# Macrotheories and Microapplications in Comparative Politics

## A Widening Chasm

Joseph LaPalombara*

## Introduction

As more than one commentator has informed us of late, no branch of political science has been in more extreme ferment than comparative politics during the last fifteen years.[1] Beginning with a summer seminar at Northwestern University in 1953,[2] followed by the Macridis critique of 1954[3] and the creation of the SSRC Committee on Comparative Politics that same year, a massive theoretical and methodological stocktaking was set in motion. That activity is still very much under way, and, as in any ongoing fermenting process, it is risky to predict what the aged product will look like. Nevertheless, those who have contributed to or sampled the intellectual output of most recent vintage may be expected and permitted to comment on the "new" comparative politics—and perhaps to suggest where and how the continuing fermentation process should be accelerated, modified, or halted. It is in this spirit that I offer this essay, fully aware that most vintners will continue to pursue formulas that they find most palatable and congenial, even when the outcome is little more than old wine in shiny new containers.

There are some who view the discipline's transformation as nothing short of revolutionary, although the exact date of the revolution's emergence is unclear.[4] For those who emphasize changes in the broad theoretical orientation of the discipline, such transformations can be traced to the 1920's and,

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1 See, for example, Sigmund Neumann, "Comparative Politics: A Half-Century Appraisal," *Journal of Politics*, IX (August 1957), 369-390. In his presidential address to the APSA in September 1966, Gabriel Almond notes both many aspects of the general ferment in the discipline of political science and the catalytic role in it played by the "subdiscipline" of comparative politics. See Gabriel A. Almond, "Political Theory and Political Science," *American Political Science Review*, LX (December 1966), 869-879.

2 Roy C. Macridis et al. "Research in Comparative Government," *American Political Science Review*, XLVII (September 1953), 641-675.

3 Roy C. Macridis, *The Study of Comparative Government* (New York, 1955). I understand that Macridis is revising this much quoted little book, and it will be interesting to learn how many of his trenchant indictments of the discipline have been quashed or removed by "good" behavior in the intervening years.

4 Gabriel Almond, in his APSA presidential address, associates the revolution with Charles Merriam's *New Aspects of Politics* (Chicago, 1925) and with the extraordinary number of seminal political scientists who emerged from graduate training at the University of Chicago in the 1920's. That list includes Herbert Simon, David Truman, Harold Lasswell, V. O. Key, and Almond himself.

indeed, all the way back to Arthur Fisher Bentley's long-neglected *The Process of Government.*[5] For others who see revolutionary thrust primarily in methodological concerns, the important advances are said to begin following World War II.[6] Whatever the method of dating, everyone is agreed about the salience of the subdiscipline of comparative politics in these transformations. Thus, Somit and Tanenhaus have found that the profession itself identified "comparative government" as the field in which the "most significant work is now being done."[7] And, more recently, Braibanti has produced detailed evidence to demonstrate how radical has been the shift of scholarly attention to the comparative field in the period 1948-1966.[8]

It is not my purpose here to establish whether these changes represent revolution or, if so, how successful revolution has been. I doubt, however, that we are now all "behavioralists," whatever that means,[9] and I am much more inclined to be rather specific in distinguishing between the effervescence of theory, on the one hand, and the development of new, more

[5] The volume was first published in 1908 and elicited from Charles Beard the comment that it contained little to interest the political scientist. It was reissued in 1949.

[6] See, for example, William C. Mitchell, "The Shape of Political Theory To Come: From Political Sociology to Political Economy," *American Behavioral Scientist,* XI (November-December 1967), 8-20. Cf. Heinz Eulau, *The Behavioral Persuasion in Politics* (New York, 1963).

[7] Albert Somit and Joseph Tanenhaus, *American Political Science: A Profile of a Discipline* (New York, 1964).

[8] Ralph Braibanti, "Comparative Political Analytics Reconsidered," *Journal of Politics,* XXX (February 1968), 25-65. See Braibanti's first footnote (p. 25) for a good list of works representing the recent effervescence of comparative government and comparative politics.

I should emphasize here that not all of the ferment of recent years is in the comparative field narrowly conceived. Books from both earlier and more recent periods which would be included on anyone's list of groundbreaking works are H. D. Lasswell, *The Analysis of Political Behavior: An Empirical Approach* (New York, 1948); David Truman, *The Governmental Process* (New York, 1951); Herbert Simon, *Administrative Behavior: A Study in Decision Making Processes in Administrative Organizations,* 2nd ed. (New York, 1957); David Easton, *The Political System* (New York, 1953); Robert A. Dahl, *A Preface to Democratic Theory* (Chicago, 1956), and his *Modern Political Analysis* (1963); Glendon Schubert, *Quantitative Analysis of Judicial Behavior* (New York, 1959); Gabriel A. Almond and James S. Coleman, eds. *The Politics of the Developing Areas* (Princeton, 1960); Karl W. Deutsch, *Nationalism and Social Communication* (New York, 1953); John C. Wahlke et al. *The Legislative System* (New York, 1962).

I should add that such a list must include Carl J. Friedrich's *Constitutional Government and Democracy,* 4th ed. (Waltham, Mass., 1968), both because of its enormous impact on the profession during the last three decades and to illustrate my point that less is "new" in political science theory and methods than we might be led to believe.

[9] See, for example, E. M. Kirkpatrick, "The Political Behavior Approach," *PROD,* II (November 1958), 9-13, whose "behavioral umbrella" seems to me to include too many political scientists. Cf. Roland Young, ed. *Approaches to the Study of Political Science* (Evanston, 1959). Eulau's *The Behavioral Persuasion* is much more meaningfully restrictive (even if open to the objection of "narrowness") in delineating what the "behavioral revolution" means. Robert Dahl's summary analysis of the "behavioral movement" is well worth reading: "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review,* LV (December 1961), 763-772.

Sidney Verba, in "Some Dilemmas in Comparative Research," *World Politics,* XX (October 1967), 111-127, makes my points about "revolution" very laconically. He says, "There has been a revolution in comparative politics. But as with all revolutions, it is difficult to date its beginning, to chart its course, and now, when the revolution has become established, difficult to say what has been accomplished" (p. 111). Later, in reflecting on "theories," he adds, "But frameworks, paradigms, and theories proliferate at too rapid a rate. In addition, the general theoretical works float well above reality, and they often are so abstract as to suggest no clear problem focus" (p. 112).

rigorous methods, on the other. The general position I shall take in this article is that whatever the vantage point from which one views the changes of recent years, they do not look like unmixed blessings for the profession. Furthermore, I shall maintain that this observation is particularly true in the field of "systems theory," "holistic theory," "general theory," or "grand theory," that is, in that body of literature that purports to provide a theoretical explanation of the entire polity, government, political system, and so on. I shall argue in conclusion that the best hope for the discipline's future growth lies in the application of rigorous methodologies to important problems conceptualized at the "middle range" and involving *partial* segments of the polity. It seems to me that this procedure would constitute one way of responding to the growing criticism of scientism in the profession—criticism that is voiced not only by normative and speculative political theorists but also by empirical political scientists and sociologists who take a dim view both of high-flown theoretical exercises and of so-called value-free theory and research.[10]

One form of response to criticisms of proliferating macrotheories is to argue that proliferation itself is a sign of health and vigor and that a wide and dizzying array of macrotheories is inevitable in a profession that has had to discard sterile, culture-bound general theories of a past era. Even where the general theories are essentially metaphorical (as almost all of them seem to me to be in political science),[11] it is insisted that all possible avenues of potential theoretical breakthrough must be kept open until we have a better empirical basis for deciding which macrotheories to accept, which to discard. Viewed from this sort of vantage point, most, perhaps all, efforts to capture and clarify the more elusive aspects of whole political systems appear courageous and praiseworthy.

A less generous reaction to much of the recent whole-systems theoretical output of the discipline is the observation that we have returned to the ancient art of scholasticism, armed to be sure with new terminology, but not any more successful than were the ancients in narrowing the gap between abstract formulations and theoretical realities. It strikes me as enormously telling that at precisely that moment in the profession's development when methodological tools will permit the rigorous comparative testing of hypotheses the distance between hypotheses and general theory should be widening and that the linkage between hypotheses and macrotheory is either terribly obscure or of such problematical logical construction that theory itself cannot be falsified.

[10] The antibehavioral views of Leo Strauss and his followers are contained in H. Storing, ed. *Essays on the Scientific Study of Politics* (New York, 1962). A typically European criticism is Bernard Crick's *The American Science of Politics* (Berkeley, 1959). In sociology, some of this protest is contained in Maurice Stein and Arthur Vidich, *Sociology on Trial* (Englewood Cliffs, 1963). See particularly the brilliant critical essay by Alvin Gouldner in this volume.

My observations about some of the consequences of these developments are contained in my "Decline of Ideology: A Dissent and an Interpretation," *American Political Science Review*, LX (March 1966), 5-18. See also James C. Charlesworth, ed. *The Limits of Behavioralism in Political Science* (Philadelphia, 1962).

[11] By "metaphorical" here I mean simply (and, all too often, simplistically) viewing the political system and its processes in terms of fundamental theoretical formulations derived from mechanics, biology, cybernetics, neurology, and so on. The metaphorical tradition is deeply rooted in political science, as expressions such as "ship of state," "father of his country," and "sick society" will attest.

Let me try to be clear here and acknowledge that a great many hypotheses about the polity can be *associated* with, say, the "pattern-variable" or "four-sector" formulations of Talcott Parsons, or the "demand-support-output" model of David Easton, or the "cybernetic" model of Karl Deutsch, or the "capability" model of Gabriel Almond. Most such hypotheses, however, do not necessarily *depend* on such models; many of them are either self-evidently true or cannot be falsified, and empirical findings concerning them do not readily lead to modifications of the general theories. Many of these formulations have perhaps helped to make American comparative politics less parochial, less focused on formal Western institutions, less morphologically descriptive, less unaware of the importance of variables that lie "outside" something called the political system or polity. But unless my reading of the current state of "general theory" in political science is grossly in error, we are not, I think, moving perceptively in the direction of what Thomas Kuhn intends in his use of the term "paradigm."[12] Nor will we do so until we reduce the proliferation of general theories, begin making a bit of progress toward that common scientific vocabulary A. F. Bentley called for over fifty years ago, and pay more attention than we have to the question of the nature of the evidence that would suggest that a particular macro-theory be modified or discarded. One step in that direction would involve greater attention to partial systems, to middle-range propositions concerning them, to genuinely comparative analysis of political institutions, processes, and behavior, and, it is hoped, to the gradual refinement of theory by inductive inference that might then provide the basis for a "new paradigm."

We are not likely to go this particular route if, like Lipset, we react to the concepts of Talcott Parsons by limiting our observations to the fact that they are "obviously subject to considerable refinements" and that "little work has been done on the problem of linking such concepts to empirical indicators."[13] Concept-refining very quickly degenerates into the scholastic game; empirical indicators require less distance than currently exists between theoretical concepts and what it is we can measure in the field, in the null-hypothesis sense of the research enterprise.

Nor is it my purpose to provide a complete critique of the whole-systems approach to theory and research in comparative politics, or indeed to suggest that such an approach should be abandoned. However, it does seem to me that much of what is questionable, and even distressing, about present theories in comparative politics can be traced to something called structural-functional analysis. Judging by the work of structure-functionalism's more visible and esteemed practitioners, it seems to me that the charge that the so-called theory amounts to little more than a New Scholasticism is well founded.[14] It is therefore necessary to say some things about structure-

[12] *The Structure of Scientific Revolutions* (Chicago, 1965).

[13] Seymour Martin Lipset, *The First New Nation* (New York, 1963), p. 344.

[14] Much of what I would score as the New Scholasticism emanates from the prolific pen of Talcott Parsons and is especially found in Parsons and Edward A. Shils, eds. *Toward a General Theory of Action* (Cambridge, Mass., 1951). The best overall criticism of Parsons' theories is contained in Max Black, ed. *The Social Theories of Talcott Parsons* (Englewood Cliffs, 1962). Black's essay is a brilliant critique, and his rendering of Parsons' central postulates in "plain English" is both amusing and sobering.

The difficulties created by Parsons (not all of them intended, to be sure) are evident in functionalism before proceeding to an examination of some alternative orientations to theory and research in the field.

**Functionalism and Cross-cultural Comparison**

We seem to be agreed that a comparative political science that is not cross-cultural as well as cross-national would fall short of supporting the emergence of what Almond once called a "probabilistic theory of politics." The logic underlying this view is well known: Cross-national studies, whether of whole or partial systems, tend to be culture-bound. Where cross-national studies focus on institutions such as legislatures, political parties, interest groups, and the like, they may obscure the nature of politics in cultural settings where such institutions do not exist or, if they do exist, represent radically different meanings for the societies involved. Even where the phenomena subjected to comparative analysis seem not to be narrowly limited in time and space (e.g., decision-making, political socialization), failure to extend analysis across cultural boundaries is likely to result in misleading and inaccurate generalizations. In short, a probabilistic theory of politics can emerge only from a consideration of the full range of cultures and societies in which politics and political systems are found.

Although such statements seem obvious enough today, it is only in recent years that some political scientists were liberated from the logical trap of assuming that the political process involves a *given* set of behaviors occurring within a *given* institutional framework. We may thus assume that it is unlikely today that political scientists who happen on a primitive tribe will conclude that legislation is absent where there does not exist some concrete approximation of the House of Commons, that public administration is wanting where a *Conseil d'État* or a Weberian-type bureaucracy is not to be found, or, indeed, that political participation exists (or is meaningful) only when it includes "free" elections or widespread public involvement in associations or political organizations ranging from the P-TA to political party directorates.

We owe these recent insights in part to structure-functionalism. Regardless of what the individual political scientist may want to do (or not to do) with functionalism, he must acknowledge that it is from this "theory" that we learned to conceptualize the political system as a set of finite, interrelated functions essential to its existence and to see that the manner in which such functions are performed anywhere in space and time is not necessarily bound to a specific set of institutions (read "concrete structures"). We learned, too, that it is not merely a formal institution that may represent a

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the work of Fred Riggs, such as his *Administration in Developing Countries: The Theory of Prismatic Society* (Boston, 1964) and in his "Agraria and Industria: Toward a Typology of Public Administration," in William J. Siffin, ed. *Toward the Comparative Study of Public Administration* (Bloomington, 1957). Other works that contain one or more of the characteristics and problems I am alluding to here would include (but are not limited to) the following: Edward Shils, *Political Development in the New States* (Gravenhage, 1962); Almond and Coleman, *Politics of the Developing Areas;* S. N. Eisenstadt, *The Political Systems of Empires* (New York, 1963); David E. Apter, *The Politics of Modernization* (Chicago, 1965); and a number of the edited volumes on political development sponsored by the SSRC Committee on Comparative Politics, including my own *Bureaucracy and Political Development* (Princeton, 1963).

"structure" of the political system but that other analytically interesting and important patterns, such as value systems, economic allocation, or attitudes toward innovation can also be viewed from a structure-functional vantage point.

Our debt to functionalism does not stop here. We are increasingly aware of the applicability to comparative politics of the maxim—long ago offered us by Malinowski—that an artifact of one culture transferred to another in form may represent a radically different meaning and relate to a quite different function in its new setting. Thus Morroe Berger found, somewhat to his surprise, that a Weberian-type bureaucratic superstructure in Egypt did not in fact produce for that society the kinds of human interactions and consequences for the political system imputed to bureaucracy in the West.[15] Similarly, Riggs has taken considerable pains to depict the survival in "modern" institutional settings of patterns of behavior deeply rooted in "traditional" cultures.[16]

Perhaps the best cataloguing of the kinds of lessons political scientists can learn from structure-functionalism is included in Gabriel Almond's widely cited introductory essay to *The Politics of the Developing Areas*.[17] A more recent and ambitious attempt is made by the British political scientist, H. V. Wiseman, whose concluding chapter is the best example I can cite of the impossible morass of jargon, fuzzy conceptualization, circularity of reasoning, truisms propounded as scientific wisdom, and appeal to more scholasticism which characterizes the work of functionalists.[18] That Wiseman is primarily involved not in making his own critique but in distilling others' arguments pro and con serves merely to emphasize this unfortunate state of affairs. A reading of Wiseman's well-intentioned exercise quickly reveals why some political scientists find the structure-functional approach or other sociological approaches to systematic analysis extremely suspect. Consider, as one typical example, the following alleged contributions to the comparative study of political systems which Wiseman uncritically accepts as having come from sociologists:

1. That the "nation" and the "state" are not necessarily the same thing.
2. That the concepts of *power* and *influence* are as important in comparative politics as are institutional foci.
3. That "in the sociological sense," a legitimate government is one that has the support of those who are subject to it.
4. That "legitimacy" is never the sole basis of a government's power.
5. That the "effective government" of a society is always government by a small minority of the population, or that "rule is always the rule of the few."

To be sure, Professor Wiseman is reporting the claims of others and is moved, regarding the first "sociological discovery" cited above, to suggest,

15 *Bureaucracy and Society in Modern Egypt* (Princeton, 1957).

16 See, for example, Riggs, *Administration in Developing Countries*, Parts 2 and 3.

17 Pages 3-64, esp. pp. 9-25 on "The Common Properties of Political Systems."

18 *Political Systems: Some Sociological Approaches* (London, 1966), pp. 101-117, et passim.

"with respect," that such generalizations are not "peculiarly sociological."[19] My point would be that if structure-functionalism clearly led to the theoretical validation of even such insights or self-evident propositions, it would represent an important gain. But it seems to me apparent that such is not the case, that most of the telling criticisms of the structure-functional approach[20]—when it masquerades as a descriptive or dynamic theory—have not been satisfyingly rebutted. Wiseman himself succinctly reflects my reservations about it when he says about T. B. Bottomore's critique, "What is most valuable in the functionalist approach, [Bottomore] concludes, is the greater emphasis and clarity given to the simple idea that in every particular society the different social activities are interconnected. It is then a matter of empirical enquiry as to which are the various social activities and how they are related."[21]

I am suggesting that once we have learned the important lesson of structural alternatives for functional performance and the multifunctionality of similar structures, little remains of structure-functionalism that is useful to political science, and much remains that can be damaging to comparative research.

To return to the matter of cross-national and cross-cultural research, it seems to me obvious that the kinds of functionally "diffuse" or "fused" societies and political systems that are of great interest to anthropology are rapidly disappearing and that the nation-states *do* manifest an amazing amount of institutional similarity—they *do* have executives, legislatures, public administrative systems, courts, armies, political parties, interest groups, and many other *institutional* arrangements that we have come to associate with Western societies but that may in fact be simply the most probable way in which "concrete structural differentiation" occurs at certain stages of political development.[22] To be sure, the functional meaning or consequences of such institutions are not the same in Africa as in Europe,

[19] Wiseman derives these generalizations from H. Johnson, *Sociology: A Systematic Introduction* (London, 1961). I find it almost impossible to believe that a British scholar, trained at Balliol College, would let such intellectual pretentiousness pass with only the mildest reproach.

[20] One of the most rigorous attacks on structure-functionalism is Carl G. Hempel, "The Logic of Functional Analysis," in Llewellyn Gross, ed. *Symposium on Sociological Theory* (New York, 1959), pp. 271-307. The Hempel critique is of functionalism as a logical system, and as such it is not immediately relevant here. Nevertheless, I do not think that anyone has provided a satisfactory reply to Hempel. See, for example, D. M. Martindale, ed. *Functionalism in the Social Sciences,* Monograph 5, American Academy of Political and Social Science (Philadelphia, 1965).

[21] P. 216.

[22] Although I cannot develop this point here, I wish to stress it for consideration. Whether it is the result of cultural diffusion or of endogenous development, the range of concrete structural alternatives for managing politics and government is, it seems to me, not only finite but limited to many of the very institutions we have come to identify as culturally "natural" to the West. After more than a decade of talking with colleagues who are non-Western scholars, reading their published output, and traveling in a number of non-Western areas, I am no longer easily convinced of the "exotic" character of such political systems.

A good case in point would be the political party, which appears to be ubiquitous, even if one may concede a variation in function, as well as the designation of "political party," for groups and organizations that would not meet a reasonable definition of the concept. On this point see the introductory and concluding chapters of Joseph LaPalombara and Myron Weiner, eds. *Political Parties and Political Development* (Princeton, 1966).

in Asia as in North America. Indeed, meaning and consequences of similar institutional arrangements may vary quite markedly in culturally homogeneous areas, as well as over time within the same nation-state. This elementary fact is not a discovery of functionalism; it was clearly understood by Aristotle, Hobbes, Burke, Rousseau, Tocqueville, and Bagehot, to name only a few whose writings appear to me to be particularly sensitive on this score and who were concerned with whole-systems analysis.

The proliferation or diffusion of structurally similar institutions over much of the globe also occurs at levels of government below the nation-state. It may be that some continents contain primitive local societies where politics is intermittent and where clearly political institutions are not easily discernible. Here functionalism may provide an important descriptive guide, as it might were we to research the local-level societies of Western antiquity. But villages the world over today appear to possess strikingly similar institutions such as chiefs, elders, and councils, and there is no reason for assuming that functionalism provides a better guide to the subtleties of the political process in such places than would, say, an approach that began with certain culture-bound assumptions about village government but then moved on sensitively to try to discern process variations in different settings. I shall return to this matter below when I try to indicate why we can fruitfully engage in either cross-national or cross-cultural comparative research within a conceptual and institutional framework that is perhaps parochially derived from a limited cultural area like the West.

I would record one final difficulty in the study of whole political systems which, while not necessarily inherent in such a focus, is very much apparent in contemporary political science. I refer to the tendency to see the political system, no matter how well or poorly bounded, within a broader social, physical, and economic environment and then to assume that the political system itself is the more or less fatalistic outcome of environmental or "ecological" factors. I believe it is primarily, although not exclusively, among those who are concerned with whole systems that politics and political systems take on the qualities of exclusively dependent variables, the product of a wide range of independent factors including industrialization, political socialization, the degree of pluralism in society, the political culture, the distribution of information and energy in society, communications patterns, social stratification, and even such things as the per capita number of telephones and radios, domestic and international flow of letters, telegrams, cables, or commerce manifested within any society.

There is thus more than a little truth to Sartori's complaint that systems theorists, functionalists or otherwise, have taken politics out of political science and have obscured the critically important fact that political institutions and political leaders constitute independent factors that manage to shape not merely the environment and some of the "ecological" factors but the operation and development of the political system (or parts thereof) itself.[23] Recent developments suggest that there is belated recognition of this problem, as witness the growing frequency with which we now read of the necessity of dealing more intensively with research on the "output" side of political systems. For all societies, that output side will include

23 See Giovanni Sartori, *Parties and Party Systems* (forthcoming).

legislation, administration, and adjudication—the stuff of government from time immemorial, which remains the same old wine no matter how many new bottles theory produces or how many new labels one puts on bottles, old or new. It is in part because this is so that I believe theoretical parsimony and manageable, reasonably rigorous empirical research in comparative politics require greater attention to "partial systems," or a "segmented approach" to theory and research. That this orientation to political science can never logically be the limit of our professional concerns will be apparent from my concluding statement in this article. But any brief for the emphasis on partial systems in comparative politics necessarily requires some specification of how we might proceed and what problems are inherent in the choices we make.

**Comparative Research on Segments of Political Systems**

A segmented or partial-system approach to comparative analysis may be institutionally or behaviorally focused, morphological or analytical in its intention and execution. Comparisons may involve a search for similarities or for differences among nation-states regarding those aspects of the political system that constitute the focus of attention. Or the comparative enterprise may involve a more dynamic focus, such as that of identifying the determinants over time of aspects of the political process and discernible changes that occur in it. Today, for example, there is widespread and still growing interest in something called "political development," which, however the term is defined, involves an attempt to test whether specific institutional, behavioral, and process modifications within the polity can be related associatively (or causally) to similar or differing but empirically identifiable factors.[24]

Such comparative research may or may not relate to theories concerning whole political systems. It may or may not be based on a carefully articulated and integrated set of propositions to be tested in two or more settings. Good research would require an understanding and specification of precisely what it is that is being compared and to what end. Now the end of any given piece of comparative research may and does vary considerably. How are laws made or enforced? How do formal occupants of political roles acquire them? What range of political participation is open to what segments of a nation's population and on the basis of what criteria? In what proportion of the problem-solving activities of society are formal institutions of government involved and in what way? What kinds of political decisions are centrally made, geographically diffused, hierarchically stratified, formally restricted to government officials or more widely shared—and through what sorts of patterned arrangements? What kinds of people "govern" formally, informally?

Clearly, the number of such questions we might pose for any nation is quite large, perhaps unlimited. It is this understanding, together with the fact that we must choose among the questions about which data will be accumulated, that naturally leads many scholars to insist that choice be

[24] The literature on "political development" is too vast to cite. The interested reader should consult the bibliography in R. T. Holt and J. Turner, *The Political Basis of Economic Development* (New York, 1966), and the excellent bibliographical essay in C. E. Black, *The Dynamics of Modernization* (New York, 1966).

disciplined by theory and theory-related propositions. Differently put, the caveat would read that neither a general, miscellaneous collection of facts about a political system nor the restriction of fact-gathering activity to narrow-gauge problems that lend themselves to rigorous, laboratory-type experimental controls is acceptable procedure in comparative politics. Moreover, both structure-functionalism and political science's disillusionment with past emphasis on the collection of legalistic, formalistic data about governmental institutions extend this caveat to fact-gathering about the "obvious" political institutions of any society. The overshadowing question we must all respond to therefore is, So what? This question compels us to ask what light our findings will shed on the *dynamics* of a political system.

Ideally, responses to the "so what" query would relate data-gathering and subsequent analysis to general theories pertaining to whole systems. For reasons I have already touched upon, we are a long way from such a desirable relationship between empirical research and theoretical formulations. At a somewhat more modest level of expectations, research on aspects of two or more political systems should be organized around a set of theoretical propositions relating to the *segment* of the total system that constitutes the focus for empirical scrutiny. Here too, however, the danger of bogging down in "general theories" of organizational behavior, decision-making, conflict, institutional development, or change is very great, and it is just as likely that scholasticism will infect our discussions of partial systems as it is that it will (and has) infected our treatment of whole systems.[25]

The seemingly verbal but really efficacious solution to this dilemma is Merton's now classic discussion of theories of the middle range. As Merton's discussion is applied to either whole systems or partial systems, I take it to mean that empirical research in the social sciences should avoid a theoretical fishing expedition and pretentious, impossible attempts to "test," say, the propositions generated out of Parsons' four-sector description of society and its subsystems. More specifically, Merton seems to be saying that comparative research is likely to be trivial unless the propositions we are probing empirically give us some (perhaps intuitive) reason to suppose that our findings will make the creation of general theories less impressionistic or deductive than they now so obviously are.[26]

It seems to me that the discipline might best proceed by formulating the

[25] The work of Simon and March on complex organizations, and an emergent "theory of the firm," are examples of whole-systems theories that are meant to apply to segments of the broader phenomena to which organizations (such as the specific organization called the "firm") relate. Equally influential in this development has been the broad field of macroeconomics, whose growing impact on political science I shall comment on below. Each of these approaches encounters considerable difficulty when it is meant to facilitate comparative analysis across space and time. Nevertheless, it is manifestly the case that administrative, organizational, and economic theory offers better "paradigms" for political science than does anything thus far generated by sociology, the sister discipline that has attracted the greatest interdisciplinary attention from political scientists.

[26] This point cannot be overstressed. Useful general or macrolevel theory in political science is likely to be made possible in the degree that the knowledge accumulated about important aspects of the political process is (1) precise, (2) rigorously comparative, and (3) generated on the basis of explicit hypotheses to be tested (i.e., negated). What will emerge from badly phrased research problems, imprecise concepts, obscurity of empirical indicators, inadequate attention to problems of comparisons across space and time, and incorrect inference are general theories reflecting all of these shortcomings, and more.

kinds of theoretical propositions I believe Merton has in mind and restricting these propositions for the moment to institutions and institutional processes that are clearly, directly, and intimately involved in the political process. Such choice, I believe, is not dictated merely by considerations of parsimony; it is dictated as well by a growing realization that we are, as I have already noted, oversupplied with general theory and much more poverty-ridden not only regarding systematic empirical research but also regarding the possession of the most rudimentary kind of information on which the success of the enterprise of a modern comparative politics must finally rest.

Let me take a moment to illustrate some of what I have in mind.

One of the great problems we confront in comparative politics today is that of the enormous imbalance in the amount of subsystemic or partial-systemic information available for the United States, on the one hand, and the rest of the world, on the other. We speak of the West as containing political systems emanating from a common philosophical-historical tradition, little realizing that many of the things we would want and wish to know about the political processes of Western societies are simply not yet available to us. Our generalizations thus remain gross observations, obscuring or ignoring the more subtle aspects of political systems that seem to emanate from a common historical-philosophical matrix. To be sure, we know more, say, about the politics of England than of Egypt, of France than of Vietnam, of Germany than of China. But anyone who attempts to muster information involving the simplest comparisons among Western nation-states quickly discerns that the gap remains great and is in no wise closed by general theoretical constructs that beg certain relationships and processes presumed to be typical of Western political systems. For non-Western nations the situation is immensely worse, and we should not obscure this important fact by pointing to dazzling arrays of aggregative statistical data available for well over a hundred countries.[27]

Because our knowledge regarding the political systems of non-Western societies was very limited indeed, and because literally dozens of such systems emerged as nation-states only following World War II, we badly needed and have greatly benefited from works dealing with whole systems in Africa and Asia. At their best, such works provide a needed general orien-

[27] It must be noted that my objection here cannot be met by the pat response that we must do the best we can with the data we have or, more directly put, that some data are better than no data at all. It seems apparent, given many publications of recent years, that (1) "theories" and propositions about political systems are too often *governed* by aggregative data "out there" in the public domain; (2) comparative matrices or tables of aggregative information about nation-states are constructed on the basis of sources that, as it turns out, rarely publish "all the news that's fit to print"; (3) error in the reporting of much of this information is probably more often systematic than random and therefore a cause of great concern, not easily diminished by asking the question, "What would it look like if we assume, say, a 25 percent error one way or another?"; and (4) the use of highly sophisticated mathematical-statistical methodologies on such data almost inevitably leads the reader to believe that the findings are "real." On the radically uneven flow and availability of information about nation-states, see Wilbur Schramm, *Mass Media and National Development: The Role of Information in the Developing Countries* (Stanford, 1964), esp. Chs. 2-3. On the perfectly fantastic failure to diffuse even what reliable research information we may already possess, see Everett M. Rogers, *Diffusion of Innovations* (New York, 1962).

tation to the kinds of phenomena of interest to the political scientist, as well as a great many generalizations that have enriched the kinds of questions we now raise for comparative treatment.[28]

Nevertheless, we must secure more, and more reliable, information about segments of these political systems before we can hope to push the enterprise of comparative politics much beyond its present essentially impressionistic stage. I find it instructive, for example, that political scientists are loath to make high-flown generalizations about the American political system (the one about which we have the greatest amount of information) while they will at the slightest stimulus generalize about large-scale societies in Africa, Asia, and Latin America, concerning which our lack of historical and contemporary information is perhaps the most striking thing we can say.

Filling these gaps is obviously essential. Doing so requires, I believe, attention to segments of political systems, whether these segments be institutional or behavioral in nature, whether their choice does or does not clearly relate to the validation or illumination of general systemic theories.[29] The comparative study of legislatures, public administrative systems, or political parties will serve as examples.

The comparative study of legislatures might range from the most traditional kind of formalistic and legalistic description of national legislatures and the legislative process to the most controlled kind of experimentation in legislative behavior, if, as is almost never the case in political science, the research site could be stringently controlled and manipulated. What we have by way of research findings today runs an interesting gamut of approaches; except at the most primitive level of post-factum comparisons, almost none of what we have has emerged from comparative research designs. At best, we are learning more things about more legislatures which relate to common theoretical concerns with such other things as decision-making models, coalition behavior, conflict management, the role of parties

[28] One important result of postwar political research in Africa, Asia, and Latin America is the recognition by many scholars of those areas that it is now necessary to return to Western political systems, both for testing propositions about contemporary political institutions and behavior and for reusing Western historical data to analyze in more systematic ways the evolution of Western political systems. In this regard, C. E. Black's *Dynamics of Modernization* is very instructive reading, indeed.

[29] I cannot overstress the problem of the information gap. In a field where I have done considerable fieldwork—interest-group organization and behavior—I can testify that the amount of even straight descriptive information about the so-called developed societies of the West is extremely limited. No one in Italy has yet produced a full-scale study of one or more interest groups; German scholars have published a few articles and a book or two; for France, the work of Jean Meynaud remains striking for its lack of intellectual company. Only in England have there been more than a few books in the field published, and these tend to treat interest groups morphologically and as pathological phenomena. Some items that the reader may want to consult for illustrative purposes in this field are Jean Meynaud, *Les Groupes de pression en France* (Paris, 1958); Joseph LaPalombara, *Interest Groups in Italian Politics* (Princeton, 1964); J. D. Stewart, *British Pressure Groups* (Oxford, 1958); Henry Ehrmann, *Organized Business in France* (Princeton, 1957); James M. Clark, *Teachers and Politics in France* (Syracuse, 1967); S. E. Finer, *Anonymous Empire: A Study of the Lobby in Great Britain* (London, 1959); Harry Eckstein, *Pressure Group Politics* (Stanford, 1960); Myron Weiner, *The Politics of Scarcity: Public Pressure and Political Response in India* (Chicago, 1960); Richard F. Hamilton, *Affluence and the French Worker in the Fourth Republic* (Princeton, 1967). Most of the scholars cited here are American. Compare these studies with the literature on American interest groups cited in Harmon Zeigler, *Interest Groups in American Society* (Englewood Cliffs, 1964).

and interest groups in legislative behavior, and the patterns of leadership and followership in such complex organizations.

I do not mean to sound excessively pessimistic here. Surveying literature, it is apparent that we have come far from earlier studies that naturally focused on legalistic analyses, in part because that was the easiest and also (lest we forget) an important way to begin. For many countries where roll-call votes are recorded, we have studies analyzing such things as party cohesion, the relationship between issues and voting or coalition patterns, and constituency-legislator relationships. Access to committee proceedings now means greater attention to variations in behavior from one legislative setting to another. From countries where direct interviewing of law-makers is possible, we begin to get interesting information about personality and behavior, about the legislator's role, his self-perception, his views of third persons and organizations, even some information emerging from the administration of TAT's and Rokeach Dogmatism Scales. We know more than we ever did about career patterns and recruitment to legislative positions, about the social, economic, and professional characteristics of lawmakers, and about how and why these characteristics have changed or remained stable over time.[30]

One can produce a similar roll call of interesting studies for such institutions as bureaucracy, political parties, and interest groups. In each of these institutional sectors there are enormous gaps; for other institutions such as the courts, the military, the police, and local governments, our information is fragmentary at best, almost never susceptible to reasonably systematic comparisons across more than a few societies. A recent effort to

[30] For England and the United States there are many "traditional" works, some of them of the highest quality, on the national legislature. The works of Walter Bagehot, Lord Campion, A. V. Dicey, Herman Finer, and Sir Ivor Jennings come quickly to mind for England. Works by Herman Finer, Carl J. Friedrich, and others have also served the United States well, as have more recent studies by scholars such as Donald Matthews, Gordon Baker, Dwaine Marvick, and James D. Barber. But once we leave these two countries, we are confronted once more with an enormous information gap. Giovanni Sartori's *Il parlamento italiano* (Naples, 1963) is an important exception, as is G. P. Gooch's now classic *The French Parliamentary System* (New York, 1935). Duncan MacRae, Jr. *Parliamentary Parties and Society in France, 1946-1958* (New York, 1967), is a ground-breaking (even if somewhat defective) work.

Article-length studies on the French legislature have been published by P. Campbell, "The French Parliament," *Public Administration* (1953); M. Debré, "Trois characteristiques du système parlementaire," *Revue française de science politique* (March 1955); and by Mattei Dogan, whose prolific works are too numerous to cite but can be found in the *Revue française de science politique* (1953, 1957), the *Revue française de sociologie* (1961, 1965), and other journals. Lewis Edinger, "Continuity and Change in the Background of German Decision-Makers," *Western Political Quarterly*, XIV (March 1961), 17-36, and "Post-totalitarian Leadership: Elites in the German Federal Republic," *American Political Science Review*, LIV (March 1960), 58-82; Otto Kirchheimer, "The Composition of the German Bundestag, 1950," *Western Political Quarterly*, III (December 1950), 590-601; Gerhard Loewenberg, "Parliamentarism in West Germany: The Functioning of the Bundestag," *American Political Science Review*, LV (March 1961), 87-102; and a few others have treated Germany. Gerhard Loewenberg's *Parliament in the German Political System* (Ithaca, 1967) is a splendid example of the kind of information and analysis of partial political systems of which we are so desperately in need. But except for these and studies of a few other countries we have a dearth of data, and almost none of the work that exists was *designed* as comparative study. We need, therefore, at both the national and local levels of many countries the kind of sophisticated, rigorously ordered and executed comparative work represented by Wahlke et al. *The Legislative System*. This volume clearly indicates how much that is useful we can in fact derive from comparative studies of segments of political systems.

organize a seminar on comparative legislative and electoral organization and behavior led me first to restrict the countries encompassed to the United States, Britain, Germany, France, and Italy. Then, as most who read these words will appreciate, the available data for continental European countries were found to be extremely limited, produced in relatively recent years by a very small handful of scholars. To be sure, the situation is improving, as European scholars themselves begin to fill in the gaps and as greater collaborative efforts involve Europeans and Americans in jointly designed and executed comparative studies. However, we remain some distance from even a minimally acceptable comparative data base for the most developed countries of the world, and no handbooks of "soft" or "hard" aggregate data should be permitted to obscure this elemental fact unless their relevance to *political science* can be unequivocally demonstrated.

To summarize, those political scientists who claim that we are deluged with randomly chosen empirical studies have never attempted, as I see it, to assess the nature of all of the supposed information we have about the political systems and processes of the West. Regardless of the range of our linguistic skill and the resources of American libraries, it is frequently impossible to come by the most elemental information about the political institutions of other countries. If this is the case, it is obvious that we are often depending on impressions that may or may not be accurate. General theories that proceed on the assumption that we *do* know much about similar institutions and processes the world over can only compound the chaos and confusion we begin with.

Thus, one of the most pressing reasons for increasing research attention to segments of political systems is the basic information gap and our need for filling it before we can subject general theoretical formulations to empirical confrontation. But other persuasive reasons can be adduced. One of these is that we must greatly increase the number of persons in other countries who are engaged in comparative political research. As excellent as their individual studies may be, we cannot depend for our knowledge of Ghana or Nigeria, Burma or India, Argentina or Chile on the small number of American—or in some cases European or indigenous—scholars who have been concerned with the political systems of such countries. We are moving in the direction of combining collaborative research and training in the comparative study of social and political systems. Such collaboration should eventually result in increased numbers of Asians, Africans, and Latin Americans who contribute to our storehouse of knowledge. The diffusion of the social sciences—certainly of comparative political science—is better served if initial joint endeavors focus on systematic work on segments of political systems rather than on speculative theorizing about whole systems which would be, at best, supported by empirical impressions rather than by what would pass for acceptable evidence.[31]

Another reason for focusing on partial-system analyses is that such foci better lend themselves to the articulation and testing of middle-range propo-

[31] For a detailed treatment of this problem, see my "Social Science in Developing Countries: A Problem in Acculturation," a paper presented at the 1965 Annual Meeting of the American Political Science Association, Washington, D.C. (mimeographed). This paper is available in Spanish as "La ciencia social en los paises en desarrollo: Problema de culturización," *Revista española de la opinión pública* (July-December 1967), pp. 9-43.

sitions. For any of the institutions normally accepted as intimately involved in the governmental process, we can produce a large number of interesting and important propositions which, while not designed to validate general theory, would permit us to make more universally applicable generalizations when validated in a wide range of nation-states. I might add that such propositions need not be strictly tied to political institutions, but might relate instead to decision-making models, analytic functional categories, or formulations concerning the relationship of personality or other psychological variables to organizational or individual behavior.

A third possible rationale for a narrower, more limited research focus is that it might bring comparative politics somewhat closer to policy-related problems. Now, I am aware that the profession has not yet settled the question of the proper scope of political science and that more than ever before political scientists insist that the profession's scientific concern is with the *process* rather than the *content* of politics or political policies.[32] Although this is not the place to try to explore that kind of thorny issue in any detail, it is necessary to stress that I do not accept the notion that our concern is exclusively process and that, therefore, only those theories and methods that give us better leverage on process are worthy of our attention. One reason for stressing the concern of the political scientist with content or policy is that to acknowledge the issue openly helps to guard against several related dangers. The first of these dangers is the assumption that political science—at least in its American configuration—now has the means of rising above "vulgar ideology" and qualifies for co-optation into the "scientific culture." A second danger would be that of "social engineering," which requires no further elaboration here. A third danger is that of indiscriminate fishing expeditions for data and what I would call the methodological escalation that accompanies such fishing. The political process, divorced from issues or problems of policy, has become such a huge umbrella concept (particularly in view of what various abstract theories now suggest are integral parts of that process) that I fear such a narrow focus would further the well established trend toward removing politics from political science. In short, I would urge that reasons such as these can lead us to make parsimonious decisions regarding what it is the political scientist studies.

An additional reason for explicit concern with policy is that those who are policymakers (as well as our students!) expect modern political science

[32] The most recent organized effort to reexplore what it is political scientists should do, and how, is James C. Charlesworth, ed. *A Design for Political Science: Scope, Objectives, and Methods*, Monograph 6, American Academy of Political and Social Science (Philadelphia, 1966). Regarding the specific matter of proper scope, Vernon Van Dyke, in his "The Optimum Scope of Political Science," ibid., pp. 1-17, makes a balanced case for greater attention to policy content. My colleague Frederick W. Watkins presents a telling case for emphasis on process, all the more striking in my view in that as a distinguished scholar of "political thought" Watkins might have been expected to emphasize the "content" side. See ibid., pp. 28-33, for Watkins' statement, as well as for the lively conference discussion that follows.

A much broader (our-house-has-many-mansions) view of the discipline's scope, particularly in the field of comparative politics of new nations, is offered by David E. Apter, "Comparative Government: Developing New Nations," in the forthcoming special issue of the *Journal of Politics* entitled "American Political Science: Advance of the Discipline, 1948-1968." Apter's appraisal of many of the trends I am discussing here is both more generous than mine and, I think, more sanguine.

to be aware of—and, they hope, to have something professional to say about—the kinds of major problems that beset domestic and international societies. In voicing such expectations, it seems to me that policymakers are simply articulating what most of us implicitly understand, namely, that more than any other field of intellectual or scientific endeavor, the social sciences are *not* expected to be merely "pure" sciences. I find it both amusing and ironic therefore that those social scientists who speak with increasing authority to policymakers concerned with problems of nation-building or community development are the economists and sociologists, with only a sprinkling of political scientists—and even these few often turn out to be experts in an outmoded, formalistic "science" of public administration.

To be sure, experts in comparative politics are sometimes also consulted, often with mutually distressing results. The policymaker strongly needs the "translated" implications of theory and research, and the political scientist —product of an intellectual pecking order that enthrones abstract theory— wishes to stress the elegance of theory and typology, leaving it to the men of public affairs to make what they can of such things. It is in such confrontations, I suspect, that the astronomical distance between our theoretical preachments and our research behavior is most strikingly revealed, and this may in part account for our tendency to shun policy matters.

The field of public administration will nicely illustrate these last observations. We are now painfully aware that Max Weber's ideal-typical formulations about authority systems and the patterns of public administration that accompany them will not take us much beyond morphological description of empirical situations. We are equally aware that prescriptions about administrative organization that derive in part from Weberian notions of bureaucracy and in part from the norms of democratic Western politics will not take policymakers far in resolving problems of social, economic, and political development. One result of such understanding is that a number of political scientists interested in comparative administration have tried to devise new general theories of administration or to construct typologies of political systems around certain differentiating criteria that are administratively based. Viewed as attempts at integrating a previously narrow, mechanistic, culture-bound public administration into the somewhat broader and dynamic field of comparative politics, such efforts merit approbation. Judged by the measure of their contribution to a general theory of politics or indeed of administrative systems, such endeavors strike me as being of limited utility.[33]

[33] Pioneers in the effort to transform the field of public administration would certainly include Fritz Morstein-Marx, whose *The Administrative State* (Chicago, 1957) reveals a debt to Max Weber unmarred by complex abstractions and excessive neologisms, and Fred W. Riggs, whose voluminous contributions have been instructive, even when readers such as myself have found some of his concepts and formulations unnecessarily complex. Riggs, however, has also been the prime mover in the development of the Comparative Administration Group, whose "Occasional Papers" series now includes a number of theoretical contributions of a less than cosmic ambition which are certain to have a favorable impact on the comparative analysis of public administrative systems. The first batch of the better "Occasional Papers" appears in John D. Montgomery and William J. Siffin, eds. *Approaches to Development: Politics, Administration and Change* (New York, 1966). I have attempted to provide an assessment both of theoretical models in comparative administration and of theories of political development in my "Public Administration and Political Change: A Theoretical Overview," in Charles Press and Alan Arian, eds. *Empathy and Ideology* (Chicago, 1967).

Where they are not essentially restatements of Weber *cum* Talcott Parsons, they are nevertheless formulated at levels of abstraction that defy systematic comparative empirical application and, for this reason among others, are of little or no use to those confronting problems of policy or operational alternatives. What differences ideal-typical morphologies adduce are generally gross; more often than not, both the typologies devised and the "models" of administrative systems suggested are based not so much on rigorously accumulated historical or contemporary evidence but on illustrations that are themselves often impressionistic. Where attempts are made to draw operational axioms from such theorizing and model-building, they often result in curious justifications for whatever patterns of power and administration actually emerge in the so-called developing areas. Above all, the classification of nation-states by the presumed characteristics of their administrative systems and the related conditions (environmental or ecological) that surround them generally results in grouping in single categories precisely those nation-states *among* which we must make refined discriminations before we can say anything meaningful of a probabilistic or prescriptive nature. In this sense, such efforts serve us no better than massive accumulations of aggregate statistical data about the world's nation-states. Such data, when subjected to high-powered, computerized analytical techniques, reveal that, say, Sweden, the United States, Britain, France, Germany, Norway, and Italy are in one group and the Sudan, Nepal, Afghanistan, and Tanzania are in another. The only striking difference I have thus far detected in these two approaches is that the former, more impressionistic approach is cheaper and probably more sophisticated.

Are we then to abandon both grand theorizing and the accumulation of empirical data about bureaucratic systems? Clearly this is not what I intend, and the sector of public administration, as a *segment* of any political system, is one of the areas in which our empirical research can be fruitfully guided both by very important public policy concerns and by theoretical propositions of the middle range. We know, for example, that economic development in almost all of the developing nations is likely to take place largely through public-sector intervention and that participation of government in such change-directed enterprises is also increasingly true of the so-called developed countries. Now, while there is a vast and growing literature produced by economists on the subject of planning, political scientists have paid scant attention to this problem, except at the fringes of macroanalytic considerations. To be sure, as our theoretical outpourings shift from the "input" side (where the political system seems to be abjectly dependent on elements in the environment) to the "output" side (where the institutions of government are recognized as having an independent impact on societal change), we begin to read about the "capacities" or "capabilities" of the political system to achieve certain ends in view, including economic change or growth. No doubt it is important to acknowledge that political institutions must confront a wide range of challenges, from the maintenance of order and the provision of social overhead capital to the provision of the kinds of material and human resources (and their integrated coordination) that planned economic growth requires. In this regard, a number of writers have served us well, although I think it once again striking that several of these are either economists or sociologists and that with rare exceptions political scientists have been late in moving in this direction.[34]

Much more needed, however, is greater attention by political scientists, whose analyses of a partial political system such as the bureaucracy either explicate the bureaucratic process of a range of nation-states in considerable detail or deal comparatively with public administrative systems in terms of their problem-solving capabilities. One important step in this direction is the series of studies of national planning now under way at the University of Syracuse, under the general editorial direction of Bertram M. Gross. These national studies involve political scientists almost exclusively; the specific intention of Professor Gross is to begin to fill the most serious gap in the planning literature, namely, the differences in the phenomena that are introduced by specific segments or aspects of the political system in different national settings.[35]

I have also recently suggested a comparative approach to the relationship of public administration to problems of development which would involve the construction of national profiles. Such profiles would require gathering data on the developmental and related goals of national policymakers, the total and kind of administrative resources available for goal attainment, the obstacles to the creation of whatever additional resources are needed for goal achievement, and the potentiality for overcoming such obstacles and of achieving a reasonable balance between goals and administrative capacity.[36] Although some of the data categories implicit in such profiling would have to be treated somewhat impressionistically, other potentially available

[34] Gabriel Almond, whose earlier work profoundly influenced important shifts in theoretical and empirical focus in comparative politics, is one political scientist who has led the movement toward greater attention to the output side of the polity. See, for example, his "Political Systems and Political Change," *American Behavioral Scientist*, VI (June 1963), 3-10, an early formulation that emerged from discussion and a summer seminar held by the SSRC Committee on Comparative Politics; and his more recent "A Developmental Approach to Political Systems," *World Politics*, XVII (January 1965), 183-214. There are other political scientists who could be named here as well, and an interesting overview of their writings and the approaches to the comparative study of public administration they represent is included in Ferrel Heady, *Public Administration: A Comparative Perspective* (Englewood Cliffs, 1966).

Nevertheless, it is significant that much of both the theoretical and the empirical leadership in public administration during that last fifteen or twenty years has been provided by the work of sociologists such as S. N. Eisenstadt, *The Political Systems of Empire* (London, 1963); Berger, *Bureaucracy and Society;* Robert K. Merton et al. *Reader in Bureaucracy* (Glencoe, 1952); Reinhard Bendix, *Nation-building and Citizenship* (New York, 1964); Philip Selznik, *TVA and the Grass Roots* (Berkeley, 1949); and Michel Crozier, *The Bureaucratic Phenomenon* (Chicago, 1964); and by the work of economists such as Joseph J. Spengler, in his jointly edited *Administrative and Economic Development in India* (Durham, N.C., 1963); A. H. Hanson, *Public Enterprise and Economic Development* (London, 1959); and Bert F. Hoselitz, "Levels of Economic Performance and Bureaucratic Structures," in Joseph LaPalombara, ed. *Bureaucracy and Political Development* (Princeton, 1963).

[35] Volumes thus far published in the Gross series include: B. Akzin and Y. Dror, *Israel: High Pressure Planning* (1966); H. J. Arndt, *West Germany: Politics of Non-Planning* (1966); D. E. Ashford, *Morocco-Tunisia: Politics and Planning* (1965); F. G. Burke, *Tanganyika: Preplanning* (1965); J. Friedman, *Venezuela: From Doctrine to Dialogue* (1965); E. E. Hagen and S. F. T. White, *Great Britain: Quiet Revolution in Planning* (1966); J. LaPalombara, *Italy: The Politics of Planning* (1966); and R. J. Shafer, *Mexico: Mutual Adjustment Planning* (1966).

[36] See Joseph LaPalombara, "Alternative Strategies for Developing Administrative Capabilities in Emerging Nations," *CAG Occasional Papers* (Bloomington, 1966).

aggregative data are available to the persistent researcher. Such data would *not* be accumulated merely because they are (correctly or falsely) easily available. Rather, decisions as to where to put data-accumulating energies to work would rest very firmly on the articulation of empirically manageable hypotheses about the relationship of public administrative organization to behavior and development. I should add that, if the hypothetical statements involve concepts as broad as the "pattern variables" of Talcott Parsons, the empirical indicators that would permit scoring—or, I would hope, ranking—each country on each of these variables as they apply to any sector of society would have to be carefully and persuasively specified, if for no other reasons than that (1) the number of such indicators approaches infinity and (2) the same indicators can be scored (rightly!) on both sides of each dichotomy. We have pretty much exhausted scholastic exercises about how much achievement orientation, universalism, collectivity orientation, effective neutrality, and functional specificity is required, say, by economic modernity or a public administrative apparatus conducive to economic modernization. For those who would in fact try to validate aspects of Parsons' theories, profiling of the kind I have in mind might be one potentially workable first step. Even for such scholars, I contend, an empirical focus on a segment of a whole political system would offer greater hope of succeeding than would a research enterprise requiring attention to the whole system.

To repeat and to reemphasize, I remain skeptical about the whole-systems approach to comparative politics. My skepticism can perhaps best be summarized by two quotations from Heinz Eulau, whose capacity to deal imaginatively and creatively with partial-systems analysis is well known. About whole-systems approaches, Eulau remarks, with characteristic bluntness, "But I have yet to read—and that includes David Easton's new book—a systems analysis from which one can derive testable propositions about politics."[37] About the most perplexing empirical problem of gathered data about whole systems, he remarks, "How does one observe whole systems? Well, I would say that at the present time it is impossible to observe whole systems. I think that one can make statements about whole systems, large systems, but that one cannot observe them."[38]

I would add that partial-systems comparisons of the kind I have discussed above should over time reduce the magnitude of the observational problem. In such a future, typologies will be less abstract, much more induced by reflections about empirical information gathered from carefully designed research on segments of the larger political system. No one will deny the desirability of a probabilistic theory of politics. My claim is that the quantum leap to the whole-systemic level of theorizing has tended to degenerate into a neo-scholasticism from which escape itself is difficult, and when escape occurs at all, it involves return to partial-systems analysis anyway.

It may well be, however, that we are at the threshold of scholasticism disguised as mathematical models, where the explicit acknowledgement of the model builders is precisely that their central assumptions have no correspondence to reality and that, indeed, it is probably better that such cor-

[37] In Charlesworth, *Design for Political Science*, p. 202.
[38] Ibid., p. 207.

respondence *not* be a major consideration for an indeterminate period. Some who urge such a line of disciplinary development ask us to look to macroeconomics for guidance—and for impressive evidence for the proposition that striking scientific advances can emerge from oversimplifications, even deliberate distortions, of the conditions of the real world. Now and then, the very same scholars who once borrowed indiscriminately from sociological theory (only to discover that the emperor was if not naked then strikingly ill-clad) now propose to do essentially the same thing with the sister discipline of economics. The same fears about the inferior status of political science vis-à-vis sociology expressed twenty years ago are now voiced regarding economics. Thus William Mitchell, a close student of Talcott Parsons, warns us that political scientists run the risk of being swallowed up by those adept at cost-benefit analysis, welfare economics, optimal rationality in goal achievement, exchange models, and systems theory.[39] For Mitchell, the emerging paradigm will be that of the new political economy. Accommodating to it will mean that "theory will become increasingly logical, deductive and mathematical. In terms of its content, we will make increasing use of economic theory, games theory, decision theory, welfare economics and public finance. Models of political systems, analogous to types of economies and markets, will proliferate."[40]

Although it may be an easy, even a logical, step from general theories in sociology to the sort of brave new world the quotation above suggests, such a step is neither inevitable nor desirable if we bear in mind the relative stage of development in the field of economics at which the breakthroughs discussed by Mitchell occurred. I refer primarily to the solid empirical base economics had established, as opposed to the very problematical empirical base that is currently available in political science and to which I have repeatedly alluded above. As Sidney Verba has aptly phrased our current dilemma, "In the old days, graduate students may have gone into the field as barefoot empiricists. Today they go equipped with elaborate systems models. . . . The barefoot empiricists didn't know where they might step; the recent students have trouble getting their feet on the ground."[41]

Verba suggests the need for a "disciplined configurative approach" and appropriately cites recent work by Dahl and others as strikingly promising examples of what he has in mind.[42] Such work is indicative of what I mean by less-than-whole-systems approaches to theory and research in comparative politics. Where the theories, models, and methods of sister social science disciplines can be used unequivocally to further this kind of work, they should be borrowed and adapted without hesitation. It seems to me that herein lies the road map to a stronger political science.

## Problems in Comparative Research

It is easy enough to say that comparative research at a partial-systems level will contribute to an additive political science. But this approach, too, is not

39 "Shape of Political Theory," pp. 18-19.

40 Ibid., p. 19.

41 "Some Dilemmas," p. 117.

42 Robert A. Dahl, ed. *Political Oppositions in Western Democracies* (New Haven, 1966). See particularly the important theoretical implications Dahl is able to draw from the country-focused chapters, Chs. 11, 12, and 13.

free of perplexing problems, some of which affect comparative politics in general, some of which are highlighted or intensified when they emerge in partial-systems analysis.

If something less than the whole system is to be analyzed, the first and most obvious problem to resolve is that of the most important unit of analysis. My reference here is not to the *empirical* unit of analysis; I assume that the individual, whether singly viewed or conceptualized in some group or associational context, is the commonly accepted empirical unit in the behavioral sciences. I am referring instead to the *theoretical* unit of analysis, on the assumption that attention must be accorded this matter if we are to avoid falling into the crudest kinds of bare-facts empiricism, which Verba describes.

The question of the appropriate theoretical unit should not be confused with the question of the independent or dependent variables in comparative research. Presumably, the determination of what theoretically causes, influences, or is associated with what and for what reasons comes at a later stage in the design of comparative research. Nor should we confuse "concrete-structural" or institutional units with what I intend here by theoretical. We may, for example, decide that we want to compare fruit, noncitrus fruit, or just apples, but for each of these choices it is necessary to indicate the focus (or foci) of central theoretical concern. Likewise for politics, we might choose to study legislatures, legislative committees, or individual legislators, but it is important to specify the single or combined set of theoretical concerns around which the comparative analysis will proceed. Such a procedure is required for several reasons, not the least important of which is that of anticipating the messiness caused by confusion as to the *level* of analysis at which research itself is directed.[43] So many of our generalizations about the political process move with apparent randomness from the micro- to the macroanalytic levels that it is difficult to know if, for example, a study of legislative roles is designed to test psychological theories about individual or group behavior or sociological theories about the institution of the legislature itself. In short, we must be clear about occasions when we intend that the study, say, of individual legislators or of legislative committees is intended to reflect in a microcosmic context propositions we intend to apply to all legislatures, to all representative organizations, to all complex organizations, to the political system, or to society.

Clearly, the social sciences now provide a rich variety of theoretical units of analysis, from the broad actor-situation framework associated with Parsons to voting behavior, where the act of voting can be conceptualized as illuminating theories about social stratification, communications, personality, functionalism, decision-making, and so on. The most widely utilized theoretical unit in political science seems to me to be decision-making, and a vast range of the research output of the discipline can be subsumed under this rubric. Thus whether we ask who governs, or who gets what, or who has how much power and how it is exercised, or what variables seem

[43] One of the most lucid discussions of Harold Lasswell's contributions to interlevel relationships and theories about them will be found in Heinz Eulau, "The Maddening Methods of Harold D. Lasswell," *Journal of Politics*, XXX (February 1968), 17ff.

to account for executive or judicial behavior, or what things are associated with distributions of popular votes, or how political elites respond to historical crises, we seem to be posing as the generalized (independent or dependent) variable the making of political—or politically relevant—decisions.[44]

To be sure, a great many political scientists are also interested in change, that most elusive of the dynamic phenomena with which the social sciences are concerned. In today's world, we want to know as never before what difference (e.g., in reaching the takeoff stage in economics or in assuring legislative stability) a single-party or multiparty system will make in Ghana or Brazil, Thailand or Turkey. The problem of how bureaucrats should be trained or the question of whether the upper reaches of a bureaucracy should be dominated by generalists or specialists, "guardians" or 'technocrats" as some would put it, has never been more poignantly posed than by political leaders and their followers who *say* that they simultaneously wish to promote man's material well-being and his freedom and dignity. For those who view "political development" as the increasing ability of political leaders and institutions to bring about a greater congruence between the demands they confront and the policy output of government, it is clear that, at one level of analysis, concern with both how in specific concrete situations decisions are made and what their consequences are is inevitable.

Decision-making of course provides a very broad analytical framework and thus does not in itself resolve all of the difficulties inherent in comparative analysis. Yet it seems to me that one of its striking advantages is that it directs our attention to the outputs of the political system and therefore to those aspects of the political process that involve formal governmental institutions. Functionalism, on the other hand, leads one to emphasize the input side of the equation and therefore tends to push research in the direction of such problems as the political socialization of children, which, while intrinsically interesting as an area of research, appears far removed from the political process. I might add that political socialization research, when it does not concentrate on subjects who are probable future political elites, begs an important question, for we have not yet succeeded in demonstrating persuasively the assumption that the values, beliefs, and attitudes about politics and political institutions held by the mass population make a difference.

To put this in terms of parsimony, I would prefer comparative research on decision-making in legislatures, bureaucracies, political parties—even in elections—to comparative studies of the political socialization of children, patterns of recruitment to governmental roles, or the system of communication found within society. It isn't that these latter concepts or analytical units are uninteresting or irrelevant; it is that their relationship to the out-

[44] It is this fact, I believe, that leads many political scientists to conclude that rational-choice models, decision-making under conditions of uncertainty, and other formulations in economics are potentially of great utility in our own discipline. This is probably true to the degree that the theoretical unit of analysis remains something smaller than the political system. Indeed, it may be necessary to narrow the focus still further either geographically and culturally, or by constraining specifications for such concrete structures as legislatures, political parties, and bureaucracies, or both.

put side of the governmental system remains extremely tenuous since we know very little in fact about what goes on in the "black box" that stands between inputs and outputs.

A decision-making focus for the political scientist should also involve, again for reasons of parsimony, a preference for obviously political institutional settings for research. Case studies of trade unions may perhaps illuminate the political process, but political parties should be preferred if they are accessible. If trade unions are placed under the empirical microscope, propositions about them should relate to some specific aspect of the political process and, more stringently, the making of political decisions. To put it simply, it is necessary to respond, in more than vague or seemingly logical or self-evident terms, regarding the relevance of research into nonpolitical institutions for the operation of political institutions themselves.

The selection of institutions or "concrete structures" as the focus for research leads to a second major problem, namely, that of the comparability of the units selected for analysis. At least the problem appears at first blush to be more complex than would be the case were we to limit our focus, say, to decision-making, influence and power, or communications and leadership in complex organizations. But, unless we are easily stampeded by what turn out to be scholastic objections by structure-functionalists, it is plain enough that whether we begin with concrete institutions or with an analytical concept such as decision-making, the problem of comparability is essentially the same.

Let me push ahead with this line of argument to identify what is really our concern.

It is possible that for some the central theoretical concern would be simply the process or structure of choice—of reaching decisions—in a wide range of simple or complex formal organizations. For such scholars, I believe, comparative research would require the most careful specification and control over certain characteristics or parameters of organizations before meaningful comparative analysis could proceed. Assuming a large enough sample of organizations, such scholars would want to control for such things as the number of decision-makers involved, the structure of their values and belief systems, the degree of hierarchy and the administrative differentiation of roles, the structure of internal communications, the patterns of authority and sanctions prevailing, the degree and kind of discretion or permissiveness in role performance, the relationship to the organizationally external environment, and so on. Organizational theorists are able to draw research samples for comparative analysis from a much wider universe than is available to the political scientist—assuming for the moment that the political scientist is interested in politics or, in the case at hand, in *political* decision-making.

This being the case, it seems apparent that the political scientist will by the empirical nature of things be less able to "control" for certain parametric conditions than will the person interested in organizational behavior. The comparative research that the latter does may greatly assist the political scientist in designing a research project and in interpreting his findings. We cannot ask of the political scientist, however, that he adhere to the same canons of maximizing the comparability of his research endeavors that would be justified for a scientist whose unit of analysis (in this case, decision-making) encompasses a much wider range of empirical research sites and units.

Essentially the same argument can be developed where, say, the theoretical unit of analysis is some aspect of functionalism and where the institutional focus for comparative research is the interest group or pressure group. As the work in the interest-group field attests, pains are taken to abstract from the infinite number of group settings in which the individual might be found something called a *political* interest group. We need not be concerned with whether John Dewey was correct in insisting that all human behavior is group-centered behavior or whether A. F. Bentley was right in declaring that if we fully comprehend the "group process" we will have comprehended everything about politics. But if the interest group is to be made the institutional focus for comparative political science, we must be concerned with the designation of criteria that will permit us to abstract from a potentially infinite number of groups those that are of particular interest to the political scientist and that meet the minimal definitional requirements for inclusion in a sample.

The comparative political scientist, then, must be guided in the first instance by the central concern of what is political or what is relevant to the political process. If this is so, then it is unlikely, except at the very abstract and empirically unmanageable level I have associated with whole-system analysis, to satisfy David Easton's thought that "ideally, the units [of analysis] would be repetitious, ubiquitous, and uniform, molecular rather than molar."[45] Where, as in systemic and functional analysis, the units seem to be ubiquitous and uniform, they are molar rather than molecular; where, as in group analysis and decision-making, they appear to be molecular, they are not uniform and probably not minutely repetitious. The dilemma of comparative politics is that we have available neither the particles of physics nor the prices of economics to subject to comparative analysis.

The problem of the comparability of the unit of analysis is also apparent when one chooses such a seemingly obvious structure as political parties as the focus for research. The political party appears to be a deceptively stable unit concerning which much comparative research can be generated. Yet, it is obvious that little attention has been paid to the question of what we are comparing when we look analytically at parties either across national or cultural boundaries or within a single nation-state. Voting studies in the United States, for all of their display of methodological rigor, have ignored this problem, as indeed they have ignored most questions of theoretical relevance until recently.[46] Nevertheless, those who purport to execute comparative research here must arrive at some workable and consistent definition of a political party if comparison is to involve oranges, apples, or lemons rather than a shifting combination of these, or fruit salad. Myron Weiner and I in a recent published symposium attempt to respond to this

[45] "The Current Meaning of Behavioralism in Political Science," in Charlesworth, *Limits of Behavioralism*, p. 17.

[46] The studies of Michigan's Survey Research Center have been notoriously rich in methodology and poor in theory. The last volume published by the group, however, represents a first and welcome step toward correcting this deficiency. See Angus Campbell, Philip Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York, 1960).

problem. It is perhaps indicative of the state of the discipline that a number of our colleagues are willing to accept as political parties any organization whose leaders or members call it such, without regard to questions of definition.[47] Such resolutions will not do. Whenever we elect a segmented or partial-systemic approach to comparative politics the kinds of problems I have raised here must be confronted and reasonably resolved.

A third major problem—not confined to partial-systems analysis—involves the nature of evidence, or the kind of data we will or can gather to validate or invalidate propositions. This problem is much too vast to pretend to treat here in detail, but a number of observations will help to round out my discussion.

First, I believe it is essential to recognize that some of the hypothesis-validating data we will need may not be easily accessible or may not be available to us at all. This is true in part because of the areas of secrecy that surround many aspects of the governmental process. It is true as well because many nation-states look with increasing reservation, even hostility, on the overseas legions of American field researchers whom the golden era of social science seems to have spawned in this country.

If we can't get at the relevant or most immediately relevant facts, what then? Why, we may have to delay—even give up—some lines of research endeavor. We may also *cautiously* choose to do the best we can with what data we have. But this alternative means that we must be scrupulously and not self-deceivingly careful about the data-collecting choices we make and, above all, that we must not gather and store data simply because they are available. It may be, as some claim, that the availability of vast quantities of data, when processed by high-speed computers, will help us to generate new theories, but for the present I find that expectation very doubtful.[48]

Second, it is necessary to pay more than lip service to the observation that much of the empirical information extant is not really comparable and that equally much of the seemingly reliable aggregative statistical data are just simply poor, that is, unreliable and subject to errors whose nature is neither random nor known to us. Such data, far from illuminating the processes we wish to study comparatively, may actually be totally misleading.

Karl Deutsch, recognizing that the quality of data available to us may vary considerably, ingeniously suggests that computers and new techniques of data analysis may help us to overcome the limitations inherent, say, in survey data or aggregative statistical information. If this is so, he says, "truth may be thought of as a relationship between different streams of evidence. A statement is more likely to be true, the larger the number of different classes or kinds of evidence that confirm it."[49] This statement seems reasonable enough, so long as our decisions about the kinds of data

[47] See LaPalombara and Weiner, *Political Parties*, Ch. 1. Cf. the chapter by Rupert Emerson in the same volume.

[48] On this point, see Karl W. Deutsch, "Recent Trends in Research Methods in Political Science," in Charlesworth, *Design for Political Science*, pp. 149-178. I do not share Deutsch's enthusiasm about future data collections, and I must confess that, until I am assured that some of the questions of comparability I have raised in this article have been more adequately resolved, Deutsch's surmise that by 1975 we may have some fifty million IBM cards of "data" to draw upon is much more disquieting than it is reassuring. See ibid., pp. 152-157.

[49] Ibid., p. 158.

to collect are made on the basis of propositions to be tested comparatively, and so long as we do not deliberately include in our "stream of evidence" data we suspect or know to be highly unreliable.

Third, as we look to history for information that will help us to confirm or disconfirm propositions about political development, we must have a better sense than we now do about what specific institutions (such as parliaments, interest groups, or political parties) have meant over time in a single society. To say this is merely to restate the central problem implicit in comparative history as opposed to loose historical chronology: On the basis of what reasonably applicable criteria can we periodize societies, the institutions they give rise to, and the impact in turn of such institutions on subsequent development? Among the many useful purposes this exercise will serve is that of permitting those of us who are interested in a *particular* kind of political development, namely, some variation of the democratic state, to identify with greater precision generalized or generalizable "stages" of democratic institutional development.[50]

**Conclusion**

Is emphasis on partial political systems or segments of them the only legitimate or fruitful enterprise for contemporary comparative politics? Clearly not, nor have I intended to make this claim. If we are as far as most of us suspect from a probabilistic theory of politics, any closure at this time regarding levels of analysis, sectors of the political system to be analyzed, or methods to be utilized in the testing of theoretical formulations would be premature—childish in the fullest sense of that term.

My purpose rather has been twofold. First, it seems to me that we ought to be absolutely candid about what it is political scientists do. This requires above all that we not be deluded into thinking we have evolved empirical general theories when what we have are a number of impressionistic, somewhat abstract, deceptively empirical observations strung together by logical statements of varying elegance. Nor should we fail to note that while ideal-typical constructs need not respond to empirical reality on a one-to-one basis, they are not very useful if we understand the real world to involve an infinite mixture of characteristics that ideal-typical constructs artificially separate, with no provision of insights into possible or probable "mixes."

My second purpose has been to suggest a rationale for emphasis on partial systems in comparative politics. Because I assume such research may serve to correct certain deficiencies in whole-systems analysis and therefore open the way to better empirical theories of whole systems, I may be said to have come full circle. That is, I am sure that most political scientists cannot—in any case, should not—sidestep concern with the difference their discoveries make in our understanding of how whole political systems are evolved, maintained, and changed.

If I want to profile the conditions that impinge on the public administra-

50 With sobering reservations about the difficulty of relating historical data to propositions about political change, I have attempted this exercise with regard to administrative change in England, France, and Germany over a period of several centuries. See my "Values and Ideologies in the Administrative Evolution of Some Western Constitutional Systems," in Ralph Braibanti, ed. *Political and Administrative Development* (Durham, N. C., 1968).

tive problem-solving capability of a sample of nation-states, my interest in doing so must surely reflect more than an abstracted scientific curiosity about the relationship between human, physical, and organizational resources and goal attainment. If some of my colleagues design a comparative project aimed at probing the relationship between a long list of social, personality, cultural, and related variables and what occurs in a national legislature, they are surely interested in something more than decision-making or power relationships in *that* kind of an organization. If another group of my colleagues seeks to understand the circumstances under which those who formally occupy religious or military roles begin to impinge directly on the policy output of political structures, they are interested in something more than the conditions that bring functionally specific institutions and role occupants into aspects of the political process where they presumably have no "logical" or "theoretically acceptable" place.

My point here is double-edged. First, I would agree with David Easton that, in considering the so-called behavioral revolution, we should distinguish very carefully between the impetus toward better methodology and the thrust toward better empirical theory. However we may resolve how to attack the problems of concern to the political scientist, we should understand that a second aspect of all of the ferment we are experiencing involves not merely method but a concern with theories about how political systems evolve and function and what influences them. But I would go beyond Easton to insist that for most political scientists there is great concern for the "good society" and for how we can devise the set of institutions and behaviors that will enhance its development and survival. While such normative concerns must be distinguished from the more scientific concerns of comparative political science, they should not be submerged to the point at which we delude ourselves in thinking that we are more like physicists or pathologists than we are. We are, I believe, the intellectual descendants of Aristotle, proud to share some of his major concerns and perhaps humbled by the understanding that we have not advanced our scientific understanding of political organization and behavior much beyond what he elucidated in the *Politics*.