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A Perspective on Comparative Politics, Past and Present

Harry Eckstein

Source: David Apter and Harry Eckstein (eds), *Comparative Politics: A Reader* (1963): 3–32.

The field of comparative politics has a long and honorable past. That its pedigree reaches back as far as Aristotle is not unusual, since just about every discipline can, in one way or another, trace its origin to Aristotle. Comparative politics, however, has a particular right to claim Aristotle as an ancestor because of the primacy that he assigned to politics among the sciences and because the problems he raised and the methods he used are similar to those still current in political studies. From Aristotle stretches an impressive line of other Greats who can be numbered, without too much distortion, among the ancestors of the field: Cicero, Polybius, and Tacitus among the Romans; Machiavelli, among others, in the Renaissance; Montesquieu in the Enlightenment; and an imposing line of sages in the nineteenth century – Tocqueville, Marx, Mill, Bagehot, Mosca, and many more. The general analysis of political systems, the classification of their types, the study of the forms of their development, and the observation of the many varieties of actual political systems are concerns nearly as old as the history of recorded thought. These concerns have at least as time-honored a place in human thought as the concern with political morality.

Yet specialists in comparative politics seem today to be preoccupied, almost paradoxically, with questions we associate not with the maturity but with the infancy of a field of inquiry – questions about the fundamentals, the “first things,” that govern the processes and ends of analysis. Such questions are raised only rarely in disciplines that have a highly developed tradition. If we are to understand the present state of comparative politics, we must know what these questions are and why they are being raised at this particular stage in the field’s development.

The Present State of Comparative Politics

Let us begin with the questions.

First of all, a host of procedural – perhaps one should say methodological and epistemological – questions are raised by contemporary students of comparative politics. What, they ask, is the nature of comparative method: how is it used, and what sorts of studies are not comparative? What can be learned by comparisons, assuming that we know how to make them properly? Is the comparative method in the social sciences, for example, really an adequate substitute for experimentation in the natural sciences, as has sometimes been claimed? Can it be used at all in a field like political science – that is, are political systems really comparable – or is each system unique, so that each particular political system is best dealt with by configurative rather than comparative analysis, by constructing a special *Gestalt*, a “profile” as Heckscher has called it? Even if this conclusion is not necessary, does not the comparative method operate usefully only within certain limits: at a relatively low level of theoretical abstraction, where analysis is not very broad in scale but confined, at most, to limited periods in time, certain geographic areas, or similar types of political structure? And what do we mean by concepts like uniqueness, abstraction, similarity? Not only are questions raised about such basic issues, but also about the proper use of specific devices for comparative analysis: for example, the proper uses of ideal and real types, sampling methods, scaling techniques, and so on.

A second set of questions concerns the use of concepts in the field. These fall primarily into two categories: questions regarding the classification of political systems and questions regarding the elements of such systems.

In political science a bewildering variety of classificatory schemes is available. The most venerable of these schemes is still much in use and was never discarded (only amended, simplified, elaborated) from the time of the Greeks to the nineteenth century. It classifies political systems according to the number of participants in decision-making processes into monarchies, aristocracies, and democracies. Since the middle of the nineteenth century, however, schemes for classifying political systems have multiplied helter-skelter, every man his own taxonomist. Today an almost embarrassing number of such schemes exist, requiring choices we do not really know how to make.

Some writers on politics use schemes consisting of two basic types, not, as in the classic case, of three. Some of these two-term schemes consist of polar types, limiting a continuum along which actual systems may be ranged, while others simply provide two “boxes” into which actual systems are placed. An example of the “box” approach is the classification of systems, now widely used, into Western and non-Western types. The continuum approach is found in a large number of schemes – for example, the division of political systems into constitutional and totalitarian, traditional and modern, or agricultural and industrial types.

Some students of politics choose instead classificatory schemes consisting of three basic terms. Weber, for example, classifies political systems, according to the legitimations of authority dominant in them, into traditional, rational-legal, and charismatic types. The Marxists classify them, according to the dominant economic class, into feudal, bourgeois, and proletarian systems. Coleman, departing less from the classic typology, characterizes them either as competitive, semicompetitive, or authoritarian. Dahl uses the terms democracy, hierarchy, and bargaining systems. (The last are added, presumably, to accommodate his interest in economic systems in which not “decisions” but, so to speak, mutual ‘accommodations’ are arrived at.) Still another series of writers uses four-term schemes. Apter, for example, labels governments as dictatorial, oligarchical, indirectly representational, and directly representational. Almond once constructed a scheme typifying political systems, obviously on a variety of bases, as Anglo-American, Continental European, totalitarian, and preindustrial.

We can find in the literature schemes even more complicated than these. Edward Shils, for example, has recommended a five-term typology, constructed specifically to deal with the analysis of “new” states: political democracies, tutelary democracies, modernizing oligarchies, totalitarian oligarchies, and traditional oligarchies. Coleman, in another classificatory proposal, has gone Shils one better by dropping one of his categories (totalitarian oligarchy, which, presumably, would not be omitted in the analysis of established as well as new states) and adding two others, “terminal colonial democracy” and “colonial or racial oligarchy.” And this is only a partial list, a sample.

We have here a considerable *embarras de richesses*. We can explain why it exists and why it should have come into existence after the middle of the nineteenth century, particularly in very recent times, for this variety of classifications is obviously a reflection of the rapid development of modern social theory and the broadening of the range of materials in the social sciences. The important point, however, is that such a disconcerting wealth of classificatory schemes inevitably raises some fundamental questions: Which scheme is more useful than others for any given purpose and, even more basic, what is the use of any classificatory schemes at all? How ought such schemes properly to be constructed, and how can one distinguish, in principle, a good scheme from a bad one?

The same questions arise in regard to the elements of political systems, taking “elements” to mean the parts into which such systems are divided and out of combinations of which they are, for analytical purposes, constituted. Following early modern usage, we used to think of these elements primarily as three: legislative, executive, and judicial structures and functions; but lately a large variety of alternatives have been proposed and used. Apter, for example, thinks of political systems as consisting primarily of government, political groups, and systems of social stratification. The last he considers the aspect of the social setting most directly and most significantly related

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to politics. Each of these elements is then further divided and subdivided to arrive at a large series of components of politics, certain of which supposedly “cluster” in typical (frequently found) political systems. Governments, for example, are held to have a certain “format” and to depend for their very existence on five structural requisites: authoritative decision making, accountability and consent, coercion and punishment, resource determination and allocation, and political recruitment and role assignment. Lasswell presents a breakdown of political systems on the basis of seven functional variables (and explicitly because of his dissatisfaction with the classic separation-of-powers formula): intelligence, recommendation, prescription, invocation, application, appraisal, and termination. (The meanings of these anything but self-explanatory terms are immaterial to the present purpose.) Easton suggests that political systems have essentially two elements – inputs (demands and supports) and outputs (authoritative decisions) – while Almond provides a complicated breakdown of both inputs and outputs into seven so-called functional categories: four for the “input” function (political socialization and recruitment, interest articulation, interest aggregation, and political communication) and three for the “output” function (rule making, rule application, and rule adjudication – the classic formula, but restricted to only one aspect of political systems).

These also are only examples to which a good many others might be added, but they will suffice to illustrate the many different grounds on which a breakdown of political systems might be based – structural categories, functional categories, structural-functional categories, system requisites, elements of formal organization, elements of informal processes. They also show why questions should nowadays be in the air regarding the most basic aspects of such analytical breakdowns: their relative utility, their purpose as such, the “logic” – if there is any – of their construction.

To some extent, the answers to such questions depend on how one answers certain other basic questions that are also very much in the air these days. Is it, for example, more fruitful to treat political systems as autonomous systems or as systems embedded in other aspects of society? If we want to link politics with its larger setting, what aspect of that setting should we stress? Does social stratification really have the explanatory power Apter claims for it, so that we can safely dispense with the examination of other elements of setting? Or are the most significant links to be made with levels of economic development (as Lipset suggests), culture (as Beer implies), or personality (as political psychologists like Lasswell appear to argue)? Even more important for the way we break down and classify our data – and indeed also for our methods – what is a political system? What really is our subject matter? Is it “states” – governmental units possessing sovereignty – as the most venerable view in political science has it – or is it any power relationship (Catlin), any influence relationship (Lasswell), any system that allocates social values? And if the last, are we really interested in *any* such allocation or only, as Easton maintains, in *authoritative* allocations? That is to say, does government, in the traditional sense, remain

our focus, or do we act upon the recommendations of a whole host of men, from Catlin to March, and examine in the construction of a truly general and comparative political science almost any interpersonal relationship, whether conventionally thought of as political or not?

Once we have dealt with such questions, a host of others, equally basic, remain. For example, what unit of analysis should we use in political studies? Should we use impersonal units, such as “roles” (clusters of expected behavior patterns revolving about a particular function) or “interactions” (acts and the responses they engender)? Or should we use a personal unit – that is, concrete individuals? Or superpersonal units, such as groups, institutions, or organizations (taking these in the specialized senses in which they are used in modern sociology)? What “perspectives” or “orientations” should we use in analyzing these units? Should we still emphasize, as we have traditionally emphasized, the study of formal constitutional structure or apply instead group theory, structural-functional analysis, the decision-making approach, communications theory – to mention only a few of the possible analytical approaches available to us? And what sort of “theories” do we want to construct through these approaches: empirical “laws,” models, causal explanations, functional analyses, equilibrium theories, developmental theories, or still other, as yet unexplored, types of theories?

These questions – about methods, concepts, definition of the field and its elements and boundaries, units of analysis, analytical approaches, and types of theories – will be recognized immediately as the most important metatheoretical and pretheoretical problems arising in any field of inquiry. (“Metatheoretical” refers to theory about theory – methodology, for example. “Pretheoretical” refers to operations that must be performed before the construction of theory proper – that is, before the formulation of testable hypotheses and their testing.) I have listed them here, not because I have any intention of answering them or resolving disputes about them, but solely because they can immediately tell us something important about the present condition of comparative politics.

In most fields of inquiry, such questions, despite their obvious importance, are either not raised at all or are raised only by the way and by men, like the members of the Vienna Circle, who have a special taste for philosophy and fundamentals – and men who are usually more influential outside their fields than in them. Why then are they raised so much in comparative politics today? After all, students of politics, as I have stated, have had many centuries to reach settled conclusions about them. What is more, preoccupation with such questions, fundamental though they are (indeed just because they are fundamental), probably hinders more than it promotes substantive research. In a way such preoccupation involves a kind of vicarious experience of research. What then can explain the apparent paradox between the venerable age of the field and the infantile questions raised in it?

The answer is both simple and important. Some historians of science tell us that, despite the myth of steady scientific progress that we have inherited

from the Enlightenment, the advance of science has not really been steady. Instead, it has been punctuated by revolutionary intervals in which the whole framework of scientific knowledge – all its basic, usually unspoken, assumptions – has come under heated debate: assumptions about the proper purpose of inquiry, about the nature of its subject matter, about what constitutes satisfactory scientific knowledge. Science always functions within a framework of such preconceptions, but the preconceptions are never opened to examination when a consensus on them exists; men do not argue questions upon which they are agreed. In such cases of consensus one may indeed get the impression of a steady unfolding of a shared perspective upon scientific work. When consensus breaks down, however – when a field is marked by dissent or is in transition from one framework of inquiry to another – the fundamentals always come to the forefront; the silent major premises cease to be silent. In such periods, if the breakdown of scientific consensus is broad enough, intensive philosophical exploration of a general sort occurs. If the breakdown is restricted to a narrow field, its practitioners will engage in metatheoretical and pre-theoretical labors that, to others, may seem exotic and unrewarding, if not irrelevant to actual scientific work.

From this we can infer what is perhaps most basic about comparative politics today: that it is a field acutely in dissent because it is in transition from one style of analysis to another. For just this reason, it is a field in which many different styles of analysis are at present to be found. Because this is the case, we cannot give any simple account of comparative politics. Instead, to portray the character of the field today we must do three things: provide an historical account of its development, explain how it reached its present state of dissension, and expound the principal discontents and aspirations of its contemporary practitioners.

The Origins of Comparative Politics

Periodization is always hazardous. Nevertheless, we can locate the beginnings of the modern study of comparative politics with fair precision at that point in time when political systems came to be conceived not as natural bodies (“corporations”) but as artifacts, created by people and therefore subject to re-creation (reform) by people. In short, its earliest source, leaving the classics aside, is Renaissance political thought, most obviously that of Machiavelli; and it comes to its first full fruition in the Enlightenment, above all in the writings of Montesquieu.

Machiavelli and the Renaissance

When Burckhardt says that in the Renaissance the state came to be regarded as “a work of art,” he does not mean that it was looked upon as something aesthetically pleasing; nor does he mean that political actions were

considered to be self-justifying, like artistic creations, rather than subject, like works of morality, to ethical codes; and he certainly does not mean that politics was not regarded as a proper subject for scientific analysis. He means precisely what he says – that the state had come to be regarded as an artifact, something that was made rather than something that simply was; for just that reason, it came to be looked upon in the Renaissance and Enlightenment as a proper subject *in itself* for “reflection and calculation.”

We can, of course, reflect on the behavior of natural objects that are only imperfectly subject to human control or not subject to it at all; the natural sciences do almost nothing else. But the more *unalterably* given we regard phenomena to be (that is, the less susceptible to human engineering), the more likely we are to be intellectually passive in regard to them, to dismiss “scientific” inquiry as futile or as an esoteric taste, or to subsume study of the phenomena to the larger contemplation of “being as such” – to metaphysics, ontology, or theology. It is no accident, therefore, that the study of politics through the broad-scale examination of political experience comes to the forefront just when we begin to talk about an “art” of governing and of “statecraft.” From this standpoint we can also understand why comparative inquiries, conducted to establish generalizations about political behavior and not merely to illustrate them, came first to be carried on when natural law doctrines were on the wane. If one really believes in a natural law that rigidly governs all human relations, then one is likely either to look for it through abstract speculation upon first principles or, even more likely, through very narrow studies of experience, since any limited range of experience – a single government, for example – will then illuminate as much as very broad ranges of experience – and the analysis of very broad ranges of experience is the hallmark of genuinely comparative studies. The point of view most hospitable to such studies is one that sees social life as governed by necessary relations, knowledge of which can be used in controlling, at least to some extent, human affairs.

In the Renaissance this point of view emerged, although art was emphasized far more than nature, and this emphasis is important. If one studies a subject primarily because one believes in the necessity of human engineering in the area it comprehends, inquiry into it is bound to be of a particular kind. Inevitably it will focus upon the discovery of techniques through which such engineering can be effectively carried out: upon *Staats-kunst*, not *Staatswissenschaft*. Machiavelli himself is the primary example. What makes a ruler successful? How can power be won, maintained, expanded? What arrangements and practices make a state powerful, stable, free, prosperous? These are the quintessential problems of the political technician, and they are precisely the problems that preoccupied Machiavelli.

Moreover, if one’s purpose is to discover directly techniques of statecraft – if, that is, one proceeds from the very beginning with what we now call “policy-oriented” studies – the methods one uses are also likely to be of a certain kind. In all probability they will be “empirical” in the most literal sense of

the term; that is, they will involve the examination of experience as if it were a record of trial and error, of a kind of thoughtless experimentation, in which some procedures are revealed to be conducive and others not conducive to certain ends.¹ If anything further is done with such “rules of prudence,” it will be to infer generalizations about psychological propensities underlying the rules and to deduce further rules of prudence, not revealed directly by experience, from the psychological propensities. Crude inductions, crude inferences from the inductions, and crude deductions from the inferences will always characterize such direct inquiries into statecraft. Certainly they characterize Machiavelli’s *The Prince* and *Discourses* teem with examples.

Consider only one, by no means the most blatant – the argument against using mercenary soldiers (Chapter XII of *The Prince*). No Prince who relies upon an armed force of mercenaries, says Machiavelli, can ever “stand firm or sure”; such troops are “disunited, ambitious, without discipline, faithless, bold amongst friends, cowardly amongst enemies, they have no fear of God, and keep no faith with men.” Why so? Because it is not a man’s nature to die for another purely for the sake of a wage; because the more competent a mercenary leader, the more, having no deep bond of loyalty to a Prince, he is likely to aspire to the Prince’s place or otherwise to overstep his powers. And what is the evidence for these assertions? The helplessness of the Italian cities before King Charles of France, the oppression of the Carthaginians by their mercenaries, the fickleness of Francesco Sforza toward the Milanese, and the successes, in contradistinction, of Rome, Sparta, and the Swiss. But what about the Venetians and Florentines, who seemed to do well enough with mercenary forces? No matter, for they were “favored by chance”: the ambitions of their mercenary captains were diverted elsewhere, and these captains *would* have caused more harm if they had been *more* competent. And so it goes, in nearly every chapter.

Montesquieu and the Enlightenment

In the Enlightenment, such simple and disingenuous inductions, aiming at the discovery of political rules of prudence, still abound, along with deductive theories of the state influenced by Cartesian philosophy. In some writers of the Enlightenment, however, above all in Montesquieu, we can detect more modern and more sophisticated concerns, if not in method, then certainly in the problems raised and theories proposed. In many ways, *The Spirit of the Laws* is, in fact, a work astonishingly “modern.”

To be sure, Montesquieu is interested, like Machiavelli, in using induction primarily for purposes of statecraft. What, after all, is his famous theory of the institutional conditions of freedom if not a rule of prudence based upon very limited and crude induction? But perhaps it would be more accurate to say that Montesquieu was interested not so much in statecraft, as Machiavelli would have understood the term, as in constitutional engineering – not in how rulers should behave but in how governments should be constituted. Unlike

Machiavelli, whose argument proceeds from human nature, Montesquieu thought of right government primarily as a matter of sociology and ecology, of adjusting governmental structure to prevailing conditions. Hence, his interest in some very modern concerns: the relations of political systems to their physical environments, the role in politics of economic factors and of “manners and morals,” problems of classifying political systems, and the like.

Any methodical arrangement of *The Spirit of the Laws* immediately gives it a contemporary ring, granted that such an arrangement must be largely imposed by others upon a study for which chaotic is a term of flattery. Take, as an example, the now widely followed scheme by G. Lanson. Montesquieu, according to this scheme, first considers the various types of government: their nature, their structural principles, and the conditions under which they arise and under which they tend to persist or decline (have “viability” or not, as we would say). He goes on to consider the functions of government, including provisions for the safety of the state (civil-military relations), the liberty of the subject, and the raising and expenditure of public monies (“resource allocation,” in modern jargon). Then there follows a long series of chapters dealing with those aspects of their “setting” that condition political systems: ecological conditioning factors, such as climate, soil, and population; social institutions, such as the “relations between the sexes”; matters of culture (the “general spirit, the morals and customs of a nation,” and religion); and economic conditioning factors (the “interrelation between commerce, morals, poverty, and the types of government”). Finally, there are some very scattered, but suggestive, hints at “developmental theory,” at social dynamics no less than social statics.

Anyone au courant with modern comparative politics will recognize these topics as a large proportion of its stock in trade. And it is not only the topics that ring familiar, but also the way they are handled. Montesquieu’s types of government, for example, are ideal types and quite consciously so in that they are logical structures based upon certain fundamental principles underlying the type, to which actual political systems only more or less correspond. Like modern sociologists, he thought of societies as being interconnected, as patterned structures, as “systems” the parts of which are interdependent in such a way that change in any one part leads to compensating changes in the others or to disintegration of the whole. Therefore, he produced an essentially mechanistic interpretation of social change, in distinction both to the voluntaristic theories prevalent in his time and the eschatological theories of history soon to be propounded. He has been called (by Meinecke, for example) one of the founders of “historicism,” but this view is tenable only if we equate historicism with any theory of social change that assigns a role to involuntary social processes or if we use the term to denote any use of the “genetic” approach in social studies and not if we use it to describe grandiose theories of the meaning and goal of history. Montesquieu’s modernity lay precisely in the fact that he worked at a nonvoluntaristic theory of social change without going over to the historicist extreme.

Was Montesquieu an aberration, a stranger to his own age? So it is often argued, but surely not correctly, for it is as plausible to regard him as the culmination of past trends of thought as to regard him as the precursor of writers still to come – not to mention other writers of his own time (such as Adam Smith, Hume, and Ferguson). Methodologically, a clear line runs to him from Descartes and through Malebranche. Montesquieu was certainly not the very crude empiricist that Machiavelli was,² but he understood what it means to assert the existence of “social laws” (as Machiavelli, with his constant harping on chance and fortune, never did). He understood that these laws are to be found by a combination of logic and observation, that proper induction requires the wide-ranging observation of many contexts, and that logic has at least an equal, if not prior, role to play in scientific analysis.

While Montesquieu’s method originated in Descartes, his problems, in contrast, were posed largely by Machiavelli and Bodin. His approach to developmental theory may be quite original, but he wrote at a time when social mechanism was very much in the air and sophisticated historiography at least beginning, however little the latter was influenced by the former. His concern with the relations between governments and their settings, especially his concern with physical environment, was anticipated in a large number of “modern” thinkers, including Bodin and Chardin. And his far-ranging empirical work was certainly connected with the very broad outlook of his age: its belief in the uniformity of men beneath their cultural differences and its relative freedom from the nationalistic and provincial biases that predisposed subsequent thinkers to regard political systems as unique and incomparable.

In Montesquieu, then, and in the writings of lesser men of the Enlightenment, we can see emerging a comparative science of politics not so very different from that which present political scientists seem to want: a “science” aiming at the construction of a structural-functional analysis of political systems, a sophisticated typology of such systems, a set of broad generalizations about the links between polity, society, economy, and environment, and a set of mechanistic theories of political dynamics – all in embryo, of course, but, in many cases, in surprisingly sophisticated form. Between the late eighteenth century and the present, however, a number of forces intervened that sidetracked political studies from these paths, so that we can regard the intervening development of comparative political studies as an elaborate veering away from and return to the lines of analysis sketched, however embryonically, by Montesquieu.

Historicism

Although Montesquieu’s ideas had many antecedents, they undoubtedly were aberrations in the sense that very different ideas set the tone in social thought immediately after his time.³ Not sociological historiography but

rampant historicism – universal history, speculations on the first causes and final end of history – became the dominant style of social thought. This style (the style of Bossuet, Vico, and Condorcet rather than Montesquieu) affected the study of all social phenomena. In the study of political institutions interest now came to be centered primarily upon historical first principles, upon the “cunning of history,” upon the construction of audacious developmental theories, unilinear in form, based on single determining principles and more often than not predicting the imminent universality of democracy – theories of change more organic than mechanistic in form. The best examples are obvious and familiar: Condorcet, with his belief in the simultaneous unfolding of reason and democracy; Hegel, with his belief in the unfolding of Reason and Freedom; Comte, with his belief in the unfolding of the scientific spirit (and, in contrast to the prophets of democracy, his prediction of the coming benevolent dictatorship of well-informed bankers); Marx, with his belief in the unfolding of utopia through class conflict.

Although historicism has long since become discredited, the field of comparative politics owes a great deal to this phase in Western social thought. In the first place, many of its concepts are still used and used fruitfully (“class,” for example). Many of its problems are still raised, above all problems about the relations between politics and economic development, politics and education, politics and the “cultures” of societies. Historicist theories also directed attention, to some extent at least, to a broad panorama of political experience. Hegel, for example (among many possible examples), was anything but a parochial thinker; his ideas ranged widely, if not very accurately, over China, India, Persia, Judaea, Byzantium, and the Mohammedan world, as well as over ancient and medieval Western history. The historicists were also responsible for much of the subsequent interest in social dynamics – especially in evolutionary theory, which helped, much more than did the less fanciful Montesquieu, to counterbalance the voluntaristic biases of political historians. Most of all, interest in broadscale theory as such derives, in large part at least, from historicism.

But if the historicists bequeathed to subsequent students of comparative politics much to aim at and much to imitate, they also gave them much to overcome. Their broad-scale theorizing was mainly a matter of abstract and formal speculation upon the broadest conceivable questions; for the canons of accurate observation – for “content,” in Hegel’s terminology – they had a monumental disregard. Their data, in almost every case, were invoked merely to illustrate, not to test, their theories, so that one searches in vain in their works for a methodologically valid bridge between theory and data. In effect, their work engendered two interests that never really meshed: an interest in the construction of the most ambitious and contentless kinds of theories, on the one hand, and an interest in detailed and formless political history, a sort of political ethnography, on the other. They did not, however, engender (if anything, they discouraged) the sort of concerns that every

young discipline ought to concentrate upon: the formulation and meticulous empirical testing of “middle-range” hypotheses and the tentative conceptual exploration of a field. The basic charge against the historicists is, consequently, that while they induced an interest in theorizing about wide ranges of data (the essence of any comparative study), both their theories and uses of data, and above all the way they related theory and data, ultimately proved sterile. They tried too early to do too much and so, in the end, contributed very little – except some interesting problems and theoretical approaches, and some very far-ranging misinformation.

Perhaps this explains why the historicists, in the final analysis, had a far greater influence upon politics (through the ideological impact of their theories) than upon political science. Concepts they used continued to be used; questions they raised continued to be raised; but the whole style of the historicists, their basic approach to social analysis, constituted only a swiftly passing phase in the development of social thought – granted the occasional appearance of throwbacks to the historicist era. A large number of forces converged in the later nineteenth century to discredit historicism: in the realm of philosophy, the rise of positivism and philosophical pluralism; in politics, the rise of nationalism; in social thought, the impact of cultural relativism; in the general climate of opinion, the reaction against the softer idealisms, the tough-mindedness and perhaps petty-mindedness that followed the great disillusion of 1848. All these conspired against theories inadequately grounded upon observation, blandly optimistic, and assuming a uniformity of development for every society and nation, so that in the end historicism came to be important not so much for the positive influence it exercised as for the reactions to which it led. Certainly this is the case if we confine ourselves to the history of the comparative study of politics.

Reactions Against Historicism

In the study of politics, the reaction against historicism took many different forms, each undoubtedly for good reasons, but each involving also a serious retrogression from the promising lines reached in the eighteenth century. Not Condorcet and his kind only, but Montesquieu and his kind as well, were rejected in the process.

Abstract Theory

One of the reactions against historicism was emphasis upon purely abstract political analysis, especially criticisms and defenses of democracy on the basis of deductions from metaphysical, ontological, psychological, and legal premises. This reaction has only a remote, though nonetheless significant, bearing on the study of comparative politics. Its relevance is, in gist, that in the post-historicist period, institutional and philosophical political studies, studies

with “content” and studies with “form,” became more rigidly separated than at any previous time in the history of political thought, a fact with the most momentous significance for the development of comparative political studies. Historicist thought, whatever its shortcomings, had at least one virtue: it joined, however unsatisfactorily, thought and data. The historicists did think about something, not just about thought. Even Hegel, who believed in the autonomy of formal thought from its content, at least undertook to fill the form with concrete matter in order to portray, if not to test, his formal theories. Those who reacted against historicism, however, did not initially attempt to improve upon what had been at best an uneasy marriage of fact and speculation. They resorted instead to outright divorce, so that in the wake of historicism (in the late nineteenth and early twentieth centuries, roughly) political thought tended to become, so to speak, increasingly subjective and the study of political objects increasingly thoughtless.

The contemporary study of politics as a separate field, and of comparative politics as a separate subdivision of the field, begins, unhappily, perhaps disastrously, at this very point in time. That fact tells us a great deal about one of the more remarkable, if not absurd, characteristics of the political science curriculum: the division of the field into the study of political thought and the study of political institutions and behavior. More to the point here, however, is that it tells us a great deal also about the development of comparative political studies in the post-historicist period.

Formal-Legal Studies

The separation of thought and data is at least partly responsible for a second reaction to historicism that does have a direct bearing upon comparative politics; the increasingly exclusive stress in the study of political actualities on formal political institutions – that is to say, on constitutional and legal structure (then called “public law”). Not all data lend themselves equally well to thoughtless treatment. Those that do so well are unequivocal data, easy to come by and subject to a minimum of interpretation; those that do so best are data that come to us, not in the usual way, inchoate and unordered, but in some already ordered form. And what data in political science present themselves in such a fashion – preprocessed, so to speak? Obviously two sorts: one, political thought itself; the other, formal institutional arrangements, prescribed in documents that are, in fact, mental constructs (and often bad hypotheses), but that can be treated as if they were raw data of political experience, for the political scientist does not invent them, but comes upon them, as he comes upon behavioral data of quite different sorts.

The emphasis in the study of politics upon formal-legal arrangements is thus a natural outgrowth of the positivistic reaction to historicism, simply because primitive positivism, in attempting to restrict the role of thought, naturally leads the analyst to steer clear of the more inchoate data. Primitive, unadulterated positivism insists upon *hard* facts, indubitable and

incontrovertible facts, as well as facts that speak for themselves – and what facts of politics are harder, as well as more self-explanatory, than the facts found in formal legal codes? And what other facts are equally conducive to *Wertfreiheit* in analysis, to what purports to be hardheaded, ethically neutral empiricism? Perhaps this argument may seem strange today. Most of the self-labeled positivists in contemporary political science are concerned with precisely the sort, of inchoate materials that their predecessors ignored: voting behavior materials, power and influence relations, elite structures, informal political processes, and so on. But this does not controvert the fact that the initial impact of positivism upon the field was to direct attention toward superficial facts, even pseudofacts; nor does it deny that the positivistic outlook as such creates, even today, a preference for the superficial over the profound.

The emphasis upon formal-legal structure that came to be the dominant empirical style of political studies in the late nineteenth century was not, however, due to the post-historicist dissociation of thought and data alone, although that dissociation alone may sufficiently explain it. One other factor that certainly made for emphasis upon formal-legal structure, especially upon constitutional documents, is simply that the nineteenth century was a great age of constitution making. In fact, one would be hard-pressed to find “constitutions,” in the sense of elaborate formal-legal codes rationally devised to create political organizations and govern political processes, in a previous period.

If we go to earlier periods, we find constitutions in the Burkean and typically British sense of the term (constitutions as historical accretions of institutions and processes that can be stated in, but are not defined by, formal rules); we find one or two prophetic documents, like the *Instrument of Government*, as quaint in their own time as they are common later; and we find charters – bills and documents called “constitutions” – that are not constitutions in the modern sense at all, but either contractual agreements between princes and subjects (such as municipalities and social groups) or solemn and explicit declarations of historically evolved political relations. This discovery is hardly surprising, for the very idea of a constitution in the modern sense could not have occurred to anyone who regarded the political order as a “natural” thing and is, therefore, properly a product of a time when mechanistic social beliefs, coupled with faith in the powers of human engineering, displaced earlier organicist and historicist ideas. Of course, these beliefs alone were not enough to make political studies focus upon constitutional documents; the documents themselves had to be there to study – as indeed they were, in constantly growing numbers, in the late nineteenth century. But the prevalent mechanistic outlook and faith in social engineering of the period explain at least why constitutional codes were taken so seriously, by politicians and students of politics alike.

Inevitably, these beliefs and interests also left a deep mark on the virgin field of “political science.” Indeed, the very fact that political science

emerged in this period as a separate, autonomous field of study divorced from philosophy, political economy, and even sociology, may have created a tendency to emphasize the study of formal-legal arrangements, quite apart from any other factors moving the field in this direction. If a study becomes departmentally *sui generis*, it will try also to assume a subject matter and techniques of study that are *sui generis*. And what subject matter can be regarded as purely political? Political behavior, in the larger sense in which we now regard it, is touched upon by the subject matters of all sorts of other disciplines: those of sociology, social and individual psychology, cultural anthropology, and economics. If there is any subject matter at all that political scientists can claim exclusively for their own, a subject matter that does not require acquisition of the analytical tools of sister-fields and that sustains their claim to autonomous existence, it is, of course, formal-legal political structure. Its study, therefore, quite naturally became the focal point of the new discipline of political science in search of a *raison d'être*.

Perhaps we ought to add to this list of factors making for emphasis on formal-legal studies (it is an emphasis that requires a lot of explaining) still one other: the emphasis in the teaching of politics at this time upon "training" – training for citizenship and for public administration and preliminary training for the law. This emphasis was particularly great in the "new" states of Europe, above all in the newly unified Germany. Sigmund Neumann has pointed out that in the Bismarckian era, the German universities, once the centers of the fight for freedom, were "gradually transformed into guardians of training for leadership in important public offices, the judiciary, the bureaucracy, and the teaching profession." The "value-free" sociology of Weber and others is regarded by Neumann as one illustration of these tendencies; the emphasis on studies in formal public law may be considered another. To what were they due? Neumann attributes them to the regime's authoritarianism and German admiration for the Iron Chancellor's successes; but we can just as plausibly regard them as responses to the new state's need to socialize men into new political patterns: to inculcate in them civic loyalty and educate them to play roles in new administrative and legal arrangements. Perhaps this is an even more plausible interpretation than Neumann's, particularly when we take into account the emphatic interest in formal-legal codes in the United States. No authoritarianism, no admiration for successful *Realpolitik*, existed here to dampen the impulse to moral criticism in politics or the drive to uncover the deeper forces determining political actualities.

It is true, of course, that the German universities were extremely influential in America around the turn of the century, but the United States had also in common with Germany a tremendous problem in political socialization, due in one case to the creation of a new political system and in the other to mass immigration. In both cases, the agencies most readily available for dealing with these functional needs were educational institutions, especially institutions of secondary and higher education. Hence, there was

a mushroom growth of civics courses providing indoctrination into citizenship and of courses preparing for participation, in one role or another, in the political structure – above all, courses in public administration, constitutional development, and public law. Courses in political “behavior,” as we now use that term, could hardly have performed the same necessary function in either system – might indeed have been dysfunctional in both settings. And it is a fact that formal-legal studies were mainly German and American in origin, epitomized in the German case by the truly gargantuan collection of monographs appearing from 1883 on, the *Handbuch des Oeffentlichen Rechts der Gegenwart* (Handbook of Contemporary Public Law), and in the American case by a study of Woodrow Wilson’s, based largely upon the *Handbuch*, which will be discussed presently.⁴

Configuration Studies

A third reaction against historicism in political studies involved a drift away from comparative studies of all sorts and toward “configurative” analysis – the analysis of particular political systems, treated either explicitly or implicitly as unique entities. Many political studies of the immediate post-historicist period exhibit a considerable narrowing of the analytical attention, a tendency to cover very little ground, and to cover it in great, often indiscriminate detail. This tendency not only restricted attention to one set of political data – formal-legal structure – but also was restricting in a geographic and historical sense. To some extent this narrowing of analysis in time and space may have been the result of the very emphasis on formal-legal structure, for such an emphasis necessarily makes one work within the compass of particular constitutional systems and is, for reasons already mentioned, appropriate only to a limited period in European history. We can see this narrowing influence of the formal-legal approach reflected even in some of the wider-ranging political studies of the post-historicist period, particularly in the large number of compilations of constitutional provisions then published and taken very seriously. But configurative analysis was also an outcome of some of the factors that produced the emphasis on formal-legal studies itself: the reaction against broad speculative theories of any sort; the influence of nationalism and its roots in the idea of national character, which logically implies that each nation is an analytically unique entity; the emphasis on citizenship training and vocational training in an age of rapidly expanding national bureaucracies.

This is not to say that only narrow political studies, confined to particular nation-states, were produced in this period.⁵ There was no dearth of studies ranging over very wide territory indeed, but it is characteristic of these studies that their theoretical import should be almost inversely proportional to the range of material included. Generally speaking, they presented a wide panorama of political materials with a theoretical equipment restricted to little more than Aristotle’s classifications of governments and to

abstract speculations on abstract questions and with the materials arranged either in terms of the three basic forms of government, in chronological order, or in a combination of chronology and forms of government.

An example of this sort of political study – probably the most famous – is Wilhelm Roscher's *Politik*, written intermittently between 1847 and 1892, but chiefly in the last few years of this time span. The revealing subtitle, *Geschichtliche Naturlehre der Monarchie, Aristokratie und Demokratie* (Natural History of Monarchy, Aristocracy and Democracy), gives the whole work away. Its principal theoretical concern is with the proper classification of states, a question Roscher settled by adding a fourth category, Caesarism, to the three classical categories and by distinguishing among plutocratic, proletarian, and middle-class states (still well within the Aristotelian framework). The study is based upon an explicit rejection of the “idealistic” studies of the times – that is, purely abstract treatments like that of Fichte, who, in Roscher's own words, “conceived political science to have only the business of depicting a best state, so that reality appeared to him as real only in so far as it corresponded to the image of this best state.” Roscher, on the contrary, sets out to do precisely what the idealists most disdained, namely, to present a *Naturlehre*, a set of “naturalistic descriptions” of the *Notstaaten* so despised by the theorists of the *Idealstaaten*. And this he does very largely, though not exclusively, in the manner of historical narrative within each of the classificatory categories he adopts.

The result is a work displaying, even by Germanic standards, a truly massive learning. Switzerland, Athens, Rome, Gaul – Egypt, Normandy, Sparta, Venice – Spanish America, Tudor England, the Hebrew State – Brahmanism, Buddhism, Jesuitism, Protestantism – Demosthenes, Henry VIII, Hannibal, Napoleon – the book is almost a political encyclopedia. In this it is reminiscent of nothing so much as the more extravagant historicist theories; but the history it presents is history without the “ism,” a matter of content with very little form, a pointless display of interminable exactitudes. It is in such works that we see the real impact of the divorce of thought and data on the field of comparative politics, just at the time when its practitioners became conscious of having a separate disciplinary identity.

Two Syntheses

Political ethnography, purely abstract speculations, formal-legal studies, and configuration studies – these are all different, even antithetical, reactions against historicism. But because they come from a single source, one should not be surprised to see them combined, however uneasily and in however ill-fitting a manner, in the large syntheses of political thought of the period. These “large syntheses” are not necessarily works of great merit. More often than not, in fact, such works are written by secondary figures, by those who ape the styles of the times rather than those who create them;

but they do provide a very broad picture of the dominant fashions in analysis. Any number of such studies could be used to exemplify the immediate post-historicist period in comparative politics, but two may suffice here: one, published in 1878 by Theodore D. Woolsey, a former president of Yale, entitled rather grandiosely *Political Science, or the State Theoretically and Practically Considered*; the other, by his later Princeton counterpart, Woodrow Wilson, a work with the even more prolix title, *The State: Elements of Historical and Practical Politics: A Sketch of Institutional History and Administration* (1895).

Both Woolsey's and Wilson's subtitles, like Roscher's, tell us, in the typically ingenuous fashion of the late nineteenth century, the most basic things we need to know about their studies. Each portrays mainly two of the anti-historicist styles we have discussed, though in each may be found examples also of the others.

Woolsey's work, an ambitious and pretentious undertaking indeed, is in effect a combination of purely abstract speculations and purely concrete political ethnography. When Woolsey talks about the state "theoretically considered," he refers primarily to two of the three categories into which German writers on politics had by then come to divide political studies: *Naturrecht* (natural rights – sometimes *Staatsrecht*, public rights) and *Staatslehre* (theory of the state). The first of these, *Naturrecht* (Woolsey calls it the "Doctrine of Rights as the Formulation of a Just State") is, of course, concerned with normative theories of political freedom and obligation. This part of the study bears no relation to anything subsequently said in it, but it is justified to Woolsey's own mind on the ground that no state worthy of the name is unjust, that justice in the state mainly consists of the safeguarding of natural rights, and that, therefore, there is no point in discussing the state without discussing the theory of natural rights – a curious syllogism, to say the least, but one that does encompass in a flimsy way the bifurcation of theory and data that confronted Woolsey.

To this concern with natural rights is added a series of concerns that Woolsey himself identifies as *Staatslehre*, a veritable rag bag of ethical and nonethical questions: "Opinions on the Nature of the State and on Its Origins," "Theories of Sovereignty," "The Proper Ends and Sphere of the State," "The Organization of States" (whether the desire for it is instinctive or habitual, the need for a "constitution," the various departments of government, distinctions between representative and nonrepresentative systems), "Theories of Communism and Socialism," "Limits and Extent of the Punitive Power of the State," and sundry normative questions ("Can the Citizen's or Subject's Connection with the State Terminate?" "What Are the Limits of Loyalty and Obedience?" "What of Conflicts between Law and Conscience?").

All these problems, normative or not, are mainly discussed abstractly in the light of the abstract speculations of other political theorists. Politics "practically considered," however, turns out to be what the late nineteenth-century

Germans understood by *Politik*: the large-scale historical examination of political institutions from earliest to modern times, mainly in terms of the classical categories; the formal examination of the “departments” and “institutions” of central and local government; and, at the very end, a few afterthoughts (one or two quite reminiscent of Montesquieu, whom Woolsey had obviously read but not really understood) on the influences of “Physical Causes on Politics,” on “National Character,” and on the “Causes of Political Change and Revolutions.” In short, the whole book, save only for the very end, is either unmitigatedly abstract or pointlessly concrete, and the quotation cited from it on the relations of the study of rights and the state, which introduces the work, is a good indication of the way Woolsey relates theory to data throughout.

Woodrow Wilson’s *The State* is admittedly his minor piece – though anything but a modest one, going on as it does through 1,287 sections, large and small. From our standpoint, however, it is much more worth examining than his more distinguished work, for two reasons: one, that it purports to be a text on politics of unprecedented scope, a summation of the empirical knowledge of the state in his time; the other, that it begins with large claims for comparative politics as the only proper approach to understanding political experience.

What is “comparative” politics to Wilson? Essentially, it signifies to him, as to Woolsey, a very detailed and far-ranging political ethnography primarily as historical narrative and secondarily through the depiction of contemporary formal-legal structure. About five-sixths of the work is devoted to such bald descriptions. Wilson begins the study with some questions about the probable origins of government – a fact whose significance we shall see later, but this subject, after a cursory consideration of evolutionary and early anthropological theories, soon takes him to the more congenial ground of classical history, where his political ethnography proper begins. The political institutions, first of Greece and Rome, then of “Teutonic Polity” in the Middle Ages, and then of German and French feudalism and monarchy are painstakingly examined; the chapters on them constitute the “institutional history” section of the work. The “practical politics” side of the study involves mainly an indiscriminate detailing of the formal-legal structures of French, German, Swiss, Austro-Hungarian, Swedish-Norwegian, British, and American governments. And all this, counting some historical discussions scattered throughout, takes up nearly a thousand sections.

Not until section 1,121 is any attempt made to draw any “comparative” conclusions. And what are these conclusions? Their modesty is perhaps as remarkable as the ostentatiousness of the data on which they are based. Essentially, Wilson distills from his materials three inferences: political change has taken the form of a very slow process of development from more primitive to more highly developed political organizations; modern political experience confirms the Aristotelian classifications, although modern monarchies, aristocracies, and democracies have some features not found

in the ancient world; governments are all pretty much alike – denying the view of those (Wilson claims the great majority) who believe in the uniqueness of political systems – but there are differences between governments, due partly to unique historical backgrounds and partly to “nation-marks,” an argument that immediately reinstates the belief in uniqueness, albeit in a milder form. Finally, in a sort of epilogue, Wilson considers some purely abstract questions in primarily an abstract way: sovereignty, the nature of law, the classification of the functions of government, political rights, whether society is greater than government, and so on. That is the total extent of Wilson’s *summa* – for a *summa* in a way it is, a summation of all the dominant modes of political thought of his time.

Political Evolutionism

Undoubtedly this is an incomplete account of post-historicist studies in comparative politics. It has dealt only with dominant themes in American and German political studies. As a result, it necessarily is less than just to those writers who were, as some writers always are, out of tune with the dominant trends, who lagged behind the times or marched ahead of them. For example, Bluntschli, in his monumental (and much neglected) *Theory of the State*, begins with an explicit rejection of two “false methods,” “abstract ideology” and “mere empiricism,” and a special plea for methods of “concrete thinking,” and lives up to this position at least to some extent (though Bentley denies it). We can no doubt find other important writers equally at odds with the tenor of the times. This is especially the case in regard to a school of writers who, more than any others, kept comparative politics alive throughout this largely anticomparative period, writers whose works have very wide scope, who combine theory and data almost on the scale of the historicists, and who are alluded to in almost every work on politics of the period, even the narrowest, most abstract, and most formal-legal – the political evolutionists.

Evolutionary studies might of course be considered a particular kind of historicism, and in some forms they do come close to what is nowadays (after Popper) generally meant by that term. Those evolutionary studies that posit some inevitable goal (such as democracy or perfect freedom) for the evolutionary process and a basic evolutionary principle (such as survival of the fittest, economic growth, progressive economic differentiation) as, so to speak, the “spirit” of the process are almost indistinguishable from historicist theories. Most evolutionists rejected, however, the too audacious, often ill-informed, theories of the historicists no less than did the pure philosophers, the formal-legalists, and the political ethnographers, although they rejected them in different ways and for different reasons.

Evolutionary theories about politics involved, in the first place, an empirical reaction against historicism in that the evolutionists paid meticulous

attention to data that the historicists had on the whole treated only in the vaguest generalities – particularly primitive, early Western, and non-Western political systems. Evolutionism involved also a theoretical reaction against historicism. Instead of attempting to write universal history, including the future no less than the past, they concentrated upon much more limited problems – particularly the problem of the origin of the modern territorial state. As a general rule, they tried merely to find the processes and laws underlying the development of complex political systems. This is in every sense a more limited concern than that which motivated Condorcet or Hegel. At their best, evolutionary studies combined the respect for data of the ethnographers with the modesty in speculation of contemporary middle-range theorists. Granted that some of the theories the evolutionists produced look very peculiar nowadays – that is less the result of any dubious procedures on their part than of the fact that they proceeded from theoretical presuppositions and asked theoretical questions that have simply gone out of fashion.

Evolutionary Theories

What sort of theories did the evolutionists produce? Essentially two kinds: theories of sequence – the stages of political development – and theories of the moving forces behind the evolutionary sequences. The most common theory of sequence traced the origin of the modern state to a continuous process of social enlargement and complication beginning with the primordial family. Among many works arguing this point of view probably the most illustrious are Sir Henry Maine's *Ancient Law* (1861) and *Early History of Institutions* (1874), in which political life is depicted as beginning with the patriarchal family and proceeding through two intermediate units, the house and the tribe, before the territorially contiguous form of the state is reached. This argument is based on a meticulous examination of Hebrew, Greek, Roman, and Hindu history.

The principal alternative to this interpretation is one that traces the origin of the state not to the family but rather to the disintegration of primitive social forms – not to the growing size and complexity of social units but to the opposite, the gradual individuation of human beings, their extrication from collectivities in which individuality itself is absorbed into the larger unit. So, for example, Edward Jenks argued, in *A Short History of Politics* and *The State and the Nation*, both published toward the end of the period under consideration (1900 and 1919), that the proper sequence for the emergence of the territorial state is not Maine's, but rather from hunting pack to tribe, from tribe to clan, from clan to family, and from there to nonkinship units, individuals, and the state. In a sense, he reverses Maine's arguments.

As to the moving forces behind these sequences, a much greater variety of theories confronts us. Some evolutionists attributed the rise of the

state, particularly the transition from the patriarchal family to the more extended political groups, to religious forces. The usual theory is that of Fraser's *Golden Bough* (for a political scientist's version, see Sir John Seeley's *Introduction to Political Science*, 1896), which traces the evolution of simple patriarchal authority through gerontocrats claiming a special skill in dealing with the world of spirits and through the rule of specialized magicians to that of the priest-king. Fraser's work was based largely on studies of societies with which Maine had not dealt in detail, such as ancient Egypt and primitive societies portrayed in early anthropological studies.

Another group of theorists, especially Oppenheimer in *The State* (1914), find the propelling force leading to the state not in religion but in force, in the building-up of gradually larger units through systematic conquest. Still another theory claims that the state comes into being through the impact of social differentiation on primitive social forms, especially through the appearance of vertical stratification. This view is argued, for example, by W. C. MacLeod in two works, *The Origins of the State* (1924) and *The Origin and History of Politics* (1921), studies in which Darwin, Marx, and early anthropology are all combined in a curious mixture.

A fourth theory linked the evolution of political institutions with economic changes, not so much in the style of Marx as in that of Rousseau's *Essay on the Origins of Inequality*. An example is Oppenheimer's *The State*, which, in effect, combines the conquest theory of the state with an economic theory of its origins. Oppenheimer argues that complex forms of government are made necessary by class distinctions based on wealth and that the institution of slavery to build up a labor force is the basic foundation of the state. ("The moment when the first conqueror spared his victim in order to exploit him is of incomparable historical importance. It gave birth to nation and state.") Some writers linked the development of the state with the development of pastoral pursuits, others with the accumulation of surplus wealth, still others with the development of the idea of property or population pressures upon resources and resulting wars of conquest or, as we have seen, social differentiation of any sort.

Finally, certain writers produced "diffusion" rather than "convergence" theories of the state. These theories argue, in effect, that the factors leading to the state did not produce it in different places through force of similar circumstances, but that the state came into being only once and in only one place through "natural causes" and then gradually spread, presumably because of its organizational superiority and through a process combining conquest, and borrowing, to other societies, like ripples in a pool. For example, G. E. Smith and W. J. Perry, in *The Origin and History of Politics* (1931), place the origin of the state in Egypt around the year 5000 b.c. Here the state emerged, in their view, through a convergence of religious and economic forces never duplicated elsewhere. From Egypt it spread, by quite another sort of inevitability, to the rest of the world.

The Legacy of Evolutionism

Developmental theories of this sort have gone out of style in our age of models, “system” theories, and equilibrium analyses; and so they have about them a musty and archaic flavor, an ambience of crumbling volumes in the dark recesses of libraries and of vain debates long since resolved in irreconcilable disagreements. Yet the pursuit of such theories spans a period from mid-nineteenth century to a mere generation ago, a period that overlaps on one end with historicism itself and on the other with the comparative politics of our own time. In fact, the larger syntheses of political evolutionary studies still smell of fresh ink; the best-known perhaps is Book I of MacIver’s *The Modern State*, first published in 1926 and reissued last in 1955, and Part II of E. M. Sait’s *Political Institutions*, published first in 1938. Sait calls his study *A Preface*, a rather melancholy fact when viewed from the perspective of our time, for it is, in fact, an epilogue and a summing-up. This useful summing-up synthesizes all the divergent tendencies of nearly a century of evolutionary thought about politics, however, as witness the following extract:⁶

The State is composed of three elements: people, government and territory. From the beginning, groups of people are bound together by the cohesive force of kinship and religion. The family is the primordial unit, which expands into sibs (*gentes*, clans) and the tribe. Among pastoral people, patriarchal discipline prepares the way for tribal government; tribesmen who are accustomed to give unquestioning obedience to their respective family heads naturally accept the authority of the council of elders or patriarchs and of the chieftain who rises out of the council. But the emergence of government – that is, an intensified regulative system – within the kinship group must be associated with economic causes, with the adoption of pastoral pursuits and the accumulation of surplus wealth. Property introduces all sorts of complications. There are disputes within the tribe to be settled; there are raids by avaricious neighbors to be repelled. The situation calls for individual leadership. Some member of the council, more energetic and enterprising than his fellows (and for that reason more wealthy), pushes his way to the front with or without the assistance of religious superstition. He, or some one who later essays the same role, is recognized as chieftain. Since the qualities of leadership are likely to be inherited, the office becomes attached to a particular family and is transmitted like other forms of property, Government exists. But although the pastoralists may confine their wanderings within roughly determined geographical limits, they are still nomads.

The territorial State does not appear until population begins to press upon subsistence. Then one of two courses may be followed, new land may be acquired by migration or the old land put to more productive use. Fertile pasturage, when brought under cultivation, will support a much larger population; and the tribesmen have long been familiar with the possibility of raising grain and vegetables from wild seed. Rather than leave the region to which they have become attached, they supplement the prevailing pastoral

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economy with the rudiments of agriculture. Gradually the herdsmen become husbandmen. The transition takes place slowly, as, by trial and error or by the imitation of some neighboring agriculturists, the methods of village are improved and their potentialities realized. Along with the new system of production come great social changes: above all, the sharpening of class distinctions, the systematic reason to slavery, the emphasis placed upon military life (first for defense, then for conquest), and the establishment of monarchy. With settlement upon the land and the acquisition of fixed abodes, the original kinship tie gives way, naturally but stubbornly, to the new territorial tie.

In some such way the state arose.

Because evolutionary political studies have passed out of fashion, their importance is all too easily underrated, but they constituted a tremendously important phase in the development of comparative politics. Above all, they kept comparative study itself alive in a period when it was threatened from every direction. Along with political ethnography they helped to focus attention on political systems other than those of the West just when the academic emphasis on training exerted great pressure toward restricting the political scientist's span of attention. They posed genuine theoretical problems when political scientists were concerned mainly with depicting formal-legal structures. They kept alive a systematic interest in links between political institutions and other aspects of society and kept political science in touch with other social sciences, especially sociology and cultural anthropology, when the newly won departmental autonomy of the field produced attitudes threatening to cut it off from a vast range of relevant data and many useful theories. To be sure, they led in political studies to the consideration of a very limited range of problems, especially concern with the origin of certain widespread political forms and the attempt to discover common sources for similar political institutions in different societies – the chief purpose of E. A. Freeman's celebrated *Comparative Politics*. But that was better at least than no concern at all with problems requiring large-scale comparisons.

Early Political Sociology

Anyone acquainted with political thought in the late nineteenth and early twentieth centuries will realize immediately that some formidable names that might have been mentioned in connection with the state of comparative political studies in this period are still missing from the picture, even after writers like Bluntschli and the evolutionists have been discussed. I refer to a number of men who loom very large in political science (not least in comparative politics) today, but who do not readily fit into any of the categories used here to characterize the political thought of their own time – men like Mosca and Max Weber, Pareto and Michels, the most illustrious of the early

modern political sociologists. All constructed large-scale theories of politics, but theories certainly not purely formal in character. With the possible exception of Michels, they all ranged over wide sweeps of data, but not in the pointlessly empirical manner of the political ethnographers. They were more interested in actual power relations than in constitutional documents, more concerned with recurrent actual patterns of authority than with the inherited formal distinctions between types of government.

They did not restrict the subject matter of their studies to the state but branched out into all sorts of other political phenomena, from the government of political parties to that of private groups, and they explored systematically the impact on politics of its setting. In doing these things, they developed novel analytical perspectives for political studies, devised new concepts, proposed empirically relevant hypotheses, and developed unconventional techniques for applying comparative methods. They engaged, in short, in just the sort of conceptual, methodological, and theoretical explorations that would seem to be the major present concerns in comparative politics.

Why then have they been omitted from the story? Simply because only the *creation* of their works belongs to the period we have been discussing. Their impact on the field of comparative politics belongs to a later time, when the concerns of its practitioners had changed in such a manner as to make them more receptive to the sociologists' ideas. But this is not to say that political scientists in general simply ignored the political sociologists. They did read them and they did teach them, but only to some extent, and only in a way: they taught them as if they had been political "philosophers" in the then familiar sense, concerned primarily with abstract and normative political theory. Without exception, the early political sociologists were represented as "critics of democracy," "irrationalists," latter-day Hobbesians, who attacked the comfortable premises of the defenders of democracy, equality, and human reason – in short, as foils to men like Locke, Mill, and T. H. Green. Anyone who becomes familiar with the work of the early political sociologists today will realize that it was a travesty of their intentions, and indeed of what they actually said, thus to represent them, even though some of them – Mosca, for example – certainly invited such treatment by drawing large normative conclusions from their sociological studies. But basically the political sociologists were treated as they were, not because of anything they did themselves, but because the categories with which they dealt seemed naturally to place them, if not outside political science altogether, then in the area of political theory rather than in the political institutions division of the field. And this is something doubly regrettable, for it means that some of the most promising modern works on comparative political institutions and behavior were long misrepresented in the "political theory" courses, to which they were only indirectly relevant, while they were ignored in the comparative politics courses, on which they had a direct and important bearing.

No wonder that students of comparative politics had to rediscover, and even to relearn, the early political sociologists for purposes of their own work. No wonder either that this rediscovery is something quite recent. In my own undergraduate days Mosca, Michels, and Pareto were still represented mainly as abstract critics of the abstract bases of democratic ideology. I remember, with some horror and some relish, the comment of a venerable teacher (not a “theorist” by any means) on an undergraduate essay about Weber’s political sociology: “An interesting analysis of a brilliant but obscure” – yes, obscure – “German thinker.”

Today, of course, the names of Weber, Pareto, Mosca, and Michels are among the more luminous in the study of comparative politics. But before they could become this there had to be a reaction against the older conception of comparative politics and the actual lines of analysis pursued in the field. This reaction in fact occurred in the 1920s and 1930s.

“Informal” Politics

One of its first manifestations – not confined to comparative politics, but, in fact, appearing at first mainly in studies of American politics – was a growing interest in political parties and pressure groups. This interest is important because parties and pressure groups are not, strictly speaking, pans of legal-institutional structure and because they link politics to other social phenomena more closely than does the formal-legal framework of a political system. The reasons for the growing interest in “informal” political processes throughout the 1920s and 1930s are fairly obvious. Most obvious of all is the fact that parties and pressure groups by this time played a greater role in the politics of most states than they had before. Parties, in the sense we now think of them, developed rather late in the history of representative systems, however much the term itself might have been in use in earlier times. Large-scale, bureaucratized, intensely active pressure groups, especially great economic and other “interest” groups, also belong to a relatively recent period. This fact, however, while important, is not alone enough to explain the increased interest in parties and pressure groups, for the mere fact that something exists and plays an important role in politics does not mean that it will necessarily be studied by political scientists. The analyst’s attention must first be prepared by operative preconceptions to seek out the data and to recognize them as significant. What theoretical influences, then, disposed political scientists to look intensively at party and pressure group activities?

One of these influences undoubtedly was political pluralism. By rejecting the idea of sovereignty and by intruding into the Lockean dualism of individuals and the state the concept of groups earlier mediating between them or coequal with the state, pluralism made all sorts of phenomena appear “political.” Under the influence of the monistic theory of the state these phenomena had appeared extraneous to politics. To be sure, the pluralists

argued mainly a normative case: that the state was only one social organization among many and that it had no special right to impose obligations upon individuals or their collectivities, that is, no special status above the other associations of society. But this normative position inevitably influenced the way politics was conceived for all theoretical purposes. In breaking down the distinction between the political and the social, the pluralists did not remove the consideration of politics from the study of society, but, quite the contrary, they invested all things social with political significance. Under their influence, one saw politics and government, power and authority, everywhere and in all social collectivities, but first of all, of course, in those collectivities most closely bound up with the state: pressure groups and parties.

There can be little doubt that the pluralistic point of view underlies, consciously or otherwise, the work of such men as Lasswell and Catlin, the undoubted pioneers in enlarging the subject matter of political science from the state to social relations as such. "The writer," says Catlin in the Preface to his *Principles of Politics* (1930), "sees no objection to calling the science of social inter-relations by the good Aristotelian name of Politics." Shortly thereafter, Catlin acknowledges his "profound debt" to Harold Laski, who was, at one time, perhaps the most celebrated of the normative pluralists. Politics he then defines as a particular kind of activity, "not as a thing," specifically as any act of human or social control. A broad definition indeed, but no broader than that with which Lasswell begins his famous *Politics: Who Gets What, When, How* (1936): "The study of politics is the study of influence and the influential," the influential being "those who get the most of what there is to get."

The growing emphasis on parties and pressure groups can also be attributed to a second major influence on political preconceptions: certain experiences made students of politics more aware than in the past of the great difference between constitutional forms and political reality. In America, the muckrakers had led the way toward the discovery of the "anonymous empire" of lobbyists and influence-wielders, conducting a kind of private government under the public facade of the Constitution and in interplay with formal authorities. This process seemed to the leading "group theorist" of them all, Bentley, to be the total sum and substance of politics.

Perhaps the most crucial experience leading to a disenchantment with constitutional forms was the fate of the Weimar Constitution, that professionally engineered document so widely acclaimed in its time, such a dismal failure in operation, which eventuated in the most extreme of totalitarian regimes. Some political scientists managed to cling to their preconceptions in the face of the Weimar experience (and the equally sorry operation of the French Third Republic) by claiming that it was all the result of faulty constitutional engineering. Many more, however, drifted toward the view that political processes are only imperfectly subject to control by formal rules and mainly the products of social and economic forces, of the interests and attitudes of the public and politicians, military officers and public officials,

capitalists and trade unionists, and the like. Certainly these experiences helped to make political scientists aware that men like Marx and Pareto, Michels and Mosca, Wallas and Lippmann, did not belong merely among the abstract, primarily normative, political theorists, but that they could help one to reach a better understanding of actual political processes than could the constitutional lawyers and writers on formal political structure.

From this growing concern with informal political processes, political competition, semipolitical groups, and actual distributions of power, there naturally followed a growing interest in the links between politics and other aspects of society. From this in turn there followed a growing interest in systematic problem solving on the middle-range level rather than in the construction of mere morphology. "Political sociology" came by degrees to be reconciled with what had passed for political science. It is clear from the literature of the period that the crisis of democracy in Europe provided the main impetus toward this reconciliation – even more than the widespread influence of Marxism, which certainly had a greater impact on political activists than political scientists in these years.

The Synthesis of Data

The reaction that took place in the 1920s and 1930s against the older conception of comparative politics had also another important manifestation. There appeared in this period a number of studies that attempted to synthesize the findings of configurative studies in large-scale comparative works and, in the course of this synthesis, attempted also to reunite political theory and political data. These syntheses – the most weighty are James Bryce's *Modern Democracies* (1921) and C. J. Friedrich's *Constitutional Government and Democracy* (1937) – are fundamentally different from those of Woolsey, Wilson, and their kind, particularly in two ways. They do not present theory and data simply as cohabitants under a single set of covers but chastely separated. On the contrary, they bring the data directly to bear on the theories, making the resolution of theoretical issues turn at least to some extent upon the evidence of experience rather than exclusively upon the promptings of reflection. And they do not synthesize configurative studies by presenting broad historical narratives in the manner of Wilson (narratives in which each link in the chain still appears as something quite unique in whole or in large part). Rather – and this is especially true of Friedrich – they present data in terms of general functional and structural categories, which, by implication, are elements of all political systems, or of all political systems of a particular sort. Because of this presentation, they are much more obviously "comparative" in nature than Woolsey's and Wilson's syntheses. Perhaps these two tendencies – the reconciliation of theory and data and the use of generalized categories for the analysis of political systems – are still rather primitively developed in the work of Bryce and

Friedrich. Perhaps also, the theories are still too much taken from purely abstract political speculations and the generalized categories from the existing corpus of formal-legal studies. This is saying no more than that their work was affected by studies already in the field, as any scientific work must be. There is much that is old-fashioned in both Bryce and Friedrich (and, of course, much more in Bryce than in Friedrich); but there is also much that is original and portentous for the future and much that is derived from the original studies of informal political processes of their own time. Bryce and Friedrich are in effect transitional figures in comparative politics; and just for that reason it is worth looking in some detail at both what is essentially old and what is essentially new in their studies.

Bryce's Modern Democracies

Bryce's *Modern Democracies* is in many ways a synthesis in the grand old manner, certainly in scope and to some extent also in content. Much of it consists of old-fashioned configurative studies of a large number of "democratic" countries: ancient Athens, the republics of Latin America, France, Switzerland, Canada, the United States, Australia, and New Zealand. In these configurative studies much space is devoted, in the established manner, to formal-legal structure. But quite apart from the fact that Bryce also gives considerable space to political parties and "the action of public opinion" – subjects not at all discussed by Wilson, whose index of seventeen pages does not even list parties, and discussed only cursorily, in the main abstractly, by Woolsey, who gives them twenty-five pages out of twelve hundred – the whole conception of the work makes it into something unprecedented, in idea, if not in every aspect of the way the idea is carried out.

The configurative studies that Bryce presents are in fact intended only to provide data necessary to achieve a broader analytical purpose. And what is this purpose? Basically, it is to solve a single substantive problem that ties together the whole prolix and often incoherent work and to solve it by applying a particular procedure that to Bryce is the only proper procedure for comparative analysis. Both of these aspects of his purpose, his problem and his method, furnish evidence of the transitional character of his work.

The basic substantive objective of *Modern Democracies* is to examine the plausibility of the justifications and criticisms of democracy on *empirical* grounds, to see what light actual experience sheds upon the abstract arguments used either for democracy or against it (in his time, chiefly for it). His object, Bryce explains in the Preface, is not to develop "theories" but to state facts and "explain" them. Explaining facts is of course precisely what most of us today understand by developing "theory," but to Bryce theory means something quite different, and significantly different. It denotes what he later refers to as the "systematic" approach: purely speculative thought, unencumbered by data. Such thought, he argues, leads only to "bloodless

abstractions,” based, more often than not, on supposedly self-evident propositions about man and society, which inevitably give rise to empirically false or dubious conclusions.

The usual procedure in arguments about democracy is, according to Bryce, to establish first certain natural human rights; to argue from these to the logical desirability of democracy (that democracy is “government upright and wise, beneficent and stable”); to posit certain propensities in the nature of man that make it possible to argue that “democratic institutions . . . carry with them, as a sort of gift of Nature, the capacity to use them well”; and then to deduce further a great many not at all self-evident propositions about the desirability of liberty and equality, the educatability of all men, the relations of literacy and political wisdom, the rightness of public opinion, and so on. Bryce himself wants nothing to do with such abstractions, nor with any discussions of schemes of political reform “on general principles.” His aim is to subject all such assertions to a single question: are they borne out by political experience and, if not, what propositions fit such experience better? The whole work, then, is intended to be an antidote to abstract theory about questions that the abstract theorists had wrongly preempted from the empiricists.

Of course, this very definition of his problem means that the abstract theorists exercised a great influence upon Bryce’s study, if only in that his own theoretical problems are derived from them. This fact alone gives the work a curiously old-fashioned tone. Like the abstract political theorists, Bryce is concerned with what is right or wrong with democracy and a host of subsidiary normative problems. (Does power corrupt? Does wealth? Can the arts and sciences flourish under democracy?) Since his data are in most cases not adequate to solve such problems, there is in his work, as in the older syntheses, still a considerable gulf between speculations and data, even though the basic aim is to bring the two together.

Furthermore, just as Bryce’s problems are rooted in tradition, so also, in some respects, are his methods of dealing with them. Not only does he present his data in the first instance through a large series of configurative studies, but his whole conception of the relations between facts and theory is primitive and old-fashioned. Nowadays we certainly do not believe that facts speak for themselves, that we need only know them in order to know what follows from them. We believe that facts are dumb and slippery, that they reveal their significance only when we have set all sorts of cunning traps for them – when we have gathered them in various ingenious ways and subjected them to various complicated processing devices: experiments, carefully chosen samples, multivariate analysis, and the like. But Bryce’s attitude to facts is essentially that of a methodological innocent, even though, like Machiavelli, he has more than the ordinary amount of shrewd common sense.

Basically, Bryce is the crudest sort of empiricist in that he believes, implicitly at any rate, that facts are really self-explanatory, and in that he

decidedly belongs to the past rather than the present. So also he echoes the past, though in a different way, in his basic conception of “comparative method.” *Modern Democracies* is indeed represented as a “comparative work”; in fact, its very first chapter, after the introduction, is devoted to an explicit discussion of comparative method, something surprisingly rare in the field. But comparative method to Bryce has only a very special and limited utility; it can yield no direct knowledge of anything he wants to know but only give a more solid grounding to those first principles from which all political positions must be deduced. Comparative method is not really intended by him to be an alternative to the “systematic approach.” In the last analysis, he uses comparison only as a way of arriving at basic premises for systematic analysis, a way supposedly superior to the formulation of “self-evident” propositions. While, therefore, the basis of Bryce’s arguments is certainly empirical, or meant to be empirical, the arguments themselves, once we leave his country studies behind, sound curiously abstract.

Methodologically speaking, Bryce is in effect both a crude empiricist and a reductionist of the most extreme sort. This combination explains all the essential characteristics of his work: why he is a theorist who uses almost no theoretical equipment and, even more important, why he carries out an explicitly comparative analysis in a basically configurative way. Bryce believes, in effect, that every concrete social pattern is something unique, something ephemeral and nonreplicable, and therefore that it can be adequately represented only by means of configurative analysis. But just because every concrete social pattern is a world unto itself, a precise social science must be based, he argues, upon psychology, upon the constants of human nature that underlie the varieties of social experience.

What then is the comparative method to do? Is it not, upon this view, irrelevant to social science? Not quite: comparative method, to Bryce, does have a role to play in social science, a psychological role: one uses it to discover the fixed characteristics of human nature by examining the differences in actual social phenomena. What we do in comparing is simply that we subtract from actual experience that which is seen not to be “fundamental” to it: anything due to “disturbing influences.” such as the influence of race, “external” conditions, historical antecedents, and so on. We are then left, as a residue, with the human constants we need for a precise social science. For the sort of issues Bryce raised, this social science is necessarily deductive once the psychological premises have been established, but for the explanation of concrete social facts it yields a simple ad hoc empiricism. To explain any concrete behavior we simply combine the psychological constants with the unique disturbing influences bearing upon the behavior pattern – and there we are.

This sort of procedure is nothing original in Bryce’s time – though there is no evidence that he knew anything of Pareto, with whose *Mind and Society* his *Modern Democracies* has much in common, not only in method but also, as a consequence perhaps, in manner: particularly in the disorganized

presentation of great heaps of information in volume after interminable volume; the whole is sifted here and there for a very few dubious propositions of cosmic import. Psychological reductionism happened to be very much in the air in Bryce's time, not least in political studies. What is important in Bryce's version of it is his insistence on the actual analysis of political systems in order to discover relevant psychological constants, rather than proceeding from common-sense notions about human nature or "self-evident" propositions.

Whatever one may think of reductionism in principle, it is certainly a procedure difficult to carry out in practice. It is no small matter to try to find in the enormous varieties of concrete social life anything constant at all, except variety itself. And so it is not surprising that Bryce is, in the final analysis, not quite true to his method. He actually distills from his configurative materials not only psychological constants but also, with more emphasis and at much greater length, certain broad ad hoc generalizations about the essential bases of successful democratic government and the contingent circumstances that help or hinder its existence.

His argument comes down to this: Successful democracy, he thinks, requires a legislature rather like the British House of Commons up to the late nineteenth century. It should consist of illustrious men who command great respect and have a high sense of political responsibility, who are not divided into many antagonistic groups and yet not subject to great party discipline either, who are not mere speakers for constituents or parties and yet can easily be integrated into majorities for the expeditious discharge of business – a legislature devoid of caucuses, groups, opportunists, and the second-rate. The possibility of the existence of such a legislature depends on the general national character of a people. This character, in turn, Bryce treats not as a simple given fact but as the product of numerous conditioning factors that he never makes explicit but that keep appearing in his analysis: demographic and geographic factors (smallness is absolutely essential: only its great size keeps China from being a successful parliamentary democracy!); the ethnic, religious, and class diversity of a society; occupational structure and economic development (agriculture is conducive to democracy, while industry, because it generates occupational diversity and class conflict, and because wealth corrupts, is a threat to democracy); history (especially the gradual development of a desire for democracy and a tradition of self-government); and a mysterious factor he refers to as "racial qualities."

While most of the work thus deals with the "disturbing influences" that condition societies, some of it is devoted, as it must be, to the constants on which these influences work. Bryce's constants resemble those of Michels as much as his method resembles that of Pareto, again without any evidence of acquaintance with Michels's work. What Bryce really discovers is not any psychological constants at all but a sociological principle and certain principles subsidiary to it. This principle is the universal fact of oligarchy in politics. He finds it to be a universal fact because "organization is essential

for the accomplishment of any purpose,” because the majority takes little interest in politics and lacks sufficient knowledge to play a positive political role, and because the natural capacities of people are unequal. Democracy in its classic form, therefore, is a human impossibility; at most it can mean only the prescription of broad ends and the selection of leaders from among competing elites by the electorate. Bryce thus develops a very early version of Schumpeter’s elitist argument in *Capitalism, Socialism and Democracy*, and couples it with some very pessimistic findings about the educability of people, the usefulness of mass media of communication, and the appetites for self-government and authority.

But this is not the place to go fully into Bryce’s substantive findings. The important thing is to note the ways in which he presents them and arrives at them. To sum these up: Bryce is, in the first place, still an abstract theorist, but one who insists on the empirical derivation of his first principles. He is also, by conviction, an exponent of configurative analysis, but he insists on using the data of configurative studies for broader theoretical purposes. Finally, he is also something of a middle-range theorist (insofar as he looks for the probable effects of particular conditioning factors like ecology, social structure, and economic structure on particular aspects of political behavior), but he assigns to such middle-range theories a relatively low importance compared with residual first principles and arrives at them through the crudest sort of empiricism. What we should note above all perhaps is that he insists that theories be fully grounded upon data and that data be presented always for theoretical purposes. Politics appears to him an activity embedded in all social relations yet not governed by any single transcendental principle. In these respects, his work represents a long step away from the world of historicism and its aftermath and toward the approach of Montesquieu, who was by Bryce’s own admission, along with Tocqueville, the model he sought so emulate.

Friedrich’s Constitutional Government And Democracy

In one sense, perhaps, Friedrich’s *Constitutional Government and Democracy* is more like the late nineteenth-century syntheses than Bryce’s *Modern Democracies*. It is packed with discussions of the abstract political theorists that are in many cases not clearly integrated with the empirical parts of the study. Bryce at least derived from the abstract theorists his analytical problems; in Friedrich, references to the political “theorists” sometimes are little more than displays of erudition, albeit impressive erudition. In many important respects, however, *Constitutional Government and Democracy* takes great strides beyond *Modern Democracies*.

For one thing, there really is no purely configurative analysis in the book. Instead of presenting his empirical materials on a country-by-country

basis. Friedrich organizes them in terms of a large number of structural and functional categories, under each of which theoretical speculations and data from a number of political systems (all Western) are given; the data are then sifted for theoretical significance. It is true that information under most of Friedrich's categories is itself presented in a country-by-country fashion, but the intent is clearly to go beyond configurative analysis; the country-by-country approach is used merely as a way of organizing the materials and not as the result of a belief in the uniqueness of each configuration. It should also be noted that many of the categories in terms of which the work is organized refer to formal-legal structure. That may, however, be the result simply of the nature of the materials available to Friedrich rather than of a narrow conception of politics on his part. In any case, the study includes much comparative material on parties, interest groups, and media of political communication; and throughout Friedrich gives considerable attention to the interplay between political forms and social conditions. In *Constitutional Government and Democracy* we thus come upon a full-fledged modern comparative synthesis, although one which still leaves many theoretical strands dangling in empty abstraction and which is still deeply rooted in the formal-legal style of early political science – two facts perhaps inevitable, given the period in which it was written.

Just as the contents of the study and the way they are organized are a mixture of the new and old, so also is the methodology underlying it, although it is a methodology very different from that of Bryce. As we have seen, Bryce's methodology had as its object, mainly the establishment of first principles on empirical grounds. The ultimate purpose of Friedrich's, on the other hand, is to defend crude empiricism in the direct (not the deductive) construction of middle-range theories, although in the course of establishing this position he passes over some of Bryce's methodological ground.

Friedrich is not nearly so optimistic as Bryce about the possibility of scientific precision in political science on any basis. In a methodological appendix that portrays exactly what he does in his substantive chapters (but that is omitted from the later editions of the work, since Friedrich himself no longer holds these views), he rejects the possibility of formulating "laws" about politics and argues that one can at most formulate only "reasonably accurate hypotheses concerning recurrent regularities" in political experience. A reasonably accurate hypothesis about politics seems to him doomed to be always a greatly inaccurate hypothesis. Why so? Because all social phenomena involve the operation of a great many variables, and the greater the number of variables bearing upon a subject, the more inexact generalizations and forecasts about it must be.

The proper method for such a subject matter, among all the methods available to us, is what Mill called the inverse deductive method, and this is simply the method of reductionism: the establishment of psychological constants by reasoning back from cultural variations to invariant underlying conditions. Here we seem to be back with Bryce. But – and this is the rub – to

find the constant human nature underlying social experience, argues Friedrich, no complicated procedures are needed; psychology is fully available to common sense, for in talking about human nature we are only talking about ourselves, and therefore about data available to simple introspection! Friedrich is almost touchingly certain, in fact, that we already know almost everything worth knowing about politics and that any “partially inaccurate” notions we may have about the subject are easily corrected by deeper introspection and a wider inspection of data, for if psychology is the basis of political knowledge, we need “only” look inward to know politics, and if, despite this fact, inaccurate notions about politics become established, the “facts” will soon disabuse us of these notions. In this way, Friedrich, having ruled out scientific precision at the outset, then makes things still easier on political scientists by holding that all “reasonably accurate” hypotheses in political science are immediately accessible to common sense – anybody’s common sense, though best of all the common sense of the well-informed political scientist. This “methodology” is nothing more than an argument for ordinary shrewdness and nothing less than an argument against “social science.”

In the substantive chapters of the work Friedrich is faithful to these views. Unlike many social scientists, he knows exactly what he is doing. His actual method in *Constitutional Government and Democracy* (though not in later works) is first to inspect a certain range of behavior (now broad, now narrow) pertaining to one of his subdivisions of constitutional government, then to generalize about it on the basis of common sense (that is, without using any special technical apparatus), and finally to see whether the generalizations so arrived at, when reduced to psychological terms, are congruent with his own common-sense notions about human nature. He collects a set of facts, reflects upon them, and checks the common-sense plausibility of the reflections; this, in his view, is really all that social scientists can do fruitfully.

We get the quintessence of this procedure in the conclusion to his chapter on electoral systems. “Proportional representation,” Friedrich says there, “has been found wanting and incompatible with parliamentary government” – as indeed it had to be, for the “natural” effect of P.R. is to splinter political forces and thus prevent the formation of majorities on which stable parliamentarianism depends. The Weimar Republic illustrates the case. But if we look at all the other cases of P.R. that Friedrich cites – Belgium, the Netherlands, Sweden, Norway, Denmark, Ireland – this conclusion seems by no means to follow. What then? Well, “there are special factors to be considered in these several lands.” Apparently, P.R. is not incompatible with parliamentary government in constitutional monarchies, or in small countries, or in countries with strong administrative traditions, or in countries where a single emotional issue divides the electorate. And “all this goes to show that the prevalent English and American opinion against proportional representation is practically sound.” What we get here is in effect a generalization based

on a single case, supported by common sense, and then an almost model exercise in what to methodologists is one of the cardinal sins: “saving the hypothesis” by enlarging it to cover all the cases that seem to falsify it – in this instance, the great majority.

But it is too easy, and quite unjust, to be harsh on Friedrich from a contemporary perspective. Despite the deliberate antisocialism to which Friedrich adhered when he wrote the book, *Constitutional Government and Democracy* deals with a host of middle-range problems that simply cry out for more methodical treatment and includes a large number of theoretical propositions that have furnished issues to comparative politics for a long time now.

The objective that unifies the work is to determine the conditions of success of constitutional government (and, by the way, to develop, through the examination of existing systems, a set of maxims of constitutional prudence). In regard to this basic problem Friedrich chooses an essentially “cultural” solution – that is, the primary significance of what Bentley called “soul-stuff,” political ideas and attitudes. Constitutional government and democracy, he argues, are threatened primarily by “intensity” in politics, especially intense disagreements over fundamental procedures and ultimate political objectives; intensity itself is measured by the extent of political enthusiasm (“consent”) and animosity (“restraint”) in a society. This broad hypothesis – which seems commonplace now but was not at all conventional twenty years ago – is the apex of a great many more limited generalizations: for example, that successful constitutional government requires a “balance of social classes” (whatever that might be), that “objective” heterogeneity in a society does not undermine constitutional government so long as there is a minimal unity of political outlook, that the number of parties in a representative system depends upon conditions prevailing in the system prior to the establishment of parliamentary government, that an inflexible constitution is to be preferred over a flexible one in societies that have no firm constitutional tradition but not in societies that have such a tradition – and many more, all based, of course, on artless inferences from very few cases.

At the end, we are left with three sets of theoretical ideas, two substantive and one procedural. First, the study presents what is in effect a set of requisites for successful democracy, many of them truistic, as they must be in view of Friedrich’s method, but some not at all obvious. These requisites fall into two categories. One comprises organizational requisites, such as a responsible bureaucracy, an efficient diplomatic service, an effective judiciary with wide powers (including controls upon administration), a legislature organized for fruitful deliberation and not merely accurate representation and unlimited debate, some sort of separation of powers, functional or territorial, a neutral arbiter of constitutional disputes, and broad but rigidly defined executive emergency powers. The other comprises social and cultural characteristics: a viable economy, low intensity in politics, an effective constitutional symbolism, informative media of communication,

and a high degree of political integration of economic and other material interests in society. Second, Friedrich presents a number of conditions that, while not requisites of effective democracy, do help to create a favorable climate for it: for example, a firmly rooted political tradition (its absence is not fatal because it can be overcome by proper constitutional devices), judicial review, the plurality systems of elections, and the existence of only two political parties in the system. And third, he provides throughout, chiefly by implication, a number of variables to use in the analysis of the functioning of all political systems, the most important of which are, in his view, political attitudes and constitutional structure, although he also resorts in places to factors such as social structure (note the requirement of a balance of social classes), history, and personality (that is, “leadership” as something that is not the product of any social forces).

All in all, then, *Constitutional Government and Democracy* is an early example of the functional approach to political analysis. It is a thoroughly comparative work, partly because a functional conception of a subject is in its very nature more conducive to comparative study than any structural definition. It is a study entirely devoted (leaving aside the generous references to traditional political theory) to the construction of middlerange theory about political institutions and behavior. In other words, its analyses are neither as all-encompassing as those of the historicists nor as narrowly restricted to configurative and formal-legal descriptions as those of the post-historicists. The middle-range theories presented deal mainly with the interplay of formal political processes with political parties and groups, and, in a still larger sense, with cultural, historical, and social forces. These are the “new” elements of the work. But *Constitutional Government and Democracy* has no real method (at most, an antimethodical methodology) and uses, geographically speaking, a rather limited range of data.

With Friedrich, however, we are at least to a large extent back in the world of Montesquieu’s political sociology. We are not yet very far beyond it or in some respects even abreast of it. But it is no accident that it is Friedrich who really begins to synthesize political science with the political sociologies of Mosca and Pareto, Weber and Michels, to all of whom there are liberal references – though mostly critical references – throughout his study. What is still missing in his work is even that beginning of a systematic and rigorous approach that we can detect in *The Spirit of the Laws*.

Postwar Developments in Comparative Politics

By World War II, then, comparative politics was characterized by a reawakened interest in large-scale comparisons, a relatively broad conception of the nature of politics and what is relevant to politics, and a growing emphasis upon solving middle-range theoretical problems concerning the determinants of certain kinds of political behavior and the requisites for certain kinds of

political institutions. Comparisons, however, were still made largely without the use of any special technical procedures; speculation and data were only beginning to be deliberately integrated. The subject-matter treated was still predominantly the sovereign state, indeed still mainly the formal aspects of Western nation-states. The concepts used for analysis were largely conventional rather than technical, no explicit conceptual schemes designed for theorizing were used, and some of the most important aspects of analysis were left implicit. The interwar period was one preeminently of ad hoc and common-sense theorizing. This brings us to our own time.

What have been the trends in comparative politics in the postwar period? The most basic have been four. First, the empirical range of the field has been greatly enlarged, primarily through the intensive study of non-Western systems, but also through research into aspects of politics previously little studied. Second, concerted attempts have been made to overcome the lack of rigor and system that characterized the field in the prewar period – to make it more “scientific,” if the use of unconventional technical concepts, systematic analytic approaches, and rigorous testing procedures may be called scientific. Third, there has been much greater emphasis upon the political role of social groups (whether explicitly organized for politics or not) and upon social institutions that play a special role in molding political values and cognitions, loyalties and identifications – agencies of political “socialization.” Finally, political systems have been analytically dissected and questions raised about them in terms of conceptual schemes largely imported from other social sciences, above all in terms of structural-functional analysis. These trends take us back fullscale at last to the political sociology of Montesquieu, and indeed greatly improve upon it.

I have not listed the trends here in any logical sequence, but neither have I listed them in a merely random way. Granted some unavoidable overlapping, they appear in the order in which emphasis upon them actually developed in the postwar period (save only for the fact that structural-functional analysis has played an important role throughout, but a constantly greater role as the trends unfold), and they appear in this order because each stage in the postwar development of the field helps us to understand why the next was embarked upon.

The Study of Non-Western Systems

The influences leading to the gradual extension of subject matter to non-Western systems are fairly plain. The most obvious of them is the fact that societies and areas that political scientists interested in current events could once safely ignore became important and obtrusive in the postwar period for a great many reasons: the emergence of many new states in non-Western areas, the impact of the Pacific and North African wars (which certainly made many Westerners intimately acquainted with areas previously regarded as merely exotic), and the fact that only the non-Western areas

were uncommitted, or open to a revision of commitment, in the power conflicts of the cold war. There was, consequently, and still is, a considerable demand in the nonacademic world for specialized knowledge of these areas, and such a demand for expertise necessarily arts as an impetus toward its acquisition, most of all in a policy-oriented and training-oriented discipline like political science.

Yet it would be much too one-sided to regard the intense postwar interest in the developing areas merely as a response to postwar politics, even conceding that the most obvious academic influence that might have made for this interest, political evolutionism, had played itself out by this time. Why had not this great interest arisen much sooner? Perhaps because financial support for studies of premodern systems was harder to come by in the prewar period – and such systems are expensive to study – but financial support was scant at the time for almost all projects in the social sciences. Perhaps because international power relations centered heavily upon the European countries; but there was Japan to contend with in the East no less than Germany and Italy in the West, there were riots, demonstrations, and mass arrests in India, there were important upheavals in China and Turkey. There was much to study outside of the West.

Why then did so very few students of comparative politics turn to study other areas? The answer is, at least partly, that their aims and preconceptions as political scientists simply did not direct their attention toward them. Perhaps the most important factor responsible for this was the almost universal emphasis in political science upon the study of democratic institutions, then, and still, to be found mainly in the West. We must remember that even Alfred Cobban's pioneering study *Dictatorship: Its History and Theory*, which now may strike us as very antique, dates only to 1939. And why this emphasis on democracy? The answer was already noted by Bryce: because of an almost universal belief not only in the desirability and possibility but also the inevitability of representative democracy in the development of nations. After all, was not all of Western history itself indicative of this trend? Even the early Soviet Union did not raise any particular problem in this regard, for one could always take its doctrines at face value and persuade oneself to believe that it was itself tending in a democratic direction. So, in their larger-scale political works, political scientists wrote, if not about the modern democracies themselves, then about the Ur-democracies of the ancient world and the historical processes leading from them to the more fully developed democracies of modern times; but it seemed pointless and superfluous to write about contemporary predemocratic, obviously transitional, systems – certainly as long as the end of the transitional process did not seem problematic.

From this standpoint, the interest in non-Western systems in political science is closely bound up with the crisis of democracy in Western Europe, the emergence of Italian and German totalitarianism, and the brutalization of Soviet Communism under Stalin. The declining faith in the inevitability of

democracy led not only to a general interest in authoritarian governments, as exemplified by Cobban's own work, but also to two other, and relatively new, interests: in the processes of political change and the forces governing it and in the social forces rather than the legal rules governing politics. All of these interests obviously helped to open the door to the study of nondemocratic, rapidly changing societies either lacking highly differentiated political systems and highly articulated formal-legal structures or possessing them only on the level of colonial authority.

Also as a result of the crisis of democracy, political scientists now undertook a more intensive searching of the early political sociologists in order to gain insights into the cause of the unexpected political experiences of the modern world, and through the works of the political sociologist – certainly through Pareto, Mosca, and Weber – they acquired at least a cursory acquaintance with a wider range of political systems than political scientists had normally possessed.

The great postwar interest in non-Western areas is therefore a reaction to prewar no less than postwar political conditions. At any rate, it is a consequence of certain modes of thought engendered by prewar political experiences. And it may also be regarded as a consequence of a trend more purely internal to the field – namely, the growing interest in middle-range theories as such. The connection here is really quite simple: configurative study is bound, by its very nature, to narrow the empirical scope of studies, and comparative study, for the purpose of formulating, and even more for testing, middle-range theories, is bound to broaden it. This is a truism, but for the present purpose an important one.

Scientific Rigor

Without slighting the role of external influences, therefore, one might reasonably have expected a broadening of the scope of comparative politics in the postwar period in any event. So also with the postwar interest in scientific method. Already in the 1920s and 1930s one can detect a certain unease about the looseness of analysis characteristic of the field. Bryce's chapter on comparative method is about a dubious version of it, but it *is* a chapter on the conditions of rigorous social analysis. Friedrich's epilogue on method is an apologia for his unscientific empiricism, but he does appear impelled to apologize for these aspects of his work. Certainly it is not difficult to see how formulations of middle-range theories about behavior within numerous contexts might lead the analyst, quite without other stimuli, toward increasing rigor of procedure and unconventional concepts and approaches; the moment one begins to question propositions like those which abound in *Constitutional Government and Democracy*, one can hardly avoid such matters, for it is precisely the lack of rigor and unconventionality that gave rise to the propositions.

But the postwar quest for a more rigorously “scientific” comparative politics is also due to certain “external” causes. It is certainly a reflection of the growing postwar cult of “behavioral” science throughout all the social sciences (taking “behavioral science” to denote (1) middle-range theorizing on the basis of (2) explicit theoretical frames of reference with the use of (3) rigorous, particularly quantitative, procedures for testing the theories). The “behavioral” approach has affected comparative politics primarily through the growing influence upon the fields of sociology and cultural anthropology, and this influence in turn may be attributed in large part (though not entirely) to the very fact of increasing interest in non-Western political systems. For one thing, when political scientists turned to the study of non-Western systems, they found other social scientists already occupying the ground, mostly cultural anthropologists but also a growing number of sociologists (or sociologically trained anthropologists); and so they naturally went to school with them and absorbed their techniques and style. For another, the theoretical equipment of political scientists, such as it was, generally failed them when they confronted political systems unlike the highly differentiated, formally organized, predominantly democratic or totalitarian systems of the West. For this reason also they went to school with social scientists who offered more appropriate theoretical tools and learned to use these tools.

The Emphasis on Setting

Just as the growing interest in non-Western political systems helped to engender a desire for going much further beyond common-sense propositions and common-sense testing procedures, so also it helped to produce – and much less obliquely – the present emphasis on the social setting of politics and on agencies mediating between the social and the political, such as political groups and agencies of political “socialization.” Because political scientists found in such systems much less differentiation between the social and political – that is, few specialized organizations for political decision making or competition – they simply could not help seeing the extent to which the political is embedded in social relations in such systems, or suspecting that it might be so also in the more highly differentiated political system.

If they were indeed confronted by specialized political institutions and agencies, these, like the whole political system, were generally very much in flux – in process of coming into being or being altered. And when political processes are unsettled – when patterns of politics are in the making rather than functionally autonomous of the conditions creating them – the non-political is always particularly obtrusive and apparent, as it was to political observers in Europe in the great age of revolutions from 1789 to 1848 and as it was in the era of the rise of totalitarianism. It is worth noting in this connection that the halcyon days of formal-legalism in the study of politics

fell precisely in that relatively calm and settled period between the great revolutions and the totalitarian era.

Here again, however, we must add other factors leading in a similar direction. We must remember, for example, that interest in the broader setting of politics, and in its more informal aspects, was already well advanced in the prewar period, above all in studies of American politics. In fact, many of the concepts, methods, and interests now being applied in comparative politics came out of the intensive study of American politics in the interwar and postwar periods – not least because of a gradual awareness on the part of specialists in comparative politics that the study of American politics was far outstripping their own specialty. The great role of the Social Science Research Council's Committee on Political Behavior in stimulating interest in applying comparatively some of the insights and techniques developed in American political studies – not least, its important role in helping to bring into being the SSRC Committee on Comparative Politics, which has done so much to help advance the field in recent years – should certainly be mentioned here.

In a way, also, interest in the setting of politics flowed almost naturally from the desire for scientific rigor in the field. It did so in two ways. First, in so far as the pursuit of rigor led to the more intensive study and emulation of sociology and cultural anthropology, it also led to the introduction into comparative politics of broad frameworks of analysis that, on the whole, regard all social phenomena as interrelated and certainly do not concentrate on any functionally distinctive aspect of society as if it were divorced from all other aspects of it.

Second, it is on the whole much easier to develop theories subject to rigorous testing by taking certain social and economic categories and relating them to politics (for example, such easily measurable categories as wealth and economic development, demographic data, occupational distributions – even value-orientation data) than by taking the often unmeasurable “pure” phenomena of politics as such – especially in societies where electoral data, the most easily measurable of all purely political data, are nonexistent, unreliable, or beside the point. As the opponents of rigorous quantitative methods in political science never weary of pointing out, the phenomena of politics, as traditionally conceived, simply do not lend themselves well to rigorous (that is, statistical, logical, mathematical) treatment – but this may (and did) as easily induce political scientists to conceive such phenomena differently as persuade them to give up rigorous methods altogether.

The influence of sociology upon comparative politics can be seen most clearly of all in the postwar emphasis upon a particular constellation of facts in the setting of politics, the facts that Montesquieu referred to as “the general spirit, the morals of a nation” and that have now come to be called “political culture.” This term, as now used, refers in general to politically relevant values (purposive desires), cognitions (conceptions of the nature of reality), and expressive symbols, from language to visual ceremony. It refers

in particular to the “internalized” expectations in terms of which the political roles of individuals are defined and through which political institutions (in the sense of regularized behavior patterns) come into being.

The emphasis upon such “cultural” data is clearly a reflection of the influence upon political studies of the currently dominant sociological frame of reference, the action frame of reference, evolved chiefly by Parsons and Shils, upon the basis of Parsons’s interpretation of Weber, Durkheim, and Pareto. At any rate, the “political culture” approach has been pioneered in comparative politics chiefly by two writers who freely admit their debt to Parsons and Shils. Gabriel A. Almond (who may rightly claim to have originated the concept in political science) and S. H. Beer. It is mainly through this emphasis on “cultural” data that the study of political “socialization” processes has come to be of great significance in the contemporary field, for if the values, cognitions, and symbols defining people’s political conduct are regarded as the primary substratum of their political behavior, then explanations of political behavior must stress ipso facto the processes through which values, cognitions, and symbols are learned and “internalized,” through which operative social norms regarding politics are implanted, political roles institutionalized, and political consensus created, either effectively or ineffectively. This, essentially, is what we mean by political socialization.

At the same time, the concern with political culture helps to explain the emphasis upon the study of political groups, although this emphasis is also a continuation of prewar tendencies and a result of basing middle-range theories about politics upon hard, preferably measurable, facts. The vogue of the group approach to politics reflects the preoccupation with political culture simply in that there are very few societies, even among the most politically centralized, that have homogeneous political cultures, rather than being composed of a variety of political subcultures; certainly there are very few such societies among the emerging or rapidly changing states of the non-Western areas.

Structural-Functional Analysis

Throughout the postwar period, but particularly, as I have pointed out, in very recent years, students of comparative politics have also made increasing use of the perspectives and categories of structural-functional analysis. What, precisely does structural-functional analysis denote in this case? The term certainly cannot be left without explication, even when used in discussions of the fields in which it originated, for structural-functional analysis seems to include a very large, perhaps all-comprehending, variety of analytical questions and procedures. One of its principal exponents, M. J. Levy, has even claimed that, as used nowadays by most sociologists, it is merely another term for “talking prose” – that the structural-functional theorists

do nothing more than state in a particular language what everybody already states in other languages. That may be so, although it makes one wonder why a structural-functional language should then be used at all; but in the postwar study of comparative politics the term does refer to certain specific, though still somewhat heterogeneous, procedures and problems.

It refers, first, to the very definitions of politics: to what, we conceive to be a political system. One can define a political system in two ways: either as a particular set of concrete organizations, such as “governments” or “sovereign states,” or as any social structures that perform whatever we conceive to be the function of politics – that is, any social structures that engage in political activities.

The latter may be considered a structural-functional definition, and this kind of definition of the political system has become increasingly common in the field. We tend no longer to think of political systems solely as sovereign states and their formal subdivisions but as any “collective decision-making structure,” or as any set of structures for “authoritatively allocating social values,” or as structures that perform the function of “maintaining the integration of society,” or as structures that perform the functions of “the integration and adaptation of societies by means of the employment, or threat of employment, of more or less physical compulsion” – and in many other ways in similar vein. Some of these structural-functional definitions, like the first two examples cited, simply define a special activity, whatever its effect upon the larger social unit in which it occurs. Others, like the last two examples, define an activity that is presumed to be a requisite of the viability of a larger social unit. The latter definitions are more strictly characteristic of the style of structural-functional analysis than the former, for the problem of the requisites of the viability of social systems, of their stability and efficient operation, is perhaps the most basic substantive concern of those using the structural-functional approach.

Just as we can define a political system in structural-functional terms, so also we can devise analytical breakdowns of political systems – construct schemes of the elements that constitute them – in such terms, and this again in two ways. One way is simply to define the subsidiary activities that go into the larger activity of politics. In effect, this is what Almond does in breaking down political function into four input and three output categories. The other way is to break down the political function into those subactivities and structures performing the subactivities that are required for the effective performance of the political function, as a viable political system is required for the effective operation of the larger social system. This is what Apter does in breaking down political systems into five “structural requisites,” and this latter procedure also is more strictly characteristic of structural-functional analysis than the former.

The purpose of structural-functional definitions and breakdowns of systems is, of course, to allow one to state and solve certain problems in which structural-functional theorists are particularly interested and that are based

upon their preconceptions of the nature of social life. For all intents and purposes, the problems typical of structural-functional analysis can all be subsumed under a single concern: the impact of any social structure or function upon the larger social unit of which it is a part (or, less frequently, upon any other structure or function to which it is related).

Social structures or functions can impinge upon social systems in a variety of ways. The structure or function under consideration may be a “prerequisite” for the larger (or related) pattern, in that it must exist before the larger pattern can exist. It may be a “requisite” for it, in that it is required if the larger pattern is to be maintained. It may be “eu-functional” if it helps the pattern to persist or “dysfunctional” if it helps to undermine it. Its operation may be “manifest” if it is intended and understood by the actors involved or “latent” if its operation is not intended and understood. Questions about such relations between structures or functions and larger social units are obviously not profoundly different from questions often raised in other terms. There is, for example, little difference between saying that something is a requisite or prerequisite for something else and saying that something is a necessary but not sufficient condition for (or cause of) another.

A distinctive preconception of societies does, however, underlie structural-functional analysis that gives to such questions an import, certain overtones, that they do not possess when raised in the language of causality or other theoretical languages. This preconception is that societies are mutually interconnected wholes, every aspect of which impinges upon every other and contributes something to the viability (or lack of viability) of the whole. Societies, upon this view, are equilibrated units that have a tendency toward inertia and change through the persistent or serious disturbance of any part of their equilibrium. They are “systems” in the technical sense of the term: hence the concern with their functional interrelations.”

It is this preconception of the nature of political systems, and of the way they fit into the larger social setting, that has gradually come to the forefront in postwar comparative politics. With it has come an emphatic interest in structural-functional problems, particularly problems regarding the requisites of any viable (stable, effective) political system or of the viability of certain kinds of political systems (for example, representative democracies) and problems regarding the functional consequences upon politics of other social patterns and upon nonpolitical patterns of political structures and activities.

What explains the present vogue in comparative politics of these preconceptions and problems? To some extent, of course, the very fact that social sciences in which structural-functional analysis is widely used have exerted an important influence upon comparative politics in the postwar period. But there are more deep-seated reasons.

Curiously enough, one of these deeper reasons is connected with the rapidly changing character of many contemporary non-Western systems – curiously enough, because structural-functional analysis is often accused of being

a purely static approach to social science. It is so represented, however, for two bad reasons: one, the very concept of equilibrium is taken (erroneously) to imply immobility; the other, the major social scientists who have developed structural-functional analysis have in fact emphasized static over dynamic studies – most of them have worked in the anti-Marxist tradition, which assumes integration rather than conflict, and consequently inertia rather than constant motion, as the “normal” state of society. But this fact represents a coincidence rather than a logical relation. Indeed, structural-functional analysis, as depicted here, seems perhaps to lead logically (if it leads logically to anything at all) to theories about the coming into being, transformation, and breakdown of societies rather than to static analyses of fixed social states.

Rather than arguing that structural-functional analysis has a logical affinity to static analysis, one should argue that it is likely to produce a particular approach to social dynamics, different from that produced by theories like Marxism or evolutionary theory – an approach that always sees social change as a transition from one static, equilibrated state to another. Marxist and evolutionary theory are perhaps more *inherently* dynamic than structural-functional analysis in one sense: one cannot imagine, in terms of them, any fixed states, any equilibria other than dynamic equilibria, at all. For the structural-functional analyst, a fixed state is entirely possible and even necessary, although it does not rule out the analysis of changes of state.

At the same time, however, theories like Marxism and evolutionary theory make it difficult, if not impossible, to think of rapid, cataclysmic changes in society; note, for example, the great difficulties created for Marxist theorists by the doctrine of “permanent revolution.” Such theories lend themselves chiefly to a conception of orderly, constant flow in social phenomena: one thing leads, never very rapidly or abruptly, to the next, and the whole flow is conceived, but only for heuristic purposes, as a series of “stages” through which societies must always pass in their life-histories. But structural-functional analysis makes it perfectly possible to think in terms of very broad and rapid changes, of one society skipping the stages of growth passed through by another or embarking very rapidly upon a new course of growth, through some large-scale change, however brought about, in one of the functional elements of society. It makes it possible to think of rapid transformations, revolutionary breaks, innovations, and metamorphoses, while other, supposedly more dynamic, approaches make it possible only to think of flows and phases.

Precisely these two characteristics of structural-functional analysis – that it leads to a conception of social change as a process from static states to other static states and offers the possibility of explaining very broad and rapid changes – make it attractive for those concerned with contemporary non-Western political systems. With what kinds of social dynamics do these systems confront us? Certainly not with orderly and constant flow. In such systems one always seems to begin with very static: traditional societies, hardly changed in essential respects for centuries – societies exhibiting “fixed states” in any reasonable meaning of the term. And from such beginnings

one always seems to proceed to the swiftest and most large-scale changes: from tribalism to the nation-state, from agrarian subsistence economics to modern industrialization, from feudalism to socialism; hurricanes of change strike the societies and swiftly transform them in ways that elsewhere took generations, even centuries.

Perhaps we shall find that this is not really an accurate depiction of what is happening in the “developing” areas. Perhaps the large-scale changes that appear to be occurring in them are merely surface phenomena under which more gradual processes of social flow proceed. But rapid metamorphosis from relatively unchanging states does *seem*, to naked observation, to be the essence of their contemporary history. For that sort of dynamism a theoretical approach at once static and dynamic is obviously the most appropriate.

Thus, the very study of rapidly changing political and social systems creates a predisposition toward structural-functional analysis, not despite but precisely because of its affinity for static theory. And thus also the present emphasis on social setting and on theoretical rigor in comparative politics has induced an increasing use of structural-functional analysis and directs the analytical attention, more perhaps than any other approach, to the whole web of relationships of which politics is a part: the social phenomena on which politics impinges and those phenomena that impinge upon politics. Structural-functional analysis is the preeminent approach to the study of social interconnections. The emphasis on rigor has induced structural-functional analysis because it at least offers the possibility of something more than crude, unsystematic description and induction, without committing the theorist to a premature, perhaps vain, search for social “laws” or for “grand theories” in the historicist manner.

Nor does it commit him to a quest for sufficient “causes” in a realm where multicausality and multivariation operate to such an extent that necessary – or favorable – but insufficient conditions of phenomena are perhaps all we can ever hope to find. Structural-functional analysis, from this standpoint, is the preeminent approach to what I have called middle-range theories – theories that go beyond mere description and common-sense generalizations, that are based upon some explicit theoretical frame of reference, that permit some rigor in formulating and testing hypotheses, and that yet do not present ironclad laws or total interpretations of the meaning of social life. Talcott Parsons, whose name is perhaps the most famous of those associated with structural-functional analysis in the contemporary social sciences, defends the approach precisely on this basis:

It may be taken for granted that all scientific theory is concerned with the analysis of elements of uniformity in empirical processes. The essential question is how far the state of theory is developed to the point of permitting deductive transitions from one aspect or state of a system to another, so that it is possible to say that if the facts in A sector are W and X, those in B sector must be Y and Z. In some parts of physics and chemistry it is possible

to extend the empirical coverage of such a deductive system quite widely. But in the sciences of action dynamic knowledge of this character is highly fragmentary, though by no means absent.

In this situation there is danger of losing all the advantages of systematic theory. But it is possible to retain some of them and at the same time provide a framework for the orderly growth of dynamic knowledge. It is as such a second best type of theory that the structural-functional level of theoretical systematization is conceived and employed.

In the first place completely raw empiricism is overcome by describing phenomena as parts of or processes within systematically conceived empirical systems. The set of descriptive categories employed is neither ad hoc nor sheer common sense but is a carefully and critically worked out system of concepts which are capable of application to all relevant parts or aspects of a concrete system in a coherent way. This makes comparability and transition from one part and/or state of the system to another, and from system to system, possible.⁸

Comparative Politics Today: An Appraisal

Because the postwar tendencies in comparative politics are illustrated and analyzed fully in many other essays and books, I conclude this essay without further describing and evaluating these tendencies. And yet, in a sense, we cannot really “conclude” it, for in the contemporary development of the field nothing has really been concluded. It would be nice if we could say that the study of comparative politics, after its many vagaries and tergiversations, had reached at last a new consensus upon concepts, methods, and analytical approaches capable of yielding a broad and precise science of political institutions. It would be nicer still if we could point to the actual existence of such a science. But there is a great distance still to go before this point is reached, and we are unlikely to reach it without further serious modifications of the field. Given its present state, it is quite inevitable that we should end on a note of ambiguity and suspended judgment, primarily for three reasons:

1. The field is today characterized by nothing so much as variety, eclecticism, and disagreement.
2. Disagreement and divergences are particularly great in regard to absolutely basic preconceptions and orientations (in terms of which one *recognizes* “scientifically” valid findings).
3. The tasks contemporary practitioners of comparative politics (especially the more radical ones) have set for themselves are so many and so difficult that they are unlikely to achieve satisfying results without further important changes in their approaches.

Dissent

It should not be supposed that in describing the four main tendencies in present-day comparative politics – structural-functional analysis, the quest for scientific rigor, concern with non-Western systems, and concern with the broader setting of politics – we have in fact described the whole field of comparative politics in the postwar period. Not at all; we have only described what is new and progressive in a field that is in fact to a large extent old-fashioned and conservative. It is important to realize that the stages in the development of comparative politics described here did not unfold in an orderly and episodic manner. In the manner of all things historical, these stages overlapped one another, each leaving within the contemporary discipline a certain residue, a particular style of analysis incongruent with other styles in the field.

In the contemporary field of comparative politics, we can in fact find, not two, but three quite distinctive styles; indeed we can sometimes find them in the writings of a single individual. One is the predominantly formal-legal, morphological, essentially descriptive, and configurative style of the immediate post-historicist period. In any of the established texts in the field (Ranney, Hertz and Carter, Cole, Zink, Neumann) that is essentially what will be found. If any approach is today dominant in the field, it is still this one. The second is middle-range theory based upon common-sense concepts and methods – crude empiricism, unguided by any rigorous procedures or explicit analytical frames of reference. That is what one finds in most of the deliberately comparative and problem-solving works of the present day, like Duverger's *Political Parties*, Rossiter's *Constitutional Dictatorship*, or Friedrich and Brzezinski's *Totalitarian Dictatorship and Autocracy*. The third is the broad and self-consciously systematic style distinctive to the postwar period.

The Concern with Fundamentals

This coexistence in the field of three quite different styles accounts, as pointed out at the beginning, for the present concern in comparative politics with a multitude of pretheoretical and metatheoretical problems. These problems were not raised in earlier times – or not raised with such intensity and by so many people – simply because no one saw anything problematic in them. Political scientists *knew* the proper subject matter of their science: the state. They *knew* what it was most essential to deal with in studying this subject matter: public law. They *knew* how to classify political systems, how to divide them into parts, the nature of the basic units to be used in analysis, and what sort of a finding was a satisfactory and trustworthy finding. Today, precisely because of the variety of approaches in the field, we are not at all sure about these and other basic matters, and so we spend almost as much time and effort in thinking

about the field of comparative politics as we spend in the comparative study of politics.

Nowhere are this self-concern and self-criticism more apparent, and nowhere are the depth and intensity of intradisciplinary disagreement more clearly revealed, than in the two general works about the study of comparative politics so far produced in the postwar era, Gunnar Heckscher's *The Study of Comparative Government and Politics* and Roy C. Macridis's *The Study of Comparative Government*. Macridis and Heckscher – the first speaking for what is essentially “modern” in the field, the second for what is essentially “traditional” – disagree not so much about whether comparative politics is to be a “science,” as may appear to be the case in the readings, but – and this is much more serious – about what a political science, properly speaking, ought to be; and that is the deepest and most frustrating disagreement that can arise in any discipline.

Macridis and Heckscher can speak for themselves; there is no need here to reproduce their arguments. In any case, all the essential issues their arguments raise, explicitly or implicitly, are sketched at the beginning of the essay. But it is essential to note one fact about their disagreements: not only do such arguments impede the development of the field by distracting its practitioners from substantive tasks, they also impede the development of the field because, while such issues are unsettled, one cannot even determine when a field has been developed. All science involves building upon tacit assumptions and silent premises; and this means that the moment such assumptions and premises are made explicit by being argued no science can be said to exist. In such cases one can only have methodology and metaphysics, only prolegomena to study, research designs, conceptual proposals, and the like – preliminaries that now in fact afflict comparative politics in astounding volume. But one cannot have that heaping up of tested theoretical findings (“cumulative research,” in the wishful jargon of modern social science) that we generally think of as science.

Would it then be better not to raise questions regarding basic preconceptions? In the final analysis, it is unnecessary to answer this question one way or the other because we simply have no choice in the matter. Pretheoretical and metatheoretical concerns become significant in fields of inquiry under certain conditions; they arise because it is necessary that they should arise, and once they have arisen they cannot be wished away. One can operate with agreed preconceptions or one can disagree about preconceptions, but one cannot operate without any preconceptions. Therefore, when preconceptions are being questioned, one can only let the questioning take its course until some general understanding is reached, or, better, one can try, by procedural argument or substantive research, to influence others to accept one's own preferred preconceptions and thus contribute to the outcome of the questioning. In one way or the other – by argument, or example – some dominant opinion will sooner or later become established,

but in the meantime one can only leave the analysis of the field open-ended or indulge in prophecy.

The Need for Simplification

What kind of a comparative politics, then, is likely to emerge out of the present disorder in the field? Whatever the final product, one thing seems certain. Even if we confine ourselves to the postwar developments in comparative politics, it seems improbable that a coherent discipline could be built upon concerns so various and complicated as are the present concerns of comparative politics. The most obvious need in the field at present is simplification – and simplification on a rather grand scale – for human intelligence and scientific method can scarcely cope with the large numbers of variables, the heaps of concepts, and the mountains of data that seem at present to be required, and indeed to exist, in the field.

Consider what the contemporary practitioner of comparative politics is supposed to know in order to be au courant with all the mainstreams of his field. He is supposed to be at once a political scientist, a logician, and a methodologist. He is supposed to know a good deal of sociological, anthropological, social-psychological, and general systems theory. His knowledge must (ideally) extend not merely to a specific country, nor even a particular region or type of government, but over the whole universe of political phenomena. He must not only know contemporary politics, but be something of a universal historian as well. And there is even a suggestion that his familiarity with political behavior should extend not only to nation-states but to every social relationship in which authority is exercised, or influence wielded, or the allocation of social values carried out. Certainly, the study of public law, in which scholars of the past made rich and busy careers, has become a mere fraction of all the things he is supposed to study. He must also learn all about informal politics, relate politics to its setting (ecological, social, economic), and be able to deal adequately with attitudes and motivations, with culture and socialization processes. These, obviously, are absurd demands to make even of the highest intelligence, the most retentive memory, the busiest industry, the most versatile manipulator of the skills of social science. They are demands that could conceivably be met by a sensible division of labor in the field, but such a division of labor presupposes some agreement on what is being divided, an accord (which we do not possess) on the desirable nature and direction of inquiry. The fact that at present it is very difficult, perhaps impossible, for any specialist in the field to know just how his work fits into any broader picture makes it necessary for everyone to work essentially according to his own lights, in terms of what he conceives to be the ultimate destiny of the field.

Dissent on fundamentals is thus reflected in lack of focus and definition in regard to “circumstantials” in the field, and in a way this is to the good. In the past, comparative politics had clearly defined boundaries only at the cost of too narrow and perhaps too inconsequential a concentration

on subject matter, formal-legal structure. Any workable approach to the field, particularly at a time when we are concerned largely with relatively undifferentiated political systems, was bound to depart from such a rigidly constricting focus. But what have we to put into its place? If the answer is that we must deal with everything instead, that nothing can be omitted, then we are lost just as surely – indeed, more surely.

The basic need of the field at present, therefore, is focus and simplification. While we can detect searches for simplified approaches in the contemporary literature about the field, these are so far *only* searches. What is more, the usual tack taken in analytical writings on comparative politics is to throw into proposed schemes everything considered in any sense relevant to political study. Thus, students of comparative politics today confront a profoundly serious problem, even a dilemma. They must not focus on formal-legal studies only; we know that from long and disappointing experience. Yet they must not deal with *everything* else – and formal-legal data to boot. They must somehow limit inquiry. Yet the most obvious way to limit political inquiry is to focus on the most obviously political thing there is, as political scientists did in the formative years of their held – namely, formal-legal structure. What, then, are we to concentrate upon? We do not know as yet; that is to say we are not agreed upon a solution.

Author's Note

This essay took stock of the history and condition of comparative politics in the early 1960s. Almost all of it remains pertinent now, although the final section needs much updating.

I called the essay a “perspective” because it is, in a way, a history of comparative politics, without being in any way a survey of writings in the field – not even of the most important writings.

The object of the essay was to illuminate the condition of the field by discussing the main phases in its evolution and the forces that have affected it, in both the present and the past. Had it been written as a conventional history, I would have dealt at length with many more writers, especially such great writers as Tocqueville, Marx, Weber, Mill, and Bagehot. I would have dealt more briefly, or not at all, with minor writers whose chief virtue is that they can illustrate, in an exaggerated way, the character of comparative political studies in their periods and not that they have made any important contribution to such studies. I would have taken some care to distinguish national differences in styles of analysis in the same periods, rather than speaking of comparative political studies only in overall terms. Such national differences have always existed and exist today: English writers on comparative politics, for example, were much less affected by what I call here the formal-legal style than others and have been less affected, perhaps for just that reason, by the contemporary reaction against that style. Finally, I would have taken greater pains to show the extent to which the predominant style of any one period is still practiced, with less emphasis, in the periods that follow, although I try to make clear throughout that the development of comparative politics has not proceeded through mutually exclusive

phases, but has involved instead a continuous heaping up of strata of analysis, if one may put it that way.

Since the distinction between comparative politics and other aspects of the study of politics is rather recent in origin, I should perhaps also point out that much of this essay provides a perspective on the whole study of politics, indeed of the social sciences, as well.

Endnote

This essay, as stated, was written at a still early stage of the “revolution” in comparative politics, but a good deal before the Terror of the Grand Theories discussed in chapter 1. Readers who wish to be brought more fully up to date on that subject might look at Oran R. Young, *Systems of Political Science*. (Englewood Cliffs, N.J.: Prentice-Hall, 1968), and James A. Bill and Robert L. Hargrave, Jr., *Comparative Politics: The Quest for Theory* (Columbus, Ohio: Merrill, 1973).

As I wrote in chapter 1, it seems to me that we have now settled down to a fundamental choice between perspectives to follow in macropolitical inquiry: between culturalist and rationalist points of departure. It would take far too much space here to explain the reasons for this and the exact nature of the perspectives. My greatest regret in the academic work I have done remains, however, the fact, that the envisaged “test” of the more fruitful path to follow, also discussed in chapter 1, was never done. Had it been, we might not now be afflicted, as we are, by attempts to join what, at the basic theoretical level, should be dissociated – attempts that have had results something very like the mess Tycho Brahe, a fine astronomical data-gatherer but a timid and messy theorist, made of his attempt to join Ptolemaic to Copernican astronomy.

In general, I have taken the culturalist tack (for reasons), but I should mention that I do not regard culture as independent of objective structure, though I also do not regard it as merely superstructural. Culture has to come from something, and that, no doubt, is something objectively structural. And it must adapt to structural objective changes (see chapter 7. below). It is, in Paretian terms, a derivative from “residues.” But that view does not provide an easy answer to the question of what to emphasize in our theorizing. In chapter 7, I try to make the point that the culturalist tack can be made more fruitful than theory based on “deeper” structural factors because of cultural “inertia.” Still, no conclusive test of the position exists.

Simplification has also occurred in another, more encouraging, way: a gradual shift, toward macropolitical theories of the middle range (e.g., in Lipset; in the literature on contemporary peasant wars and, more generally, collective political violence; in Inglehart, et al.) From the weight of such work, if not more directly, a result about the value of culturalist or rationalist perspectives might gradually emerge.

Notes

This essay originally was published at the introduction to *Comparative Politics: A Reader*, ed. by Harry Eckstein and David E. Apter (Glencoe, Ill.: Free Press, 1963), 3–32.

1. I use the word “empirical” here in its conventional sense, not technically – that is, not with specific reference to the British school of “empirical” psychology and epistemology.

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2. I refer to Machiavelli's "method" as viewed from the perspective of social science. I refer to neither the quality of his work as such nor the aptness of his method when viewed in the light of his avowed aims—which were *not* primarily to produce social science.
3. These ideas set the tone in social science, but certainly did not monopolize it. That Montesquieu's style of thought was not without influence in succeeding generations can be seen most clearly perhaps in Tocqueville's works, although Tocqueville was far from typical of his own period. Large-scale comparative studies flourished also in this period in fields somewhat peripheral to social science: comparative geography, comparative philology, comparative religion, and comparative jurisprudence; a brief treatment of these, with references to more comprehensive works, is given in Fritz Redlich, "Toward Comparative Historiography: Background and Problems," *Kyklos: Internationale Zeitschrift für Sozialwissenschaften* II (1958): 362–389. Redlich points out that these fields were influential upon some of the earliest nineteenth-century writings in comparative politics, above all E. A. Freeman's *Comparative Politics* (New York, 1874).
4. The emphasis on formal-legal studies undoubtedly varied in this period from country to country, depending on special considerations – whether, for example, the study of politics was an autonomous branch of university life, whether there were serious problems of political socialization, whether the country concerned had a written constitution – although in the field as a whole there was a strong trend toward such studies. Perhaps the main exception to the trend can be found in Great Britain. Here there was no written constitution to analyze; here also the common law tradition, in contrast to the continental Roman law tradition, directed attention to usage and other "informal" aspects of politics. British politics was exceptional and so also, perhaps necessarily, was British political science. Bagehot's *English Constitution* is certainly not a formal-legal study; nor, in the strict sense of the term, is Dicey's *Law and Opinion*. Yet even British political studies were not totally out of the mainstream of development in the field, certainly not so much that we can consider nineteenth-century formal-legal political studies merely a continuation of a long-standing emphasis upon public law in Roman law countries. For one thing, theory in Britain in this period reflects the subjectiveness of political thought everywhere, not only in the case of the political "idealists" but even among the utilitarians. Informal political processes were widely neglected – Bagehot, for example, talks about "party," but, unlike the monarchy, it is not considered deserving of a special chapter. Lacking a constitutional document, British writers on British government tended to treat actual behavior as if one could read formal-legal rules into it and as if one had "explained" it when the formal rules it implied had been made explicit. In this connection, it is worth noting that this period is not only the great period of continental constitution making, but also that of the great codifications of procedure in Britain. Thus, in Britain, there is some tendency toward turning the study of politics into the study of public law, while on the continent the latter practically swallowed the former. British writers did, however, keep alive in this period a broader and more analytical tradition in political study. In this way, they may have contributed to the later revulsion against formal-legal dies.
5. Nor is it to say that narrow, and largely formal-legal studies have nothing to be said for them. Many of them set standards of scholarliness, solidity, and resistance to fads that contemporary practitioners of comparative politics might well emulate.
6. F. M. Sait. *Political Institutions: A Preface* (New York: Appleton, 1938), 135.
7. Most contemporary structural-functional theorists treat this as a point of view from which to analyze societies, not, as was once the case, as gospel truth. The reader interested in the differences between contemporary and the older functionalism should read R. K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1949). chap. 1.
8. Talcott Parsons, *The Social System* (New York: Free Press of Glencoe, 1951), 20.