This article emphasizes comparative politics research that combines the tools and theories of economics, primarily microeconomics, with the tools and theories of political science. It traces and assesses the transformation of comparative political economy into an approach that actually is both political and economic and outlines a research agenda for the future.

THE ECONOMIC TURN IN COMPARATIVE POLITICS

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> "The existence of the state is essential for economic growth; the state, however, is the source of man-made economic decline."
>
> —Douglass North, 1981, p. 20

What is the role of government in creating or inhibiting economic growth and development? How do a state's economic policies affect equality and liberty? How do economic factors constrain political decision making? These are the central questions of comparative political economy. They represent at least part of the agenda of the great 18th- and 19th-century political economists—Adam Smith, David Ricardo, and Karl Marx among others—and of those economists who took up this tradition again in the early 20th and mid-20th century, among them, John Maynard Keynes, F. A. von Hayek, and Joseph Schumpeter. These questions are most certainly at the heart of the research of those who today label themselves *comparative political economists*. However, they are transformed by a generation of economists and political scientists who take politics and economics seriously, something too few of their precursors did.

AUTHOR'S NOTE: *I received extremely useful comments on earlier drafts from Kenneth Shepsle, James Alt, Steve Hanson, Debbi Elms, Cynthia Horne, Mary Alice Pickert, Karen Remmer, and especially James Caporaso.*

COMPARATIVE POLITICAL STUDIES, Vol. 33 No. 6/7, August/September 2000 822-844

Smith, Ricardo, and the other classical political economists asked macro questions from firm micro foundations. They were comparative political economists in their attempts to understand processes of political economic development and variation between countries. They did not emphasize politics per se, but they and their critics, for example, Karl Polanyi and Alexander Gerschenkron, were attentive to how governments affects economies. They were concerned, as were Hayek and, later, Milton Friedman, that government interventions in the economy can have profound disincentive effects and detrimental consequences for liberty. Maynard Keynes did not doubt this, but he also recognized the possibility of welfare-enhancing government policies, beyond the provision of justice and the protection of property rights.

Marx had the greatest initial influence on the other social sciences for Marx was the political economist most concerned about how the economy influences politics and, more precisely, how property rights affect the form of government. His impact on development and modernization theorists was profound. The interrelationship between the economic structure and political institutions was—and remains—a focus of considerable scholarly attention. However, what comparative political economists generally take from Marx is his structuralism. Few fully appreciate the underlying micro analytics.[1]

Marx and the other grand economists may have influenced the agenda of comparative political economy in political science, but most had little effect on the method. The economists tended to ignore politics (and still generally do); only Schumpeter (1942) provided enduring insights about how economic models might illuminate political behavior. Political scientists shied away from economics. However, until the 1960s, comparative political economy neither made full use of the tools of economics to understand distinctly political phenomena nor attempted to transform economic theory to make it a truly political economy. There were numerous investigations of the relationship between economic and political variables, but these generally took the form of descriptive case studies or, by the second half of the century, statistical correlation.

By the late 1980s, comparative political economy had evolved at least two variants.[2] The first involves a representation of the economy and relies on economic variables, but it does not necessarily use economic methods or call on economic theory. Research tends to be on economic policy making by and the economic performance of national governments, in both the advanced industrial and in the developing countries. Examples of this kind of work are Amsden's (1989) *Asia's Next Giant* and Wade's (1990) *Governing the Mar-*

1. A few did, of course. For example, see Roemer (1986). Also see Elster (1985).
2. See Caporaso and Levine (1992) for a somewhat different typology of political economy.

ket, winners of the 1991 prize for the best books in the American Political Science Association's section on political economy. The second variant applies microeconomic methods to political behavior and to institutional emergence, stability, and change. The research relies on economics but does not investigate the economy. Cox's (1997) *Making Votes Count*, the 1998 winner of the political economy award, exemplifies this mode of research. The first variant explicitly deals with economic issues and may or may not use economic models. The second explicitly invokes economic models and may or may not address economic issues.

These two approaches are not mutually exclusive. Increasingly, some of the most interesting scholarship in comparative political economy combines a representation of the economy with microeconomic methods. Illustrative are Bates's (1997) *Open Economy Politics*, Iversen's (1999) *Contested Economic Institutions*, and Boix's (1998) *Political Parties, Growth and Equality*, which earned the 1999 prize in political economy. At the same time, rational choice scholars known primarily for their work on parties and legislatures now take on more general questions of economic and political development and change. Schofield's (2000) recent contribution to the *Annual Review of Political Science* is a case in point, as are some of Weingast, Montinolo, and Qian's (1995) arguments about market-preserving federalism or Geddes's (1999) most recent work. For scholars such as these, the dividing lines between the two previous variants are often thin and sometimes nonexistent.

The purpose of this article is to consider what the program of comparative political economy might be in the next decades rather than to provide a comprehensive literature review. The emphasis is on research that combines the tools and theories of economics with the tools and theories of political science; thus scant attention will be given to a great deal of excellent work often classified as comparative political economy. The article also gives short shrift to institutionalism, given that it is the subject of two other articles in this issue by Carey (2000 [this issue]) and Scharpf (2000 [this issue]). The next section of this article focuses first on the transformation of political economy into an approach that actually is both political and economic. The second section addresses the cumulative contribution to understanding political phenomena derived from research in comparative political economy. The emphasis will be on ideas and evidence revealed by reliance on this approach. The penultimate section offers an evaluation of the mutual benefits to political science and economics that political economy produces. The final section outlines a research agenda for the future.

ECONOMICS APPLIED TO POLITICS

Contemporary comparative political economy now refers as much to the application of economic reasoning to political phenomena as to the effects of politics on the economy or the economy on the polity. Increasingly, those who call themselves political economists must actually learn the theory and methods of contemporary economics and the theory and methods of political science. It took a long while for political scientists to make use of marginal utility theory, which transformed economics at the turn of the past century. Until there were demonstrable applications to questions of institutional design or political mobilization, the microeconomic method held little appeal for political scientists. Game theory, with its emphasis on strategic interaction and suboptimal outcomes, had a much shorter period of gestation before its adoption by political scientists. The turn to modern economic reasoning and models in the investigation of political problems and phenomena took place much earlier in American politics than in comparative politics, but it is now entrenched in both.

A short history of the evolution of contemporary comparative political economy helps make sense of this alteration. The precursors are in fact representatives of two quite distinct traditions in economics that then produced what were, until recently, the two distinct variants in comparative political economy. The first is framed by Keynesianism and represents either a development of his arguments or a theoretical alternative, represented most famously by Milton Friedman and later by the supply-side economists and those using models of rational expectations. The second is more rooted in microeconomics and game theory applied to politics, that is, rational choice or public choice.

POLITICS AND ECONOMICS

Lindblom (1977) was hardly the first (also see Mills, 1956), but he was one of the most persuasive of those who raised the question of how the distribution of economic power affects the distribution of political power and the behavior of governments in democracies. This structuralist approach echoed the neo-Marxian literature (e.g., Block, 1977; Bowles, Gordon, & Weisskopf, 1984; O'Connor, 1973) and reached its analytic climax in two seminal articles by Przeworski and Wallerstein (1982, 1988) and in Przeworski's book (1985) *Capitalism and Social Democracy*, which attempted to clarify the nature of the constraints capitalism imposes on government economic policy and on democracy.

Although it was principally radical economists who considered the causes of or the antidemocratic effects of the distribution of power, there were mainstream economists thinking about how economic policy making both is influenced by and influences the economy. With Keynes came the presumption that fiscal policy is a tool for adjusting the economy. Although M. Friedman (1968) and the monetarists critiqued this perspective, they also saw a role, although a far more minor role, for government through its monetary policies.

It is not surprising then that economists and political scientists began to consider the reasons that governments choose their policies. The concept of political business cycles generally builds on the Phillips curve assumptions that there is a trade-off between unemployment and inflation. Nordhaus (1975) was the economist most credited for pioneering this approach, but political scientists Hibbs (1976) and Tufte (1978) soon offered alternative versions of the political business cycle. There is little evidence for any of these models (Alt & Crystal, 1983, pp. 103-125; Boix, 1998, pp. 6-8), and first M. Friedman (1968) and then the rational expectations economists, for example, Lucas (1972), debunked the Phillips curve itself and questioned the extent to which fiscal macroeconomic policy can actually affect the economy.

Nonetheless, the model of partisanship, which Hibbs (1977) largely initiated, still animates research, as will be discussed below. There continue to be investigations of the claim that political parties on the Left are more likely to support policies that reduce unemployment and that parties on the Right are more likely to support policies that emphasize inflation control, but the arguments are now supplemented by political economic models and theories that make the claims more logically coherent and more testable.

RATIONAL CHOICE

It is not necessary to ally with rational choice to be a comparative political economist, but rational choice in comparative political economy has become increasingly prevalent. Even so, the application of microeconomic theory to comparative political phenomena takes several forms. One distinction rests on the subject matter. Some scholars emphasize electoral and legislative behavior, whereas others are more interested in popular mobilization or problems of economic and political development. A second distinction is methodological. There are those who apply, even develop, sophisticated formal models and game theory, and there are others who use a variant of rational choice that relies on the logic but not the actual mathematics of microeconomics. Myriad other differences also exist, some reflecting serious theoretical disputes and some the division of labor between those most apt at formal modeling and those concerned with analyzing data and testing hypotheses with evidence and logic. These are not hard and fast lines of division; increasingly, scholars are able to engage in both formal theory and fieldwork, and many scholars study both legislatures and development or collective mobilization and government policy.

LEGISLATORS, VOTERS, AND POLITICAL PARTIES

Rational choice begins with several important contributions to American politics by economists. Arrow's (1951) impossibility theorem raised the question of whether any rules exist that will permit the outcomes of legislative votes to be both democratic and rational. Downs (1957) used the Hotelling spatial model to produce a seminal analysis of the centripetal tendencies of political parties in a two-party system. He also applied economic reasoning to voting behavior, concluding that there was little rational basis either to vote or to invest in the collection of information about candidates and platforms. The works of Arrow and Downs raised significant issues about the limits of democracy. Their concern was with the rules inherent in the democratic process rather than with the disproportionate influence of business or the structural constraints imposed by the economy.

It is Riker (1962), however, who deserves the primary credit for recognizing the importance of economic theory for understanding politics. Riker, as a scholar and an institution builder, ensured that game theory and spatial modeling became invaluable tools in the investigation of the origins and maintenance of political institutions.[3] *The Theory of Political Coalitions* (Riker, 1962) was an application of economics to political questions, but it was an application from the perspective of a political scientist who clearly distinguished the political and economic spheres. For Riker (1962, p. 11), collective outcomes in politics result from deliberation and conscious processes by rational and strategic actors interacting with other rational and strategic actors, whereas in markets, the rationality of actors is more mechanical (also see Amadae & de Mesquita, 1999, pp. 277, 290). With *The Theory of Political Coalitions*, positive political economy and rational choice were born within political science. By the early 1980s, the impact on the subfield of

3. See Amadae and de Mesquita (1999) for an account of how Riker (1962) created positive political economy as an approach within political science and how he built the Rochester School and helped build the Public Choice Society.

American politics was clear and significant. There were some important early forays into the question of the process of coalition building to form governments, but as Amadae and de Mesquita (1999) note, “Only relatively recently, however, has positivism begun to take hold in the study of comparative politics” (p. 287).[4]

STATE FORMATION, POLITICAL MOBILIZATION, AND DEVELOPMENT

Contemporary investigations of popular mobilization and protest also have a source in economics. Olson’s (1965) *The Logic of Collective Action* makes the free rider or collective action problem a part of the political science vocabulary. His book debunks the previously dominant interest group model in which shared interests automatically translate into collective action. Rather, he argues that although rational individuals may want a particular collective good, they will prefer to have others contribute to its production while they free ride; they will contribute or participate only if there are selective incentives that change their private cost-benefit calculations.

Olson relied (1965) on expected utility analysis as did Downs (1957), and they seemed to reach similar conclusions about the nonrationality of participation. What distinguished Olson from Downs was not only the subject matter but also the implied strategic interaction between those making the decision to free ride. In Downs’s argument, it does not matter what others do; it is rational to vote only if you are the only voter or certain you are the tiebreaker. For Olson, whether to free ride can depend on the social pressures to which you are subject and thus the nature of your interactions with others. The size principle, repeat interactions, and selective incentives all bring strategic interactions into collective action problems. Thus, it did not take long for scholars to translate free riding and its suboptimal outcomes into a prisoner’s-dilemma game (e.g., see Hardin, 1971, 1982; Schelling, 1978; Taylor, 1987). More important than offering an alternative formalization of the Olson model, the move into game theory made it possible to generalize collective action problems to a variety of political problems. The effect was to advance analyses of state formation, common resource problems, and of course, political mobilization. For example, Taylor (1988) elaborates the collective action problem as a means to understand revolutionary behavior, and Ostrom (1990) uses it to understand the variation in political solutions to natural resource issues.

4. Laver (1998) does an excellent job of reviewing and analyzing this literature—to which he was a significant contributor. Also see Carey (2000).

Another breakthrough came with the publication of North's (1981) *Structure and Change in Economic History*. North builds on Olson's (1965) free-rider problem and on Coase's (1987) notion of transaction costs to develop an institutional analysis that permits him to theorize about the role of the state in prohibiting certain alternatives and facilitating others. His later work raises some of the key questions confronting comparative political economists today: path dependence and historical determinacy on one hand and the role of informal institutions, ideology, and cultural influences on action on the other. As an economic historian, he also adds the emphasis on long-term secular change; he is less concerned with the immediate implications of a particular set of government policies than with economic development over time.

There were soon numerous important efforts to translate the collective action problem and rational decision-making models more generally into contexts of revolution, rebellion, and mobilization throughout the world and to the everyday choices of peasants and farmers in developing countries. Popkin's (1979) *The Rational Peasant* had a major impact on political science by applying rational choice to agricultural societies.[5] Drawing on his research in Vietnam, Popkin argued against the prevalent view of peasants existing in a Polanyian moral economy (Scott, 1976) and emphasized the importance of creating selective incentives through the efforts of organizers and political entrepreneurs. Bates's (1981) *States and Markets in Tropical Africa* further transformed comparative political economy. By demonstrating how rational choices—by peasants, workers, or politicians—can lead to irrational policies and suboptimal outcomes, he provided insight into questions of development that had long perplexed scholars. Levi's (1981, 1988) theory of predatory rule built on Olson, but particularly North, to explain variation in tax policies and the organization of tax collection across time and place. She derived her argument from the assumption that rulers maximize wealth to the state and combined transaction cost and structural analysis to elucidate organizational differences. Popkin, Bates, and Levi claimed that the same set of simplifying assumptions should be able to account for behavior in a wide range of historical periods, polities, and economies. The real action in these and similar models is in the constraints on actors's choices, the incentives they face, and the strategic interactions in which they must engage. Moreover, two kinds of constraints began to emerge as critical. The first is the constitutions or rules—the institutions—that delimit action and choice and that may also be a source of incentives. The second is the individual but

5. Ilchman and Uphoff (1971) were the real pioneers in using early rational choice, however.

aggregated responses of ordinary peasants, workers, and subjects, whose behavior changes the real costs of possible government actions.

The use of the rational actor model in settings that have long been the monopoly of area specialists, anthropologists, and historians raised a controversy, continuing into the current day, of the appropriateness of transposing assumptions developed to model the individualist actors of Western capitalism to the more collectively oriented people living in non-Western societies and precapitalist economies. In spite of the widespread skepticism toward rational choice among comparativists, there was soon a flood of applications of rational choice models to other countries, other periods, and other kinds of actors.

ASSESSING THE ECONOMIC TURN

Knowledge of economic theory permits political scientists to pose and evaluate a number of hypotheses, some of which have long been around in political science and some of which arise out of the application of economic theory to politics. Let me illustrate with two examples. The assertion that parties vary on a left-right spectrum in terms of their economic policies and that those policies make a difference does not require sophisticated economics. However, the application of economic tools has made possible some illuminating analyses and empirical tests. Machiavelli built his 15th-century consultancy to the Prince on the assumption that rulers are self-interested actors trying to maintain and enhance power, and few Weberians would find startling an argument based on that assumption. However, only in the past two decades have we witnessed a systematic investigation of the implications for developing countries and the generation of testable and often counterintuitive hypotheses.

Moreover, as economic theory was taken up by political scientists, it was transformed. The attention to institutions and norms, to the richness of context, to questions of conflict and power, and on occasion, to nonegoistic motivations differentiates it sharply from the straightforward application of economics to politics that characterized much of the early public choice school. This is, if anything, even truer of those who use rational choice in comparative politics than in American politics.

What follows is a brief assessment of two of the major bodies of work in contemporary comparative political economy. The first explicitly models the economy and examines macroeconomic policy. The second is self-consciously rationalist and applies modified microeconomic reasoning to a range of problems in comparative politics. Institutional analysis plays a role in both of these literatures and will be addressed to some extent (for fuller elaborations, see Carey, 2000; Scharpf, 2000). The discussion touches on, but does not elaborate, important contributions to the study of legislatures and elections, development, collective mobilization, and state formation.

ECONOMIC POLICY

> Economic policy is political because it affects the distribution of wealth and incomes in society. Therefore, understanding policy requires understanding the distribution of power among major social interests. Economic policy is also political because it reflects decisions made by elected politicians in an institutional context. (Alt & Crystal, 1983, p. 33)

One important strand of comparative political economy investigates the effects of government on macroeconomic policy, especially the political factors that influence or alter the choices of politicians. The emphasis continues to be on the electoral pressures that politicians face, their own ideologies, the legal and constitutional constraints within which they find themselves, and the structural constraints produced by the distribution of economic power within the society.

Those concerned with economic policy making have to get right the factors that influence the decisions of government actors and the consequences of their actions. This is not an easy task, but it is one that economic theory might actually assist. For example, Rogowski (1989) applies the Samuelson-Stolper model to illuminate the variation in dominant political coalitions as a consequence of the expansion or contraction of trade and the distribution of land, labor, and capital in a particular country. Frieden (1991) translates the economists' concept of asset specificity into an important determinant of political pressure by corporations, with significant consequences for the economic policies of developing countries. The rational partisan model, developed by Alesina and colleagues (Alesina, 1989; Alesina & Rosenthal, 1995), rebuilds the partisanship approach based on a better model of the economy. Although it finds confirmation of partisanship, it also finds that party effects on unemployment, inflation, and rates of growth are very short lived.

Innovative and more sophisticated uses of economic theory are only part of the story, however. Work by political scientists, often qualitative or statistical work, on labor unions, central banks, and other sources of political power and pressure, as well as more general research on the variations in the welfare state, all contribute to a more refined understanding of macroeconomic pol-

6. See Thelen (1999) for a useful review and contrast of this literature. Also see Scharpf (2000).

icy making. With the advent of both comparative historical institutionalism and the new economic institutionalism in the 1980s came an increased concern for the various agencies within government and the rules affecting elections, legislative decision making, and centralized bargaining.[6]

Among the ways to think about the relationship between political behavior and macroeconomics is the model of corporatism, initially articulated by Schmitter (1974) but later and significantly modified by Lange and Garrett (1985, 1987; Alvarez, Garrett, & Lange, 1991) and Scharpf (1991), among many others. The corporatist scholars aim to account for variation in the policy behavior between parties and between countries.[7] To varying extents, they build on Olson's (1982) argument that encompassing coalitions tend to reduce the inflationary pressure of labor militancy, but crucial to the analysis is the elaboration of the specifics of institutional arrangements that affect elections, wage bargaining, and monetary policy.

What has emerged from more than a decade of focus on the relationship between party ideology, country-specific institutions, and macroeconomic policy is the transformation of the models comparativists use to understand the political relationships at the center of macroeconomic policy. The result is a lively research program, blending rich empirical research with theoretical insight, as exemplified by the work of Soskice (1990), Garrett (1998), Boix (1998), and Iversen (1999). From the simplistic partisanship of Left versus Right and employment versus inflation has emerged a new set of arguments concerning partisanship that involves a wider variety and complexity of variables. A relatively minimal sacrifice of parsimony has produced a deeper understanding of the institutions, coalitions, and sources of change that underlie the politics of macroeconomic policy.

RATIONAL POLITICS: AN OXYMORON?

The literature on macroeconomic policies starts with collective actors such as parties, unions, and banks.[8] The literature on the collective and macro outcomes of individual choices obviously begins with individuals. Both literatures model strategic interactions, and both rely on theory drawn originally from microeconomics, although the second is generally more self-conscious in its use of microeconomic theory. However, the distinct starting place of their models means that the questions they raise and can answer are different. More precisely, one of the central concerns of the second approach is the

7. Not all scholars who think about features of corporatism use economic theory, of course. Nonetheless, scholars such as Swenson (1989) and Pontusson (1991) clearly influence the direction of the discussion and the development of the models.

8. Golden (1993) and Iversen (1999) are among the few exceptions.

suboptimality of decisions, both individual and aggregated. Why can the choices of rational people have such blatantly irrational outcomes? This question often leads to a related concern about institutions as a source of distortion and variation and about institutional design as a means to improve decision making. Thus, the agenda of the scholars of political decision making is both positivist and normative, and often, the same scholar deals with both aspects in his or her work.

Some of the rationalist comparative political economy concerns the strategic choices of voters, legislators, and elites in advanced capitalist democracies. In other words, the models developed initially in social choice theory and in American politics are transported into comparative politics—as are some of the ongoing debates about institutional stability versus chaos theorems and the appropriate kind of game theory to use in modeling the key interactions (cf. Laver, 1998; Schofield, 2000). The influence now goes in both directions, from comparative politics into American politics and vice versa. Indeed, theoretical progress in political economy sometimes requires explicitly comparative work. For example, Cox (1997) significantly advances the theoretical implications of the Duverger laws, and Bates (1997) of cartel models by investigating numerous countries.

One of the most explicit and influential explorations of the suboptimality of choices is Tsebelis's (1990) *Nested Games* (also see Scharpf, 1988), in which he uses game theory to model the effect of multiple arenas on one hand and of the effort to alter the rules of the game on the other. He then derives testable hypotheses and offers plausible support for them with some case study material. His book is a model of how to develop empirical theory. However, it lacks the in-depth case studies that characterize a second archetype of nonstatistical rationalist comparative political economy. Exemplars of those who combine formal theory with original archival or field research are economic historians such as Greif (1989, 1998) and political scientists doing extensive field work, such as Ostrom (1990) or Laitin (1998).

Although it has its origins in the legislative and party politics of advanced industrial democracies, the rationalist research program is clearly transportable to settings with very different political arrangements, as the work of Greif (1989, 1998), Ostrom (1990), and Laitin (1998) indicates. There are now a raft of studies that use rational actor models to understand reform, institutional constraints, and institutional change in Latin America, Africa, Asia, and the post-Socialist societies (e.g., see Geddes, 1994; Oi, 1989; Treismann, 1999). However, rationalist comparative political economists do not confine themselves to the behavior of elected officials, bureaucrats, and other state actors. There are also those who take seriously both the role of nonstate institutions, such as churches (e.g., Gill, 1998; Kalyvas, 1996) and unions (Golden, 1997), and the actors within civil society, such as ethnic and linguistic groups (Fearon & Laitin, 1996; Hechter, 1987; Laitin, 1992, 1998) or the citizens themselves (Levi, 1997). Some of the most intriguing new work is on the choices that individuals make about their behavior during rebellions, riots, and civil wars (Kalyvas, 2000; Parikh, 2000; Petersen, in press). Others, such as Rosenthal (1992) on postrevolutionary France, Firmin-Sellers (1996) on Africa, and Whiting (2000) on China, use political economic analysis to focus on the politics of the construction of different kinds of property rights in countries trying to transform their economies. In the process, they are able to help explain important outcomes and variations not easily understood before.

What ties all of these very different projects together is a simplifying assumption about the maximand of the key political actors, an explicit model of the decisions they face and their available choices, and an analysis that gives institutions an important explanatory role. However, the methodological individualism of this kind of work does not mean benightedness about the effect of different roles that actors might have. Whether an actor is a ruler, a legislator, or a citizen; whether an actor is an employer, a union leader, or a rank-and-file worker; or whether an actor is a revolutionary, counterrevolutionary, or neutral, the actor's preference ordering is derived from a realistic and defensible assumption about the preferences of persons in similar situations. There can, of course, be differences between scholars in the maximand they assign, even to the same kind of actors. For example, Geddes (1994) assumes that a country's leaders are maximizing career interests, whereas Levi (1988) assumes that they are maximizing revenue to the state. The evaluation of their arguments rests not on the assumptions themselves but on the hypotheses and testable implications derived from the initial assumption.

The diverse projects undertaken by comparative rationalists tend to share an additional attribute. Instead of trying to develop a general theory of state formation, revolutions, development, or some other major problem, they tend to pose questions that are more susceptible to observation and testing. They model a portion of the critical dynamics in a way that permits testing of parts of the idea. This is in keeping with advice originally proffered by Riker (1990) and more recently annunciated by Bates, Greif, Levi, Rosenthal, and Weingast (1998) and Geddes (in press). There are, of course, rationalists such as North who ask the big questions, but those influenced by North tend to concern themselves with understanding variations in the sources of norm change (e.g., Ensminger & Knight, 1997) or revenue production policies (e.g., Levi, 1988). Other rational comparativists focus on the effects of particular institutional arrangements for a specific range of outcomes (e.g., Golden, 1996).

Rationalists are often distinct from other comparative political economists in the questions and models they use. For example, Geddes's (1994) analysis of administrative reform in Latin America contributes to a large literature on state capacity and development. However, by narrowing the question to one in which she can identify specific actors, institutional constraints and incentives, and scenarios, she is able to isolate the mechanisms that make change possible—or not. She has moved the discussion of reform beyond the structural accounts that have dominated the development literature, but she also contributes to the possibility of cumulative scientific knowledge. If she has done her job—and I for one think she has—then she uses rational choice to do more than redescribe what others might see through another method.

THE VALUE OF THE ECONOMIC TURN

Formal models, game theoretics, and econometrics have now migrated from economics into political science. Moreover, rational choice and public choice have now been around long enough in the study of American politics to warrant serious discussion and debate about the usefulness and power of this approach (J. Friedman, 1995; Green & Shapiro, 1994), and some of the standards for the assessments of Americanist work are also being applied to comparative political economists. Critics within comparative politics are appropriately demanding that rationalists demonstrate what value is added by using complicated techniques or rational choice assumptions.

There has now been enough time, enough big books, and enough major scholars who identify themselves with the economic turn in comparative politics to once again raise the question of the value added by this approach.

METHODOLOGICAL VALUE

One important positive effect is the addition of new methodologies to the tool kit of comparative political economists. It is increasingly incumbent on today's comparative political economy students to learn not only statistics and econometrics but also game theory and formal modeling techniques. In some instances, these more quantitative tools are also combined with the more traditional methods of the comparative political economist: fieldwork, elite interviews, and archival research.

This is definitely a change, but is it a change for the better? At a recent American Political Science Association roundtable on methods in comparative politics, several graduate students spoke from the floor to express concern about the hegemony of rationalist and formalist techniques. They had conquered these techniques, they claimed, because they feared that they would not get jobs without these skills. Perhaps the conquest of formal theory is as essential to comparativists as statistical or fieldwork skills—perhaps not. However, the combination of developing a tool kit, becoming sufficiently proficient in the languages and history of particular places to analyze them, and understanding political science theory more generally does put a heavy burden on graduate students. In addition, it does seem that the proper balance between methods is yet to be achieved.

The continued emphasis on going beyond description and engaging in theory development is good, but ultimately, a theory is judged to be powerful only if it stands up to meaningful empirical tests. This requires not only technique but also the skills of actual data collection and analysis. Thus, it is not surprising that some of the most eminent figures in comparative political economy are skilled at both theory building and empirical research as well as the analytics of rational choice. Moreover, they offer a variety of models from which to choose. Bates and Laitin, for example, speak several languages and engage in time-consuming fieldwork in often dangerous locales. Golden and Geddes began as area specialists who have brought their detailed knowledge of particular locales to bear in their theory testing. Przeworski uses the most advanced statistical techniques available to analyze large-*N* databases. Cox, Levi, and Rogowski rely on archival and other historical sources.

Increasingly, as a consequence first of Przeworski and Teune (1970) and more recently of King, Keohane, and Verba (1994), the emphasis in comparative politics is on methodologies that contribute to building knowledge; the model is that of science. For rationalists, however, the problem is how to meet the criteria laid out in such books when researching a single case and using formal logic. One strategy is what a group of scholars label *analytic narratives* (Bates et al., 1998)—the attempt to use game theory and rationalist logic to discipline the narrative and to use the narrative to help clarify the identities, preferences, and strategic interactions of the key actors. By making explicit what many rationalist researchers have actually been doing for some time, it is now possible to begin an explicit evaluation of this methodological approach.

THEORETICAL VALUE

If the traditional model of politics is, "Who gets what, when, and how?" then rationalist comparative political economy definitely represents a major theoretical shift. There are still distributional questions, but distribution is no longer the primary focus. Rather, the model of politics is more of the form, "What makes who do what, when, and why—and how does each act in response?"

There are two bases by which to evaluate rationalist comparative political economy. First, does it offer more powerful explanations than those provided by competing paradigms? Second, does it offer insight into processes not previously or sufficiently explored?

A common and sometimes fair criticism of rationalist comparative political economy is that it simply provides a new technology for raising the same question and providing exactly the same answer that other careful comparativists would. However, it is more likely to be the case that rationalists are trying to account for different phenomena than other comparativists. Whereas political and economic development, state building and breakdown, and nation building writ large may be the focus of many who consider themselves comparative political economists, the rationalist comparativists are generally trying to explain only a particular set of reforms, collective conflict, or state policy. Contrast the research on democratization that tries to lay out the pathways to economic development and political democracy (e.g., Luebbert, 1991; Moore, 1966) with investigations of how specific features of federalism or particular property rights are linked to sustainable political and economic reform.[9]

Of course there are nonrationalists who also ask a narrower set of questions to get at issues of state building and state capacity. Sven Steinmo's (1993) and Karl's (1997) work come to mind, but both resist the search for universal mechanisms and even laws that undergird rational choice. Thus, the paradox in comparative political economy is revealed: Rationalists narrow the questions to those that can be answered and, in the process of understanding a particular phenomena, rely on generalized mechanisms. Comparative political economists from a more structuralist or historical institutionalist tradition generally—but not always—ask bigger questions and tend to make broad comparisons across cases; however, they conclude that the explanation lies more in the specifics than in the general. This makes it hard to compare the two programs, and it suggests that there will be relatively few pieces of research that have the same dependent variable but use different approaches.

Where the real theoretical value of rationalist comparative political economy lies is in its formulation of testable hypotheses that other paradigms might not produce. These are sometimes but not always nonintuitive, but the best of them are always nonobvious, at least until published. The reason that Bates, Laitin, and other rationalists have gained such stature in comparative

9. Bunce's (2000 [this issue]) article addresses the literature on democratization. Also see Kitschelt's (2000 [this issue]) article.

politics is that they raise new questions or provide new answers or do both at once.

Already, rationalist political economy has eliminated some models of reality, such as the interest group model of political mobilization. In comparative politics, it has introduced other models of reality about the nature of institutional equilibria within legislatures and polities and about the role of government in the economy. These claims do not yet have the force of the free-rider problem, but they have nonetheless redefined the theoretical debate.

WHERE DO WE GO FROM HERE?

Despite the emphasis on what distinguishes rationalist from other comparative political economists, there is increasing convergence. Moreover, given that comparative political economy is as much—or more—an approach to the study of politics than a specific substantive area of research, there is an immense overlap with the work of other comparativists, be they political economists or not. The boundaries are permeable, and there is much to learn by taking into account findings from the whole menu of perspectives. It is no longer satisfactory (if it ever was) for rationalists to dismiss the research of those who have chosen not to master economic theory and formal models as mere storytellers, adding only to our descriptive understanding. It is also not acceptable for other comparativists to dismiss rational choice as scientism and empty abstraction.

Yet there is only beginning to be border crossing among comparative political economists who rely on economic theory and those who do not. There are three major reasons. First, the language of game theory, formal models, and rational choice is foreign to many political scientists, who find the arguments impenetrable and inaccessible.[10] The extremely technical nature of some of the most important work in this genre is off-putting to many comparativists. Second, the assumptions, abstractions, and simplifications of reality are often anathema to those who relish the in-depth and down-and-dirty study of political behavior and institutions. These two objections could be overcome with increased familiarity, training, and tolerance of alternative approaches, as has been advocated by virtually all the presidents of the

10. Rogowski (1999) makes a related point about the failure of political scientists to incorporate the contributions of Gary Becker.

11. Peter Lange, Ronald Rogowski, Robert Bates, David Laitin, and David Collier have all contributed to the debate about the best way to accomplish this end through their presidential letters in the *Comparative Politics* newsletter.

comparative politics section of the American Political Science Association.[11] Debates continue about the appropriateness of different methods and models for different problems, but an increased willingness to communicate across the divide will (and has begun to) alleviate counterproductive segregation.

Rational choice scholars and formal modelers have not always helped in overcoming this divide, however, and they are thus responsible for the third reason for the relative lack of cross-fertilization. Too often, the debates are very internalist. Arguments over the advantages of cooperative versus non-cooperative game theory or which is the best of the admittedly simplifying assumptions about the motivations of government actors (cf. Laver, 1998; also see Schofield, 2000) are not likely to be of interest to those not already convinced of the value of the basic approach. Of course, the economic turn is still relatively new in comparative politics, dating from the late 1970s. Thus, comparative political economists of the rationalist and formal stripe have been trying to sort out among themselves where the approach has leverage and how to improve the model so that it actually helps explicate important puzzles and takes politics and governments seriously.

The good news is that borders are being crossed. Many rationalists studying economic policy have taken on board the arguments of Hall (1986, 1989) and Weir (1989) that ideas also matter. In particular, the decline in the acceptance of Keynesianism has not only influenced policy makers but has also redefined party positions. Recent work by Pierson (2000) and Thelen (1999) incorporates the concept of path dependence, first developed by economists, and encourage careful and rigorous application to political phenomena being studied by historical institutionalists. The attention to both structure and agency is apparent in recent research on economic and political reform (e.g., see Levi, 1997; Stokes, 1999). In addition, of course the collective action problem is omnipresent in virtually all forms of comparative political economy.

There is even hope that the learning process is now going in both directions between political science and economics. The major finding of a recent book of conversations between Nobel economists and political scientists reveals that there is, in fact, conversation (Alt, Levi, & Ostrom, 1999). The agenda created by Herbert Simon, the one Nobel laureate in economics who is actually a political scientist, is now the agenda of a number of eminent economists eager to explore the bounds and limits of rationality. At the same time, the significance of political institutions that distribute resources and may even affect cognition, beliefs, and preferences has become part of the vocabulary of economists interested in politics. With this frontier broached, it may actually be possible to have a comparative political economy that is as political as it is economic.

This discussion brings us back full circle to the issues raised at the beginning of the article: the positive and normative questions concerning the role of the state in political and economic development and the price in liberty and equality it might exact. By providing a sounder empirical base for understanding just what the state has done in the past and why its policies vary and with what effect, it is possible to turn to the normative questions with a better grasp of the costs, benefits, and trade-offs of the position one advocates. It is just such a theoretically informed empirical grounding to public discourse that Adam Smith attempted to provide when he founded modern comparative political economy. However, his version of laissez-faire is not the only possible or sensible stance, and there is now firm grounding for a range of possible positions. One can but hope that informed advocacy would improve public debate and, better yet, public policy. That would indeed be a significant contribution of comparative political economy.

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