Comparative Politics: The State of the Subdiscipline

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Comparative politics is emerging as a distinct subdiscipline of political science, defined by both substantive and methodological criteria.[1] Substantively, research in comparative politics seeks to account for variation among political units on consequential social, political, cultural and economic outcomes. Comparative politics research places these outcomes on dimensions, for example a dimension that goes from a Hobbesian state of nature to political order, and seeks to account for the placement of a political unit in a specific time period on that dimension. It then seeks to account for differences in placement along that dimension among political units and for the same political unit in different time periods. In this sense, queries such as "what differentiates countries that experienced violent civil wars since World War II from those that have not, and what is the explanation for those differences?" are quintessential questions on the comparative politics agenda.

Methodologically, there is an emergent new consensus about how best to answer such questions. In earlier decades, there was a consensus about a specific comparative method. It was differentiated from a statistical and from a case-study methodology, and it emphasized through the use of strategic controls the isolation of key variables that could explain variations in outcomes. Beholden to the discussions of J. S. Mill, comparative theorists worked out the implications of using the method of similarity, or the method of difference, to capture the workings of explanatory (or independent) variables.[2]

In the early 2000s, a new consensus is on the horizon, one that emphasizes a tripartite methodology.[3] In its first component, cross sectional

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[1] . James A. Caporaso, the editor of Comparative Political Studies, writes in the introduction to a special issue of the journal "Comparative Politics in the Year 2000: Unity within Diversity" (2000, 699-700) that comparativists have a commitment to "explanatory accuracy" that creates high "barriers to entry," a "division of labor," and ultimately a "fragmented discipline." Caporaso explicitly contrasts comparative politics to the leaner and more theoretical international relations field. Readers of the review herein might see it as evidence in support of Caporaso's charge of fragmentation. Yet I am encouraged by the orientation and training of the coming generation of comparativists, who are ready to join in on the emergent consensus that I outline here.

[2] . The classic statements on the comparative method, by Eckstein, by Lijphart, by Przeworski and Teune, by David Collier, and by Skocpol and Somers are all cited and neatly developed in Lichbach and Zuckerman (1997b).

[3] . This statement is not quite right. Lichbach and Zuckerman (1997a) reflect a widespread belief that the field is divided by a set of paradigmatic approaches -- structural, cultural and rational choice -- each with its own insights. Alternatively, many (e.g. Hall 1997) remain indebted to Samuel Beer's teachings to focus on the relative power of three independent variables: interests, ideas and institutions. The identification of

or diachronic data are seen as important to find statistical regularities across a large number of similar units, not only to give the researcher a clear sense of how well reigning theory works to explain variations in outcomes, but also to see if the explanatory variables that are being introduced by the researcher have explanatory power. Whereas earlier statistical techniques were seen as alternatives to the comparative method, they are increasingly seen as an important element in that method, but only one step towards explanation.

Referees in works submitted in comparative politics today demand that researchers account for their findings theoretically, and in so doing provide a logically coherent account of outcomes. Theory assures us that our causal stories are coherent and non-contradictory, and it also points us to other outcomes that ought to have occurred if our theory is correct. Whereas in the earlier consensus, the comparative method was seen as an approach to the testing of theory (best exemplified in the methodological classic of King, Keohane and Verba 1994), today theory and its testing are now seen as parts of an interactive process within the comparative method. To be sure, our theories need to be put to test on data sets not part of our back-and-forth process sending us to data, back to theory and back to data again. But comparativists do not merely take theory off the shelf and test it; rather they formalize the interpretations of their data, and they are thus making theory while testing it.

Theory herein refers to work that (a) postulates relationships among abstract variables, (b) has rules of correspondence such that one can map values for a large number of real world cases on each of the variables, and (c) provides an internally consistent logic that accounts for the stipulated relationships. Theoretical work has long been done by Hobbes, by Tocqueville, by Marx, by Weber, and by nearly all the greats in the political theory canon, without explicit formalization. However, as demonstrated by Elster (1983) in regard to Marx and many in regard to Hobbes, these works are susceptible to formalization and doing so enriches our understanding of the internal logic of these theories. Today, formalization is a standard

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competing approaches or alternative explanatory variables is, in my judgement, the wrong way to go in developing a discipline. Doing so focuses attention on the explanatory limits of a particular method or a favored variable rather than the degree to which we collectively in comparative politics have accounted for important variations on outcomes. The emergent consensus is not in the methodological literature but rather in the practice of scholars addressing substantive issues. This essay has, I admit, a Leninist goal of creating a vanguard to facilitate what is historically emergent.

accoutrement to theory, but not a necessary component of it. When I equate formalization with theory in the course of this paper, it should be read as “susceptible to formalization.”

Formal theory as practiced today increasingly endogenizes the core variables that are of concern to political analysts. For example, models are considered incomplete if they analyze the effects of ethnic fragmentation on the probability of low economic growth. They should analyze as well the impact of low economic growth on ethnic fragmentation. With principal variables endogenized, it is often hard to ascertain for purposes of testing what is on the right and what on the left side of the expression. Formal theory has thus created new challenges for statistical analysis. One approach is to rely on “comparative statics” that capture elements of a complex model. For example a statistical test could be performed to see if (other things held constant) ethnic fragmentation increases with long periods of economic stagnation, and another test to see if (again, other things held constant) high levels of ethnic fragmentation lower the future levels of economic growth. Another approach is to test the observable implications of the model rather than its stipulated relationships. There is no inherent problem in maintaining a focus on a single variable as dependent while endogenizing a set of variables in the accompanying formal model; but combining a formal and statistical model is not as straightforward as it had been in the past, say with expected utility models.[4]

Finally, as the third component in the tripartite methodology, comparativists examine real (and increasingly, virtual)[5] cases to see if the results from the statistical analyses and the theoretical accounts apply to the world. The examination of actual cases in narrative detail allows comparativists to address questions of how historically there has been a translation of values on independent variables onto values on dependent variables. In the grand tradition of social theory, theory and narrative are inextricably intertwined, and is often referred to as “empirical theory” (Dahl 1964, 101-04). In this review I classify work written in that tradition as part of the third component, narrative. I do this because I believe their fundamental contribution has been in finding regularities through the juxtaposition of historical cases. Theorization of these regularities has

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4 . See Alt and Alesina (1996) for a defense of endogenization in political economy; and for examples of statistical tests of models in which independent and dependent variables are both part of the equilibrium.

5 . See Lustick (2000) for an innovative approach to the use of virtual data for the study of the construction of ethnic identities.

tended to be an implicit rather than an explicit element of their enterprise; practitioners report that they are testing rather than making theory.

If statistical work addresses questions of propensities, narratives address questions of process. In juxtaposing theory to cases, as comparative narratives demand, methods of similarity and difference are especially useful in picking up cases that are on and off of the regression line. Also, single case narratives help pick up changes in the values of key parameters, where theory would expect changed outcomes. To the extent that case studies, done through ethnographic, interview, or archival work, find these changes in parameter values and identify subsequent changes in outcomes, added confidence is given to the theorized account.

In this tripartite method, there is no agreement, nor reason why there should be one, on the sequence of these three elements. Furthermore, there is no expectation that all three elements will be part of every study. But as progress is reported on accounts for the range of outcomes for specific dependent variables, comparativists need to satisfy two audiences. First, they must demonstrate that their work meets standards within their own methodological community. But second, they should feel challenged, even threatened, by advances by scholars within the other two methodological traditions, and seek to adjust or delimit their claims in light of findings in those traditions. The result of such practices, and emerging within our subdiscipline, is a wider corpus of work reflecting advances in all three elements of the tripartite method.

The dependent variables that engage the attention of the comparative politics subdiscipline are not timelessly and unambiguously arranged, like the unanswered conundrums that drive mathematicians, at least until a solution is found. There are two crucial differences between the questions that drive political scientists and those that drive mathematicians. In comparative politics, questions are chosen because they have vital interest for the world we live in. Questions concerning democratization are prominent on the agenda of comparative politics today in large part because they are on the agenda of citizens, politicians and the informed public around the world. Comparativists will drop old questions, not because they are solved, but because new questions have pushed their way onto the political agenda. Choice of the dependent variable cannot be separated from the goals, interests and generational perspectives of the researchers and research community.

Also, questions comparativists ask about outcomes get specified anew in each era, as the way we ask our questions about political outcomes changes over time. In the Hobbesian period, civil war meant the collapse of monarchy; in today’s world it increasingly means rebellions fought in the name of an ethnic group against state authority. While there may well be explanatory factors that cross eras and types of civil wars, small re-specifications of a dependent variable can have large repercussions concerning the significance of independent variables. Barrington Moore Jr.’s (1966) question in his classic book on democracy was the susceptibility of democracies to fascism where property rights but not individual liberties were protected; many democratic theorists inquire today as to the susceptibility of democracy to a breakdown where private property rights are endangered even if some liberties are protected. These are different dimensions, although both could use “degree of democratic consolidation” to name it. Researcher’s values – the relative importance of property rights and individual liberties – can not so subtly influence the specification of a dependent variable, again with implications for the explanatory power of different independent variables. Therefore reviews of progress in comparative politics by different reviewers or written in different periods will surely highlight different dependent variables.[6] Reviews, even when treating the same general topic, must be sensitive to the precise way the dimension that encompasses the outcome to be explained has been specified. In light of this second factor, questions in comparative politics never get satisfactorily solved, as on the brink of discovery they get specified in a new way, opening up new lines of inquiry.

In this review, to illustrate the progress in the comparative politics discipline over the past decade, I shall examine work on three outcomes, each of which has political relevance in our age, and each of which therefore has engendered a considerable amount of comparative research. The three outcomes are democracy, civil war, and forms of capitalism. For each of these outcomes, I will report on the collective assault on a problem within the context of the tripartite methodology.

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[6] . My predecessor for this decadal review, Rogowski (1993) made no mention of comparative democracies as on the agenda. See Shin (1994), fn. 9, p. 138 for a sampling of the tide of publications on democracy that followed on the heels of Rogowski’s review. Of the three dependent variables singled out for attention in my review, only one (forms of capitalism) received serious attention by my predecessor.

Democracy

Comparative studies of democracy and its alternatives have focused on the factors that differentiate democratic countries from nondemocratic ones. [7] Here I will discuss studies, in the tradition of S. M. Lipset (1959), relying primarily on cross-sectional analysis. I will then examine new work, in the tradition of Barrington Moore, relying principally on patterns as elucidated through historical narratives. Finally, I will discuss the state of democratic theory in light of the recent advances in the comparative field.

Statistics

What distinguishes democracies from nondemocracies? This is a question that begs for cross sectional statistical analysis. Przeworski, Alvarez, Cheibub and Limongi 2000 (hereafter Przeworski et al) have made a fundamental contribution to comparative politics in compiling a data set that enables them to provide fresh answers to this age-old question. Their data are consistent with S. M. Lipset's classic finding (1959), namely that there is a strong relationship between economic development and democracy. But Lipset's data did not allow him to distinguish two possible reasons for this correlation. Are democracies the result of modernization, as many of Lipset's followers assumed to be the correct interpretation of his results? Or, as Lipset himself mooted, do democracies survive more successfully once a certain level of economic growth is attained? Meanwhile, poor democracies fall into dictatorship. In this scenario, democracy tends to survive if a country is modern, but democracy itself may arise randomly, exogenous to the level of economic development.

Przeworski et al provide powerful evidence that modernization is not the cause of democracy. They collected data from 141 countries annually from 1950 through 1990 and coded them as to whether they were democracies. (They code democracy as a dichotomous variable. However,

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[7] . There is a burgeoning literature on institutional mechanisms supporting the democratic equilibrium, a literature that is dominated by Americanists, but is now analyzed by students of all democratic systems. For reasons justified elsewhere (Laitin 1998), I would classify this work as the core of a field that ought to be called "the mechanics of democratic rule." I would consider Shepsle and Bonchek (1997) the exemplary text of that field. The field "American politics" should be excised (unless we include "Somali politics" as a fifth field, and "Yoruba politics" as a sixth, etc.). The literature in American politics that doesn't fit into "mechanics of democratic rule" can easily be folded into the comparative politics field as defined in this paper, as there is no reason to exclude from comparativists' purviews the American case. For my interpretation of the relationship of comparative politics to the three other subfields of political science, see Laitin 1998.

though using the Coppedge-Reinicke scale (1990), which includes elements of what O'Donnell calls the "full institutional package" of polyarchy (in Diamond, ed. 1997, 41), they get similar results.) Their metric, the probability of a transition to democracy, shows "dictatorships survived for years in countries that were wealthy by comparative standards…conversely, many dictatorships fell in countries with low income levels."

Meanwhile, their data show that if "the causal power of economic development in bringing down dictatorships appears paltry…per capita income has a strong impact on the survival of democracies." In fact, over $7000 in per capita income brings a zero probability of the collapse of a democracy, where there is a 12% chance if income per capita is less than $1000. The collapse of Argentina's democracy at $6055 is the highest in the data base. O'Donnell (1978) used the Argentina case to demolish Lipset, but he did this, according to Przeworski et al, by examining a "distant outlier". Three of the four transitions to authoritarianism at per capita incomes of greater than $4000 occurred in Argentina, and the fourth in Uruguay (Przeworski et al, pp. 90-98). Per capita income is indeed the most powerful predictor of democracy, and Przeworski et al correctly predict 77.5 per cent of the 4126 annual observations merely by knowing per capita income. Consistent with this finding is the finding that democracies survive more successfully under conditions of economic growth, whereas dictatorships fail equally under conditions of growth and conditions of economic decline.[8]

What about non-economic variables in the consolidation of democracy? Linz and Stepan emphasize the role of institutions, and thereby downplay the Przeworski et al findings. While they acknowledge that high GDP is favorable to democracy, they insist that this fact "does not tell us much about *when, how,* and *if* a transition will take place and be successfully completed…economic trends in themselves are less important than is the perception of alternatives, system blame, and the legitimacy beliefs of significant segments of the population or major institutional actors." To support this point, and relying on a comparison of Netherlands and Germany in the 1930s, Linz and Stepan show that the economic decline was equal in

[8] . But see Remmer's (1991). She makes a cogent attack, though a statistical analysis of voter volatility in Latin America, on those who see economic crisis as the death knell for democracies. Przeworski et al predict correctly in Latin America on the basis of GDP per capita alone, and for them, the economic crises of the 1980s were not consequential. Nonetheless, Remmer's finding that economic crisis may not have the effects (when party structure is controlled for) merits further testing.

both countries, but only in the latter there were strong groups able to articulate blame for the economic crisis (1996, 77). In earlier work (1990), Linz emphasized the importance of political institutions (favoring parliamentary over presidential systems) and sequencing of elections (favoring a sequence from central elections to regional ones). In their monumental comparison of transitions and consolidation in southern Europe, South America and Post-Communist Europe (1996) they point to five necessary conditions for the survival of democracy, which include a vibrant civil society, an autonomous political society, the rule of law, a usable state, and a set of rules, norms, institutions and regulations that undergird an economic society. Furthermore, there are seven independent variables (each with a range of values, most often nominal) that help predict successful consolidation. They include the relationship of state to nation, the type of prior regime, the leadership base of the prior regime, the pattern of the transition to democracy, the legitimacy of major institutional actors, and the environment in which the democratic constitution was drafted.[9]

The alternative to economic and institutional variables is that of political culture. Lipset (1994), while acknowledging the importance of economic prosperity, insists that legitimacy, the key to sustenance of democracy, requires a supportive political culture. Diamond (1999) too insists on the importance of regime legitimacy and a political culture that favors democratic institutions. Survey research, he points out, shows again and again that people condition their support on democracy less on economic conditions and more on the institutional workings of the political system.[10] The corruption of the regime, the behavior of parliamentarians, and the responsiveness of elected representatives all play important roles in assuring legitimation. The key criterion for legitimation is that all significant political actors believe that democracy is appropriate for their society, and all significant political competitors believe that democracy is ‘the only game in town.’ Although Diamond acknowledges that economic performance plays a role in all regressions, “many more political variables than economic ones have significant effects” on survey support for democracy (quotes from 65, 193). The strongest advocate of the political culture foundation for democracy is Inglehart who claims “that over half of the variance (in a

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[9] . Before laying out their five necessary conditions and seven independent variables, Linz and Stepan warn their readers, “we will not restrict ourselves to the procrustean bed of this framework. The specificities of history are also important” (p. xiv).

[10] . Here Diamond (1999) relies on data from Richard Rose, William Mishler, and Christian Haerpfer (1998), and Shin and McDonough (1999).

sample of European or run-by-European states) in the persistence of democratic institutions can be attributed to the effects of political culture alone” (1990, 41).

Statistical re-specifications of Inglehart’s data by Muller and Seligson (1994) (who also enhance the scope of those data with material from Latin America) show that for most elements of the civic culture package, Inglehart had the causal arrows going in the wrong direction. With changes in the level of democracy across decades as their dependent variable, Muller and Seligson show that interpersonal trust is not an explanation for democracy but a result of having experienced a long period of democratic rule. The only variable in the civic culture package that holds up as having independent causal influence on democracy is that of the population favoring moderate reform (over revolutionary change or the suppression of reform).

Przeworski et al, to be sure, examine other factors besides economic level and growth. Once economic controls are added, duration of democracy, coming from a suggestion by Dahl, is not significant. Nor do cultural factors, such as the majority religion, seem to have much explanatory power. Knowing the degree of ethnic fractionalization, which many scholars have seen as an added hurdle for democratic consolidation, adds almost nothing to the predictive power of their hazard model. Educational levels, however, do add predictive power, independent of economic levels. And there is suggestive (but not very conclusive) evidence that parliamentary democracies are less subject to collapse than presidential democracies. Since parliamentary regimes are more likely in rich countries, but poor parliamentary regimes are poorer than poor presidential ones, Przeworski et al did not have much confidence in the robustness of their finding that while 28 percent of parliamentary regimes died, 54 percent of presidential regimes suffered similarly.

Przeworski et al have set a new standard in research differentiating democratic from nondemocratic regimes. Yet much empirical work remains to be done. For one, political system variables have been insufficiently specified to be used in statistical analyses. The dichotomous variable of parliamentary vs. presidential hardly captures theoretical intuitions about institutional stability (Shugart and Carey 1992). Does presidentialism allow for the election of non-representative candidates (Linz and Valenzuela 1994)? Cox shows that this depends on how well voters can coordinate and how strategic they are (1997, 233). Perhaps presidentialism, associated with a two-party system, denies minorities outlets to modulate majorities, outlets that are available to them in the PR systems associated with parliamentary rule? But minorities in two-party systems play a role in pre-election coalition building; meanwhile minorities in PR systems play a role in post-election cabinet building. Neither system is inherently more compatible with minority representation.[11]

For purposes of cross-sectional testing of the hypothesis that political institutions matter for democratic survival, what are the alternatives to the presidential/parliamentary dichotomy? Cox suggests electoral systems differ as to where coordination failures occur, and who pays the cost for such failures (Cox 1997, 15.2--15.3). Tsebelis’ work (1995) provides, through the comparative analysis of “veto points”, a way to capture degrees to which minorities can protect themselves against majorities, and this should provide more conclusive tests than the noisy presidential/parliamentary variable. Niou and Ordeshook suggest a dichotomy of integrated vs. bargaining systems, which maps only partially with presidentialism and parliamentarism. Linking a better specified dimension of democratic institutions to democratic consolidation is clearly an open area for new research.

Second, Przeworski et al have ignored several opportunities to challenge their economic variables with a variety of institutional ones that are prevalent in the democracy literature. Political system variables tell us little about the capacity of democratic states to protect property rights, secure a rule of law, and to administer laws without corruption. Linz and Stepan’s work gives conceptual foundations for newly reconstituted institutional variables. Treisman has begun to use data on comparative corruption in a way that can be appropriated by democratic theory. Also ignored is the institutional power of the military. Not only state institutions should be considered, but societal ones as well. Przeworski et al have no indicators for the strength or density of civil society. In light of the gaggle of books and papers that purport to show the importance of civil society for the consolidation of democracy (in addition to those I’ve reviewed so far, see Putnam 1993 and Schmitter 1997), it is a surprise that they did not collect systematic data (even if they would need to impute for missing years) on this factor. That Przeworski et al do not have well-designed tests for political and

[11] . These issues are addressed theoretically in Taagepera and Shugart (1989), who set up the terms for a debate that remains lively, most notably in the pages of Electoral Studies.

societal institutions, in order to see if they alter the coefficients of the economic variables, is an invitation for new research.

Third, as periodic survey data become increasingly available, advocates of legitimacy or political culture as sources of sustenance for democracy can now dock these data onto the Przeworski et al set, seeking to find whether, with proper controls and with the imputation of data for missing years, a political culture in favor of democracy is a causal favor in democratic consolidation. Muller and Seligson (1994), with a limited data base, suggest that a promising variable is degree to which the modal voter favors moderate reform. It would be worthwhile to know whether this variable or some other element of the civic culture package holds up in a broader data set.[12]

Fourth, as Przeworski et al would be the first to admit, their cross-sectional findings are nearly impossible to interpret causally. What are the mechanisms that undermine poor democracies or sustain rich ones? It seems impossible to narrate the progression of events from democracy to dictatorship or reverse in terms of abstract variables such as per capita income. Here is where the other two prongs of the tripartite methodology come into play – with a need to theorize the discovered relationships and the need to get down to the level of cases (as do Linz and Stepan, who are substantially less impressed with Przeworski et al’s statistical models from the viewpoint of actual case histories), to see if actors in the real world of democracy are conditioning their behavior on the factors that the models highlight.

Narratives

What pushes some countries at specific historical periods into democracy? How do fledgling democracies persevere when they face crises? These are questions that require sensitivity to change over time, and lend themselves better to historical rather than statistical analysis. To be sure, Przeworski et al’s data allow for some diachronic analysis, and Linz and Stepan are quite sensitive to sequencing in their studies of particular cases. But they are less focused on who precisely is doing the acting and where these people fit into the social spectrum? In the past decade, very much in

[12] . Muller and Seligson find that measures of inequality wash out the effects of GDP per capita. Przeworski et al lack data (which are becoming increasingly available) to test this on a universal sample of cases.

the Barrington Moore tradition, research on the historical role of social classes in the making and unmaking of democracy has made some progress in addressing these narrative questions, and here I will review the studies of Luebbert (1991), Rueschemeyer, Stephens and Stephens (1992), and Ruth Collier (1999).

Moore's (1966) classic study of social alliances and democracy portrayed the bourgeoisie as the source of modern democracy, and bourgeois strength as the key to democratic maintenance through the tumultuous interwar period. Where the bourgeoisie was weak, and needed an alliance with the landed classes, fascism was the result. And where the bourgeoisie was weak, and peasants could be mobilized into revolutionary action by leftist intellectuals, communism was the outcome. Scores of studies to develop, refine, and challenge this pattern have been published in the thirty-five years since its publication.[13] Throughout the past decade, this tradition remains vibrant.

Luebbert was primarily interested in the maintenance of democratic institutions under conditions of crisis, and more specifically through the interwar depression. Like Moore, he found the key to democratic strength in the interwar period to be in the middle classes. But he demonstrated that the so-called marriage of iron and rye (the weak bourgeois alliance with the landed aristocracy) was not the source of fascism. In fact, he showed, rural support for fascism did not require a landed elite. In Germany, Spain and Italy, rural support for fascism was found mainly in areas in which the family peasantry rather than the landed elites predominated. Only in southern Italy was there a landed elite that could deliver votes, and they (ironically) sided with the liberals (concerned more for patronage than with class conflict). Their support for fascism came only after Mussolini attained power.

Liberal democracy survived in Britain, France, Switzerland, Belgium and Netherlands, Luebbert argues, because before World War I, the middle classes were not divided by religion, language, region, or urban-rural differences; and where there were such differences, they did not work to divide the middle classes politically. A united middle class was not afraid of workers, and slowly but inexorably incorporated them into the electoral system. Workers may not have maximized economic returns in their alliance

[13] . For a review of this literature, see Wiener (1976) and Ross et al (1998).

with the middle classes, Luebbert reckons, but they received dignity in being accepted in the corridors of liberal power. Class collaboration (middle and working) compensated for lower material benefits for workers and for slower gains in the right to vote. In the interwar years, because liberal hegemony was not internally divided, radical working-class parties had no middle class allies. Those union people who sought to challenge the lib-lab alliance invariably failed.

Liberal hegemony failed where preindustrial cleavages divided the middle classes. Divided among themselves, the middle classes were afraid to ally with workers. Under these conditions, workers had to build trade unions as coherent organizations. So at the time of World War I, divided liberals faced united workers. After the war, strong unions extracted high material benefits, and long-term peace required the political subordination of markets. With the failure to create an urban-based coalition, both workers and middle classes sought alliances with the peasantry. Social democratic outcomes rested on alliances of the urban working class and the middle peasantry or family farms. Here peak trade union associations had great power. Norway, Sweden, Denmark, and Czechoslovakia were examples. However, whenever socialists sought to organize the agrarian proletariat in politics, the family peasantry was pushed into an alliance with the middle class, which became a fascist alliance. Germany, Italy and Spain are examples. Thus, one of the bitter historical ironies: where interwar working-class leaders committed themselves to social justice through the taking up the cause of the rural workers, they forced a coalition of middle classes and family peasants, and this was the route to fascism.

Not only Luebbert, but many others in the Moore tradition give far more attention to the independent role of the working class, which is a factor that plays only a small role in Moore’s alliance patterns. Rueschemeyer, Stephens and Stephens, in their comprehensive historical treatment of Western Europe, Latin America and the Caribbean, cannot find empirical support for Moore’s principal claim in regard to the bourgeoisie. *Pace* Moore, Rueschemeyer et al find that the middle classes, after their inclusion into the power framework, are ambivalent toward democracy. Therefore, it takes the working class (which, unlike peasants, can organize themselves politically) to affect the true balance of power (where no social group can establish hegemony over the others) upon which democracy rests. The generic historical sequence is one of capitalist development that transformed the class structure, and subsequently strengthened the working and middle classes while weakening the landed class. This led to conflicts that while bloody in the long run, eventually advanced the cause of democracy.

Their original supposition, going into the study, was that the working class was the "most consistently pro-democratic force," except where it was mobilized by a charismatic but authoritarian leader or a hegemonic party linked to the state apparatus. This point makes little sense theoretically. The middle classes only wanted to include themselves and no one below them. This is the same with the working class. Neither was more democratic. It is just that the working class was lower on the totem pole, and once they were included, the vast majority of the population had voting rights.[14] Furthermore, in their case studies, Rueschemeyer et al amend their generalization about working class power as the key to democracy. Only under conditions where a party system can effectively protect the interests of the upper classes, they find, will these classes accommodate to the pro-democratic pressures of the working classes.

Collier, in her Paths Toward Democracy, also seeks to delineate the role of the working classes in democratization. Examining cases from both the 19th and late 20th centuries, and from Latin America as well as Western Europe, she finds several distinct paths towards democracy. By looking at seven distinct patterns of democratic initiation, she finds that labor plays at least some role in four of those patterns. Her narratives provide plausible evidence of labor's role in democratization across historical periods. But there is a methodological problem with this argument, foreshadowed by the Rueschemeyer et al recognition of the need to protect the interests of the upper classes if democracy is to be successfully implemented. Collier only examines labor mobilization in the initiation of successful democracies. If she had coded labor mobilization for every year, she might have found that the higher the mobilization, the lower probability of democratic initiation. This is a real selection bias problem. It could be the case -- profoundly undermining the Collier thesis -- that the stronger labor shows itself, the more reluctant the right is to accept a democratic constitution. A more complete data set could determine whether Collier's thesis holds, or its opposite.

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[14] . The equation of a particular group's or class's outward commitment to the ideology of democracy with the attribution of causality to that group or class in explaining democratic outcomes is common, especially in the case study literature. See, e.g. Hsiao and Koo, 1997. The key question for democracy is not a group's or class's desire to undermine autocrats, but the probability that a group or class-coalition in power will leave power should an out-group win an election. On this point, see Przeworski (1991), chap. 1.

The historical expositions that accompany studies in the Moore tradition, from non-democracy to democracy, as well as the reverse, provide a rich narrative complement to the cross sectional studies. And as is usually the case, the implications of the cross section and the narrative findings are in some tension with one another. As Rueschemeyer et al point out, regional comparisons allow for a large set of sequences under different contexts that can all lead to democracy. Therefore "the similarity of the correlation between development and democracy in different contexts is fortuitous…the only underlying homogeneity is the overall balance of power between classes and between civil society and the state. While this is enough to produce the correlation between development and democracy observed in the statistical studies, the same balance of power between pro- and anti-democratic actors can be produced in a large number of ways" (1992, 284).

These historical comparisons are impressively detailed. Yet the proliferation of paths and sequences reduces one's confidence in the generality of the findings in any study. Either the studies are historically circumscribed, with the author unwilling to make projections about countries in different eras or different areas (as with the case of Luebbert), or the studies are so broad as to lead to a congeries of possibilities and little way of knowing which of many paths will be followed by a case not already in the data set (Collier, Rueschemeyer et al). Rueschemeyer et al intimate that better statistical work would include regional dummies, as the patterns seem to be regionally specific. Collier's work, however, shows that similar patterns can cross regions, but not eras. Until there are better specified variables, ones that can be coded for cases outside the domain of cases in which the pattern was originally found, the narrative-based diachronic approach will not challenge sufficiently findings relying on the statistical approach. But both the statistical and narrative approaches have set new problems for the third element of the tripartite methodology, that is theory.

Theory

Democratic theory in the past decade has focused a good deal on the microdynamics of democratic stability. Przeworski (1991) has addressed the problem of why actors out of power might choose not to rebel against democratically elected rulers. Weingast (1997) has addressed the problem of why democratically elected rulers might choose not to confiscate property rights (including voting rights) from their enemies, to assure longevity of rule, and thereby undermining the democratic system. Whereas Przeworski asks the conditions under which democracy is immune from revolution from below; Weingast asks the conditions under which democracy is immune from revolution from above.

In several important ways, the theoretical literature is out of touch with the empirical regularities discussed earlier. For one, what is the relationship between per capita income and democracy in these models? If this is a robust empirical finding, then our theoretical models should have a parameter for per capita income such that the democratic equilibrium is more unstable to the extent that the parameter goes down in value. It is a gap in theory that our models do not show how and why economic wealth and democratic stability are part of the same equilibrium, and how that equilibrium is arrived at. Second, why should certain institutional forms be more conducive to democracy than others? What is it about parliamentary rule that makes it more stable than presidential? Or perhaps there is a characteristic (e.g. veto points) that clusters around one value in parliamentary systems and another in presidential ones? And shouldn’t democratic theory be more concerned with the endogenous selection of institutions, following from Geddes’ (1996) empirical work on Latin America and Eastern Europe? Clearly, our theorizing about democracy should be oriented to accounting for the (albeit weak) institutional findings.[15] And third, in a system with workers, a middle class, two classes of peasants, and a landed class, under what conditions will working class mobilization yield democracy? Luebbert’s approach, which finds that the bourgeoisie and workers can reach a class compromise under certain conditions, is consistent with a model developed by Przeworski and Wallerstein (1982). But this cannot be the only democratic equilibrium, and theorists should be modeling the patterns identified by scholars in the Moore tradition to check for equilibrium possibilities.[16]

[15] . In a promising line of research, Londregan (2000) has found that in the framework of legislative committees, it has been possible to slowly erode aspects of the undemocratic constraints in post-Pinochet Chile. Perhaps it is institutions at a much more disaggregated level, such as with committee structure, rather than a more aggregated institutional structures, that make not only democratic consolidation but democratic enrichment more likely.

[16] . A more radical approach to the Moore tradition is suggested by Gourevitch (1998). He points out that Moore’s core insight is to find the root of political conflict to be in the axes of cleavage. Since micro regulation has replaced macroeconomic policy among the advanced industrial countries as the core cleavage, Gourevitch finds it unlikely that battles among social classes will impinge on political institutions. The fragmented specialized issues of micro-regulation, however, will begin to carve their way into political coalitions, conflict, and institutions. If Gourevitch is correct, the Moore tradition should find its way into the microanalytic game theoretic approach that has long been considered its rival.

Democratic theorists have not only worked on equilibrium theory, but they have also addressed issues on the criteria for judging a system as democratic. Suppose, as Shaffer (1998) shows, that when Senegalese use the word we translate as "democracy", they mean something substantially different from meanings that are well accepted in the West? Do we code systems as democratic, his work demands that we ask, based on local criteria of success or some abstract standard? David Collier and Steven Levitsky have shown (1997) that even in our enclosed scholarly community of political scientists, we have no decontextualized standard of democracy against which all systems can be judged. These arguments, if cogent, are clearly a threat to high-n statistical comparisons, for they don't allow a standard specification for the dependent variable to hold for all areas and all eras.

Meanwhile, other theorists have sought a well-specified but richer understanding of democracy. Bollen (1993) has specified an underlying concept of "liberal democracy" and has sought through the use of structural equations and confirmatory factor analysis to correct for systematic and random biases in several standard measures. O'Donnell has argued that there is no real democracy unless the informal workings of institutions squelch particularism (1997, 46). Coppedge and Reinicke (1990) also have sought to capture something of the depth of democracy in their scaling technique.

On these issues of specification of the dependent variable, I accept Przeworski et al, who plead for a minimalist definition of democracy, demanding only contestation (with an opposition that has a positive probability of winning office) and autonomy (with the winners of the election actually ruling the country) (p. 15). They argue for minimalism not because they are uninterested in issues of equality, or representativeness, or accountability, or of the economic well being of people. Nor would they deny that the goods that people hope to realize from democracy differ cross nationally. Their point is that the more we disaggregate our key variables, the better we can determine the relationship among them. By putting many good things in our specification of the dependent variable (e.g. those essential characteristics for a polyarchy, as outlined by Dahl in his Appendix to Preface to Democratic Theory 1956), we will not be able to determine, for example, the degree to which contestation is associated with an informed and educated electorate, whether elected officials are acting as agents of only their patrons, or the degree to which contestation brings public policy closer to the ideal position of the median citizen. In part because Przeworski et al disaggregated, they were able to demonstrate a surprising relationship between democracy and economic growth (that while some dictatorships had the greatest growth rates in the world, they have also had some of the slowest, and on average, controlling for initial conditions, there is no difference in the performance of democracies and dictatorships).

This plea for disaggregation puts a new burden on data collection and on theory. In regard to data collection, Przeworski et al's mode of operation is not to overcome bias through statistical corrections, as with Bollen, but to get better data that are less subject to biased coding. This requires less in structural equations and more in the development of externally valid indicators. In regard to theory, Przeworski et al's disaggregated approach demands that we work out models showing the interaction of such factors as elections, policy shifts in the direction of the ideal point of the median voter, information of voters, and equality. We have long assumed that this is a coherent package of valued goods, but we have little in the way of theory to show why or under what conditions these separate variables cohere.[17]

The past decade of comparative research on democracy has been a rich one empirically, both statistically and in the exploration of historical and contemporary cases. If the empirical advances help re-stimulate the political theory of democracy, it would be a great achievement.

Order

Since the end of World War II, Hobbesian fears of disorder, and the war of all against all, informed the subfield of international relations, but students of comparative politics could forget Hobbes, and ask Lasswellian (1936) questions of who gets what, when and how. To be sure, the dependent variable of "order" in recent comparative politics had its historical dimension, and it specified the problem as one of the emergence of the great revolutions. Skocpol (1979) through Goldstone (1991) argued that the number were too few to allow for statistical methods. Skocpol relied on critical comparisons and Goldstone on a Boolean schema developed by Ragin (1987). Theoretical work on the "J" curve (Davies 1972) and on resource mobilization (Tilly 1978) lent themselves to statistical tests, but

[17] . Schmitter (1997, 244) calls for disaggregation for similar reasons, though he would not give primacy to elections, as Przeworski et al do, by appropriating the name "democracy" for cases where there is electoral contestation and uncertainty with the winners of those elections actually ruling.

most work using these theories have been in the historical narrative tradition.[18]

Events in the late 1980s brought Hobbes back into the center of comparative politics. The states of the “second world” collapsed. Several states in the “third world”, mostly in Africa and Asia, collapsed as well. And an unnoticed trend since the end of World War II became quite clear, begging for explanation. This trend is the decrease in the probability of inter-state war, and the increase in the probability of civil war. Furthermore, civil wars were increasing in number in large part because they were in many cases interminable, whereas interstate wars have been far more likely to end in a negotiated settlement. With the dependent variable re-specified as the ability of a state to withstand collapse, or ethnic and other forms of insurgency, the number of cases facilitated statistical analysis. Data sets such as the Minorities at Risk and State Failure allowed comparativists to sort out statistically polities that were more or less subject to rebellion (Gurr 2000, Collier and Hoeffler 2000, Fearon and Laitin 1999). In a complementary effort, new theoretical work has sought to identify the causal mechanisms that might be driving the statistical findings. Because the breakdown of order was in many cases caused by insurgents acting in the name of ethnic/national groups, much of the theoretical advances build on the seminal work of Horowitz (1985), whose focus was on ethnic conflict in general. Case-based narrative research (e.g. Kalyvas forthcoming, Varshney 2001) relies on theory and statistical methods to elucidate the workings of theoretically derived mechanisms.

Statistics

Ted Gurr’s “Minorities at Risk” and “State Failure” teams, in their books, articles, and accompanying data-sets invigorated the field of comparative ethnic conflict and civil war. In Minorities at Risk (1993), Gurr reports on a data project that involved extensive coding on demographic, cultural, social, military, economic and political variables for 233 “politicized communal groups” from 93 countries in all the world’s regions. To be included, groups must have either experienced discrimination or have taken political action in support of collective interests. This data-set has been criticized for several problems, probably the most severe being that the cases

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[18] . Statistical work on the sources of order (because it did not address the great revolutions), and here I refer to the work of Hibbs (1973), tended to get lost in comparative research on revolution.

were chosen on the dependent variable, thereby leaving out many groups which, for lack of mobilization, were not seen as being "at risk." Nonetheless, the Gurr team circulated their data to the research community, and has worked on improving it based upon community criticisms.

What do the data show? In the chapter "Why Minorities Rebel" (1993 pp. 123-138) Gurr claims that level of group grievances and strength of the group's sense of identity are the most important independent variables. Yet, oddly, no statistical model provided in the book demonstrates this reported finding. And considerable work using the data set in the wider research community (the second generation users) finds otherwise. Fearon and Laitin (1999) report that the level of GDP per capita in the country (a variable they added to the data set) is the most robust "predictor" of rebellion. Toft (1998) reports that the geographical concentration of groups in historic territories is a powerful predictor of rebellion. Saideman reports (2001) that foreign support is important for rebellion, and this foreign support is more likely to be forthcoming if the group bordered on a country that was dominated by their ethnic kin. Meanwhile, no paper controlling for GDP and geographic concentration has shown that level of economic, cultural, or political grievances can differentiate cases of high rebellion from cases with low or no rebellion. In fact Laitin (2000) reports that degrees of language grievance have no relationship at all to rebellion, and in some specifications he reports a weak negative relationship, showing lower levels of rebellion the greater the grievances over language policy.

The second generation findings from the MAR data set are in accord with many of the central findings from the "State Failure Task Force" (Esty et al, 1995, 1998). Unlike MAR, the State Failure data set used country/year as its unit of analysis, and the dependent variable was state failure, a concept that included revolutionary and ethnic wars, mass killings, and disruptive regime changes. The most robust explanatory variables included overall living standards in the country (measured by infant mortality), level of trade openness, and level of democracy (where "partial" and "recent" democracies are most likely to suffer failure). Consistent with the non-findings on grievances by second-generation MAR analysts, the State Failure Task Force found (almost) no support in their statistical models for the hypothesis that ethnic discrimination or domination generates state failure.

Narratives

Case studies of the breakdown of legitimate domination reflect our extremely troubled world for regimes (at least in comparison to the less troubled world for inter-regime breakdowns of order). Here I will review two of the ways in which the dimension of disorder has been analyzed: explaining collapse of state authority; explaining the eruption of ethnic violence and civil war.

The collapse of the Soviet state -- given the wide acceptance in political science that Samuel Huntington (1966) got it right, viz., that Leninist systems may be inept in providing many public goods, but they could produce order -- came as a shock to political scientists, even those who were specialists in Soviet studies. On the causes of the Soviet collapse, area specialists have been divided: Suny (1993) sees it as caused by the emergence of national consciousnesses, seeded by the Soviet state, that could not be contained by that state; similarly Beissinger (forthcoming) sees it as due to the tides of nationalist mobilization that undermined the regime’s ability to maintain order; Roeder (1993) sees it as inherent in the sclerotic institutional arrangement of Leninist states, where selected officials had powerful incentives not to innovate; Hough (1997) sees it as caused by the loss of will by the Soviet intelligentsia (and incredibly self-destructive policies by Gorbachev) to lead what was sure to be an extremely difficult political and economic transition; and Solnick (1998) sees it in the loss of confidence by agents of the state in the ability and will of the Soviet leadership to exert domination over government and society, and therefore these agents grabbed as much property as they were able, to insure themselves a livelihood should the state collapse. As it was rational for any agent to steal from the state, it was rational for all to do so, and thus there was a cascade that emasculated the resources of the Soviet state. Lohmann’s discussion of informational cascades and the breakdown of the East German regime has a similar dynamic. The lesson these narratives provide for theory is the “equilibrium” aspects of what once was called “institutionalization.” Seeing political order as an equilibrium compels us to analyze it in terms of coordinated expectations; suggesting that even highly institutionalized polities, given informational cascades of possible breakdown, can unravel at breakneck speeds.

State collapse in Africa has also generated a significant narrative corpus. Despite a cogent literature elaborating on the weaknesses of the post-colonial African states (Callaghy 1987; Migdal 1988; Young 1994), professional practice within political science continued to give state officials and state policies priority in its analyses. But with the publication of Bayart (1993), Mamdani (1996), and Reno (1995), a radical shift occurred. In Reno’s image the “Shadow State” -- the set of informal networks of state officials, ethnic chiefs, members of secret societies, local thugs, foreign governments, international firms, and independent traders -- exerts domination over countries in near-total disregard for the apparatus that claims a seat in the United Nations. The shadow state constitutes political authority; the formal state, that is the bureaucratic apparatus that negotiates with foreign governments and makes commitments to international agencies, is a ruse. Shadow state networks can topple formal states, and the costs of sustaining a rebellion are low. Foreign patrons, such as Col. Qaddafi, who has been willing to supply training and weapons to support a gaggle of local insurgencies, are a resource of immense importance in organizing a rebellion. Another resource is international aid from NGOs that comes in response to the collapse of the state. This aid is confiscated and deployed by rebels with the same ruthless energy as smuggled diamonds (Maren 1997). Ethnic ties are another resource, useful for recruiting armed bands of supporters by local tyrants, but these ties are of far less use in many cases (e.g. Bazenguissa-Ganga 1999) than is often portrayed in accounts that are based more on justifications of the rebellion by rebel leaders than on actual observance of the exploitation of resources by rebels.

The most compelling narrative of state collapse that I have read is that of Liberia, by Ellis (1999). In this shocking yet clear-headed exposition of collapse, readers learn of insurgents cutting out and eating the hearts of their enemies, castrating scores of innocent civilians and keeping the excised organs as trophies. In these dramas, rebels rely on renditions of traditional magic as a resource to sustain and extend domination. Ellis argues that the colonial state was only a thin layer covering indigenous systems of rule. The colonial state dissipated in large part because with the end of the Cold War, there were no patrons interested in propping it up. Once dissipated, unconstrained contests for power, in which memories of traditional practices played a powerful role in insurgent strategies, reduced client states into the depths of anarchy. Ellis’s is hardly the last word in accounting for the collapse of the colonial state, in Liberia, in Sierra Leone, in Somalia, in Congo (Brazzaville), in Congo (Kinchasa), and in Cambodia, but it is inconceivable that a good general theory of state breakdown will be written that is not informed by the narrative corpus in which Ellis’s is a model.

Narratives of ethnically based violence have been equally rich. Perhaps the most compelling narratives have been provided by Brass (1997), who examines a range of local incidents, some of which blow up into ugly riots, and become classified as "communal violence" in standard accounts. The clear message of this book is that there is a class of actors known as "riot professionals" who have an interest in turning everyday forms of local violence into a large-scale communal riot. These professionals may be politicians who need the violence to solidify their voting blocs (as confirmed by Wilkinson 1998); alternatively, they may be entrepreneurs who gain profit from the looting that riots promote. Once an incident catches the attention of riot professionals, they seek to activate the masses, who can use the violence to loot for themselves, or to settle scores with local enemies. Kalyvas' (forthcoming) microscopic study of a region in the Greek civil war similarly found a powerful alliance between urban ideologues who had macro agendas such as communism and village actors who had local scores to settle, and were willing to denounce neighbors as enemies of the occupying army in order to justify murdering them. These ethnographic studies of violence show that the solidarity between leadership and killers in civil war cannot be explained simply by pre-existing solidarities, and it must be accounted for in its own right. Furthermore, both the Brass and Kalyvas narratives make clear that ethnically-based and ideologically-based civil wars may have quite similar dynamics. The separation of ethnic war from civil war (as suggested by Kaufmann 1996) as objects of study seems not to be a useful one.

Comparative case studies have yielded some interesting new hypotheses in regard to ethnic violence. Bunce (1999) compares the dismemberment of the Soviet Union, Yugoslavia and Czechoslovakia, also with the goal of differentiating the violent case (Yugoslavia) from those that split apart with minimal violence. She identifies two factors of importance: the interests of the military and whether the dominant national group had its own institutions under the ancien regime. Since there were no unique Russian or Czech institutions in the communist period, post-communist leaders of these republics were compelled to minimize tensions between them and those republics that had their own institutional apparatuses. This minimized the level of violence. The Serbs had their own institutions under the ancien regime, and with a military that had a strong interest in maintaining the federation, violence ensued. Varshney (2001) compares the few Indian cities that have had significant communal violence with comparable cities (in terms of demographics, history, and region) where violence has been minimal. He finds that preexisting patterns of civic engagement, where Muslims and Hindus belonged to labor unions, a political party, or business associations helped cauterize communal conflict before an ugly incident could serve as a spark for violence.

Theory

International relations theorists began addressing the violence that ensued after the collapse of the Soviet Union and the Yugoslav Federation. Posen (1993) recognized the inter-nationality situation as quite familiar: anarchic. Relying on a "security dilemma" framework, he explained cases of violence based upon such factors as a national group's overestimation of the weakness of state authority, and the window of opportunity that chaos held for the fulfillment of long term goals. Walter and Snyder (1999) edited a volume where security dilemma ideas were applied to cases in Africa and Asia as well. However, Fearon (1998) discounted the mechanism of the security dilemma and hypothesized that under conditions of newly gained independence, the ruling faction (or ethnic group) was unable – even if it wanted to -- to commit to the future well being of losing factions (or minority ethnic groups). Under such conditions, minorities would have an incentive to rebel early, rather than wait to see if the cheap talk of the ruling group was honest, because to wait so long would mean having a much lower chance of winning a rebellion.

These international relations models tended to assume that ethnic groups were sufficiently self-organized as to act like states, as unitary actors. The apparent rapid rise of ethnic consciousness and groupness, however, required some explanation. Kuran (1998) suggested that levels of group solidarity had a cascade or tipping quality to them. If you have some weak ties to an ethnic identification, and an increasing number of people similarly situated begin to wear ethnic clothes, perform ethnic rituals, learn historic languages, and portray themselves as members of that ethnic group, the greater the pressure will be on you to follow suit. Depending on people's hidden preferences for ethnic attachment, it is possible to move from complete demobilization to near-total mobilization in a rather short period. Snyder (1993) identified a clear signal that sets off a cascade -- the weakening of the state. This signal increases demand for protection from one's national group.

Insurgencies, however, are not always the result of state failure -- they arise under stable conditions as well. Thus the need for a theory to account for rebellion in light of the failure of the standard grievance models to differentiate countries susceptible to rebellion from those that are not. Collier and Hoeffler (2000) modeled rebellion as the apex of organized crime. Rebels don't extort from shopkeepers as do mafias, but they control the export of primary produce. Leaders of rebellions therefore need sufficient number of followers in order to challenge the state military forces at the various choke points in the sale or export of primary products. Subject to availability of primary products, recruitable looters, and a weak army of the state, rebellions will prosper. Fearon and Laitin (1999) develop a model of insurgency (also opposed to a grievance model) where young men choose whether to join the legitimate economy or to join a rebellion; meanwhile the state decides how many resources to put into counter insurgency. These two simple models, though differently constructed, both help explain why country level GNP (worse job opportunities for youths; and lower predicted levels of counter insurgency spending), availability of primary products, and group concentration of population (especially if concentrated in mountainous zones) are better predictors of rebellion than variables that measure cultural differences or group grievances.

While theoretical work on the question of the breakdown of order and the rise of civil wars within states has been developing rapidly, it has not kept pace with the cross sectional and narrative reports. Findings from state failure, for example, linking state failure to low levels of trade openness, have not been theorized. Nor has the failure in statistical models to find any relationship between grievances, discrimination, and inequality and rebellion received adequate theoretical treatment. Most glaringly, the narrative work has portrayed consequential players (e.g. riot professionals) and has shown high levels of intra-group and inter-state fragmentation, but theory hasn't specified the implications of wars between moderates and radicals among insurgents, or between armies and presidents within states.[19] The greater the attention to the details of disorder, the more powerful will be our future models.

Forms of Capitalism

[19] . The exception is DeNardo (1985) who models intra-rebel dynamics.

As Rogowski highlighted in the previous decadal review of comparative politics (1993), the political economy of the advanced industrial states is a research program of considerable energy and growth, impelled by the OPEC-induced oil crisis of the mid-1970s.[20] The research program, indebted to the seminal work of Alexander Gerschenkron (1962), was that of historical institutionalism.[21] In the classic text of the period (Katzenstein 1978), the dependent variable was that of political strategies among OECD states in adjustment to the collapse of the Bretton Woods system and to the oil shocks. The authors in Between Power and Plenty held that different countries had (given their historical trajectories) distinct institutions, and policy makers were constrained by those institutions in the formulation of strategy. The institutional capacities and interests of Ministries of Finance, Central Banks, commercial banks, and labor unions set limits to and provided opportunities to respond to the economic hard times. Historical institutionalists envisaged a continuum of different types of capitalism, ranging from strong states relative to society (associated with mercantilist political strategies) to strong societies relative to state institutions (associated with liberal political strategies).

Comparative political economy did not settle on clearly identified values on a dependent variable, one for each country/year, and seek to map the impact of a variety of independent variables on the dependent variable. In different studies, economic growth, economic stability, wage equality, redistribution, the social groups paying most heavily for the costs of readjustment, and political strategy were featured on the left side of the field's equations. But in the 1970s there was an implicit and as we shall see by the 1990s an explicit sense that these outcomes formed into coherent packages that Esping-Andersen (1990) was to call the different "worlds" of capitalism. I am therefore highlighting the dependent variable for this research community as those different "worlds". To be sure, much analysis in this field has specified relationships within each world, for example, the impact on wage equality of the electoral power of different parties (Iversen and Wren 1998). But the glue that holds this field together is the question raised by Gourevitch (1978) as to how distinct political economies (the dependent variable) will adjust to common international challenges (the independent variables).

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[20] . For an insider's guide through this extensive literature, see Hall (1999).

[21] . For a comprehensive account of historical institutionalism as used in comparative politics, see Thelen and Steinmo (1992).

Debates within this research program have been on the causal factors (sectoral balances, timing of industrialization, level of social partnership among classes, and land/labor/capital ratios) explaining the emergence of these distinct political economies. Because of the methodological orientation of the historical institutionalists, the literature they produced was rich in narrative, with case studies (Zysman 1977) and comparisons (Katzenstein 1985a; Gourevitch 1986) being the dominant mode. Even Rogowski’s (1989) strongly theoretical treatise -- where land/labor/capital ratios explained political coalitions -- contained historically-based narratives elaborating the theory with real-world cases.

Less prominent than the historical institutionalists, a long tradition of statistically-based research (much of it done in Europe, but Hibbs 1977 reflects the work on both sides of the Atlantic) was revealing a stability to the institutional patterns elaborated by the institutionalists, with a wide variety of policy outcomes conditioned on the type of capitalism for each OECD country.

In the past decade, globalization and mind-boggling technological change have continued to impact on political economies. In addressing these new impacts, but with forms of capitalism remaining the dependent variable, research has developed along lines compatible with the tripartite methodology. Econometric analyses of OECD-produced data explaining cross-national differences on economic growth, wage equality, and government spending on social services has developed strongly. As would be expected, cross-sectional data analysis compels researchers to specify variables more tightly than “three forms of capitalism”, but the statistical tradition has much still to incorporate from the narrative tradition.

In the past decade, there has been a new attention to theory. Institutionalists did not formalize the patterns that they had discovered. As a result, there were important gaps to be filled. Questions obvious to theorists, such as why there wasn't convergence toward the institutional patterns that were most efficient, did not get addressed. Why, for example, if Britain lacked the political institutions to control the City, could it not construct them to enhance political effectiveness (Blank 1978)? Historical institutionalists did not have a well worked-out answer on what maintained institutional patterns over time? More important, historical institutionalists emphasized the interaction between politics and economic, but did not incorporate this insight into testable models. The theorization of forms of capitalism as equilibria pushed the field to address new questions, and will permit (in the coming decade) statistical tests that highlight (and don’t hide) the endogeneity of politics and economics.

In this review of work in the 1990s, I will first report on the cross-sectional statistical research. Then I will report on theoretical developments. Finally, I will discuss the narrative work that continues to flourish in the historical institutionalist tradition, with an eye as to how the three approaches can be better integrated.

Statistics

“Forms of capitalism” is a vaguely specified dependent variable, and its values are nominal. This suited the historical institutionalists, who were more interested in coherent narratives than high r-squares. But there is a more compelling reason for the vagueness of the dependent variable in the political economy of the advanced industrial countries than the interests of institutionalist practitioners. Consider the problematic of the field a decade ago, as seen by Rogowski (1993), who tried to refashion the historical institutionalist literature into one that would be more subject to econometric testing. For him, the consequential dependent variable was comparative economic growth. Given the economic recession caused by the oil shock of the 1970s, the following variance required explanation: “Among the economically advanced nations, the continuing Japanese ‘miracle’ and the quite respectable growth of the continental European economies [that] contrasted sharply with the dismal record of the U.S. and the U.K.” (1993, 431). A variety of theories was offered. Peter Hall (1986), for example, sought to account for Britain’s long economic decline based on a theory of economic ideas. Others stressed interests and institutions. However, a decade later the countries were reversed in growth records, and explanations for economic decline had to account for Japan’s long recession rather than America’s. The more compelling reason for the vagueness of model specification is that the world economy has been changing so rapidly that it is hard to place political units on any important dimension, and have confidence that the relative values for those political units would stay sufficiently stable as to allow for a community of scholars to account for the variance.

In this same period, data from OECD states on a wide variety of dimensions became available. Some (such as inflation) were produced and standardized by governments themselves; others, such as indicators of corporatism (Schmitter and Lembruch 1982) or central bank independence, required careful construction by the scholarly community; others still, like union concentration, were built from virtual scratch by the scholarly community (for a preliminary analysis of a new data set, see Wallerstein, Golden, and Lange 1997). These data allow for the statistical tests of many theoretical speculations. From political responses to the OPEC crisis, comparativists moved to other variables on the left side of their expressions: explaining the trade-off on inflation vs. unemployment; explaining the level of trade openness; and explaining variation in wage equality.

Much of this statistical work showed in many different forms (what European social science had been finding for decades) that social democracy is stable, is associated (in contrast to free market liberalism) with a larger government sector, greater equality, the public investment in task-specific technical skills, yields growth advantages in some sectors and has a comparative advantage (over liberalism) in the face of economic shocks (Cameron 1978, Hibbs 1977, Garrett and Lange 1986, Garrett 1998). Furthermore, there is a third form of capitalism, associated with Christian Democracy, that presents a unique package of high equality, low taxes, and by so doing sacrificing growth (Swenson 1989, Iversen and Wren 1998).

Perhaps the most hotly debated issue in the study of the comparative political economy of the advanced industrial states is in assessing the impact of globalization on the different forms of capitalism. A consensus view in the field is that the impact of globalization is strong, but its impact remains obscure. Some, such as Rodrik (1998), see demands for widespread growth in government spending, especially welfare spending, as a form of insurance against the shock of job loss and social dislocations in the face of globalization, at the terrible cost of losing all mobile capital. Others, such as Scharpf (1991) and Lambert (2000) see globalization undermining the welfare state in even solidly corporatist governments. Garrett (1998) is far more optimistic about democratic corporatism, and sees it as a best response to the forces of globalization, cushioning market dislocations and providing lucrative investment sites for mobile capital. Iversen and Cusack (2000) challenge the consensus, and argue that the effects of globalization are weak, in comparison to technological changes in production. To the extent that those who see the forces of globalization to be strong, we should expect increasing convergence of structure and strategy of OECD states; to the extent the Iversen/Cusack position is correct, we should expect variations in economic growth, in growth of the welfare state, and in wage equality, depending on technological profiles of state economies.

Theory

Hall and Soskice (forthcoming), in specifying historical institutions as equilibria, have begun to connect the work of the historical institutionalists and the statistical analysts. "Since its inception in the 1960s," they write, "one of the principal objectives of the modern field [of political economy] has been to explain cross-national patterns of economic policy and performance. Its central theme has been the importance of institutions to economic performance, and substantial efforts have been made to identify the institutions that condition such patterns." Relying upon endogenous growth theory, which would have us predict that different national rates of growth are conditioned by the institutional structure of the national economy, Hall/Soskice focus on comparative institutions. Since institutions affect the character of technological progress and rates of economic growth, understanding institutions as equilibria plays a direct role in explaining growth. [22]

The Hall/Soskice approach is based on the "new economics of organization", with the firm as the fundamental unit in a capitalist economy adjusting to exogenous shocks. In the model, firms reduce risk by making commitments to their workers and to other firms, and this occurs in several spheres: (a) bargaining over wages and working conditions; (b) securing a skilled labor force; (c) getting finance; (d) coordinating with other firms, e.g. on standard setting; and (e) getting employees to act as agents of the firm. Strategies to resolve these commitment/coordination problems are conditioned on the national institutional environment. The "national political economies" are the principal units of analysis, as "we expect the most significant variations in institutions and firm strategies to occur at the national level," and their "regulatory regimes" that are the preserve of nation-states. The principal dimension on the dependent variable is nations "in which firms coordinate their activities primarily via conventional market mechanisms (liberal market economies [or LMEs]) and those in which firms make substantial use of non-market forms of coordination (organized market economies [or OMEs])." LME's are Coasian, keep arms length from other

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[22] . This move, to see the political foundation of modern markets, was foreshadowed by Ruggie's (1983) notion of "embedded liberalism."

firms, engage in formal contracting; OME’s have much incomplete contracting, widespread sharing of private information, and more collaborative inter-firm relations. In equilibrium, LME firms should invest in “switchable” assets that have value if turned to another purpose; OME firms should be more willing to invest in asset specificity, which depend on the active cooperation of others. The separate components of these political economies are complementary, in the sense that high returns from one component entail high returns for another component in the system. So OME firms that give long-term employment contracts profit when they are in a financial system that doesn’t punish short-term losses. Complementarity explains the clustering among the solutions to the commitment problems across the spheres of risk.

The model makes predictions in regard to the exogenous shock of globalization. The microeconomists’ assumption of pressures to liberalize everywhere, they argue, is based on the (wrong) view that the key to profitability is lower labor costs, which is true for LMEs but not OMEs. Thus the hypothesis that under globalization, OME firms might locate to LME countries in order to get access to the radical innovations; meanwhile LME firms may move some activities to OME settings to secure quality control and publicly provided skills to labor. (Here they would make a different prediction from Garrett 1998, who sees the OME as a superior equilibrium in the face of globalization). A second hypothesis is that conflict between labor and capital in the face of globalization will be low in OMEs, where capital and labor often line up in support of existing regulatory regimes (and where labor unions will remain strong); but high in LMEs, where business is pushing hard for deregulation of labor markets (and where labor unions will weaken). The social cleavages that result will therefore be different in the two political economies.

The Hall/Soskice approach takes account of many of the cross-sectional findings in the comparative political economy field, most importantly the apparent stability of social democracy under a wide range of challenges, but also the inter-correlations of high government spending, social welfare provisions, and union density, that come together as a package. Once, however, you endogenize politics and economics, new forms of statistical testing (as suggested in Alt and Alesina 1996) are in order and remain on the agenda. This approach also takes into account the principal framework of the early historical institutionalists, who took for granted the equilibrium properties of the different forms of capitalism. It makes sense of why we should expect some degree of cross-national variation in effects of the apparently homogenizing force of globalization. (For a complementary theoretical account of why we should expect greater heterogeneity as a result of globalization, see Rogowski 1998). And finally, it presents a compelling alternative to price theory, which sees institutions as constraints to efficiency, but not as sets of equilibria that are dynamically stable. But, as we will see in the next section, the findings in formal theory diverge somewhat from a new generation of narrative work in historical institutionalism.

Narratives

The historical institutionalists have continued to write empirically dense narratives that speak directly to the dependent variables that have defined statistical and formal research, but its impact on the practice of statistical and formal modelers has been limited. Consider Katzenstein (1985a, 1985b). He sought to explain how political stability in the small European states could be maintained under conditions of enormous economic flexibility. The answer was in corporatist governance. He identified two sub-types of the democratic corporatist form of capitalism, liberal and social. Like many of the historical institutionalists, he provided an historical account for these patterns, highly influenced by the structural factor of smallness making these states “takers” rather than “makers” of international rules. What distinguished the second volume (Katzenstein 1985b), however, was the careful sectoral analyses in Austria (the social corporatist example) and Switzerland (the liberal corporatist example). In these narratives, the ideology of social partnership coming from a common sense of vulnerability plays an important role in sustaining country-wide institutions (1985a, 87-89). This ideological variable is hard to specify for more general explanations, but it should have paved the way for future cross-sectional and theoretical work that encompassed this factor (as well as other variables identified in the narratives), as a test of the magnitude of their effect on sustaining historical institutions. In general the fuzzy variables that attracted the historical institutionalists as consequential rarely find their way in cross-sectional statistical research or in formal theories of the market.

In the 1990s, with questions turning toward breakdown of institutional differences, Ronald Dore and Suzanne Berger (1996), having observed industrial processes in Japan and Europe, were convinced that national political economies would retain their institutional integrity. They commissioned a set of narrative studies to assess the extent of political and market convergence in light of economic globalization. Their intuitions were in large part confirmed, as were those of the historical institutionalists in regard to the oil shocks. National institutions were retaining their historic peculiarities in the face of globalization. Thelen and Kume (1999) find similarly in regard to labor policy in Germany and Japan. It is notable that these studies have not compelled those who have emphasized the homogenizing impact of globalization to respecify their models, to figure out why, at least in the short run, the world isn't conforming to their predictions.

Now consider Pierson (1994). He uses the narrative mode in comparing the conservative attempt to dismantle to the welfare state under Thatcher and Reagan. By examining two of what Hall/Soskice call LMEs, one would have predicted that under conservative governments, there would have been significant retrenchment. Pierson finds, instead, from a careful narrative of conservative challenges to a set of welfare programs, grand goals but very limited success. Seeking to explain the failures to cut back programs conservative leaders considered inefficient and even evil, Pierson finds institutional structure to be of quite limited power. For example, consider the institutional variable of veto points. The numerous veto points in the US system as compared to the few such points in the UK would lead to the prediction that given the same goals, Thatcher would be more successful than Reagan. Wrong. Reagan, on the margin, was more successful. Pierson finds, instead, that the relative success of programs could be explained by the very features of the programs being dismantled. He calls this "policy feedback", a variable that has not been explored in the statistical literature. It would not be easy to explore statistically, since every policy has many dimensions of policy feedback, some allowing for easy dismantling, others blocking any change. Consequently, there is no simple value of policy insulation for such programs as US Social Security, UK National Health Service, or unemployment programs in both countries. Pierson suggests that, "a more promising strategy is to develop middle-range theories that acknowledge both the complexity of feedback and its context-specific qualities" (p. 171).

But two more concrete proposals suggest themselves. Given the differences the cross-sectional and theoretical literatures find between LMEs and OMEs, Pierson's study should be replicated across this divide to see if policy feedback is consequentially important in the same way in two different political economies. The Hall/Soskice portrayal of LMEs suggests that they would be far more capable of dismantling welfare state programs (and their theory would predict that by holding steady against the welfare state, Reagan and Thatcher effectively held back its predicted development given growth in GDP);[23] but Pierson’s study suggests an alternative -- that it isn’t institutional structure but the policy complexities of welfare state benefits that make them resistant to exogenous shocks. Pierson (1996) examines four cases that do cross the OME/LME divide (Sweden and Germany are added), and finds in contrast to the Hall/Soskice portrayal, that there is no clear evidence of OME relative success in sustaining the welfare state than in LMEs.

Second, Pierson’s list of programmatic criteria should be organized such that programs across a set of countries could be coded; and then it would be possible to do some exploratory statistical tests on “policy feedback.” In Pierson (1996) data on relative retrenchment are presented cross-nationally; but no attempt is made to capture policy feedback (and a set of other proposed independent variables, pp. 176-78) with cross-national data. (More progress is made in Myles and Pierson forthcoming). Narrative (supplemented by some informal but sharp theorizing) uncovered a plausible variable to explain crucial outcomes in political economy; this variable requires attention in the formal and cross-sectional domains. [24]

Still to be assimilated by scholars in the comparative political economy field, Herrigel (1996) in his historical examination of German industrialization finds that the notion of a national economy with its peculiar institutions to be a sham. Close examination shows that there has long been two intersecting German institutional frameworks, and each with its internal logic. The implication of Herrigel’s work is that future cross-sectional studies are making a grave error to the extent that they use OECD tapes that rely on country-level trends. To be sure, if central bank independence or monetary policy is the key independent variable, this may present no problem. But if variables such as Katzenstein’s (density of social networks) are being tested, Herrigel’s work demands that we disaggregate our economic data to the lowest administrative level. To develop such data

[23] . Pierson does not perform general equilibrium tests of his model. This may help to explain why he believed that the massive budget deficits incurred by Reagan would long endure, and help conservative successors, arguing fiscal necessity, to dismantle other parts of the welfare state.

[24] . Given Lambert (2000), we see that dismantling the welfare state (in Australia) is not as formidable a task as Pierson’s book suggests. This variation can easily be taken advantage of in cross-sectional work.

(though OECD is beginning to collect some data at the level of region) would be an enormous enterprise. But if the variables pointed to by Katzenstein and Herrigel are seen to be consequential, there can be no alternative than to seek major funding for far more disaggregated economic data than OECD supplies the research community for free.

The theoretically informed narratives of the historical institutionalists present several clear challenges and opportunities to the statistical and formal models in comparative political economy. The overall research program remains vibrant -- it lacks only the sense of challenge to reconcile inconsistent findings across the tripartite methodology.

Conclusion

This has not been a comprehensive review of the field of comparative politics in the 1990s. Rather, it has taken three dependent variables, broadly specified, to illustrate the tripartite methodology that is emerging as standard within the field. In this conclusion, I shall mention a set of dependent variables that are the focus of considerable research in the subdiscipline, but which I have not reviewed here. I will then summarize the most stunning substantive findings in the field in the early 2000s for the dependent variables that I have treated. Finally, I will suggest an agenda to sustain scientific progress in comparative politics for the coming decade.

There are several dependent variables that have attracted considerable attention in the comparative politics subfield in the past decade. Here I will mention two. Research seeking to explain the selection of political institutions (electoral laws, parliamentary vs. presidential systems, the central bank) has been especially vibrant, in part a reaction to a problem posed by Riker in 1980. If institutions are so important for political outcomes, he asked, institutional choice would have to be thought of as endogenous to the political process. This area of research has been further propelled by the constitutional craze of the post-Soviet republics, the wave of new democracies, and the institutionalization of the European Community as a political unit. How these institutions got selected remains but a question on the comparative politics agenda. Research on the formation and re-formation of political identities has been another growth area. This harks back to the questions of political cleavages that seem set in stone that Lipset and Rokkan addressed in their 1967 paper. The apparent switch from “class” identities as defining political mobilization to “ethnic” identities has also motivated this research. As with Lipset and Rokkan, many comparativists seek to uncover the relationship of mobilized identities and political party formation. My purpose is not to review those literatures in the course of this essay, but to state as an observable implication of my thesis in this paper that researchers relying on each of the three elements of the tripartite methodology are interdependent, whether they want it that way or not, as all are addressing the common question of variance on these dependent variables.

I shall now choose what I consider the most robust finding over the past decade in accounting for each of the three dependent variables that have been addressed in this paper. In regard to democracy, the finding that high GDP per capita helps explain the consolidation of democracy but not its initiation is a finding of great importance. In regard to order, models that seek to explain ethnic civil wars using information about the culture of the group seeking secession or control over the state have failed. Similarly models using information about grievances perpetrated on the minority by the rulers of the state have failed as well. These failures have opened up the way to account for these wars with country-level data such as per capita GDP, population size, terrain, and economic growth. In regard to forms of capitalism, we have learned that each form of capitalism constitutes a robust equilibrium, and is far less subject to homogenizing effects that one might predict in looking at globalization and the revolution in electronics. If this last perspective is correct, my successor writing the review of comparative politics for the next decade will not have a section on the causes of the demise of social democracy.

A final question: what does this review imply for the organization of research in the comparative field of the future? I think it is unreasonable to demand that all comparativists have highly cultivated statistical, formal and narrative skills. I also believe it would be a great loss to the political science discipline if one of these skill sets were to define the field, and diminish the presence of colleagues who had skills in the other elements of the tripartite methodology. One great fear is a Chomsky-like revolution in comparative, where the formal theorists drive out of the discipline the field workers. An equal fear is if the field workers put up barricades separating themselves from the findings in the formal and statistical worlds. Utopia is my (admittedly shallow and perhaps naïve) understanding of the social organization of physics. In that discipline, there is a division of labor between the experimentalists and theorists, and the same level of disrespect across methods as we have in comparative politics. But the difference is that in physics, it is unimaginable that the experimentalists would ignore the implications of the most recent theoretical findings, if only to blow them out of the water. Meanwhile, theorists grudgingly seek to account for empirical realities that experimentalists report. Methodologically, this review finds that in comparative politics, interdependence across the tripartite methodological divide, with grudging toleration built on mutual suspicion of practitioners across the divide, is a key to scientific progress. Those in the narrative tradition often see a Manichean world of them vs. the quantitative folk; here it is shown that formalists and statisticians also face challenges in reacting to each other’s developments. This review shows specks of evidence that despite difficulties inherent in any division of labor, a common focus is emerging. This is a focus on consequential dependent variables and an joint attempt to address variance across polities on these variables, by scholars working within three methodological approaches. Those scholars who are part of this division of labor are remaking the comparative method.

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