

COMPARATIVE POLITICS AT THE CROSSROADS

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Estudio/Working Paper 1991/27
September 1991

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“COMPARATIVE POLITICS AT THE CROSSROADS”

Comparative politics is both a subject matter and a method of analysis. In principle, the two should complement each other; in practice, they frequently do not. As a subject, comparative politics is the special field of teaching and research within the discipline of political science that is customarily devoted to “the politics of other countries or peoples.” At least in the United States, its position in academe has usually been marginal, flanked on the one side by American Politics (which are implicitly treated as incomparable) and, on the other, by International Relations (which are explicitly regarded as more consequential). Despite the number and variety of “other countries and peoples,” vastly more political scientists are employed to study the domestic politics and foreign relations of the United States. Of all the American doctorates granted in this discipline in 1990, only 24% were devoted to comparative politics. Of the total membership of the American Political Science Association, only 7% listed themselves under the rubric, “comparative politics” (although another 9% claimed the status of “foreign area specialist”).

The situation elsewhere used not to be very different. However, in recent years, teaching and research involving comparisons among European countries has increased significantly, and there are encouraging signs that a similar trend is emerging within Latin America and parts of Asia. For example, it has become almost inconceivable that a German, Italian, Dutch or Spaniard would seriously attempt to understand his or her politics without at least some reference to the politics of neighboring states -- if only because of the high levels of interdependence and joint policy-making embedded in the European Community. Scholars

from France and the United Kingdom may still be convinced, along with the Americans, of the uniqueness of their brand of politics, but that too may be waning. Only in these countries could one imagine that an introductory course in politics would refer just to the country in which it was being given.

As a **method**, comparative politics involves an analytical effort to exploit the similarities and differences between political units as a basis for developing “grounded theory,” testing hypotheses, inferring causality and producing reliable generalizations. As John Stuart Mill (1843) observed some 150 years ago, the practical difficulty of applying experimental techniques to political matters makes comparison of observed variance across “natural” units the most feasible -- if still the second-best -- technique for developing scientific knowledge in this field. Recourse to it is as old as political inquiry itself. Plato and, especially, Aristotle not only made systematic use of it, but developed core concepts, typologies and hypotheses that are still of great utility. As we shall see, the “fashion for comparison” has waxed and waned over the subsequent centuries, but the “urge to compare” never completely disappeared.

If and when **the subject and the method** are successfully blended to produce reliable and cumulative generalizations across a wide range of settings, then comparative politics would cease being an exotic sub-field and become virtually synonymous with the scientific study of politics. American Politics would provide material for just another case; International Relations would enrich our understanding of the broader contexts in which the cases are located. All systematic political inquiry -- except for those few areas appropriate for controlled experimentation -- would be explicitly or implicitly comparative.

Despite considerable effort by a distinguished group of scholars over the past four decades, such a synthesis is still on the distant horizon. As we shall see, there are some formidable obstacles to be overcome.

THE DEPENDENCE UPON THEORY AND CONCEPTS

“Comparativists” bear a double burden. Their topic demands that they produce useful information about foreign polities, but their method requires that they develop and apply analytical categories that are equivalent across the units they are examining. “Theory” is indispensable for establishing this comparability, but its tendency to rely on general and abstract concepts interferes with easy comprehension by non-specialists and often generates understandings that are far removed from the one that the actors themselves use to describe and justify what they are doing.

Some original practitioners of the sub-discipline could afford to ignore this difficult issue. By confining their attention to those Euro-American polities with a common politico-cultural heritage and similar range of socio-economic development, they could unselfconsciously rely upon everyday labels and assumptions. James Bryce (1921-27), A. Lawrence Lowell (1896) and Woodrow Wilson (1889) are examples of learned scholars around the turn of the century who felt no need for elaborate and explicit conceptualization in going about their task of describing the **institutions** and **norms** of political life in Great Britain, the United States and “the Continental Powers.” To the extent that they had a theory, it was rooted in the comfortable assumption that eventually all countries would evolve towards a similar set of democratic practices.

The Impact of Events

World War One, the Russian Revolution and, especially, the rise of Fascist and National-Socialist regimes during the 1920s and 1930s radically shattered this assumption of benevolent convergence. The range of variation within Europe in institutions, behaviors and justifications increased -- rather than decreased -- with dramatic and tragic consequences. Just when, after World War Two, the Western European polities seemed to be returning to a common evolutionary path, the worldwide process of decolonization and attendant struggles for national independence introduced new sources of diversity into the field. The excitement that accompanied these changes -- plus the policy needs for information coming from the U. S. government and the financial incentives for research coming from major foundations -- produced a significant shift in the attention of comparativists away from Europe toward the “communizing nations” of the Second World and, especially, the “developing nations” of the Third World. Coping with this extraordinary increase in the number of units and the diversity of their situations demanded a major effort in explicit and elaborate theory-building. Much of this focused on the concept of “political development” and the search for “functional equivalences” beneath the bewildering variety of institutions, practices, norms and beliefs.

The ‘Golden Age’

During the 1960s and 1970s, comparative politics became the major locus of theory-building within the discipline as a whole and its innovations subsequently affected the more established fields of American Politics and International Relations. Faced with explaining the “constitution” of the Soviet Union, “elections” in Albania, “parliaments” in Ghana, “professional associations” in Libya, “ministerial selection” in Sri Lanka, “federalism” in

Argentina – not to mention “civil-military relations” in Ecuador or “budgeting” in Zaire, one could not simply apply the usual labels without serious risk of distortion. The similarity in rhetoric hid an obvious absence of analogous behavior, intent or consequence. But what could replace the old comfortable rubriques? What makes a concept “transcultural” -- hence, “transportable” across polities of otherwise great diversity (Sartori, 1970).

The answer to these questions led postwar comparativists to rely increasingly on general theory. Their first reaction was to postulate a universalistic characteristic of all polities, namely, their tendency to form **systems** whose components were interdependent and homeostatic. Then, they defined the system components not as institutions but as **functions**, i.e. generic tasks that must be performed if the postulated equilibrium was to be reached and sustained. As these initially diverse systems were affected by a generic similar process: **modernization** (i.e. economic development, urbanization, secularization, literacy, industrialization, rationalization, bureaucratization, and so forth), the result would be **political development** (Almond and Coleman, 1960; Pye, ed., 1963; Apter, 1965; Pye and Verba, eds., 1965; Almond and Powell, 1966; Binder, ed., 1971).

With these fundamental notions at hand (taken largely from social anthropology and biology, often via the sociology of Talcott Parsons), they courageously sallied forth not just to explain what was going on in an extraordinary diversity of settings, but also to create a new “universalistic” understanding of what politics was all about and why political development was bound to occur.

The results of this ambitious venture were mixed. Quantities of new, and often very useful, data were gathered about “exotic” places. Novel (or long forgotten) aspects of state-society relations such as clientelism and patrimonialism were opened up to inquiry. Sensitivity certainly grew with regard to the variety of ways in which political business could be conducted

and institutions could be exploited. The exclusive emphasis upon comparisons between “advanced polities” was irrevocably altered. Nevertheless, already by the early 1970s, serious doubts about the paradigm began to arise and by the 1980s the field had lost its unity of purpose. Complaints accumulated that the systemic functionalism was excessively abstract, weak in providing specific and researchable hypotheses, and incapable of orienting the collection of empirical data. Moreover, its Americo-centric aspects became increasingly apparent. Equilibrium did not always set in. Functional tasks were not so nicely differentiated or performed so complementarily. Political systems on the “periphery” proved to be less autonomous and more subject to dependency and domination effects. Assumptions about the coherence and identity of “national” societies and their corresponding polities turned out to be overoptimistic. Traditional cultures were more varied and resilient than anticipated. They did not give way so easily to the “imperatives” of modernization. Autocracy rather than democracy became the more probable outcome. Instead of the expected political development, something much more akin to political decay emerged in much of the Third World - and in the Second World of Communism a very different pattern of domination installed and consolidated itself.

The Reaction of Area Specialists

Meanwhile, the “American” promotion of comparative politics had spread to Europe. Practitioners there also tended to chaff at the limitations and assumptions of systemic functionalism, but for different reasons. European polities may have been manifestly more self-equilibrating than those of the Third World (after an initial decade of postwar uncertainty in some countries); however, the interdependence of components and, hence, the configuration of institutions often differed markedly from the paradigm case of the United States. Contrary to the

prevalent view from America, it was not specialized performance of functions, overlapping cleavage patterns, broadly aggregative parties and limited state interventions that could account for the relatively high degree of order that emerged. Also for Europeans, the flagrantly “ahistorical” nature of the general paradigm contradicted the abundant evidence for the persistent role of different historical residues, sequences and trajectories in the region.

Individual European scholars began during the 1960s to elaborate **alternative models** of political order and political change. Basically, they took the case they knew best (usually the one in which they lived and taught), summarized it and generalized its characteristics to other settings (Daalder, 1987). Stein Rokkan (1970, 1975) used his native Norway to produce an elaborate theory of historical cleavages and center-periphery relations; Giovanni Sartori (1976) analyzed the party system of Italy to challenge the American assumption of centrist-centripetal tendencies by showing how polarized-centrifugal patterns of competition could emerge; Gerhard Lehmbruch (1967) and Leonard Neidhart (1970) came up with models of Proporz- or Pluralitare-demokratie to explain the special features of Swiss and Austrian politics; just as Arend Lijphart (1967), Hans Daalder (1966) and Val Lorwin (1971) exploited the cases of the Netherlands and Belgium to show how “segmented” rather than “overlapping pluralism” could produce stability through a consociational form of democracy. While all the above were (at least initially) intended only to make sense out of the peculiar characteristics of continental European politics, Juan Linz (1964) abstracted the characteristics what was a markedly deviant case in this context, Spain, and came up with the definition of a distinctive type, the authoritarian regime, that very quickly was picked up by scholars working on other parts of the world. Joseph Lapalombara’s appropriation of parentela and clientela from the Italian political jargon to explain certain peculiarities of that country’s interest politics enjoyed a similar, if less successful, fate (1964).

For Europeanists were not the only ones who resisted the universalistic appeal of systemic functionalism. From the start, specialists on such foreign areas as Latin America, Africa, Asia and the Middle East regarded the pretensions of comparative politics with considerable skepticism. Their domain of expertise was being invaded by outsiders (some of whom, it must be admitted, did possess detailed knowledge of specific cultures, languages and histories). The concepts being thrown at “their” exotic societies seemed excessively abstract, insufficiently informative and hardly value neutral. Not only were they biased toward the rational utilitarianism of Western societies, but they also seemed to justify various forms of policy intervention -- not to say, imperialist manipulation -- by the already developed powers that were sponsoring and consuming comparative research.

In addition to the general, culturally- and historically-based objections mentioned above, scholars working on and/or coming from these Third World countries began to develop alternative concepts and theories. The most significant of these focused on the international context within which politics was being conducted -- particularly the unequal exchange between **central and peripheral economies** (Prebisch, 1963; Frank, 1967), due to the differential historical development of a single **world capitalist system** (Wallerstein, 1974, 1980; Zolberg, 1981 for a critique). In a broader sense, it was argued that the conditions of **late development** (Gerschenkron, 1962; Hirschmann, 1958, 1963, 1968) and **dependency** upon external sources of demand, investment capital, ownership of enterprises, elite values and models of mass consumption altered the basic parameters of policy choice and led to different political outcomes (Cardoso and Faletto, 1979; Evans, 1979). Guillermo O’Donnell (1973) drew the conclusion that under these conditions of delayed, dependent development through import substitution the most likely result would not be democracy, but protracted **bureaucratic authoritarian rule**. This

thesis produced a great deal of critical discussion (Collier, ed., 1979) and had an impact far beyond the South American context from which it originated. A related conceptual innovation was the notion that **corporatism** not **pluralism** was the most probable response in the realm of interest associations to such differences in patterns of development (Schmitter, 1971; 1974). This idea was very quickly diffused across national, regional and cultural boundaries and, suitably reformulated, led to a major revision in the way that Europeans conceptualized their interest politics (Schmitter and Lehmbruch, eds., 1979; Lehmbruch and Schmitter, eds., 1982).

One group of area specialists paid little or no attention to all this ferment and controversy: those who worked on the Soviet Union, Eastern Europe and China. With very few exceptions, they easily agreed that what was going on their bailiwicks was incomparable and required a different approach. **Totalitarianism** both provided an overarching concept and served to justify a unique set of methods of observation and inference that could cope with its secretive and sinister nature (Friedrich, 1954; Friedrich and Brzezinski, 1956). The static and isolated character of this sub-field left its practitioners quite unprepared for explaining the remarkable (and unanticipated) changes that emerged during the late 1980s, and they have since been busy trying to join their long-lost comparativist brethren.

THE PROLIFERATION OF METHODS, LEVELS AND DESIGNS

If this dispersion of critiques and innovations were not enough, the sub-discipline of comparative politics has also been buffeted in recent decades by a proliferation of methods, levels and designs. The original comparativists were scholars (all DWEMs: Dead, White,

European Men -- but not Boring) who worked alone (although it is rumored that Aristotle employed a lot of assistants in his (since lost) research on the 158 constitutions of the Greek Polis), used mainly publicly available information about the public leaders and institutions of sovereign states in multiple settings (often mixed with a good deal of “itinerant wandering” and “participant observation”), and drew their conclusions based on prevailing standards of logic and inference (even if, occasionally, reaching unusual and counterintuitive conclusions). They made extensive use of typologies as simplifying devices to establish equivalences and differences. For example, Aristotle first separated Greek city-states into three descriptive categories: rule by one person, rule by a few persons and rule by all citizens and, then, further divided them normatively into good and corrupt versions of each. Machiavelli (1532b) relied heavily on the dichotomy between republican and princely government; Montesquieu (1748) on a trichotomy of republics, monarchies and despotisms. Tocqueville (1835-40) broadened the focus to include private as well as public institutions, masterfully exploited the contrast between aristocratic and democratic societies and offered the field a new telos, the ineluctable drive towards equality. Since these promising beginnings, the classification systems have multiplied and the thematic foci have shifted from one putative goal or end-state to another -- without producing much in the way of accumulated wisdom or conceptual convergence.

As mentioned above, comparativists have always supplemented “official” sources of information with a good deal of data-gathering on their own. Through travel, personal experience in politics and access to primary sources, as well as the reading of history, they have attempted to get beyond constitutional forms and legal categories to get at what “really” determines the similarities and differences. Field research abroad for an extended period is still an obligatory rite de passage for all its practitioners.

The Behavioral Revolution

What is distinctive about recent practice is the reliance on new forms of data, new means of compilation and new techniques of analysis. Public opinion polling developed in the study of American politics and soon spread to comparative politics. The first efforts were relatively crude efforts to find out “if foreigners think like us” (e.g. Buchanan and Cantril, 1953), but with the spread of survey research facilities across the world and increasing sophistication in the conceptualization and translation of items, it became possible to design and execute multi-national projects based on the attitudes of individuals. The Civic Culture (Almond and Verba, 1963; also Almond and Verba, eds., 1980) was a particularly ambitious, “landmark” study that analyzed the opinions of mass publics in Great Britain, Italy, the Federal Republic and Mexico. Since then, the volume of research on “comparative political behavior” has grown almost exponentially. Research is now conducted routinely on such items as voting preference, electoral turnout, citizen tolerance and subjective competence, confidence in institutions, modes of participation, salience of class and other bases of cleavage, party identification, difference in elite-mass values, etc. in a wide range of polities, and the results have been gathered and summarized in important collective volumes, e.g. Lipset and Rokkan, eds., 1967; Allardt and Rokkan, eds., 1969; Rose, ed., 1974; Budge *et al.* 1976; Verba, Nie and Kim, 1978; Budge and Fairlie, 1982. It has even proven to be possible to study comparatively the emergence of new, allegedly post-materialist, values (Inglehart, 1977) and citizens' dispositions for engaging in “unconventional” political actions (Barnes, Kaase *et al.*, 1979). Within the European Community, mass surveys covering all 12 member states are routinely conducted and reported in its Eurobarometre publications. Sizeable archives of these **attitudinal data** been compiled, such as the one at the Inter-University Consortium for Political and Social Research at the University of Michigan,

which allow researchers easy and inexpensive access to original data for the purpose of secondary analysis. For Europeans, the Newsletter of the Data Information Service of the European Consortium for Political Research in Bergen provides a convenient reference for keeping up with sources.

The Appearance of Aggregate Data

But the explosion of sources and archives has not been confined to surveys. With the independent development of national accounting systems and statistical services -- and the growing availability of computers -- scholars began to collect and manipulate large quantities of **aggregate data**. International organizations contributed to the standardization of many of these measures of national performance and comparativists were soon adding indicators of their own. The publication of this information in "handbooks" that covered virtually the entire universe of national states added a further stimulus (Banks and Textor, 1963; Russett *et al.*, 1964; Taylor and Jodice, 1983). By now, machine readable data sets on a wide range of subjects are available from commercial sources, and more will be coming on CD-ROM disks.

The impact of this explosion of aggregate data has been at least as profound as that of survey data. It has permitted researchers for the first time to examine the full range of variance, "the universe," on a given subject. Previously, time, expense and human limitations restricted the number of cases that a comparativist could deal with. These compilations have allowed analysts to bring the powerful tools of statistical estimation to bear and to incorporate many variables simultaneously in their analyses. The first efforts focused on relative simple correlations such as that between "social mobilization and political development" (Deutsch, 1961) and "economic development and democracy" (Lipset, 1959). Subsequently, the scope of inquiry

widened and the complexity of the models increased to tackle issues such as the political determinants of mass violence (Hibbs, 1973); industrial conflict (Hibbs, 1976); welfare policy (Wilensky, 1975); growth of state expenditures (Cameron, 1978); social equality (Jackman, 1975); “regime governability” (Schmitter, 1981); rebellion (Gurr, 1970). The burgeoning field of **comparative public policy** owes a great deal to the stimulus of these new data sets and statistical techniques (Ashford, ed., 1978; Rose, ed., 1976).

But these “gifts” have not been unmitigated blessings for comparativists. Much of the work ignored elementary problems of conceptual equivalence across units (not to mention, empirical validity in the data); depended upon crude inductive methods and ex post theorizing; neglected to measure concepts with adequate indicators; relied on static cross-sections and ignored time-dependent, dynamic relations; used statistical tools that were inappropriate for testing the postulated relationships; and failed to distinguish between correlation and causality. Nevertheless, the quality of both data and inference has improved consistently over time, and new techniques of statistical estimation are being introduced which should correct many of the analytical errors (Achen, 1983; Ragin, 1987).

The Problem with Units

Both survey and aggregate data initially reinforced the already firmly entrenched tendency to use the national state as the exclusive unit of analysis for most comparative purposes. This was unfortunate for three reasons. (1) Most newly independent states had much less coherence and autonomy than the established states of Europe, North America and the “White Commonwealth.” This violated a basic assumption of the comparative method that the units should be equivalent in their capacity to act with regard to the properties being examined.

(2) All comparisons are jeopardized by Galton's problem (Naroll, 1965): the possibility that the observations of a given variable are not "really" independent, but caused by an underlying process of diffusion. With the "trade, investment and communications revolution" that emerged during the postwar period, especially within certain, highly interdependent, regional contexts such as Western Europe, national states effectively lost their sovereign control over many policy processes. (3) The figures generated as national frequencies or averages often masked very substantial differences between unevenly developed regions and sectors within these countries. At times, this internal variation could be greater than that between the polities being compared. The national datum became an artifact of a non-existent unit.

Growing sensitivity to these issues has resulted in several modifications of the method. Universal "samples" have been abandoned in favor of smaller and more homogeneous sub-sets of units. Some of these are geo-cultural areas, but many are based on new analytical categories that cut across these regions: "advanced industrial countries," "newly industrialized countries" and, perhaps, if the proper euphemism can be found, "really backward countries." Another trend is to exploit the infra-national variation by comparing systematically the conditions and performances of municipios, departements, provincias, regioni, Laender, estados autonómicos, Soviet republics, etc. This approach began with U.S. states (e.g. Dye, 1966) and cities (e.g. Clark, 1974), but now covers a wide range of polities where sub-units have some significant degree of autonomy (e.g. Tarrow, 1977; Tarrow, Katzenstein and Graziano, eds., 1978; Sharpe, ed., 1979; Rowat, ed., 1980; Sharpe, 1981; Ashford, 1982; Rhodes and Wright, eds., 1987). Another strategy has been to focus on the meso-level of economic sectors (Hollingsworth, Streeck and Schmitter, forthcoming). There is even an embryonic literature dealing comparatively with the emergent properties of supra-national authorities (Lindberg and

Scheingold, 1970) and international organizations (Haas, 1964; Schmitter, 1971).

The Choice of Design

The classic format was for a scholar to study two or more units explicitly selected for their mix of common and contrasting features. Michels (1911), Ostrogorski (1910), Duverger (1954), Lipset (1960); Alford (1964), Zolberg (1967), Putnam (1973), Hecllo (1974), Inkeles and Smith (1974), Katzenstein (1984), Skocpol (1979) are good examples of this strategy of research. Much of the production of comparativists, however, consists of “single country monographs” in which only one unit is ostensibly analyzed. The most famous and enduring of these is Tocqueville’s De la Democratie en Amerique (1835-40). What accounts for its comparative status is both the way in which it is conceptualized, e.g. as a contrast between two types of societies, the democratic and the aristocratic, and the consistent, if often implicit, contrast which is made with an “absent” case, i.e. France. This case-study method has been used extensively in the postwar period, especially for dissertation research given the limited means at the disposition of young scholars. Not only have these monographs contributed heuristically to the eventual development of “grounded theory,” examined critically “deviant cases,” and served as “proving grounds” for new techniques (Eckstein, 1975), but they have also been replicated in other settings. It is important to remember that comparison is not just an event, but a process. As such, it can involve the same scholar subsequently extending his coverage to other cases or other scholars picking up the original conceptualization and operationalization and applying them again. Apter (1955), Eckstein (1963), Weiner (1962), Lapalombara (1964), Lijphart (1968), Berger, (1972), Stepan (1971, 1978), Schmitter (1971), Scott (1976), Evans (1979), Popkin, (1979), Pempel

(1982), Bates (1981), O'Donnell (1988) are examples of single country monographs that have become part of the comparative corpus.

The advent of aggregate data and, to a lesser extent, survey data encouraged a shift from “small N” strategies of single or paired cases to “large N” strategies which might even include the total relevant universe. Comparativists found themselves analyzing units whose language, culture and history -- even whose location -- they did not know, simply because they were available in some UNESCO or ILO data series! When the issues of incommensurability and unintelligibility became more salient, the enthusiasm for this design declined rapidly and researchers reverted to working on selected subsets of units where they were more familiar with the quality of the data and where they could bring more variables into the analysis -- especially, the effects of historical time, sequence and memory.

Much intelligent discussion has focused on whether it is preferable, when comparing a small N of cases, to use a “most similar systems” design in which as many variables as possible could (putatively) be held constant allowing only those under surveillance to vary -- say, to study the impact of revenue windfalls upon the party system by comparing Norway and Sweden -- or a “most different system” design in which the effect of the same variable is traced by comparing two systems which otherwise have as little in common as possible -- say, the impact of a petroleum boom on Norway and Nigeria (Karl, forthcoming). Both have their generic advantages and disadvantages (Przeworski and Teune, 1970; Lijphart, 1975) and the choice hinges largely on whether one is seeking to maximize the specificity or the generalizability of one's findings.

One major limitation upon the design of comparative research in the past has been definitively broken. Most studies were carried out by a single scholar, almost invariably an American or a European, working in relative isolation. With the increase in resources and the diffusion of competence in the social and political sciences around the globe, it has become possible to put together teams of scholars working on different countries from different disciplines. Originally, these were staffed, funded, conceptualized and carried out almost exclusively by American academics -- the Committee on Comparative Politics of the Social Science Research Council was the prototype -- but this is now less likely to be the case. Most of the recently edited and multi-authored volumes cited in this essay were produced by collaborative efforts which were international in every aspect. Moreover, around the ECPR in Europe and the FLACSO/CLACSO in Latin America, important new concentrations have formed to promote comparative research.

The State of the Art

It has been alleged that the personality of scholars tends to resemble the characteristics of the unit or units they study. If this were the case, most comparativists would risk schizophrenia as they are caught between the conflicting demands for providing specific and accurate information and searching for reliable and verifiable generalizations. Instead, they seem to have avoided such a collective pathology by vacillating over time between the two objectives -- although individual practitioners have occasionally proven that it is possible to satisfy both simultaneously.

Their response to the postwar demand for a universalistic and relevant "science of politics" was initially enthusiastic and, then, increasingly skeptical. To the excesses of systemic functionalism, they reacted by stressing the specificities of culture, geographic

location, economic exploitation and social structure, while seeking to avoid excessive reliance on the idiosyncracies of each case. The need for “history” (Tilly, 1984), “thick description” (Geertz, 1973) and “bringing the state back in” (Skocpol, 1984; Evans, Rueschemeyer and Skocpol, 1985) were the slogans bandied about. Of course, some eminent students of comparative politics had long been speaking this sort of prose without knowing or proclaiming it. The works of Seymour Martin Lipset (1960), Barrington Moore, Jr. (1966), Reinhard Bendix (1964, 1978), Gaston Rimlinger (1971), Kenneth Dyson (1980), and Perry Anderson (1984) contain all three elements. Even the more “mainstream” practitioners associated with development theory responded to the critiques. Samuel P. Huntington consistently stressed the longer-term cultural and institutional aspects of political change and arrived at much less sanguine conclusions about the likely outcome (1968, 1971). The SSRC Committee on Comparative Politics in its later years turned in its attention back toward Europe and recuperated a more historical perspective (Grew, ed., 1978; Tilly, ed., 1975; also Almond, Flanagan and Mundt, 1973).

But comparative politics has not merely returned to its point of departure. Along the torturous route, it picked up new concerns, new concepts and a lot of new converts. It may have momentarily lost a clear sense of direction, but there are definite signs of movement, even of enthusiasm, among its contemporary practitioners. Few would question, however, that the sub-discipline is at an important crossroads.

Three challenges are looming on the horizon for comparativists: one to their theoretical foundations; one to their basic units of analysis; one to their subject matter.

(1) The current **fashion for rational choice and game theoretic explanations** raises the specter of a possible return to universalistic premises -- based this time not on unconscious

adjustment and functional equilibration at the macro-level, but upon stable solutions worked out through repeated exchanges between individual actors at the micro-level. If completely successful, this approach would not only convert entire departments of political science into dependencies of orthodox liberal economics, but it would also wipe out the accumulated stock of comparativists' assumptions about the significance of cultural, institutional and obligational factors. History would be reduced to the passage of time and the iteration of exchanges; institutions would be contingent upon continuous calculation; preferences would be given rather than socially constructed; maximizing self-interest would be the only admissible norm.

(2) **Unprecedented increases in interdependence** -- through trade, investment, production, diffusion of images and tastes, spread of international regimes and obligations, etc. -- have greatly eroded the autonomy (not to mention, the sovereignty) of the national states that have, so far, provided the sub-discipline with its principle units of observation and analysis. Galton's dilemma has run wild. In such a globalized context, no polity can choose and act independently. Will comparativists be able to shift their bases of inference, as well as their units of inquiry? Or will they be confined in the future to analyzing intertemporal and regional variations of a single "world system"?

(3) **The wave of democratization** that has swept across the world since 1974 offers to comparativists the attractive prospect of, once again, being able to focus on a common topic. They looked on with dismay at the "praetorian politics" (Huntington, 1968) and "breakdown of democracy" (Linz and Stepan, 1978) in the 1960s and early 1970s; now, they are turning their attention to the more heartening processes of "transition from authoritarian rule" and "consolidation of democracy" (O'Donnell, Schmitter and Whitehead, 1986). Whether this will trigger a resurgence in theoretical speculation about evolutionary convergence, or a greater

speculation about evolutionary convergence, or a greater conceptual sophistication about differences in the types of democracy that are emerging remains to be seen. If the past is any guide, two things are certain: comparativists will divide into “generalizers” and “specifiers” in response to these issues (Tilly, 1984); and (2) the debate between them will contribute to keeping their sub-discipline lively, controversial and consequential.

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