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Chapter 12 Comparative Politics: An Overview

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I Introduction: The Discipline of Comparative Politics

Ever since Aristotle set out to examine differences in the structures of states and constitutions and sought to develop a classification of regime types, the notion of comparing political systems has lain at the heart of political science.[146] At the same time, however, while perennially concerned with such classic themes as the analysis of regimes, regime change, and democracy and its alternatives, comparative politics is not a discipline which can be defined strictly in terms of a single substantive field of study. Rather it is the emphasis on comparison itself, and on how and why political phenomena might be compared, which marks it out as a special area within political science. Indeed, precisely because there is no single substantive field of study in comparative politics, the relevance and value of treating it as a separate sub-discipline has often been disputed (see the discussion in Verba 1985; Dalton 1991; Keman 1993a).

The discipline of comparative politics is usually seen as being constituted by three related elements. The first, and most simple element is the study of foreign countries, often in isolation from one another. This is usually how comparative politics is defined for teaching purposes, especially in Anglo-American cultures, with different courses being offered on different countries, and with numerous textbooks being published about the individual countries which are incorporated in these courses. In practice, of course, however useful this approach may be in pedagogical terms, there is

[146] See Books II.b and IV.b of Aristotle's The Politics.

often little real comparison involved, except implicitly, with any research which might be included under this heading being directed primarily to the gathering of information about the individual country or countries concerned. Indeed, one of the problems associated with the distinctiveness, or lack of distinctiveness of comparative politics as a sub-discipline is that an American scholar working on, say, Italian politics is usually regarded by her national colleagues as a "comparativist," whereas an Italian scholar working on Italian politics is regarded by her national colleagues as a "noncomparativist." This, of course, makes nonsense of the definition.

The second element, which is therefore more relevant, is the systematic comparison between countries, with the intention of identifying, and eventually explaining, the differences or similarities between them with respect to the particular phenomenon which is being analyzed. Rather than placing a premium on the information which may be derived about these countries, therefore, the emphasis here is often on theory-building and theory-testing, with the countries themselves acting as cases. Such an approach clearly constitutes a major component of political science research more generally, and, indeed, has been the source of some of the most important landmark texts in the discipline as a whole (e.g. Almond and Coleman 1960; Almond and Verba 1965; Lipset and Rokkan 1967; Lijphart 1977).

The third element within comparative politics is focused on the method of research, and is concerned with developing rules and standards about how comparative research should be carried out, including the levels of analysis at which the comparative analysis operates, and the limits and possibilities of comparison itself. Precisely because the act of comparison is itself so instinctive to both scientific and popular cultures, this third element is sometimes assumed by researchers to be unproblematic and hence is neglected. And it is this neglect, in turn, which lies at the root of some of the most severe problems in the cumulation of research, on the one hand, and in theory-building and theory-testing, on the other hand.

Unusually, then, comparative politics is a discipline which is defined both by its substance (the study of foreign countries or a plurality of countries) and by its method (see Schmitter 1993: 171). At the same time, of course, this immediately undermines its distinctiveness as a field of study. In terms of its method, for example, comparative politics is hardly distinctive, in that the variety of approaches which have been developed are also applicable within all of the other social sciences. Indeed, some of the most important studies of the comparative method (e.g., Przeworski and Teune 1970; Smelser 1976; Ragin 1987) are directed to the social sciences as a whole rather than to political science per se. In terms of its substantive concerns,

on the other hand, the fields of comparative politics seem hardly separable from those of political science tout court, in that any focus of inquiry can be approached either comparatively (using cross-national data) or not (using data from just one country). It is evident, for example, that many of the fields of study covered in the other chapters of this book are regularly subject to both comparative and non-comparative inquiries.[147] If comparative politics is distinctive, therefore, then it is really only in terms of the combination of substance and method, and to separate these out from one another necessitates dissolving comparative politics either into political science as a whole or into the social sciences more generally.

Given the impossibility of reviewing the broad span of developments in political science as whole, and, at the same time, the undesirability of focusing on methods of comparison alone, a topic which has already received quite a lot of attention in the recent literature (see, for example, Collier 1991; Keman 1993b; Bartolini 1993; Sartori and Morlino 1991), this chapter will deal instead with three principal themes, focusing in particular on the contrast between the ambition and approach of the "new comparative politics" of the late 1950s and 1960s, on the one hand, and that of the current generation of comparativists, on the other (for a valuable and more wide-ranging review, see Daalder 1993). The first of these three themes, which is discussed in Section II, concerns the scope of comparison, which is perhaps the principal source of difference between the earlier and later "schools" of comparative politics. Although much tends to be made of the contrasting approach to institutions adopted by each of these two generations of scholars, and of the supposed neglect and then "rediscovery" of institutions and the state as a major focus of inquiry, this can be misleading, in that the apparent absence of an institutional emphasis in the 1950s and 1960s owed more to the global ambitions (the scope of their inquiries) of that earlier generation, and hence to the very high level of abstraction at which they constructed their concepts, rather than to any theoretical downgrading of institutions per se. Concomitantly, the rediscovery of institutions in the 1980s and 1990s owes at least as much to the reduction in the scope of comparison, and hence to the adoption of a lower level of conceptual abstraction, as it does to any theoretical realignment in the discipline.

The second theme, which is discussed in Section III, concerns the actual topics and questions which are addressed in comparative political inquiries, and where quite a marked shift in focus can be discerned, with

[147] It is thus interesting to note that when she was preparing the second edition of The State of the Discipline, published in 1993, Ada Finifter was advised that "not only do we need more comparative chapters than there were in the first edition, but all the chapters should be comparative" (Finifter 1993: viii).

much more attention now being devoted to "outputs" rather than to "inputs," and to the outcomes of politics and the performance of government rather than to the determinants of politics and the demands on government (see also Rogowski 1993). This also relates to the changing scope of comparison, in that it clearly makes much more sense to ask whether politics matters—a question of outputs and outcomes—when the scope of comparison becomes restricted to just a small number of relatively similar cases. The third theme, which will be addressed here in Section IV, concerns some of the problems which are currently confronted in comparative research, with particular attention being devoted, on the one hand, to the role of countries as units of analysis, and, on the other, to the use and, indeed, virtual fetishization of indicators. The chapter will then conclude with a brief discussion in Section V of some present and future trends in comparative politics, focusing in particular on the renewed emphasis on context, as well as on in-depth case analysis.[148]

II Scope

Writing in the early 1960s, in a most valuable and broadly based review of the past and present states of comparative politics, Harry Eckstein (1963: 22) noted that comparative politics could then be 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." Eckstein's reference point here was to the early stages of what is often now considered to have been "the golden age" of comparative politics, when a series of major and path-breaking research programs were initiated by Gabriel Almond and his colleagues on the American Social Science Research Council's Committee on Comparative Politics (founded in 1954). And what is perhaps most striking in this characterization, and what was also perhaps the most important feature of the new approach developed by the Committee, was precisely the attention which was beginning to be devoted to "large-scale comparisons." Rejecting the then traditional and almost exclusive emphasis on the developed world, and on western Europe and the United States in particular, and rejecting also the use of a conceptual language which had been developed with such

[148] For an earlier version of some of this discussion, focusing in particular on the comparative method, see Mair 1995.

limited comparisons in mind, Almond and his colleagues sought to develop a theory and a methodology which could at one and the same time both encompass and compare political systems of whatever sort, be they primitive or advanced, democratic or non-democratic, western or non-western. As Almond (1970: 16) was later to emphasize in a subsequent review of work of the Committee and of the development of comparative politics in this period, their strategy had been intended to bring together scholars working on countries across the globe, and to persuade them that they were "members of a common discipline concerned with the same theoretical problems and having available to them the same research methodologies."

The broadening of concerns in a geographic or territorial sense was also necessarily accompanied by a broadening of the sense of politics itself, and, in particular, by a rejection of what was then perceived as the traditional and narrowly defined emphasis on the study of formal political institutions. Indeed, reading the work of the major comparativists of the 1950s and 1960s, one is constantly struck by an almost palpable frustration with the approach to the study of political institutions which had prevailed up to then. Two factors were particularly relevant here. In the first place, the traditional emphasis on institutions was seen to privilege the formal and legal aspects of politics at the expense of what might be termed politics "in practice," and to privilege the "official" story at the expense of what was increasingly believed to be an alternative and "real" story. Thus "realism" rather than "legalism" was to become the keyword for the new comparativists. Secondly, a broadening of the sense of politics was also required in order to incorporate a recognition of less formally structured agencies and processes which spread the scope of the political quite far beyond the formal institutions of government alone. This shift developed directly out of the new global ambitions of the discipline, with the rejection of legalism going hand in hand with the rejection of a primary focus on western polities. Moreover, not only did this new approach allow for a more nuanced analysis of non-western regimes, but it also encouraged the new generation of comparativists to pay attention to less formalized aspects of politics even within the study of the western regimes themselves. Thus students of western European politics were now encouraged to abandon their "formal and institutional bias" and to focus instead on "the political infrastructure, in particular on political parties, interest groups, and public opinion" (Almond 1970: 14).

Global ambitions, and the need to develop a more broadly defined conception of politics and the political system, had two important consequences. The first was simply the beginning of an extraordinarily fruitful

research program in comparative politics, the sheer scale, coherence and ambition of which has since remained unrivalled,[149] and the recollection of which remains enshrined in an image of this period as being the "golden age" of the discipline. "Comparative politics is [now] and has been disappointing to some," noted Verba (1985: 29) in a pessimistic review, "but it is disappointing in comparison to past aspirations and hopes." Since that golden age, it is often felt, the discipline has gone into retreat, with scholars complaining, at least in conversation with Verba, about "division, fragmentation, and atomization in the field . . . [and the lack of] clear direction, leadership, or a commonly held and agreed-upon set of theoretical underpinnings" (1985: 28).[150] Second, conscious that "the challenge facing comparative politics [was] to elaborate a conceptual apparatus in keeping with the vastly extended global scale of its empirical investigations" (Rustow, 1957/1963: 65), there also emerged a new approach to the study of politics which was to be encapsulated within the now much criticized notions of "structural-functionalism." Prior to this, as noted above, comparative politics had been dominated by the study of established, clearly-defined, and economically advanced democratic systems, all of which were more or less characterized by an apparently sharp division between state and civil society, and by a conception of the state which viewed it as composed of specific (and comparable) institutions—executives, parliaments, bureaucracies, judiciaries, military forces, and so on—each playing its own specific role within the system. Global comparisons, by contrast, implied not only the inclusion of non-democratic regimes, but also very underdeveloped countries with so-called "primitive" political systems, in which it was not only difficult to establish the boundary between state and civil society, but in which it was also sometimes almost impossible to identify specific political institutions with a specific purpose.

Along with global ambition, therefore, came the abandonment of an emphasis on the formal institutions of government, and, indeed, the abandonment of an emphasis on the notion of the state itself, which was to become translated into the more abstract references to "the political system." As Almond (1990: 192) later noted, this new terminology enabled scholars to take account of the "extra-legal," "paralegal" and "social" institutions which were so crucial to the understanding of non-western politics, and, as Finer (1970: 5) suggested, was required in order "to encompass prestate/non-state societies, as well as roles and offices which might not be

[149] See, for example, in addition to a lengthy series of important monographs: Almond and Coleman 1960; Binder et al. 1971; Coleman 1965; LaPalombara 1963; LaPalombara and Weiner 1966; Pye 1962; and Pye and Verba 1965.

[150] See also the review by Daalder (1993: 20), who speaks of the "exhilaration" associated with the "political development boom" in the 1960s.

seen to be overtly connected with the state." Moreover, this new language could also serve the interests of those students who remained concerned with western polities, since even here a new wave of scholarship had begun to "[discover] that governmental institutions in their actual practice deviated from their formal competences" and had begun to "[supplement] the purely legal approach with an observational or functional one. The problem now was not only what legal powers these agencies had, but what they actually did, how they were related to one another, and what roles they played in the making and execution of public policy" (Almond, Cole and Macridis 1955/1963: 53). Hence the emergence of structural-functionalism, in which certain quite abstractly defined functions were defined as being necessary in all societies, and in which the execution and performance of these functions could then be compared across a variety of different formal and informal structures.

Since then, of course, this then novel and path-breaking approach has itself been subject to extensive criticism and counter-reaction, with a new wave of scholarship emerging in the 1980s which stressed the need to return to the study of institutions and to restore primacy to an analysis of "the state." If the approach of Almond and his colleagues might be characterized as one which "identif[ied] the subject matter of political science as a kind of activity, behaviour, or, in a loose sense, function . . . no longer limited in any way by the variable historical structures and institutions though which political activities may express themselves" (Easton 1968: 283; see also Fabbrini 1988), then the new approach which began to be asserted in the 1980s was one in which context became crucial, and in which it was precisely the "variable historical structures and institutions" which were now seen to play a central role (Thelen and Steinmo 1992). In the first place, institutions, and the state itself, now increasingly came to be seen as relevant "actors" in their own right, in the sense that they, or those who occupied their offices, were seen to have their own autonomous interests, and were thus also part of "real" politics (e.g. Skocpol 1985; see also Mitchell 1991). Second, and perhaps most crucially, institutions were also seen to have a major determining effect on individual behavior, setting the parameters within which choices were made and through which preferences were both derived and expressed (March and Olsen 1984; Shepsle and Weingast 1987). Third, institutions, and institutional variations in particular, were also seen to have a major effect on outcomes, with the capacity of actors to realize their ends being at least partially determined by the institutional context in which they operated (e.g. Scharpf 1988; Lijphart 1994a).

From one reading, then, we appear to witness an almost cyclical process,

in which institutions, and possibly even the state, are initially privileged as the basis on which political systems might be compared; in which these institutions are later relegated as a result of the prioritizing of "a realism that recognized the processual character of politics" (Almond 1990: 192); and in which they then acquire a new relevance as part of that real politics itself, and as the context which determines individual behavior and performance. From this reading, therefore, we see a series of paradigmatic shifts (Evans et al. 1985), which travel right to the heart of comparative political analysis itself. From another reading, however, the contrasts are much more muted. In a trenchant review of some of the early work of the neostatists and new institutionalists, for example, Almond was at pains to emphasize the real continuities which existed across the different schools, arguing that there was little in this so-called new approach which was not already present, either implicitly or explicitly, in much of the earlier literature, and that its terms were essentially "indistinguishable from 'behavioral' or structural functionalist definitions" (Almond 1990: 215).

But while Almond may have been correct in claiming that the reality underpinning the new terminology is less novel than has been claimed, the conceptual language involved is certainly different, and it is here that the key to the contrast between the two approaches can be found. In brief, it is not a problem of whether Almond and his colleagues neglected the importance of the state and of institutions more generally, or of whether Skocpol and many of the new institutionalists have now redressed that imbalance; this is, in the main, a fairly futile debate. Rather, and returning to the main question, it is a problem of the scope of the comparisons involved. For while Almond and his colleagues were consciously developing a conceptual language which could address the need for global comparisons, even when the particular analysis was in practice restricted to just one case or to just a handful of cases, much of the work engaged in by the more recent comparativists is explicitly adapted for application to a more limited (and often quite unvaried) set of comparisons, be it limited to regions (western Europe, Latin America, etc.), or even, as in the case of Skocpol (1979), Hall (1986), or Scharpf (1988), to just a very small number of countries. The result is that while Almond and his colleagues were required to operate at a very high level of abstraction (see Sartori 1970), developing concepts which could travel to and be relevant for all possible cases, the more recent school of comparativists have contented themselves with a relatively middle-range or even low level of abstraction, in which the specificities of context become crucial determinants (see also below).

It is not therefore a problem of shifting paradigms, but rather a problem of shifting levels of abstraction, which, in turn, is induced by a shifting

scope of comparison. In this sense, as was the case with the structural-functionalist "revolution" in the late 1950s and 1960s, the change is not so much a reflection of developments at the level of theory, but rather at the level of method. For once comparisons become more limited in scope, whether by restricting the focus to one region, or to a small number of cases, it becomes possible to bring into play a degree of conceptual specificity and intensiveness which is simply not feasible at the level of global, all-embracing comparisons. In other words, institutions and the state come back in not only because they are seen to be more important per se, but also because the lower levels of abstraction involved have allowed them to come back in, and have created the room for this type of grounded analysis. In the end, therefore, what is striking about the categories adopted by the structural functionalists is not the fact that they were more process-oriented, or that they were more society-centered, or whatever, which is in any case highly debatable (Almond 1990: 189–218); rather, what is striking about these categories is the enormously high level of abstraction which they required in order to allow them to travel from world to world, and in which institutional specificity was absorbed upward into the more abstract notions of role, structure and function. If institutions and the state have come back into prominence, therefore, it is at least partly because the scope of comparison has become more restricted,[151] and it is this which is perhaps the most striking development within comparative politics in the last two decades or so.

This narrowing of the scope for comparison can be seen in a variety of ways. In the first place, and most practically, it can be seen in the now virtual absence of comparative analyses with a global, or even cross-regional ambition. To be sure, a variety of contemporary textbooks on comparative politics (e.g. Blondel 1990; Hague et al. 1992), as well as a number of established courses,[152] do attempt to remain inclusive, and aim to develop a framework which can accommodate first-, second- and third-world systems. With very few exceptions, however, contemporary research in comparative politics tends to be restricted by region, or even to a very small number of cases,[153] notwithstanding the fact that there now remain few, if any, terrae incognitae. This orientation clearly stands in sharp contrast to at

[151] Although it may also be argued that distinct ideological impulses were involved: see, for example, Almond (1990: 189–218), Mitchell (1991), and Chilcote (1994: 121–76).

[152] See, for example, the discussion about "Teaching Comparative Politics for the Twenty-First Century," in PS: Political Science and Politics, 28 (1995): 78–89.

[153] One relatively recent exception is the study of government ministers by Blondel (1985), which, appropriately enough, is described on the jacket copy as "build[ing] a framework that is devoid of national reference." One possible counterweight to the tendency to limit the number and range of cases may yet be provided by the growing interest in democratization processes, which clearly have a cross-regional relevance (see also below).

least the ambitions which were originally expressed by the Committee on Comparative Politics in the 1950s, and to that earlier work which, even when restricted to just one or a handful of cases, persisted in applying concepts which were believed to be universally valid.

Second, there is an increasing tendency for the profession as a whole to become compartmentalized into more or less self-sufficient groups of, for example, Europeanists, Africanists, and Latin Americanists, with very little communication taking place across the boundaries of regional expertise. In part, this is simply a consequence of the pressures for increased specialization; in part, however, it is also a consequence of increased professionalization, with the critical mass of scholars in the different fields of expertise, and their associated journals, now having grown sufficiently to allow for self-sufficiency. In a somewhat different context, Almond (1990: 13–31) has already famously referred to the development of "separate tables" in political science, by which groups of scholars are divided on the basis of both ideology (left versus right) and method (soft versus hard). Perhaps more realistically, however, we can also conceive of the separate tables being constituted by regional specialists, with their separate European, Asian, Latin American, and African kitchens, and, even within these parameters, being increasingly further subdivided by academic specialisms, with the party people eating separately from the public policy people, and with the local government experts eating separately from those involved in electoral research. For not only has the growth of the discipline acted to cut regional specialists off from one another, but, even within the different regions, it has also tended to foster the self-sufficiency of specialist fields, each with its own narrow network and its own set of journals (or, to continue the analogy, with its own menu), accentuating the trend towards fragmentation which was already regretted by Verba in 1985 (see above, and also Keman 1993a; for a more sanguine view of the process, see Macridis and Brown 1986, and Dalton 1991).

Third, and perhaps most importantly, the methodological debate within comparative politics, and perhaps within the comparative social sciences more generally, has increasingly tended to stress the advantages of "small N" comparisons. Thus, for example, it is quite instructive to compare Lijphart's 1971 review of the comparative method, which devoted considerable attention to ways in which scholars could compensate for, or overcome, the problem of having to deal with just a small number of cases, with a similar and more recent review by Collier (1991), which devoted a lot of attention to the sheer advantages of small N comparisons.

From one perspective, this new attitude can be seen to gell with many of the sentiments expressed by much of the other recent writings on the comparative

method, whether these be within political science, sociology, or history, or even within an attempted multi-disciplinary synthesis (e.g., Ragin 1987; 1991), and which lay considerable stress on "holistic" analysis and on the need for in-depth understanding of particular cases. From another perspective, however, and notwithstanding the shared desire to move away from global comparisons and universal categories, much of this contemporary work in comparative politics might better be seen as consisting of two distinct "schools" or approaches (see also Collier 1991: 24–6). On the one hand, there are those researchers who persist in attempting to derive generalizable conclusions or in attempting to apply generalizable models across a range of countries which, in contrast to the global ambitions of the first postwar generation of comparativists, is usually limited in terms of region or status. On the other hand, there are also those researchers who seem increasingly wary of multiple case comparison, even when limited to a relatively small N, and who stress the advantages of close, in-depth analyses of what is at most a small handful of countries, in which the advantages offered by looking at the whole picture are seen to outweigh the disadvantages suffered by limited applicability.[154] Despite their contrasts, however, there is a sense in which each approach can lay claim to offer the best option for the future. As Collier (1991) notes, for example, recent advances in quantitative techniques now appear to afford a much greater opportunity for statistical analyses across relatively small numbers of cases, and may lend the conclusions derived from such analyses a greater strength and authority.[155] Relatively in-depth qualitative case analyses, on the other hand, despite their obvious limitations, have the advantage of being more grounded, and, at least at first sight, can also prove more sensitive to the insights now being afforded by both the "new institutionalism" and the rational choice paradigm.[156] Indeed, the renewed interest in case studies in recent years, and the associated emphasis on understanding the full context in which political decisions are made, has most certainly been stimulated by the potential offered by these new insights (see also Section V, below).

[154] See, for example, Rhodes's (1994) discussion of state-building in the United Kingdom, which includes a trenchant defense of the capacity of the case-study to produce generalizable conclusions.

[155] As, for example, is the case with the very closely argued analysis by Scharpf (1988) concerning the capacity of governments to implement public policies.

[156] See, for example, Tsebelis's (1989: 119–234) application of his nested games approach in a close analysis of British Labour Party activists, Belgian consociationalism, and French electoral coalitions.

III Questions

In many respects, the broad direction of the questions addressed by comparative political inquiry has remained largely unchanged through generations, and perhaps even through centuries. How might regimes be distinguished from one another? What accounts for regime stability, and what accounts for regime change? Which is the "best" form of government? The attention devoted to these "big" questions has tended, of course, to ebb and flow with different generations of scholarship, with interest being recently reawakened in the aftermath of the recent wave of democratization (see, for example, Diamond and Plattner 1993), and being reflected most obviously in the extraordinary volume of new literature on transitions to democracy and on constitutional engineering and institutional design.[157] Indeed, it is precisely this reawakened interest in democratization, and the search for general patterns and predictions, which may well restore a sense of global ambition to comparative politics, since it is really only in this context that students of developing countries are beginning to reopen lines of communication with those whose field has been largely restricted to the developed west, and that the expertise of students of the former "second world" is finally seen as being relevant to mainstream comparative politics.

But these are clearly the classic themes, the hardy perennials of comparative politics, and once we move beyond these it is possible to see quite important shifts in the sorts of questions which tend to be addressed. In a recent review of the state of comparative politics, for example, Rogowski (1993: 431) noted five trends from the 1980s which certainly appeared to suggest a new research agenda, and which included: "A far greater attention to the economic aspects of politics . . . Increased interest in the international context of domestic politics and institutions . . . An altered and sharpened focus on interest groups . . . A revival of interest in state structures and their performance . . . [and] Further work on nationalism and ethnic cleavages." This is, of course, just one list among potentially many, and even after the lapse of just a couple of years, one might be inclined to relegate the once pronounced concern with, say, interest groups, and give priority instead, say, to the burgeoning interest in transitions to democracy and in the working of democracy itself. Notwithstanding any such qualifications,

[157] For one very good recent example, see the stimulating and very thoughtful debate on the respective merits of presidential systems and parliamentary systems among Linz, Lijphart, Sartori, and Stepan and Skach in Linz and Valenzuela (1994: 3–136). Indeed, it might even be said that it is here that we see the practice of comparative politics at its very best, and hence also, this being a recent debate, the ideal counter-argument against the notion that, in some way or other, the discipline has now begun to pass its sell-by date.

however, what is particularly striking about this list, and what would surely also be common to almost any other such list which might currently be prepared, is the attention devoted to the outputs, or even simply the outcomes, of political processes and political institutions, and hence the attention to politics as an independent rather than a dependent variable. In other words, what is striking here is the sheer extent of concern with the impact of politics rather than with the determinants of politics (see, for example, Weaver and Rockman 1993). This is where the increased interest in political economy comes into play, for example, as well as that in state structures and institutions, whether the latter be framed within a more traditional discourse (e.g. Lijphart 1994a) or within the terms of reference of the new institutionalism (e.g. Hall 1986; Evans et al. 1985).

Here too, then, it is also possible to discern a difference between the new generation of comparativists and that which blossomed in the late 1950s and 1960s. Nor is this simply coincidental, for, at least in part, it is the abandonment of the ambition towards global comparison and universalism which appears to have provided the space in which these new questions can become relevant. There are two steps involved here. In the first place, as noted above, a restriction of the scope of comparison has allowed more attention to be devoted to institutional specificities, and this in itself has helped make it possible to ask whether politics matters. Second, restricting the scope of comparison also means that it now makes more sense to ask whether politics matters than would have been the case in comparisons which attempted to embrace three different worlds, since, in the latter case, and inevitably so, differences in levels of economic development, or even political culture, would have been likely to appear much more relevant (e.g. Castles 1982). Indeed, once comparisons are restricted to relatively similar cases, such as, for example, the advanced industrial democracies, in which the levels of economic development, or the patterns of political culture, or the structures of society are relatively invariant, then the researcher is almost necessarily forced back onto the inevitably varying political structures and processes.[158] And precisely because the possible "determinants" of politics—at the level of economy, (contemporary) culture, or society—in these similar cases do vary so little, these varying political structures and processes then increasingly assume the status of an explanans rather than an explanandum, and thus help to draw attention to enquiries into outcomes and outputs. Whatever the reasons, however, it is certainly true that

[158] As well as, increasingly, onto the variance of historical traditions, where a more nuanced version of political culture and political traditions then also comes into play (see, for example, Castles 1989; 1993; Katzenstein 1984: 136–90; Putnam 1993). The danger here, however, is that an emphasis on the crucial role played by historical traditions may sometimes lead to essentially ad hoc explanations, if not to a degree of fatalism.

comparative political inquiries are now much more likely than before to ask about the difference which politics makes, rather than to ask what makes politics different. In other words, confronted with variation in institutional structures and political processes, contemporary scholars are now much more likely to want to assess the impact of this variation rather than, as before, and most notably in the late 1950s and 1960s, asking why these differences have emerged in the first place, and this clearly does indicate a major shift in the direction of comparative research.

Evidence of this shift can be seen partially in the variety of new trends noted by Rogowski (1993, see also above), as well, indeed, as in almost any reading of the contemporary literature (see, for example, Keman 1993b). It can also be seen, and perhaps more interestingly, in the trajectory of individual scholars and schools of research. Among individual scholars, for example, it is possible to cite the case of Arend Lijphart, who has for long been one of the foremost authorities in the discipline, and whose work has progressed over time from an inquiry into the conditions which gave rise to certain types of democracy to an inquiry into the consequences of certain types of democracy. Lijphart's first major work in the field of comparative politics concerned the elaboration of a typology of democratic regimes, in which the various types identified, and most notably consociational democracy, were defined on the basis of two crucial determining variables—the degree of conflict or co-operation among élites, on the one hand, and the degree of fragmentation or homogeneity in the political culture, on the other, with the latter being located firmly within a conception of social divisions and social pluralism (Lijphart 1968). What is most interesting in this particular context, however, is that as Lijphart's work developed, and as he attempted to modify and build on these initial ideas, the specifically social side of the equation became less and less important, such that in his highly influential depiction of two more generalized models of democracy (Lijphart 1984), the question of the social determinants of the political structures with which he was concerned was essentially relegated to the margins (Lijphart 1984, see also Bogaards 1994). Ten years later, in his most recent work in this field, the change in emphasis was even more evident, with the inquiry now having shifted into the question of the performance of the different types of democracy, and with the question of determinants being almost wholly ignored (Lijphart 1994a).

Similar shifts can also be seen among different schools of research, with the democratization literature offering perhaps the most obvious example of the way in which the explanans has moved from an emphasis on the "objective" social and economic conditions for democracy (e.g. Lipset 1959) to an emphasis on the importance of élite decision-making, on "voluntarism,"

and on the types of institutions and political structures involved. Whether democracy can emerge, therefore, and whether it can be sustained, is now seen to be much less dependent than before on levels of social and economic development and much more dependent on political choices (Rustow 1970), on "crafting" (Di Palma 1990), as well as on the outcomes of rational actions and information (e.g. Przeworski 1991). As Karl (1991: 163) puts it, "the manner in which theorists of comparative politics have sought to understand democracy in developing countries has changed as the once dominant search for prerequisites to democracy has given way to a more process-oriented emphasis on contingent choice" (see also Karl 1991, more generally, as well as Whitehead, chap. 14, below). In a similar sense, the question of the consolidation and sustainability of new democracies is now also seen to be much more closely associated with the actual specifics of the institutions involved (e.g. Linz and Valenzuela 1994). Here, then, as is also more generally the case in a variety of different fields of inquiry in comparative politics, the questions now revolve much more clearly around what politics does, rather than what makes politics the way it is, with the result that, more than two decades after an early but very powerful appeal for just such a shift (Sartori 1969), comparative inquiries are now finally more likely to emphasize a political sociology rather than simply a sociology of politics.

IV Problems

At one level, work in comparative politics is often frustrating. The scholar devotes much time and effort in gathering comparable cross-national data, in ensuring that no relevant factor has been excluded from the analysis, and in building a general and preferably parsimonious model which can explain the phenomenon in question wherever and whenever it occurs, only then to be confronted at some conference or other with some national expert who complains that it's really not like that around here and who then goes on to offer a much more nuanced but essentially idiographic counter-explanation (what Hans Daalder refers to as the "Zanzibar ploy"). At another level, of course, work in comparative politics allows one to be happily irresponsible, in that it is always possible to pre-empt the Zanzibar ploy by prefacing one's broad theory with the caution that while the conclusions are not necessarily true for any particular country, they are nevertheless certainly true more generally. In both cases, however, the real difficulty is essentially the same: although country tends to form the unit of

analysis and observation, the scholar must nonetheless work at one remove from country, and, regardless of whether the number of cases is limited or extensive, must translate a national experience into an operational category. And without wishing to enter into a discussion of the pros and cons of different comparative methods, this immediately presents those engaged in comparative politics with two particular problems.

The first of these problems was already alluded to by Rogowski (1993), and has frequently been highlighted in contemporary discussions of the discipline, and concerns the extent to which country continues to provide a meaningful unit of analysis. One aspect of this problem is the difficulty of identifying what is specific to national politics in an increasingly international environment. Insofar as comparative research does increasingly focus on outcomes and outputs, for example, then it is also increasingly likely to resort to explanations and determinants which lie outside the control of any one national state. To be sure, it is possible to construct a similar-cases research strategy in which precisely the same international environment is common to all the relevant cases, and in which it can then be taken as a given which will not explain any subsequent cross-national variation which might be found (see, for example, Scharpf 1988), but the opportunities for such a strategy are necessarily both limited and limiting (Mair 1995). In any case, to the extent that national institutions and national governments lose their capacity to mould their own national environments, then to that extent the study of comparative politics faces potentially severe problems.[159] A second aspect of this problem concerns the sheer validity of country as a unit of analysis, even regardless of any relevant international context. The difficulty here is posed by the simple fact that countries themselves change over time, and hence in addition to puzzling over cross-national variation, researchers also need to be conscious of cross-temporal variation, in which country A at time X might differ as markedly from country A at time Y as it does from country B at time X (Bartolini 1993). Indeed, this difficulty becomes particularly acute when research is focused on institutional structures, since it is usually at this level that significant changes can and do occur. In other words, if institutions do matter, how can those countries be analyzed in which these very institutions change? One possible solution to this problem which is emerging with increasing frequency is simply the dissolution of country into particular subsets of variables, with the recent study by Bartolini and Mair (1990), and most especially that by Lijphart (1994b), offering useful examples of

[159] And not just comparative politics: note Susan Strange's (1995: 55) provocative suggestion that with the waning of the state as the most important unit of analysis, "much of Western social science is obsolescent, if not yet quite out-of-date."

the gains which can be made by abandoning the notion of countries as single and indivisible cases and by the adoption of multiple observations from each country. The focus of Lijphart's recent study is electoral systems and their political consequences, and it is precisely these electoral systems, rather than countries as such, which constitute the relevant cases in the enquiry. Thus, for example, although France is one of the twenty-seven democracies included by Lijphart in his research, France as such does not constitute one of the relevant units of analysis; rather, the six different electoral formulae which France has adopted since 1945 constitute six of the total of seventy cases which are analyzed in the study (Lijphart 1994b). To be sure, this is far from a novel strategy, and a similar approach has long been adopted in comparative coalition research, for example. Nevertheless, it is an increasingly common strategy, and suggests a much greater willingness to experiment with alternative units of analysis and hence to make provision for cross-temporal variation (Bartolini 1993).

The second problem involved here is perhaps more acute, and involves the reliability of the various measures and indicators which are used in order to translate national experiences into comparable operational categories, a problem which has become even more pronounced as scholars have attempted to build into their analyses measures of variation in political institutions and political structures. Social and economic explanans have always proved relatively easy to operationalize, and in this sense the appeal of "objectivity" in the sociology of politics (Sartori 1969) has always been easy to appreciate, not least because of the apparent reliability of such sources of data as the World Bank, the OECD, the European Union, and even survey research. Once institutions begin to be measured and compared, however, reliability appears to falter, while at the same time hard data—in the sense of data which mean the same thing in every context—often prove unavailable. The result is an endless search for suitable "indicators," and even, at the extreme, the apparent fetishization of such indicators. One useful example of such an approach was the Lange–Garrett–-Jackman–Hicks–Patterson debate which took in the pages of the Journal of Politics in the late 1980s concerning the relationship between leftwing strength, as measured by party and organizational (i.e. trade-union) variables, and economic growth, and which was subsequently cited in a review of recent developments in the comparative method (Collier 1991: 22), as "an exemplar of a methodologically sophisticated effort by several scholars to solve an important problem within the framework of a small-N quantitative analysis." The debate did certainly represent a very valuable and important contribution to comparative political research, and it was also certainly marked by a pronounced

methodological and statistical sophistication, with much of the to-ing and fro-ing between the authors revolving precisely around different methodological approaches. That said, however, it was also striking to see how the initial question of whether economic growth can be associated with left-wing strength was eventually transformed into a problem of statistical technique and case-selection, and how the more fundamental problem of how exactly leftwing strength could be measured and operationalized was essentially ignored. In other words, while the methodology was debated, the indicators themselves were taken from granted. And when one goes back to that debate, and looks to see precisely how these crucial indicators were derived, then one is simply directed to an article from the early 1980s, in which the "the left" is "broadly defined to include Communist, Socialist, Social Democratic, and Labour parties, as well as several small parties that are to the left of centre on a Downsian ideological continuum," and in which leftist strength in government is indicated by the extent to which these parties control government, "as indicated by their control of portfolios in the cabinet," as well as by "the strength of governing leftist parties in parliament" (Cameron 1984: 159), while levels of trade union membership and the organizational unity of labor are based on data reported in the Europa Yearbook (Cameron 1984: 165).

Now, my point is not that these indicators are worthless; far from it—they might well be very solid, and could certainly have been the best that could be found at the time of the original study by Cameron. What must be emphasized, however, is that they are simply indicators; they are not, nor can they ever hope to be, the real thing. And hence if a long debate is to rage in a reputable journal concerning the very important substantive question of whether leftist strength can be associated with economic growth, surely one of the first questions that springs to mind should not be about statistical techniques, but should rather be about the accuracy and reliability of the indicators themselves. For if the indicators no longer offer the best indication of what is supposed to be the underlying reality, then no amount of statistical engineering will result in the cumulation of understanding. Is some notion of the "Downsian left of centre" the most appropriate dividing line to define left and right, or might some other measure not be tested? Is control over portfolios per se the best indicator of governmental influence, or might account not be taken of precisely which portfolios were involved? Might the level of membership in leftist trade unions not offer a more appropriate measure of leftwing strength than membership in trade unions per se, and did the Europa Yearbook really continue to remain the best source of hard, reliable, cross-national data for this crucial variable? In the end, of course, these indicators might well prove to have been the best

possible indicators then available to the contributors to this busy debate; what is simply surprising is that nobody thought to check this out.

There are, of course, numerous other examples which might be cited in which potentially fallible or arbitrary indicators have been accorded an almost biblical status. The Castles–Mair (1984) data on the left–right placement of parties in a number of western democracies, for instance, are generally seen as quite authoritative, and continue to be frequently employed in studies which follow along similar lines to the work cited above. These data probably are authoritative; but it is also possible that they are not, and the picture which they draw, based on a relatively small number of expert opinions in one snapshot sample, should not perhaps be accorded the significance and weight which they normally receive, and should certainly not be automatically assumed to have a validity extending both long before, and long after, their actual application. The same might also be said of the various indicators which were initially developed by Arend Lijphart (1984) as a means of elaborating his influential distinction between majoritarian and consensus democracies, and which have subsequently been incorporated in a variety of different analyses; although these particular indicators may well offer one of the best means by which these two types of democracy might be distinguished, they are not necessarily the only option, and any application of Lijphart's indicators should certainly take account of the specific time period (1945–80) to which they apply, in that a different slice of time can lead to quite a different categorization of the cases (see, for example, Mair 1994). Robert Putnam's (1993) modern classic on Italian democracy is certainly far-reaching in both its argument and its implications, and has been highly praised for its capacity to link patterns in contemporary political culture to their early modern foundations; but even here, despite the intellectual breadth of the study, the key measure of institutional performance on which the analysis depends is based on just a small number of indicators, some of which derive from observations which were taken in the course of only one calendar year (Morlino 1995).

The real problem here, then, as is often the case in comparative political research more generally, is that the analysis of the relationship between variables is assumed to be more important than the quality and reliability of the variables themselves, a problem which has become even more acute as increased priority has been accorded to various institutional and political factors, and their operational indicators. It is also a very severe problem, for despite the evident increase in statistical and methodological sophistication of comparative political research in recent years, and despite the very obvious theoretical ambition, the actual data which are employed

remain remarkably crude (see also Schmidt 1995). And since it is precisely this lack of solid comparable data which is encouraging the virtual fetishization of whatever indicators might be available, regardless of their potential fallibility, it must surely remain a priority for comparative political research to follow the advice laid down by Stein Rokkan on many different occasions, and to continue to stimulate the collection of systematically comparable data which can really "pin down numbers" (cited by Flora 1986: v–vi) on cross-national variation.

V Conclusion: Present and Future Trends

All studies in comparative politics share at least one attribute: a concern with countries, or macro-social units, as units of analysis or, at least, as units of observation (Ragin 1987; Keman 1993a). At the same time, comparative analysis will also often seek to arrive at generalizable propositions, which, in their most extreme form, would seek to explain phenomena whenever and wherever they occur. The inevitable result is a tension between an emphasis on country-specific factors, on the one hand, and universal relationships, on the other. But whereas the then new comparative politics of the 1950s and 1960s tended to place the emphasis on universal relationships, and thus global comparisons, the tendency within comparative research over the past decade or so has been to move away from general theory by emphasizing the relevance of context.

In part, this tendency reflects the renewed influence of historical inquiry in the social sciences, and especially the emergence of a "historical sociology" (Skocpol and Somers 1980; Abrams 1982) which tries to understand phenomena in the very broad or "holistic" context within which they occur (see also Thelen and Steinmo 1992, and Section II above). More general theories, by contrast, are seen to involve the artificial disaggregation of cases into collections of parts which can then be compared cross-nationally, and in which the original configuration of the aggregated "whole" is forgotten (see Ragin 1987: ix–x). Understanding the full picture as a whole and in depth is therefore seen to be preferable to a more general explanation of particular fragments of that picture. In part, however, this return to context is also the result of exhaustion and frustration. When the universe of comparative politics expanded in the late 1950s and 1960s, and when data on more and more countries became available to comparativists, there developed an inevitable tendency to compare as many cases as possible, and research tended to be driven by the elaboration of deductive models

which could then be tested with as big an N as possible. Explanations were then enhanced through either an expansion of data sets, or through a refinement of the explanatory variables, or through a clearer specification of precisely what needed to be explained. Much of the development of coalition theories in the period from the 1960s to the 1980s, for example, can be seen in this way, with an ever more extensive range of countries being included as cases; with more variables being added to the models, such as policy, ideology, governing experience, and so on; and with more precise definitions of what actually constituted a "winning" coalition (see the reviews in Browne and Franklin 1986; Budge and Laver 1992). In a similar vein, much of the work which sought to assess the impact of "politics" on public policy outcomes (e.g. Castles 1982) developed by means of taking in as many cases as possible, and then by enhancing explanatory capacity through the constant refinement of the measures and definitions of "politics" (involving party ideology, party policy, institutional structures, structures of interest representation, and so on), on the one hand, and the measures and definitions of "outcomes" (levels of expenditures, policy styles, different policy sectors, and so on), on the other. In both fields of study, therefore, the goal remained one of explaining the relevant phenomenon in as general a manner as possible, while seeking to improve the capacity to explain by a constant modification of measurement tools.

Most recently, however, this strategy appears to have changed, not least because the capacity to enhance the amount of variance explained has more or less exhausted itself, with a further refinement of the various models now appearing to offer little in the way of explanatory gains. Coalition theorists, for example, now tend to place much more emphasis on inductive models (e.g. Pridham 1986), and are now much more concerned with understanding the broader national context within which each coalition game is played out, while those who are attempting to explain public policy outcomes are now tending to revert much more towards in-depth, case-sensitive, holistic studies. Francis Castles, for instance, who has pioneered much of the best comparative work in this latter area, has recently gone from developing broad, deductive models in which context played little or no role (Castles 1982), to more culturally specific studies in which distinct, but largely unquantifiable "traditions" (the English-speaking nations, or the Scandinavian nations) are accorded an important role (Castles and Merrill 1989; Castles 1993), as well as to more country-specific studies, in which the national context appears paramount (Castles 1989). The result has been a shying away from more generalized models and a renewed emphasis on the deeper understanding of particular cases or countries, where, often inductively, more qualitative and contextualized data can be

assessed, and where account can be taken of specific institutional circumstances, or particular political cultures. Hence we see a new emphasis on more culturally specific studies (e.g., the English-speaking nations), and then nationally specific studies (e.g., the UK alone), and even institutionally specific studies (e.g., the UK under the Thatcher government). Hence also the recent and increasingly widespread appeal of the very disaggregated approaches which emerge within the "new institutionalism" (e.g., Tsebelis 1990; Ostrom 1991).

At the same time, however, it would be largely mistaken to read this recent shift as simply a return to the old emphasis on the study of individual countries which pre-dated the efforts of the 1954 Committee on Comparative Politics, in that there remains one major contrast between the earlier single-country approach and the present rediscovery of context, a contrast which has now begun to play a crucial role in the development of comparative political science as a whole. For whereas the earlier focus on single-country studies was developed at a time when political science itself was at a very early stage of development, and at a time when the centers of disciplinary excellence were concentrated in just a handful of departments in a small number of countries, the present concern with context has emerged following a massive expansion of the discipline in terms of both internationalization and professionalization (Daalder 1993). Formerly, for example, collections of national studies such as that represented by the pioneering Dahl "oppositions" volume (Dahl 1966) were quite exceptional, in that it was only rarely that scholars with expert knowledge on countries or cases could be found and brought together to discuss the application of similar hypotheses to their countries or cases. Nowadays, however, this sort of pooling of resources has become quite commonplace, and forms a core strategy within many cross-national (but usually regionally specific) research projects in a variety of different disciplines. This is particularly the case within comparative politics, where the development of common training methods and paradigms, together with the expansion of formalized international networks of scholars (such as the European Consortium for Political Research, ECPR) have insured that political science scholars, at least in the different regions, have now begun to speak what is essentially the same disciplinary language. As a result, it is now relatively easy, money permitting, to bring national experts together and then to cumulate their knowledge into a broad comparative understanding which is at the same time sensitive to the nuances of different contexts (see, for example, Pridham 1986; Budge et al. 1987; Castles 1989; Katz and Mair 1994; Laver and Shepsle 1994). And precisely because these local experts are being brought together, and then aggregated, as it were, it is proving possible,

through the combination of in-depth and more generalized approaches, to build up plausible, convincing, and yet sufficiently nuanced comparative analyses. In other words, as a result of the international networks and cross-national collaboration which has been facilitated by the professionalization of political science as a whole, case-study analysis is now being adapted to generalizable theories and models, thus offering a strong potential for linkage between these two traditionally distinct approaches. This, then, is the current stage at which comparative political research finds itself: the bringing together of more case-sensitive, context-sensitive groups of studies which, through team effort, and through collaborative group effort, can genuinely advance comparative understanding, and can genuinely contribute to the development of comparative politics.[160] It is, to be sure, a form of comparison which is much more limited in scope than that envisaged by the Committee; perhaps paradoxically, however, and to return to Eckstein (1963: 22), it is also a mode of comparison which seems much better suited to "solving middle-range theoretical problems," even though, as suggested above, these problems are now more likely to concern the consequences of politics, rather than, as Eckstein saw it, its "determinants."

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[160] At the same time, however, this strategy also carries its own dangers. More specifically, the combined effect of both internationalization and professionalization now risks creating an essentially two-tier profession, in which there are the genuine comparativists, on the one hand, that is, those who initiate and design such cross-national projects, and who are then responsible for the cumulative interpretation; and the country experts, on the other hand, that is, those who, time after time, as participants in these projects, interpret their own country or case in the light of the frameworks set by the project initiators. This distinction, to be sure, need not be hard and fast, and those whose job it is to interpret country X for project A may later develop their own projects and recruit their own teams of experts. In practice, however, much depends on the research and training infrastructures within the different countries, such that those national political science professions in which there is a greater emphasis on the need for cross-national research, and those in which funding is available for such research, will tend to produce the project initiators; whereas those in which the focus is more nationally oriented will tend to produce the country experts. It is thus no accident that comparative politics, and even comparative European politics, is now disproportionately dominated by American scholars, who are the principal beneficiaries of the cross-nationally oriented American National Science Foundation.

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