reliance upon knowledge that is less scientific or even nonscientific in character. The alternative for a dis-

[6] Kuhn, *op. cit.* pp. 159–60.

[7] Nagel, *op. cit.* p. 462.

cipline based upon consensus about itself is consistently to sacrifice relevance to rigor.

This position seems to me to contain two additional implications. First, it implies the need to include under relevance the function of assessing the probable consequences of proposals and events for the system and for the values implicit in it. I cannot accept the counsel of those who would have political scientists say, "Let George do it" when the question arises of assessing matters that fall within the subject-matter that they claim as their province. That seems to me defaulting on an obligation, and further, I do not believe any responsible fraternity of social scientists gives that answer, whatever its protestations to the contrary. This does not mean that we should run up the flag for the latest panacea, that our highest aspirations should be as political actors, or that we can fail to distinguish responsibly between what we know with some degree of reliability and what we merely judge to be valid. But it does mean that as political scientists we may have something to say. It means, further, that we cannot escape the obligation to predict, and a function of prediction is to sharpen and to broaden moral choice.

Second, if a fresh consensus about the discipline must include, for an indefinite future, a less or non-scientific component and if a continued assessing or evaluating function must rest fundamentally upon that component, then it seems to me clearly to follow that the classics in this field of thought, especially perhaps the more speculative documents, must be a central part of the training for and the practice of political science. I am aware of Whitehead's epigram, that "A science that hesitates to forget its founders is lost." But one may add that perhaps a discipline that does indeed forget its founders may also be lost. Remembering them does not, however, require that their writings be treated as scripture; revision is not equivalent to neglect.

Neglect amounting to forgetting would not be new to the field. The non-theoretical bias of the earlier agreement within the discipline thrust the study of political thought out of the mainstream and retained it largely as a gesture toward polite learning. Something akin to this attitude persists, it seems to me, in the contemporary Philistinism that regards everything in the classic literature as having only an ethical or normative revelance. Small wonder if in consequence of this sort of attitude, the study of the history of politicial thought appears to be the most isolated segment of the field, if a reciprocally fruitful dialogue between that segment and the more empirically oriented areas has been rare, or if younger political scientists committed to the empirical side of the discipline may see little in the classics that is relevant to their concerns. Such isolation, though understandable, seems neither necessary nor desirable.

The study of politics is old, but political science as a self-conscious discipline has not come a long road. On that way the discipline has inevitably been marked by the intellectual, moral and political influences—often contradictory and fruitless—that have dominated the days of its growth. From such influences, as much as from its own substance, have come the conflicts and disputes we have known for at least a decade. In attempting to trace these controversies I have had no thought of urging an end to disputation, an outcome whose undesirability would be exceeded only by its improbability. Rather I have tried to argue that if we can see the sources of our disagreements, we may be able to redefine some of them and to choose for a further investment of energy those most likely to encourage the growth of our collective capabilities.

Redefinition and redirection will depend upon the emergence of a new and broadly based consensus about the discipline. That may not occur; a persistent sectarianism may be inescapable. Continued and growing dissensus, however, can have unfortunate implications. It would encourage an indiscriminate rejection of much that is valuable both in our inheritance and in our contemporary efforts. It would be likely to restrict the range and power of our inquiries. It would be likely, finally, to reduce the relevance of those inquiries in days that ask for the maximum in trained intelligence. Should a new agreement emerge, it may take a long time and may be clearly recognizable only by hindsight. The challenge, I submit, is worthy.