

## Single Country Studies and Comparative Politics

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In some of the conventional sub-fields of political science—“American politics” springs to mind—the question of whether single country studies are obsolete would be dismissed as absurd. Generalizable findings about important intellectual questions ranging from the origins of bureaucratic autonomy (Carpenter 2001) to the role of mass and elites in public opinion and social movements (Lee 2002) have been the subject of recent, rigorous monographs that look only at the United States. There is no change in tastes or technologies on the horizon that will render such studies obsolete in the foreseeable future.

For a specialist in comparative politics, though, the question posed for this symposium—are single-country studies obsolete?—is slightly harder to dismiss out of hand. It should not be. Comparative politics is fundamentally about using comparison across different units of analysis to delineate the causal mechanisms that explain variation among political, social, and economic outcomes in those units and beyond them.<sup>1</sup> Those units of analysis are often individual countries, since many variables of interest are defined at the level of the nation-state, as are the statistics that track them. Yet the appropriate unit of analysis for any theoretical question should always be determined by the content of that question. The greater ease of acquiring comparable quantitative indicators, and the potential for exploiting both temporal and spatial variation through regression techniques that use pooled cross sectional time series, are technological advances that may well have given impetus toward the perceived obsolescence of the single country study. While both national comparisons and advanced statistical techniques using such data have moved knowledge forward in a variety of fields of inquiry within comparative politics, neither has rendered obsolete the potential for generating important and generalizable findings from single country studies. In the remainder of this essay I will review some of the important advantages of these studies over multi-country comparisons.

Perhaps the most widely used “great book” in graduate courses on comparative politics in the United States is a single country study: Robert Putnam’s *Making Democracy Work*, a book familiar to every reader of this publication (España-Nájera et al. 2003). This book has, rightly, generated substantial praise and criticism, and I have no intention of repeating either here (cf. Tarrow 1996, Boix and Posner 1998). Instead, let us use the example of Putnam’s study to highlight the features of single country studies that can be so appealing to scholars of comparative politics.

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<sup>1</sup> Or as Robert Putnam says, comparative politics is all about generating “portable expectations.”

Putnam exploits the possibilities for comparison that exist both sub-nationally (among Italian regions) and temporally (Italian regions today vs. Italian regions in past time periods). Sub-national variation among administrative units is the most easily available strategy to practitioners of a single country studies, and it has the great virtue of holding many other potentially causal variables constant. Multi-country studies using regression techniques can throw in a vector of control variables, including country dummies, to deal with this problem; but, so often, the institutional and cultural features that make the country dummies significant are themselves integral to the causal story that is being told. Sub-national comparison is no silver bullet in this regard, especially in a country as internally heterogeneous as Italy, but it is a valuable asset of the country study.

Temporal variation is also useful, particularly since many contemporary debates in political science are concerned with issues of sequencing and contingency in causal analysis (Pierson 2004, Sewell 1996). Putnam uses this temporal variation to great effect, both to demonstrate that social capital at time  $t_0$  is a better predictor of economic development at  $t_1$  than vice versa, and to illustrate his argument for the origins of divergent patterns of civic engagement hundreds of years before the regional reforms he studies. In the area of comparative politics in which I work most extensively—comparative political economy—we can observe these same sorts of strategies at work.

Indeed, in comparative political economy, one important debate is whether national level “varieties of capitalism” are even appropriate units of analysis, given the heterogeneity within the units. Hall and Soskice (2001) have developed an influential rationalist model of the institutional differences that structure modern capitalism, using insights from the new economics of organization to demonstrate the institutional complementarities that distinguish liberal market economies (LMEs), such as the United States, from coordinated market economies (CMEs) such as Germany. Hall and Soskice focus almost entirely on inter-country variations in the institutional structure of capitalism to explain what they observe as “systematic differences in corporate strategy across LMEs and CMEs” (2001: 16).

There are good reasons to focus on the national-level of variation in comparative political economy—many of the institutions emphasized in that literature depend on national level regulations, as in the areas of industrial relations or of finance. Yet we do not know that the most important systematic differences in corporate strategy are generated among countries, rather than among *regions* of the same country or among *sectors* across different countries. Two prominent single country studies—Gary Herrigel’s *Industrial Constructions* (1996) and Annalee Saxenian’s *Regional Advantage* (1994)—have shown significant regional variations in the sort of institutions stressed by Hall and Soskice *within* the two national cases they take as paradigmatic: Germany and the US.

Tracing both cross-sectional and temporal variation, Herrigel’s book shows how the “German political economy” is really made up of at least two distinctive social and industrial models, each of whose influence on the country’s productive tissue has varied over time. The decentralized industrial order, which has many of the features characteristic of the stylized model of the German CME depicted by Hall and Soskice, is

regionally concentrated in southwestern Germany. Other industrialized parts of the country are characterized instead by an autarkic industrial order, in which patterns of inter-firm horizontal collaboration are not at all like those described by Hall and Soskice. Analogous regional variation has been well documented in the Italian case (cf. Locke 1995), but also in France, a country not typically associated with regional heterogeneity (Culpepper 2003, Hancké 2002).

In a similar vein, Saxenian's book shows how high-tech industrial infrastructure in two American sub-regions—Route 128 and Silicon Valley, which are of course indistinguishable in the sort of framework adopted by Hall and Soskice—have profoundly different local institutional contexts. And it is local institutional context that proved to be decisive in determining the ability of these regions to nurture companies bent on pursuing innovation in the computer and electronics industry during the 1970s and 1980s. One recent puzzle within the varieties of capitalism literature is most likely answered by the sort of local informal institutions studied by Saxenian, Herrigel and others. The varieties of capitalism literature holds that CMEs are poorly equipped to promote radical innovation, but the literature lacks an answer so far as to why CMEs Sweden and Finland have been so successful in promoting high-tech companies (Ericsson, Nokia), while Germany has lagged in such fields. This question, as with most of the fundamental questions in comparative politics, will need to be answered by considering variation among country cases. Yet it could well be a single country study, attentive to the temporal and cross-sectional variations within one of these countries, that provides insight into the answer.

Some scholars may not be persuaded by such an argument. "OK," the skeptic may say, "but Hall and Soskice (and much of the rest of the field) are right that most relevant regulations are national, so these local variations in regional, informal institutions are of minor significance. We care most about what is happening on average, and the most important structures of national political economies are determined by national regulation." This is a slightly crude version of what I take to be the dominant view in the field of comparative political economy. In an important recent contribution on *the* major regulatory trend of the recent era—neoliberal reform—Richard Snyder (2001) has deftly critiqued and underlined the limitations of this view: "the subnational comparative method helps overcome a major limitation of existing work on the politics of neoliberal reforms: extreme dependence on aggregate, national-level data. This striking 'whole nation bias' has obscured the possibility that neoliberal reforms, rather than unleashing market forces, can trigger reregulation processes at the subnational level" (Snyder 2001: 16).

Looking at the coffee sector in four regions in southern Mexico—one country, one sector—Snyder explores the dynamics of how different regional policy regimes emerge from the interaction of three variables: the content of the regional regulatory project (following national deregulation), the response of small coffee producer organizations, and the strength of those organizations. If we were coding the Mexican coffee sector as either deregulated or regulated, we would be excluding much of what Snyder discovers to be the real politics of "deregulation": whether and how it is implemented across a

country. Snyder's work generates a set of interesting hypotheses that others can apply in different national cases, exactly as any comparativist aspires to do. What makes his findings plausible and generalizable is the rigor of his subnational comparison, a rigor that could well have been sacrificed in a multi-country research design that necessarily trades off some depth of analysis for the greater country breadth achieved.

None of this should be construed as an attack on studies that compare multiple countries. Most of my own work is indeed of this sort, and the virtues of studies that are able to compare multiple countries are well-known. The argument in this essay should be read instead as a defense of methodological pluralism in comparative politics, and perhaps even more narrowly for comparative political economy. Other areas of inquiry within comparative politics have been less quick to move away from single country studies. Mark Beissinger's (2002) magisterial analysis of the rise of nationalism and the break up of the Soviet state was indeed a celebrated reminder not only of the important possibilities of single country studies, but of the fact that today's sub-national region is tomorrow's independent country.

The single country study has lost none of its capacity to have generalizable implications since Tocqueville published *Democracy in America* or Robert Dahl *Who Governs?* There will be bad single-country studies, as there always have been, but there will also be great ones. Those that are particularly effective maximize their explanatory leverage by exploiting the availability of comparable units of analysis, whether over time, space, or some other organizational dimension of variation. The ease of making cross-country comparisons using readily available, but sometimes misleading, aggregate national data certainly does not render the single country study obsolete. If anything, such work provides an important correction of existing work that may flatten the variegated political texture of sub-national regions in order to generalize about current trends.

The danger of obsolescence of the single-country study does not lie in technological or methodological advances. Instead, I suspect that the most serious threat in rendering this valuable tool of inquiry obsolete lies with changing tastes: in particular, the tastes of purchasers of books as refracted through the demands of publishers. Those publishers have to sell enough books to make themselves viable in the marketplace, and in comparative politics I would guess (without hard evidence) that publishers of serious university presses are less likely to gamble on the publication of a single-country study than they were fifteen years ago. If this speculation is right, it would be a pity if the next *Making Democracy Work* goes unpublished because its focus on a single country is perceived as of insufficiently broad. Comparative politics, at its best, uses comparative cases to illuminate general mechanisms of social and political causation. It is this test, and not the number of countries in a study, that should remain the benchmark against which we assess the quality of comparative research.

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