

Political Science: The State of the Discipline

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**COMPARATIVE
POLITICAL PROCESSES
AND POLICYMAKING**

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Comparative Public Policy: An Assessment*

M. Donald Hancock

Public policy studies are indeed a multiform menagerie: they are narrative, quantitative, cross-national, cross-sectional, single policy cross-nationally, several policies cross-nationally, not to mention the many volumes devoted to the metatheory and metapolitics of policy studies. Any day now we will surely have a tortured soul cry out: "Enough!" and present us with "A prolegomenon to any future. . . ." (Diamant, 1981, p. 103)

By the early 1970s comparative public policy had emerged as a recognized subdiscipline within political science. A significant milestone in this development was reached when the American Political Science Association designated Arnold J. Heidenheimer, Hugh Hecllo, and Carolyn Teich Adams' book *Comparative Public Policy: The Politics of Social Choice in Europe and America* as the best political science publication on United States national policy in 1975. The citation noted: "Cross-national policy research is still in its infancy; the [Gladys M. Kammerer Award] will be well-used if it stimulates the development of this field" (*PS*, 1976, p. 441).

The Association's hopeful expectations have been amply realized. During the late 1970s and early 1980s, numerous additional policy-oriented volumes and hundreds of journal articles have appeared, many of them genuinely comparative. At least five journals devoted exclusively to policy questions have been established in the United States and Canada. Comparative policy courses have been incorporated in both graduate and undergraduate curricula throughout North America. An advanced textbook series published by Temple University Press is well underway. And specialized con-

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ferences and panels on comparative policy themes have been sponsored by Cornell University, the Council for European Studies, and both the national and regional professional political science associations.

Yet the very proliferation of comparative public policy studies raises important intellectual issues. The first concerns the enormous diversity in the scope, emphasis, methods, and findings in the existing literature. In light of this diversity, Elliot Feldman (1978) recently concluded that comparative policy analysis is a non-field lacking a "guiding theory" to focus research. Feldman's contention raises the obvious, and essential, initial question of assessment: What *is* comparative policy analysis? The first part of this essay will attempt to answer this question by formulating a conceptual framework for classifying the myriad studies that comprise the comparative policy literature. Even more important, however, are issues concerning the *content* of comparative public policy research. As my second, and more substantive undertaking, I will, therefore, survey the principal research foci, methods, and policy evaluations used within the subfield on the basis of the classificatory framework presented in the first section. In my conclusion, I will suggest, in light of what has and has not been accomplished to date, some priorities for future research.

A FRAMEWORK FOR ANALYSIS

"Comparative public policy," Heidenheimer-Heclo-Adams assert, "is the cross-national study of how, why, and to what effect government policies are developed" (1975, p. v). Most toilers in the proverbial vineyard of policy analysis would presumably concur with this textbook definition. "Policy analysis," Thomas Dye has observed, "is finding out what governments do, why they do it, and what difference it makes" (1979, p. 652). More simply, Ira Sharkansky states: "Policies are actions taken by government" (1970, p. 1).

The first step in assessing comparative policy research involves disaggregating the "how," "why," and "to what effect" components of the definition. Some scholars emphasize the former component, others the second, still others the third, and yet others various tangential or more general aspects of the subject. This has led to the formation of a wide variety of classificatory schemes that encompass the topics that ostensibly comprise comparative policy analysis. The most parsimonious is a bibliography of comparative policy studies compiled by B. Guy Peters for the inaugural issue of *Policy Studies Review* (1981). In Peters' view, the major research categories within the subdiscipline include: (1) general and theoretical studies, (2) tax policy, (3) economic policy, (4) social policy, (5) public expenditure studies, and (6) health policy. A partially overlapping and slightly longer list (albeit one emphasizing domestic public policy) is that contained in Fred I. Greenstein and Nelson W. Polsby's edited volume on *Politics and Policymaking* in the *Handbook of Political Science* series (1975): (1) research in policy analysis, (2) economic policy, (3) science policy, (4) welfare policy, (5) race policy, (6) comparative urban policy, (7) foreign policy, and (8) policy evaluation.

Douglas E. Ashford, Peter J. Katzenstein, and T. J. Pempel also distinguish among eight policy categories in their *Comparative Public Policy: A Cross-National Bibliography* (1978): (1) administrative reform, (2) economic management, (3) local and regional reorganization, (4) labor relations, (5) race and migration, (6) social security, (7) higher education, and (8) science and technology. In their first edition of *Comparative Public Policy*, Heidenheimer-Heclo-Adams concentrated on a somewhat different catalogue of topics: (1) health care, (2) secondary education, (3) housing, (4) city planning, (5) local government, (6) transportation, (7) income maintenance, and (8) taxation. They have extended their inventory of comparative policy topics in the second edition of their book (1983) to include assessments of (9) “the politics of social choice, (10) “economic policy,” and (11) “policy constraints in the welfare state.” Finally, the compilers of the *United States Political Science Documents* utilize 19 reference categories to encompass domestic and comparative policy studies, of which “policy evaluation” and “policy implementation” regularly contain the largest number of entries.¹

Each of these classificatory schemes serves the useful purposes of codifying previous research, reporting new findings and insights, and/or guiding the interested scholar to relevant bibliographical sources. None, however, captures the full scope of comparative policy analysis and the wide array of empirical, methodological, and theoretical concerns demonstrated by its practitioners. To that end, I propose a more detailed classificatory framework in the form of a commonsense taxonomy based on the diverse components of the policy process itself.

As illustrated in Figure 1, the taxonomy categorizes the general and theoretical perspectives characteristic of the subdiscipline as well as the central empirical and analytical foci of comparative policy research itself. The former encompass contrasting metatheoretical assumptions about society and the policy-making process as well as different methodological approaches. The latter range from the description and analysis of the “how” and “why” of policy-making—including principal determinants of policy initiatives and choices—to assessments of specific policy outputs and their short-term, historical, and other consequences (i.e., the “to what effect” components of the Heidenheimer-Heclo-Adams definition cited above). One advantage of the taxonomy is that it makes explicit the fact that comparative policy analysis is more than the sum of its parts. Instead, it is a microcosm of the conceptual, methodological, and analytical diversity within political science as a whole; it is distinctive largely because of its emphasis on the causes, different forms, and social consequences of government action, or nonaction, in a variety of historical and contemporary settings.

In the sections to follow, I shall utilize the various categories of the taxonomy as a means to structure my assessment of the dominant approaches, findings, and problems that distinguish comparative policy analyses. For reasons of manageability, I have restricted my survey—with several exceptions—to the Anglo-American political science literature. My assessment of specific books and articles embraces a relatively small number of representative studies drawn from more than 300 bibliographical citations which I compiled for this essay.² I have selected these studies on the basis of the signifi-

FIGURE 1
A Taxonomy of Comparative Policy Analysis

General and Theoretical Perspectives	Policy Determinants	Policy Outputs and Evaluation
<i>Subject Matter</i>		
Contrasting Concepts of Society Contrasting Definitions of Policy Contrasting Research Methods and Units of Analysis	Levels of Economic Development Elites Bureaucracy The State Types of Political Regimes Political Parties Interest Groups	Policy Areas: health, pensions, incomes policy, education, housing, taxation, employment, etc. Contrasting Evaluations: (1) Short-term: policy "impact," effectiveness (2) Longer-term: systemic consequences, e.g., "crises" of democracy, the welfare state, and/or capitalism; emergence of varieties of corporatism (3) Analytical: relevance of empirical findings and conceptual refinements for theory-building; explanations of "history," inter-system differences, etc. (4) Prescriptive
<i>Illustrative Scholars</i>		
Ashford (1977, 1978) Cyr and deLeon (1975) Feldman (1978) Heclo (1972) King (1973) Peters (1977) Rose (1973)	Anton (1980) Cameron (1978, 1982) Castles (1978, 1979) Flora and Heidenheimer (1981) Heclo (1974) Heidenheimer (1973) Heidenheimer-Hecko-Adams (1975)	Hibbs (1977, 1982) King (1973, 1981) Kjellberg (1977) Lindberg (1975, 1977, 1982) Peters (1972, 1975, 1977) Smith (1969, 1975) Wilensky (1975)

cance of their findings and/or their suggestiveness for future research. No doubt I have excluded authors whose work fully deserves equal attention to those whom I cite; I apologize in advance for such omissions. To do proper justice to a evaluation of the subject would require a book.

GENERAL AND THEORETICAL PERSPECTIVES

In an early assessment of comparative policy analysis as an emergent sub-discipline, Richard Rose (1973) compiled what has become a standard set of justifications for the comparative study of public policy. The first justification, he noted, derives from the consideration that

[t]he major problems that face one Western government are often the same as those that face its neighbours. . . . Whether the existence of common or similar problems results in a similar response is a question of practical and theoretical importance. Men of affairs may see the justification for comparative policy studies in the prospect that their government will be able to borrow policies or institutions from another country for use in resolving domestic problems. . . . Scholars too have appreciated the "quasi-experimental" features of comparative research in circumstances in which controlled experiments are merely impossible. (pp. 68-69)

In addition, Rose suggested that comparative policy analysis helps scholars avoid "culture-bound generalizations" which might arise from single-country policy studies; identifies "nondecisions" by national leaders who fail to respond to issues that lead to policy action in other countries; raises new questions concerning the role and importance of governmental institutions; and enables the scholar to test the "significance of social and economic conditions for public policy. . ." in a variety of settings (pp. 68-70).³ Feldman (1978) has added to Rose's inventory of basic justifications the hopeful expectation that comparative policy analysis will yield "embracing theories for politics, as well as policy, beyond the boundaries erected by the details of systems because comparison helps establish norms for judgment and helps distinguish the essential from the trivial" (p. 187).

Underlying such objectives is what Charles Anderson (1982) has characterized as an "implicit social democratic orientation" among most practitioners of comparative policy analysis. Virtually all of the scholars whose work is cited in this chapter tend to look with favor on the activist state and its twentieth century socioeconomic achievements. As Anderson observed in commenting on the initial draft of this overview of the subdiscipline:

Except for Samuel Huntington, whose work does not seem to figure prominently in intellectual debate or research, few scholars have written explicitly from a neo-conservative point of view. Marxist analyses are often taken into account, but little comparative policy study generally recognized as important is written from a neo-Marxist perspective. The great enthusiasm for Scandinavian institutions and practices, the relative neglect of Southern Europe and economically conservative regimes rather gives the game away. Many have gone to Sweden in search of inspiration but there has been no similar pilgrimage of scholars to find out how Margaret Thatcher "does it." (p. 5)

Presumably most members of the subdiscipline would endorse the proclaimed objectives of a comparative approach to policy research and analysis. Basic difficulties lie, however, in achieving them. From the outset, students of comparative public policy have confronted a set of problems agonizingly familiar to all comparativists: the high personal and financial costs of research in a variety of national settings, the need for adequate linguistic and interpretive skills, the frequent absence of reliable or equivalent national data, and the difficulty of gaining access to relevant officials and offices. Even more fundamental, as Arthur Cyr and Peter de Leon have noted (1975), are problems involving contrasting conceptualizations, units of analysis, and research methods. These include the following basic differences among scholars and their approaches to comparative policy analysis:

1. *Contrasting views of society as a whole.* Most comparative policy analysis is based on a “liberal” concept of rational individual self-interest and social pluralism. Characterizing this mainstream approach to policy research is the utilization of economic or behavioral analysis to explore the policy-making process and its social consequences.⁴ A minority view, however, is represented by Ashford, Katzenstein, Pempel, and others who employ varieties of structural analyses to explain policy outcomes. In Ashford’s perspective, for example, the nature of the *state*—including its institutional arrangements and elite constraints on policy choice—plays the central role in determining policy outcomes. As Ashford observes: “The state is better described by its policies than by its principles and alleged norms of individual choice and preference” (1977, p. 572).

2. *Contrasting conceptualizations of policy.* Strongly influencing the dichotomy mentioned above are basic differences in underlying conceptualizations of the linkage between policy and politics. Consistent with their utilization of individual-pluralist units of analysis, the majority of comparativists interpret policy outcomes as the product of antecedent economic, social, or political factors. Policy, in their view, is thus a *dependent* variable, comparable to “outputs” in David Easton’s systems model of the political system. In contrast, structuralists such as Ashford (1978, 1977, 1981, 1982) and Katzenstein (1978) have followed the lead of Theodore Lowi (1964, 1972) and Robert H. Salisbury (1968) in conceptualizing policy as an *independent* variable. Accordingly, Ashford approaches the study of both the British and the French political systems from the perspective of the “politics of policy-making” (1981, 1982). Similarly, T. Alexander Smith (1969, 1977) and Francesco Kjellberg (1977) agree with Lowi that expected policy outcomes determine the choice of different conflict arenas. (Each of these authors will be discussed below.)

Less deterministic than the preceding opposing views that policy is either the cause or effect of politics is Rose’s notion of policy as process (1973, 1976). Concurring with Donald Schon (1971) that process models provide a useful means to learn from case studies and extrapolate them to other countries and new policy issues, Rose has devised an elaborate framework of analysis that is distinctive for its comprehensive scope and open-endedness. As his recommended agenda of comparative research, Rose urges policy specialists to analyze in sequence: (1) the “initial state” in a society prior to public recogni-

tion that a policy need exists; (2) how policy issues are placed on the “agenda of political controversy”; (3) how demands are advanced; (4) the importance of the form of government for policy deliberations; (5) available resources and existing constraints; (6) the move toward a policy decision; (7) the determinants of governmental choice; (8) the context of choice; (9) implementation; (10) the “production of outputs”; (11) policy evaluation; and (12) feedback.

3. *Case studies vs. comparative studies.* Traditionally, most comparative policy analysis has involved only one or at most a limited number of countries and/or policy areas. Important examples include the Heidenheimer-Heclo-Adams volumes (1975, 1983); Heclo’s earlier *Modern Social Politics in Britain and Sweden* (1974); Ashford’s *Policy and Politics in Britain* (1981) and *Policy and Politics in France* (1982); and Leon Lindberg’s edited volumes on *Stress and Contradiction in Modern Capitalism* (1975) and *The Energy Syndrome* (1977). Common to all of the case study scholars is their reliance on “qualitative” analysis which seeks—through the utilization of diverse sources including historical evidence, official documents, legislative statutes, interview data, and descriptive socioeconomic statistics—to “interrogate” (in Heclo’s apt phrase) the purposes and consequences of policy actions.

In contrast to the case study or configurative approach, other scholars have undertaken more broadly-based comparisons of a number of countries. They have typically utilized quantifiable public expenditures or similar hard data as indicators of policy choices and system performance. Prominent among them are Peters (1972, 1974, 1975, 1977, 1979), Douglas A. Hibbs (1976, 1977), and David Cameron (1978, 1982).

4. *Longitudinal vs. correlational analysis.* Among scholars who characteristically view policy as the product of economic, social, or political forces, a further distinction can be drawn between the utilization of *longitudinal vs. correlational analysis*. Anthony King (1981) has succinctly described this contrast in his assessment of the effects of electoral outcomes on policy choice. Longitudinal research, he notes, involves efforts by the scholar “to see whether changes in policy in each of the policy areas have coincided . . . with changes in the party or parties in power in the given country or countries. . . .” Correlational analysis, on the other hand, seeks to determine statistically which of various economic, political, and other factors “does the best job of explaining any variation that exists among the countries’ policies” (p. 311).

Differences in conceptualization about society, units of analysis, and methods have determined in turn the contrasting research foci and policy evaluations that characterize the empirical components of the comparative public policy literature. These contrasts are the subject of the following two sections.

DETERMINANTS OF POLICY CHOICE

Common to the contrasting paradigms of comparative analysis is an effort to explain policy outcomes in terms of at least implicit causal models of the policy process. But there is a notable lack of consensus among policy

analysts concerning the relative importance of the factors that ostensibly determine policy choices.

An early, and continuing, dispute concerns the relative importance of “economics” vs. “politics” in explaining policy outcomes. The instigators of this debate within political science were primarily students of American state politics whose innovative conceptualizations and research techniques—borrowed from orthodox economics—subsequently helped influence the emergence of comparative policy analysis as a distinct subdiscipline. Basing their conclusions on correlational analysis of the effects of various socioeconomic and political variables on policy choices, Richard E. Dawson and James A. Robinson (1963), Richard Hofferbert (1966), Thomas Dye (1966), Charles F. Cnudde and Donald J. McCrone (1969), and others argued that levels of economic development were more important than the political characteristics of individual states in determining the provision of government services within the American political system. Their findings were sharply attacked by critics such as Herbert Jacob and Michael Lipsky (1968) who faulted Dye *et al.* on both logical and empirical grounds; nonetheless, they underscore the obvious point that differences in regional and/or national wealth make possible greater or lesser degrees of public investment in education, health programs, and other social provisions.

Numerous scholars have followed the lead of the American state politics specialists in employing correlational analysis to test the relative significance of various policy determinants. But only one comparativist of note, Harold Wilensky, has corroborated the centrality of economic development for the provision of general social services. In his *The Welfare State and Equality* (1975), Wilensky reports that aggregate cross-sectional analysis of linkages between levels of national development and public expenditures on education, housing, and the military in 64 countries reveals that

economic growth and its demographic and bureaucratic outcomes are the root causes of the general emergence of the welfare state—the establishment of similar programs of social security, the increasing fraction of GNP devoted to such programs, the trend toward comprehensive coverage and similar methods of financing. (p. xiii)

Wilensky promptly amends this generalization by noting that important differences exist in the degree and organization of welfare services even among the rich nations as a group. He attributes them to “specific differences in political, social, and economic organization—by the degree of centralization of government, the shape of the stratification order and related mobility rates, the organization of the working class, and the position of the military” (p. xiv).

Most other students of comparative public policy, in contrast, assume without serious question the ascendancy of politics over economics in determining policy choices. To Anthony King (1973), for example, the most important policy determinant is ideology. His principal argument—in an article published in the early 1970s—is that *ideas*—specifically, the ideas of those who govern—have decisive political, social, and economic consequences. With reference to national American politics, King asserts categorically that

“elites, demands, interest groups, and institutions constitute neither necessary nor sufficient conditions of the American policy pattern; ideas, we contend, constitute both a necessary condition and a sufficient one” (p. 423).

Without disputing King’s central contention that politics are primary, other scholars have emphasized the various factors, either singularly or in combination, that King relegates to secondary importance after “ideas.” Some, such as John Higley (1981) and Douglas R. Boulter (1980), have focused on the pivotal role played by national political elites in setting the policy agenda and deciding policy outcomes. Important contributions to the elite approach can be found in studies of participation by high-level civil servants in national policy decisions in Mattei Dogan’s edited volume on *The Mandarins of Western Europe* (1975) and Thomas J. Anton’s *Administered Politics* (1980). Other policy analysts have concentrated on political parties and/or organized interest organizations as key instigators of policy outcomes. One example is Francis Castles, who has persuasively argued that Social Democratic electoral and organizational strength is the principal determinant of Scandinavia’s comprehensive and distinctive commitment to what he terms the “welfare state provision” (1978, 1979). My own work on Sweden and Germany substantiates Castles’ findings with the important qualification that many of the Social Democratic reform initiatives—particularly in the area of industrial relations—follow prior initiatives undertaken by the affiliated national federations of labor (Hancock, 1982).

On a more comprehensive theoretical level, several European and American scholars have sought since the mid-1970s to reformulate group theory as a means to assess new economic policy-making linkages among government, business, and labor representatives that have emerged in recent decades in a number of advanced industrial nations (notably Austria, the Netherlands, West Germany, Sweden, and Norway). Established primarily to stabilize labor-management relations in the interest of maintaining economic growth, such linkages characteristically involve policy consultations among high-level policy actors concerning wage and price stability, taxation, employment, and social services. The linkages may be either institutionalized and conducted on a regular basis, as they were following the introduction of a trilateral consultative system in 1967 in West Germany in the form of *Konzer-tierte Aktion* “concerted action”), or they may be informal and sporadic, as has been the case since the official demise of concerted action in 1977.⁵ Philippe Schmitter (1974, 1979), Gerhard Lehbruch (1979, 1982), Leo Panitch (1980), and Ulrich von Alemann and Rolf G. Heinze (1979) are among those policy analysts who see in policy deliberations among national government and interest group officials the emergence of different forms of “societal,” “liberal,” “political” and/or “economic” corporatism. Their theoretical and empirical findings—to which I shall return in the conclusion to this chapter—constitute an important ongoing paradigmatic shift within the subdiscipline.

Still other comparativists emphasize the policy significance of institutional arrangements—or, more abstractly, “the state”—as their primary unit of analysis. As the leading exponent of the structural approach to comparative policy studies, Ashford has consistently eschewed descriptive statistical or

correlational analysis in favor of critical assessments of policy outcomes in terms of their legitimizing function for the state as a whole. For Ashford, as previously noted, the organization of institutionalized power is the most important determinant of policy outcomes. He has applied this interpretation in his studies of both British and French politics (1981, 1982). A good example of a systematic comparative study of political determinants of policy outcomes is David Snyder's evaluation of the influence of organizational and political factors on strike activity in the United States, France, and Italy (1975).

A largely conventional pluralist view of policy determinants can be found in much of the comparative case study literature. A judicious use of diverse source material characterizes both Hecló's study of social politics in Britain and Sweden (1974) and the Heidenheimer-Hecló-Adams examination of social policies in seven advanced democracies (1975, 1983). In the former case, Hecló asserts categorically that

policy patterns do not flow straightforwardly from analysis of general correlations among aggregated variables. The policy process seems too delicate for that. Understanding how, or whether, politics has affected the advent of modern social policy seems to depend less on statistically unearthing and more on inductively building up generalizations from less tidy accounts. (1974, p. 12)

Accordingly, both Hecló and his collaborators selectively assess multiple factors of policy choice and development over time, with particular emphasis on the role of political parties, institutions, and bureaucracies.

A more complex version of eclecticism is Leon Lindberg's edited volume on *Stress and Contradiction in Modern Capitalism* (1975). The book's 17 chapters were commissioned to reveal contrasting Marxist, pluralist, and elitist perspectives on policy processes and system change in advanced West European and North American democracies. The result is that the book's four co-editors and other contributors variously emphasize social class, political parties, ideology, multi-national corporations, intellectual paradigms, interest groups, and even the alleged "political ascendancy of the sports metaphor in America" as determinants of policy outcomes. In his later collection of invited essays on energy policies in seven countries (1977), Lindberg and a new set of collaborators employ a much more conventional descriptive-analytical approach to comparative policy research which focuses on the decision-making role of government agencies, public and private enterprise, and organized interest groups.

POLICY OUTPUTS AND EVALUATION

The relative utility of these contrasting interpretations of the policy process is best demonstrated through an assessment of their substantive research findings and their analytical and prescriptive components. Few scholars have narrowly applied King's "primacy of ideas" thesis to explain policy choice. Instead, most comparativists have embarked on exploratory voyages of discovery designed to validate positive hypotheses or propositions concerning the policy significance of key political actors and/or the socioeconomic effects of

specific policy outputs. Various policy analysts have simultaneously utilized cross-sectional and longitudinal statistical analyses to test the “primacy of economics” argument advanced on behalf of orthodox economists by some students of American state politics. The results of these dual research strategies have been, on balance, empirically and analytically productive.

A positive result has been the compilation of a good deal of basic descriptive information concerning the scope, organization, costs, and benefits of public actions affecting health services, education, housing, incomes, taxation, and other policy areas in various countries (mainly West European and North American). This is certainly the case with the books and articles that comprise the mainstream public policy literature, including the contributions already cited by Hecló, Heidenheimer, Peters, and Cameron. A second contribution of comparative policy research is the cumulative insight that, while levels of economic development obviously matter in facilitating or restricting the provision of social services in general, *political* acts, and omissions, by policy elites play the key role in determining the content, timing, and social purpose of specific policy choices. This is the consensual theme of empirical findings by scholars employing otherwise widely divergent approaches to comparative policy analysis—among them Cameron, Castles, Lindberg, and Ashford.

At the same time, the very existence of sharply divergent conceptualizations and research foci has severely inhibited cumulative scholarship within the subdiscipline as a whole. At most one can discern cumulative trends within each of the various conceptual and methodological approaches. As articulate and prolific representatives of the mainstream approach, Hecló et al. have been able to collaborate on three occasions in refining and extending their empirical findings. In the process they have contributed a wealth of empirical data and discerning analytical insights into the policy-making process in a number of West European countries and the United States. In terms of their evaluative purpose, both Hecló's *Modern Social Politics in Britain and Sweden* (1974) and the two editions of the Heidenheimer-Hecló-Adams text on *Comparative Public Policy* (1975, 1983) embrace three of the four categories under the “policy evaluation” heading in the taxonomy presented in Figure 1 above. Singularly and jointly, the three authors are interested in exploring the short-term effects of policy decisions and omissions on the lives of citizens in the various countries in question; they address systemic consequences of the “welfare backlash” in the concluding chapter of the *Comparative Public Policy* volume; and in both the latter instance and in an edited volume by Peter Flora and Heidenheimer (1981) they utilize comparative policy analysis to analyze the evolution and performance of various welfare states in Western Europe and America. The result of their successive studies is enhanced understanding—if not of critical alternatives to established institutions and policy outcomes, then of the subtle complexities of social policy as both historical development and imperfect achievement.⁶

Scholars employing longitudinal and correlational analysis have been, by definition of their methodological approach, more rigorous and systematic in their choice and manipulation of data. An early, somewhat inclusive example—one inspired directly by the work of the American state politics specialists—

is James Bennett Hogan's comparative examination of the influence of social structure on public policy in Mexico and Canada (1972). Hogan utilized stepwise regression analysis to explore simple, partial, and multiple correlations among six independent political variables and nine dependent policy measures in the two countries. He found that political factors (especially the nature of the party system) were more important in determining policy outcomes in Canada than in Mexico, where "decisions of state policy makers [similar to those in the United States] are responsive to stages of socio-economic development. . ." (p. 502). The evaluative component of Hogan's analysis is primarily analytical; unable to determine conclusively why Canadian policy patterns differ from those in Mexico, he concludes that "more 'methodism' is not the recipe for reducing remaining unexplained variables in state politics and policy. The answer, if there is one, lies in better theory and more rigorous conceptualizations" (p. 509).

Both of these qualities characterize influential articles published later in the decade in the *American Political Science Review* by Douglas Hibbs on "Political Parties and Macroeconomic Policy" (1977) and David Cameron on "The Expansion of the Public Economy: A Comparative Analysis" (1978). In the former case, Hibbs utilized aggregate data on levels of unemployment and inflation between 1960 and 1969 in 12 industrial democracies to test the policy significance of government by left, center, and right parties. He found a positive correlation between low unemployment/high inflation and executive rule by left parties and, conversely, high unemployment/low inflation and government by center-right parties. Thus, his findings seem to confirm that political parties in fact pursue socioeconomic objectives that correspond to the policy preferences of their core constituents.⁷ With important implications for both the primacy of economics vs. politics debate cited earlier and the future efforts to devise better theories to guide comparative policy research, Hibbs concludes:

Macroeconomic outcomes, then, are not altogether endogenous to the economy, but obviously are influenced to a significant extent by long- and short-term political choices. The real winners of elections are perhaps best determined by examining the policy consequences of partisan change rather than by simply tallying the votes. (p. 1487)

Cameron's purpose is analytically broader than that of Hibbs—namely, to explain the underlying causes and consequences of the dramatic increase between 1960 and 1975 in the economic role and extractive resources of governments in 18 advanced industrial democracies. Using correlational analysis, he found that political, institutional, and international factors were significantly more important factors prompting governmental growth in North America, Japan, and a variety of West European countries than economic or fiscal explanations. Specifically, he discovered a pattern of more rapid growth of the public economy in countries with centralized rather than federal political systems as well as in countries with "open" rather than closed economies.⁸ Through regression analysis, Cameron demonstrates that "the openness of the economy is the best single predictor of the growth of public revenues relative to the economic product of a nation" (p. 1254).

While the evaluative intent of Cameron's analysis is obviously analytical, his findings—particularly those in a subsequent article on “The Limits of the Public Economy” (1982)—have powerful prescriptive implications. In the latter case, Cameron reports—on the basis of correlational analysis, this time of relations between government spending and rates of economic growth, unemployment, inflation, and capital formation in 19 industrial nations—that high levels of government spending and budget deficits have not been responsible for stagflation in Western Europe during the 1970s and early 1980s, as many fiscal conservatives have alleged. A key reason, he suggests, is that

a large and expanding welfare state may be beneficial and helpful to a capitalist economy and to the very groups that are often most critical of it. Why? Because by socializing collective bargaining to a degree and offering a social wage as an inducement for wage moderation by workers, the highly developed welfare state assists in limiting labor's share of national income, thereby increasing corporate profits and the funds available for capital investment. (p. 58)⁹

A sharply contrasting use of theory to guide comparative research is evidenced in the “policy as independent variable” approach employed by Smith (1969, 1975); Kjellberg (1977); and Peters, John C. Doughtie, and M. Kathleen McCulloch (1977). Their contributions constitute another instance of cumulative scholarship within the subdiscipline in that all five scholars have sought, albeit in different ways, to modify Lowi's typology of different policy types for purposes of comparative policy research.

Smith's approach is to define policy outcomes as “bundles of government decisions based on issues. . .” (1975, p. 1) and to examine 12 cases of policy conflicts in five Western democracies under four policy headings. The latter, each of which involves different sets of legislative and executive actors and decision processes, include: (1) distributive policies, (2) sectorally fragmented policies, (3) emotive policies, and (4) redistributive policies. The result of Smith's labors is a set of informative case histories of diverse policy outcomes ranging from decisions on agricultural subsidies in West Germany and the 1930 Smoot-Hawley Tariff Act in the United States (which he depicts as examples of distributive policies) to the nearly-revolutionary events of May 1968 in France and the struggle in West Germany over codetermination (which involve redistributive issues). Smith concedes a number of problems with his approach, including the dual difficulties of fully operationalizing Lowi's typology and “relating participants and institutions to the precise level of conflict. . .” in the empirical assessments of specific cases (p. 167). An additional problem with Smith's analysis is that his case studies are not consistently comparable in either substance or historical timeframe and therefore do not readily lend themselves to generalizations about the policy process either in the individual countries included in the volume or cross-nationally.

A more promising, if highly abbreviated, attempt to modify Lowi's typology for purposes of comparative policy analysis is Kjellberg's imaginative article on “Do Policies (Really) Determine Politics? And Eventually How?” (1977). Kjellberg is interested in basically the same question as Smith—and indeed draws on Smith's 1969 *Comparative Politics* piece as one of his concep-

tual sources—namely: “in what way does policy—the content and form of governmental activity—affect the political pattern that evolves in different situations?” (p. 555). In seeking an answer to this question, Kjellberg substantially modifies Lowi’s typology of policy types by substituting the concepts of “direct” vs. “indirect” policies for Lowi’s categories of “distributive” and “regulatory” policies, respectively. Kjellberg defines the former type of public activity as one aiming “at a direct allocation of individual or private goods. . .” (p. 562). “Indirect” policies, in contrast, are those “[r]eferring to the establishment of standards and general principles of future allocations. . . . To put it differently,” Kjellberg adds, “this kind of policy introduces a jointness of supply—according to rules based on criteria of equity or some other criteria—but does imply a jointness of consumption” (p. 562). He then wedds his reformulation of Lowi’s policy types to the fundamental distinction, derived from economic theory, between individual (private) and collective (public) goods. The linkage between different types of policies and private vs. public benefits is depicted in Figure 2, which is adapted from Kjellberg’s article (p. 562). Kjellberg posits that policy-making patterns will differ in each of the cells because the anticipated outcomes will vary.

He tests his typology on the basis of quantitative data drawn from case studies of postwar municipal Norwegian policy development affecting welfare, housing, and general planning. Unfortunately, the brevity of his reported findings does not provide a sufficient basis for fully evaluating the utility of his typology; nonetheless, they are highly suggestive. Kjellberg concludes that welfare policies have shifted from the provision of direct individual benefits (cell 1) to indirect benefits (cell 2), while housing benefits have become increasingly indirect and collective (cell 3). A parallel development has not (yet) occurred with respect to local planning, allegedly because of the reluctance by government officials and interest group spokesmen to implement national planning directives. The most striking aspect of Kjellberg’s analysis is that changes in the provision of welfare and housing benefits have altered the policy-making process itself. Two important consequences have been to “[strengthen] the bureaucratic-professional element in the decision-making process . . . and to close the process off from outside influence by moving decisions from wider political forums to committees and other restricted groups” (p. 565).

A third application of Lowi’s policy categories is the attempt by Peters, Doughtie, and McCulloch (1977) to devise a research strategy that links dif-

FIGURE 2
Kjellberg’s Typology Scheme for Public Activity

Types of Allocation	Types of Benefits Provided	
	Individual	Collective
Direct	1	3
Indirect	2	4

ferent policies and contrasting regime types. Seeking to construct a theoretical explanation of the relation between policy inputs and outcomes, they draw on both Salisbury's earlier modifications of Lowi's typology (1968, 1970) and Arend Lijphart's typology of democratic political systems (1968) to construct an integrated typology of political systems and public policies, as shown in Figure 3. The purpose of their own typology is to generate a series of hypotheses concerning the correlation between the posited mode of policy-making in each of the cells and the type of political system indicated.

Utilizing one-tailed t tests to test the significance of differences of means for each of the policy areas and types of political systems, Peters and his co-authors found that redistributive policies are indeed associated with "depoliticized" democracies (as well as with consociational systems), distributive policies with "centrifugal" systems, regulative policies with con-

FIGURE 3
An Integrated Typology of Political Systems and Public Policy^a

		Input Behavior ^b	
		Integrated	Fragmented
Decisional System	Integrated	Depoliticized Democracy: Redistribution (Ex.: Sweden)	Consociational Democracy: Regulation (Ex.: The Netherlands)
	Fragmented	Centripetal Democracy: Self-Regulation (Ex.: United Kingdom)	Centrifugal Democracy: Distribution (Ex.: France)

^aAdapted from Peters, Doughtie, and McCulloch (1977, p. 335).

^bThe distinction between "integrated" and "fragmented" forms of input behavior and decision systems refers to "the level of fragmentation of demands coming from the social and cultural environment" (p. 334). The authors follow Lijphart's lead (1968) in defining the characteristics of the different regime types as follows (pp. 330-331):

(1) *Depoliticized democracy*: coalescent elite behavior; inclusion of all major organized interest groups in the decision-making process; "low levels of politicization of important national issues."

(2) *Consociational democracy*: fragmented political culture mitigated by "coalescent or accommodate behavior of the political elites. . ."

(3) *Centripetal democracy*: relatively homogeneous political culture; active competition among political elites and organized interest groups in efforts to shape or influence public policy.

(4) *Centrifugal democracy*: "lack of agreement and integration at both the elite and the mass level"; unrestrained political conflict; strong potential for political instability and turmoil.

sociational systems, and self-regulative policies with “centripetal” democracies. They conclude from their demonstration of a positive relation between different policy outcomes and political systems that a modified version of Lowi’s typology in fact seems suitable for cross-national research.

Further applications of the Peters-Doughtie-McCulloch typology will be required to determine its full utility, as the authors themselves readily concede (p. 350). In its present form, the typology constitutes a conceptual advance over Smith’s utilization of Lowi’s policy types for comparative purposes, and can be usefully tested against the alternative approach sketched by Kjellberg. The statistical analysis employed by Peters and his colleagues in their initial evaluation, however, obscures many important aspects of the policy process—including its developmental dimensions and the interaction among identifiable institutional and group actors in the actual formation of policy.

The latter criticism hardly applies to Ashford’s structural approach to policy analysis in his *Policy and Politics in Britain* (1981) and *Policy and Politics in France* (1982). Consistent with his theoretical views concerning the primacy of the state, Ashford combines historical, cultural, and institutional analysis in crafting complex mosaics of the politics of policy-making in both countries. In each case, he examines administrative reform, economic policy, industrial relations, local and regional policies, social security, and immigration. Not a table, figure, or graph accompanies his detailed evaluation of the inhibiting effects of social and political consensus and the institutionalized pattern of adversarial relations among policy elites on effective policy outcomes, in the case of Britain, and the process of institution-building through successive regimes in France. The result of Ashford’s rich analysis—to complete the circle from the pluralist approach to his own structural paradigm—is remarkably similar to the contributions of Hecló, Heidenheimer, and other mainstream scholars: incisive descriptive-analytical understanding of the “how” and “why” of policy outcomes and their cumulative consequences for diverse patterns of historical development.

CONCLUSION: SOME IMPLICATIONS FOR FUTURE RESEARCH

The preceding assessment of different conceptualizations, research methods, and findings which comprise contemporary comparative policy analysis both confirms and qualifies Feldman’s critical view of the subdiscipline cited at the beginning of this chapter. He is correct in observing that comparative public policy lacks unity as a field. It could hardly be otherwise, given the considerable epistemological and methodological diversity within political science as a whole. But it does not follow that comparative policy analysis simultaneously lacks guiding *theories*. On the contrary, as I have sought to demonstrate, theories abound within the subdiscipline and

have profitably helped focus research and analysis within each of the designated approaches.

Comparative policy theory has been most fully developed by scholars such as Smith, Kjellberg, Peters, and Ashford who in different ways have interpreted policy as an independent variable. Pluralist scholars such as Hecló and Heidenheimer have refrained from articulating explicit theories or abstract models of the policy process but their empirical research has proved, on the whole, more impressive in both descriptive and analytical terms than many of those who have. The most sophisticated combination of theoretical and methodological rigor is found in recent correlational analyses conducted with considerable success by Hibbs and Cameron. The sum of these achievements is that comparative policy analysis has proved far more productive in generating new data, explanations of social change, and analytical insights than some critics have been willing to concede.

This is not to imply, however, that the subdiscipline is without its weaknesses or conceptual and research gaps. These, too, abound, and must be systematically addressed if comparative policy analysis is to continue to develop. Among them are the following deficiencies or omissions:

1. Much work remains to be done in the comparative analysis of public policies in the third world. Admittedly, a number of excellent configurative third world policy studies have appeared in recent years, including Susan Kaufman Purcell's penetrating analysis of *The Mexican Profit-Sharing Decision* (1975) and a succession of edited volumes dealing primarily with Latin America. Among them are David Collier's *The New Authoritarianism in Latin America* (1979), Merilee S. Grindle's *Politics and Policy Implementation in the Third World* (1980), Abraham Lowenthal's *The Peruvian Experiment* (1975), James Malloy's *Authoritarianism and Corporatism in Latin America* (1977), and Alfred Stepan's *Authoritarian Brazil* (1973). But third world policy studies—especially of a genuinely comparative nature—are an exception within the subdiscipline. Moreover, they rarely find their way into the major political science journals (as opposed to more specialized publications such as those devoted to area studies). Among the titles surveyed for this study, an overwhelming majority—approximately 70 percent—dealt with the advanced industrial democracies of Western Europe and North America. In contrast, fewer than 10 percent explored the policy process in Latin America or Asia. Systematic policy studies on the Middle East and Africa are virtually non-existent.

2. A critical omission even in the literature on North America and Western Europe is the absence of rigorous conceptual and empirical attention to policy implementation. Ashford (1978) touches on this omission in his own overview of comparative public policy—but largely as a warning: “Examining how policies are transformed into state activities can lead to the unproductive study of mechanics of policy in the old-style public administration sense. It can also produce generalizations that defy empirical treatment.” His recommended strategy for “treading this narrow path between inconsequential and the transcendent is to examine more closely how states pursue similar objectives by very different means” (p. 90).

Erwin Hargrove (1983) has made a preliminary move in the latter direc-

tion with his suggestion that policy implementation can usefully be analyzed in terms of Lowi's familiar typology of policy types. He writes:

The assumption beneath the development of [a typology of different kinds of policies] is that processes of implementation will vary according to the character of the policy being implemented. It is assumed that it is possible to classify types of policies so that the categories can be used as a basis for predicting the implementation processes within each category.

Hargrove has formulated his proposal with an eye to domestic American policy implementation but logically it could be extended to comparative implementation studies as well.

3. An additional goal of future comparative policy research should be to broaden its object of inquiry. To date, most analysts have focused on what governments do; the absence of government policy in a given area has typically been cited as a nondecision and a corresponding indicator of either lagging national development or differences in the scope of government. Yet the absence of overt government activity does not necessarily mean the absence of policy activities. So-called nondecisions may well mask a significant array of private social and economic policies by corporations, charitable organizations, unions, and other non-government actors. Such activities are eminently worthy of systematic comparative investigation.¹⁰

4. Comparative policy analysis can be broadened usefully in another crucial way as well. As indicated under the output and evaluation column in Figure 1 above, most policy studies deal with various social outcomes. More recently, however, some scholars have begun to evaluate national economic outcomes from a political perspective as well. Among them are Cameron, Lindberg, and J. Rogers Hollingsworth (1982). For future reference, promising ventures—both of which explore a comprehensive range of economic management topics in various industrial democracies—include forthcoming co-edited volumes by Lindberg and Charles Maier on *The Politics and Sociology of Global Inflation* and Peter Merkl and Haruhiro Fukui on *The Political Management of Economic Change in Japan and the Federal Republic of Germany*.¹¹ What remains as a next step in project design and comparative research is to combine the analysis of social and economic outcomes into integrated assessments of, for example, contrasting policy and institutional responses to persisting conditions of stagflation in the international economy.¹² The rapidly growing literature on varieties of corporatism in advanced industrial democracies is of considerable theoretical import in this context. More than the principal guiding theories in comparative policy analysis itself, the corporatist literature emphasizes the systemic and developmental consequences of policy linkages and trade-offs among key political and economic actors.

The attempts by Schmitter, Lehmbruch, Merkl, Fukui, Lindberg, and others to explore complex modes of institutional-group interaction in order to determine economic and social outcomes constitutes an ongoing transformation of comparative policy analysis. From their initial emphasis on social policies and patterns of public expenditures, which characterized the emergence of the subdiscipline in the 1970s, scholars in both North America

and Western Europe have become increasingly concerned in the 1980s with problems of comparative macroeconomic management. In part, as Anderson (1982) has suggested, this transformation reflects a "shifting climate of public concern" rooted in the changed international economic conditions of the 1970s and early 1980s (p. 4). However, it also reflects increasing conceptual and empirical sophistication on the part of comparativists as they delve ever more deeply into the intricate processes of policy-making with its developmental consequences in a variety of national settings.

Efforts to extend comparative policy studies to underexplored topics and new, more complex manifestations of institutional-group interaction will inevitably transform the subdiscipline into an even greater "multiform menagerie" in the years ahead. But for the full promise of comparative policy analysis—as sketched by Rose, Feldman, and others—to be realized, even more empirical research and theoretical refinements are essential before one can confidently proclaim "Enough!"

Notes

1. As comprehensive as these various classificatory schemes are, they nonetheless omit several policy areas which have been the object of recent comparative analysis such as employment policy, abortion policy, energy policy, and inflation.
2. Because of limitations of space, the attached references are restricted to the books and articles cited in this essay and some other especially relevant citations.
3. Similar arguments have been advanced by Arthur Cyr and Peter deLeon (1975) and Howard Leichter (1977).
4. This characterization of the mainstream approach is based on Douglas Ashford's earlier assessment of comparative policy studies (1977).
5. Haruo Shimada and I explore these contrasting modes of policy linkages in our chapter on "Wage Determination in Japan and West Germany: A Corporatist Perspective" in Peter Merkl and Haruhiro Fukui (Eds.), *The Political Management of Economic Change in Japan and the Federal Republic of Germany* (forthcoming).
6. A contrasting approach to the historical development of the modern welfare state can be found in Norman Furniss and Timothy Tilton's *The Case for the Welfare State: From Social Security to Social Equality* (1977). The authors depict Britain as a "social security state," Sweden as a "social welfare state," and the United States as a "positive state."
7. For critical assessments of some of Hibbs' findings, see Cameron (1982) and Nathaniel Beck (1982). Hibbs has recently extended his argument concerning the linkage between class voting and macroeconomic outcomes with particular reference to the British case (1982).
8. By the "openness" of a nation's economy to the world economy, Cameron means, inter alia, the degree to which a country is dependent on international trade to sustain productivity and growth.
9. Cameron has expanded on this theme in his chapter on "Taxes, Spending, and Deficits: Does Government Cause Inflation?" in Leon Lindberg and Charles Maier (Eds.), *The Politics of Inflation and Recession* (forthcoming).
10. Mark Nadel (1975) has previously underscored this point by observing: "Limiting the concept of public policy to government policy tends to trivialize political science in that such a narrow concept misses some of the most significant alloca-

tions of values for citizens. Furthermore, these non-governmental allocations are increasingly intertwined with the activities of formal government. To factor out only governmental outputs for research thus tends to make policy studies a heuristic exercise divorced from the real world of policy-making and policy impacts. Such a limited concept is analogous to prebehavioral political science, in which the focus was on the legal and formal institutions of government to the exclusion of much of the world of political behavior. Similarly, the challenge now is to analyze public policy—whatever its source” (p. 33).

11. The Lindberg volume, scheduled for publication in 1983 by the Brookings Institution, consists of 16 chapters arranged under three broad headings: (1) collective interests and policy outcomes; (2) states, citizens, and public choices; and (3) political economies in conflict. Country foci include the United States, Canada, West Germany, Sweden, and Japan. Merkl and Fukui conceive their comparative study of postwar Japan and West Germany as an empirical test of alternative explanations for economic outcomes in the two countries, among them: (1) the ideology of the ruling party leadership, (2) political pragmatism, (3) bureaucratic politics, (4) interest group pluralism, (5) neo-corporatism, and (6) international interdependence. Some 30 Japanese, West German, and American scholars are collaborating in the study; chapter topics range from economic and advisory systems to energy resources, public and private investment, prices and inflation, and regional and local economic change.
12. An important precedent for such an effort is Katzenstein's edited volume (1978) on the foreign economic policies of advanced industrial states. Katzenstein and seven collaborators explore the interplay between international and domestic factors of policy-making in the United States, Britain, West Germany, Italy, Japan, and France.

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11

Studying the Politics of Development and Change: The State of the Art

*Joel S. Migdal**

When Lerner (1958, p. 45) surveyed Middle Eastern societies thirty years ago, the word that came to mind as he sought to make sense of the many images he encountered was “chaos.” It is not a term most social scientists would use very comfortably in describing any sort of situation. Lerner’s initial bewilderment at the dizzying pace and scope of change, though, was not atypical, nor was his response to societies seemingly engaged in a headlong rush into confusion. As Eckstein (1982, p. 457) put it, “The development theorists tried, in essence, to find patterns in pervasive novelty and seeming flux—to get bearings in a world devoid of all fixity and precedents.” Lerner’s reaction, much like that of other social scientists, was to ferret out a pattern, a system—indeed, even to *impose* an intellectual order where social and political order could not be discerned. The term development came to denote the movement from social and political “chaos” in Africa, Asia, and Latin America towards some implicitly understood order.

From the beginning, the field of development and change was constitutive; it was the musings of scholars seeking the principles of political and social orders and the conditions initiating them. Although the study of formal constitutional process was already considered somewhat antiquated in political science by the end of the 1950s, writing on non-Western politics came to be nothing less than excursions into how societies and states might be constituted—or better yet, reconstituted.¹ The field of development, in some senses, housed the new successors to Hobbes, Montesquieu, and the other

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political philosophers who had sought constitutive measures in a similarly chaotic Europe.

The flush of excitement associated with the changes in the world map from 1947 to 1965 gripped political scientists who were seeking to untangle the debris of the old to articulate new bases of order. Few scholars agreed on any one approach to this great challenge. Pye (1965) wrote an article enumerating the many meanings the term political development had already taken in the literature. Half a decade later Huntington (1971) seemed to throw up his hands, saying that as long as there is a lack of a precise definition, the term political development can have no analytic value. All it does, he asserted, is describe some common field hoed by scholars.

For all the intellectual diversity the field spawned, there were important shared perspectives about the nature of political and social change in the Third World. But, in the late 1960s and early 1970s, serious critics attacked some of these notions, including the teleology, unidirectionality, and evolutionary determinism in the development literature.² Those criticisms initiated a new era of vitality for the field, as many, but by no means all, older perspectives were jettisoned and new approaches blossomed.

In assessing the state of the field, it is worth looking back at some of the major shared notions. A number of these ideas continue to sway, even today, interpretations of how change occurs. We will then turn to three major currents that have greatly influenced research in recent years and close with a discussion of how this new scholarship has affected our understanding of first principles.

APPROACHING THE STUDY OF DEVELOPMENT AND CHANGE

Several landmark books, appearing within a few years of one another, inaugurated the fields of development and modernization across the social sciences (Almond and Coleman, 1960; Millikan and Rostow, 1957; Rostow, 1960; Lerner, 1958). The Almond and Coleman volume placed studies of the developing areas firmly into the subject matter of comparative politics writ large. It gave birth to a highly touted nine-volume series, "Studies in Political Development," which spanned a fifteen-year period. Both the Almond and Coleman work and the subsequent series grew out of the prestigious Committee on Comparative Politics of the Social Science Research Council. Oddly, though, these works had only a muted impact on the burgeoning field in the 1960s and 1970s. Certainly, the volumes were read, discussed, and reviewed (see, for example, Montgomery, 1969). They did make political scientists take the politics imbedded in kinship systems as seriously as those found in Western parliaments. But the volumes were also much ignored. No school developed; they did not shape ongoing research. An obligatory opening footnote citing the Committee's work would be encountered in many monographs and articles, but there would be little evidence that it made a contribution to method or substance.

The work that played the role of midwife to the several branches of the field of political development was by the sociologist, Lerner (1958).⁹ All these new branches shared, in one way or another, a commitment to Lerner's "system" of modernity. The notion of a system rested on the belief that the seemingly diverse aspects of socio-political change are actually related in a pattern of high co-variation.

By the mid-1960s, many scholars accepted three key features found in the Lerner book without necessarily endorsing Lerner's central hypothesis. First, a general research focus in those years was on domestic change. The herald of such change may have been the rapid transformation of the international environment, but the internal transformations siphoned off almost all the interest of scholars. Second, at the macro-level, political scientists focused upon the creation of central institutions (the term "state" was not yet in vogue) and their ability to transform society. Third, at the micro-level they used surveys and other research tools to assess the process of individual change and its relationship to social processes, such as urbanization, industrialization, and the like. Deutsch's (1961) concept of social mobilization, with its stress on the relationship between the breakdown of personal commitments and these near-universal social processes, became the byword for interpreting aggregated individual change.

Understanding macro-change—the configuration of institutional transformations in an entire society—demanded a framework of a different order. Several such frameworks were employed often differing only in terminology. The most popular was the modern-traditional dichotomy used by Lerner (1958) and other important social scientists such as Almond and Coleman (1960), Apter (1965), Black (1966), Eisenstadt (1966), Levy (1966), and Shils (1960). Also widely employed was the metaphor of center and periphery (see, for example, Lerner, 1966, and Shils, 1975). Among the other concepts used to make fairly similar distinctions are: elite-mass (Mosca, 1939); diffracted-fused (Riggs, 1964); Great Tradition-little tradition (Redfield, 1960); and even, at times, urban-rural (Sjoberg, 1960; and Tilly, 1967). Some scholars added intermediate terms, such as transitional or prismatic, but the end-points of the continuum were the most theoretically important concepts. A formal theory of the relationship between the two sectors was never developed, but it is worth looking for a moment at some of the widely shared assumptions held by those employing such dichotomies. Although there may be variability in the size and strength of the two components of each dichotomy, the dichotomies themselves were meant to be enduring analytic tools not bound by time or space. The intent in the field of political development was to use the tool to depict a beachhead imagery; that is, in contemporary societies the locus of "development" is the modern sector or center (or elite, Great Tradition, urban areas)—the beachhead of change—and the locus of the "underdeveloped" part of society is the traditional sector or periphery (or masses, little tradition, rural areas).

The modern sector or center was seen as activist and aggressive; its authority was the motor of social and political change. The strength of the center lay in its integration, a result of the high consensus among elites sharing modern values. (The meaning of "modern values" did not, for most

authors, stray far from the instrumental side of Parsons', 1951, five pattern variables.) To political scientists, the array of public institutions—the state—should have been the most interesting component of the center. Yet, it was not until the latter 1960s and 1970s that the state qua activist organization, began to become a major subject of research (see Nettl, 1968; and Tilly, 1975c). Before then, a rather hazy image prevailed of interlocking authoritative institutions in the modern sector or center. But the importance of authority was clearly understood. The very need for authority implied a measure of resistance in the society. Values were not fully or equally shared throughout the society and did not impel everyone towards the same type of behavior; otherwise, there would have been no politics at all. Social and political control of imperfectly integrated parts underlay the use of the modern-traditional and center-periphery dichotomies.

The traditional sector or periphery consists not of those parts of the population that exercise authority, but “of those strata or sectors of the society which are the recipients of commands and of beliefs which they do not themselves create or cause to be diffused, and of those who are lower in the distribution or allocation of rewards, dignities, facilities, etc.” (Shils, 1975, p. 39). Authority expands the modern sector into the traditional. The leadership is never satisfied to live and let live but wants everyone to obey and to accept the validity of its rules of the game. Rewards and punishments exercised through the modern sector's organizations are used by the elites to facilitate the acceptance of their decisions and views.

Indifference to the structure, resiliency, and autonomy of the traditional sector or periphery marked many of the earlier major political science works on development as well as many contemporary studies—though there have been, to be sure, important exceptions (see Dahl, 1971; Huntington, 1968; Cardoso & Faletto, 1979). The periphery, while capable of change, took on in the 1950s and 1960s, and frequently continues to wear, a two-dimensional visage.

The European experience, in which nationalism has been the crucial component of the belief system and people have become increasingly attached to the larger territory in which they live, served for many writers as a model of change for the rest of the world. The Western experience, portrayed in grossly oversimplified terms, was seen as a process of change that involved the major centers' winning over minor centers and a shift from unimposing bureaucratic empires and feudal systems to modern, dynamic, effective centers. And that process was then presumed to be universal. The *direction* of development, it was assumed, is away from the primordial (biological criteria of affinity) towards attachment to the larger territory; the *form* of development is away from weak, non-intrusive centers to active, dominant centers; the *substance* of development is towards a civil society, marked by modern values and procedures. It is true, a noted author (Shils, 1975) suggested, that many states in Asia and Africa “have *not yet* become societies in a modern sense because they do *not yet* have effective centers” (p. 44). Rulers face “a population which is *not yet* formed into a society but which consists of a number of proto-societies” (p. 89, emphasis added). Even on these continents, however, the evolution seemed to be clear, and what must be overcome was the lag.

What is lacking currently, it was implied, are the key ingredients of authority and power. Huntington (1968) noted that in many cases in Asia, Africa, and Latin America “governments simply do not govern” (p. 2). “Government implies power,” wrote LaPalombara (1971) but “the most unequivocal and uncontestable statement one can make about most of the new nations today is how little power those at the center actually possess” (p. 53).

If Third World societies, in fact, lack the authoritative element, if governments really do not govern, then the field of development may have been not very different from Alice in Wonderland. The ideas developed to analyze macro-level change—modern and traditional or center and periphery—were geared to explain the impact of the authoritative sector of society on those that are the recipients of commands. The analytic lenses filtered a pattern of change in which the primary struggle was that between a relatively united, institutionally strong elite against an undifferentiated mass. But were the institutions and elites really what they appeared to be? The questions posed by political scientists of those years tended to be directed more towards what these elites and their institutions had *not yet* become, i.e., modern integrated sectors or strong centers, rather than to what they actually were. Without an authoritative center, with governments that did not govern, analysts seemed to be describing a non-existent situation. The challenge for political scientists was made all the more imposing because in much of the Third World there was an adoption of institutional forms and names from the West (states, parliaments, parties, and the like). It was seductive to assume that there *had been* a convergence of elites and values in these institutions and that the outputs would eventually be those that were expected. It was tempting to assume that a state, *any* state, was tied into other central institutions sharing with it important values and that such a state was activist and powerful.

Huntington (1968) had a clear and lasting impact on the field largely because he took institutions seriously. He looked at what the political institutions of societies actually were, not at what they had “not yet” become nor at what they formally were supposed to be. Questions concerning the real political capabilities of states, of the possibility of institutional decay or breakdown, now became central topics of concern. For political scientists, the political institutions were returned to the limelight, no longer subsumed within the broader category of center nor made the simple outgrowth of non-political events, as they were in Lerner’s work.

Huntington’s analysis was in some ways a technical one. The guiding question was what specific kinds of mechanisms maintain political stability even in the face of increased political demands—potentially destabilizing demands growing out of the near-universal process of social mobilization. The effective mechanisms, he answered, were political institutions, especially political parties: Institutions that are adaptable, complex, autonomous, and coherent. Still left to be answered, however, were the political-philosophical questions that had informed the field from the 1950s: What are the principles—rather than the mechanisms—of social and political order? Why have some societies generated effective constitutional principles and institutions while others have not? What are the processes of change involved in constituting

new orders? If modern sectors or centers are not what they are supposed to be how can we explain order and change?

In the 1970s and 1980s the means chosen to answer these macro-level questions resulted in a number of startling changes in the development field. First, the field, which usually had been defined by a residual geographic area — *non-Western, non-Communist*, neither from the first nor second worlds but from a heterogeneous Third World—was now extended into all other geographic regions, including the West. Second, in a subdiscipline that had regarded itself as *au courant*, that had concentrated on the subject of becoming modern in the post-World War II era, there was now an unexpected return to history. And such history was not simply the obligatory background preceding the “real” analysis but was a primary subject of research. Third, in a field that had restricted itself almost exclusively to domestic concerns, that had placed itself firmly under the heading of comparative politics (see Rustow, 1968), there were now new frameworks that were as much international as comparative.

THREE MAJOR RESEARCH CURRENTS

From the Third World to the First (and Second)

An irony of the first change, the spreading influence of the development field beyond the Third World into Europe and North America, is that the study of non-Western societies has been saddled with so many handicaps compared to research elsewhere. Access is frequently limited. Government statistics are often haphazard and unreliable; other base-line materials are also scarce. Sampling is problematical because of the heterogeneity of the population. Nonetheless, the intellectual excitement of social scientists observing change in that momentous era of successful independence movements infected those studying other societies as well. Kesselman (1970), for example, toyed with Huntington’s concept of institutionalization, weighing its usefulness for explaining French politics; Inglehart (1970) did much the same for Europe with Deutsch’s notion of social mobilization.

In one sense, Europe was part of the development field from the outset. The very connection of social change to development, as Nisbet (1969) makes clear, goes back to the earliest European writings. And the Western notion of development gathered special force in the theories of the late nineteenth and early twentieth centuries. When it came to giving substance to words such as “developed” or “modern” in the Third World, writers both implicitly and explicitly fell back on those patterns typical of the West. In addition, they used their understanding of the processes of change in Western history to project along which route non-Western states and societies would evolve. Not only did they assume the content of modernity but also the nature of the process—development or modernization—that would bring societies to modernity. Most interesting is how these Western cases were incorporated into development models, for it could be argued that the American and European “models” that were used bore little resemblance to the *actual* processes of

change that had taken place in the United States and Europe. It was not until the 1970s, however, that the European experience began to be examined more closely to see what kinds of distortion had crept into the models used in understanding processes of change in the Third World (e.g., Rokkan, 1973).

What some writers came to question was the utter confidence that infused the works of those who used the modern-traditional metaphor or other similar imagery. Had centers coalesced or states centralized in Western history as completely and smoothly as had been assumed? Have peripheries been as passive and malleable as has been thought? In an excellent monograph, Berger (1972) found that even as highly centralized a state as France found itself faced with a peasantry and "its imperfect insertion into the body politic" (p. 2). In France, there had arisen "corporative organizations" characterized by their efforts to regulate peasant matters fully without tying these matters into the politics of the state. The corporative organizations were able to build a reservoir of political loyalty by assuming functions important to the peasantry. They then jealously guarded this arena of conflicts and interests that lay beyond the reach of the centralized state and, as a result, they inhibited "change in the political system by withdrawing from the domain of parties and the state those issues on which alignments of interests and values are formed" (Berger, 1972, p. 168).

Berger's later work extended some of the conceptions which underlay this analysis to European cases besides France and to sectors besides the peasantry (Berger & Piore, 1980). Various segments of some European societies have continued to differ substantially from one another. These variations (or dualism) have not been "mere way stations to ultimate convergence" through the authority of centers or states. Rather "traditional" segments have endured "because of the ways in which [their] political and economic interests overlap with those of the modern sector" (Berger & Piore, 1980, pp. 3, 87). In this, European societies differ little from non-Western ones, "The evidence from both developed and developing countries suggests the persistence, not the disappearance, of the traditional or informal sector" (Berger & Piore, 1980, pp. 4, 5). As Rogowski and Wasserspring (1971) put it, even in advanced industrial societies, "nothing compels individuals . . . to become atoms, bonded only by the nexuses of cash and self-interest" (p. 44). Perhaps Europe looks more like the Third World than the Third World was thought to look like Europe.

Also reflecting the impact of Third World development literature on the understanding of the constitutive principles of European society and state is the growing corporatist literature. Although the term corporatism referred earlier in this century to the fascist states of Europe, in the post-War era it was applied almost exclusively to Iberian cultures and their offshoots, most notably in Latin America. Only after its elaboration in that context did the concept return to the advanced industrial states of West Europe, now in a much more benign form.

Corporatism for most writers meant something quite different from Berger's corporative organizations. For Berger (1972), the corporative organizations try "to acquire the power and authority to rule their own household" *outside the state* (p. 9). In the Iberian and Latin American literature

and later in the materials for other parts of Europe, the corporatist organizations are “recognized or licensed (if not created) *by the state*” (Schmitter, 1974, p. 93, emphasis added). The state has moved to center stage.

Researchers working on post-War Spain, Portugal, and Latin America resuscitated the term corporatism almost reluctantly after the ignominy it suffered by the time of the Nuremberg Trials. Even revived, it at first retained a negative taint. Corporatism, after all, was the historical antithesis of liberalism, placing the group—with its special bonds and rights—over the individual. It was thought to exclude the ambrosia of the twentieth century, industrialization and modernization. Corporatism was an atavistic survival in a world of more dynamic isms. “The Iberian and Latin American political systems have retained a mausoleum-like appearance” (Wiarda, 1976, p. 5). As Newton (1974) put it, “in the experience of the Atlantic world the corporate state is an anachronism, and a faintly tawdry one at that” (p. 35). The term corporatism came to be associated with states and societies mired in habits and institutions ill-fitted to the twentieth century. “Corporatism and the corporatist tradition,” wrote Wiarda (1977), “are a ‘natural,’ almost inherent part of the Iberic-Latin political culture” (p. 4).

By the mid-to-late 1970s, practically all these associations began to die. Rather than the antithesis of liberalism and democracy, corporatism began to appear in titles such as “Liberal Corporatism and Party Government” (Lehmbruch, 1979), “The Development of Corporatism in Liberal Democracies” (Panitch, 1979), and “Corporatism, Parliamentarism and Social Democracy” (Jessop, 1979). No longer associated exclusively with Iberic and Latin American cultures, corporatism blossomed into a tool of analysis for other parts of Europe, Japan and elsewhere. And instead of being the scourge of industrialization, corporatism has been heralded as the foundation for advanced industrial growth and adaptation (e.g., Katzenstein, forthcoming; Pempel, 1978). More and more, writers came to accept “that corporatism, like liberalism or socialism, may take a variety of forms, both as between nations and within a single nation over time” (Wiarda, 1977, p. 5). The new authoritarianism in Brazil following the coup of 1964 (Stepan, 1973), Salazar’s old Portugal (Schmitter, 1975), the Portuguese shift towards socialism after the 1974 coup (Wiarda, 1977), Japan’s corporatism without labor (Pempel & Tsunekawa, 1979), along with many other cases, all became subjects of corporatist analysis. The difficulty with the concept’s success is that without proper specification and disaggregation, it may become little more than a residual category that is used to explain nearly all state-society relationships. Such fears have already been expressed by some (e.g., Nedelmann & Meier, 1979; Pempel & Tsunekawa, 1979). As Schmitter noted (1974), “It has become such a vaguely bounded phenomenon that, like clientelism, it can be found everywhere and, hence, is nowhere very distinctive” (p. 86).

For political scientists, the return of corporatism has had a welcome by-product, the return of politics to the limelight. The field of development has been overshadowed from the beginning by economic issues and by the economists (though within the discipline of economics, development has been fairly low in status of late). At the same time, sociologists from Talcott Parsons and Edward Shils, in the Weberian tradition, to Barrington Moore and Immanuel

Wallerstein, in the Marxist mode, had dominated macro-level approaches. Corporatism identified as central to both economic change and societal structure two interrelated types of organizations that lie clearly in the domain of the political scientist.

The first type of organization is that of interest representation. Corporatist analysis thus became part of a larger body of work within political science. It stood as one kind of interest representation among the several different sorts familiar to the discipline, pluralism being the most identifiable. Schmitter (1974) played an important role in "restricting the concept, so to speak, to refer only to a specific concrete set of institutional practices or structures involving the representation (or misrepresentation) of empirically observable group interests" (p. 87).

The second type of organization within corporatist analysis that falls within the political scientist's bailiwick is the state. Corporatism was not the only concept that reintroduced the state into analyses in the 1970s and 1980s, but it did follow in Huntington's path of extricating political institutions or processes from more general social phenomena. Of the criteria that Huntington gave for assessing the capabilities of political institutions, none so grabbed the imagination of political scientists as that of autonomy. A major focus of attention came to be the autonomous or semi-autonomous state (e.g., Nordlinger, 1981; Poulantzas, 1975).

What role does corporatism demand of the state? Here, scholars divided according to their regional interests. Those writing on West Europe disentangled corporatism from its illiberal and anti-democratic past. Now corporatism—sometimes used with qualifiers such as *neo*, *liberal*, or *societal* (Offe, 1981)—became a political-economic tool or structure that coexisted with, indeed, integrated into, parliamentary government. It was viewed almost exclusively in the industrial sector (but see Keeler, 1981) as that mechanism that could assure survival and even growth in a fast-paced, open international economy. Corporatism's great advantages for advanced industrial democracies are that it promotes tranquility in industrial and political relations and adaptation in sector investment in response to changing international economic circumstances. A corporatist political structure enables the state to advance domestic tranquility, at least in the short run, by negotiating among self-seeking groups that are at odds with one another and coopting them into collaborative policy arrangements. Labor and capital, especially, participate in national economic planning and incomes policy bodies (Panitch, 1980, p. 160). As Schmitter (1981) noted, "The relative ruliness and effectiveness of the outcome is impressive" (p. 318). Corporatism furthers industrial adaptation through national planning as the state modifies "the free operation of the market by incorporating into the public decision-making apparatus those groups that are affected by the unhampered operation of the market" (Hernes & Selvik, 1981, p. 104).

Those working on Latin America stood on a different understanding of corporatism, one laced with political authoritarianism. In these cases, the state does not merely negotiate with interest organizations in order to preserve social peace nor simply license and incorporate them into the policy-making apparatus so as to insure smooth industrial adaptation. Here, regimes

incorporate, even reshape, groups to deal with the near-impossible task of maintaining political ossification, the continued operation of an antiquated elite system of rule along with class harmony, in the face of rapid industrial growth. These are states “within which populist interests and participatory politics are reduced in scope, distributional concerns ignored or placed in low priority, and the maximization of economic growth and rapid industrialization given a top priority” (Graham, 1982, p. 14). The corporatist structure does not just have the state mediate within voluntarist arrangements among existing functional groups, as in West Europe. In Latin American corporatism, the state creates these groups or, at the very least, imposes firm control over them. The tensions of social and economic change, then, have demanded substantial changes in politics as well, leading to a new sort of political system characterized by the bureaucratic-authoritarian regime.

Collier and Collier (1979, pp. 978-979) attempted to bridge the gap between European-style and Latin American-style corporatism by viewing the two types not as a dichotomy but as part of a continuum with considerable variation within each one. Their argument is that corporatism can be categorized for different societies by classifying the inducements and constraints employed by the state in respect to group representation. Nonetheless, the thrust of the literature on Latin America emphasizes the authoritarian character of the state. I will return to this literature, especially that on bureaucratic-authoritarianism, later in the essay when considering how some in the field have moved from such dichotomous paradigms as modern and traditional.

Here, it is worth noting that the revival of corporatism and the development of bureaucratic-authoritarianism in Latin America had reverberations for study far beyond Western Europe. Chirot (1980), for example, wrote an essay entitled “The Corporatist Model and Socialism.” Though the article dealt largely with the case of Romania, it did raise the point that corporatism offers the same advantage of social and political stability to socialist states driving towards rapid industrialization that it provides for those in the Third World. Corporate structures have emerged to deal with the immediate problems generated by rapid social and economic change, despite the ideal of the Communist Party to create a unitary society.

Corporatism’s application to Eastern Europe followed the spread of Third World development literature to other aspects of socialist politics. Indeed, Triska and Cocks (1977) noted “a growing need to integrate Communist studies more closely within a broader comparative politics framework” (p. xv). They focused their volume on political development, and a small but growing number of other scholars did likewise (e.g., Jowitt, 1971; Triska & Johnson, 1975; Connor, 1975; Paul, 1979). The Latin American materials also had an impact on work about socialist and non-socialist states in Asia. A workshop on the Political Economy of Taiwan run by Columbia University’s East Asian Center in 1980, for example, focused on the relevance of a model of Latin American-style authoritarianism for Taiwan. Cumings (1981) wrote an essay, “Corporatism in North Korea,” and another (forthcoming) on the relationship of politics and economics in the four industrial states of Northeast Asia: Japan, North Korea, South Korea, and Taiwan. For

the latter two, Cumings suggested dropping the recently fashionable acronym NICs (Newly Industrialized Countries) for one that includes a political dimension and reflects the similarity to Latin American cases. That acronym is BAIRs, or Bureaucratic-Authoritarian Industrializing Regimes.

All in all, the influence of development literature beyond the specific regions to which it had originally applied has been truly dramatic. The impact has come unexpectedly from the study of areas considered to be world peripheries, the non-Western world, to shape the way social scientists have viewed those regions thought of as world centers, the Socialist bloc and especially Western Europe. Cumings (1981, p. 11) noted that the entry on "corporatism" in the *International Encyclopedia of the Social Sciences*, published in 1968, read simply, "see fascism." Since then, the term has gained considerable renown transcending its earlier association with European fascism. It has been transformed from a term concerned parochially with traditional Iberian and Latin American societies to one dealing with the dynamics of change in a number of regions. The decline of static conceptions associated with "the end of ideology" and "post-industrial societies" opened the door in the West for theories and frameworks stressing social and political transformation. It was this new emphasis on change that enabled approaches dealing with the Third World to have such a telling effect elsewhere.

The Return to History

Also influencing the Europeanists' new-found interest in Third World studies was the reconsideration of European history mandated by the development literature. Through the 1950s and 1960s, notions about where changes in the Third World were heading rested upon implicit assumptions about the previous course of "development" and "modernization" in Western Europe and the United States. Beginning in the late 1960s, there were voices of dissent against some of those suppositions, paving the way for a later reorientation of the field. Huntington (1968) gave a telling critique of the use of the U.S. as a model for the Third World, and Moore's (1966) landmark study was to serve as an example of macro-level historical analysis that looked closely at state-society relations.

It was not until the 1970s, though, that a concerted reexamination of European historical development got underway. As Merkel (1977) put it, "While taking maximal advantage of available historical—especially European—scholarship, we must attempt to bridge the gulf between historiography and theories of development" (p. 463). Until then, ideas used by those studying the Third World about cohesive centers and steady centralization in Europe went largely unquestioned.

Some of the most influential books joining theories of development with European history were edited volumes (Almond, Flanagan, & Mundt, 1973; Tilly, 1975a; Grew, 1978a). Almond was the most intent on maintaining the continuity of the field, even with its new departure into Europe and into history. Matter-of-factly, he wrote,

The logic of our undertaking was elementary. As the Western nations were in some sense modern, and the non-Western ones were in almost all cases not

modern but seeking to become so, the historical experience of the modern nations had some relevance for our understanding of the problems and prospects of modernizing efforts among the new nations. . . . (p. 3)

Our search for a cure in history now took a more modest, empirically grounded, form. The logic of our inquiry was simple. Since the development that we were seeking to explain occurred in history, why not select several historical episodes, examine them in great detail, try out our varieties of developmental explanation, and see how they fit? (p. 22)

The volumes edited by Tilly and Grew demonstrated more skepticism about the ability to skip back and forth easily between development theories and actual historical circumstances and between the West's past and the Third World's present. The irony of the questioning attitude one encounters in these two books is that they are the last of the SSRC Committee on Comparative Politics' series, "Studies in Political Development." That series, after all, had canonized some of the most important notions now questioned by the Tilly and Grew books. Perhaps the doubts stemmed from the fact that Tilly is a sociologist and Grew, an historian; indeed, almost all the authors in Grew's volume are historians. The commissioning of the Grew book by the SSRC Committee in the latter part of the 1960s may have reflected not only a renewed interest in history but also an attempt to confirm the universality of the members' latest schema, the so-called "crisis" approach. Be that as it may, the results of both the Grew and Tilly volumes did little to sustain the Committee's earlier works.

Tilly departed from the Almond, Flanagan, and Mundt book in two important ways. First, Tilly (1975b) and his co-authors used the volume as an opportunity to debunk "misconceived models of Western experience as the criteria of political development" (p. 4). This reexamination of the European experience challenged some sacred assumptions about the nature of change. For example, the dichotomous model of change (e.g., center versus periphery) was brought into serious question by the finding that "the Europeans of 1500 and later did not ordinarily expand from a highly organized center into a weakly organized periphery" (p. 24). Second, the Tilly volume raised doubts about the relevance of European political change for current Third World states. At best, it argued, some broad inferences may be drawn and some generalizations made about comparative processes of state-building.

What seems to have been less obviously pursued after the appearance of the Almond, Flanagan, and Mundt volume is the quest for a universal theory of development that could explain European history as well as events in the contemporary Third World. More and more, one sees a focus on specific historical forces—whether in a single country or, as is increasingly the case, in world historical terms linking the fate of nations—in order to explain the root causes of differing types of social and political change.

There are, however, several promising paths that have been pursued seriously in the last few years to get at such causes. The distant and not-so-distant past, even outside Europe, now has become increasingly acceptable as part of the purview of political scientists; the horizons of the field have widened considerably. Excellent studies, such as those by Vital (1975, 1982)

on Zionism or Perry (1980) on China, seek to understand the political changes that have had deep ramifications in the post-War period by examining events in the nineteenth and early twentieth centuries. Dominguez (1980) took a longer look back at the insurrections that brought the end of Spanish rule in the Americas. In his introduction (pp. 1-2), he was explicit about the need for a dialogue between history and the viewpoint of contemporary political science.

Another path came directly from the questioning by some that a universal model of development could be created. As Almond (1973, p. 2) and others recognized, the concepts and classification schemes of the 1950s and 1960s were Western in character. Grew (1978b) noted, "To argue that modernization is 'a new type of "Great Tradition" ' pointing toward a worldwide civilization is at the same time to admit that the roots of the process lie in Western Europe" (p. 5). Scholars, especially in the Third World, have responded by creating alternative, indigenous models of change. These models, wrote Wiarda (1981) "represent serious and fundamental challenges to many cherished social science assumptions and understandings and even to the presumption of a universal social science of development" (p. 2). Among the works cited by Wiarda are those by Mehta (1978) and Véliz (1980). One could add Hyden's (1980) impressive work on Tanzania, among others.

The cure-in-history that Almond mentioned had an additional result linking European and Third World processes of change, even in the absence of a universal theory. The result was a revival of Gerschenkron's (1962) notions about late development found in his famous essay on economic backwardness. Gerschenkron's major premise ruled out a simple replication from country to country of historical processes—his own interest as an economist was industrialization—because the environment within which change has taken place has varied so markedly in different periods. Late developers, to achieve the same industrialization, had to apply "institutional instruments for which there was little or no counterpart in an established industrial country" (p. 7). Grew (1978b) pointed to the notion of late development as the most important link between European history and change in the Third World.

Most European nations thought of themselves as late developers; many felt they had too long been a cultural or economic colony of others (Italy, Germany, Poland); nearly all experienced the contradictions between efficiency and equality, change and stability. More than the older monarchies or pressured states to the east, the centralized republics of southern Europe . . . have had an experience comparable to that of the Third World. (p. 35)

Gerschenkron's ideas had never gone completely out of fashion. Hirschman (1968) had applied them imaginatively to Latin American cases, speaking of their "late, late" industrialization. In recent years, however, there has been an increased interest in notions of late development, still mostly in Latin America. Kurth (1979), for example, drew the analogy between European and Latin American cases. Hewlett and Weinert's (1982) volume on Brazil and Mexico focused specifically on the implications, largely political, of late development. In that book, the article by Bennett and Sharpe (1982) is the most explicit about the need to understand the peculiar role of the state as

banker and entrepreneur in terms of Gerschenkron's outlook. The late, late development in Latin America, they argued, had created problems greater in scope than those faced by the late developers in Europe and, therefore, has elicited different state responses.

One of the interesting outcomes of the cure-in-history, still largely inchoate, is a broad perspective emerging in the development literature on the nature of institutional change. The standard outlook on institutional change has been expressed best by the neoclassical economists (see North, 1981; Davis & North, 1971). Institutions are simply the established systems of rules within which people deal with one another. An institution changes, according to the neoclassical formulation, at the margins. That is, when certain parameters or environmental conditions change—the appearance of people with different abilities or of a shifting capital stock due to varying population numbers and human knowledge—there is a corresponding adjustment process in the rules.

Thus, institutions change incrementally; with each new benefit or cost accorded by varying conditions, rules for human behavior and interaction are transformed. People are willing to change the rules when the expected benefits of the new institutional arrangements outweigh the expected costs. A *system* of rules, or an institution, includes innumerable, individual prescripts, so that the system as a whole, bound by written laws as well as moral codes, will change slowly and at the margins. Long-term institutional structure and change, in North's own view (1981, pp. 201-209), derive from the tension between the benefits to be gained through organizational specialization of institutions and the costs coming from such specialization—as in setting up specialized bureaus. Nonetheless, North did not seem to deviate from the neoclassical perspective of institutional change taking place incrementally, along a fairly smooth curve, at the margins.

Implicitly, at least, it is precisely this view that has been brought into question in the return to history by political scientists. In the field of international relations, a new concern with long waves or long cycles (Modelski, 1978) drew from an earlier literature of economic thought, represented by Kondratieff (1935) and Schumpeter (1939), and from an even longer philosophical tradition in Western civilization (Nisbet, 1969, p. 211 ff). Others in international relations developed interesting notions about change in international regimes—a regime being none other than an international institution. Over time, Krasner (1982) argued, regimes face pressures that, contrary to the neoclassical vision, may build without bringing corresponding adjustments in the rules. A lag exists until the pressure is so great that there is a sudden, massive change. This image is not at all one of smooth curves and changes at the margins.

The development field, too, has begun to diverge from the neoclassical outlook in ways very similar to those set out by Krasner. Here, the word most often used to convey the inducement of sudden, massive change has been "crisis." The Social Science Research Council Committee on Comparative Politics introduced the term as a central concept in the field in 1971 with the publication of the seventh volume in the series, "Studies in Political Development" (Binder *et al.*). Crises, though, had an odd meaning in this book. They

are not necessarily “critical, episodic, political upheavals” (Binder, 1971, p. 69); rather they denote “the functional requisites of a modern or developed political system” (p. 67). In fact, Verba (1971, p. 299) preferred the term “problem area” to “crisis.” In any event, crisis implied little more in this volume than the movement from the traditional to the modern along a series of dimensions reminiscent of Parsons’ pattern variables. There seemed to be no quarrel yet with the neoclassical interpretation of institutional change. Binder (1971) wrote that the crises are likely to appear as recurrent issues that “may be coped with by the *gradual* arrangement of certain standardized patterns of response” (p. 69, emphasis added).

Though the concept of crisis has continued to be vague in meaning (Sandbrook, 1975), it has been applied in recent years in ways that make it a promising complement to the neoclassical approach. Studies on the Third World (e.g., Stepan, 1978; Migdal, 1974a, 1982), on Western Europe (e.g., Berger, 1972; Gourevitch, forthcoming; Katzenstein, forthcoming), and on Europe *and* the Third World (Skocpol, 1979) focus on the consolidation of new institutional arrangements as a result of historical crises that erupted from building pressures. In all these cases, the model of institutional change was not a continuous curve representing incremental alterations at the margins. It was, rather, an image of history as discontinuous, as bursting at rare moments with catastrophic suddenness. Some of these authors emphasize elite actions in response to environmental change. Stepan (1978), for example, wrote of

corporatism primarily as an elite response to crisis, a response that involves the attempt by elites who control the state apparatus to restructure the relationship between sectors of civil society and the state. This ‘crisis response’ explanation of the existence of corporatism competes with one in which corporatism is viewed as a function of historical continuity. (p. 47)

Others (e.g., Scott, 1976; Migdal, 1974a) are more interested in the response of non-elites, especially the peasantry, to important changes in their environment. No one has as yet culled from this material a theory or set of generalizations as formal as those in neoclassical economics. How and when new compacts emerge, creating whole classes of rules, is still shrouded in mystery. Nonetheless, the use by scholars of a still implicit, alternative interpretation of institutional change in diverse regions in itself suggests how attractive a more developed theory might prove to be.

Adding an International Perspective

“Is the traditional distinction between international relations and domestic politics dead?” Gourevitch (1978, p. 881), surveying the literature on the influence of international factors on domestic politics, opened his essay with this question. Certainly, if the mushrooming of research on the merging of these fields is any indicator, the answer is yes. Along with the two topics surveyed above—the geographical extension of development studies into areas other than the Third World and the growing importance of history—interest in the impact of international elements on domestic change has marked a

major departure for the development field.

Gourevitch (1978) noted two aspects of the international system having powerful effects on the character of domestic regimes: the distribution of power in the state system and the distribution of economic activity and wealth in the international economy. "Put more simply, political development is shaped by war and trade" (p. 883). While some very interesting material has appeared on the distribution of power (e.g., Anderson, 1974; Skocpol, 1979), the real burgeoning of interest has come in the effects of international economic influence and control on domestic structures.

Unlike practically all other streams in the development field—indeed, in the social sciences generally—the dependency literature has not been an American invention, packaged and shipped off to eager academic consumers in the Third World. Latin American scholars, following in the footsteps of economist Raul Prebisch, created dependency explanations against the current of accepted works in development. The standard writings had anchored the field securely in the waters of comparative politics: domestic, immanent factors lie at the heart of any causal explanation of systemic political change, or lack of change. Only in the 1970s and 1980s, did the ideas and concerns of the dependency theorists begin "creeping into mainstream North American social science" (Fagen, 1978, p. 287) on Latin America and, to a much more limited degree, on Africa and Asia.⁵

Evans (1979) summarized the thrust of the dependency literature:

The starting point is still relations with the external world. A dependent country is one whose development is "conditioned by the development and expansion of another economy" (Dos Santos, 1970, p. 236). Dependent countries are classically those whose histories of involvement with the international market have led them to specialize in the export of a few primary products. While the income from these few products is absolutely central to the process of accumulation in the dependent country, for the center each product represents only a tiny fraction of total imports, and can usually be obtained from several different sources. The development of the dependent country, however, requires the continued acceptance of its products in the center. Therefore, economic fluctuations in the center may have severe negative consequences for the periphery, whereas an economic crisis in the periphery offers no real threat to accumulation in the center.

Complementing and often underlying dependence based on trade relations, is dependence based on foreign ownership of the productive apparatus of the dependent country. When the principal aspect of dependence is that key sectors of the local productive apparatus are integral parts of capital that is controlled elsewhere, then accumulation in the dependent country is externally conditioned more by the "development and expansion of center-based capital" rather than by the "development and expansion of another country." The asymmetry is there nonetheless.

Dependence is then defined most simply as a situation in which the rate and direction of accumulation are externally conditioned. (pp. 26-27)

The danger of dependency is that it can become the residual variable that explains anything and everything, much the same as the role played by

the term "traditional" in earlier models of development. The temptation of making international inequality a *deus ex machina*, which explains the true causes of all poverty, can lead to neglect of the complex linkages between outside pressures and internal mechanisms. To be sure, the writing on dependency has been extremely uneven, much of it suffused with tendentious arguments. Frank (1967), for example, went beyond existing notions about dualistic societies in seeing underdeveloped sectors as modern creation ("the development of underdevelopment") and as products of international relationships. At the same time, however, he squeezed Latin American events and anomalies into his framework and, even more so in his subsequent book (1981), mercilessly bent worldwide cases to the needs of his theory.

A body of research and theory has appeared under the dependency heading, nonetheless, that has been very provocative. Cardoso's (1973) work on associated-dependent development, as a case in point, took account of the phenomenal industrial growth, accompanied by immense foreign investment, in countries such as Brazil. The restructuring of politics after the 1964 coup in Brazil must be understood, he argued, as an outgrowth of the new economic realities there and their relationship to external capital. Evans (1979) took up much the same subject in an insightful analysis, trying to untie the three strands of a Gordian knot, the multinational corporations, local private entrepreneurs, and state-owned enterprises. In a somewhat confusing book, Cardoso and Faletto (1979) made a valiant effort to tackle the difficult problem of analytically incorporating different domestic structures in Latin American societies as more than the mere playthings of international forces. The political struggles in a society, they stressed, must be weighed alongside political-economic structures of domination, both internal and external. Although an overwhelming share of the literature has concerned First World-Third World relations, several pieces have begun to explore the dependent role of some Socialist countries (e.g., Reitsma, 1982).

Probably, the most valuable contribution of the dependency literature was that it put the question of the change of social and political institutions into a world capitalist context. "The dependency framework, in other words, explicitly rejects the unified state as actor as a useful conceptual building block of theory" (Caporaso, 1978, p. 2). Many of the works in the 1960s and early 1970s were not concerned with what model might replace the one based on states-as-building-blocks. On the whole, scholars simply assumed a bilateral relationship between the U.S. and/or American-based multinational corporations, on the one hand, and dependent Latin American societies, on the other.

In the mid-1970s, the notion of a world system did offer an alternative to the perspective of single societies or states as individual building blocks. The paradigmatic shift, as some have called it, owed much not only to dependency literature but also to work in the field of international relations. An important volume edited by Keohane and Nye (1970) marked a shift away from seeing international relations solely as the interaction of independent states—the billiard ball model or what they termed the state-centric view of world affairs. They built on another, neglected tradition, that of transnational relations. Here, interactions are as likely to be peaceful as conflictual and lead to sorts

of interdependence in a single transnational society undreamed of in the state-centric model.

The new literature on international regimes has been only one product of the paradigmatic shift. Modelski (1979) drew specifically on the transnational literature and on the “cure in history” to develop an approach to international relations based on the idea of the modern world system (Modelski, 1981). A world system perspective in the field of development and change also borrowed from other disciplines, such as Wolf’s (1969) anthropological work dealing with the effects of nineteenth century capitalism on peasant societies. Most important of all, however, was the flowering of world system theory in sociology, especially in the work of Wallerstein (1974, 1980), but in that of others, as well (e.g., Chirot, 1976, 1977; Meyer & Hannan, 1979; Kaplan, 1978; Goldfrank, 1979; Skocpol, 1979). Their macrosociological views built on the point raised by the dependency theorists: that is, it is misleading to assume that once external “factors impinge on a society, the main consequences occur through the internal structural processes that maintain the coherence of the society as a bounded system” (Meyer & Hannan, 1979, p. 3). They rejected

the intellectual tradition [that] emphasizes the treatment of societies as real units of analysis. . . . This is clearly naive. The economies, states, and cultural systems of almost all national societies are historical creatures of the European political economy. . . . Further, the current evolution of most national societies is greatly affected by the economic, political, and cultural events which occur entirely outside their boundaries. Economic developments in Africa, the Near East, or Latin America are clearly resultants, for better or for worse, of dominant world markets and technologies. Similarly, political events in such areas (e.g., the Nigerian civil war, the creation of an independent Angola) are also creations of the world system. (Meyer & Hannan, 1979, pp. 11-12)

The advance by world system theorists beyond the dependency framework is illustrated by the term “system” itself. It denotes an entity, other than a single society, within which there is an established set of rules for human interaction, what some have referred to as an ongoing division of labor. The term “world” signifies that the area of such an entity extends beyond individual societies or culture-groups; it does not mean necessarily that such a system blankets the entire globe. Most world systems historically have been world empires, such as the Roman Empire. One world system, the one we live in, has had no unifying political structure but has maintained its rules or division of labor through market exchanges (capitalism); it is a world economy. This modern world system “flourished, expanded to cover the entire earth (and thereby eliminated all remaining mini-systems and world empires), and brought about a technological and ecological ‘explosion’ in the use of natural resources” (Wallerstein, 1976, p. 349).

World systems have beginnings and all besides ours have had ends. The momentary events and relationships in any society can be understood only in terms of the life history of the system as a whole. This premise of the world system theorists has dovetailed with the return to history by many political

scientists to produce a growing trend counter to the cross-sectional analyses so common in comparative politics.

The level-of-analysis problem has bedevilled political scientists for decades, and the world system approach has only complicated matters by rejecting all the levels commonly used by comparativists. It is not surprising, then, that political scientists have shown rather cautious interest in the new theories. Wallerstein's economic determinism and his relegation of the state to secondary status have created concern. Uneasiness has also surfaced since Wallerstein's theory neglects any independent role for domestic institutions and culture in processes of change. Nonetheless, despite these and other reservations, a number of political scientists have begun to use the world system rather than single states or societies, as their point of departure (e.g., Modelski, 1978; Zolberg, 1982). It is likely that in the coming decade the level-of-analysis debate will intensify and that the old distinctions between comparative politics and international relations will continue to erode. The world system field is now a growth industry with its own journal (*Review*), an institute headed by Wallerstein (the Fernand Braudel Center), a series of annuals, and more. With this sort of infrastructural support, one can expect a paradigmatic battle, which may bring new vigor into the field.

BACK TO FIRST PRINCIPLES

For close to a decade, the field of development and change has undergone a rejuvenation, overcoming the inertia brought on by sterile definitional battles and, later, by the loss of faith in the U.S. as the model of the first new nation. Scholarship has been freed from the Procrustean notions that the future of the Third World is faithfully reflected in the mirror of Western history. Has the new vitality in the field also led toward some acceptance of new constitutive principles? Have the recent forays into the uncharted waters of history, new geographic regions, and international politics challenged the assumptions of the earlier theorists and their dichotomous models about the nature of order and the causes of change? The answer to both questions seems to be a qualified yes: qualified because so many of the ramifications coming from the new work are still inchoate and because important connections to past assumptions do survive.

The issue of authority has been at the heart of the field's concerns. Those relying on center-periphery or other such dichotomies to comprehend the twists and turns of political changes made suppositions about the source and direction of authority. They assumed flows from a society's central elites and their integrated institutions to the rest of the population, which was often seen as rather undifferentiated. Dependency and world system theorists departed from an essential tenet of that premise, the integrity of the society itself as a basis for interpreting the source and direction of authority. They construed the most important movements of authority to be those across national boundaries. In world system analysis, the flows lie in the unequal exchanges constantly reproducing the worldwide division of labor, enforced when necessary by strong states. Order and change depend not primarily on the mix of values, institutions, and elites in a society but on the niche of that society in a

much larger configuration. The values, the institutions, and the elites reflect a world pattern and act to ensure in their small corner, at least, the conditions necessary for the constant reproduction of the world division of labor.

Ironically, world system theory, inspired by an even earlier borrowing of terms by Prebisch and others (United Nations, 1950), used the same center-periphery metaphor. Now, however, the center, or core as some call it, signifies the portion of the world that appropriates the lion's share of world surplus through the international market, with the added backing of strong state mechanisms. The periphery includes the politically weakest entities, where populations are organized to produce commodities needed by the center. Like earlier models the internationalized version construes change in largely dichotomous terms (though Wallerstein adds an intermediate category, the semiperiphery). The international model treats the periphery in much the same way as the earlier conception: the periphery is passive and malleable in the face of a strong, integrated center. The center is the repository of authority.

Even for those who have accepted the shift away from a single society to transnational society, the conditions of order and change in an individual society still are created at that society's center—now because of the local center's ties to the world core. Political scientists from both sides of the paradigmatic divide have given special attention to the state as the storehouse of authority. As Anderson (1967) put it, "There is a predisposition to see state action as a primary factor in the process of economic development" (p. 3) and, one could add, in countless other processes as well. "Many contemporary notions about development," Anderson added, "seem to posit government as a kind of 'omnipotent given' that could if it would set matters right" (p. 5).

Oddly, this state-centered view runs directly counter to a perspective that has been described as maintaining "a pervasive grip upon citizens, journalists and scholars alike" (Nordlinger, 1981, p. 1). This alternative outlook depicts authority flowing from society to state, not vice versa. Nordlinger calls this view society centered. It includes both pluralism and Marxism, which portray state officials as subject to the desires, even control, of interest groups or social classes embedded in civil society. One might account for the contradictions between state-centered and society-centered perspectives by pointing to the state-centered portrayals largely as ones of non-Western countries while the society-centered descriptions are of democratic, industrial countries. Even so, it might also be noted that the pendulum for scholars studying the West has also swung towards a state-centered interpretation, emphasizing the autonomy, or at least the relative autonomy, of the state (e.g., Nordlinger, 1981; O'Connor, 1973).

In the Third World literature, bureaucratic-authoritarian interpretations have, if anything, strengthened Anderson's observation of fifteen years ago; the state remains at the foundation of many causal theories of order and change. This conclusion is not as true of much of the empirical on-site literature, which often has stressed the disarray in many states. But, in the general theories, civil society has most often been presented as the clay in the hands of the potter, the state. The premises of the dichotomies in which strong, integrated centers mold weak, diffuse peripheries, seem as popular as ever. The

most influential of the writers on bureaucratic-authoritarianism has been O'Donnell (1979). He contended that increasing industrialization in Brazil and Argentina activated the popular sector inducing more rigidity by other sectors and, eventually, unregulated conflict. These tensions combined with weakening political institutions to create a crisis alleviated only by a "coup coalition" that excluded the popular sector and established a bureaucratic-authoritarian regime. The new military-led governments adopted "a technocratic, bureaucratic approach to policy making (as opposed to a more 'political' approach through which policies are shaped by economic and political demands from different sectors of society, expressed through such channels as elections, legislatures, political parties, and labor unions)" (Collier, 1979, p. 4). In the bureaucratic-authoritarian literature, the state looms larger than before, more shielded from societal pressures and inputs (see O'Donnell, 1979). And, in this respect, this literature has blended in effortlessly with the corporatist works discussed earlier. In both sorts of studies, the state "is characterized by strong and relatively autonomous governmental structures that seek to *impose* on the society a system of interest representation based on *enforced* limited pluralism" (Malloy, 1977, p. 4, emphasis added).

It remains highly questionable whether states are indeed so formidable. Indeed, one questions whether corporatist and bureaucratic-authoritarian theories portray accurately the roots of change and the maintenance of order. Like the nineteenth century German philosophers, such as Fichte and Hegel, who saw the state as the guide to social transformation, these theories assume all too glibly the endless capacity of the state to shape society. Some recent and some not so recent literature has implied that such state-centered perspectives may veil important elements of change and order. These same works, however, would not give heart to theorists using pluralist, Marxist, or other society-centered outlooks mentioned by Nordlinger (1981). What may be hinted at in these other writings is a new understanding of the causes of change and the nature of constitutive principles, rejecting the notion of the state or center as the sole repository of authority and also spurning the traditional society-centered approaches of the comparative politics field.

Grindle (1977) conducted a fascinating study of public policy in a state noted for its bureaucratic-authoritarianism and corporatism, Mexico. The results of the state's efforts in creating and pursuing a new rural development policy were less than glowing. At every step in the policy process formidable obstacles appeared, thwarting the intent of central policymakers. Grindle noted one community worker's response:

"Going out and meeting with peasants can be a dangerous business in Mexico. It threatens a lot of people." In some remote areas, the *caciques* [local bosses] were considered to be an unassailable force, even by the party. In one state, the reaction of such local influentials had caused the governor to request that CONASUPO [the state agency] completely remove the Field Coordination Program from his state. (p. 160)

Influence or authority here does not appear to be the exclusive domain of the state at all. This conclusion echoes a point made by Anderson (1967) a decade earlier. In his research on Latin America, he found "limited govern-

ment, limited not so much by constitutional arrangements, as that term is conventionally used, as by the resources which government has at its disposal, and the relations between its role and function in the social order and that of other institutions" (p. 5). Drawing heavily on African cases, another work portrayed political conditions not greatly different from those described by Anderson for Latin America. Heeger (1974) depicted the "chronically weak [political] institutions which hinge on fragile bargaining relationships between elites in the center *and* the periphery" (p. 9, emphasis added). He was deeply critical of studies on political consolidation, which

mistook the organizational aspirations of the political elites for reality and posited a cohesion that simply was not there. . . . Elite-mass consolidation was viewed as being comprehensible in terms of but a single successful linkage between center and periphery—charisma, the party, etc. To put this in another way, where such linkages were seen to exist, the relationships they were seen as establishing were given more coherence than they possessed in reality. The actual multiplicity of such links and their possible contradiction were either obscured or ignored altogether. (p. 49)

One article addressed directly the corporatist literature on Latin America and its premise of states capable of molding societies. The consensual order at the center so important to dichotomous models of change, Hammergren (1977) suggested, is largely absent in Latin America, but corporatist writings persist in picturing "the convergence of political power at the center and top of corporations" (p. 448). Hammergren went on to note, "The tradition of local caudillos, caciques, or *gamonales*, especially in more isolated areas of the country, the presence of regional elites even in more developed areas, the maintenance of economic ties between internal and external groups with minimal participation by the state, all point to a very limited penetration of society by the national center" (p. 449).

All this should not lead one to dismiss the state too quickly. These criticisms do not necessarily lead us back willy-nilly to society-centered perspectives in which the state is little more than a stage for playing out of conflicts among the factions and segments of society. The state is limited, to use Anderson's term, but it is surely not dead. It has tremendous resources available from international sources in the form of foreign aid, direct investment of foreign capital, and international loans as well as, in some cases, political-military support. Internally, its ability to mobilize resources, to regulate society, indeed even to reshape society may be substantial in specific sectors, regions, or policy-areas. Theorists of corporatism and bureaucratic-authoritarianism rightly highlight the state's "elimination of a whole network of intermediary groups and actors" (Hammergren, 1977, p. 456).

It is an exaggeration of many cases to say governments do not govern (Huntington, 1968), as it is misleading to dismiss the Third World simply as consisting of proto-societies or of no societies at all. These are societies with distributions of authority that do not fit existing state-centered or society-centered models very well. And, one may add, if the authors quoted above—Tilly on the limits of European centers in history and Berger and Piore on the bounds of state authority in Europe today—are right, then these models are

equally misleading for some cases outside the Third World. What we need to know is how to describe and assess these distributions of authority and how to understand the changes that brought them about. What caused different distributions of authority in different societies? In which areas can states use their extraordinary powers and in which areas may they be all but helpless against other authoritative bodies? If the state is not all-powerful, if elites in the periphery may at times predominate, what becomes of the dichotomous models of change and order? Where do we turn for constitutive principles?

Anderson (1967) wrote that the state "is not synonymous with the social order, but is one institution among the many that make up organized human life. Government has an impact on other social institutions and in turn is affected by them" (p. 5). Social organizations of all shapes scattered throughout society may be repositories of authority. In many countries, varied groups have managed to maintain rules of behavior and ways of life in direct conflict with the codes of the state or the norms of the center. They have held onto their ways with leech-like tenacity despite the greater resources and the substantial determination of state leaders, and despite the international norms that have mandated these leaders' active role in fostering social change. Berger and Piore (1980) described the European situation: "Various segments of society organize around different rules, processes, and institutions that produce different systems of incentives and disincentives to which individuals respond" (p. 2).

States are in conflict with a heterogeneous flock of other social organizations that do not share the rules of the state. Whether and to what degree states can successfully triumph in their conflict with such organizations varies. These variations are rooted in the different ways world historical forces have been played out. Incisive studies on the United States and Europe treat states, not as "omnipotent givens," but as variable in their ability to effect social policy and reshape society (Krasner, 1978, p. 57; Katzenstein, 1978b). The specific types of order and change in a society are the outcomes of the struggles over the rules of the game among social organizations, including the organization that is usually the weightiest of all though not always strong enough to end the struggle altogether—the state. How that struggle has developed and how it will proceed depend not only on domestic factors but on important historical and contemporary actions and alliances originating in the larger world system.

NOTES

1. Very few authors acknowledged their connection or debt to the earlier constitutional writers. An exception came in one of the very best books written on Africa, that of Kilson (1966).
2. The most notable rejection of the idea of patterned change is found in Whitaker (1967). Among other serious, critical articles are Tipps (1973), Schwartz (1972), Gusfield (1967), Migdal (1974b), Skinner (1971), and Shiner (1975).
3. In Lerner's concept of empathy, we find the basis for the "psycho-cultural school," which has included such notable scholars as McClelland (1961) and Inkeles (1974); in Lerner's stress on communications, we find the beginnings of the "diffusion of innovation" literature, with important works by Pool (1963) and Rogers (1969); and

in Lerner's focus on the city as an engine for change in an entire country, we find the roots of the growing literature on urbanization and broad structural change in the Third World (e.g., McGee, 1971; Qadeer, 1974). In addition, Lerner's method, his use of formal, cross-national survey of such huge dimensions, paved the way for other major studies of this sort. Some prime examples include Almond and Verba (1963); Liebman, Walker, and Glazer (1972); Inkeles and Smith (1974); and Verba, Nie, and Kim (1978).

4. Although they take on somewhat different meanings for different writers and are most often left wholly undefined, the concepts of center and periphery have been accepted by a surprisingly broad spectrum of those writing of macro-sociological and political change.
5. It is interesting to note that dependency literature introduced the first serious challenge by Marxist and Neo-Marxist works of the dominant approaches in the U.S. to development and change. For a review of this literature and its relationship to modernization literature in the U.S., see Higgott (1978).

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