

Regime Type and Diffusion in Comparative Politics Methodology*

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In recent years, several prominent political scientists have argued that quantitative and qualitative methodologies should be seen as united by a single logic of scientific inference. King, Keohane and Verba's seminal book *Designing Social Inquiry* (1994, hereafter referred to as KKV), which jumpstarted this debate, emphasized how scholars might transfer many of the methodological tools of statistics to qualitative research, thus infusing small N comparative case study research with the presumably greater rigour of large N quantitative approaches. But the central goal of unifying quantitative and qualitative methodologies has since been embraced by scholars with quite diverse theoretical orientations (Tarrow, 1995; Van Evera, 1997; Munck, 1998; Coppedge, 1999; Laitin, 2003; Brady and Collier, 2004).

Just exactly how this reconciliation of quantitative and qualitative methodological approaches should be effected in practice, however, remains highly contentious. Scholars sympathetic to qualitative methods have called into question the "statistical worldview" espoused by KKV, with its underlying assumptions that causation in social life is generally linear and that units of observation can generally be understood as independent and homogeneous for analytic purposes (Ragin, 1997; McKeown, 1999). This worldview appears to ignore the intrinsic embeddedness of all observable social phenomena in specific geographical and

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historical contexts, which makes assumptions of unit independence and homogeneity difficult, if not impossible, to sustain (Abbott, 2001; Mahoney and Rueschemeyer, 2002; Pierson, 2002; Ragin, 2000). Relatedly, the statistical worldview tends to downplay the severity of the various problems for causal inference posed by social science's typical reliance on observational as opposed to experimental data (Campbell, 1975; Caporaso, 1995; Brady, Collier et al., forthcoming).

In response, quantitative scholars have noted that advanced statistical techniques might ameliorate or eliminate many of the above concerns. On the most basic level, the simple insertion of dummy variables to account for particular geographical and/or institutional contexts can effectively control for hypothesized effects of regional location. Others have utilized more sophisticated quantitative methods for studying "neighbour effects" on domestic policy change. Time-series analysis can often take into account the existence of "path dependence," as well as noting the "critical junctures" in history where relationships among specified variables may suddenly change. Indeed, in some of his recent work, King acknowledges the importance of the kinds of contextual factors highlighted by various critics of KKV—but without retreating at all from the general assumptions that such factors can be usefully conceptualized as "variables," and that statistical techniques are generally superior to small N case study analysis for establishing causal inferences in the social sciences (King, 2001; King et al., nd.).

For all its promise, the project of uniting quantitative and qualitative methods in political science has thus reached something of an impasse. Participants on both sides of the quantitative/qualitative debate are convinced that this methodological divide should eventually be transcended, but few have abandoned the conviction that their preferred approach sets the standard by which progress in this endeavour should be judged. Evidently, we still lack consensus on precisely where the distinctive strengths of each methodological approach lie, and how these strengths can be combined effectively in systematic investigations of the political world.

In this essay, we argue that a satisfactory synthesis of quantitative and qualitative methods for making causal inferences in comparative politics depends upon the resolution of a prior theoretical problem at the stage of research design: establishing a typology of political regimes and accounting for the mechanisms of their reproduction and diffusion over time and space. In this sense, the solution to contemporary debates about methodology lies in part in the classic literature in the subfield of comparative politics. Indeed, such consensual classics in comparative politics as Barrington Moore's *Social Origins of Dictatorships and Democracy* (1966), Reinhard Bendix's *Kings or People* (1978) and Theda Skocpol's *States and Social Revolutions* (1979) all take as their starting point the pivotal historical influence of modern liberal capitalist states—and of

Abstract. In recent years, several prominent political scientists have argued that quantitative and qualitative methodologies should be seen as united by a single logic of scientific inference. Just exactly how this reconciliation of quantitative and qualitative methodological approaches should be effected in practice, however, remains highly contentious. For all its promise, the project of uniting quantitative and qualitative methods in political science has thus reached something of an impasse. Participants on both sides of the quantitative/qualitative debate are convinced that this methodological divide should eventually be transcended, but few have abandoned the conviction that their preferred approach sets the standard by which progress in this endeavor should be judged. Evidently, we still lack consensus on precisely where the distinctive strengths of each methodological approach lie, and how these strengths can be combined effectively in systematic investigations of the political world. In this essay, we argue that a satisfactory synthesis of quantitative and qualitative methods for making causal inferences in comparative politics depends upon the resolution of a prior theoretical problem at the stage of research design: establishing a typology of political regimes and accounting for the mechanisms of their reproduction and diffusion over time and space.

Résumé. Ces dernières années, plusieurs politologues éminents ont soutenu qu'il faudrait considérer les méthodologies quantitative et qualitative comme étant unies par une même logique de déduction scientifique. Comment réaliser cette réconciliation des approches quantitative et qualitative dans la pratique demeure cependant un sujet hautement contesté. Tout prometteur qu'il soit, le projet d'unifier les méthodes quantitative et qualitative en science politique se retrouve en fait dans une impasse. Les participants des deux côtés du débat quantitatif/qualitatif sont persuadés qu'il faudra un jour transcender cette fracture méthodologique, mais ils sont peu nombreux à avoir abandonné la conviction que l'approche qu'ils privilégient établit la norme qui permettra d'évaluer les progrès accomplis. Il est évident qu'il n'y a pas encore de consensus quant aux forces respectives précises de chaque méthode, ni sur la manière de les combiner efficacement pour procéder à des études systématiques du monde politique. Dans cet article, nous avançons qu'une synthèse satisfaisante des méthodes quantitative et qualitative pour arriver à des déductions causales en politique comparée exige qu'on s'emploie à résoudre d'abord un problème théorique à l'étape de la conception de la recherche, à savoir l'établissement d'une typologie des régimes politiques et l'inventaire des mécanismes de leur reproduction et de leur diffusion dans l'espace et dans le temps.

the main regimes that competed with them for geopolitical dominance—on global political change. Yet with the decline of the various “grand theories” of political and social development—in particular, modernization theory, Marxism and dependency theory—that dominated the social sciences until the 1970s, theoretical attention to the definition of regime types and the analysis of their patterns of diffusion has diminished.

While the absence of any consensual typology of regime types in comparative politics is at the core a theoretical problem, it has profound methodological implications. Quantitative researchers, even while they increasingly acknowledge the importance of controlling for historical and geographic variables, lack any clear method to indicate which of the potentially infinite number of contextual factors affecting a particular dependent variable are actually worth testing statistically. Meanwhile, qualitative comparativists are often forced in practice to construct new regime typologies for each new study, generating a cacophony of competing labels for the same empirical cases.¹ A shared focus on

understanding the evolution of political regimes and classifying regime types in analytically useful ways, we argue, will thus advance methodological debates among statistically oriented researchers and comparative historical analysts alike.

In what follows, we first briefly discuss the origins and nature of the “classical” consensus on the importance of regime types in comparative politics. Second, we show how insufficient attention to the evolution of regimes over time and space tends to produce two interlinked methodological problems for causal inference that have been underemphasized both by KKV and many qualitative comparativists as well: conceptual stretching and “Galton’s problem” (that is, spatiotemporal autocorrelation). Third, we demonstrate how these problems weaken the causal claims of two important books on comparative democracy: Gary Cox’s primarily formal and statistical analysis in *Making Votes Count* and Robert Putnam’s historical and statistical study, *Making Democracy Work*. Fourth, we show how attention to the evolution of regime types and their diffusion reveals an additional methodological problem in comparative politics that is ironically found both in classical approaches and in KKV: namely, a form of selection bias against weak and marginal cases. We conclude with some reflections on the implications of these methodological points for contemporary comparative politics research.

The Classical Approach to Comparative Politics

The comparative politics subfield as it developed in the United States after World War II took as its central theme the problem of explaining the origins, and projecting the future, of the modern capitalist world. In this respect, the subfield was shaped by the three most influential grand theorists of the nineteenth and early twentieth centuries—Marx, Weber and Durkheim—and in particular by the grand synthesis of these theorists presented in the work of sociologist Talcott Parsons (Janos, 1986). For the “modernization theorists” who dominated American social science during the 1950s and 1960s, the rise and spread of modernity appeared to be a generally progressive development, likely to result in the realization of ever-greater levels of material affluence and individual freedom (Rostow, 1960; Parsons, 1964). By the 1970s and 1980s, however, continuing poverty and unrest in the “developing world,” combined with increasing doubts about the future stability of Western capitalism itself, gave rise to a series of critiques of the teleological and functionalist elements of the modernization paradigm, and spurred a revival of Marxist and critical theoretical perspectives on the spread of global capitalism (Wallerstein 1974; Evans, 1979; Evans, Rueschemeyer et al., 1985). Still, neither modernization theorists nor their critics ever

doubted that understanding the dynamics of modern liberal capitalism, and of the anti-liberal regimes that challenged it, was at the very core of comparative politics (Tilly, 1984).

Thus, for comparativists in the classical tradition, the choice of what to study was only partially a question of case selection in order to provide maximum inferential leverage. To be sure, the classical scholars did not reject the basic assumptions that motivate KKV's description of the proper standards of causal inference. Indeed, classical theorists such as Moore, Bendix and Skocpol largely follow KKV's methodological rules of inference: they sample on their independent variables and do not select on their dependent variables (or if they do, they then double-check their causal inferences against additional out-of-sample cases), and they discuss a great many observable implications of the theories in the main body of the argument as well as in interesting asides. But these comparativists were not merely interested in establishing causal "laws" to account for the phenomena that interested them (though these "laws"—in Moore's case, for example, "no bourgeois, no democracy"—are often what people remember). For comparativists in the classical tradition, an equally crucial aspect of case selection involved categorization by regime type.

For example, Barrington Moore chose England, France and the United States (three cases with relatively similar values on the independent and dependent variables) as the first three chapters of *Social Origins* because they represent the archetypes of paths to liberalism. In some sense, of course, this is selecting on the dependent variable (though the inclusion of the other non-democratic cases later in the book is in line with KKV's general advice about how to strengthen causal inferences based upon intentionally-selected cases). Yet Moore's method of comparative-historical research mandates the choice of the most important countries where liberalism first developed, as these cases are uniquely important for understanding subsequent developments in the rest of the world. Moore could have easily chosen to include Holland, Belgium or Switzerland as cases, but his decision not to do so was clearly not accidental, as the following passage shows:

Does not the exclusion of the smaller Western democratic states produce a certain antipeasant bias throughout the whole book? To this objection there is, I think, an impersonal answer. This study concentrates on certain important stages in a prolonged social process which has worked itself out in several countries. As part of this process new social arrangements have grown up ... which have made certain countries political leaders at different points in time during the first half of the twentieth century.... The fact that the smaller countries depend economically and politically on big and powerful ones means that the decisive causes of their politics lie outside their own boundaries. Therefore a general statement about the historical preconditions of democracy or

authoritarianism covering small countries as well as large would very likely be so broad as to be abstractly platitudinous. (Moore, 1966: xiii)

In KKV's terms, then, Moore is quite consciously willing to run the risk of one sort of selection bias—an under-representation of small agrarian societies in his sample of successful liberal democracies—in order to avoid the (in his view) more serious type of selection bias that might emerge because of an insufficient appreciation of the causal impact of the powerful European and North American democracies in shaping the institutions of less powerful neighbouring states. This is a methodological trade-off that scholars less well versed than Moore in classical theoretical treatments of the rise of Western European capitalism might easily have missed altogether.²

A similar sensitivity to the importance of the diffusion of regime types is evident in Bendix's magisterial survey of changing modes of political legitimation in European history, *Kings or People*. Bendix's key independent variables are relative backwardness and degree of intellectual mobilization. He samples on them in a wide range of cases in order to assess their combined impact on differing cross-national ideas of "the people" and "the mandate to rule," and thereby tie early types of legitimation to long-term democratic and non-democratic outcomes. At the same time, however, Bendix is keen to inform his readers that his discrete cases emerge in a process of intellectual diffusion across Europe on a gradient from West to East, and from there on to the rest of the world. Each case of national transformation exercises a decisive influence on intellectual mobilization in the cases beyond its borders. In this way, the mandate of the people that began in sixteenth- and seventeenth-century England rendered kingship less secure in other states. In turn, the French Revolution, by way of the "international demonstration effect," mobilizes intellectuals around the idea of popular sovereignty in every other European and non-European case. Furthermore, the phenomenon of ideological diffusion is not restricted to the "left" side of the political spectrum. On the "right," Meiji oligarchs found inspiration from the ready-made authoritarian models of Prussian bureaucratic reformers. In short, for Bendix the evolution of regime types can be understood only if the assumptions of unit independence and homogeneity are attenuated and the diffusion of powerful regime types is taken into account.

Finally, Theda Skocpol's classic study of *States and Social Revolutions* displays a similar attentiveness to problems of regime type and diffusion. Skocpol's use of Mill's methods of agreement and difference to control for the various independent variables in her study—chiefly, the degree of international pressure on states, the relative power of anti-reform landed elites, and the degree of peasant solidarity in villages—shows her pioneering sensitivity to the kinds of methodological problems

for small N research later highlighted by KKV. Yet Skocpol also makes it quite clear that her study is designed to discover the key causes of social revolution in agrarian empires, not in all regimes; she realizes as well that the diffusion of models of revolution from one regime (such as the Soviet Union) to another (such as Communist China) cannot be ignored in any comprehensive causal account (Skocpol, 1979: 288).

Thus a methodological concern for understanding the key historical “turning points” that have generated enduring political regimes unites most comparativists in the classical tradition. Moore, Bendix and Skocpol are all keenly aware of how much their cases affect each other (which is why all of their cases come in roughly chronological order). All of them take as an analytic starting point that the global success of liberal capitalism set up an unprecedented institutional challenge to the rest of the world, and that the emergence of regimes such as fascism, communism and post-colonial nationalism can be understood at least in part as responses to this challenge. In short, the classical scholars in the field of comparative politics implicitly adopted an evolutionary view of human institutions, and based their selection of cases on the principle that some forms of institutional innovation are more powerful than others in generating the basic structures governing human social interaction for long periods of time and over broad geographical spaces. Such concerns are relatively neglected in recent discussions of comparative politics methodology.

Conceptual Stretching and “Galton’s Problem”

The importance of taking regime type into consideration when selecting cases for comparison can be illustrated by focusing on two interconnected methodological problems that logically arise when this is not done: conceptual stretching and Galton’s problem. Concerning the first of these problems, Sartori’s classic essay remains a touchstone for later work (Sartori, 1970; Sartori, 1984; Collier and Mahon, Jr., 1993; Collier, 1995). Concepts used to generate testable hypotheses in comparative politics research, Sartori argued, are often analytically useful only in the institutional context from which they were first derived and in which they are most frequently employed. Thus scholars with no theory of how different regime types are historically generated and reproduced often assume the theoretical equivalence of formally similar institutions or social arrangements, when upon closer observation they may function in very different ways depending upon the type of regime context in which they are embedded. Sartori cited such examples as studies of “pluralism” in communist systems, which inevitably defined “pluralism” so broadly that no empirical case of “non-pluralism” could possibly exist,

and comparative studies of “political parties” that ignore the liberal ideological context in which “parties” are understood as loyal oppositions rather than self-interested factions (Sartori, 1970; 1976).

Despite their wide-ranging discussion of methodological issues facing political scientists working on cross-national issues, KKV devote very little of their attention to the underlying conceptual issues involved in classification of comparative data (Laitin, 1995). The danger of conceptual stretching is mentioned nowhere in KKV; Sartori’s seminal essay on the topic does not even appear in the bibliography. To be sure, the authors recognize the need for scholars to develop classification schemes of some sort, noting that in both quantitative and qualitative research, “the categories and measures used are usually artifacts created by the investigator and are not ‘given’ in nature. The division of nations into democratic and autocratic regimes or into parliamentary and presidential regimes depends on categories that are intellectual constructs, as does the ordering of nations along such dimensions as more or less industrialized” (KKV, 1994: 152). However, KKV provide very little advice about how comparativists might produce better methods for grouping “nations” or “regimes” into scientific categories—let alone for deciding whether such ordinary-language terms as “nation” or “regime” represent useful categories for any particular empirical study. They merely argue that “[t]he closer the categorical scheme is to the investigator’s original theoretical and empirical ideas, the better” (KKV, 1994: 152)—an approach to classification which by itself fails to distinguish the periodic table of elements from the system of astrological signs, both of which are quite consistent with the ideas of the investigators employing them.

Indeed, as Munck (1998) has pointed out, KKV’s advice to search continually for additional observations that might be brought to bear on one’s theory—however sensible in the abstract—could easily become a formula for conceptual stretching in the hands of less historically sensitive researchers. In some cases, an effort to incorporate “observations from a different time period, or even from a different part of the world” (KKV, 1994: 48) into one’s research may be a good way to test a theory’s broader applicability, but in other cases it may encourage false analogies between essentially different social phenomena. KKV, for example, laud Atul Kohli’s inclusion of the cases of Zimbabwe under Mugabe and Chile under Allende to buttress his argument (based primarily on data from India) that effective anti-poverty policy requires an ideologically-consistent left-of-centre party with strong organizational capacity (KKV, 1994: 219; Kohli, 1989). Both they and Kohli fail to ask the essential question: Do the proposed universal categories utilized in this study—“anti-poverty policy,” “ideology” and “organizational capacity”—all “travel” successfully across regimes in different historical and geographical situations? To decide the answer to this question requires conceptual clarity about the

definitions of these abstract terms—that is, it demands a preliminary taxonomical discussion of the sort that KKV explicitly discourages (KKV, 1994: 48). Arguably, in this instance, greater attention to taxonomical issues would have dissuaded Kohli from placing Mugabe's ZANU(PF) in Zimbabwe in the same category as the Communist Party of India, Marxist in West Bengal, and from drawing the incorrect inference that anti-poverty policy would likely be as effective in the former case as the latter (Kohli, 1989: 234). Indeed, in light of the problem of conceptual stretching, Kohli may well have been wise to devote the bulk of his research effort to examining the process by which anti-poverty policies were generated in different regions within the rather distinctive political context of post-independence India (KKV, 1994: 144–146).

Such concerns have led qualitative methodologists to place increasing emphasis on the importance of classification systems for designing, and making good causal inferences in, comparative historical research (Collier and Mahoney, 1997; Ragin, 2000; Mahoney, 2002). Still, the absence in comparative politics of any shared taxonomy of regime types can lead to problems in qualitative methodology as well. Charles Ragin, for example, argues that in qualitative research oriented toward uncovering “diversity” among types of cases, “population boundaries are not taken for granted, nor are they fixed. Instead, they are fluid. They can be revised up until the very end of a research project, as the investigator's knowledge of cases grows and deepens” (Ragin, 2000, p. 38). Yet such an inductive approach to classification could easily generate as many taxonomies as social researchers. The work of Collier and Levitsky (1997) on “democracy with adjectives” makes an invaluable methodological contribution by formulating a set of principles for refining and/or extending political science concepts, but the authors do not explain how to arrive at good “root concepts” for regime classification in the first place. “Democracy” itself, for example, is famously problematic as a taxonomical category; the unavoidable normative associations of the word led Robert Dahl (1971) to substitute the term “polyarchy” in his regime classification system. Would the comparative study of “polyarchy with adjectives” be more or less revealing than that of “democracy with adjectives”? Or should some other classificatory concept be substituted for both? While it is beyond the scope of this essay to propose and defend a complete alternative theory of regime types, we expect that as comparativists focus directly on the methodological problem of defining regime types and charting their patterns of diffusion across time and space, theoretically satisfying answers to such questions will be more easily attained (Snyder and Mahoney, 1999; Munck, 2001).

If the absence of a theory of regime type can generate conceptual stretching, the absence of a theory of regime diffusion tends to generate a second methodological problem: an insufficient attention to the

inevitable interdependence of one's cases (or, as KKV prefer, observations). Causal inference through the experimental method requires that one's units of observation be wholly independent of one another, which is why the principle of random selection and assignment is so important. Human institutions, however, are arguably never independent of each other in this sense, and KKV underestimate the methodological difficulties of dealing with this problem in non-experimental contexts (Brady, Collier et al., 2004). Since "cases" in comparative politics (such as countries) can often be traced back to a common origin, attempts to test the influence of experimental variables in such cases often fall victim to "Galton's problem"—named after the nineteenth-century anthropologist who challenged statistical approaches to anthropological data that failed to take temporal and spatial autocorrelation into account.³

Recent work, especially in sociology and geography but increasingly in political science as well, in both the qualitative and quantitative tradition, has attempted to address the problem of diffusion in new and innovative ways (Oberschall, 1989; Koopmans, 1993; O'Loughlin, Flint et al., 1994; Strang and Soule, 1998). Much of this work can be conveniently summarized as focusing on either "agent-based" or "structure-based" diffusion. Agent-based diffusion explanations examine how ideas flow across units to inspire political entrepreneurs, who in turn influence outcomes in ways that can be isolated from purely "domestic" effects (Meyer and Hannan, 1979; Keck and Sikkink, 1998).⁴ Structure-based diffusion stresses, by contrast, the facilitating factors of proximity, neighbourhood, the flow of trade and other impersonal forces in shaping outcomes independent of the intentions or cognitive states of the relevant actors (Brams, 1967; Most and Starr, 1989; Sachs, 1997; De Melo, 1998).

Whether agent- or structure-based, diffusion explanations have alerted political scientists to important contextual variables that they might have otherwise ignored. In the qualitative tradition, recent studies of the transnational diffusion of ideas about human rights have demonstrated the ways in which nonstate advocacy networks can generate changes in the behaviour of states, even those that are dictatorial, through appeals to allied NGOs in globally powerful liberal states that can subsequently pressure the target state to democratize (Keck and Sikkink, 1998; Risse, Ropp et al., 1999). In the quantitative tradition, political geographers have argued that the results derived from deploying Gary King's EI method of ecological inference need to be conditioned on various hypothesized spatial dependencies (Anselin and Cho, 2002). Students of democratization have similarly noted how institutional effects often diminish once a country's "neighbourhood" is accounted for (Kopstein and Reilly, 2000). New methods for assessing spatial autocorrelation developed by geographers, such as the Moran's I and the G_i^* statistic, add a degree of contextualization

to statistical models that may alert scholars to the danger of omitted variable bias (Agnew, 1987; Getis and Ord, 1992; Anselin, 1995).

Yet even if careful historical process tracing or sophisticated statistical techniques might allow researchers to account for the temporal and spatial impacts of “regime context” in many instances, unless one already has a developed theory of regime types, one will have no reliable method for operationalizing such contextual variables in the first place. Increasingly, comparativists with insufficient exposure to the classical scholarly debates on how regime types should best be classified tend to assume that “countries” or “nations” can be taken as generally equivalent and independent units; thus most large N research in the subfield uses statistical data compiled by country. But not all countries are created equally. Statistical studies of the pace of market reform in “all 28 post-communist countries,” for example, often neglected to account for the fact that 15 of the 28 were Soviet republics until 1991, and are in many respects decisively influenced by institutional factors stemming from their common historical legacy (Fish, 1998a; 1998b; Hellman, 1998). Thus strong correlations between the “pace of market reform” and “democratization” found in the post-communist region may actually be the spurious effect of variations in state capacity between former Soviet republics and the states of Eastern and Central Europe, variations which account for a country’s success in both democratic and capitalist institution-building (Hanson, 1998; Kopstein and Reilly, 1999). Even inclusion of a dummy variable for “former Soviet republic” status does not completely handle the problem of omitted variable bias here, since the three Baltic republics were incorporated into the USSR only during World War II—with predictable advantages for post-communist liberalization compared to the original Soviet republics. The use of time-series or panel data, too, requires sensitivity to historical turning points when institutions and social forces are undergoing rapid change; thus surveys about Russian attitudes toward “democracy” in 1990, 1992 and 1995 elicited very different response patterns based upon the different political meanings attached to the word just before the Soviet collapse, during the initial post-communist euphoria about Westernization, and a few years later when hopes of a quick democratic “transition” had been dashed (see, for example, the progressively more pessimistic assessments of Russian public opinion in Gibson, 1993; 1996; 1998). With enough contextual knowledge of the dynamics of diffusion of the Soviet regime type over time, proper statistical tests of institutional and structural causes of change in the region can be developed—but the former is necessarily preliminary to the latter (Ekiert and Hanson, 2003).⁵

While KKV do not discuss “Galton’s problem” per se, they do devote sustained attention to the methodological challenges posed by the

inevitable embeddedness of social science evidence in particular historical contexts:

The problem of bias when the selection of cases is correlated with the dependent variable is one of the most general difficulties faced by scholars who use the historical record as the source of their evidence, and they include virtually all of us. The reason is that the processes of “history” differentially select that which remains to be observed according to a set of rules that are not always clear from the record. (KKV, 1994: 135)

They approvingly cite the work of Charles Tilly on state-formation in early modern Europe, who points out that the ubiquitous studies of modern European states such as Germany, France and Britain have already selected cases of institutions that survived the competitive process that eliminated alternative political units over the past several centuries (Tilly, 1975). But if the comparative-historical approach advocated by Moore, Bendix and Skocpol—or indeed Tilly himself—is correct, then essentially *every* phenomenon of interest to social scientists is affected by historical selection processes of this sort. If so, it is always crucial to note the historical origins of the institutional environments one is studying in order to specify properly the “scope conditions” under which one’s proposed causal inferences will likely apply. Thus, KKV’s tendency to downplay the methodological importance of classifying regime types and charting the dynamics of their diffusion across time and space tends to obscure some widespread and serious problems of inference in the comparative politics field. This omission, moreover, flows logically from KKV’s neo-positivist skepticism about the value of conceptual taxonomies for orienting scientific research (McKeown, 1999).

As might be expected, qualitative methodologists interested in comparative historical analysis have been more prone to emphasize Galton’s problem as a particularly sticky issue for conventional statistical approaches in the social sciences (e.g., Tilly, 1984, p. 22; Hall, 2002, p. 382n7). Recent work pinpointing the causal mechanisms that generate historical “path dependence” through dynamics of increasing returns or “reactive sequences” surely mark important advances in our understanding of just how local events at particular “critical junctures” can generate broad, long-term political and social patterns of human interaction (Collier and Collier, 1991; Pierson, 2000; Mahoney, 2000). But there is much work to be done in teasing out the implications of this approach for the methodology of case selection in comparative politics. As Rueschemeyer (2002, pp. 326–7) points out, “Historically delimited explanations—historically delimited in the causal patterns they claim and in the domains within which the causal relations are said to hold—inevitably involve an endless number of unspecified conditions because unique historical phenomena are inherently

related to everything under the sun.” In this respect, a theory of regime types and their diffusion over time and space is necessary in order to differentiate the creation, consolidation and expansion of novel types of formal social organization—such as capitalism, communism and fascism—from event sequences of merely local and episodic significance.

Regime Type and Diffusion in Contemporary Comparative Politics

The methodological problems discussed in the previous section, we believe, crop up in a wide variety of contemporary studies in the field of comparative politics. In this section we illustrate the point through an examination of two justly praised recent books on the sources of democratization and democratic institution building: Gary Cox’s *Making Votes Count* and Robert Putnam’s *Making Democracy Work*. Both of these books, we emphasize, represent valuable and enduring contributions to scholarship. They both do an exemplary job of utilizing the sorts of suggestions made in KKV for solidifying their causal claims. Putnam’s work, in addition, makes use of qualitative concepts of path dependence to buttress his theoretical argument. Yet a greater appreciation for the causal influence of powerful regime types and their diffusion over time and space, we think, would have sharpened their causal inferences and further increased their value for comparativists.

Cox’s Making Votes Count

Gary Cox’s award-winning book on the dynamics of electoral systems marks a dramatic advance in research on institutional rules and political outcomes in the world’s democracies. Cox’s commitment to empirical testing of hypotheses derived from formal models is particularly impressive. In fact, Cox implicitly follows many of the methodological suggestions of KKV. He makes a clear effort to find ways to multiply the number of observable implications of his theory; he attempts to replicate previous studies with different data; and he presents his findings with appropriate scientific modesty. His careful attention to the microfoundations that theoretically generate his macropolitical findings is also welcome. In all these respects and more, Cox’s scholarship sets a high standard for political science research that should be emulated by others. Nonetheless, his central argument—that a modified form of “Duverger’s Law” interacts with the number of social cleavages in a given polity to generate the number of parties competing and governing—might well have been strengthened by more sustained attention to the problem of regime types and diffusion emphasized by classical scholarship on the origins of liberal democracy.

Duverger’s Law in its original form was the proposition that single-member district elections tend to produce bipartism (Duverger, 1954;

Riker, 1976). A corollary hypothesis was that proportional representation (PR) systems would tend to generate multipartism. Cox develops several ingenious formal models to draw out the logical implications for strategic voting and strategic entry of a wide variety of real and proposed electoral systems, refining Duverger's original arguments to show that district magnitude places only an upper bound on the number of parties that can compete effectively in a district. Below this upper bound, Cox argues, the number of parties will vary according to the number of social cleavages that exist in a given society.

Cox's argument represents a pathbreaking effort to combine the insights of neo-institutional analysis with the more sociological approaches to party formation developed by such scholars as Sartori (1976) and Lipset and Rokkan (1967). Yet a closer examination of the empirical evidence presented in support of Cox's argument in light of the problem of regime types and institutional diffusion suggests that further steps are necessary to unite the two perspectives. To begin with, Cox's definition of "regime type" is wholly institutionalist: he assumes that "states" are independent units, and he further assumes that they can be scientifically categorized according to their formal institutional rules alone. No attention is paid to the question of whether Cox's chosen independent and dependent variables capture theoretically analogous empirical phenomena in diverse global contexts—that is, to the problem of conceptual stretching outlined by Sartori. Were the 10.32 "effective electoral parties" in Ecuador in 1984, for example, really comparable to the 5.25 "effective electoral parties" in Denmark in 1985?⁶ In both cases, multimember districts might seem to be generating multipartism, as the institutionalist approach would lead us to expect—but the *kinds* of "multipartism" generated in these different geopolitical and social contexts might well differ in systematic ways which a fully neo-institutionalist presentation obscures.

Cox might well reply that nothing in his argument precludes a more detailed examination of types of multipartism; the finding that electoral district magnitude places an upper bound on the number of effective parties across geopolitical and social contexts is surely significant, even if parties function differently in different places. Yet because Cox neglects the problem of institutional diffusion, this causal inference itself suffers from the form of omitted variable bias generated by Galton's problem. In fact, nowhere in Cox's book does the author explore the potential causal impact of geographical placement or historical legacies on either electoral rules or party formation. This leaves open the very real possibility that the relationship between the latter two variables is spurious: historical legacies and institutional diffusion may well account for both the adoption of particular electoral institutions and the emergence of two-party or multi-party systems.

Cox's failure to examine this counterargument is odd, as he is careful at the outset to deal explicitly with various sociological criticisms of neo-institutional theory. Cox rightly dismisses those "sociological" theorists who simply assert that scientific generalization of any sort is impossible in political science. Yet his argument against the most powerful and widespread sociological critique of Duverger's hypotheses—that electoral rules are themselves a product of pre-existing social forces—is surprisingly weak. First, Cox points out that the sociological argument still assumes that electoral laws "matter," since otherwise powerful social groups would not bother to try to change them in their favour. This is certainly true, but irrelevant to the argument that the observed correlation between electoral rules and the number of parties may be the spurious effect of preexisting social forces at the time electoral institutions were designed. Next, Cox admits that where electoral rules are perceived to be unstable, their ability to constrain the behaviour of powerful social actors disadvantaged by such rules will diminish or disappear; he insists, however, that "in fact electoral laws are not everywhere and always easily changed" (Cox, 1997: 18). This is also true, but sociological theorists do not argue otherwise; rather, they insist one must therefore gauge the degree to which electoral rules are considered stable or unstable in a country when assessing their influence over political outcomes (e.g., Bunce and Csanadi, 1993). Cox's sample, instead, consists simply of all countries listed as "democratic" by Freedom House around the year 1985, whether their electoral institutions were perceived by local actors as stable or not. Third, Cox points out (correctly) that both social structure and institutional rules theoretically may be, and empirically surely are, simultaneously important. This is a welcome call for both institutionalists and sociological theorists to avoid turning the other approach into a straw man, but again does not address the problem of the potential spuriousness of the correlation between electoral laws and multipartism.⁷ Finally, Cox illustrates the importance of institutional rules by examining differences in the number of parties in upper and lower houses in countries where electoral rules for the two houses differ, arguing that the observed variance obviously cannot be a product of differences in social forces. However, since almost all of the variance here is explained by the mechanical effect of translating votes into seats, this comparison does not exclude the possibility that the effective number of *electoral* parties (rather than parties in the legislature) is generated by sociological rather than institutional variables.

A large number of potentially important sociological variables have been discussed in the literature on comparative party formation. Here we restrict ourselves to the question of whether the diffusion of institutions from powerful regimes might have affected both Cox's independent and dependent variables, rendering the correlation between them spurious.

To test this alternative hypothesis, we reexamined Cox's key regression analysis of the sources of multipartism in 51 contemporary democracies (p. 215). In this regression, shown in model number 1 in Table 1, Cox tests the relative effects of electoral district magnitude, the percentage of assembly seats allocated to the upper tier of the legislature, the temporal proximity of legislative and presidential elections plus an interaction term to account for the number of presidential candidates, the number of ethnic groups in the polity, and the interaction between district size and multiethnicity. Cox finds that while the number of ethnic groups by itself fails to be a significant predictor of the number of parties in a polity, all of the above institutional variables have a statistically significant association with the number of effective parliamentary parties; moreover, the

TABLE 1
Dependent Variable: Effective Number of
Electoral Parties

Independent variable	OLS model Standard errors in parentheses		
	1	2	3
Constant	1.77 (.55)	3.74 (.34)	4.17 (.28)
LML	.47 (.13)	—	—
Upper	4.25 (1.73)	2.30 (1.94)	—
Proximity	-4.09 (.959)	-.405 (1.02)	—
Proximity*Enpres	1.64 (.29)	1.53 (.31)	—
Eneth	.45 (.29)	—	—
Britcol	—	-1.11 (.44)	-1.67 (.45)
Adjusted R ² =	.504	.448	.200
N =	51	51	51

Source: Cox, 1997

Legend for variable names

LML = Electoral District Magnitude

Upper = % of seats allocated to upper tier of legislature

Proximity = temporal proximity of legislative and presidential elections

Proximity*Enpres = interaction term to account for number of presidential candidates

Eneth = the number of ethnic groups in the polity

Britcol = dummy variable for former British colony status

interaction between multiethnicity and district size is also a significant predictor of multipartism (shown in model 1).⁸

Cox interprets these findings as showing that the number of seats per electoral district interacts with the number of underlying social cleavages—as well as other formal features of the electoral system—to shape decisively the scope of party competition in a given polity. Where electoral district magnitude is small enough—that is, where it approaches the SMD system—the party system tends toward bipartism; where electoral district magnitude is greater, social cleavages such as those generated by ethnic differences can express themselves in multiple party organizations. In effect, if Cox's reasoning is correct, even an extremely fractured society can induce bipartism through the adoption of an SMD electoral system. All of this sounds very much like a ringing confirmation of the basic intuition underlying Duverger's Law.

Yet Cox fails to control for any of the historical or geographical variables suggested by the classical approach to comparative politics. Indeed, new research suggests that institutional diffusion plays a crucial role in explaining the adoption of electoral rules in different countries. Cox himself cites an important article by Blais and Manicotte (1997) that demonstrates statistically the significance of "status as a former British colony" on the adoption of SMD winner-take-all systems. Blais and Manicotte also argue that the adoption of PR is far more likely in continental Europe and, through a process of diffusion, Latin America, since "South American constitutional lawyers were trained in continental Europe and looked to Europe as a model for the choice of an electoral system" (Blais and Manicotte, 1997: 113). This leaves open the possibility that the strong and significant causal effect of the interaction of ethnic cleavages and multimember districts on the number of electoral parties discovered by Cox might disappear were the regression to be rerun with appropriate geographic and historical dummy variables. In other words, the proposed independent variable "permissive electoral system with many social cleavages" may in effect be a proxy for "democracy that is not a former British colony." Former British colonies may have ended up with SMD plurality systems and bipartism due to their common patterning after the Westminster model; in addition, many of them contain fewer politically salient ethnic cleavages due to the nearly total political hegemony of English-speaking migrants (e.g., the United States, New Zealand, Australia) or their relatively small population size (e.g., the former British colonies in the Caribbean). Democracies in the rest of the world may have ended up with PR and multipartism due to the diffusion of the continental European system, combined with the fact that most of these nations contain many politically salient social and ethnic cleavages. It follows that Cox could have tested for endogeneity by examining the effects of former British

colony status on the value of his dependent variable—the number of effective political parties.

Model 2 in Table 1 presents the findings of our efforts to re-run Cox's regression analysis with the removal of his featured institutional variable, district magnitude, as well as the main sociological variable for ethnic divides, and the insertion of a dummy variable for former British colony status.⁹ Although a part of the fit is lost, this amount is surprisingly small—six per cent of the explained variation—given how blunt this dichotomous variable is as a measure of imperial legacy and that it has substituted for two highly refined continuous variables. In fact, the model continues to fit quite well. Were we to devise a more refined version of a variable for colonial legacy, it stands to reason that the fit might be as good or even better than the combination of many of Cox's institutional variables. What this indicates is that classifying a country as having permissive electoral rules and many ethnic divides may indeed be the same thing as saying "country that was not a former British colony."

At a minimum, attention to the diffusion of important regime types in Cox's case, as demonstrated by our alternative model, suggests that more research is required to disentangle historical institutionalism from rational choice institutionalism in explaining the number of effective electoral parties. Yet in a book that presents by far the most wide-ranging and systematic examination to date of how electoral laws affect democratic politics, Cox never tests for the possible impact of powerful regime types and their diffusion across time and space—precisely the factors initially highlighted by classical works on comparative politics such as those of Moore, Bendix and Skocpol.

Putnam's Making Democracy Work

Robert Putnam's *Making Democracy Work* (1993) has been one of the most influential books in comparative politics in the last two decades. It revived the political culture tradition of empirical inquiry and has spawned important parallel research programs on civic involvement in the United States (by Putnam and others) and a large number of industrialized and developing countries. At the same time, the book is pedagogically useful because it employs almost every contemporary line of inquiry in political science: survey research, neo-institutionalism, rational choice and comparative history. Furthermore, the argument is straightforward: Italian regions with high levels of civic involvement—predominantly in the North—consistently provide better government services and a more comprehensive range of public goods than those regions—predominantly in the South—where civic ties remain weak and patron-client networks are pervasive.

Putnam stands the traditional narrative of Italian politics on its head by arguing not that the economic backwardness of the South has led to a pervasive pattern of bad government but rather that the weakness of civic traditions in the South, and the resultant dearth of “social capital,” has produced a vicious cycle of bad government and poor economic performance. At the same time, the strong associational life of northern Italy produces good government that provides the necessary public goods to foster economic development. In sum, political culture determines economic and political development, and not the other way around.

From the standpoint of KKV there is very little to criticize in Putnam’s book at the level of research design (although his work has been criticized in its interpretation of its own survey results and statistical analyses [e.g., Goldberg, 1996]). Indeed, KKV single out *Making Democracy Work* as an exemplary model of how to combine quantitative and qualitative research (KKV: 5; Tarrow, 1995). Putnam uses the entire range of Italian regions to sample on his independent variable of civic involvement in order to show that governmental performance correlates much better with associational density than it does with levels of economic development. He generates as many observations as he can, given the limited number of regions, and has extended the observable implications to the United States. Of course, as with any important book, Putnam’s study has been subject to attack from any number of angles. Our intent here is not to retrace these criticisms but to reexamine some of its findings in light of what we have said about the importance of the diffusion of powerful regime types. As we shall see, Putnam does have some sense for spatial diffusion of norms and institutions but his theory is mostly devoid of any meaningful discussion of regime types. Indeed, it appears he is uninterested in the typical regime types that interest comparativists—liberalism, authoritarianism, communism or even (strange indeed for Italy) fascism (Tarrow, 1996).

Why is this the case and how does it affect Putnam’s work? We will restrict ourselves to a consideration of his controversial chapter 5, which offers an explanation for the variation in associational density between the North and the South of Italy.¹⁰ After establishing that associational density correlates nicely with the provision of public goods, Putnam wants to provide an explanation. He settles on an account stressing the long-term continuities in Italian political development. Putnam’s logic is one of path dependence, with the critical juncture in Italian political development to be found in the late medieval period. Two types of solutions to the problem of medieval chaos were available to Italians: autocratic feudalism and civic republicanism. For a whole host of contingent reasons, the former took root in the South and the latter in the North. From then on, the history of regional disparities in Italy is set. No matter which kind of regime type (macroparasite, to follow McNeill, 1976) or disease

(microparasite, again McNeill) dominated a region, the North maintained its traditions of civic republicanism and the South its autocratic clientelism. Thus it is of little interest whether Italy was divided, liberal, fascist, a nation-state or whatever. What determines these differences in today's Italian democracy is the same thing that determined them for the last 600 years: civic and associational activism. Putnam might therefore have just as well titled his book *Making Government Work*, as there is really very little about democracy per se that affects the value of the dependent variable (Levi, 1996).

Nor does Putnam attend to the possibility that his independent variable might also be pitched at too high a level of abstraction to take into account the various types of "civil society" associated with different Italian regimes. Indeed, a closer look at more recent historiography of the crucial turning points in Italian state development suggests many problems with Putnam's assumption that "civicness" has meant essentially the same thing from the medieval period to the present. First, as Sabetti (1996) has pointed out, a new revisionist literature has highlighted equally active civic traditions inherited from the medieval period in the most important southern Italian cities, as well as forms of peasant cooperation in the *mezzogiorno* indicative of "virtuous circles" of trust and reciprocity. If these "southern" forms of civil society nevertheless failed to produce the "good governance" observed in the North, we require some theory that distinguishes among different regional forms of "civicness" to explain why. Second, Putnam ignores the fact that dense civil society networks oriented toward anti-liberal ideologies frequently undermine democratic regimes, rather than reinforcing them—as was the case in Weimar Germany, where the Nazi takeover of power was greatly facilitated by the presence of local organizations of conservationists, choral groups and the like that could be mobilized for fascist purposes (Berman, 1997; Hanson and Kopstein, 1997). Indeed, a parallel process took place in liberal Italy from unification to Mussolini's takeover of power: Italian liberals in this period largely failed to create an active network of liberal civil associations linking the regime to society, while, as in the case of the Weimar republic, anti-liberal socialist, Catholic, and fascist elites had much greater success in welding civil society to their respective ideological causes (Hine, 2001). It is crucial in this regard that the fascist movement enjoyed its greatest early successes in the very centres of northern Italian "civil society" lauded by Putnam. Even today, networks of trust and reciprocity in leading northern centres of entrepreneurship, such as Milan, coexist with and are even buttressed by general distrust of outsiders such as immigrants from the South (Cento Bull, 1996). Attention to taxonomical issues, then, calls into question both sides of Putnam's proposed correlation between "civil society" and "democratic

effectiveness,” and forces us to examine more disturbing causal linkages between local civic networks and regime outcomes (Chambers and Kopstein, 2001).

An extended consideration of the impact of the diffusion of regimes over time and space is similarly absent from Putnam’s causal argument. Indeed, Putnam’s assumption that Italian regions pass the test of unit homogeneity is made quite explicit: “The Italian regional experiment was tailor-made for a comparative study of the dynamics and ecology of institutional development. Just as a botanist might study plant development by measuring the growth of genetically identical seeds sown in different plots, so a student of government performance might examine the fate of these new organizations, formally identical, in their diverse social and economic and cultural and political settings” (Putnam, 1993: 7). But a deeper engagement with the historiography of Italian political development illustrates that Putnam’s natural “experiment,” like Cox’s test of Duverger’s law, fails to account for Galton’s problem.

In particular, Putnam does not tie the development of Italy’s regions to the broader context of European politics. As elsewhere in Europe, after all, it was Napoleon’s conquest of Italy in the early nineteenth century that first put the problem of constructing a unified Italian nation-state squarely on the political agenda. Ideas, too, diffused from North to South; Italian democratic radicals like Mazzini were directly inspired by the Jacobins, while centrist liberals looked to the Orleanist parliamentary monarchy of 1830 as a model of democratic development (Lovett, 1982). The spread of democratic activism from below among Italian workers and intellectuals took place in waves of protest synchronized with rebellions elsewhere in Europe, cresting in revolutionary years such as 1830, 1848 and 1870 (Tilly, Tilly et al., 1975: 247). All of this suggests that the origins of Italian democracy cannot be studied apart from broader trends in general European development.

But these broad trends had a dramatic social impact on the entire Italian peninsula, and the Risorgimento united rebels and democrats from both North and South. How, then, did the social trajectories of the two regions begin to diverge so greatly? Crucial to understanding the mid-nineteenth-century (not medieval) origins of civic “distrust” of the Italian state in much of the South is the fact that the eventual institutional consolidation of the new unified Italian state took place through conquest by Piedmont, and subordination to that northern region’s model of centralized state bureaucracy, carried out by northern elites who generally feared and disrespected the southern population. The rapid industrialization of northern Italy in the 1890s—again, largely a result of the region’s geographic proximity to the major markets and centres of technological innovation on the European continent—along with subsequent mass migrations from the South to the North further

exacerbated regional social and economic disparities. Internally divided southern elites did their best to lobby for state protection from threatening domestic and international economic competition, but only at the cost of cementing the long-term dependence of much of the southern economy on central subsidies—a system further reinforced by fascism. In the postwar period, too, the wealthy North was far better positioned than the South to take advantage of the dynamic new free trade zone of the European Community; at the same time, the experience of national reconstruction under Allied supervision after World War II, and especially the creation of pan-European liberal institutions, tended to rechannel northern Italy's civic activism away from enmeshment in the fascist state and back in a pro-democratic direction—as occurred concurrently in West Germany. All of these forces combined to ensure that the enormous social gap between the North and the South would endure throughout the twentieth century and beyond.

None of these geographical and geopolitical factors, however, are examined in any depth in *Making Democracy Work*. Putnam's extended attention to contextual and historical factors is thus strangely acontextual and ahistorical. Of course, deep causal explanations are perfectly legitimate but, as others have noted, Putnam does little to explain how such deep continuities are sustained (Kitschelt, 2003). Although this does not by itself disprove the causal inferences in his book, it does underscore the need for testing of other rival hypotheses in addition to the one he examines (economic development). Such considerations, naturally, do not mean that we should ignore domestic developments. Any history of Italy must consider, as Putnam does, the early divides between the North and the South. But how these divides were sustained and politicized by subsequent historical actors is likely at least in part a function of the influence of and contact with evolving powerful regime types.

Evolution and Marginal Cases

An approach to comparative politics that places the problem of regime evolution and diffusion at the centre of discussions of research design will naturally force scholars to pay greater attention to the issue of where regimes and institutions come from in the first place (Thelen, 1999). This brings us to a final methodological problem of case selection: the need to avoid a form of selection bias that focuses only on the most successful forms of institutional order. An historical examination of the regime types that have most decisively influenced human society shows that institutional innovations generally begin not in established, institutionally stable environments, but in marginal, weak societies ignored by powerful politicians and mainstream scholars alike. If one is interested

in developing a more adequate evolutionary theory of institutions, then, it seems methodologically critical to focus attention on failed attempts at state formation, economic development, nationalism and so forth.

Strangely enough, however, this is one problem that most of the classical scholars in comparative politics, adherents of KKV's approach, and contemporary qualitative methodologists seem to share. Thus Moore (1966) tends to denigrate the importance of studying smaller, weaker countries; Bendix (1978) concentrates almost exclusively on the "usual suspects" in European political development; and Skocpol (1979) focuses primarily on what she defines as the "great" revolutionary episodes in modern history. Both modernization theorists and their classical critics, driven by their common concern with powerful capitalist states and their most serious international competitors, tended to ignore cases of institutional and developmental failure that might have helped to pinpoint the relative causal importance of particular independent variables deemed important for explaining outcomes in more widely analyzed cases.

Among contemporary works on political science methodology, too, exhortations to select "important" or "significant" cases are remarkably widespread. Stephen Van Evera, for example, advises us to seek out cases "of intrinsic human or historical importance"; later he states bluntly that focusing on the "politically irrelevant" is "both a crime and a blunder" (Van Evera, 1997: 86–87, 97). From a very different methodological perspective, Charles Ragin argues that "one of the distinctive goals of comparative social science is to interpret significant historical outcomes"; this goal, he maintains, justifies the selection of "extreme values on a more general dependent variable" and the exclusive study of cases with these extreme values (Ragin, 1987: 11). One would think that KKV, given their sophistication concerning matters of case selection more generally, and their specific attention to Tilly's argument about the evolutionary origins of modern state institutions, would urge scholars to include weak, pathetic, marginal political units in their samples. Instead, they too adopt as one of their two criteria for choosing research topics the maxim that "a research project should pose a question that is 'important' in the real world" (KKV: 15). Indeed, they emphatically counsel comparativists to avoid "the risk of descending to politically insignificant questions" (KKV: 17).

We agree, of course, that scholars should address important *questions*; but sometimes they can only do this by addressing seemingly unimportant *cases*—a point that KKV's concern about "descending" into political insignificance tends to obscure. A focus on "politically significant" countries or movements at any one point in time can blind us to the ways in which seemingly marginal politicians and groups can quickly catalyze powerful institutional changes once the global environment changes. Lenin in Paris in 1910, for example, headed a Bolshevik party

with almost no financial resources, a tiny network of committed activists in Russia, and almost no support from competing socialist groups both in Russia and the West; Hitler, meanwhile, was barely making a living selling postcard sketches in Vienna's cafes. If one were to study political regimes in 1910 in terms of the widespread advice to focus on important cases or questions, then, these men and their ideas would hardly seem worthy of systematic study. Yet in fact, much of the first half of the twentieth century was shaped by their later success.

The point here is not that one should analyze in retrospect only marginal figures, such as Lenin and Hitler, who later became powerful. In fact, an unbiased sample of marginal politicians or movements will always consist primarily of those destined to remain marginal. A purely scientific approach to case selection must eschew any effort to pick cases for reasons of "relevance" to contemporary political life. If one accepts an evolutionary approach to human institutions, to dissuade scholars from the study of weak regimes or "crackpot" thinkers would be like rejecting research in evolutionary biology on one-celled creatures or species existing only in one place on earth—precisely the sort of research through which evolutionary theory has arguably progressed most rapidly. It may well be a dangerous career choice to study a marginal movement, political figure, or set of policies. But such considerations tell us more about the sociology of knowledge than about the logic of social scientific inquiry. They should therefore, in the best of all possible scientific worlds, not affect case selection. As long as a case, no matter how seemingly marginal, can be convincingly tied to a large and "important" question, it is worth subjecting to a serious analysis.

Conclusion

This essay has tried to demonstrate how the emphasis on regime types, historical turning points and institutional diffusion characteristic of the classical literature in comparative politics can contribute to contemporary debates about political science methodology. We should emphasize that it has not been our intention to criticize the goal of most work in comparative politics methodology—that is, to develop a more rigorous approach to social science methods so that the entire universe of global polities and proto-polities can be drawn upon to understand the nature of politics. Our hope is that a renewed attention to the issues of regime type and diffusion will help sharpen comparative politics methodology by highlighting issues of case selection and variable testing that are often underemphasized in mainstream approaches. Far too frequently, critics of social science neo-positivism have assumed that an emphasis on historical context must invalidate well-established principles of social

science methodology (cf. Somers, 1998). We share the faith of KKV, embraced by many of their critics as well, that there must be a general logic to finding and analyzing social and political patterns that unites quantitative and qualitative approaches. In fact, all of the really good books in comparative politics—classical and contemporary—take as a given much of what KKV have to say about the logic of causal inference.

At the same time, we wish to underline that the failure to pay sufficient attention to the origins and diffusion of powerful regime types almost inevitably leads to conceptual stretching and Galton's problem. This type of error, the foregoing analysis implies, will not be random, occasional or mildly important, but systematic and robust, contaminating all but the most trivial lines of inquiry. It will generate weaker causal inferences, faulty explanations and inaccurate predictions, whether one is using quantitative or qualitative methods. Our analysis therefore suggests that the pitfalls of omitting extended discussions of regime typology and methods for studying diffusion in comparative politics methodology are so fundamental that any suggestions for unifying qualitative and quantitative methods that fails to address these issues will be unsuccessful.

Our advice may at first glance appear to be contradictory: the search for empirical generalization by comparing multiple observations or cases is correct and admirable but the search must always be undertaken in the near certainty that the observations or cases under investigation influence each other. Yet the contradiction is more apparent than real. The best work in comparative politics has always been keenly aware of the importance of spatial and temporal context, and has even celebrated it, as in much of the new literature on "comparative historical analysis" (Rueschemeyer and Mahoney, 2002; see also Linz and Stepan, 1996; Ertman, 1997; Goodwin, 2001). At a minimum, knowledge of key turning points in history is necessary for analyzing the "scope conditions" that put temporal and spatial bounds on the validity of causal generalizations. Stated more boldly, however, there is something irreducible about history that gets lost when one considers standards of descriptive and causal inference alone. Comparative politics is certainly about discussing generalities and using cases to generate and test hypotheses about these generalities. But it is also about understanding the origins and influence of regime types like liberal democracy, fascism and communism. The fact of the matter is that these regime types originated, sustained their influence, and (in two cases) collapsed, at particular historical junctures. Comparative politics as a field has always cared deeply about world-historical context, while at the same time retaining a commitment to methodological rigour—in fact, this attention to the world-historical context, as we argue here, is inextricably tied to its methodological rigour (see also Pierson, 2000). Our paper is a plea to keep in sight the former while thinking of the latter. The need simultaneously

to theorize about political generality and to pay attention to the particulars of historical development is what makes the enterprise of comparative politics at once so frustrating and tantalizing.

Notes

- 1 Thus Collier and Levitsky (1997) note the proliferation of different “subtypes” of democracy generated by qualitative scholars of democratization trying to be faithful to the particular features of their chosen cases. See also Sartori (1981).
- 2 Of course, KKV might object that a statistical test could in principle be designed to tease out the relative influence of “proximity to already-powerful democracies” and “commercialization of agriculture” in explaining the emergence of democracy over time in larger and smaller states alike, thus obviating the supposed “trade-off” here. In practice, however, the detailed and precise time series data necessary for such tests are rarely available in comparative historical research; instead, multiple regression analyses are often based upon static multicountry samples. In the case of Moore’s hypothesis, such an approach might incorrectly suggest that commercialization of agriculture had no impact on democracy, because of the mix of commercialized and agrarian states in the category of successful European democracies.
- 3 The debate about Galton’s problem, and possible methodological means for overcoming it, has continued ever since in the field of anthropology; see Naroll (1961); Naroll and D’Andrade (1963); Handwerker and Wozniak (1997). Political scientists, too, have often noted the problem; see, for example, Przeworski and Teune (1970: 51–53) and especially Collier and Messick (1975).
- 4 Keck and Sikkink argue that the work of Meyer and his collaborators places too little emphasis on agency, treating advocates of liberal norms in the developing world as mere “enactors” of Western models (Keck and Sikkink, 1998: 33). Still, in comparison with more geographically oriented structural studies of diffusion, Meyer’s work, like Keck and Sikkink’s, can be usefully characterized as agent-based.
- 5 The same argument could be made about the studies of “parliamentary” versus “presidential” regimes and their effects on post-communist democratization cited by KKV—again, with no reference to diffusion effects (KKV, 1994: 83–84).
- 6 See Cox’s data set at <http://dodgson.ucsd.edu/lij>.
- 7 At one point Cox writes: “Does anyone believe that the United States would remain a two-party system, even if it adopted the Israeli electoral system?” (Cox, 1997: 19). This misses the point entirely: the US is extremely unlikely to adopt the Israeli electoral system—given the findings of both the agent-based and the structural literature on institutional diffusion—as US elites do not see Israel as a desirable model for emulation and the two countries have no geographical proximity.
- 8 Slight differences between our replication and Cox’s published results exist but are not substantively important for the argument. They result from small discrepancies between the publicly available dataset and the dataset used for Cox’s original input of the data. Personal communication with Gary Cox.
- 9 Britcol is significantly (at .05) and negatively correlated with the effective number of political parties in a country, and by itself explains 20 per cent of the variance in this dependent variable. See Table 1, model 3. Colinearity between former British colony status and the log of district magnitudes precludes a meaningful conventional “tournament” between these two variables.
- 10 Putnam claims that differences in “social capital” can explain variations among northern and southern Italian regions, not only variation between the North and the South; but this part of Putnam’s argument has been persuasively challenged by Goldberg

(1996), and it plays little role in the historical explanation given in chapter 5 of *Making Democracy Work*.

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