

Political Science:

THE STATE OF THE DISCIPLINE

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CONTENTS

Ira Katznelson and Helen V. Milner

Preface and Acknowledgments xiii

Ira Katznelson and Helen V. Milner

American Political Science: The Discipline's State and the State of the Discipline 1

1 | The State in an Era of Globalization

Margaret Levi

The State of the Study of the State 33

Miles Kahler

The State of the State in World Politics 56

Atul Kohli

State, Society, and Development 84

Jeffrey Frieden and Lisa L. Martin

International Political Economy: Global and Domestic Interactions 118

James E. Alt

Comparative Political Economy: Credibility, Accountability, and Institutions 147

James D. Morrow

International Conflict: Assessing the Democratic Peace and Offense-Defense Theory 172

Stephen M. Walt

The Enduring Relevance of the Realist Tradition 197

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PREFACE AND ACKNOWLEDGMENTS

Ira Katznelson and Helen V. Milner

We began work on this successor to the two prior State of the Discipline volumes (Finifter 1983; Finifter 1993) in 1999 when we were selected by president-elect Robert Keohane to chair the program committee for the 96th annual meeting of the American Political Science Association in 2000. Soon after our designation, APSA inquired about our willingness to edit a volume in conjunction with our plans for the meeting. We had already selected "Political Science as Discipline? Reconsidering Power, Choice, and the State at Century's End" as our theme, so this seemed a natural fit.¹ We asked numerous colleagues for advice about the most important topics and potential authors for such a volume and remain grateful for the outpouring of useful responses. Our goal from the outset was to reflect the vibrant, often contested, diversity of political science while chronicling the past decade's scholarship and prompting thought about future directions. We asked the book's contributors to provide critical reflection on their particular fields and to indicate and advocate promising avenues of research for the future. The essays in this book are not just retrospective appraisals; they also strongly express views about where the discipline should be going. Many of the essays are quite spirited. Since each is

1. The meeting's program statement read as follows:

Power, choice, and the state have been central concepts in political science since its founding as a self-conscious discipline. By reconsidering these orienting themes, we seek to initiate discussion about both the distinguishing characteristics and internal coherence of political science as well as late-century developments which challenge how the discipline deploys its key organizing concepts.

From its founding, political science has transacted with history, its parent discipline, and the other social sciences. Once (perhaps again), the "state" defined its special focus. Arguably, "power" and especially "choice" have been more widely shared across disciplinary lines. Are there, or should there be, clear and coherent distinctions among these disciplines for the creation of knowledge? Further, do power, choice, and the state integrate political science across its own subfields and methodological variations? Will political science fragment, much as sociology has, or narrow its focus, much as economics has done?

Worldly developments at century's end challenge more than the character of the discipline. They command reexamination of the concepts of state, power, and choice. Sovereign states are being pressed on many fronts, from globalization to ethnic and regional movements. Their number, scale, capacities, and competitive advantages vis à vis other forms of political organization have become open issues. Power, too, is not static. The nature of power, even what we mean by the term, has been brought into question by nuclear weapons, the velocity of financial exchanges, and new communications technologies, among other transformations. In these circumstances, the very character of choice and rationality of agents become open issues.

stamped by the author's personal voice, there is no uniformity of style or approach.

The authors first sent us short proposals in early 2000. We then had long discussions with each to ensure broad and pluralistic coverage. Each paper was presented at a panel at the APSA 2000 meeting in Washington, D.C., where it received extensive comments by at least two discussants. We added our own written comments. The authors then were asked to revise their contributions for collegial discussion and review at two conferences, held with APSA support, in December 2000. There, they were subjected to vigorous critical discussion, led off by two commentators. These sessions produced a great deal of constructive criticism and critical feedback and facilitated the development of a common sense of the mission of the volume and a shared understanding of its overall design. Following these productive sessions, we wrote the authors once again early in 2001 to summarize those discussions and point toward changes we believed should inform the final texts. The final iterations of the essays were submitted in the late spring and early summer of 2001. Each paper thus has been reviewed by at least two and often four scholars in the same field, in addition to the interventions by the editors. As it turned out, our colleagues in this venture proved uncommonly conscientious, responding quickly and thoughtfully to these various editorial entreaties. We thank them in the warmest possible terms for this service to the discipline of political science. Their, and our, only compensation will be the impact this book has on the discipline, a result now in the hands of our readers.

We also are indebted to the many nonauthors who contributed to this venture. At the American Political Science Association, Catherine Rudder and Robert Hauck, especially, provided intellectual comradeship and administrative support. We salute their leadership. At W. W. Norton and Company, we have had the dedicated partnership of Roby Harrington, Avery Johnson, and Carey Schwaber, backed by a terrific production team. We are particularly grateful to the community of scholars who have read and commented on various versions of the chapters in this book. In addition to colleagues who wish to remain anonymous, these include David Baldwin, Brian Barry, Robert Bates, Henry Brady, Gregory Caldeira, John Carey, David Collier, Gary Cox, Robert Dahl, Geoffrey Garrett, James Fearon, Russell Hardin, Robert Jervis, James Johnson, Mary Katzenstein, Robert Keohane, Claire Jean Kim, Gary King, Peter Lange, Anthony Marx, David Mayhew, Nancy Rosenblum, Sven Steinmo, Paul Pierson, Ronald Rogowski, Kenneth Shepsle, Kathryn Sikkink, Robert Shapiro, Jack Snyder, Sidney Verba, and Margaret Weir.

There are, of course, many ways to organize a volume assessing the state of the discipline. We resisted the most prominent possibility that would have divided the book into the four conventional subfields (American, comparative, international relations, and political theory), plus perhaps a methods section, because we do not think that this division best

advances the discipline. The subfields increasingly inform and blend into each other. The essays confirm this proposition. Many share substantive themes such as the nature of the state, globalization and its impact, liberalism and democracy, the power of institutions, and concerns about identity, participation, and citizenship. Of course, scholars choose different approaches to these topics. But whether they privilege historical institutionalism, rational choice, or constructivism and postmodernism, they are engaged in what, broadly, is a common disciplinary discussion, as we discuss further in our introductory essay. We think that this targeted diversity is important. Given our prior views about the character of the discipline (Katznelson 1997; Milner 1998), the essays here lend support to our beliefs about the state of the discipline.

Our particular intellectual architecture focuses first on the state (“The State in an Era of Globalization”), followed by democracy (“Democracy, Justice, and its Institutions”), and then agency (“Citizenship, Identity, and Political Participation”), concluding with the means of inquiry (“Studying Politics”). In this order, the essays flow from considering the state broadly, irrespective of regime type, to the subset of liberal democratic regimes to political participation and behavior mainly within such a framework. This organization, we believe, reflects both the emphases and choices made within political science. The methodological papers that end the volume discuss alternative ways of addressing each of these political phenomena. We had commissioned a paper on courts and public law, but unexpectedly pressing demands made it impossible for its author to finish the essay before the book went into production.

Soon you will see that we think about the discipline in terms of an extended, although highly varied but coherent, intellectual conversation. May our debates long continue!

*American Political Science: The Discipline's State and the State of the Discipline*¹

Political Science as a debate might be political science at its best.
(Lindblom 1997, 262)

■ | Political Science as a Discipline

State of the discipline implies a discipline, yet even this much about political science cannot be taken for granted. As an organized profession, political science has existed for nearly a century. However, it has been a capacious, often cacophonous, undertaking. Political scientists possess sharply divergent views about their research and pedagogy. They disagree not only about what they should study and what constitutes a persuasive argument and evidence but also about how to understand the discipline's past. By the second page of his recent history, Gabriel Almond felt compelled to note that there exist many competing alternatives to his preferred progressive-eclectic approach (Almond 1996; also see Almond 1990). Further, political scientists often lack confidence in their enterprise. Charles Lindblom's iconoclastic assessment of the discipline records its meager contributions, amateur selection of research questions, obsession with method rather than substance, and absence both of social criticism and scientific authority (Lindblom 1990, 1997). Is there a discipline at all, we might wonder, before moving on to chronicle its condition?

Readers of this volume will not miss the remarkable heterogeneity and contested qualities of political science. We choose to put them to the side, not because we are disengaged or lack particular preferences about these

1. We have benefited from comments on our first draft by Robert Dahl and Sidney Verba at the American Political Science Association 2000 Annual Meeting, by Gregory Caldeira, Jeffry Frieden, David Laitin, Paul Pierson, and Barry Weingast, who offered formal critiques at two meetings to discuss the volume in Washington, DC, in December 2000, and by most colleagues in this book.

matters but because we believe that clarifying the kind of discipline political science has been can offer a fresh appreciation of what political scientists possess in common despite their differences and can refocus our understanding on continuities and their implications. The essays in this book are not just markers of variety. Together, they underscore how, despite its many internal epistemological and methodological divisions, political science in the United States continues to constitute a distinct scholarly undertaking. Coming to terms with the discipline, moreover, facilitates activities that we want to encourage. These include promoting collaboration by scholars in diverse subfields, clarifying various scientific and normative debates, and expanding the reach of political science by making the discipline's historical omissions more apparent and thus more remediable. Sharing with Almond a taste for scholarly diversity, we think that the eclecticism of the discipline can most effectively produce political knowledge by becoming self-conscious about the limited but compelling sets of questions it has addressed.

In his skeptical consideration, Lindblom observes that political science "is a name given not to a field of conventional scientific inquiry but to a continuing debate" about specific subjects and claims. These, he argues, do not get resolved; "on any given big issue of fact or value, debate in political science tends to be endless rather than declining (or terminating in a finding)" (Lindblom 1997, 260–61). We agree with Lindblom that the image of a long-running debate captures well the dynamics of our discipline as a continuing process of inquiry engaged with understanding fundamental political issues. But what has this debate been about? What have been its achievements, limitations, and possibilities? These are our central questions.

For about a century, political science as an organized professional enterprise—intertwined with, but distinct from, history and the other social sciences—has set boundaries, enabled collaboration and cumulative progress, punished outliers, induced a common state of mind, and defined positions characterized by frontiers between conformity and challenge. The diverse elements in political science have formed a single broad family with recognizable features and distinguishing characteristics: a pragmatist orientation to the modern state that makes the analysis of power and choice a constitutive feature; attention to the nature and stability of liberal political regimes and, increasingly, to democracy; and a dedication to study the state and liberal democracy in ways that are transparent and systematic. Political science thus has been a discipline with recognizable content and boundaries, even if there are disputes over their exact nature. The existence of a discipline is both limiting and productive; without it we are adrift, so knowing what it consists of is a pressing matter.²

2. As Goodin and Klingeman claim, "the same received disciplinary traditions and practices which so powerfully mould and constrain us are at one and the same time

For better or worse, political science within and across its subfields has operated as an interwoven branch of knowledge. But this unity is a particular kind. Characterized by a wide variety of questions, methods, and borrowings from other disciplines, it is distinct from the crisp methodological individualism that marks postwar neoclassical economics or analytical philosophy. It is also less permissive and open than, say, the fields of English literature or anthropology in recent decades. Marked by an emphasis on liberal politics, a tension-ridden engagement with democracy, and conceptual bearings that rest on a nonmetaphysical view of the modern state, political science is recognizable as a clear-cut scholarly endeavor whose development has been shaped by coherent and delimited intellectual debates. As a discipline, political science may lack a defining paradigm, but not a defining identity. It has used its principal concepts and methodological tool kit to work on a limited number of core problems.³

But it is not mainly the enabling and confining institutional terrain of political science on which we focus. American political science has specialized in developing particular kinds of social knowledge. The modifier *American* has to be taken seriously. Clearly, biblical, as well as classical Chinese, Indian, Greek, and Roman texts, not to speak of medieval, early-modern, and modern political thought in the West, present ample evidence that methodical reasoning about political authority hardly has been limited either to the United States or produced under the auspices of the American Political Science Association. Humankind's rich lineage of political thought, however, does not constitute political science in the modern sense (even if its texts and teachings have been incorporated, albeit in

powerfully enabling. The framework provided by the structure of a discipline's traditions both focuses research and facilitates collaboration, unintentional as well as intentional. A shared disciplinary framework makes it possible for mere journeymen to stand, productively, on the shoulders of giants. It also makes it possible for giants to build, productively, on the contributions of legions of more ordinarily gifted practitioners. . . . Discipline, academic or otherwise, is thus a classic instance of a useful self-binding mechanism. Subjecting oneself to the discipline of a discipline . . . is conducive to more and indisputably better work, both individually and collectively" (1996, 4–6).

3. By the time the *American Political Science Review* made its first appearance in 1906, the American Political Science Association, though just three years old, had secured a membership of some 400 scholars and practitioners. Ever since, virtually all practicing political scientists have been oriented by this publication and professional association, even when they disliked them. Further, the departmentalized organization of the major research universities and liberal arts colleges and a growing number of journals and research bodies, including the National Science Foundation, have encouraged the development of peer-understood criteria for making judgments about quality, authority, and prestige. As students of professions have observed, these institutions help produce a center of gravity defined by a combination of innovation and elite standard setting (Larson 1977; Friedson 1986; McDonald 1995). In political science, as in other knowledge-based professions, such arrangements never constitute a fully welcoming environment.

mixed fashion, in the discipline). The timing and character of the self-conscious attempt to define a specific, professional domain for the study of politics that took place in the United States at the turn of the last century had a formative impact on the discipline.

Bearing the marks of its origins, the discipline has been infused by prevailing assumptions about political values and about how to conceptualize and study the modern state. Most significant has been the discipline's attention to political liberalism (not in the partisan sense but in the sense of the doctrine fashioned in early modern Europe to guide relations between states and their citizens) joined, over time, by a heightened emphasis on democracy and its requisites. American political science was born in the Progressive Era as a nonpartisan, specialist profession geared to produce new knowledge in order to understand and help sustain liberal political regimes (Kloppenbergh 1986), even when their democratic character was curtailed. Its most innovative features concerned the development of a realistic and empirically oriented liberal theory of the state that takes power and choice seriously.⁴ The discipline's focus on liberalism has been most pronounced, if also most taken for granted, in its studies of the United States. But this cohering thrust to systematically understand the liberal state also has been important in political theory, which is often marked by a strong contractarian bent; in comparative political studies, where attention to the differences separating liberal, democratic regimes from other forms of rule is manifest; and although to a lesser extent, even in international relations, where concerns about how liberal, democratic regimes can thrive in an anarchic world have been salient.

The character of the founding of political science further helped shape its contours by pushing certain areas of inquiry into the margins. Demarcating itself from history, political science showed a greater concern for current events. To differentiate itself from sociology, it became relatively disinterested in the social bases of political action and inequality. In distinguishing itself from economics, it mainly left issues of political economy to other scholars, at least until recent decades.⁵ Further, born at the heyday of

4. Ever since, American political studies have been influenced by the liberal political tradition that Louis Hartz (1955) controversially signaled as the hallmark of American political culture and development. His claim that liberalism possesses an ascendant and undisputed place in the United States has not gone unchallenged, of course (Greenstone 1993; R. H. Smith 1997). Arguably, Hartz's claim has been more on the mark for the discipline of American political science (and especially studies of American politics) than for the history of the United States as a political regime.

5. In 1940, Benjamin Lippincott opened a consideration of "the bias of American political science" this way: "Political problems, as Aristotle observed, are at bottom psychological and moral. It is equally true, however, that the political activities of men, as Aristotle also pointed out, are affected in a fundamental way, often crucially, by economic factors. Yet curiously American political scientists, so far as

segregation, political science initially treated race as mainly beyond its ken.⁶ Later, each of these areas became contentious inside the discipline, as dissatisfied scholars sought to bring history, social analysis, political economy, and studies of race into its core. Because they have done so with at least a degree of success, many of these issues are now important elements in the discipline.⁷

Because of the broadly shared political tradition of political science, the scope of the discipline has been deep but restricted. Much of its effort has been to understand liberal institutionalism under democratic conditions, while comparing the U.S. experience to those of others and seeking knowledge about the liberal polity in a dangerous global environment. Thus, throughout its organized history, American political science has emphasized a liberal version of the state; that is, it has searched for the identification of rules based on civic and political rights to restrict potentially predatory state actors and to make the modern state permeable to the power and choices of members of society. Democracy, liberalism's first cousin, has been treated by comparison in a more ambivalent fashion, often with distrust by political scientists for the presence of the mass public and of groups thought to be ill informed and ill prepared for citizenship, and thus available for illiberal mobilization inside the liberal polity.⁸

"Liberalism," of course, is a broad term, with many meanings. From its origins in seventeenth-century England, political liberalism has entailed

their writings are concerned, have been all but oblivious of this elementary truth" (1940, 125). The work of Charles Beard (1913) and Robert Dahl and Charles Lindblom (1953) are notable exceptions.

6. In the first quarter-century of the *American Political Science Review*, there are three articles that might be considered exceptions to this omission (Rose 1906; Stephenson 1909; Roach 1925).

7. For an argument that they remain more to the periphery than at the heart of the discipline, see Walton, Miller, and McCormick 1995. With these emphases and limits, the discipline's virtues and defects have tended to be mirror images. Working on preferred subjects, political science has made impressive gains, especially when measured against the achievements of its first quarter-century. Its attainments based on an intensity of focus, however, coincide with the costs of a focused set of concerns.

8. This tension is a centerpiece of the scholarship of Robert Dahl, who, more than any other political scientist in the past half-century has placed democracy and its tension-ridden relationship with key features of the liberal political order at the center of his work. The development of "polyarchy" as a central concept by Dahl was geared to grapple with just this tension (see, among other works, Dahl 1956, 1961, 1982, 1989, 1997, and 1998).

The discipline's ambivalence about democracy was crystallized by the fall of Weimar and the flourishing of fascism. For a manifestation of this uncertainty and equivocation, see the discussion of "morbific politics" in the concluding chapter of Truman 1971 [1951].

a small number of core ideas: defense against arbitrary government; nonrevocable rights to free speech, association, assembly, and property for individual citizens who are the irreducible unit of the polity; toleration for diverse beliefs and practices; protections for minorities against the power of the majority; and institutions to enable actors in civil society to represent their interests inside the state. The questions of democracy have appeared as issues about membership and as concerns for the institutions that would represent the majority and constrain the executive. Who will get to participate in the liberal polity? What forms will political participation take? How will rights and duties vary by population category? What types of institutions are best for representing the preferences and interests of the public to the political leadership? What form and organization of institutions are necessary to effectively constrain political leaders so that they cannot stray too far from the interests of civil society? By establishing rules for governance, liberalism and democracy delineate a set of contested boundaries, including those that define where the state meets its citizens, where the individual meets the community, where members of the polity meet outsiders, where sovereignty meets property, and where a liberal state meets a global system of states, empires, and other forms of rule. Each of these charged borders has been subject to controversy; each has defined key subjects for analysis and debate by political scientists.

The discipline has a distinct understanding of the modern state. It has been distinguished both from the relative inattention to the state in the cognate disciplines of economics and sociology and especially from the more unitary, less concrete, and normative continental European approaches to the state, which many of the discipline's founders encountered firsthand in their studies in Germany. By contrast, American political science has pursued a quest to understand the state at a lower, more realistic, and behavioral level of abstraction.

"State" is an awkward concept for Americans, yet, much revised from its European lineage, it has fashioned one of the discipline's main conceptual tools. Its level of abstraction was brought down to make its institutions, relationships, networks, and actors identifiable for systematic studies. Political science strives "to assemble on common ground those persons whose main interests are connected to the scientific study of the organization and functions of the state," Frank Goodnow asserted in his inaugural presidential address to the American Political Science Association, and so it remains (cited in Reeves 1929, 2). For nearly a century, political scientists have been studying, debating, refining, extending, and deploying a state-focused agenda, though often without using the word itself.

From this vantage, we can see why the most familiar periodizations of the history of political science often overlook deep continuities that have made up political science during its first century as an organized discipline. We are familiar with the typical ways in which the history of political science is told. An early legal-formal constitutionally oriented

discipline was supplanted by a more scientific, behavioral impulse. In turn, behavioralism was replaced by a more heterogeneous postbehavioral period during which many approaches have emerged, some of which push toward unification of the discipline via a method-specific research program, including, most notably, one based on game theory and strategic interaction.

These histories are both too simple and too complex. Their simplicity results from a tendency to overstate the internal consistency of a given period within and across subfields. Thus, for example, even after the behavioral revolution had transformed American politics, in 1968 the founders of *Comparative Politics* lamented the relative weakness of this scientific impulse in their domain. On the other hand, these historical narratives are too complex because their periodization tends to miss the manner in which American political science has been continuous across epochs, not only with regard to its attention to a liberal political tradition and engagement with democracy but with regard to its realistic approach to the modern state, combining governance with power and choice. Underneath its flux, political science has been uncommonly continuous in its central concepts and substantive themes.

Standard disciplinary histories tend to identify the early period with studies of the state, the middle epoch with studies of power, and the latest era with studies of choice. Though each key concept has known moments when it has been featured more prominently than the others, the discipline constantly has deployed all three at the same time in order to achieve realistic and behavioral understandings of the modern state. Political science has had to come to terms with a capable U.S. national state despite a certain liberal aversion to state capacity. Recurrently, practitioners have done so by borrowing from Europe-centered state theory, which they have modified by lowering the level of abstraction, turning the big questions into mid-level researchable ones. By exploring the linkage between liberalism and democracy, moreover, political science has had to repeatedly confront issues of power. And by focusing on liberal democratic institutions, it has spotlighted the making of political choices as well as structural constraints on choosing. The repertoire of political science thus has been fixed and focused on debates about justice and membership, legitimacy and identity, the capacities of citizens and the qualities of choice, the meaning of power and functioning of state institutions, the requisites for liberal democratic states and their stability, and the relations of liberal democratic states to the international order. A century on, we continue to be challenged by the questions that most vexed our founders.

■ | Beginnings

The genesis of American political science combined a number of key elements. Its founding was an aspect of a wider trend in the history of social knowledge. At least since early modern Europe, scholars have attempted to produce knowledge, as opposed to mere information, by means of systematic thought and investigation organized in cooperative terms.⁹ Later, from the 1860s to the second decade of the twentieth century, there were major advances in the West to this structured pursuit of knowledge. In this period, new forums, organized in disciplines and based on claims to rationality and science, were fashioned. The knowledge community divided into distinct specialties, eventually segmenting into the modern disciplines that we now know (Ringer 1969, 1992). At the time, a second split was under way between academic practitioners of the social sciences, located mainly in institutions of higher education, and scholars more directly concerned with the impact of knowledge on public affairs, with homes mainly in governmental bureaus or not-for-profit institutions.

As part of this reorganization of social knowledge, the American discipline of political science was founded in a double act of boundary formation. It demarcated its zone of inquiry both from those of cognate disciplines, especially history, economics, anthropology, and sociology, and from more instrumental, short-term policy studies. Though these borders were permeable to other scholarly disciplines and to policy concerns, political science came to occupy intellectual territory its members understood to be its own. The discipline's singularity and significance were recognized early in its existence.

Substantively, this arena for political inquiry shared an understanding of the modern state as a complex normative and institutional setting with other national efforts, especially German, that also were committed to study modern politics systematically. But there was a key difference. In the United States, the impulse to study the state was associated to some extent with the desire to control and contain the state by civil society. Late-nineteenth- and early-twentieth-century U.S. studies of the state went hand in hand with a concern to tame state power by liberal values and practices, including consent, toleration, representation, and individual rights. Woodrow Wilson's classic 1887 essay on public administration provides a case in point. By the late-nineteenth century, he stressed, the United States had begun to develop a robust national state. Its effectiveness re-

9. In this enlightened spirit, academies for the study of politics were founded, for example, in Paris in 1712 and in Strasbourg in 1757. In tandem with new print technologies, the creation of centralized libraries, the growth of large cities, and improvements to communication, not just the creation but the dissemination of knowledge became possible on the basis of a broadly shared standard of rationality and transparent method (Burke 2000).

quired the application of up-to-date tools of bureaucratic governance, the most-advanced examples of which could be found on Europe's continent. This importation, he cautioned, would both be incomplete and dangerous unless it were accompanied by the elaboration of ideas and institutions governing the exchanges between the state and its citizens guided by the protections promised by the liberal political tradition.

A year earlier, the statement introducing the *Political Science Quarterly*, the country's first modern political science journal, had defined "the domain of political science" similarly in terms of "the dominant position . . . the science of the state is assuming" at a moment when its scope was expanding significantly. "The conception of the state as a mere protective association against external force and internal disorder is antiquated," Munroe Smith wrote on behalf of the journal's Columbia University editors. "The state is everywhere exercising other functions than the protection of person and property and the enforcement of contract. Whether the increasing importance of the state be deplored or applauded," he concluded, "the fact remains that it is rapidly becoming, if it is not already, the central factor of social evolution" (1886, 8). Writing a half-century later, Frederick Watkins noted that "Among contemporary social scientists it is a virtually unquestioned assumption that the state forms the basic concept of political science" (1934, 1).

Wilson observed that the systematic study of the state had not yet taken root in the United States but that it had in Europe, especially in the "strong" states of France and Prussia. Yet these models, he argued, should not be imported into the United States without major modification. "We should not like to have Prussia's history for the sake of having Prussia's administrative skill; and Prussia's particular system of administration would quite suffocate us. It is better," he asserted, "to be untrained and free than to be servile and systematic." In the United States, the modern state had to balance efficient administration with popular sovereignty under a liberal constitution. He thus called for a particularly American science of politics and administration, one that filtered the knowledge and practices of other regimes "through our constitutions, only to put it over a slow fire of criticism and distil away its foreign gases" (1887, 207-19).

Characterized by a focus on formal institutions, public administration, and law, the core of the country's new political science was infused with an emphasis on the elements of political liberalism articulated by Wilson. It focused primarily on the interactions between the national state and civil society, which occurred through processes such as those governing interest representation, public opinion, and elections. The discipline also rested strongly on the premise of the modern state, which implied a global order divided into sovereign states, the separation of sovereignty from property, the demarcation of the state from civil society, a split between the ruler and the institutions of the state, and a bundle of normative justifications for

public authority. On this basis, American political science began as a quest to understand and secure liberal regimes against competitors, such as monarchies and illiberal empires.

The relationship between liberalism and democracy has been a charged feature of the discipline from its Progressive Era founding. American liberalism in this period was being tested by the process of democratization. The country's massive demographic changes, occasioned by the entry of new immigrants, the organization of labor, the assertiveness of urban political machines, and the demands of populist social movements, brought new participants into American politics. As the effective size of the polity and expectations for political participation grew, the problem of how to manage liberalism and democracy together came to the fore. These pressures for participation and patterns of mobilization produced important democratizing institutional changes, culminating in votes for women and the direct election of senators that were supported by many members of the young discipline inclined toward social reform. But others were concerned about the potentially disruptive social forces empowered by expanding democracy. About liberalism, the founders of political science were rarely ambivalent. About democracy, they were more so.¹⁰

As a science rooted in political liberalism and mindful of the demands of democracy, members of the discipline worked from its founding on a small number of recurrent issues. These included an understanding of the features that distinguish liberal democratic regimes from other forms of public authority, whether dictatorial, oligarchic or, later, totalitarian; the manner in which a state can function in a perilous international environment often hostile to liberalism and democracy; and, most prominent of all, the rules governing relations between the state and its citizens in civil society. Thus, early on, the themes of public opinion, voting, interest representation, and legislative behavior—the central institutions of a liberal democratic political order—were placed at the forefront of domestic political studies. But it took some time for this orientation to mesh with the period's scientific aspirations that political science shared with its sister disciplines and to advance the ongoing shift from theology and metaphysics to the social sciences as sources of guidance.

In 1925, the historian Harry Elmer Barnes edited a volume of assessments of the history and prospects of the social sciences. Barnes exhorted that “we must bring [them] up to the same level of development and objectivity which have at present been attained by the natural and applied

10. As a consequence, as Terence Ball (1995) has observed, American political scientists have tended to oscillate between a vision of their role, on the one side, as educating the citizenry to overcome their lack of information in order to become thoughtful political participants and, on the other side, as servants of social stability and order in the face of skepticism about the hazards of mass political participation.

sciences" (1925: xv–xvi). The book's essay on political science by Walter James Shepard of Ohio State could not have given Barnes much heart. Shepard wrote to underscore intellectual progress, noting that not only had political science separated successfully from other disciplines, it now also was internally differentiated into distinct subfields. He identified the distinctiveness of political science as a concern to develop both a science and a philosophy of the state. What was different about modern political science, he averred, was that the theoretical aspects of this quest to understand the state had become a specialized branch of the discipline. Now, speculative and deductive political theory was complemented by studies of the state that increasingly were grounded in data, history, and comparison. Treating the scientific method as entailing the systematic accumulation of fact, the elaboration of data into causal sequences, and generalization from these sequences, Shepard concluded that the discipline was making "distinct progress toward a really scientific character" (1925, 427).

Read with the testimony provided by his text, however, this seems more an act of wishful thinking than a warranted conclusion. The discipline's rules of evidence and procedures for inference still were primitive. The discipline remained suspended between its nascent rejection of history (the founding of the American Political Science Association formally represented a breakaway from the American Historical Association), its ambivalence about normative theory, and its still early effort to discover a scientific character. By and large, the work accomplished in the first-quarter of the twentieth century was more descriptive; it tended to involve efforts at the semisystematic factual debunking of abstract philosophical dogma in favor of concrete, empirical descriptions of how politics actually works.

Judged retrospectively, the key element of the early years of political science was not only its nascent devotion to the scientific project but also its pragmatic view of the modern state. The discipline focused on constructing realistic portraits of how, in fact, it was linked to its own citizenry in its quest to secure liberal polities. With good reason, Almond identifies the research program at Chicago between 1920 and 1940, led by such considerable figures as Charles Merriam, Harold Gosnell, Harold Lasswell, Leonard White, and Quincy Wright, as the place where first, major advances were made in developing an empirical social science. This observation is correct; yet it misses some of the earlier pragmatic underpinnings that inspired the Chicago School.

Arthur Bentley's *The Process of Government* (1908), not cited at all in Shepard's or Almond's review essays, is exemplary of a pragmatist effort to establish the study of liberal democracy on a realistic footing; it also was a precursor to more systematic scientific ventures.¹¹ The key feature of this

11. Bentley's early work was methodological, stressing a methodological individualism he soon was to abjure (1895).

often opaque text was a reconceptualization of the state as a process of interaction between the government and its active citizens. Influenced by John Dewey, Bentley shifted our understanding of political conflict from abstract competing theories about the state, which were dominant in late-nineteenth- and twentieth-century works of political science, to clashes between actual groups with specific social interests.¹² Though in later work, Bentley documented vast class inequalities and their impact on political participation, here both the social structure and the economy were ignored in favor of group interactions within a system of transactions (Bentley 1969).¹³ This “process of government,” the historian of the social sciences Dorothy Ross observed, “was a liberal process” (1991, 334). By placing interest representation within a systematic framework, this practical and scientific approach to the state, focusing on institutions, power, and choice, was created to facilitate a more realistic understanding of the political order.

A significant portion of political science scholarship in the United States, ranging from neo-Marxist and neo-Weberian macroanalysis to institutional scholarship to studies of group formation and political participation, continues to approach the state by focusing on the functioning of liberal democracies, especially the one at home.¹⁴ Ever since Barrington Moore published his major work (1966) on alternative regime pathways, macrohistorically oriented scholars have sought to figure out the causes of these alternative trajectories (e.g., Downing 1992; Ertman 1997; Luebbert 1991). The subjects considered in more microlevel scholarship—including public opinion, voting, political parties, and legislative behavior—likewise are concerned with requisites of liberal political orders. They too are extensions of the manner in which the state was operationalized in the early years of the discipline.

Subsequent work has had deep affinities with the themes and orientation of the discipline’s founding period. Even though political scientists often stopped well short of methodical inquiry in the first phase of the discipline’s history, it is a mistake to divorce as prescientific this moment from later efforts to create systematic political knowledge. Indeed, the empirical scholarship promoted in Merriam’s review of “The Present State of

12. Bentley attended Dewey’s University of Chicago lectures and later they were frequent correspondents (Ratner and Altman 1964).

13. To our knowledge, he never wrote about race or the exclusionary practices of Jim Crow. This silence about race and racism was quite typical even among progressive liberals.

14. There are important exceptions, of course. An older vein of work focusing on sovereignty and jurisprudence (e.g., Willoughby 1896; Merriam 1900) and more recent research at the intersection of historical sociology dealing with how modern sovereign states supplanted other forms of political rule (e.g., Tilly 1990; Spruyt 1994) consider states without placing their regime type at the center of analysis.

the Study of Politics” (1921, 21–22) and his advocacy of “a new world made over by modern science,” in *New Aspects of Politics* (1925, 173–85) were intimately related to more traditional research on sovereignty and the state that had characterized his doctoral thesis on political theory since Rousseau (1900). In both, he sought to understand the requirements for a liberal political order under conditions of uncertainty, including democratic uncertainty and the insecurity generated by international relations. By turning the study of the state in an empirical direction, pre–World War II political scientists bid what they hoped would be a decisive farewell to the rarefied style of late-nineteenth-century German and French studies in favor of a focus on the two-way linkages connecting the state to its citizens.

■ | The Discipline’s State

Despite the discipline’s methodological plurality, virtually all its practitioners can recognize their participation in this particular kind of probe. Since World War II, political scientists have been concerned to understand the state as a bundle of norms, a site of power, and an ensemble of institutions. In the face of various illiberal alternatives and international dangers, they have tried to grasp the sources of liberal democratic stability and instability. Moreover, as Levi and Kahler point out in their essays that follow, this task has become ever more urgent with the globalization of politics. Within this context, political scientists also have sought to better comprehend the dynamics of political citizenship, participation, choice, and their consequences. They have done so in partially bounded subfields using a wide variety of tools. Notwithstanding, the agenda has continued to be well defined.

States are composed of bundles of norms, not just institutions or patterns of behavior. Hence it is hardly surprising that the character of liberal politics and the relationship between liberalism and democracy have long been staple questions of political theory. How such states can and should provide justice thus has been a core normative question. Justice is important in all political systems but especially in the liberal democratic state. Justice here concerns how to reconcile the individual citizen with the state and how to find guidelines for the distribution of key assets. Principles of justice mediate between the rights possessed by individuals and those held by the state. As Waldron reminds us in his contribution, norms of justice help define the public and private spheres, and they bind the state’s ability to use or threaten to use force. Such norms also adjudicate value conflicts among individuals in civil society. In effect, they provide decision rules for how individuals should be treated by others and by the state. They also address issues of membership, cultural and group diversity, and distributions

not only of welfare but of cultural assets (e.g., Walzer 1983; Kymlicka 1989; for a skeptical reading, see Barry 2001). Rawls (1971) and Barry (1995), most notably, have elaborated important and controversial decision rules for justice.

Others, of course, have been suspicious of such efforts, viewing them as insufficiently attentive to often incommensurable differences in culture and identity. As Coles observes in this volume, they have worried about the manner in which such principles can perform as hegemonic covers for hierarchy, discipline, and social control (Foucault 1971, 1977). Such thought falls, at times, within the central ambit of liberal theory, as in the case of Isaiah Berlin (1990), who made incommensurability and value pluralism the bedrock of his vision of liberty. Others find footing more in republican and communitarian traditions (Sandel 1982), strong democratic theory (Barber 1988; Barber 1998, I. Shapiro 1996), or feminist thought (e.g., Pateman, 1988; I. Young 1990). As Benhabib's and Gutmann's contributions here underscore, these bodies of work suggest that a key problem for liberal democracy is how to deal with fundamental conflicts of culture and value without resort to nondemocratic procedures and complacency about issues of membership and inclusion.

Two mechanisms that some theorists have explored to move beyond singular, universalistic guidelines are discursive processes and the contraction of claims by the public sphere to a defensible, consensual minimum. The first is associated most vibrantly with Habermas's wager on communicative ethics (Habermas 1971, 1990) and elaborated by theorists of civil society who stress the power of intersubjectivity (Cohen and Arato 1992). Since reason behind a Rawlsian veil of ignorance can neither produce widely agreed deductive principles for deciding among individual and group claims nor unmask the power they may cloak, they argue, conditions must be provided for unfettered communication to allow people to mediate among themselves through discussion and argumentation and to understand the hegemonic discourses in which they take part. Such full and free discussion is necessary if liberal democracy is to be more than a cover for the reproduction of inequality. These approaches also are controversial because they raise concerns about freedom of speech and the limits on free speech in liberal societies (Holmes 1993). The second approach, associated most importantly with recent work by John Rawls (1993) to advance what he calls political liberalism, retracts the scope of the claims made by the public sphere to an arena where an overlapping consensus exists. At issue is whether liberalism in fact can accommodate deep differences, while conforming to its own most cherished principles.

In part, this is a question of power, one of the discipline's main organizing themes. Power, of course, has been much debated (e.g., Dahl 1957, 1961; Bachrach and Baratz 1962; Lukes 1974; Polsby 1980; Gaventa 1980; Baldwin 1989). Simplifying considerably, two conceptions have been paramount. The first and dominant position, grounded in the work of Weber

(1946) who treated power as the ability of one actor to get another to do something the latter would not otherwise do, with some persons' thus causing the action of others to conform to their preferences, was announced as a core feature of political science by Watkins (1934) and famously elaborated by his student Robert Dahl (1957). A second, more critical perspective, owing more to Marx than Weber, reminds us that liberal politics can be a cover for pervasive inequality and privilege. A focus on overt conflict and behavioral causality can neglect power as agenda setting and as shaping worldviews and preferences, even language itself (e.g., Schattschneider 1965; Bachrach and Baratz 1962; Lukes 1974; Gramsci 1973; Foucault 1971, 1977). Because such nonovert forms of influence often do not have behavioral correlates, they pose dilemmas for empirical research.

Two consequences follow from this second conception of power. If power is invoked through language and ideas, then a thorough understanding of the connotations and limitations built into the discourse of the time is important for understanding how power is being exerted (Connolly 1969). Interpretations of language and the meanings of action thus are fundamental to power analysis. For this reason, constructivists and other proponents of this view believe that normal political science often is superficial. Being embedded within liberal values, many political scientists, they claim, rarely question the assumptions or language used by the actors they study. Instead, such critics argue, scholars should realize that it is less important to understand what voters think or prefer than how dominant structures and agents have shaped why they do so.¹⁵ Scholarship in this vein is a powerful source of critique.

Gramscian ideas of hegemony often are deployed by critical theorists and constructivists (e.g., Honig 1993; Wendt 1999; R. Cox 1983). They view elites who rule, whether capitalists, men, or members of other dominant groups, as a unitary hegemonic bloc which devises and gains from enduring structures that dominate the lives of others, even shaping their very desires. Their conception of power is both subtle and blunt—subtle in that the very structures of everyday life manipulate often unconsciously, blunt in that power tends to run mainly in one direction (for a counterflow, see Scott 1976, 1990). Since, according to them, power under conditions of hegemony cannot be grasped by direct methods of observation, special methods, including deconstruction, are deployed to discern the nature of power. Such methods often, however, raise concerns about the problems of counterfactuals and the possibility of falsification.

By contrast, the more pervasive liberal conception is less bluntly encompassing and in some respects more subtle. Here, individuals are central; structures of power ultimately are traced back to individual actions. So, too, are the formal institutions and less formal rules within which they

15. Echoing these themes, Lindblom finds that both scholars and citizens often are impaired (1990, 1997, 267–68).

connect to each other and come to decisions. Individuals, in this approach, form and know their own desires and beliefs; false consciousness is an elusive mystification. Language can be important from this standpoint, as studies of rhetoric (e.g., Riker 1986), propaganda, and the framing of issues make clear. But as it usually is not seen as a structure of power in itself, language, especially language used by analysts themselves, can be utilized as a neutral medium of communication. This conception of power also is more subtle in its acknowledgment of mutual influence. In an environment characterized by complex social and political relations, unidirectional power relations are unlikely. Models of strategic interaction stress this interaction. When individuals cannot achieve their goals without the help or at least acquiescence of others, they are mutually bound up, even if unequally. To change the behavior of others one must alter one's own, thus allowing the other to have influence, a pattern at work in the issuing of threats and promises (Schelling 1960; Jervis 1970; Baldwin 1989). This recognition has led to counterintuitive conclusions about how to exercise power, such as those informing nuclear deterrence theory where leaving oneself vulnerable to destruction can be the best way to ensure that one stays secure.

Both approaches to power are closely linked to studies of choice that confront many of the same issues. The theme of individual choice has reappeared throughout the history of the discipline, a subject expanded on for different subfields in this volume by Calvert, Weingast, and Powell. Political science finds itself sitting uneasily between economics, where choice under constraint is paramount, and sociology and anthropology, where constraint by limiting structures or culture is fundamental. The liberal democratic vision of the state intersects this tension. For individuals to matter, there must be some capacity for choice. Voting and elections are meaningful only if some options are available.

With regard to choice, political science has been characterized both by disagreement and a division of labor. Some colleagues emphasize the structural restrictions under which any choice occurs, a theme that developed for different parts of the discipline by Thelen, Pierson and Skocpol, and Orren and Skowronek in their essays that follow. "Instead of reducing behavior to individual decision-making," Charles Tilly counsels, "social scientists urgently need to study the relational constraints within which all individual action takes place" (1998a, 34). For both historical institutionalists and constructivists, these circumstances are paramount and derive from history. Mechanisms by which the past imposes itself on the present, including path dependence (Pierson 2000a), and the weight of culture and language shape and narrow choices open for individuals. Sharing much with sociologists, historians, and anthropologists, these political scientists emphasize the constraints on behavior that institutions, among other structures, impose (e.g., Katzenstein 1996a; Hall 1986; Katznelson 1981).

Another part of the discipline places these confining structures in the

background and emphasizes acts of choice. This orientation treats institutions as part of the choice set of individuals; institutions can and should be endogenized as they are not separate from individuals and their behavior. Choosing institutions goes hand in hand with making choices under particular institutional rules. Constrained choice is the only type of choice ever available, as economists are well aware. So, as Risse points out in his contribution, there is a bond between the two schools, even if it is not often recognized, just as there are potentially unrealized fruitful possibilities in encounters between the two main views of power.

The debate over choice also involves methodology. Rational-choice theory is a contested method in the discipline. It provides a core for economics, even though many economists feel “a chafing dissatisfaction with the standard neoclassical paradigm of economic analysis” (Alt, Levi, and Ostrom 1999, xv). But agreement on methods does not integrate political science as a discipline. Rather, the introduction of new methods often has been not only a source of innovation but also a cause of anxiety throughout the history of the discipline, as the essays by Cameron and Morton, and Green and Gerber in the concluding section of this book suggest. This, in fact, is one key reason why it makes little sense to periodize the history of political science by dominant scientific techniques. Indeed, as Laitin argues in his paper, in recent years there has been a growing effort to blend research methods and use their respective strengths to triangulate on the evidence.

For some colleagues, power or choice, rather than state, liberalism, or democracy, are the discipline’s master concepts. They see political science as the study of “the organization of power” (Holden 2000, 3; earlier see Lasswell and Kaplan 1950) or as a set of strategic interactions producing equilibria under determinate historical conditions (e.g., Bates, Greif, Levi, Rosenthal, and Weingast 1998). But important as power and choice have been as key concepts in political analysis, they define only two of the entry points to the discipline’s quest to understand politics in the modern state. The discipline’s nonmetaphysical attention to the state has primarily manifested itself not with power or choice alone but with power and choice in tandem with a concern for the functioning of political institutions. The state is not studied as such but from different vantages to discern the operation of its distinct parts.

The liberal democratic state has been a recurring source of deep anxiety, a theme that Shapiro takes up in his essay on democratic theory in this volume. The fall of the Weimar Republic and, more broadly, the collapse of many other constitutional democracies with the rise of fascism and bolshevism in the interwar period alerted the discipline to the terrible consequences of unstable democracies. Later, Arrow’s Impossibility Theorem (1963 [1951]), a key instance of incisive analytical work on the core problems of liberal regimes, set forth the theoretical challenge in stark terms. Instability is an immanent feature of liberal democracy. Under broad con-

ditions, majority rule leads to the cycling of coalitions and policy; only nondemocratic practices can alleviate this deep tendency, convoking a trade-off between stability and democracy.

Determining sources of instability and ungovernability in liberal democratic institutions has been a major area of research for decades. Where some see individual voters as the problem for stable democracy, others fault the mechanisms that aggregate their preferences (e.g., Eckstein 1966). A state is a composite of institutions that may not fit well together since each was designed for a distinct task or problem. For liberal democratic states, this web is even more complex since it also requires institutions to ensure accountability and enforce limits on actors. It must provide security and justice as well as representation and restraint. The basic tasks of the state as Leviathan can collide with the elementary demands of a liberal democratic polity. The historical failure of countries to follow the relatively peaceful path to democracy charted by England defines a puzzle that still animates the discipline (e.g., Bendix 1977; Almond, Flanagan, and Mundt 1973). Interestingly, recent work by Przeworski and Limongi (1993) and Barro (1997) broadly confirms, albeit with variation in emphasis, the early studies of Moore (1966) and Lipset (1959, 1994), among others, finding that democracy is premised on certain socioeconomic requisites. These claims, too, link the state to its civil society in complex ways, as Kohli elaborates in his essay here.

The functioning of state institutions has defined a major vein of research throughout the history of the discipline, as a number of the essays in this book make clear. Institutions now are understood to provide one solution to Arrow's problem. They induce policy stability but do not overcome his concern for the loss of democracy (e.g., Shepsle 1979; Austen-Smith and Banks 1999). Recent literature on institutions shows exactly how they possess nondemocratic mechanisms that can privilege certain actors and move outcomes away from the preferences of median voters (e.g., Romer and Rosenthal 1978, 1979; Baron and Ferejohn 1989a, b; Ferejohn and Shipan 1990). This new institutionalist literature reminds us that political science always has been concerned with institutions. The study of electoral rules, for example, has been a long-standing feature of the discipline. From Duverger (1959) to Rogowski (1987b) and Cox (1997), political scientists have been theorizing about the impact produced by different ways of translating votes into office holding. Greater proportionality has a variety of consequences for democracy: more parties, more fine tuned representation of interests, more coalition governments, less policy movement, and perhaps more instability. Against these must be weighed the costs and benefits of more majoritarian systems (winner take all) that allow short-term stability within governments but longer-term instability as governments and their policies change. The stable democracies of Britain and the United States for years were our only models, and many scholars accepted their lessons. But more recent research into other stable systems has shown that this in-

struction is too simple. Some forms of proportionality combined with other institutions, such as corporatism, also have been consistent with representative and stable liberal democracies (Lijphart 1968, 1977; Linz and Stepan 1978; Katzenstein 1985; Rogowski 1987b).

Party systems as institutions of liberal democracy essential for connecting voters and politicians have been another area of persistent attention, one that Fiorina addresses in his contribution here. Related to the debate over electoral rules, disputation over the functioning of two-party and multiparty systems, not to mention attention to one-party systems, is long standing, as recent retrospectives on the American Political Science Association's 1950 report "Toward a More Responsible Two-Party System" and V. O. Key's landmark *Southern Politics in State and Nation* (1949) make evident. The happy conclusion initially drawn from the U.S. and British cases was in favor of two-party democracy. This finding was muddied by Downs's prediction (1957) that such two-party stability can be achieved only with a huge cost in terms of choice. Convergence to the median position by both parties reduces substantive choice even as it underscores liberal democracy's central principle of rule by the majority. Since Downs's seminal work, we have learned much more about party systems and their implications for the success of liberal democracy (e.g., Sartori 1976; Lijphart 1977; Powell 1982; Cox 1987; Aldrich 1995).

The executive branch of government also has been increasingly attended as it has absorbed responsibilities for more, and more complex, policymaking and policy implementation. Presidential and parliamentary (as well as mixed) systems define and determine the executive differently. In many ways the executive is a simpler branch in presidential systems; winning a national election gives the leader and his party control over the presidential system. For parliamentary systems, where the executive comes from the legislature, forming a government is a central issue. When the U.S. and British examples dominated discussion, this issue did not seem very pressing. But over time, the revival of liberal constitutional regimes, after the defeat of fascism and Nazism in Europe and, later, with the collapse of communism, raised this question anew. Early work predicting minimum winning coalitions and minimally winning connected coalitions (e.g., Riker 1962; Axelrod 1970) has grown increasingly sophisticated and empirical as scholars struggle with the failure of these predictions (Strom 1990). The game of government formation has been linked backward to parties' electoral strategies and forward to their policy preferences, improving our understanding of actual outcomes (e.g., Warwick 1994; Laver and Shepsle 1996; Baron 1991, 1993).

Legislatures also have garnered an enormous amount of fruitful attention, especially in American politics where Congress, whose powers are unusually enhanced by the country's separation of powers, arguably is the center of the Constitution (Wilson 1956 [1885]). Representation, of course, is the core concept of political liberalism; it lies at the heart of

Locke's *Second Treatise* (1980 [1689]) and is the core element of government by consent. By representing the preferences and interests of members of civil society, legislatures are the key institution for liberal democracy. A central puzzle, notwithstanding, is which of their activities—including legislation, legitimation, oversight, and constraint of the executive, as well as the representation of voters' preferences—enhance chances for the persistence and success of such polities. For an extended period, Congress provided the principal model, since other parliaments could not legislate or self-organize in the American manner (Polsby 1968).

As Gamm and Huber show in this volume, recent scholarship has reacted against this understanding, showing that legislatures, even if not American in organization and style, can influence policy and the executive through other mechanisms (e.g., Huber 1996a; Tsebelis 1994, 1995). Still, studies of Congress continue to dominate legislative studies. Recent work has reopened two areas of particular interest: the ability of the legislature to constrain the executive and the functions of committees. These contributions, too, place the preservation and enhancement of liberal democracy at their center. This normative value is enhanced when the legislature is able through anticipated reaction to constrain the executive and make better policy choices in a complex environment because of the informational role of its committees (e.g., Epstein and O'Halloran 1999; Krehbiel 1991, 1998).

The attention that political scientists have paid to the state includes a long-standing concern with how different institutions interact. Indeed, strategic interaction among the institutions of democracies has been a key concern from the discipline's start; the long-standing notion of checks and balances involves such interaction. How should these relations be constructed to be consistent with liberal principles that put a premium on limiting the power of rulers? Long a key concern, the topic of balance of power between executive and legislative branches today is being examined more thoroughly than in the past. In presidential systems where the executive and legislature are independently elected, the existence and character of the game defining the relations between the two branches are most apparent. Still, such strategic interaction occurs in all liberal democratic political settings as actors within the institutions vie for influence over policy. Thus both in American and comparative politics, scholars have sought to flesh out the games that arise from this interaction. Recent models and data reveal subtle ways in which legislatures delegate to the executive yet maintain control through threats to unseat governments or revoke its delegation and, in turn, ways executives use their threats of the veto or decree power to move legislatures in their preferred direction (e.g., Shugart and Carey 1992; Huber 1996a, b; Epstein and O'Halloran 1999; Cameron 2000). Some work also has extended this approach to the study of judicial systems.

This literature on institutional interaction also has returned to the

key theme of gridlock that so obsessed analysts of Weimar's fall (Holborn 1982). Divided government, with the executive and median legislator in different political parties, promotes checks and balances and policy moderation but can be a recipe for policy immobility, making leaders unable to respond to a changed environment (for a skeptical view see Mayhew 1991). Moving beyond divided government, some scholars have explored the general conditions under which the strategic interaction among various institutions of democracy can lead to policy gridlock (Tsebelis 1995; Krehbiel 1998). Such policy immobility paradoxically can be a central source of democratic instability and failure. Institutions thus help solve the Arrovian problem of cycling majorities and instability in democracies but, in turn, raise serious concerns about the ability of such states to respond to changing conditions due to their status quo biases.

The question of governability under democratic conditions has been connected for decades to puzzles about the rational qualities of mass publics and their readiness for political participation. Reaching back to the work of Walter Lippmann (1922) and Lindsay Rogers (1949), one branch of research has treated the central problem of liberal regimes as residing with its citizens rather than with its rulers or rules. "Democracy is of a massive nature. Therefore it cannot function without masses," Roberto Michels (1927, 762) wrote in an early issue of the *American Political Science Review*. The beliefs, opinions, and ultimately the behavior of individual citizens either is suspect or is treated as consistent with democracy only under some conditions of political culture or civic life. The latter theme has been especially important in comparative politics, where important scholarship has focused on the correlates of democratic behavior and civic culture (Almond and Verba 1963, among others) and on the role of associations in producing the development of trust among citizens (Tocqueville 1969 [1832] to Putnam 1993a, 2000). In the American politics subfield, a robust literature on public opinion has been very important, if also subject to controversy. From Lazarsfeld et al. (1944) and Campbell, Converse, Miller, and Stokes (1960) to Popkin (1991), Page and Shapiro (1992), and Zaller (1992), scholars have asked whether citizens have knowledgeable opinions about politics and whether these form a coherent (enough) system of beliefs either to permit democratic control of elites or informed participation in the public arena.

This search for, and these signs of, relatively knowledgeable and consistent citizens has also extended to voting, a theme Scholzman addresses in her essay in this volume. Together with public opinion, this work has broad implications for understanding the relationship between states and liberal values. Our knowledge about and methods for studying voters have developed greatly over time. This interest in public opinion, beliefs, and behavior is founded on a vision of the state where individual citizens, especially citizens as voters, provide the foundation for popular sovereignty. The possibility of choice by voters and the circumstances guiding that

choice thus are of central importance to a liberal society (Lazarsfeld, Berelson, and Gaudet 1944; Campbell, Converse, Miller, and Stokes 1960; Nie, Verba, and Petrocik 1976). Our understanding of that public has expanded as it has come to include more diverse individuals, themes that Burns, and Dawson and Cohen take up here in their essays, respectively on gender and on race.

The public's interaction with the state also occurs in collective, not just individual, forms. Interest groups are an important element of liberal democracy, but they too pose vexing issues. Collective action by groups pursuing their interests often is seen as beneficial; how otherwise would the state know the concerns of its citizens or the intensity of their preferences? On the other hand, such groups pose grave challenges. The optimistic view of groups suggests that if all interests could form and press their demands, political leaders would know well the desires of the public (Truman 1971 [1951]). The literature on logrolling, however, has been less sanguine. If all groups act collectively in defense of their interests, a state might end up with policies different from those any one group desires and much worse than otherwise, as the Smoot-Hawley tariff demonstrated (Schattschneider 1935). Such pessimism appeared early in the discipline and was famously reinforced by Mancur Olson's demonstration (1965) that not all groups could or would act collectively; only certain privileged ones would. Worse yet, these likely would be groups that could reap highly concentrated benefits from their desired policies at the expense of the public interest. Democratic politics look very different in the presence of such active lobbies representing only parts of the public (Lowi 1964). Recently this debate has continued over the benefits given by the signaling and informational aspects of interest groups as compared to their corrosive impact on the representation of the public interest in policymaking (e.g., Chubb 1983; Walker 1991; Austen-Smith and Wright 1992, 1994; Kollman 1998).

If political science has been concerned intensely to understand the impact of institutional design and democratic participation on the persistence of liberal regimes, it also has had to consider international relations where the state appears as a nakedly coercive institution. A paradox is at work in the international relations subfield. The main tradition in international relations does not privilege the liberal democratic state, as the essay in this volume by Walt shows. Rather, the state often is conceived as a unitary dictator.¹⁶ The realist tradition, however, has been concerned with how liberal states fare in an anarchic international system; from Morgen-

16. From one perspective, this account is consistent with key features of the history of liberal regimes that have conferred rights on members but, whether in geopolitics or in the conquest of overseas empires and their treatment of native peoples, have been prepared to act in a more severe manner.

thau (1948) to Kissinger (1957) and Waltz (1959, 1979), liberal democratic states have been viewed as dubiously able to perform the activities necessary to assure their survival in a brutal international environment. How can these states counter their enemies as effectively as illiberal regimes free from the constraints of representation and rights? In part, answers to this have focused on the role of international institutions in defusing the anarchic elements of the international system (e.g., Krasner 1983; Keohane 1984; Martin and Simmons 1998).

Both policy instability and immobility in liberal democracies have been causes for concern in understanding states' foreign policy responses. Allison's work (1971) on bureaucratic politics, for example, focused attention on the inner workings of democratic institutions, showing that competition among bureaucrats significantly affected foreign policies. Others have brought executive-legislative interaction into the picture. Trying to use models of the state from the American and comparative fields, they have shown that the strategic interaction between democratic institutions need not lead to foreign policy failure. The interaction might make foreign policy more difficult to conduct, but it might also give the democratic state an advantage in international bargaining (e.g., Fearon 1994a; Milner 1997; Milner and Rosendorff 1997; L. Martin 2000; Schultz 2001).

Most important, the literature on the democratic peace has recently had a profound impact, as Morrow argues here. Reversing the realist view of liberal states as hopeless in foreign affairs, this literature has argued that they may be far more able to conduct peaceful foreign policy and cooperative relations with other states than other regime types (e.g., Doyle 1986b; Russett 1993; Bueno de Mesquita, Morrow, Siverson, and Smith 1999). The complex internal workings of democracies actually enable them to function better in the international environment. This optimistic view is not new (Wilson 1919), but it certainly never before has captured a dominant position in international relations.

Concern with the state's relationship to citizens in civil society and to other states is complemented, of course, by an interest in the state's relationship to the economy, a theme that Geddes's contribution addresses in relation to developing countries. Here, political science has a checkered record, both attentive and neglectful. Many scholars have taken up Marx's challenge to consider the association linking capitalism, liberalism, and democracy (Schumpeter 1942; Polanyi 1944; Hayek 1994 [1944]). Some think capitalism threatens democracy. Markets may be efficient but they also produce very divergent outcomes for different individuals and groups. Yawning inequalities of wealth and market distributions can translate directly into political power (or its absence), thus threatening the robustness and meaning of majority rule. Moreover, markets left to their own devices may produce so much insecurity that individuals can become willing to sacrifice liberal rights and democratic regimes on behalf of security and

more equal resources (e.g., Polanyi 1944; Katznelson 1996). If liberal democracy is to thrive or in some instances even survive, active state intervention to regulate markets and reshape their patterns of distribution becomes imperative. This requirement is the basis, of course, for the vast literature on modern welfare states (e.g., Esping-Andersen 1992; Flora and Heidenheimer 1981). Reciprocally, democracy also may threaten market-based economies. Demands for redistribution and rent seeking by interest groups can be particularly potent in democracies and thus erode the efficient operation of markets. The requirements for democracy and well-functioning markets, especially in a highly globalized environment, may pose even more very difficult issues (e.g., Garrett 1998b; Rodrik 1997).

Other scholars have underscored how liberal democracy and capitalism may be mutually supportive (Friedman 1963; Lindblom 1977). Liberalism, especially its impulse to secure private property rights and ground political rights in property ownership, may be the only foundation for the growth of capitalism. As Polanyi (1944), North and Weingast (1989), and others have argued, the operation of markets depends crucially on the political system—a point recognized as well in international political economy (e.g., Gilpin 1987). Economic growth is most likely to be robust where governments ensure private property rights and equalize social and private returns. Much current research shows that the rule of law is essential for economic prosperity (Barro 1997; Haldenius 1992). Capitalism thus is considered by some to be a bulwark for democracy (e.g., Hayek 1994 [1944]; Friedman 1963). Without private property and markets, citizens are left to the economic prey of the state. Without their own means of economic support, how can individuals resist the encroachments of the state, let alone control its behavior? As Lindblom (1977) points out in his study of the way the advantages of business can bias a political system, historically there have been no democracies without market-based economies, though the degree of market freedom has varied under liberal democratic auspices.

Issues of political economy thus have been important to the quest by political science to come to terms with the modern state in a realistic fashion. However, political economy has played less of a role in the history of the discipline than the other subjects surveyed here. Indeed, much of the theory and research in this area has been ceded to economists, leaving political scientists to focus rather more on the core zone of liberal politics where the state meets civil society. Currently, though, there is growing interaction between economists and political scientists who study political economy, in part as a result of a growing affinity of research methods, as the essays by Frieden and Martin, and Alt in this volume demonstrate. What political scientists bring to these collaborations is their long-standing concern with the state and problems of liberal democracy.

■ | State of the Discipline: An Afterword

The state of the discipline? Contested and methodologically diverse, political science nonetheless remains focused, as it has for a century, on a particular understanding of how to study the modern state and liberal democracy. Though there have been shifts, of course, in emphasis and method, attempts to periodize the discipline's history mislead if not grounded in these powerful continuities delineating the discipline. Moreover, though political science has not produced fixed findings in the strong sense of the term (Lindblom 1990), its intellectual debates have been cumulative and its disputations have grown more textured, more variegated, and in many respects, though not all, more capable over time.

The essays in this book, of course, record a discipline almost unrecognizable to its founders in method, range of inquiry, and achievement. Political theory has surmounted the descriptive history of ideas that dominated in the early-twentieth century. Comparative political studies have vastly expanded in scope beyond an institutional record of what foreign governments do. Americanists, deploying fresh systematic tools, have parsed the country's institutions and behavior with great analytical detail. Students of international relations have far more thoroughly grasped the tensions that confront liberal democracies in a dangerous and predatory world as they engage in trade, finance, diplomacy, and war.

Still, Goodnow, Beard, Bentley, Merriam, and the other creators of modern political science would know that they belong to this family of pragmatic, institutional, and realistic inquiries. They, too, wished to understand the state, and especially the liberal state, in an age of democratization and globalization, to assay liberal democracy via the study of institutions and approaches to justice, to treat as privileged the study of participation and behavior by citizens in these regimes, and to improve the tools they possessed to advance the scientific study of politics. They would not have been taken aback by the character or the content of the reviews of the discipline present in this book. As practicing political scientists, we continue to carry our history with us.

This observation is no mere antiquarianism. If we focus mainly on variety, methodological differences, or the massive changes that have characterized this history of the discipline rather than these continuities, we will fail to see both its cumulative attainments and the costs, as well as the gains, that have attended the discipline's bounded set of inquiries. If the state of the discipline is good and advancing, it also still must be judged as wanting to the extent to which key vexing questions—among others, questions of the robustness of democracy, the inclusiveness of citizenship, the powers of ascription and illiberalism, the varieties of legitimate political identities, the deep incommensurabilities of values and cultures, the dark side of human psychology and propensity for cruelty and violence, and the

illusion that all the pieces of the liberal puzzle can neatly line up together—are left unresolved.

Political science may have a particularly notable role to play in sharpening understandings of its landmark themes of state, democracy, justice, and participation at this historical moment. Writing about the character of global political life at the start of the twenty-first century, Geuss, a political theorist who has worked on both sides of the Atlantic, observes that there has been convergence on “a single ideal model” of “the democratic liberal state with a capitalist economy, and a commitment to a set of human rights for its citizens . . . with five distinct elements here—liberalism, democracy, the state, capitalist economy, the doctrine of human rights.” He notes further that “in much contemporary thinking about politics it is tacitly assumed that these five items form a more or less natural, or at any rate minimally consistent and practically coherent, set” (2001, 3). As an organized discipline, political science has been ahead of its time in that it has taken this group of traits and institutions as its domain for some eleven decades. No one who apprehends the field can confidently conclude that these dimensions form a stable constellation neatly or automatically. If the discipline is to successfully marshal its full potential, it must be done with an inquiring spirit that reckons with the relationship among these values and concepts in a diverse and increasingly insecure global environment.

| **Political Science:
The State of the Discipline**

1 | The State in an Era of Globalization

How might we best conceive of the state given its transformations over time and the pressures that shape it? The papers that consider these questions highlight such internal challenges for states as ethnic and regional conflict and such external conundrums as the anarchic state system and international conflict, and they underscore the powerful and global economic forces with which states have to contend.

This part opens with Margaret Levi's reflections on "The State of the Study of the State." Addressing definitional questions, she considers what we know about what the state does and can do and about the relationships among the institutions of state, government, and market. She then turns to analytic and methodological issues. Throughout, she addresses normative questions concerning the role states should play in the economy and polity, raising problems of democratic accountability. Nettl (1968) rightly observed that the concept "state" within the social sciences goes in and out of fashion due to the combination of problems encountered in using this bulky term in rigorous analysis and the extent to which the state seems to be a primary factor in what concerns the contemporary world. For Levi, a crucial current problem lies in the failure of scholars to develop adequate methodologies and theories of stateness. Despite a massive literature on the state developed over the course of centuries, she believes state theory to be still in its infancy. By combining an interdisciplinary perspective with adequate attention to the variations and specificities of state actions and effects, there is hope, however, that we can untangle the complex relationships within states and among states, governments, and civil societies.

Miles Kahler's "The State of the State in World Politics" argues that after decades of axiomatic importance in international relations, the somnolent theoretical world of the state has come to life. New research is diverse, but its agendas share an initial assumption that the state is a variable, not a constant in world politics. Research in political science has emphasized globalization as a source of new political and sectoral coalitions or as

a constraint on government policies. Investigations of its effects on the state, Kahler underscores, have been hampered by concentration on state capabilities as the only factor of interest. Three strategies promise a more measured and accurate portrait: modeling the preferences of nonstate actors and their bargaining with the state, explaining the migration of governance functions from the national state to other public and private sites, and charting the advantages of particular institutional designs in a globalized environment.

A second research agenda on unit variation in international relations regards the modern nation-state as only one, temporarily successful, contender amid numerous competitors over the course of history. Kahler thus explores antecedent and contemporary alternatives to the sovereign, territorial nation-state. He discusses the significance of variations in the relationships linking state and nation and assesses how the emergence of particular types of units and their disappearance influence system outcomes. Finally, he shows how institutional analysis can provide means for defining and measuring variation in the characteristics of states or other units in world politics. These research agendas require theoretically informed taxonomies, a rejection of any sharp distinction between domestic and international concerns, and careful attention to the environments surrounding states and their influences on variation among states.

In "State, Society, and Development," Atul Kohli examines how varying patterns of state authority emerge in developing countries and how state authority is in turn used to push these countries along a variety of developmental pathways. These questions are of enduring interest to scholars of comparative political development. While these concerns can be addressed from multiple theoretical standpoints, he argues that over the last quarter-century, the most important research has been informed by what he labels a state-society perspective. Following a summary of the key elements of this neo-Weberian perspective, the essay reviews research in this tradition, exploring two main themes: the emergence of a variety of authority structures (revolutionary, authoritarian, and democratic regimes, as well as failed states) in the context of development and the impact of state structures and actions on such outcomes as good governance, economic growth, and redistribution. The essay takes into account some of the main criticisms of this research tradition by culturalists and rational-choice scholars, maintaining that when judged against such criteria as the capacity to generate cumulative and generalizable knowledge, to connect the deep knowledge of area specialization to social and political theory and analysis, and to attract the interest of a range of risk-taking senior and emerging scholars, the state-society perspective represents a robust research tradition in the study of comparative political development.

Jeffrey Frieden and Lisa Martin review developments in a part of international relations in "International Political Economy: Global and Domestic Interactions." They observe that this relatively new field has moved

from a stage of paradigmatic conflict to a more consensual approach to analysis. The biggest challenge facing the field is in understanding the simultaneous interaction of domestic and international factors as they determine foreign economic policies and international economic outcomes. Rigorous work on domestic-international interaction now is possible because it can build on well-established intellectual building blocks at the domestic and international levels. Frieden and Martin develop this argument by presenting an analytical organization of major work in international political economy and illustrate it with empirical applications drawn from international trade, monetary relations, and other issue areas. Though strong frameworks for analysis have been developed, they conclude by averring that the scope for applying them and extending them to new issues remains considerable.

James Alt considers "Comparative Political Economy: Credibility, Accountability, and Institutions." Where multilateral sanctions provide the only enforcement incentives for accountability, cooperation and coordination problems can arise and institutions can help solve them. In the literature, such institutions bestow a large role on the credibility of monetary policy and on centralization and transparency in fiscal policy. Progress in understanding monetary policy has come from analyzing strategic interactions among central banks, labor market agents, and partisan governments and legislatures. Prominent among explanations of variations in fiscal scale and balance are common pool problems and the interaction of electoral and budget process institutions. A review of this vast literature demonstrates the importance of relating policy outcomes not only to economic fundamentals but also to the ideological and redistributive goals of political parties and interest groups and the institutional contexts in which policy is made.

James Morrow's "International Conflict: Assessing the Democratic Peace and Offense-Defense Theory" examines another area of international politics in which states are fundamentally challenged by external forces. He claims that international relations was characterized by four main movements during the decade of the 1990s. First, game theory attracted attention as a way to study the microfoundations of the field. Second, interest in statistical methods revived both in international political economy and conflict. Third, the constructivist movement challenged the questions, approaches, and results of the mainstream. Fourth, the end of the cold war led to new problems for study and a shift in focus away from great power rivalry. The first three of these trends created methodological ferment, which occurred with less acrimony than in past decades. However, he observes, the field has not reconciled this ferment into a commonly accepted approach for using these methods. Seeking to advance such a development, the essay reviews progress in the game theoretic microfoundations of conflict, the democratic peace, and offense-defense theory. Each has predominantly used one of three methods: formal models,

large-*n* statistical studies, and case studies, respectively. Morrow seeks to show how the three methods can be combined fruitfully, a task none of these areas has yet achieved.

Closing the section is Stephen Walt's essay on "The Enduring Relevance of the Realist Tradition." Though widely criticized, he avers, the realist tradition has yet to be supplanted by an alternative perspective with similar range or equal explanatory power. Although critics have been quick to announce the obsolescence of realist theory in the wake of the cold war, the real world seems to have paid scant attention to such academic obituaries. Major powers remain acutely sensitive to the distribution of power, are wary of developments that might leave them vulnerable, and still strive to enhance their position at the expense of potential rivals. Although states do cooperate in a variety of ways, military force remains a depressingly constant feature of international political life. As events in the Balkans, Central Africa, South Asia, and elsewhere remind us, insecurity and the struggle for survival are still important elements of the human condition. Substantively, the essay reviews the main developments in the realist tradition since the publication of Kenneth Waltz's *Theory of International Politics* in 1979, showing how recent efforts have helped refine the basic logic of the theory, broadened its range of applications, and creatively explored its implications for the future.

MARGARET LEVI

The State of the Study of the State

The concept of state is not much in vogue in the social sciences right now. Yet it retains a skeletal, ghostly existence largely because, for all the changes in emphasis and interest of research, the thing exists and no amount of conceptual restructuring can dissolve it.

(Nettl 1968, 55a)

Nettl's words, published more than thirty years ago, resonate today. After a period of scholarly emphasis on theories of the state, social scientific interest in the state seems to have taken a back seat to institutions, on the one hand, and social capital, on the other. Consideration of the state as a conceptual variable ebbs and flows; there are good reasons for claiming that the state is useless as a concept and equally well founded resistance to shelving it. We know that the nation-state is a problematic notion, given the number of nations without states and the number of states without nations. We know that the continental European model of the state has limited descriptive or theoretical usefulness in understanding Britain, the Americas, or the antipodes, let alone the developing world. We have found the efforts to measure the degree of stateness far from satisfactory. Even so, "The State" captures the combination of centralized, far-reaching coercion with the complex of staff, governmental institutions, and nongovernmental actors and agencies in a way that nothing else seems to.

A state, whether it arises out of or is imposed on civil society, is at the core of modern Western political theory à la Bodin and Hobbes, and it is a bedrock of comparative political and sociological analysis à la Marx and Weber. The existence within a given territory of a state apparatus that concentrates violence, coordinates regulation, and possesses a government able to make and implement public policies has long been considered a key component of economic and political development. So, why has it proved so difficult to use the concept of the state in analysis?

There are several reasons, and they have more to do with the methodology of understanding the state than with the applicability of the concept.

First, the state is a composite of factors, not a single variable. Sometimes, only one or a subset of those factors, for example, the ruler or the ruling class or the bureaucracy, is doing the crucial explanatory work. Good analysis requires differentiating among the features of the state in order to assess their relative importance; the state becomes less than the sum of its parts. Second, the state is an abstraction, but key decisions are made by state personnel or rulers not by the state *per se*. Once again, the analysis focuses on a particular aspect of the state, and the state becomes less than the sum of its parts. Third, the state is sometimes what is affecting a situation, sometimes the focus of action, and often what is transforming and being transformed at the same time. This requires a dynamic model with complicated feedback loops. Such models are often the stuff of caricature rather than hardheaded inquiry. Fourth and finally, the state is historically and geographically bounded; it is a notion that does not have meaning in all places and at all times. There have not always been states, and there are not states everywhere. Some of the efforts to identify it or to assess its degree of penetration may simply be misplaced. Analysts are not always careful in specifying the scope conditions of their concepts, and much state theory illustrates the problems that arise from this failure.

The concept of the state is also out of fashion because of shifts in emphasis in political practice. There is attack, from the right and from the left, of the integrative, centralizing, and coercive features of the state, and there is skepticism about the reality of the state's sovereignty and relative autonomy in the international sphere. The very model of the state that was the goal of many countries in the past three centuries is now being questioned in advanced industrial democracies as well as in the transitional postsocialist countries and the Third World. At the same time, in both developed and developing countries, there is mobilization for change and demands for popular autonomy and control. The targets are the large states and the mammoth corporations and international organizations those states support. Democratization is a major rallying cry for a wide range of groups and individuals who feel that the state is distant and, more importantly, unrepresentative of their interests, concerns, and values.

In many advanced industrial countries, there is also an attack on the large government that the traditional model of the state seems to imply. Margaret Thatcher, Ronald Reagan, and neoliberal economists who emphasize deregulation and *laissez-faire* have initiated a serious reconsideration of social policies and taxation levels. In developing countries and within the international banking and aid communities, the turn from government reflects the corruption, cronyism, and inefficiencies that are as likely to flow from the state's personnel as growth-enhancing policies. When the rearrangement of the global economy is added to this mix, large-scale transnational corporate decision making appears to be of far greater consequence than state-based decision making. And, of course, the emphasis on civil society by both policymakers and protesters reflects the col-

lapse or inadequacy of governments and the very states in which they are embedded. It seems that states are not always where the action is or, for some, should be.

Among comparativists an urgent question is whether we are witnessing the end of the era of the modern state and large government.¹ These concerns are implicated with the contemporary debate over the appropriate role of government and democratic interventions, especially in Europe and in transitional and developing countries facing serious state-building and governance issues. At the very heart of state theory is how to best achieve social order, promote economic growth, and facilitate democratic expression. Thus, it is very difficult to separate the normative and institutional design questions about the state from the empirical program focused on explaining variations in state arrangements and their effects.

As this brief introduction suggests and as the following literature review makes clear, the range of subjects included in discussions of “The State” are very numerous indeed. The literature leads us to definitional issues, then to consideration of what we actually know about what the state does and can do and of the relationship among the institutions of the state, government, and market, and finally to analytic and methodological issues. Throughout I address the normative questions of what role the state should play in the economy and polity and raise problems of democratic accountability. All of these topics have received considerable scholarly attention—past and present; I apologize in advance to all authors whose important work I fail to note.

It does not worry me that the study of the state per se is currently out of fashion; swings of the pendulum are the stuff of social science. What does worry me is that we may not build usefully and constructively on the scholarship to date. I am a guarded optimist; I believe that there is a possibility for cumulative knowledge in the social sciences but only if we take into account the work that has gone before and only if we pay attention to the methods and findings currently in play. More critically, I want us to maintain a research program on the state with a healthy skepticism about the role of the state. I am most certainly not advocating that the state, let alone big government, is always a positive force in development. Douglass North, an economic historian committed to understanding the role of the state (1981, 1990) is the author of a statement I quote often: “The existence of the state is essential for economic growth; the state, however, is the source of man-made economic decline” (1981, 20). It is this anomaly of state intervention that our research must address.

1. There are a considerable number of articles on this subject. See, e.g., Evans 1997; Hagopian 2000; the symposium in *Governance* 13: 2 (April 2000), edited by Graham Wilson, pp. 233–78, which includes articles by Wilson, Sbragia, Peterson, and Michiletti.

■ | How Did We Get Where We Are?

State theory, as it is often called, has gone through several incarnations, and several quite distinct controversies have dominated the discussion at different times. The early theorists concerned themselves with the problem of social order. To understand its source, Thomas Hobbes, John Locke, and Jean-Jacques Rousseau rested their arguments on assumptions about the state of nature. Since the nineteenth century, anthropologists have been debunking these pictures of stateless or, at least, acephalous society, but one legacy is the ongoing debate about the circumstances under which cooperation and the production of public goods is possible without a state. The underlying logic of liberal theory also gave rise to another set of controversies about whether the origin and maintenance of the state is based on consent or coercion, controversies with clear links to concerns about democratic accountability. These remain lively issues among those contesting state interventions in the market or individual lives.

The classical economists, the original liberals that neoliberals revere, for example, Adam Smith and David Ricardo, and their counterparts in political philosophy, for example, David Hume, Jeremy Bentham, and John Stuart Mill, generally emphasized an important but minimal role for the state in protecting contracts and offering certain public services that would not otherwise be provided. However, the main target of the economists was the elimination of regulations and monopoly privileges that inhibited trade. For the philosophers it was the use of the state to promote individual liberty and the inhibition of state-based infringements on that liberty.

The most famous critics of classical liberalism, Karl Marx and Friedrich Engels, focused on the role of the state in promoting economic growth and improving the human condition, the “high modernist” project James C. Scott (1998) and others condemn. As depressions, inequality, imperialism, and Marxist revolutions led to the significant expansion of state power, theory was not far behind. Leninism gave a formidable role to the state in building socialism. By comparison with today, so too did the politics and economics of governments that accepted Keynesianism, social democracy, or some form of the New Deal. Mid- and late-twentieth-century literature on the state reflected the governing elite’s preoccupation with the importance of the state role and with popular demand for, or resistance to, government intervention and protection. Research was generally framed around the issues of the development of modern states. Detailed historical and comparative research characterizes much of this work. This was the era of grand models of modernization, on the one hand, and of area studies, promoted in the United States by the State Department, on the other hand. This was the heyday of what are now labeled the “old institutionalists,” many of whose classic works are now receiving a renewed reading (e.g., Stinchcombe 1997; Remmer 1997).

Beginning in the late 1960s, leftist scholars revisited Marxist and class analysis as a basis for the understanding and investigation of the relationships between states and capitalism. In France, Nicos Poulantzas (1968, 1969) and Louis Althusser (1971) initiated a worldwide debate. In the United States, James O'Connor (1973) raised the alarm bell about the "fiscal crisis of the state"; in Germany, Claus Offe and Jürgen Habermas discussed the "legitimation crisis" (see, esp., Offe 1973a, b, 1984; Habermas 1975), and subsequently other major scholars took up similar themes and concerns (see, e.g., Block 1977, 1980; Jessop 1990; Przeworski and Wallerstein 1982).

The capitalist state controversy was one impetus behind the outpouring of major texts that situated the state in history and used forms of structural analysis linking the form of the state to the nature of the economy. Perry Anderson (1974a, 1974b), Charles Tilly (1975, 1990), Theda Skocpol (1979), Michael Mann (1986, 1993), and other state-centered structuralists focused on the interaction between state arrangements, the holders of state power, geopolitics, and civil society, particularly its class composition. They tended to work with large-scale macro variables and to treat the state as a unitary actor. Their mantra became "bringing the state back in" (see Evans, Rueschemeyer, and Skocpol 1985). The first of these authors tended to be historians and sociologists, but they were soon joined (and sometimes preceded) by numerous political scientists in comparative politics (e.g., O'Donnell 1973) and international relations (e.g., Krasner 1978; and Katzenstein 1978, 1984). Over time, their self-identification changed from state-centered structuralists to historical institutionalists.²

At approximately the same time, there arose an alternative approach to analyzing the state, largely initiated by Douglass North (1981) and Yoram Barzel (1989) in economics and developed by Robert Bates (1981; also see Bates and Lien 1985) and, I like to believe, myself (1981, 1988) in political science. These models rely on neoclassical economics but bring in a strong dose of structuralism in the form of property rights or class power. All build on the microfoundation of constrained rational actors, who face collective action problems and opportunity costs, have only relative bargaining power, and must consider the transaction costs of their policies. In a literature review in my 1988 book, I labeled the impulse behind this approach as "bringing people back into the state." For the rationalist state theorists, the state per se is not the actor; the actors are the agents who compose the state and the social, political, and economic groups who make demands on the state.

The action in rational choice theorizing about the state comes from the assumptions about the maximands of the key actors and variation in

2. See Thelen 1999 and Pierson and Skocpol this volume for a discussion of the evolution and contributions of historical institutionalism.

the constraints on their action; rationalists argue over the appropriate maximand and the most critical constraints. For example, Edgar Kiser (1994) posits power maximizing by rulers in contrast to my presumption of revenue maximizing; the adjudication of the power of our theories is through testable implications. After deducing a plausible set of goals for state actors, rationalist state theorists then focus on the rules and arrangements that constitute the state and on the relative bargaining power and influence of nonstate actors. The combination of the institutions that constitute the state and of power relationships within society structures the incentives on state actors to establish, implement, and enforce the policies that they do.

Battle lines were being drawn between these two very distinct approaches to state theory, the one grounded in historical macrosociology and the other in microeconomics. Throughout this whole period, of course, innumerable scholars of comparative politics offered analyses of the role of the state in political and economic development that did not fit neatly into either camp. Some of the names that come immediately to mind are Alice Amsden (1989), Robert Wade (1990), and Stephen Haggard (1990).

The most recent research on the state comes from several perspectives. Carrying on the historical sociological tradition are Brian Downing (1992) and Thomas Ertman (1997). Both emphasize geopolitics, but Downing is focused on fiscal extraction and military mobilization while Ertman stresses the role of local governance institutions. The rationalist perspective is represented by David Laitin (1992; also, see Laitin et al. 1994), Barbara Geddes (1994), Edgar Kiser and X. X. Tong (1992, 1999) and Yoram Barzel (2001; also, see Kiser and Barzel 1991; Barzel and Kiser 1997), and Mancur Olson (1993), whose work focuses on state building, state reform, and the origins of democracy. There is also a strong tradition within international relations, represented by Henrik Spruyt (1994), Janice Thomson (1994), and Andrew Moravcsik (1997, 1998a), whose concerns with sovereignty led them to think more deeply about the state itself. In addition, there are numerous texts trying to synthesize the various approaches to the state and suggest new directions (e.g., Hall and Ikenberry 1989; Hobson 2000) as well as some serious efforts to test specific aspects of some of the major claims of state theorists (see, e.g., Rasler and Thompson 1985; Thompson and Rasler 1999). Finally, an important group of younger scholars is applying and improving the theoretical models of the past to understand the transitions in contemporary states (e.g., Whiting 2000; Grzymala-Busse 2002; Luong 2002; Kang 2002).

Most of the work cited above is state centered, whether defining the state or state personnel as the key actors. Another tradition, equally long in the tooth, concentrates on the alternatives to the state as a system of regulation and means of ensuring cooperation and social order. The contemporary version is found in the scholarship that locates community-based and spontaneous solutions to collective action problems. Michael Taylor's

seminal work (1987 [1976]), Elinor Ostrom's award-winning *Governing the Commons* (1990), and the opus of William Riker have influenced numerous researchers to search for decentralized nongovernmental, albeit often institutional, arrangements for producing collective goods and for securing cooperation and exchange. In line with this program was the final major work of the great sociologist James Coleman (1990b, 300–21) in which he reflected on how social capital, defined as the social structural resources that derive to an individual from social relationships and social organization, can facilitate individual actions. In Coleman as well as in the even more group-oriented work of Robert Putnam (1993a, 2000) on social capital, the role of the state is not condemned; it is ignored or treated as secondary.

There is another group of scholars, some of whom began with a focus on the state, whose historical researches led them to discover nonstate solutions to social order and collective action problems. The modeling of these alternative institutions for regulating markets, such as the Hanseatic League or merchant guilds (Greif 1994b; Milgrom, North, and Weingast 1990), or for ensuring social peace and economic growth where there are potentially warring factions, such as the *Podesta* (Greif 1994a, 1998), fueled the version of the new economic institutionalism that now influences comparative politics.

A very different research tradition, associated with the popularity of interpretive and Foucaultian approaches, also produces criticism of state-centric research. Joel Migdal (1988, 2001; also see Migdal, Kohli and Shue 1994) introduced the state-in-society approach in order to emphasize the importance of social groupings in delimiting the penetration and effectiveness of states in many developing countries. Scott (1998) went further. He defines the state as a “totalizing” project with grand but failed schemes to “improve the human condition” (part of his subtitle). Paul Brass (1997, 2000) makes a similar claim. Migdal, Scott, Brass, and others emphasize power relations and the resistance of the populous as the subjects of interest rather than the state.

The reemphasis on the state as a conceptual variable produced good work that in turn stimulated a dialogue with scholars who wish to deemphasize, combat, or more simply reconsider the import of the role of the state in economic and political development. But what have we actually learned as a result of the large body of work now available? It is to that question I now turn.

■ | What Is the State?

One of the problems scholars confront in their efforts to use the state as a conceptual variable is how best to define it. Without knowing what the

state is, it is difficult to determine what role it is playing, how “strong” it is, and so on. Scholars long tended to rely on Max Weber’s definition, “A compulsory political organization will be called ‘a state’ insofar as its administrative staff successfully uphold [*sic*] the claim to the *monopoly* of the *legitimate* use of physical force in the enforcement of its order” (1978 [1956], 54, Weber’s emphasis). This definition captures several key attributes of a state: its coerciveness, its reliance on physical force, and the existence of an administrative staff. However, as the newer literature on the state teaches us, the extent to which the state has a monopoly of physical force and the extent to which the use of physical force is legitimate are variables, not elements of a definition. Moreover, Weber’s definition does not capture the territorial qualities of states or, sufficiently, its regulatory capacities by means of laws and norms. His work certainly confronts such questions, but his classic definition does not seem to get us where his agile mind otherwise takes us.

Let me offer a less parsimonious but more comprehensive alternative, drawn from the literature I have just reviewed. A state is a complex apparatus of centralized and institutionalized power that concentrates violence, establishes property rights, and regulates society within a given territory while being formally recognized as a state by international forums.³ All states share some common characteristics: a legal structure and coercive apparatus that creates and enforces property rights; a system of laws and norms that regulates interactions among those who live in the state; a mechanism for trading with, defending against, and attacking those in other states; and procedures and agencies for taxing and policing the population. A state performs these roles by means of a wide variety of institutions, agencies, social norms, and actors, some governmental and some not. This approach to conceptualizing the state leaves open the question of legitimacy and general popular acceptance; some states have these characteristics and some states do not. It also means there can be varieties of states: some large, some small; some deeply penetrative into the society, some not so embedded. Thus, each state, even states with virtually similar laws, constitutions, and governmental arrangements, is distinct, because the combination of the complex of factors is almost by necessity particular and unique.

It is with the state that governors, subjects, and citizens identify. Citizens who oppose a particular set of governmental officials may still feel loyal to the state itself. “Indeed,” as Migdal argues, “what has distinguished the modern state from most other large-scale political organizations in history, such as empires, has been its insinuation into the core identities of its subjects” (1994, 13). Patriotism and nationalism are among the forms such an identity takes. The salience of the state as an identity is verified partially

3. Formal international recognition excludes statelike organizations such as the mafia.

by the existence, in the modern world, of the phenomenon of statelessness. There are those who either feel no identity with any given state or feel identity with a nation or people who lack a territorial base or have lost control over a traditional territorial base.

States are comparable to but distinct from city-states, empires, and other such alternative means of institutionalizing power and identity over a given territory. States contain but are also distinct from their laws, bureaucracies, and governments.

States can be formed, but they are not selected. Governments are selected. A government is the organization that sets and administers public policies, and types of governments are distinguished by arrangements for making and implementing law and legislation. They can be authoritarian, democratic, or some other regime type. Government is, often, the main engine of the state, for it is the government that arranges the legal waging of war, administration of justice, collection of revenue, and provision of official state benefits. But major shifts in the personnel, policies, or even form of government can change while the state remains stable. It is difficult to imagine the state changing without its government also changing.

The study of the state raises separate issues from those the study of government does. Origins of states, whether they exist at all, and questions of state building and state transformation focus scholarly attention on historical and demographic processes as well as on internal social forces and power dynamics within a particular geographical space. Of equal importance must be attention to global factors, including trade, war, and colonialism. Corporations, parties, NGOs, and classes as well as government officials and citizens (or subjects) figure into the study of the state. The analysis of government—its size, organization, policies, and regime type—focuses our attention almost exclusively on interactions among actors in very specific roles, for example, legislators, executives, organized interests, citizens, ambassadors, who are influenced by economic pressures both within and outside the country's borders and by competing ideologies and groups. These actors are constrained by the institutions and norms of the state and by the relative bargaining power of other state agents, subjects, and enemies. They are embedded within the state but are not the totality of the state.

As scholars began to systematically investigate the role of the state as repressive or facilitative of political liberalization and economic growth, the emphasis tended to switch from the state, a relative abstraction and a complex set of variables, to the particular elements of the state that were doing the work: rulers, citizens, bureaucrats, legislators. The government is most definitely a compulsory political organization, and among its central roles are extraction in the form of money and people from its populous and regulation of the economy through the creation and protection of markets. Although attention may have shifted from the state to the government, we

shall soon see that a larger conception, which includes nongovernmental actors and societal norms, are a crucial part of the story.

■ | The State in Economic and Political Development

We know quite a bit about some of the roles the state plays in economic and political development although, as is evident by a quick glance at the world around us, there is no recipe book for success. In what follows, the emphasis is on the domestic responsibilities of the state and not on the part it plays in trade or war.⁴

First, the state is an enforcer of contracts. To be sure, markets can exist without states, but the kinds of markets we identify with advanced economies depend on stable government and the rule of law. What we do not know is when bureaucratic or juridical implementation is essential, and under what circumstances and in what forms coordinative (rather than cooperation or coercive) mechanisms will do the trick—although we are beginning to get some ideas (Carey 2000).

Second, the state is a provider of public goods that firms require but might not supply: standardized weights and measures, bridges, roads, education. Not all markets require state-provided infrastructure, but complex and sophisticated networks of trade certainly do. The production of these goods reduces the costs of exchange and enhances productivity by ensuring the capacity of its capitalists to locate, build up, and allocate physical capital and appropriately skilled labor.

Third, the state in the political economy provides social insurance. The debate over the welfare state is, of course, a debate over the extent of the state's responsibility for its unemployed, displaced, or poor.

We know that securing contracts, reducing the costs of exchange, and providing public goods involves the provision of information about potential trading partners and resources, education and other forms of human capital production, and the creation or protection of social relationships and networks that facilitate a variety of cooperative and exchange activities. Discussion of these issues will lead us to a possible fourth role of the state: encouraging civic-mindedness and civility within the polity.

While there is consensus on the roles of the state, or at least the first three, we do not know what it is most efficient and equitable for the state to provide directly and what the trade-offs are in having private provision, regulated or not by the state. There are lots of claims of knowledge about this, especially by neoliberal economists, but there is only beginning to be hard evidence. And some of what we think we know is now up for grabs. Perhaps this current period of neoliberalism, by creating a basis for compari-

4. See Kahler this volume for a discussion of more international issues involving the state.

son with past practices, will generate sufficient research to enhance our understanding of these issues.⁵

LEVIATHANS AND PREDATORY STATES

Part of why the state is in such bad odor among many political analysts and policymakers has to do with the extent to which it has proved a significant “. . . source of man-made economic decline” (North 1981, 20) and an inhibitor of democracy and equality. Given that the recent resurgence of interest in the state focused on problems such as revolution, taxation, and the inefficiencies created by state corruption, public ownership, and so on, it is not really surprising that the state becomes identified as the problem rather than the solution.

The perception of the state as a threat to liberty is embodied in the notion of the state as Leviathan. Hobbes used this term as a descriptor of the amassing of the population into one grand commonwealth: “This is the generation of that great Leviathan, or rather (to speak more reverently) of that *Mortall God*, to which wee owe under the *Immortall God*, our peace and defence” (Hobbes 1985 [1651], 227, Hobbes’s spelling and emphasis). According to the OECD, the term derives from two earlier usages: a mythical and gigantic aquatic sea creature and a man of enormous power and wealth. It has come to represent the idea of the state as huge in size, long in reach, and vast in power. While presumably beneficent, at least in Hobbes’s view, the Leviathan is meant to act in the interests of the aggregated populace whether individuals concur with its policies or not.

For Hobbes, the existence of the state enhances liberty by providing physical security of person and property. It also ensures economic prosperity by enforcing property rights and thus security of production and trade. But in its very formulation, the Leviathan is a nondemocratic, even anti-democratic institution. Who does and should control the state were and continue to be subjects of investigation and contestation. I have already rehearsed the outlines of important normative arguments, but the empirical question of what states do and under what conditions remains.

Models, particularly models derived from public choice and rational choice perspectives, have clarified the reasons why states may be inclined to rent seeking, overtaxation, and other so-called predatory practices. Rents are returns “in excess of a resource-owners opportunity costs” (Tollison 1982, 30), and rent seeking refers to behavior that generates social waste because of socially unproductive activity dedicated to winning licenses, official monopolies, or other economic rights. The work of Anne Krueger,

5. The model of what I have in mind is Waterbury’s book (1993) on the Egyptian state; Smith and Lipsky’s comparison (1993) of government versus private provision of social services in the United States; or Snyder’s investigation (2001) of deregulation in Mexico.

Robert Bates, and others (Krueger 1974; Bates 1981; Bates and Krueger 1993) provides evidence that rent seeking exists and is wasteful. And there is little doubt that government agents can be corrupt, using their control over economic resources as a means to enhance their personal wealth. Nor is there much question that rent seeking and corruption can prove highly inefficient for economic actors who must bribe and lobby to survive as well as for the economy as a whole.

By conceiving of government decision makers as rational actors, it is possible to see how such outcomes might arise. In my initial account of the state, rulers are always predatory in the sense that they will seek to maximize the revenues of the state, but their predation is subject to the constraints of their relative bargaining power, their transaction costs and their discount rates (1988, 10, *pass.*). Rulers who are relatively unconstrained, who are captured by particular sets of powerful actors, who are unable to control their agents, or who are faced with crises that require huge additional funds are likely to tax in ways that may damage long-term economic growth. The documentation of such rulers and policies is legion. Rulers may be personally efficient in terms of extending their length of office but disastrous for the economy as a whole or for the rights of groups within the polity.

Even so, as North (1981), Evans (1992), and many others have argued, the state may be the means of solving the very problems it creates. Adam Smith, more than 200 years ago, emphasized the need for very different state policies. What more contemporary scholars claim and have begun to investigate systematically are the conditions for transforming and constructing the institutional arrangements of a developmental state.

A GROWTH-ENHANCING AND DEMOCRATIC STATE

As destructive as states can be of well-being, economic and political, they are also “essential for economic growth” (North 1981, 20) and can facilitate democratization. The contribution of an effective state is more than defense against the Hobbesian “war of all against all.” It is also, as is universally accepted, the production of public goods that enhance physical and human capital. But infrastructure and a well-educated polity are not sufficient conditions for political and economic development. Increasingly, it is becoming apparent that also essential are an array of social relationships and norms that serve as resources for individual activity—what some analysts label social capital. The question here is the extent to which government activity facilitates social capital that then makes not only the market but also the government and society work better.

The argument that a centralized state destroys the social cohesion and social networks of traditional communities and thus undermines the potential for cooperation is a claim with a distinguished pedigree (see, e.g., Scott 1976; Taylor 1982; Gellner 1988; Ostrom 1990). But centralized states may

also help build social networks and relationships of trust and authority that may serve the populace well. It can be key in providing the resources and targets for facilitating the growth of organizations and voluntary associations that generate a flourishing civil society. For example, the creation of the U.S. post office was not only essential for commerce and savings (Carpenter 2001); it was also a critical factor in building and sustaining the potential for civic engagement in the very period that Tocqueville observed (Skocpol 1997, 459–63). This institution of central government made possible social, political, and economic relationships across time and space by cementing information exchange by building canals, railroads, ports, and roads and through the circulation of newspapers, documents, and people. Kevin Costner's film *The Postman* may have been off the money but on the mark.

There is a considerable literature on how national states create the "opportunity structure" and other resources that enable groups to engage in collective action, sometimes in opposition to the state, sometimes in its support, and sometimes to achieve its own ends vis-à-vis other societal actors.⁶ In her presidential address to the Social Science History Association, Theda Skocpol argued that "the early national U.S. State created a framework that encouraged widespread voluntary association," (1997, 476) and she traces the evolution of that framework as well as the state-society symbiosis that led to translocal organizations capable of active participation in national policymaking. She limits her analysis to the United States, but the underlying point is relevant to a wide range of societies and eras (see, e.g., McAdam, Tarrow, and Tilly 2001).

There are numerous examples that make this point, but here I will offer up a few illustrations of how state policy provides an opportunity structure for citizen action networks, even when that may not be the intention. Central and colonizing states may well have transformed linguistically, religiously, and racially distinct groups into minorities, but such states also offer a new basis for power relationships and definitions of ethnic status and languages (e.g., Hechter 1975, 2000; Laitin 1992; Laitin et al. 1994). The antipoverty programs of President Lyndon Johnson did, at least for a time, offer resources to poor people who then mobilized to change local governmental policies (Bachrach and Baratz 1980). Sidney Tarrow (2000), a scholar closely associated with the theory of contentious politics, provides a compelling account of how the very rules that states establish may enable and legitimize citizen action and aid the formation of networks of activists, as happened around the Mad Cow disease controversy in Europe. Several authors (Rothstein 1990, 1992, 39–46; Western 1997) have documented how variations in the administration of public unemployment insurance affect unionization; in those countries where the scheme is administered by unions, union density is higher.

6. For a useful review of this concept and of the social movement literature generally, see McAdam, Tarrow, and Tilly 1997.

We may today question the positive aspects of patronage, which is often a form of corruption and cronyism, but it is also, as Robert Merton (1949) so astutely observed, a means of creating both human capital and social capital (although he used neither term) in the form of a network that linked government actors to neighborhood, especially ethnic, residents. Robert Michels (1962 [1919], 185–89), even longer ago than Merton, noted how the state provides jobs that buy off discontent by creating bonds of dependency and loyalty as well as by offering a livelihood. There is some evidence for both of these propositions and considerable room for pinning them down even further.

Attention to the civil norms that facilitate law-abidingness and civic engagement need not be insulated from attention to the way states help produce as well as benefit from such norms. I am following Ensminger and Knight (1997, 2) in delineating social norms as “informal rules that structure behavior in ways that allow individuals to gain the benefits of collective action.” There are numerous examples of ways in which the establishment of certain government laws and policies promote norms which then change the way people think and which also promote collectively beneficial outcomes. Changes in thinking about slavery (Engerman 1997) clearly had extrastate sources, but the resulting changes in the law seemed to have an effect of changing peoples’ conception of slavery over time. I find a similar process in my own work (1997a, 103–6, 205–8, 211) on the evolution of conscription practices and the search for a more equitable draft; I find that changes in conscription practices reflect but also alter what constitutes the perception of a fair and equitable draft. Bo Rothstein (1998) analyzes how the creation of universalistic welfare policies at an earlier period in Swedish history continues to affect the beliefs of Swedes about what is fair and thus the kinds of legislation they will accept and which can be implemented at relatively low cost. Finally, in a quite different realm, Robert Keohane (1999, 236–37) explores how government bureaucracies help generate the creation of transnational networks that become the carriers of ideas that secure international cooperation.

As important as the social networks, organizations, and norms a state nourishes are the relationships of trust it cultivates.⁷ Laws, enforced by the state, provide insurance and sanctions against illegal opportunism; trust, on the other hand, lubricates both exchange and governance. Interpersonal trust is essential to economic growth because it facilitates the making of contracts, reduces the costs of exchange, and eases renegotiation when the situation changes. Tocqueville noted how important trust is to business relationships (1990 [1835], 387–90), and many subsequently have emphasized the importance of trust in the development of complex capitalist economies. Only a few (e.g., Fukuyama 1995), however, have recognized

7. This section draws heavily from my “A State of Trust” (1998). Also see my *Consent, Dissent, and Patriotism* (1997a).

the major role the state plays in creating the kinds of trust that lead to both better government and a more productive economy.

States can also help to produce interpersonal trust and reduce transaction costs through regulations directed at reassuring consumers that they are getting what they believe they are paying for and that they will be safeguarded against a wide variety of human-created dangers. Technically these regulations often constitute a form of contract enforcement, and abuses by the seller are subject to the courts. However, the effect is a kind of generalized trust or, more correctly, confidence in the market and a greater willingness to engage in productive trade. Whether the public is aware of it or not, it is government regulations that provide the backdrop to consumer willingness to give out credit card numbers over the phone, use ATMs, get on and off airplanes, even buy property. Those countries with less trustworthy and transparent governments tend to have significantly higher transaction costs of exchange and dead weight loss, as empirical studies are beginning to demonstrate (see, e.g., DeSoto 1989; Campos and Root 1996).

The ability of a state to generate interpersonal trust may rest largely on the trustworthiness of the state itself. What trustworthiness means in this context is the extent to which its decision making, policies, and implementation procedures are considered fair and nondiscriminatory by the standards of the era, but it also and even more centrally means that government actors credibly convey that they will act in the interests of the citizens they are meant to serve.⁸ Thus, widespread incompetence or corruption is usually an indicator of untrustworthy state agents, and so, too, generally is behavior that favors a particular class, racial, ethnic, linguistic, or racial group.

One of the major findings of my research on the state (1988, 1997a, 1997b) is that cost-effective means to extract revenue and men depend on low-cost mechanisms for increasing compliance among taxpayers and that even the most autocratic rulers depend on a certain amount of quasi-voluntary compliance. The creation of quasi-voluntary compliance, let alone consent, requires a trustworthy government, in particular one that meets current standards of fairness in both its decision making and policy implementation. Citizens and subjects are in a kind of tit-for-tat arrangement; each cooperates as long as there is some assurance that the others will.

The implication is that certain kinds of states are likely to be better able to extract resources, in money and men, from their populations. There are two necessary conditions. The first is sufficient capacity to penetrate deep in the society, a capacity that rests on the power of the state to pacify the countryside and to build an adequate bureaucratic apparatus. The second is the existence of a government, which may be more demo-

8. This conceptualization of a trustworthy state comes largely from my own work (1997a, 1998) but also from that of Russell Hardin (2002).

cratic but certainly is more likely to be relatively consensual and fairer in the way it inaugurates and implements policies. At the least it will be more credible (Whiting 2000). But the implication is also that, without trustworthy government, the polity will feel betrayed and will be more likely to resist or rebel. Indications of considerable citizen free riding that government could but is not controlling are likely to provoke additional citizen non-compliance. So, too, is evidence of discriminatory government practices, violations of policy bargains, or poor bureaucratic treatment of citizens. States enforce rights and rules other than those associated with economic and material property contracts. By protecting minority rights, states facilitate cooperation among individuals who have reason to be wary of each other. By legalizing trade unions or enforcing child labor laws, states reduce the costs to workers of monitoring and sanctioning employers, and thus may raise the likelihood of both trust and productivity.

The amount of socially and economically productive cooperation in the society affects, in turn, the state's capacity to govern and the vitality of economic exchange. Confidence in the trustworthiness of the state has additional consequences for governance as well. It affects the level of citizen tolerance of the regime and also the degree of compliance with governmental demands and regulations.⁹ Destruction of the belief in the state's trustworthiness may lead to widespread antagonism to government policy and even active resistance, and it may be one source of increased social distrust. Research on eastern Europe adds additional credence to this claim (see, e.g., Rose 1994; Mishler and Rose 1997; Sztompka 1999). The effect can be the breakdown in state capacity, even where there is strong governmental infrastructure.

Once confidence in the trustworthiness of the state has been destroyed, its rebuilding often requires extraordinary efforts, as we are learning on the ground in numerous parts of the world. In fact, the current moment in history provides a remarkable laboratory for understanding state destruction, transformation, and rebuilding. There are numerous scholars right now producing the in-depth research that will provide a basis for generating and testing hypotheses and contrasting their explanations with the alternatives.¹⁰ And many of them believe in and are committed to cumulative knowledge.

9. This point is increasingly well documented by scholars using both quantitative and qualitative methods. See, e.g., Tyler (1990, 1998), Peel (1995), Levi (1997a), Scholz (1998), Rothstein (1998).

10. Any list I provide will neglect important work of which I am not aware or have simply forgotten to include. So, let me just list some of the relevant recent and forthcoming books in the Cambridge Studies in Comparative Politics series: Kitschelt et al. 1999, Bunce 1999, Easter 2000, Migdal 2001, Stokes 2001a, Stokes 2001b, Snyder 2001, Kreppel 2001, Beissinger 2001, Murillo 2001, Jones Luong 2002, Kang 2002, Grzymala-Busse 2002, Franzese 2002, Lehoucq and Molina forthcoming.

At one level, in fact, we know quite a lot about this process. Here is where the historical and sociological literature on the origins and development of states really comes into its own, as well as the work of the quite considerable number of comparativists who have been tilling the fields for some time. But despite all the detailed accounts and despite considerable advances in understanding the pathways to any particular configuration under study, we still lack an adequate dynamic understanding of how to resolve issues of corruption, improve poverty, and ensure the well-being of populations by means of the state or, for that matter, by other means. But, as I said earlier in this essay, I am a guarded optimist.

STATE-PRODUCED DEFICITS IN TRUST AND DEMOCRACY

A growth-enhancing state is not, even if achieved, a sufficient condition for political democracy. A centralized state does not necessarily ensure against descent into political violence and economic dissolution, nor is it the only or even best way to promote positive economic and political cooperation at the local level. Nor, for that matter, does the reduction in the size and interventions of government necessarily enhance democracy.

There is a huge literature on democracy, its origins, fragility, and sustainability, and comparativists have made considerable progress in understanding certain features of democratization as well as in acknowledging the need to analyze the particularities of each case (see Bunce 2000 for an excellent review). According to Bunce, there are at least two things we know about the relationship between the state and the survival of democracy. First, the maintenance of democracy generally requires “rough agreement on the composition of the nation and boundaries of the state” (2000, 712). Second, “A strong state, in short, is a guarantor of democracy—much as it is a guarantor of capitalism” (715). Democracy has a chance of stability only where there is a stable rule of law.

But even with a strong state and democratic institutions in place, democracy can come under pressure. This can occur as a result of a reduction in democratic accountability, state capture by particular interests who do not serve the general welfare, and failures of government to deliver what the polity believes it wants and deserves. The analysis of the state as composed of actors with goals that are constrained by and respond to trustworthy governmental institutions begins to get at the answer to several puzzling issues in contemporary social science: why democracies tend to be more productive of economic growth than authoritarian regimes, and why we are witnessing what appears to be an increase in citizen skepticism about government and politics in the established democracies.¹¹

Mancur Olson (1993) argues that the more encompassing the interest

11. There is, of course, some serious question as to whether they really are. See, especially, Przeworski and Limongi 1993; Przeworski et al. 2000.

of the ruler in the polity, the more likely the tax system will provide the proper incentives for growth and the rulers will provide public services in return for their extractions. An alternative, more-compelling, and better-substantiated view is that sophisticated rulers and elites create or take advantage of political institutions, such as parliaments, that not only facilitate bargaining over taxes but also provide a means to sanction rulers who renege on agreements; in other words, such institutional arrangements make the promises of rulers credible (Levi 1988, 117–21; Root 1989; Brewer 1989; North and Weingast 1989; Ertman 1997, 221). Credible commitments and self-enforcing institutions significantly reduce the citizen's need to make a personal investment in sanctioning and monitoring state agents and thus enhance citizen trust of state and government. This is, of course, theorizing that permits explanations of specific actions. It lacks the grandeur or sweep of the Olson conjectures, but it is observable and falsifiable.

There is little question but that what Scharpf (1999) calls the “input-oriented mechanisms” are under considerable pressure as a result of global market forces, the creation of regional government in Europe, and the scope of government just about everywhere. Increasingly, important sources of power reside in private organizations or in nonmajoritarian organs of the state. The problem is not as Karen Remmer (1997, 52) and others have argued that “the activities, resources, and relative weight of the state are being reduced.” But rather, as Peter Evans argued, “The danger is not that states will end up as marginal institutions but that meaner, more repressive ways of organizing the state's role will be accepted as the only way of avoiding the collapse of public institutions” (1997, 64, in the article immediately following Remmer's in *World Politics*). The question of the effects of the changing locus of power and policies of the state deserves the theoretically informed empirical investigation that is beginning to occur.

But there are some things we know already. We know that the state is often the captive of economic pressures—international and domestic—and powerful societal actors that influence the range of options available to state actors. The Marxists long asserted this proposition, and it is part of the canon of scholars of development. Only fairly recently, however, have scholars disaggregated and tested key components of this claim. Przeworski and Wallerstein (1988; also see Przeworski 1985, 1991) helped initiate a whole spate of sophisticated formal and quantitative work on the structural dependence of the state on capital. Work on tariffs and cartels by economic historians, comparativists, and IPE scholars are beginning to reveal some determinant conclusions about the interplay between domestic and international factors (see, e.g., Bates 1997b; Moravcsik 1998). Many of the authors of recent research on the social policies and economic regulation within the changing global and regional economies are engaged in real debate about the role of globalization, the effects of partisan politics, and the influence of labor unions and corporations on the state. They are test-

ing and falsifying each others' hypotheses while also offering alternative accounts (see, e.g., Garrett 1998b; Boix 1998; Iversen 1999; Scharpf and Schmidt 2000b; Swank 2002; Hall and Soskice forthcoming).

But it is not only the lack of popular input or the inordinate influence of business or corporate interests that accounts for the democratic deficit. There are also real problems of "output oriented legitimization" (Scharpf 1999). Part of the story lies in the reduction of welfare or other social benefits that characterized so many European governments, particularly the social democratic governments, of the post-World War II era. Part of the story is also the increased costs of governance combined with politically driven reductions in tax income that make it extremely difficult to pay for what citizens want and need. So, we have a new version of the "fiscal crisis of the state."

Of equal or perhaps greater significance in "output legitimization" is the fact that national governments must find ways and rules that inhibit "the race to the bottom" and convince citizens to share in the commitment to this new vision of the collective good.¹² This is no easy task.

■ | Methodological and Conceptual Issues

To maintain a program of research about the state is one thing, and there are still many attempting to do just that. To improve the quality of that research is quite another. The state shares many characteristics with the elephant so misdescribed by the blind men of Chelm. It is vast yet full of parts whose connections only make sense if you can grasp the whole—difficult to do if your perspective permits a view only of certain elements. The problem intensifies if you are interested in the origins of the state or its likely future, let alone its institutional design or best practice.

My argument is that the strategy of the men of Chelm is wrong. As we know from other stories about them, their real deficiency is the lack of reason, not sight. The state is too complex and too varied to grasp the whole, even if it were possible to offer a static picture of the exterior of the beast with all its peculiarities and particularities. Some scholars react by providing rich and detailed accounts of state processes and governments, and others offer grand theories derived from either wide reading or formal logic. We must go beyond thick descriptions of specific states at specific times to develop models and falsifiable hypotheses derived from realistic and logical presuppositions about the state and the interactions of its agents with each other and with the larger society. Good theory requires understanding the relationships between the parts, how they connect. At best, we can develop only partial theories, which will increase our under-

12. I am loosely paraphrasing and strongly building on Scharpf (1999).

standing and leverage. I concur with Barbara Geddes (her paper for this volume) when she argues, "The tradition that most impedes the development of a body of knowledge in comparative politics is . . . our selection of big, inadequately defined outcomes to explain."¹³ Although my long-term goal is to further our capacity to understand the large outcomes of political and economic development, the approach I take is to consider pieces of the puzzle, model the situation as best I can through a combination of rationalist deduction and inductive investigation, and use comparative statics to produce falsifiable hypotheses, which are then tested against empirical evidence.

I am also arguing for generating testable hypotheses from models that may have high predictive power but that ". . . are likely to be of limited scope and will only represent certain subsets of the complex multiarena and multilevel interactions that are characteristic of real world processes" (Scharpf 1997, 31; also, see Moravcsik 1998a, 17, pass.; Bates et al. 1998). This turns much of what has been claimed about rationalist theory on its head, especially its pretention to universalism. The universalism rests on the assumptions of rationality or the general theory of interest, not in the models of particular interactions or the findings about them.

What I have just said suggests that a lot of the debate about state origins and impact will remain unresolved because the scope of the enterprise is too large. There is another reason as well. The adjudication of theoretical claims, be they deductively or inductively derived, involves getting down to the nitty-gritty of data collection and hypothesis testing. The hypotheses must, of course, be derived logically, and the assumptions must be defensible. It is more than appropriate to criticize failures on either of these dimensions, and, of course, theories can be superseded by alternatives that account for more of the variation. Yet, despite the detailed evidence often gleaned and the rich narratives offered, many authors do not in fact derive and test hypotheses. Rather, we have witnessed ". . . the multiplication of plausible claims. . . . Where hypotheses are rarely discarded, they are rarely confirmed" (Moravcsik 1998a, 17; also see Remmer 1997).¹⁴

But another question immediately arises: How do we most effectively go about deriving these hypotheses? Here there are a myriad of strategies, many of which are equally productive of good model building in the service of cumulative knowledge. I shall outline mine (shared by others) with the recognition that it is hardly the only possible route, although, of course, I believe it is the best.

13. As John Carey will no doubt realize, I benefited immensely from his comments on my earlier description of appropriate methodology.

14. Now, I should add, with the possible exception of legislative research in the United States, there are probably no areas of political science that have met these criteria.

Whether the subject is the government or the state, three similar analytic problems arise. The first is that state and government institutions, are, as Stinchcombe (1997, 1) puts it, “staffed.” Even if one accepts, as I do, the definition of institutions as rules of the game as opposed to “the process by which organizations and procedures acquire value and stability” (Huntington 1965, 394), someone still has got to see to it that the job gets done. There are individuals involved who are making purposive decisions and upholding values as well as interests. Stinchcombe is quite right when he critiques many of the “new economic institutionalists” for neglecting the role of individuals in their accounts just as the rationalist state theorists were correct to take the structuralists to task for treating states as depersonalized.¹⁵

The second major analytic point is that it is inappropriate to treat the state as a unitary actor. There can, of course, be spokespersons who reflect the aggregated preferences of the key players in the state or government apparatus. It is not enough, however, to disaggregate to the component organizational parts; it is essential to provide a microfoundation based on individuals and groups. This brings us immediately into the world of preferences, motivations, collective action, and coordination problems.

The third and final point is it is essential to understand state actors as persons responsive to other domestic or international players and sources of pressure, as actors engaged in strategic interaction. There can be states and their representatives more autonomous from society or more powerful internationally than others. But the best extant theories of the state take seriously these factors and model the strategic interactions among state actors, between state and societal actors, and among the representatives of various states. In the process, they bring to bear the role of institutions, transaction costs, collective action, contentious politics, and social movements.

Note I have not yet said anything about rational actors, although that is clearly an assumption in virtually all the most-interesting theories of the state. However, not all such theories are by rational-choice scholars. Thelen (1999) emphasizes the “creative borrowing” and the learning that has gone on between rationalists and historical institutionalists. Many of the latter now explicitly discuss collective action and agency problems; they have also learned that accepting actors as rational does not preclude them from also presuming that actors are ideological, emotional, altruistic, and so on. Rationalists, meanwhile, are learning that the recognition of the

15. Stinchcombe’s other major point—and it is one of Migdal’s (1994) as well—is the importance of values. Yet, part of what the state is all about, at least for many state theorists, is gaining authority and credibility and making coordination possible. Parchment and constitutions are good because they coordinate members of the polity around certain norms, values, and rules (see, e.g., Carey 2000; Hardin 1999). So part of this debate is more terminological than real.

role of ideas and values does not preclude rationality in the form of strategic or self-interested behavior, and they are taking on board interpretive materials. Historical institutionalists tend to embed actors in frameworks of meaning and provide them with nuanced identities that affect their preferences; this is only beginning to happen among rationalists (Bates et al. 1998).

Over time, as several scholars have noted (Hall and Taylor 1996; Levi 1997b; Thelen 1999) there has been some convergence in the research programs and tools of analysis of what were once extremely distinct and competitive approaches to institutions and the state. For some the major difference between these two approaches is “the relative centrality of ‘equilibrium order’ versus ‘historical process’ in the analysis of political phenomena” (Thelen 1999, 381; also see Orren and Skowronek 1994). Undoubtedly, those within the historical institutionalist school are more likely to be interested in instances of state transformation, revolution, and civil war; with how preferences arise and are altered; and with dynamic explanations. Undoubtedly, rationalists tend to be more concerned with stability, to take preferences as given, and to rely on comparative statics. But even this distinction is breaking down.

There is, nonetheless, a theoretical and methodological divide that continues to be the subject of symposia and debate.¹⁶ What matters for my purposes here, however, is not the arguments among the state theorists and the varieties of institutionalists but what we have collectively learned that informs the burning research and policy questions we currently face.

■ | Conclusion

We come full circle. In the '70s and '80s, a new generation of social scientists argued for “bringing the state back in.” In this era of “taking the state out,” the mantra carries considerable appeal, at least to me. But with some provisos. The state needs to be constituted or reconstituted in those countries operating without the rule of law and institutional or, perhaps as Stinchcombe (1997) might have it, institutionalized mechanisms of coordination. In those countries with functioning states, there must be some attention to the democratic deficit produced by the reorganization of state responsibilities. What has changed most markedly is the role of government and the public sphere. Government, properly instituted, is a major impetus to economic growth, political development, and collective goods.

16. For example, see Kiser and Hechter 1991 and the subsequent 1998 “Symposium on Historical Sociology and Rational Choice Theory” in the *American Journal of Sociology*, with articles by Somers, Kiser and Hechter, Boudon, Goldstone, and Calhoun. Also see the symposium on *Analytic Narratives in Social Science History* (2000) with articles by Carpenter, Skocpol, Parikh, and Bates et al.

Government, badly instituted, is a major font of poor economic performance, elitist privilege, and social waste. The antigovernment ideology embodied in neoliberalism may prove an antidote to reliance on the state for inappropriate tasks and may prove a corrective to inefficient and inappropriately costly regulations. However, the effect of this ideology has been to obscure the important roles the state can and often does play in promoting a productive economy and a participatory polity. These are roles the state plays not only historically or in developing countries but also in the most-advanced industrial and democratic countries in the contemporary world. And these are roles the state can and should continue to play.

But I do not want to end on a normative note. Although I think the normative issues are critical, no normative program can be effected without real understanding of what is actually happening, why it is happening, and what is possible. I have followed Nettl in claiming that the state goes in and out of fashion as a social science construct due to the combination of the problems encountered in using the concept in rigorous analysis and the extent to which the state seems to be a factor in what concerns the contemporary world. Nettl, for example, argues that the loss of interest in the state reflected the combination of the growing influence of American social science (1968, 560) and the growing importance of third world countries (1968, 560). A concept like nation-state was not applicable, and the concept of the state had almost as little meaning. It was neither adequate as a descriptor of processes taking place in those countries nor useful in theory building about those processes. For Nettl geography is determinative. In my view, the failure of scholars to develop good methodologies and theories leads practitioners to search for alternative concepts as the basis for explanations and policy.

What I have offered, I hope, is an outline of the research that still needs to be done based on an assessment of what we, as scholars, have accomplished thus far. Despite the multicentury literature on the state, despite all the thinking and writing that has been addressed to the consequences—positive and negative—of state intervention in the economy and of the problems of democratic accountability, state theory remains in its infancy. The blind men of Chelm looked only at the exterior. We have begun to delve into the interior, to understand how the pieces fit together, what are the sources of change and dynamism, and what makes for a healthy or diseased state. However, the tools of investigation and the body of careful case research necessary for adequate theory building and testing are only just becoming available. By combining an interdisciplinary perspective with adequate attention to the variations and specificities of state actions and effects, there is hope that we shall untangle the complex relationships within the state and among the state, government, and civil society.

MILES KAHLER

*The State of the State in World Politics*¹

The career of the state in international relations research has been a peculiar one: central as a matter of faith but often taken for granted, a theoretical sideshow. That core belief in the central role of sovereign states in world politics has been repeatedly challenged during the twentieth century. World War II—a monumental clash of militarized states—paradoxically brought a wave of doubt about the state system. With typical prescience, E. H. Carr (1941) questioned the untrammelled application of the principle of self-determination, pointing out that military and economic trends called for larger units in world politics, not a host of small ones. He was largely wrong on the military threats to small or weak states in the postwar decades, but he clearly framed the looming conflict between economic interdependence and political autonomy. A decade later, John Herz predicted the end of the conventional territorial state as its “hard shell” of defensibility was stripped away by the instruments of total war, particularly nuclear weapons. He later retracted this stark prediction in the wake of decolonization and the birth of dozens of new states. Like many others in the 1960s, however he pointed out the “synthetic” quality of these states and argued for an international role in their “hardening” over time (Herz 1976, 114–18, 242–43).

These lines of inquiry were set aside as a long peace among the great powers persisted and economic integration among the industrialized countries deepened. The research agenda of international political economy initially questioned the unitary and coherent internal ordering of the state. State-society relations figured prominently as a determinant of foreign economic policy; multiple transnational channels connected societies

1. The author wishes to thank Robert O. Keohane, Kathryn Sikkink, Jeffrey Frieden, Margaret Levi, David Lake, the editors of this volume, and the participants in the State of the Discipline conference for comments on earlier drafts of this paper. The author also acknowledges the valuable research assistance provided by Pablo M. Pinto.

(Katzenstein 1978; Keohane and Nye 1977, 24–25). Its emphasis on the roots of state policy in society was subsumed by a call to “bring the state back in” during the 1980s, however. The distinction between strong and weak states dominated other variation on the state-society dimension.² Competition from other international actors faded, in theoretical debates at least, and the agenda of complex interdependence was set aside (Keohane and Nye 1987). By the end of the 1980s, the state as an unproblematic, uniform, unitary, and rational actor seemed to satisfy conventional realists and neorealists (who saw homogeneity imposed by international competition) as well as those of a more game-theoretic and rationalist bent.³ Even those sharply critical of the individualist assumptions of neoliberalism and neorealism accepted that “states are the principal actors in the system” and defined states in a way that was completely familiar (Wendt 1996, 1999).

Since the importance of the state was taken as axiomatic, a careful definition of statehood was seldom offered. Nonetheless, the outlines of the ideal-typical state in world politics were clear if not always explicit. States were territorial: they claimed to rule over a carefully delimited space. Within that space they were sovereign: they reserved for themselves core political functions of internal order and external defense (war making)⁴ The result was a system of “defensible units, internally pacified and hard-shell-rimmed” (Herz 1976, 111). These material characteristics could be defined as a normative identity as well, framed by mutual recognition, sovereignty, and territoriality (Van Creveld 1999, 1, 416). The state was “the basic, irreducible unit” of international relations, “equivalent to the individual person in a society” (Nettl 1968, 563).

The somnolent theoretical world of the state exploded in the 1990s. The comfortable bipolarity of the cold war ended in the collapse of one superpower state. Old preoccupations with interstate war (military and economic) dissolved into an awareness of widespread violent internal conflicts and the encroachments of globalization. Predictably, although many of these developments were hardly novel, they quickly produced one last twentieth-century wave of proclamations that the state was endangered, in decline, or eclipsed.⁵ In the earlier alarms described, the principal threat to the state was modern military technology. In the 1990s, collapsing internal capabilities and global economic integration were the designated threats.

2. This distinction in the U.S. case pervades Ikenberry, Lake, and Mastanduno 1989, for example.

3. In the debate between neoliberals and neorealists, both sides accepted without question this prevailing consensus (Baldwin 1993). Reasons for this convergence are discussed in Kahler 1997.

4. On the definition of the state, compare Waltz 1979, Van Creveld 1999, 1, and Wendt 1999, 201–14.

5. See Guéhenno 1995; Van Creveld 1999; Evans 1999.

The international environment, which had propped up the state, now seemed less supportive and possibly subversive of its role (Evans 1997, 65).

These alarms also paralleled and in some measure stimulated a wave of serious scholarly attention to the state in international politics. The new research was diverse, a marriage of different theoretical and methodological interests. These agendas intersected on one point: the state had for too long been taken as a constant in the study of world politics; it was past time to consider it as a variable. Like any good marriage, this one contained something old, something new, and something borrowed.

The *old*, or at least familiar, was the initial agenda produced by the “new” phenomenon of globalization. Economic integration at the global level (a narrow definition of globalization) had been underway among the industrialized societies for decades. The characteristics and scope of integration and the political issues it raised have changed over time, but research on globalization recast an earlier agenda in international political economy, when sovereignty also appeared to be at bay. Attention centered on decline in territorial control by the state—the rise of dense cross-border channels for economic, political, and cultural exchange—and the resultant competition with other economic and political actors, international and domestic, that were labeled international civil society. The state’s *centrality* was challenged and, in some cases, its *capabilities* as an autonomous agent.

A *new* research agenda on the state in world politics is based on a growing awareness that the state has too often been defined as the modern European state, a species delimited in both time and space. Our familiar ideal type emerged from competition with other units organized very differently. As a description of units, it is hardly characteristic of many of the polities labeled states in the contemporary world. Governance functions are concentrated in the nation-state, delegated to international institutions, and devolved to smaller units within the state in different regional patterns. *Variation* among political units and in the relations among them defines this new attention to the units in international politics. The norm is no longer assumed to be the modern European state in one type of anarchic setting. This line of research is the most subversive of the conventional view of the state and the most significant for political science.

Finally, students of world politics have also *borrowed* in their renewed investigation of the state. Institutional analysis, absorbed from the study of American and comparative politics, has played two roles. Treating the state as a composite of institutions allows a more refined analysis of how states and other political units vary and a more accurate estimation of the consequences of that variation for foreign policy and international politics. Institutional analysis has also offered common methodologies for examining political actors of different kinds, undermining the assumption that international anarchy draws a sharp line between domestic and international politics. One can examine the state through a lens that permits its compar-

ison with other political units. This third avenue of research decomposes the *unitary* character of the state and considers the consequences of different institutional combinations.

Each of these old, new, and borrowed elements is considered in the following sections. None has yet produced a coherent research agenda on the state in world politics. Each could be aligned productively with other ongoing research in the discipline. Whether this new interest in the oldest concept in international relations will persist is hardly certain in a faddish field. What follows should confirm, however, that the questioning itself has reopened and refreshed a once-familiar subject.

■ | Globalization and the State

Growing economic interdependence in the 1960s directed attention to many of the same political outcomes that are prominent in the contemporary globalization debate: the growing power of nonstate actors and the emergence of political networks and coalitions that cross national boundaries. The central concerns of those analyzing interdependence also differed from those evaluating the effects of globalization, however. For those writing in the shadow of the 1930s and the realities of the 1970s, the consequences of interdependence for interstate cooperation were most important. Maintaining regimes of economic openness was a primary goal in the face of growing spillovers that hampered policy autonomy and created political conflict (Cooper 1968). Others emphasized the effects of interdependence on interstate bargaining through increased vulnerability to the policies of other states (Keohane and Nye 1977). Policymaking within and between states had changed, but the state itself was not called into question.

Political scientists have developed a successful research agenda on the political effects of globalization, but they have not emphasized the effects of globalization on the state or a particular configuration of political institutions. Instead, researchers have treated globalization as either a source of new political and sectoral *coalitions* under conditions of increasing economic integration or a *constraint* on government policies. Effects on national policy autonomy have been found to vary with the size of the economy, which is associated with economic openness; issue area (for example, monetary policy versus industrial policy); and other concurrent policy choices (for example, adopting a fixed exchange rate under conditions of international capital mobility) (Garrett 1998a, b; Frieden 1991b; Mosley 2000). Those who examine the effects of internationalization on domestic coalitions and policy outcomes often accept intermediation by domestic political institutions as an important explanation for national variation. They have treated the state and other domestic political institutions as sig-

nificant intervening variables that offset or amplify the effects of internationalization (Keohane and Milner 1996). The effects of globalization on those institutions have received less attention.

Claims that globalization has direct effects on the state and its role in world politics are often based on a definition of globalization that incorporates cultural, environmental, and social networks, as well as economic interdependence (Keohane and Nye 2000). Even defined in more narrowly economic terms, contemporary global economic integration may be substantially different from and, on several dimensions, deeper than earlier periods of economic openness (Baldwin and Martin 1999; Bordo, Eichengreen, and Irwin 1999). Underlying many of these differences is technological innovation: large and continuing reductions in the costs of cross-border communications, which promote cross-border financial flows, facilitate international production networks, and underpin the emergence of new forms of transnational political organization.

These novel features of the contemporary international economy could in turn threaten or transform the role of the state in ways that earlier interdependence did not. Interstate conflict that resulted from growing economic interdependence drove the older agenda of international political economy. The anxieties surrounding globalization emphasize potential conflicts between democratic accountability—located in the state or its subordinate political units—and private actors, particularly economic agents, whose bargaining leverage in domestic political systems has been inflated. The bargaining power of these private actors—particularly mobile capital or multinational corporations—is assumed to increase because they have a more credible exit option from the confines of a single state. Exit is eased in part by technology, in part by the adoption of investor-friendly policies by other states. Observed mobility is not required for the political effects on the state that have been claimed for globalization; a credible threat of exit is sufficient to shift the balance between state and private economic actors.

The logic of expanded transnational political action is similar. Although the exit option is less significant, democratization, coupled with more open borders and lower communication costs, produces possibilities for political arbitrage or leverage that did not exist in the past. Margaret Keck and Kathryn Sikkink describe the strategies of transnational advocacy networks and a “boomerang pattern” that capitalizes on these developments to increase the influence of nongovernmental organizations that have international allies (Keck and Sikkink 1998).

This assumption of a shift in bargaining power between states and nonstate actors leads those pessimistic about the effects of globalization on the state to emphasize declining state capabilities, whatever the national policies pursued. Capabilities could be undermined through several avenues: downward pressure on state revenues, technological disadvantages

for hierarchical organizations, a loosening of the identities that link citizens to states, and a reduction in state scale.

Globalization may reduce state capabilities by placing downward pressure on revenues. As taxable economic actors become more mobile and shop for low-tax jurisdictions, the resource base of the state could decline. Taxes on mobile capital are likely to become ineffective. As highly paid individuals become more mobile and more capable of constructing international strategies of tax avoidance, the income tax base of the state could be further eroded. A more-refined view of the effects of globalization suggests that overall revenues need not decline but that the incidence of taxation is likely to shift to less mobile factors, such as labor and more regressive forms of indirect taxation. These distributional effects, rather than a necessary decline in total revenues, create a political limit on state revenues. Revenues may also be shifted toward subnational or local governments, since their tax base would be less affected by capital and labor mobility. If such a shift occurred, it could also affect the internal political balance between levels of government (Rodrik 1997; Tanzi 1998).

Technological innovation and diffusion have lain behind the economic transformations of globalization. Those information and communications technologies may have an independent effect on states and other hierarchical organizations by supporting networked organizations in economics and politics, organizations that are “neither hierarchical nor reducible to market relations” (Keck and Sikkink 1998, 200). Globalization and its technologies, a world of markets and networks, is interpreted as hostile to the hierarchical world of the state and its bureaucratic mode of organization.⁶ New transnational organizations, whether economic or political, use the new network technologies far more effectively than traditional states or corporations. Thus, technologies can appear to have an affinity to the competitors of states, directly reinforcing the second-order effects described earlier.

In one popular view, globalization’s homogenizing effects on national and local cultures, like the exit options that it provides for key political actors, undermine an important tie that binds states and populations. Both advanced industrial societies, such as Canada and France, and many developing countries portray large-scale, unrestricted trade in cultural products as incompatible with indigenous national cultures. More directly, a renewal of large-scale migration (although substantially smaller than in the pre-1914 era) calls into question existing notions of citizenship and conceptions of national identity (M. Weiner 1995).

Finally, theoretical arguments suggest that economic openness renders large-scale political units unnecessary: as barriers to trade and investment decline globally, the advantages of a large internal market can be

6. On the relationship of networks and markets, see Rauch and Casella 2001.

captured directly through participation in the world economy (Alesina and Spolaore 1997). A similar argument underlies Richard Rosecrance's "virtual state" (1999), in which territorial rule is replaced by economic access. Such pressures, even if unmatched by other countervailing trends, would only produce more states of smaller size rather than units that are different in kind. The historical record also undermines the claim of an association between smaller scale in political units and economic openness: in the decades before 1914, the last period of global economic openness, large-scale units dominated world politics.

The preferences and behavior of nonstate transnational actors over time, the persistence of strong border effects that indicate the importance of different national policy regimes, and alternative explanations for any observed reduction in state capabilities have all been marshaled to counter claims that globalization systematically undermines states. The modern state and global capitalism originated at the same time, and they have co-evolved for centuries.⁷ Their relationship might best be described as symbiotic or complementary rather than competitive. The strategies of nonstate actors support this view of complementarity with the territorial state. Modern multinational corporations, however globalized their production networks, maintain close and dependent relations with particular states (Hirst and Thompson 1996). Multinational corporations also display the traits of distinctive national capitalisms. Nationality in the operations and strategies of these corporations "is given by historical experience and the institutional and ideological legacies of that experience, both of which constitute the essential structures of states" (Pauly and Reich 1997, 4). In similar fashion, leverage within the political systems of powerful states largely determines the influence exercised by transnational political networks, even as their activities undermine conventional views of state sovereignty (Keck and Sikkink 1998).

The persistence of strong border effects also suggests that globalization is far from producing a single market in goods, capital, or labor. In other words, the policy regimes created by states—admittedly only one source of border effects—remain influential. Although trade has grown faster than global national income during the past half-century, trade (measured by sales of goods) within OECD national economies remains approximately ten times greater than trade between those economies.⁸ Border effects are even higher for international migration, even between two neighboring countries with a history of substantial migration, such as the United States

7. The dependence of capitalism on a fragmented international political system (the modern state system) has been argued by Robert Gilpin, with reference to Jean Baechler.

8. Helliwell 1998. This estimate excludes the effects of common membership in a trade bloc, such as the EU or NAFTA, and a common language. Both of these reduce border effects.

and Canada (Helliwell 1998). Although international financial markets are more integrated than goods markets, at least among the industrialized countries, several indicators suggest the incomplete nature of that integration: national savings and investment rates remain highly correlated (the Feldstein-Horioka effect) and national investment portfolios remain highly concentrated on national assets (Obstfeld and Rogoff 2000). The barriers to a borderless world are not only the discontinuities and risks imposed by national states and their policy regimes. Information discontinuities, emphasized by Rangan and Lawrence (1999), depend on language and other features that differentiate national societies. The combination of increasing economic specialization, an often-overlooked consequence of globalization, and national policy regimes may slow the erosion of border effects by global economic integration.

Finally, alternative explanations may account for evidence that state institutions have changed or state capabilities have weakened. Peter Evans calls into question the evidence itself, claiming that pressure for a diminished state role in economic policy reflects the international dominance of the United States and a politically driven “Washington consensus.” The power of that Anglo-American consensus, not the demands of globalization, has shifted normative discourse and policy against state-centered options (Evans, 1997). Geoffrey Garrett argues that globalization, in the form of increased capital mobility, does not explain weaker economic performance in social democratic corporatist states during the 1990s (Garrett 1998b). Expansion of the state’s role in insurance against economic insecurity has often been linked to growing economic openness; demands for a welfare state expand at the same time that globalization may undercut national abilities to finance such a state (Rodrik 1997). Recent research suggests, however, that technologically driven processes of deindustrialization—not globalization—may better explain the growth of the welfare state (Iversen and Cusack 2000).

The alleged effects of globalization on the state have too often sustained logical but contradictory arguments that have not been investigated systematically. Concentration on state capabilities as the principal outcome of interest has also narrowed and distorted an investigation of globalization’s effects. Without a more careful accounting of the dimensions of institutional change and how central they are to the units that we label states, generalizations of the kind that have pervaded debates over globalization are futile.

This requirement is easily illustrated in the effects of global financial integration, which has progressed further than integration in the markets for goods or labor. The constraints imposed on monetary and exchange rate policy by increased capital mobility are summarized in the “trilemma”: fixed exchange rates, an autonomous monetary policy, and capital mobility cannot be realized simultaneously. Given increased global financial integration, the trilemma has been translated by Barry Eichen-

green into a sharp dilemma for small, open economies: completely fixed exchange rates (including currency unions or the adoption of another national currency) or exchange rate flexibility are the only sustainable choices in the longer run (Eichengreen 1994, but see Frankel 1999). Even if one accepts this “hollowing out” thesis, global financial integration substantially constrains national policy choice; it does not eliminate the role of states in monetary policy. In similar fashion, Eric Helleiner questions widespread arguments that regulatory arbitrage on the part of financial institutions has undermined that dimension of state capabilities vis-à-vis private actors. Rather, it has changed the location of regulation and the need for interstate coordination (Helleiner 1999).

Benjamin J. Cohen argues that globalization has intensified currency competition within national borders, undermining the Westphalian model of territorial money. Rather than a sweeping assertion of the state undermined, however, Cohen confirms two more qualified findings. The model of exclusive national currencies was only established in the nineteenth century, at a time when international economic integration was growing. This contemporary marker of state sovereignty is of fairly recent vintage and developed concurrently with a first stage of global financial integration. In this regard it resembles the regulatory and macroeconomic roles that have also been constrained by financial integration. Renewed currency competition in recent decades has not undermined the ability of all states to supply money, one of their core functions. It has instead established a clear hierarchy among states based on demand for their currencies. A more fundamental threat to the state itself only lies in an uncertain future: the possibility that purely private money may begin to compete with and eventually overtake national (state-supplied) currencies (Cohen 1998).

If supply of a national currency is usually regarded as a core state function, then a central bank—closely associated with autonomous national monetary and financial policies—is a key state institution. Its autonomy and influence within the state has been influenced by global financial integration through at least two avenues. An independent central bank provides a path to monetary policy credibility for countries with liberalized capital movements and flexible exchange rates. Sylvia Maxfield (1997) suggests a second connection between globalization and central bank independence for developing economies: the belief that such institutions are a useful signaling device in attracting foreign capital. Maxfield’s evidence does not suggest a necessary connection between global financial integration and central bank independence, however: institutional change is based on political perceptions, conditioned by national need for capital inflows.

Clark and Hallerberg (2000) offer a similar contingent model of the relationship between globalization (in the form of international capital

mobility) and choice of state institutions. Governments change the instruments by which they may attempt to influence electoral outcomes when they choose an exchange rate regime in an environment of mobile capital. Given that monetary policy becomes the effective instrument under a flexible exchange rate regime, governments may be reluctant to give up that instrument to a more independent central bank. This model of electoral competition predicts an institutional choice (central bank independence) that is not determined by globalization *per se*, but rather by a political calculus that still allows considerable government discretion.

These more complex and contingent models of the effects of globalization on the state point to three avenues that would permit more systematic empirical investigation. Explicit or implicit bargaining between the state and nonstate actors represents the microfoundation of many arguments about the effects of globalization. Better models of that bargaining relationship and how it changes as economic integration deepens would provide a useful grounding for research on globalization and its effects on the state. Such modeling would force more attention on the preferences of nonstate actors and the credibility of their exit or cross-border options.

Economic integration may produce a concentration of core governance functions in the state or their selective dispersion to other sites of governance at the international, regional, or subnational levels (Kahler and Lake 2000). If such deconcentration proceeds far enough, a “hollowing out” of state functions might occur, although that is far from inevitable. Growing cross-national spillovers or economies of scale in the production of public goods may point toward a transfer of some state functions to international or regional institutions. At the same time, intervening political dynamics, often based on transnational political organizations, may create externalities that are less easy to identify and explain. The influence of globalization on governance transfers requires additional exploration and explanation. Such transfer, in the European case and others, may produce alternative institutional bundles that make up units distinct from and potentially in competition with states.

Thinking of the state as an institutional composite—an institutional profile that changes over time—offers a third perspective on globalization and its effects on the state. Without such institutional specification, estimating the effects of globalization on the state quickly becomes an argument over how to define the state. An evolutionary model does not ask whether globalization brings about a generalized eclipse of the state. Instead it enquires whether particular *types* of states are likely to be successful in a new economic environment. In order to contend with an unevenly integrated international economy, certain state institutions will expand or decline over time. Comparative research has often found little evidence of overall institutional convergence among highly integrated economies (Berger and Dore 1996). Nevertheless, the preceding discussion of central

bank independence suggests that globalization may create common incentives—under specific political conditions—for adopting particular institutions.

THE STATE DISPLACED: UNIT VARIATION IN WORLD POLITICS

If globalization could threaten the central position of the state in world politics, that threat would arise from a redefinition of territoriality, a permanent loosening of control over boundaries. Its competitors in world politics derive their power from cross-border economic and political arbitrage. This prospect, if it exists, is based on their *lack* of territoriality, their freedom to shift with ease between political jurisdictions. Although private actors might claim governance functions from the state or press for a different institutional design, they do not attempt to displace the state.

Usurping the state's role as sole territorial unit in world politics would represent an agenda far more radical than the familiar (if unproven) effects of globalization. Such an agenda questions the monopoly that the modern state has exercised on imaginations and research agendas in the study of world politics. Instead of taking the state and stateness as given in international relations (the use of *international* suggests how taken for granted both are), the state and its key characteristics become variables. The contemporary nation-state becomes only one political competitor, albeit a central one, that is a dominant unit in world politics at the turn of the century. J. P. Nettl (1968) urged social scientists to consider stateness as a conceptual variable (while claiming that the external role of the state was invariant). In recent research the political constituents of world politics—the external dimension of the state and its attributes—are assumed to change over time and across political space. Rather than a simple Ptolemaic world politics with the state at its center, a galaxy of alternative political forms claims our attention.

At the center of the new agenda are explanations for the modern nation-state's evolution and its systemwide spread, taxonomies of other units and the systems that they once dominated, and investigations of the consequences of unit variation on external behavior and system outcomes. One key unresolved theoretical issue runs through this research. Recent scholarship on nationalism has emphasized the constructed quality of national identity and nationalist ideologies. From this conception of nationalism, it is a few short steps to the view that the units in world politics themselves are constructed, a corollary of the assault on the invariant nature of the state. But constructed by whom and for what purposes? Is this construction by the other units in the system or by individual agents within the units? Is it primarily normative (infusing sovereignty and other attributes with meaning) or is it based on rational calculation of material interests such as military power or wealth? Within each part of the new agenda, these theo-

retical disagreements remain. Whatever the response, however, the state will never be the same again.

The neorealist conception of the state claimed that the type of unit engaged in strategic interaction was irrelevant so long as the units were functionally equivalent. On that claim rested neorealism's concentration on capabilities as the only significant unit-level variable within anarchic systems. John Ruggie opened debate over unit variation by attacking this assumption, arguing that neorealism did not accommodate the possibility of systems—such as feudal Europe—in which units were differentiated (Ruggie 1983). In response, revisionists attempted to incorporate differentiated units into a structural realist frame, deriving different anarchies from that unit variation (Buzan, Jones, and Little 1993). Others have rejected a neorealist frame and extended their search for unit variation back in time and into the non-European world.

PRESTATE AND ANCIENT SYSTEMS

One method for capturing unit variation and its effects is to extend the range of theorizing historically, far past the norm in international relations (typically the twentieth century). This strategy permits examination of the links between states and particular behaviors (such as war making) through units that share only certain attributes with states. These approaches also shed light on the evolution of states and the reasons for their relative success among other competing political forms.

Although political scientists have seldom examined the practices of prestate societies toward one another, anthropologists have debated whether war, often taken as a fundamental attribute of state systems, is characteristic of societies without states. Lawrence Keeley, Steven LeBlanc, and others have discovered widespread evidence of violent (rather than ritualistic) warfare among prehistoric and prestate societies (Keeley 1996; LeBlanc 1999). Drawing on the patterns of warfare among such societies, Keeley suggests that interstate conflict may be lower by comparison because of better control over members of society (preventing the outbreak of revenge cycles) and a higher level of interunit institutionalization. The importance of such institutions is illustrated in those prestate societies that have constructed resilient security institutions, such as the Iroquois federation (Crawford 1994).

Ancient polities and their interaction have also attracted attention. Although most analysis is at the system level, unit characteristics provide one explanation for system outcomes that diverge from the modern state system. Christian Reus-Smit (1999) contrasts the fundamental institutions of the society of Greek city-states with those of later societies of states. He explains those institutional differences by constitutional structures that are grounded in beliefs about the moral purpose of the state. Early Mesopotamian systems and the interaction of Mayan polities have been

investigated (Cioffi-Revilla and Landman 1999; Cioffi-Revilla 2000). Patterns of state formation and decline or dissolution have been traced to growing status or income inequality under a wide range of conditions (Midlarsky 1999). Fine-grained analysis of one long-lived system—the Amarna system of the ancient Middle East—has illuminated the relationship between diplomatic practices and norms on the one hand and the construction of a system among culturally dissimilar units on the other (Cohen and Westbrook 2000).

Data limitations have shaped research on the relations among prestate and ancient societies. Systems are more easily investigated than units, even though the units diverge from modern states in interesting ways. Only a careful reading and interpretation of historical and ethnographic data—requiring skills that are often beyond the reach of political scientists—can generate an accurate portrait of the self-understanding of these societies and their external relations. In an interdisciplinary dialogue regarding the Amarna system of the ancient Middle East, political scientists were quick to discover practices analogous to those in the contemporary state system; scholars trained in ancient languages and history were more attentive to the risks of anachronism. Both methods could be deployed to considerable effect: historical and linguistic techniques to “get inside” the historical agents and their world-view and social scientific methods to compare their self-understanding with actual practices and with modern patterns of interaction (Cohen and Westbrook 2000).

EVOLUTION AND VARIATION IN THE WESTPHALIAN SYSTEM

Gaining purchase on variation in the units of world politics does not require such distant historical investigation. The modern state itself has evolved over time, and it emerged in competition with other units that enjoyed considerable military and economic success. Unit variation can be captured by examining that evolution over time and by analyzing its now-extinct competitors. This emergent-actor approach to variation has most often relied on historical and comparative approaches. Lars-Erik Cederman (1997), however, has applied a class of models used for other complex systems to simulate the emergence of state systems and nationalism.

Charles Tilly (1990) and Hendrik Spruyt (1994) have challenged the assumption of a unilinear development from feudalism to the modern European state. Both have constructed a wider taxonomy of political units that emerged from the feudal order (city-states and city-leagues), unit types that competed for dominance with the sovereign territorial state. Both have also emphasized a selection mechanism centered on military competition to explain the ultimate dominance of the modern territorial state. In Tilly’s case, evolving military technology and the development of capitalism in larger territorial states eroded initial financial advantages of the city-state. Spruyt emphasizes the internal institutional advantages of the state in the competition with other units. He also suggests other, nonevolutionary mecha-

nisms: the mutual empowerment of states through practices of recognition and legitimation and the institutional choices of influential social actors.

One difficulty with such selection models is the incomplete outcome in Europe: other types of units survived for centuries into the era of the modern state. Also, the European state itself was not a constant. The external and internal attributes of sovereignty have evolved over time. Stephen Krasner disaggregates sovereignty into four dimensions and demonstrates that two of these—Westphalian sovereignty (domestic political autonomy) and legal sovereignty (recognition by other sovereigns)—have had different content (measured in state practices) in different historical periods. Westphalian sovereignty in particular has been “frequently transgressed” (Krasner 1999, 224). Although Krasner finds the source of this variation in the preferences and practices of dominant states, others trace different understandings of sovereignty to an evolving process of collective social construction (Biersteker and Weber 1996).

STATES AND NATIONS

More than any other contemporary events, the dissolution of the Soviet Union and Yugoslavia underlined the transience of apparently secure states. A decline in the frequency of interstate conflicts relative to domestic insurgencies and civil wars placed the question of state collapse and ethnic identity at the center of the comparative politics research agenda. As Barbara Geddes and David Laitin describe in this collection, explanations for the mobilization of ethnic identity into politics and the collapse of political order into violent conflict have been sought using an array of methodologies. Both international relations and comparative politics have spent much less time considering the political units within which identity and order are constituted.

The demise of modern states drew attention to nationality, an attribute of those units that had been largely ignored in international relations (Lapid and Kratochwil 1996a). The manufacture of new nationalisms in the 1990s brought to the fore the constructed character of both national identity and the states to which it attached itself. Nationalism necessarily concerns the state, since nationalism is usually defined as a theory of political legitimacy that requires a congruence between ethnic and political (state) boundaries (Gellner 1986; Hobsbawm 1992). In an authoritative compendium of nationalist movements, John Breuilly (1994) claims that nationalism is a “form of politics” that has “possession of a state” as its “major objective.” These definitions themselves imply invariance in nationalism, however, an invariance belied by the historical record. The attachment of ethnic movements to a claim for statehood has varied over time; unless one classifies only movements with such a claim as nationalist. Conventional definitions divert attention from this important fact.

Nationalism and the state have varied over time, as has the link between the two. The first assumption (variable nations) is now the center-

piece of research on ethnicity and nationalism; the second lies at the core of renewed interest in the units of world politics. The link between the two has received much less attention. As Cederman notes, however, if both state and nation are treated as variables, the possible outcomes are multiplied: groups may choose to define their ethnic identities according to particular criteria, those identities may find political expression (or not), that political program may or may not include the demand for a fully sovereign state that is associated with a nationality, and states may define themselves as mono- or multiethnic political entities.

Consider the Scots and Scottish nationalism. Creeping union with England was largely a constitutional process in 1707; its economic benefits increased over time but were not a major motivation for union. Expression of Scottish local identity shifted over time, from North Britain, to Caledonia, to Greater Britain, and then back to Scotland (without rejecting other, broader identities). By the nineteenth century, Scotland was a “melting-pot of different, overlapping identities” (Lynch 1991). Political claims for a more exclusive Scottish identity began in that century but did not take the form of demands for a sovereign state until well into the twentieth century. The link of that national identity to familiar ethnic characteristics such as language or religion remained tenuous. In response to recent political assertiveness by Scottish nationalists, the British state devolved power to both Wales and Scotland, creating a more decentralized state. Whether such an institutional response, well short of sovereign statehood, will satisfy nationalists (or the median Scottish voter) is uncertain.

Why states define themselves in national terms, and why and when ethnic groups determine that their identity requires a state are research questions that are central to a definition of political units in contemporary world politics. Answers to the first question—why states choose nation building—often rely on military competition, which creates demands for the extension of direct rule and top-down nationalism on the part of state elites (Tilly 1996). This reading of the benefits of a nation-state are drawn in part from the experience of the French Revolution, when a citizenry mobilized by nationalism appeared to provide the military means for the French state to defeat its dynastic rivals (Posen 1993a). The politics of incorporating this “knowledge” into the practices of rival states was uncertain, however. The success of citizen armies required interpretation by other political elites, and the exclusion of mercenaries and other noncitizen soldiers did not emerge rapidly as a point of state identity (Avant 2000). The extension of direct rule and concomitant nation building by the state could also be attributed to an ideology of “state simplification” that attempted to transform multilingual and disorderly societies into “something more closely resembling the administrative grid of [the state’s] observations” (Scott 1998, 82). Whatever their source, the extension of direct rule and programs of nation building that aimed to inculcate uniform language and culture in turn produced resistant nationalisms within the state (Hechter 2000).

Resistance to nation building, which can be violent, drives up the costs of such programs. The potentially high costs of nation building demand a fuller explanation for projects to construct a link between state and nation. Presumably a perceived reduction in administrative costs (costs of resource extraction, use of a conscript army) more than offsets the estimated costs of nation-building programs and containing resistance. Over time some states have been transformed into relatively successful nation-states in which national identity becomes hegemonic—"common sense" for the population in David Laitin's terms (1986). In other cases, persistent and costly efforts to meld national identity with existing states have failed. The reasons for the record of success and failure across states and over time represent another puzzle in the adoption of the nation-state model.

The second question linking nation and state—why some nationalist movements claim statehood and not others—also remains unanswered. Mobilized ethnic groups may stop short of demanding a sovereign state because of invested economic interests or an inability to overcome collective action problems (Hechter 2000). Questioning the link between state and nation casts that most familiar of actors in world politics as a far more problematic construct. As the nation-state model expanded outside Europe, the connection between state and nation became more tenuous as a basis for international politics, and the gap between the empirical and juridical state widened.

THE EXPANSION AND FAILURE OF THE NATION-STATE MODEL

During World War II, E. H. Carr recalled the failures of blanket self-determination and the misguided creation of nation-states after the previous conflict. He warned against experiments in state building across ethnically diverse territories in the non-European world, arguing instead for "balkanisation" as a positive outcome, "a far greater measure of devolution and an immense variety of local administration rooted in local tradition, law, and custom" (Carr 1941, 56). Two world-historical surprises occupied the second half of the century: first, the rapid demise of one unit type—empire—that had persisted for millennia, quickly succeeded by postcolonial imitation of a nation-state model imported from former colonial masters. "The state, once a rare political construct confined to the western extension of a rather small continent, has spread its rule all over the world" (Van Creveld 1999, 334). An emerging consensus now recognizes that the export of the nation-state model (and its willing import by elites in the developing world) has often been a failure, producing rather than preventing internal disorder and external weakness (Badie 2000).⁹

The disappearance of empires has been treated historically and comparatively, but rarely theoretically.¹⁰ The existing literature has concen-

9. For similar judgments, see Van Creveld 1999, 332; Tilly 1998b, 407.

10. For example, comparisons of the collapse in the Soviet empire with earlier imperial disintegration: Dawisha and Parrott 1997.

trated on long-standing debates over why empires emerge, rather than the question of how they have varied in their institutional design and why they have persisted or failed. Much of the debate surrounding the underpinnings and end of the European colonial empires has contrasted models based on self-interested actors motivated by material or political interests and explanations that emphasize normative change at the global level.¹¹

Empire had required the stripping of sovereign recognition from many of the sovereign territories; winning such recognition from the European powers offered some measurable insurance against forceful incorporation. That lesson in state selection explains only part of the widespread adoption of the Western nation-state model in the aftermath of decolonization. Given resistance to European rule and availability of both precolonial models and postcolonial rivals (such as pan-Arabism), adoption of the ill-fitting template remains a puzzle. Recent research on pre- and postcolonial state systems offers both a deeper understanding of the consolidation of new states and competing explanations for the hold of this European import on their elites. Two types of explanation dominate: on the one hand, reinterpreted international norms of sovereignty and territorial integrity and, on the other, the calculus of domestic elites.

In their consideration of regional politics in the Middle East, Malik Mufti (1996) and Michael Barnett (1995) offer different interpretations of the consolidation of a regional state system whose units were in question as the colonialists departed. Barnett gives greater weight to emerging norms of sovereignty and Arab nationalism in the region; Mufti, to the self-interested strategies of political elites, whose interest in pan-Arabism as a domestic prop declined as their hold on state power strengthened. Paralleling Barnett's normative explanation, Robert H. Jackson (1990) argues that quasi-states in the developing world are actively sustained by an international regime of negative sovereignty that differs from both the preimperial sovereignty regime and the positivist doctrines that reinforced European colonialism. In his recent analysis of the African state system, Jeffrey Herbst (2000), on the other hand, awards an important role to the "importers" of the sovereignty regime, the new political elites in Africa who rejected more differentiated precolonial units and welcomed an inherited regime of fixed borders. A supportive international environment, in Herbst's view, diminished the incentives for constructing internal rule over the national territory and laid the basis for today's failed states.

UNITS WITHIN UNITS: VARIATIONS ON A THEME OF HIERARCHY

Renewed interest in empires and their demise points to a final theoretical shortcoming in the view of units that has prevailed in international relations. The conventional view of the modern state system incorporated a

11. For the former, Davis and Huttenback 1986; Kahler 1984. For the latter, Jackson 1993.

particular model of the state as well as an assumption that normal relations among those units were anarchic. Just as the state has been deposed from its position as an invariant feature of world politics, the assumption of anarchy has also been reexamined. With reexamination has come an awareness that international relations and domestic politics display a spectrum of relations among units that is wider than a simple divide between anarchy and hierarchy. Anarchy has not been the defining characteristic of all world politics; hierarchy within domestic politics has often been uncertain as well. States and other units have engaged in hierarchical relations (involving more or less coercion), as well as consensual unions and cooperative arrangements, of greater and less formality. Clusters of international or interunit relations may in turn become units of a different sort, engaging in their own relations with one another. Rather than Herz's conventional view of hard-shelled states of a single type within a competitive, anarchic setting, one begins to perceive units of different types managing an array of relations, both internally and externally. The sharp divide between domestic and international politics breaks down.

David Lake (1999) has used this core insight to reexamine the foreign policies of the United States, a powerful state during the twentieth century, when military competition was often intense. He develops a model of interunit contracting to explain a "rich tapestry" of security relationships. Lake's model is based on joint production economies (which determine whether security cooperation or unilateralism will dominate) and two additional variables that determine the anarchic or hierarchical form of any security relationship: costs of opportunism (in turn influenced by relationally specific assets) and governance costs. This approach not only captures a greater degree of variation in security policies, beyond decisions to ally or not ally; Lake also provides explanations for those policies over time.¹² Units defined by internal relations of greater or lesser hierarchy among constituent units also characterized the antebellum Philadelphian system in the United States, an alternative to both federation and empire that has been described by Daniel Deudney (1995). The richest territory for exploring the array of relations within and between units can be found in the burgeoning literature on the European Union (EU), however.

THE EUROPEAN UNION: ECONOMIC INTEGRATION AND UNIT VARIATION

The institutions of European integration draw together economic integration (globalization) as a source of unit variation and the extension of that variation beyond nation-states. The EU was defined from its beginnings by conflicting models of European institutions—a trajectory toward federalist hierarchy versus a setting for intergovernmental bargains. Whether its constituent political elites were motivated primarily by economic interests in

12. For a similar argument, see Weber (1997).

the face of growing trade and investment flows or wider foreign policy and political goals has also remained controversial.¹³

By the 1990s, analogies to other intergovernmental organizations were weakened by more extensive delegation to European institutions, the wider scope of European bargains, and dense political linkages between European and national institutions (Kahler 1995). Although development of the European legal system and constitutionalization of the European Union permitted comparison to a domestic legal order, the EU did not approach the foreign policy and security powers of any existing federation in its outer face.

The study of European institutions has gradually been liberated from comparison with the singular benchmark of the nation-state. Postliberation, research on the European Union illustrates the value that is added by expanding the field of unit variation. The EU is best captured in a wider array of institutional models that sidesteps the earlier concentration on attaching a statelike label to its institutions. Its recent development and political processes have been characterized as a regulatory structure based on efficiency gains as well as a parliamentary model based on democratic legitimacy (Caporaso 1996; Dehousse 1998). Policy networks, rather than more hierarchical or state-centric models, have also been advanced as characteristic of European political processes (Risse-Kappen 1996). Even the external relations of the EU, usually portrayed as the failure of a common foreign policy, have been subjected to revisionist scrutiny. Instead of a failed superstate, the European Union is portrayed as an "exclusive international club," creating its own network of partners and clients through negotiation and the incentives of prospective membership (Zielonka 1998). In evaluating the EU's performance in the sphere of foreign and security policy, as in other policy domains, observers now realize that the appropriate units of comparison must be specified precisely (Jørgenson 1998).

The European Union provides a final example of the historical and theoretical limitations of conventional conceptions of the state and the benefits of embracing the agenda of unit variation that has been described. Research on the EU also demonstrates that empirical investigation of unit variation requires the specification of unit dimensions and a means for measuring them. When the state was unquestioned, this dilemma did not exist. Now that the state has become a variable, taking the measure of that variable becomes essential. Institutional analysis has begun to play that role in the case of the EU. The next section suggests how that borrowed set of tools can be applied to other analytical tasks.

13. For a strong statement that economic motivations were dominant, see Moravcsik 1998a.

FORGING A CONCEPTUAL TOOL KIT: THE STATE AS AN INSTITUTIONAL COMPOSITE

More precise analysis of the consequences of globalization for the state or the effects of unit variation on world politics requires an ability to identify and measure variation within the category of states or the larger field of political units. In system-dominant approaches, such as neorealism, the assumption of institutional convergence under conditions of international competition obviated the need for such institutional analysis. Although some still use the state as an unproblematic starting point, most neorealists now concede the need for unit-level variables in order to explain international outcomes. Stephen Van Evera's treatise (1999) on the causes of war, for example, includes polities as diverse as ancient Rome, ancient China, and modern European states as implicitly similar units.¹⁴ His core explanation for war, however, is misperception regarding the international structure of power. Most of those misperceptions are, in turn, caused by variations in state institutions and policies.

Alternative approaches to institutional analysis could be used to both enrich the dimensions of variation in states and other units in world politics and sharpen the measurement of that variation (Hall and Taylor 1996). When the state was "brought back in" during the 1980s, historical institutionalism framed the state as an institutional endowment, slow to change, imposing relatively fixed constraints on political actors within its borders. The degree to which those constraints were binding over time measured state strength vis-à-vis domestic society, a distinction with implications for foreign policy and international outcomes.¹⁵ A second conception of the state was more compatible with rationalist and game theoretic approaches to political institutions and international politics. The state was defined as an actor, represented by "politicians and administrators in the executive as independent participants in the policy process" (Ikenberry et al. 1988, 10). Each of these approaches to the state had its drawbacks. State strength represented a single dimension of state institutions, and one that proved difficult to measure. The state-as-executive view often eliminated any political model of how state preferences were formed and removed the state from wider domestic political processes.

Development of institutional analysis during the past decade has allowed reconstruction of conceptions of the state (and other units in world politics), enriching the dimensions applied and sharpening their measurement. In an admirable survey of institutionalist approaches to foreign policy, Ronald Rogowski suggests one set of institutional dimensions based on

14. The causes include dominance of professional militaries over policy; construction of "chauvinist myths," largely through state educational systems; failure of bureaucratic evaluation of key policy ideas; and vagueness in official strategy (Van Evera 1999, 256–57).

15. For a recent example of state-centered realism that awards a central place to state strength, see Zakaria 1998.

principal-agent theory: a franchise (defining the set of principals), rules and mechanisms for representation of those principals, and decision rules that govern those agents or representatives (1999, 119–22).

Although these dimensions are limited to the internal ordering of the state, they add two key features to earlier typologies of the state. In contrast to those who limited the state to the executive and bureaucratic apparatus (and implicitly viewed legislatures and their power as signs of a weak state), legislative institutions are central to rational-choice institutional analysis. In addition, explanation requires attention to the composition of the constituents or principals who can retain or remove state officials. These may be a democratic electorate, politicians (in the case of bureaucratic personnel), or, in nondemocratic regimes, a selectorate. The state and its institutions are firmly embedded in a wider domestic politics.

In its application to the state, rationalist institutional analysis displays strengths and shortcomings. Both are illustrated in one of the most active research agendas in political science: the democratic peace. The finding that democracies rarely, if ever, make war on one another has produced a large and growing body of research that both documents and contests that finding. The distinctiveness of both the foreign policies of democratic states and dyads of those states have been extended to other foreign policy domains as well.¹⁶ Two alternative explanations are advanced for the democratic peace and related observations: normative and institutional. Institutional explanations now appear to fit existing observations of democratic states, their foreign policy behavior, and international conflict with greater accuracy.

The institutional attributes that explain the democratic peace remain in dispute, however. At least three institutional alternatives have been advanced. The first emphasizes the information environment of democracies, and particularly the reduction in information asymmetries that result from greater transparency and clearer signaling between democracies. The second regards democratic institutions as constraints on leaders who make the decisions for war or peace, altering their incentives (through the threat of removal) and permitting easier mobilization of opposition to conflict.¹⁷ A third alternative builds on two of these constraints—the breadth of the democratic franchise and the criteria for selection of democratic leaders—to argue that democratic leaders who opt for war are likely to try harder to win and to avoid conflicts they believe cannot be won (Bueno de Mesquita et al. 1999). Discrimination among different institutional arguments in this instance, as Schultz points out, is difficult because the institutional variables in question are highly correlated (1999, 238).¹⁸

16. These findings are summarized in Bueno de Mesquita et al. 1999.

17. These two models of institutional influences are summarized by Schultz 1999.

18. Schultz does devise a research design that permits such discrimination, although he cannot conclusively rule out the competing institutional argument.

The testing of increasingly sophisticated institutional models against foreign policy data has produced a cumulative research agenda on democratic states and their foreign policies that is unmatched in international relations. Despite its accomplishments, this agenda includes a limited set of institutional dimensions: the franchise and rules of representation. Institutional analysis in this instance borrows heavily from the study of contemporary advanced industrial democracies, which may limit its usefulness for investigating a wider array of units. Another avenue of institutional analysis may be more promising in this regard: the role of institutions in promoting credible domestic and international commitments. Peter Cowhey (1995) has described how divided powers in the United States and Japan's parliamentary system offer contrasting obstacles in the form of veto gates to initiating new policies and reversing existing commitments. Both within the group of stable democracies and across democratic and nondemocratic regimes, institutional design will typically grant a greater ability to change policy to meet external circumstances only at the cost of lower accountability and reduced credibility of commitments.¹⁹ By measuring veto gates and other institutional devices that enhance credible commitments, one could construct a spectrum of states between poles of credible states and flexible states.²⁰

Delegation is a third feature of political institutions that may be used as a commitment technology. As described earlier, independent central banks may provide such an anchor for national monetary and exchange rate policies. Delegation to institutions or units *outside* the national state, however, provides the basis for additional dimensions of state and unit variation: the centralized state versus the deconcentrated state. Whether produced by the pressures of economic integration or other causes, national states display considerable variation in their willingness to delegate key governance functions to international and regional institutions on the one hand or subnational units on the other. Cross-regional variation on this dimension is striking: European states have delegated considerable powers to the European Union and other regional and global institutions; they have also instituted significant devolution to states, regions, and localities within their borders. In other regions, such as the Middle East, states remain highly centralized and are reluctant to delegate any significant functions to regional institutions. Research on international institutions and on federalism have seldom been joined to determine whether and how these two types of delegation might be related.

19. This trade-off and the question of how institutions reinforce the credibility of commitments figures prominently in Auerswald 2000, MacIntyre 2001, Martin 2000, and Mansfield, Milner, and Rosenthal 2000. On veto players, see Tsebelis 1995, 1999.

20. Compare this distinction to the trade-off of decisiveness and resoluteness in Cox and McCubbins 2001.

The institutional analysis described thus far is individualist and rationalist in its assumptions; self-interested actors construct state institutions through strategic interaction. An alternative sociological view of institutions emphasizes their normative content and the role of the institutional environment in constituting actors and their interests.²¹ Peter Katzenstein (1996a) has argued that key security institutions in the Japanese state are circumscribed by a normative consensus, one that has been shaped by political contestation over the postwar decades. Military institutions and policies have also been shaped by distinctive and persistent strategic cultures (Johnston 1995). Martha Finnemore documents institutional and normative importation from the international environment in the spread of scientific research bureaucracies. In this case there is little evidence of financial or other incentives offered by the external actors; the construction of these institutions seems to be related to a particular normative identity of the state as “engine of progress and modernity” (Finnemore 1996a, 129).

A normative approach to defining state institutions faces different obstacles than rationalist strategies. The most familiar is that leveled by Krasner in his analysis of the central norms of sovereignty as “organized hypocrisy,” persistent, resilient, but with shifting effects on state practice. The multiplicity of norms and their empirical specification over time is also source of uncertainty. Post hoc explanation is a serious risk without clear definition in advance of the norms and their content (Kowert and Legro 1996). Finally, the political process through which certain norms define state institutions and others fail remains obscure. The model of norm dynamics proposed by Finnemore and Sikkink (1998) provides one model for norm-driven politics, but its scope is unclear.²²

Whatever the approach to institutions taken, reconstructing the state as a composite or bundle of institutions would require changes in the direction of institutional research. A taxonomy of institutional composites or bundles is required in order to define unit variation. The set of institutions that defines the modern nation-state or any of its variations must be specified. Most institutional research to date has concentrated on only one dimension associated with states: internal institutional configurations. Even within that dimension, attention has been heavily concentrated on advanced industrial democracies and representative institutions. Institutional analysis has been applied less frequently to other parts of the state apparatus that span a wider array of states and have important implications for international relations. Military institutions, for example, particularly relations between military bureaucracies and their political superiors, have been a historically important determinant of both state capabilities and po-

21. For a typology of such approaches, see Jepperson, Wendt, and Katzenstein 1996.

22. It does not appear to fit the norm-based account of Katzenstein (1996a), for example.

litical stability (Avant 1994). Movement from professional to conscript militaries (and back again) has been a source of political controversy and an institutional choice with external implications (Levi 1998a; Avant 2000).

Other dimensions that lie outside the internal ordering of the state and define the spectrum of units have received far less attention. Territoriality is often identified as a core institutional feature of the modern state. It is also a key dimension of unit variation: most political organizations are territorial in some sense but their territoriality differs from contemporary practices and interstate conventions. Cohen (1998) contrasts the Westphalian model of monetary geography—currency use matches state territories and their boundaries—with a far more complex portrait of transactional domains that span conventional boundaries. Territoriality defined as the coincidence of exclusive policy domains on a delimited territory has begun to unravel. Powerful states extend their policy domains well beyond their national boundaries through sanctions or agreement. Modeling the institutions of territoriality, which are central in defining state and unit variation, should become a central part of the institutionalist research agenda on the state.

Bridging both rational-choice and sociological institutionalism, unit environment appears and reappears as an exogenous and sometimes ad hoc feature in explanations of the state and its institutional profile. Avner Greif's narrative (1998) of the Genoese city-state awards a central but fluctuating role to external threat and intervention in establishing a self-enforcing political system. Finnemore (1996a) outlines a dominant role for the international normative environment in defining certain state institutions but also allows for an undefined level of local variation. The means by which the environment influences institutional development and variation requires clarification. Rationalist models (and conventional international relations) often rely on implicit notions of competition and selection. Sociological institutionalists typically adopt some combination of diffusion, socialization, and learning. The precise causal steps from environmental change to institutional evolution are too often omitted, however.

Treating the state and other units in world politics as composites of institutions has the attractive side benefit of providing international relations and comparative politics with a common vocabulary and methodology. The bright line between domestic and international politics disappears: nearly all politics becomes polyarchic, lying between pure anarchy and pure hierarchy.²³ This common institutional approach to states and other

23. As Milner notes, her use of polyarchy differs from the earlier (and widespread) use coined by Robert Dahl meaning democratic rule. A state's movement away from hierarchical organization in its politics is measured by the degree to which the preferences of key actors diverge, the level of power sharing among those actors, and the centralization of control over information (1997, 11, 1998).

political units has already enriched research on the European Union. The European Union is treated as a “highly developed political system” with a dense field of institutions that lacks both a state apparatus and high level of mass political participation (Hix 1999). Institutional analysis has often taken state institutions as fixed, defining the parameters of a game, and has then attempted to assess the policies that result from a particular institutional equilibrium. Any application of institutional analysis to the now-variable state or other units will require endogenizing state institutions, viewing them as a target of political calculation, actor strategies, and normative constitution.

QUESTIONING THE STATE: FROM BLACK BOX TO PANDORA’S BOX?

The state in international relations has rapidly moved from ideal-typical constant to a variable that is subject to empirical investigation. The field of international relations has begun to investigate states rather than taking them for granted. This renewed interest has been driven by events—globalization, the collapse of cold war verities—and a convergence of different research streams—political economy, history, institutional analysis. The new agenda also marks the continuing and very slow motion breakdown of Eurocentrism in international relations.

The three research avenues described here bring different assumptions and different aims to this project, but their progress is also linked. Globalization or international economic integration has been one driver of unit variation over time. Although the rhetoric of globalization often proclaims the demise of the state, the realities of globalization raise research questions that chart far more modest outcomes. Political scientists have concentrated on the effects of globalization on policy autonomy and new political coalitions rather than state institutions. In assessing the effects of globalization on the state, bargaining among states and nonstate actors requires more accurate modeling. The outcome of such bargaining (or the anticipated reactions of governments to the demands of private actors) may produce a migration of some state functions to subnational actors or to regional or international institutions. Over time, such transfers could transform the existing institutional profile of the nation-state. The pattern of such transfers and their explanation provide a second avenue for investigating the influence of globalization on the state. A third means of transformation could also be examined: whether a globalized environment rewards or discourages particular institutional designs among states. Any of these approaches—bargaining outcomes, shifting levels of governance, or environmental selection of different institutional configurations—provides a more measured and accurate portrait of globalization’s effects on the state.

Unit variation is the label that I have given to a research agenda that

has more profound theoretical implications for conventional views of the state. The modern nation-state is simply one, temporarily successful contender amid a number of competitors over the course of history. Unit history has been extended to include the earliest civilizations and prestate societies. The ancestry and rivals of the sovereign territorial state are explored. The nation is disconnected from the state. And the empirical emptiness of the concept of nation or state is conceded for much of the world. The state as we knew it has not disappeared, but its central theoretical and historical position in international relations is undermined. This emergent actor agenda, in Cederman's phrase, has to date emphasized explanations for the emergence of particular types of units and the disappearance of others. In addition to those processes, the significance of such variation must also be assessed: have systems composed of different units exhibited different behaviors and outcomes? By means of which institutions—internal or interunit—were different units defined?

Finally, estimating the effects of globalization on the state and explaining larger patterns of unit variation require more precise unit definitions and a means of measuring variation. Institutional analysis may provide answers to these knotty problems of definition and measurement. However, current institutional research requires redirection in order to capture the scope of institutional changes raised by both globalization and unit variation. Its concentration on the contemporary industrialized democracies and their foreign policies should broaden to include a wider range of units and dimensions of variation. The dimensions of territoriality provide only one example of variation that could be illuminated by the application of institutional analysis. Recent research on the European Union demonstrates the potential of institutional analysis applied beyond the confines of the state.

Although this account has necessarily given a false coherence to an anarchic and effervescent research environment, several issues unite the old, the new, and the borrowed. If the now-open black box of the state is not to become a theoretical Pandora's box, theoretically informed taxonomies are required. One advances this recommendation cautiously in the field of international relations, where taxonomies with short half-lives have been particularly numerous. Nevertheless, a shared ability to identify and name the new subjects of investigation will be required.

Second, this research implies that sharp distinctions between domestic and international should be rejected. The perspective of unit variation confirms that what is a unit in one setting will be an environment in another. The old hard distinction between internal hierarchy and external anarchy, challenged by Milner, Lake, and others, has broken down theoretically and empirically. The warlords of Somalia are units in an anarchic setting within a (quasi-)state; European states inhabit an economic space and security community that is a unit in formation as well as a decidedly nonanarchic environment.

Finally, units can form the environment for other units and those environments vary as well. A blurring of the analytic distinction between domestic and international does not erase the lines between units and their environments. It is on this issue—the relative importance of the environment in defining unit identities and interests—that researchers divide. Rather than replicating old debates that treated this question by pronouncement, however, it would be better to view it as contingent and empirical. Careful definition of the environment, whether normative or material, and close investigation of the interaction between actors and that environment are required.

These challenges to the monolithic conception of the state in world politics could spell an end to its usefulness as a concept. From Nettl's "basic, irreducible unit," the state may become simply one combination of institutions, sharply bounded in time and space. Given the deep roots of the state in the study of international politics and its contemporary dominance as a political unit, it seems unlikely that the shorthand of the state will disappear. That shorthand should be deployed in a more tentative and precise way, however, recognizing that the state of today is not the state of the past or the future. Nor does the state's peculiar identity encompass all of the institutional possibilities in world politics.

WHY DOES IT MATTER?

Although the interaction of Mesopotamian empires or precolonial African states may offer intrinsic intellectual interest, skeptics may also question whether researching such historical exotica is a self-indulgence, diverting attention from critical contemporary issues. Certainly, rigorous scholarly attention to globalization can only clarify debates where misinformation and demagoguery are rife. Even the larger research agenda surrounding the state is very close to the headlines.²⁴ East Timor and Kosovo are international protectorates, a new or reinvented exercise of collective hierarchy, established after the fragmentation of two nation-states. Colombia is a state that has given political control over portions of its territory to armed insurgents. In sub-Saharan Africa the era of the quasi-state has collapsed in cross-border intervention and failed states. The research described here is not alone in questioning the state; the world contributes as well.

Clear diagnosis is not the only contribution that this research agenda can make, however. Already, those who wish to rebuild the old nation-state system and those who propose alternatives offer competing visions of the future. For much of the 1990s, an analogy from interstate to intrastate conflict dominated the discussion of ethnic conflict. Ethnic violence was traced to insecurity. The prescription for its end followed: reestablishing security within strong and ethnically homogeneous states. Partition became

24. The headlines in this case are drawn from *Foreign Affairs*, July–August 2000.

a solution of choice. Drawing on some of the theoretical innovations described here, David Laitin (1998a) has pointed out the low probability that nations (if they exist) can be aligned closely with states. Jeffrey Herbst (2000) questions whether state failure in Africa can be addressed successfully without radical changes in the terms of external support and an opening to institutional alternatives. Gidon Gottlieb proposes a “new space for nations” that is “not organized territorially into independent states” (Gottlieb 1993, 36). Their proposals reflect a first, provocative wave of prescription derived from this theoretical opening of the state in world politics.

ATUL KOHLI

*State, Society, and Development*¹

How varying patterns of state authority emerge in developing countries and how state authority is in turn used to push these countries along a variety of developmental pathways are questions of enduring interest to scholars of comparative political development. Over the years these concerns have been addressed from a number of theoretical standpoints. Following World War II, and especially in the heyday of the cold war, liberal and radical scholars debated these issues under the rubric of modernization and dependency perspectives. Over the last two to three decades the best research in the subfield has instead been framed by what may be loosely labeled a state-society framework. Informed by this neo-Weberian standpoint, scholars have pursued a series of important questions: Why is it so difficult to create coherent state authority in the developing world? Where such state authority emerges, what are the origins and dynamics of revolutionary, authoritarian, and democratic regimes? How and why do states vary in their effectiveness in promoting social and economic development? In what follows I critically review some of the more recent important scholarly contributions aimed at unraveling these puzzles of intellectual and real-world significance.

After briefly identifying the defining elements of the state-society perspective, this review is organized in two main parts. The first part discusses research that seeks to explain the emergence of a variety of authority structures in the context of development, especially revolutionary regimes, bureaucratic authoritarian regimes, and democracies. The focus in the second part is on research that seeks to explain the impact of state structures and actions on the society and economy in developing countries. The

1. I would like to thank Jeffrey Herbst, Joel Migdal, and Deborah Yashar for their helpful suggestions on an outline of this essay; Sheri Berman, Jason Brownlee, Forrest Colburn, Peter Evans, James Mahoney, and two anonymous reviewers for their useful comments on an early draft; and Erik Kuhonta for his valuable research assistance.

essay concludes by briefly acknowledging some of the main criticisms of this research tradition, especially those developed by the more culturalist and rational-choice scholars, and by suggesting some useful future directions of research.

The general argument developed in this essay is that, when judged against such criteria as the capacity to generate cumulative and generalizable knowledge about important issues on the one hand and the ability to attract the interest of a range of senior and emerging scholars on the other hand, the state-society perspective represents a robust research tradition in the study of comparative political development. This is mainly because there is a good fit between what attracts scholars to this subfield and the state-society framework. Scholars of comparative political development generally pursue research on problems of real-world significance in one part of the world or another. Working within a state-society framework thus enables them to pursue theoretical issues at a middle level of generality without sacrificing either empirical specificity or relevance. While the resulting research generally clusters around problem areas, broadly shared assumptions and concepts also create a wider research community in which scholars can readily move from the study of one problem to another.

More specific arguments developed here focus less on paradigmatic issues and more on substantive debates in the study of comparative political development. Three interrelated themes that emerge during the review are notable at the outset. First, while the study of authoritarianism and democracy in developing countries is a lively research area, future research will benefit from being more genuinely comparative, especially across developing regions. It will additionally be important to situate the study of such processes of authority formation in their global context, both ideological and economic. Second, the study of authority structures will eventually need to move beyond the continuing categorization of regime types into democracies and authoritarians. While these are useful categories for some purposes, increasingly the variety within democracies will loom large. Moreover, it remains unclear whether democracies are good at facilitating such other-valued outcomes as economic growth and fair distribution. Other regime typologies—for example, predatory and developmental states that cut across the issue of democracy versus authoritarianism—may be as important for structuring research, especially if the focus is on issues of how state authority is used to promote or inhibit economic development. And third, the study of the state's role in economic development—again, a lively research area with important contributions—will benefit in the future by focusing as much on political as on bureaucratic variables and needs to be pushed toward asking where do differing state capacities to pursue development come from.

Prior to a full discussion, several caveats are also in order. First, I have for the most part avoided issues of the sociology of knowledge in this review essay. While these are of great interest, this is not the right place to worry

about, for example, how the end of the cold war might or ought to influence the comparative political study of developing countries. Second, the criteria by which it was decided to review some bodies of literature and not others, as well as to discuss some scholarly contributions in more detail than others, has an element of arbitrariness. The main criteria for inclusion were scholarly contributions that build on each other and thus create vibrant knowledge communities clustered around significant research problems. Scholarly significance was also important in deciding what to include and what to exclude. In order to minimize unnecessary “canonization,” however, it is also important to state up front that my criteria for inclusion were influenced by my ready familiarity with some literatures, personal tastes, and my intellectual networks. Another way in which inclusion is somewhat arbitrary is that not all the scholars whose works are discussed may be happy to be included in a “club” that they neither helped establish nor sought to join.

Third, while I pay some attention to paradigmatic battles, the focus below is on research and debates within a shared intellectual tradition. This is a matter of intellectual taste. There are those who like to worry about how to study a problem and there are those who study problems; I decidedly belong to the second group and hence my focus. While debates on first principles are important, they also often rehearse well-defined positions rather than produce genuine give and take. No wonder paradigmatic battles involve fewer intellectual issues and more political battles over the control of departments, personnel issues, and the life of the profession. And finally, among the criteria that I use for judging scholarship in this review are the importance of the questions raised, the intellectual sophistication with which they were handled—including originality and generalizability—and their impact on future scholarship.

■ | The State-Society Perspective

The intellectual lineage of the state-society perspective can be readily traced back to debates between Marx and Weber, in which, while taking Marx seriously, Weber argued that political, social, and ideational life could not be reduced to economic phenomena. Weber, of course, went on to demonstrate how ideas may influence economic and political outcomes; why authority and legitimacy structures varied, even within similar economic circumstances; and the autonomous significance of modern states and bureaucracies. Evident in this Weberian dissent was a preference for something less than an evolutionary and general theory of politics and society on the one hand and a related strong claim that state and society are analytically autonomous but mutually constitutive in the real world on the other hand (Bendix 1962, 286–90). These early intellectual posi-

tions eventually influenced the emergence of what is discussed here as the state-society perspective.

After World War II, when the comparative political study of developing countries gained popularity worldwide but especially in North American universities, such Weberian modesty was lost in the grand ideological and theoretical debates involving liberal and Marxist scholars. Many social scientists, including political science scholars of development, embraced the “totalizing” vision of structural-functional sociology and hoped that modernization theories would gain a paradigmatic status.² An occasional dissent, evoking the earlier Weberian caution against grand theorizing could be heard but was lost in the forward march of general theory building (Bendix 1967, 1973; Rudolph and Rudolph 1967). The real opposition to modernization theory thus came from dependency scholars, who, with their Marxist sympathies, were equally committed to an alternative set of grand theories.³

A new generation of scholars in the 1980s sought to distance themselves from what had arguably become a dialogue of the deaf between modernization and dependency scholars. “Bringing the state back in” was an important salvo in this intellectual development (Stepan 1978; Evans, Rueschemeyer, and Skocpol 1985). The central claim was simple but powerful, namely, that state structures and actions often enjoy considerable autonomy from the underlying social structures and ought not to be treated as socially determined. A variety of scholars, in turn, found this theoretical stance congenial: those with Marxist sympathies who felt that “economic determinism in the last instance” was a formulation that could not solve most of the analytical anomalies faced by Marxism;⁴ pluralist and other scholars of interest group politics who had noticed that state-structured interest groups—or corporatism—were significant determinants of political and economic outcomes (Schmitter 1974; Stepan 1978; P.J. Katzenstein 1985; Collier and Collier 1979); former modernization scholars who became dissatisfied with structural-functional political analysis;⁵ and, of

2. Among numerous critical reviews of this body of literature, see Huntington 1971.

3. For reviews, see, among others, Palma 1978 and T. Smith 1979. Note that this generalizing tendency was more true of someone like Andre Gunder Frank than of, say, Fernando Henrique Cardoso, whose claims were less grand, almost Weberian in his insistence on history.

4. The Marxist scholars who sought to save Marxism from growing anomalies included Althusser 1969 and Poulantzas 1973. For a review of Marxist theories of the state, see Wright, D. M. Gold, and Lo 1975. Among state-society scholars who might have been sympathetic to Marxism but distanced themselves, were Theda Skocpol and Peter Evans. Also interesting here is the contrast between the early and later work of Guillermo O'Donnell.

5. This took some time in coming but eventually came. See Weiner 1991; Price 1991. Also notice Huntington 1968—an early dissenter—whose work was quite statist from the beginning.

course, Weberian comparativists who had all along resisted economic and social determinism of political life (Bendix 1964, 1978). A neo-Weberian consensus of sorts thus quickly emerged. Even when some scholars felt that statism was going too far in neglecting societal variables, this dissent was often friendly and within the broad state-society framework.⁶ Numerous volumes employing a state-society framework thus emerged over the 1980s and the 1990s; arguably this framework became the preferred framework of most scholars working on the comparative politics of developing countries.⁷

Leaving aside this brief intellectual history sketch, how might one characterize the core theoretical elements around which the state-society perspective coheres? This characterization is not easy because the coherence, though real, is somewhat loose. There are few foundational texts. And the level of theoretical self-consciousness varies from study to study. Moreover, not everyone uses the same language: while many scholars use the categories of state and society, some prefer political elites and masses, others political elites and social classes, and yet others choose to break the macro concepts of the state and society into more specific components, such as leaders, ruling parties, regimes, and bureaucracies on the political side and into economic and noneconomic groups, upper and lower strata, organized and mobilized forces, or civil society on the social side. The results to the uninitiated may seem chaotic, but as we will see, they are not.

The first thing that distinguishes most scholars working in the state-society tradition is what they are not. They are not committed to working within and furthering research in one of the grand paradigms that seek to create very general theories and that compete for our attention. State-society scholars are thus neither Marxists (or dependency theorists) nor structural-functionalists (or modernization theorists) and, in terms of more recent debates, are often at odds with rational-choice scholars. While eschewing grand theory, most state-society scholars are very much social scientists, committed to generalizable knowledge—though often at a middle

6. See Migdal 1988 and Migdal, Kohli, and Shue 1994. Less friendly reaction along these lines may be Scott 1998.

7. An incomplete list of major book-length studies of comparative political development that employ a state-society framework would include the following: On **Africa**, see Herbst 2000 and 1990, Bratton and Van de Walle 1997, Young 1994, Price 1991, and Callaghy 1984. On **India**, see Weiner 1991, Rudolph and Rudolph 1987, and Kohli 1987. On **China**, see L. White 1998 and Shue 1988; an early precursor on China along these lines was Schurmann 1966. On **Latin America**, see Hagopian 1996, Collier and Collier 1991, Stepan 1978 and 1988, and Evans 1979. On **Northeast Asia**, see Woo-Cumings 1999, Woo 1991, Wade 1990, Amsden 1989, and C. A. Johnson 1982. On **Southeast Asia**, see Hutchcroft 1998, Doner 1991, and MacIntyre 1991. On the **Middle East**, see Waterbury 1983. For broadly comparative work, see Kohli forthcoming, Bermeo 2002, Collier 1999, Waldner 1999, Evans 1995, Waterbury 1993, Haggard 1990, Migdal 1988, and Skocpol 1979.

level, seeking to theorize not about all of politics but about more limited classes of political phenomena—and its empirical verification. This systematic commitment to seek causal explanations and to worry about their broader applicability also distinguishes the state-society scholars from those who seek to know what happened in specific empirical situations as an end in itself, as well as from scholars inspired by the literary turn in some of the social sciences that leads to a philosophical worry about causal explanation as a worthy end of scholarship.

More positively, state-society scholars generally share several important initial assumptions.⁸ First, while state-society scholarship has emerged as a reaction to social and economic determinism, state-society scholars continue to share the key assumption of modern sociology, namely, that social reality is *sui generis*. Societies and other social formations, according to this well-known standpoint, develop characteristics that are independent of the individuals who compose these groups, that often outlast the individuals who may have helped create the groups, and that often socialize the next generation of individuals who become part of the groups. As a result, social reality needs to be studied directly, rather than as a summation of numerous individual actions. It ought not to be necessary to state this basic assertion of Sociology 101, but for the growing popularity of economic theories that treat the individual as a basic unit of analysis and rest their hopes on developing macro understandings by aggregating micro insights. Political science, including comparative political study of developing countries, has always built on neighboring social sciences, especially sociology and economics. It is thus important to note at the outset that most state-society scholars are closer to sociology: they build on the assumption of social reality as *sui generis*, take for granted that social and political reality has enduring and thus structural qualities, seek to study them in their own terms rather as an aggregation of numerous individual actions, and remain suspicious of the claim that one can develop macro understandings from numerous micro beginnings.

The second important initial assumption concerns the autonomy of the political within the social sphere. According to this theoretical position, political structures and processes, though influenced by societal dynamics, are not determined by them. Or to restate it more strongly in the original Weberian language, state and society represent two analytically autonomous arenas of legitimate coercion and associational life respectively, that mutually condition each other in the real world (Bendix 1962, 286–90). The implications of this simple but powerful assertion, in turn, are of far-reaching significance. Negatively, they suggest that socially reductionist theories of politics are false starts, thus cautioning against the

8. Following a well-established position in the philosophy of social science, one does not ask of such assumptions whether they are right but how useful they are as building blocks of knowledge.

utility of such grand social theorizing. More positively, this starting assumption opens up numerous avenues of research for political scientists and political sociologists interested in explaining political outcomes that are influenced in part by social forces and in part by political factors. It thus ought to be no surprise that numerous debates in the comparative political study of developing countries that we will visit below concern the relative explanatory weights of the role of states versus social classes, of political versus economic variables, and of elites versus masses.

A related third starting point for state-society scholars is a shared belief that ideas and institutions on the one hand and economic interests on the other hand cannot be reduced analytically to each other; instead, they mutually condition each other, and must both be taken into account for satisfactory comparative political analysis.⁹ This initial assertion also has significant implications. For example, it follows that attempts to create materialist, or economic, theories of ideas and institutions are likely to be unsuccessful. Conversely, it is also the case that the tendency to view all interests as subjective constructs is an analytical blind alley. Instead of these more extreme positions, state-society scholars generally adopt more balanced assumptions: interests often have a subjective component, and given varying institutional configurations, there is always more than one path open for social actors to pursue their interests; and ideas and institutions that take root are often not at odds with the interests of the powerful in society.

And finally, state-society scholars tend to take history seriously. This seemingly banal assertion turns out on further reflection to be anything but that. Taking history seriously, of course, does not mean that all or even most state-society scholars undertake historically oriented research. Many scholars focus on recent developments and gather data, not from archives but via fieldwork. Taking history seriously suggests instead a shared belief that the past influences the present and, as a result, that satisfactory causal explanations must have a strong over-time component. This analytical standpoint follows in part from taking institutions seriously. As patterned beliefs and practices, institutions take time to root. Once rooted, institutions mold behavior. Political outcomes are thus partially path-dependent, influenced by institutions of an earlier origin, forcing state-society scholars to be sensitive to the impact of the past on the present.

Working at a middle level of theory and guided by such assumptions that state and society, as well as institutions and economic interests, mutually condition each other, has, of course, not led to the development of a single paradigmatic community but to the emergence of clusters of scholars with shared interests, who coalesce around specific research problems requiring explanation. This is because the main criteria for choosing a re-

9. I am indebted to Peter Evans for his help in clarifying this and the next general point in the discussion.

search problem for state-society scholars is not so much the utility of that research effort in building grand theory but the real-world importance of the problem. Theory for these scholars thus becomes a means for analyzing empirical puzzles deemed important, both by scholars and by members of the broader society. Generalizable insights that result from such research, in turn, set up future research aimed at confirming, challenging, or modifying these findings. State-society research on comparative political development has thus clustered around numerous significant questions that demand attention: Why do revolutions occur? Why is it so difficult to create viable states in some developing countries? Why have so many former authoritarian countries become democracies of late and what is their likely future? What types of states are capable of generating economic growth in the developing world? Under which conditions are states likely to do something to alleviate the worst of poverty and degradation so common in the developing world? The research I review here seeks to answer these and other significant questions. As noted already, I have organized the review in two parts: the first part focuses on the emergence of a variety of state structures in the developing world, and the second part reviews what we know about conditions of state efficacy for promoting development.

■ | Creating State Authority

Creating state authority that is coherent, enduring, and legitimate has always been a long drawn-out challenge, and it is no different in the developing world. Most of these states became states—or acquired a façade of a modern state—as a result of their encounter with colonialism. Among the characteristics of the European state system that Asia, Africa, and Latin America acquired, only the idea of a set boundary around a territory has proven to be resilient.¹⁰ How stable governance ought to be organized within these boundaries, however, has often proven elusive. For example, Latin American countries emerged sovereign in the nineteenth century and have struggled ever since to discover authority structures that will endure, especially swinging back and forth between authoritarianism and democracy in the post-World War II period. Whether authoritarian or democratic, most regimes in Africa have retained strong elements of personalism and patrimonialism. By contrast, Asian countries have done somewhat better. The two giants, China and India, created revolutionary and democratic states respectively that have more or less endured over the past half-century. In the remaining Asian countries, however, from the Ko-

10. For one discussion of why even weak states, such as in sub-Saharan Africa, have continued to maintain territorial boundaries inherited from colonialism, see Jackson and Rosberg 1982.

reas to Indonesia or from Bangladesh to western Asia, authority structures have proven to be just as brittle as in the rest of the developing world, needing to be held together by force, only to be challenged periodically for their lack of legitimacy, and again facing problems of coherence. The resulting political drama of round after round of failed efforts at institutionalizing authority has excited scholarly imagination and generated a very large body of literature.

As I review the contributions of state-society scholars to this literature, it is important to note at the outset that many of these studies are responding to or are at least informed by prevailing arguments of an earlier vintage. One powerful argument that frames the subfield is that stable democracy—the most desired form of coherent, enduring, and legitimate state authority in the contemporary era—is a product of economic development. The original argument was, of course, put forward by Marx when he suggested that bourgeois democracy was a product of the advanced economic system created by the bourgeoisie, namely, capitalism. A more recent and influential restatement of this Marxist position was made by Barrington Moore, Jr. (1966), in his claim, “no bourgeoisie, no democracy.” Seymour Martin Lipset’s widely cited modification of this Marxist argument (1959) expected all forms of economic development—whether capitalist or not—to lead to democracy, though the facilitating role of the middle class remained important in this argument as well. Whether in its Marxist or more liberal versions, this argument suggests that one should not expect stable democracy without an advanced economy. It can thus be argued that the failure to create coherent, enduring, and legitimate state authority in the developing world results from the economic condition they all share, namely, relatively low levels of economic development.

While many scholars have accepted this broad, framing argument, especially its validity at the two end points of low and advanced levels of economic development, just as many scholars have found it too general or the underlying theory not well specified or troubling in its inability to explain numerous departures from the norm, including the movements back and forth from authoritarianism to democracy. An early and also deeply influential such statement of dissent was Samuel Huntington’s *Political Order in Changing Societies* (1968). Very much in the spirit of state-society framework insofar as it sought to highlight the autonomy of the political and the autonomy of institutions, Huntington proposed that economic development did not guarantee democracy. On the contrary, economic development mobilized groups and individuals out of their traditional social niches and made them available for new political commitments. If this process of social mobilization was not matched by a parallel process of deliberately crafting such political institutions as parties, the demand of social groups would exceed the state’s political capacity for accommodation and response, leading to political chaos and decay. This argument was pro-

posed in the middle of the 1960s. Since many developing countries were then experiencing political instability, the argument found growing empirical support and also came to be widely embraced.

These two arguments concerning the determinants of authority structures in the developing world, one emphasizing socioeconomic variables, and the other the salience of political institutions, constitute the master narrative of the subfield. Numerous state-society studies on the formation of authority structures that followed were framed by these debates. While a few scholars chose to study the emergence of revolutionary states, and a few more the issue of failed states, most scholars focused on the dynamics that might help explain the movement from authoritarianism to democracy and back. When building their explanatory edifice, some scholars explicitly went back to Moore, Lipset, or Huntington, but many simply assumed that a key theoretical puzzle was to assess the relative salience of interests versus institutions and of socioeconomic versus political variables, including, of course, how the two conditioned each other. The results constitute an exciting body of scholarship that tends to cluster around a specific research problem to be explained.

REVOLUTIONARY STATES

Studies of conditions under which major social revolutions occur and revolutionary states are established have a long and distinguished history. Historians have pored through the records of individual revolutions, political philosophers have worried about their desirability, and political and social analysts have sought to generalize about their causes and consequences. Both the French and Russian Revolutions led to numerous such ruminations. Following World War II, the revolution in China and other near-revolution-like conditions in the emerging developing world attracted the attention of a variety of scholars. Not surprisingly, these studies were informed by prevailing theoretical standpoints, namely, class analysis and interest group politics, and by structural-functional approaches, though psychological theories of revolutions also enjoyed their moment.¹¹ From the standpoint of this essay, a seminal contribution in the development of this literature was Theda Skocpol's *States and Social Revolutions* (1979). Prior to *States and Social Revolutions*, revolutions were understood mainly in terms of underlying socioeconomic (or psychological) determinants, whether those were class conflict, value disequilibrium, unmet demands of interest groups, or relative deprivation experienced by the masses.¹² On the basis of detailed historical studies of major revolutions, Skocpol turned

11. One good and ready guide to the large and relevant literature is Goldstone 1980.

12. For a quick review of these approaches, see Skocpol 1979, ch. 1, 5–14.

around this entire explanatory edifice; she argued instead that it was the prior weakening of state structures that enabled a variety of latent social conflicts and rebellions to coalesce into major social revolutions.

Skocpol's *States and Social Revolutions* has been reviewed extensively and like most major works it has attracted both admirers and critics.¹³ There is no need here to go over this familiar ground. What is relevant here instead is the place of *States and Social Revolutions* in the development of the state-society literature, especially as it concerns developing countries. The first observation on this issue is that Skocpol was reacting to Barrington Moore's *Social Origins*. Moore had not only linked the emergence of capitalism to democracy but had also argued that cases of failed capitalism, especially where landowning elites failed to embrace commercial agriculture and became vulnerable to peasant revolts and even revolutions. Skocpol does not so much reject this argument—on the contrary, significant elements of it are embraced—as she modifies it by emphasizing the state's autonomous role. Her major contribution was to suggest that what all successful revolutions share is a prior weakening of the agrarian bureaucratic states. Under growing international pressures, some of these states fail to reform because of their dependence on landowning elites, crumble, and thus open up the political space for revolutionary forces. This argument fits well the three cases of France, Russia, and China that Skocpol discussed in detail and some other cases as well. Just as Weber had pluralized Marx's economic argument and made it much more broadly applicable and attractive—though by the same account, also less parsimonious—so Skocpol pluralized Moore, or made the analysis more multivariate, for the study of grand revolutions.

The second observation concerns the impact of *States and Social Revolutions*, both on the broader literature in comparative political development and on, more specifically, the future study of revolutions. The broader impact was considerable; *States and Social Revolutions* was one important stream among several that joined the flooding river that eventually became the state-society perspective. Its key general insight—leaving aside the heroes and villains of the story, as well as their political implications—was somewhat similar to that of Samuel Huntington in *Political Order*, namely, that political order results when state power is effectively grafted on top of social power. When for one reason or another this state power weakens or disintegrates, it then becomes possible to mobilize around inequalities of power in society with the hope of creating a new and revolutionary order. This general standpoint enabled Skocpol to argue effectively for the need to “bring the state back in,” especially in an intellectual setting dominated by socioeconomic determinism.

The more specific impact of *States and Social Revolutions* on the fu-

13. See the references to the critiques of Skocpol's work and her responses in Skocpol 1994.

ture study of revolutions was even more significant. It essentially set the agenda for the next generation of scholars interested in revolutions in the developing world. Some scholars criticized it for ignoring leadership ideologies and built their own edifice (Colburn 1994). Others criticized it from a rational choice perspective, arguing either that Skocpol underestimated the strategic behavior of revolutionary leaders or that what was missing were the micro foundations of revolutionary behavior (DeNardo 1985; Taylor 1988). Especially relevant for the purposes of this essay, however, were the contributions of scholars who sought to extend the statist perspective to the study of revolutions in the developing world.

An important recent contribution in this domain is Jeff Goodwin's *No Other Way Out* (2001).¹⁴ Goodwin seeks to explain comparatively all revolutionary movements—and a few successful revolutions—in the developing world during the cold war period. While focusing especially on Southeast Asia and Central America, he extends and modifies Skocpol's analysis to explain more recent revolutions and near-revolutions. Among the explanatory factors he emphasizes, two are central. First, revolutionary movements were generally a response to repressive, exclusionary authoritarian states that were relatively weak in the sense of being incapable of executing state authority, for example, personalistic dictatorships. Second, some of these states were especially vulnerable because they themselves helped create revolutionary movements that were able to further weaken these already weak states. While often persuasive, one wonders how Goodwin's analysis would have to be modified against cases from sub-Saharan Africa, where personalistic weak states have often led to inconclusive civil wars rather than to successful revolutions. Nevertheless, among the important implications of Goodwin's argument is that democratic and well-organized modern states may not be vulnerable to revolutions.

The literature on revolutions has clearly been additive. Instead of the earlier emphases on class inequalities or on the role of grand ideologies and legitimating value structures, recent scholarship suggests that the organization of state authority is the critical variable in understanding both whether revolutionary movements develop and whether they succeed. We know now, for example, that revolutions in the developing world do not seem to occur within states that are quite legitimate, for example, democracies, or within those which can organize repression effectively, for example, bureaucratic-authoritarian regimes. Instead, sites of revolution have been mainly highly illegitimate patrimonial regimes (as, for example, in Central America) or colonial regimes made ineffective due to special circumstances (as, for example, in Vietnam). The state's capacity to legitimate itself or to exercise legitimate coercion thus emerge as a key variable in understanding contemporary revolutions.

14. Another useful study which I chose not to discuss for reasons of space is Wickham-Crowley 1992.

In concluding this discussion on revolutionary states, it should be noted that this literature lost some of its vitality in the 1990s, especially among political scientists. The underlying reasons are not obvious. It may be because the prospects of more grand social revolutions appeared dim, and because some former revolutionary states like the Soviet Union collapsed and others like China and Vietnam metamorphosed into single-party authoritarian regimes with market economies. The idea that revolutions offer a shortcut to freer and better societies thus lost most of its elan in the late-twentieth century. While reinterpretation of past revolutions will continue to attract scholarly attention, comparative political development scholarship is more often moved by emerging real-world trends. An important analytical question that one is left with then is this: If the era of grand social revolutions is indeed over, why? What has changed in the global setting, in the organization of states, or in the patterns of social relations within states that would help explain this significant change? Following Skocpol, is it possible that the spread of the modern state—in lieu of agrarian bureaucracies, that is—with its enhanced bureaucratic and coercive capacities—even in democratic settings—has essentially closed off political spaces in which revolutionary challenges to the existing social order could be launched?

BUREAUCRATIC AUTHORITARIAN STATES

Authoritarianism in the developing world comes in various forms. Monarchy in Saudi Arabia, personalistic rulers like Mobutu in Zaire, single-party rulers of Mexico, and military rulers like those who governed Brazil in the 1970s or Park Chung Hee of South Korea have all at one time or another been labeled authoritarian. Without a qualifier, authoritarianism is thus mainly a residual category—all regimes that are not democracies—and as such is too amorphous to be analytically useful. These regimes emerged in different circumstances, are (or were) organized differently, often rest(ed) on a variety of underlying social coalitions, and are (or were) more or less efficacious in promoting development. To develop generalizable knowledge about their origins and performance, therefore—knowledge that is not so general as to be totally uninteresting—scholars have often focused on subcategories of authoritarianism.

While a useful literature exists on both personal rule and on such other long-enduring authoritarian states as the “rentier states,” the type of authoritarianism that has generated most theoretically informed debates is the bureaucratic-authoritarian regime.¹⁵ Such regimes often came to

15. For one useful typology of authoritarian regimes, see Chehabi and Linz 1998. On personal rule—mostly in the African context—especially on neopatrimonialism, see Clapham 1982. Also see Bienen and Van de Walle 1991. Notable contributions on the “rentier state” include Karl 1997 and Chaudhry 1997.

power after the breakdown of democratic experiments and resulted in the installation of authoritarian regimes run by bureaucrats—mainly military and technocratic elites—who promised orderly and rapid development. A number of such regimes came to power in the 1960s and 1970s, especially in Latin America but also in such other countries as South Korea, Indonesia, and Pakistan. The book that set the agenda for the study of these types of regimes was, of course, Guillermo O'Donnell's *Modernization and Bureaucratic-Authoritarianism* (1973).

O'Donnell's argument suggested that democracy in such South American countries as Brazil and Argentina was compatible in the 1950s with easy import substitution, a type of development model that rested on protected foreign corporations producing consumer goods for the rich elite of these countries. Once elite demand was saturated, however, this easy phase was exhausted, and what was needed next was deepening of industrialization, or a movement away from consumer goods to heavier industries. Capital and technological requirements for the latter were severe, requiring forceful intervention from the state to remove such bottlenecks as low rates of domestic savings, labor activism, inflation, and limited foreign investment. An authoritarian and interventionist state, led by a military and technocratic elite, thus became somewhat of a functional necessity, helping explain their rapid emergence in numerous middle-income countries that had undergone some industrialization but were struggling to forge ahead further.

This brilliant and highly suggestive political economy argument for authoritarianism demanded empirical scrutiny and indeed received it during the 1970s and the 1980s.¹⁶ As one might expect, the scrutiny produced mixed results for the validity of the argument. Scholars with deep familiarity with significant individual countries like Brazil noted that numerous heavy industries were already in place prior to the 1964 military coup, and thus the functional need to deepen industrialization could not have been a major cause propelling authoritarianism (Serra 1979). Had O'Donnell and other Latin American scholars looked to a country like India, they would have also readily noticed the comfortable coexistence of democracy with heavy-industry-based import substitution. The deepening argument was thus found to be of questionable merit. By contrast, if O'Donnell's argument was understood more broadly to suggest that authoritarianism emerged as a way of coping with growing demands that fledgling democracies were unable to meet because of their political incapacity to remove a variety of bottlenecks—an argument that would be quite similar in spirit to the one proposed by Samuel Huntington in *Political Order* (1968)—then experiences of several Latin American countries provided supporting evidence.¹⁷

16. The most notable attempt was Collier 1979.

17. See Collier's conclusion in Collier 1979. Also see James Mahoney's very useful review of these materials (2000), which helped me formulate some of my thoughts.

This broader interpretation of course shifted the focus of analysis away from political economy concerns and toward more political factors, such as the role of growing polarization along class lines or elite-mass lines in precipitating military coups. Detailed analysis within Latin America led to the view that there were several patterns of authoritarianism, even within the region. Karen Remmer's more quantitative work (1989) disaggregated O'Donnell's hypothesis significantly, and David and Ruth Collier undertook comparative historical analysis in their majestic study, *Shaping the Political Arena* (1991), arguing that early patterns of labor incorporation had a significant long-term impact on party and regime dynamics. Since issues of breakdown of democracies go well beyond Latin America, scholars interested in such diverse middle-income countries as South Korea and Turkey also sought to extend O'Donnell's argument to make sense of authoritarianism in their respective parts of the world (Im 1987; Richards and Waterbury 1990).¹⁸

With the onset of the "third wave" of democracy, some of the interest in "why authoritarianism" may decline in the future but is not likely to go away. This is in part because past authoritarian arrangements continue to cast long shadows in many new democracies and in part because many of these new democracies remain vulnerable to future breakdowns. Moreover, reinterpreting the past is not only the domain of historians; more recent history continues to fascinate political scientists as well. Notice, for example, the important new book by Nancy Bermeo (2002). In this study Bermeo is troubled by a variety of past analyses of why democracies breakdown, especially Sartori's, but including Huntington's and O'Donnell's, that implicitly or explicitly implicate the popular sectors in such breakdowns. She thus undertakes a comparative analysis, first of interwar European cases and then in much greater detail of several Latin American cases from the 1960s, to examine the proposition that emergence of authoritarianism was preceded by mass disaffection and popular protest. On the basis of meticulous research, she documents with great care that such was not the case. She thus suggests that the threat of popular sectors has been exaggerated; installation of authoritarian regimes has to be understood instead in terms of ideologies and interests of the ruling elite.

It ought to be clear that O'Donnell's arguments about the origins of authoritarianism in the 1970s initiated an entire research program of studies that built on each other and have led to considerable cumulation of knowledge. As a result, we now know that the choice of the broad economic development strategy probably had little to do with the onset of authoritarianism, that the roots of military coups were more likely political, that these political patterns are probably influenced both by varying levels of economic development and by varying past political histories, but that

18. Another work—with earlier European cases as inspiration—that I have not discussed here was Linz and Stepan 1978.

in trying to understand this politics, one also ought not to exaggerate the role of popular protest. One is thus left with other political issues that still require further research: the roots of weak democratic institutions to begin with; the relative proclivity of military elites to intervene more readily in some periods than in others; the role of threat perception when authoritarian elites intervene, especially as this perception is influenced by prevailing ideologies; and the role of international factors. Future research on these and related issues will definitely benefit from more cross-regional research. Latin American materials have played a central and useful role in the development of this literature, but Latin American literature also suffers from an “almost Europe” regional bias. If one genuinely wants to understand the origins of more-bureaucratized authoritarian rule in developing countries, future research efforts ought to include such cases as South Korea and Turkey, as well as pursue such comparative questions as: Given two cases like India and Pakistan, with shared histories of British colonialism, why does Pakistan experience more frequent military coups than India?

DEMOCRATIC STATES

The reemergence of democracy in a variety of developing countries over the 1980s gave rise to an enormous body of literature.¹⁹ The central question motivating this research concerned the conditions of democracy in developing countries, especially factors that help democracies emerge but also conditions under which democracy might endure and flourish. Scholarly responses can be conveniently divided into those which emphasize socioeconomic variables, those which underline the significance of such political variables as leadership and institutions, and, of course, those which seek to provide synthetic accounts.

Relevant socioeconomic factors that may influence democratic processes can and have been conceptualized differently: some scholars think of them in terms of social classes, others in terms of levels of economic development, and yet others in terms of associational groups in civil society. Each of these conceptualizations has stimulated its own school of research. First, the focus on class analysis that Moore reintroduced to the study of comparative democracy has found a variety of expressions. The role of agrarian classes in the making of democracy, for example, was recently reemphasized in a fine study by Deborah Yashar (1997). Other scholars have pushed the argument in different directions. One important modification was the coauthored volume by Rueschmeyer, Stephens, and Stephens, *Capitalist Development and Democracy* (1992). On the basis

19. The literature is so large that an attempt at providing all the relevant citations would be foolhardy. For some recent reviews of this literature, see Shin 1994, Geddes 1996, and Mahoney 2000.

of a wide variety of quantitative and case materials, especially from historical European and Latin American cases, Rueschmeyer, Stephens, and Stephens emphasized the role of the working class in both broadening and deepening democracies. While commercialization of agriculture may remove some important antidemocratic forces from the historical scene, they suggest that the type of democracy preferred by capitalists is limited democracy. It takes workers in alliance with various other popular-sector actors to push for full suffrage and to give democracy some substantive depth.

Such a provocative argument was not likely to remain unchallenged, and it was not. Ruth Collier in a recent book (1999) takes issue with the Rueschmeyer-Stephens-Stephens thesis that workers are vanguards of democracy. While giving the working class its due, especially in contemporary Latin America, she undertakes detailed historical case studies to argue that Rueschmeyer, Stephens, and Stephens did not quite get it right; besides workers, critical forces that supported transitions to and consolidation of democracy were political, especially the roles of elites and parties. This argument is considerably less parsimonious than that of Moore, but by the same token, it also seems to better accommodate historical complexities and contingencies. A similar contingent tone is notable in Eva Bellin's recent important works (2000, forthcoming). She suggests that the roles of capitalists and of workers in promoting democracy in developing countries is contingent on concrete circumstances, especially their prior dependence on the authoritarian state.

Second, the old Lipset argument that economic development promotes democracy has experienced something of a comeback in recent decades. A number of scholars have noted that the correlation between higher levels of development and democracy continues to hold very well, and this some five decades after the original argument was proposed by Lipset (Diamond 1992; Lipset, Seong, and Torres 1993; Przeworski and Limongi 1997; Przeworski, Alvarez, Cheibub, and Limongi 2000). However, what should one do with this correlation, especially in light of the fact that the original theory underlying the correlation was not very strong and that a variety of countries, at different levels of development, have recently started again to experiment with democracy? Are countries at low levels of development condemned to remain undemocratic? What is one to do with the Indian case, which may only constitute an n of 1 but which with a billion people living democratically within it, throws up a substantial challenge to this view: Can India's experience not be replicated in a Nigeria? Why not?

Faced with both normative and analytical dilemmas, scholarly research has proceeded in several directions. Przeworski and Limongi (1997), for example, distinguish usefully between issues of transitions and survival; countries can transit to democracy at a variety of levels of development—because transitions depend on contingent factors, including

leadership choices—but the chance that democracy will survive is a direct function of the level of economic development.²⁰ Diamond, Linz, and Lipset have organized three massive edited volumes which seek to preserve the core of the old Lipset argument but which also pluralize the argument enough to accommodate a wide range of cases.²¹ And finally, Samuel Huntington (1991) has provided an evocative reformulation of the theme: while countries at low and high levels of development may indeed be associated with authoritarianism and democracy respectively, countries in the middle exist in a “zone of choice,” where a number of identifiable factors can influence the nature of national authority structures.

And third, the Tocquevillian emphasis on associational life as a key ingredient of democratic life has also in recent years found renewed interest. The most important restatement of this position is, of course, Robert Putnam’s historical study of Italy, *Making Democracy Work* (1993a). Putnam’s well-known argument is that democracy in northern Italy functioned better than in the south because the north has for a long time enjoyed relatively dense associational life that promotes citizenship, civicness, and a sense of democratic efficacy. In the context of contemporary developing countries, similar arguments have been proposed for transitions, suggesting, for example, that civil society activism was an important ingredient pushing for democratic openings.²² Moreover, the Putnam hypothesis relating the quality of democracy to the density of associational life has been modified to explain democratic deepening in specific cases and has given rise to a number of important research projects on developing countries.²³ Critics, of course, have hardly left this argument unchallenged. Aside from methodological issues, an important substantial criticism, for example, proposed by Sheri Berman (1997b) is that associational activism can just as easily be a source of breakdown of democracies as of their superior functioning; much depends on the quality of such institutions as political parties that mediate the relationship between demanding groups and the state.

This emphasis on political variables is also evident in a significant body of related literature. For example, several volumes that received quite a bit of attention sought to explain the relatively sudden transition to democracy in the 1980s, especially in Latin American countries (O’Don-

20. Przeworski (2001) has also recently undertaken the more ambitious task of building a theory that may help explain the correlation of wealth and democratic health.

21. See Diamond, Linz, and Lipset 1988. The first of the four volumes sets a general framework, the latter three deal with Africa, Asia, and Latin America respectively.

22. On Brazil, see, for example, Alvarez 1990.

23. See, for example, Varshney 2002. Also note that the Swedish International Development Agency financed a substantial project along these lines on India and South Africa. For early results, see Blomkvist et al. 2001.

nell, Schmitter, and Whitehead 1986a, b, c; O'Donnell and Schmitter 1986). Since some of the contributors to this volume had argued earlier that authoritarianism was rooted in structural factors, the sudden emergence of democracy posed a significant intellectual puzzle. The resolution, if it can be called that, was to emphasize the role of such voluntaristic variables as leadership. While it would be an oversimplification to suggest that, according to this literature, leaders can will democracy if and when they so wish, there is no doubt that the focus of this literature was on democracy from above. The argument ran that the transitions from authoritarianism to democracy often resulted from divisions among leaders, as a result of "pacts" among incumbent and opposition elites, or due to the changing cost-benefit calculus of the ruling elite. Numerous case studies, in turn, supported this general orientation.

It would be easy to criticize this literature as rather atheoretical, depending too heavily on voluntaristic acts of leaders for an explanation. And a number of authors did precisely that.²⁴ Others went on to develop their own explanations of transitions instead, emphasizing, for example, the role of economic crises.²⁵ While these criticisms and alternate explanations are valuable, three other observations need to be made. First, to the extent that leaders always encounter structural constraints and learn the limits of the possible through a process of trial and error, leadership actions are not only a voluntaristic variable. Second, the emphasis on the role of leaders originated from a deep understanding on part of scholars like O'Donnell that the shift from authoritarianism to democracy—though an important and normatively desirable change—was also a relatively limited change, especially if viewed from the standpoint of broader socioeconomic changes.²⁶ Put bluntly, transitions were not revolutions and did not involve any profound structural change in the social location of power. How much of a role should then be attributed to underlying structural variables? This leads to a third observation. The question of "why transitions," though important, is also a little too narrow to lead to theoretically interesting answers. The analytical focus ought to include what follows the transition, namely, the quality of governance offered within the new democracies.

A longer-term perspective on the building of new democracies naturally shifts attention away from leadership actions and toward more institutional concerns. This institutional literature on developing country democracies has gone in several directions. First, there is a growing realization that the authoritarian past casts a long shadow on new democracies. For example, powers of militaries do not vanish in new democracies, and if the authoritarian rulers were essentially personalistic, such as in many sub-

24. See, for example, Remmer 1991a.

25. See, for example, Haggard and Kaufman 1995.

26. See, for example, O'Donnell's subsequent essay (1994) and Kohli 1993.

Saharan African countries, new democracies are hardly able to transcend the problems of patrimonialism (Linz and Stepan 1996; Bratton and Van de Walle 1997). Second, a debate of sorts has emerged on whether a parliamentary or a presidential system of government is more suited to help new democracies stabilize.²⁷ This literature would benefit from a greater sensitivity to underlying issues of social power, though, to be fair, issues of ethnicity have received some attention (Horowitz 1985). And finally, the study of such exceptional cases as India, in which democracy has survived, provides a variety of insights into the role of institutions, not only in the transition to democracy but also in its consolidation and in the reemergence of problems of governability, when underlying institutions weaken (Kohli 1991, 2001).

Before concluding this section, a note should also be made of a couple of volumes that are distinguishable, not for accentuating one specific argument or another but for providing fairly balanced, synthetic accounts. One such important work is Samuel Huntington's *The Third Wave* (1991). Huntington in this volume seeks to develop a multivariate explanation for the "third wave" of democracy. The result is a study of considerable value for a general readership. Another important volume by Haggard and Kaufman (1995) is for a more-specialized audience but is also synthetic in ambition. It provides a balanced account of both transitions, especially emphasizing the role of economic crises that preceded them, and of the troubles faced by new democracies as they pursue a neoliberal economic agenda.

It again ought to be clear that the issue of democracy in the context of developing countries has given rise to a rich and complex state-society literature, the works of which often build on each other. In conclusion, one may thus ask, what has been learnt, and where should the literature go? First, the focus on leadership has proved to be a tad too narrow; it may be useful for understanding transitions to democracy—though even this has been challenged convincingly—but certainly needs to be broadened for issues of longer-term significance. Second, the study of consolidation of democracies is likely to be best framed by a simultaneous focus on socio-economic and institutional factors on the one hand and to be best conducted in a broader, comparative context on the other hand. For example, the literature seems to suggest that more-advanced developing countries, with moderate inequalities and with well-organized political parties, may be in the best position to consolidate democracies.

The issue of what happens when one or more of these key ingredients are missing, ought to be investigated in a cross-regional context; scholars of Latin America, for example, often do not know much about what is happening in India or Africa, and vice versa, and are poorer for that neglect.

27. See Linz 1990a, the subsequent debate between Linz (1990a, b) and Horowitz (1990) on "Presidents vs. Parliaments," and Lijphart (1991, 1992).

And finally, the deepening of democracy—that is, of its embrace by the popular sectors—has not received much attention. Are new democracies strengthened or weakened by inclusionary politics and economics? How does globalization impact such classic dilemmas faced earlier by democracies that are by now well established? Answers to such questions also ought to be developed by cross-regional research that simultaneously focuses on the role of political institutions on the one hand and on the role of class and other forms of associational activities on the other hand.

■ | Using State Authority

States in most developing countries play a significant role in structuring their respective societies and economies. This “state-heavy” characteristic was in some cases inherited from a colonial past but for the most part was a product of ideas and interests of sovereign rulers, especially their perceptions concerning how to maintain order and to promote economic development. The sizable and interventionist states, in turn, have molded a variety of political and economic processes in the developing world, including interest group politics, patterns of class and ethnic cleavages, the nature of political institutions, and, of course, rates and patterns of economic growth. The analytical emphasis of state-society scholars on the “autonomy of the political” has brought this state’s architectonic role into sharp relief, resulting in important new research over the last two decades. I first review the research that sheds light on the modes in which states structure the society and politics of developing countries; while individually insightful, these researches are often only loosely connected to each other. By contrast, the researches on the states’ role in promoting or hindering economic growth that I review second build on each other more self-consciously and have generated an influential set of cumulative findings of real-world consequence.

HOW STATES STRUCTURE SOCIETY AND POLITICS

A variety of factors give states and state elites some degree of autonomy from underlying social forces in developing countries: the capacity to define and pursue the national interest; the exercise of legitimate or not-so-legitimate coercion; the tendency of political ideas, organizations, and institutions to survive the initial forces that brought them into being; the weakness of organized social forces; and, of course, the needs of ruling elites to prolong their hold on power. Partially autonomous states, in turn, have often structured the nature of, and relations among, socioeconomic groups in developing countries, with long-term political significance. Examples of scholarly works that underline the fecundity of this perspective

can be conveniently grouped into those that focus on how states have structured interest groups and institutions, ethnic politics, and distributional politics along class lines.

Two important books on Latin America, one by Alfred Stepan (1978) and the other by Ruth and David Collier (1991), demonstrate the state's impact on interest group politics, especially labor politics, and thus on the evolution of long-term political trends in the region. Among scholars of political development, Stepan was one of the earliest to emphasize the analytical autonomy of the state and to demonstrate the empirical utility of this standpoint. He suggested that Latin American states could be usefully categorized as more inclusionary or more exclusionary in their mode of labor incorporation. The nature of state-labor relations, in turn, was consequential for a variety of political trends. For example, he hypothesized that Mexico's relatively stable political evolution was based on inclusionary state-labor relations, whereas the more exclusionary model of Brazil generated political polarization and regime instability. Stepan of course also went on to examine a number of other statist hypotheses in greater detail against Peruvian materials.

David and Ruth Collier built on the type of framework suggested by Stepan and also emphasized the "autonomy of the political" as they compared and contrasted state-labor relations in the more-industrialized countries of Latin America. Their main categorization was similar to Stepan's but emphasized the distinction between state-controlled labor and labor mobilized and controlled by parties. If Brazil and Chile are examples of state corporatism, Argentina and Mexico are more cases of political parties' taking the lead on the labor question. Among their important arguments is the claim that party-led labor mobilization may invite reaction over the short run but is also likely to lead to longer-term integration of labor in the political system and thus to smoother political evolution. The impact of this important book by the two Colliers is already significant and is likely to grow over time as students are influenced by the general standpoint and digest the details. Aside from the framing importance of the concepts of path dependence and critical junctures—subjects best left for another discussion—the emphasis on the "autonomy of the political" was much needed in Latin American studies and is already bearing fruit. Notice, for example, a fine new book by Richard Snyder (2001) that demonstrates how neoliberal economic policies in Mexico have only created new needs for political management and thus given rise to a new form of statism in that country.

A similar statist standpoint informed my book on India, *Democracy and Discontent* (1990). In *Democracy and Discontent*, I argued that the weakening of such political institutions in India as the ruling national party had created an opening for personalistic and populist leaders to concentrate enormous powers, further deinstitutionalizing the polity, mobilizing demands that could not be readily met, and creating problems of governability. Unlike Huntington, however, a more explicit statist standpoint led

me to suggest that political mobilization was more a product of elite initiatives and less of ongoing socioeconomic development and that deinstitutionalization also resulted more from the self-serving behavior of leaders and less from demands outpacing associational regrouping. Other younger scholars have followed up this argument, subjecting it to rigorous quantitative examination, confirming parts of the analysis but also questioning other parts, leading to important modification.²⁸

A broadly comparative book that sought to analyze state capacities to achieve a variety of social goals in developing countries was Joel Migdal's *Strong Societies and Weak States* (1988). As the title suggests, Migdal argued that most states in developing countries were inefficacious and that the roots of this inefficacy lay in the presence of powerful social actors who managed to defy state edicts. Migdal's case materials in this influential book were mainly from sub-Saharan Africa and the Mideast, with the case of Israel's providing a check on the argument insofar as exceptional national security considerations there have helped create a much more efficacious state. From these inductive propositions, Migdal also argued for a broader analytical need to have a more-balanced state-society focus.

The focus on the state's structuring role has also influenced research on identity politics, including research on nationalism, race, and other forms of ethnic politics. A major contribution here was Benedict Anderson's *Imagined Communities* (1991 [1983]). In a subtle and brilliant book, Anderson persuades or provokes his readers less by rigorous hypothesis testing and more by his intuition and his historical and geographical sweep. If modern nationalism is understood as a social-psychological force that enables sizable human groupings to imagine themselves as communities that share and cherish important traits, Anderson's study provides nothing less than an analysis of the political and economic changes that undergirded the rise of modern nationalism. While he emphasizes a range of such factors, including such economic ones as the rise of capitalism and the related emergence of profit-oriented print technology, the state's role in constructing modern nationalism also looms large in his analysis. Thus, for example, roaming bureaucrats, traveling from place to place within specified political boundaries, became a source of national consciousness in Latin America, even prior to its emergence in western Europe. The use of such ruling instruments by states, including colonial states, as the census—needed to categorize citizens for a variety of tasks, including taxation—provoked individuals and communities to develop both national and subnational identities. And, of course, states that arrived late on the global scene, such as Asian and African states, undertook numerous deliberate state-led efforts to construct national identities, with more or less success.

Anderson's emphasis on the constructed nature of identities has inspired an enormous amount of research, especially by historians and an-

28. See, for example, Wilkinson 2001.

thropologists, who with their own epistemological assumptions are often less interested in causal generalizations than political scientists. The issue of how states construct identities, however, is important from a variety of standpoints and has hardly gone unnoticed by political scientists. A significant recent study, for example, is Anthony Marx's *Making Race and Nation* (1998). Marx undertakes a comparative analysis of Brazil, South Africa, and the United States around the issue of political determinants of racial stratification. South Africa for Marx provides a case at one extreme of legal racism, and Brazil with its more moderate social racism at the other end of the continuum, with the United States somewhere in between, though closer to South Africa. On the basis of a detailed historical analysis of each case, Marx explains these variations in racial patterns as a product of intraelite politics at an early stage of state building: the need of white elites to maintain unity and stability led them to legalize apartheid in South Africa, and by contrast, a moderately consensual and less-threatened elite created a more inclusive pattern of racial stratification in Brazil. This provocative macrohistorical hypothesis is likely to provoke future research that seeks to confirm these postulated relationships with reference to other cases or that challenges the proposed relationships while reinterpreting the same or other historical cases.

Another important study in this genre is Deborah Yashar's *Contesting Citizenship* (forthcoming). This is a detailed comparative analysis of the emergence of indigenous movements in Latin America, especially in Ecuador, Bolivia, and Peru. The study asks, why have indigenous movements emerged in Latin America in recent years and, within that frame, why are they more significant in cases like Ecuador and Bolivia but less significant in a case like Peru? The answer Yashar develops revolves around the changing role of the state in Latin America. The shift from corporatist to neoliberal arrangements has simultaneously reduced the autonomy and security of communities and enhanced their political spaces to organize dissent. Indigenous movements have emerged where this broader political change came to be combined with preexisting dense social interactions within indigenous communities. By contrast, where peasant networks were weak and national political opening was limited, indigenous movements have remained relatively insignificant.

Finally in this survey of recent research on the state's impact on the society and politics of developing countries, one may take note of some work on state's distributive role. In my 1987 book, *The State and Poverty in India*, I sought to analyze the conditions under which state intervention aimed at alleviating rural poverty in India succeeded or failed. On the basis of comparative regional materials within India, I identified the ideology and organization of the ruling parties on the one hand and the social base of the party's power on the other hand as the key variables that helped explain variations in redistributive outcomes: a well-organized social democratic party in power with a middle- and lower-class base, I argued, was

most successful at prioritizing and pursuing antipoverty policies. This argument and research has subsequently been challenged, modified, or extended to other parts of the world by a number of other scholars.²⁹

Besides India—the country with the largest number of poor people in the world—the other country around which state's distributive role has received considerable attention is the world's most unequal country, namely, Brazil. A recent study, for example, investigates the impact of democratization on equity (Weyland 1996).³⁰ Focusing on such specific policy areas as taxation and social security, Weyland analyzes why even democratic governments in Brazil have not been able to pursue policies that may benefit the lower classes. His answer focuses on organizational fragmentation in Brazil, including the absence of well-organized political parties that could aggregate the interests of the poor, as well as fragmentation within the state that weakens the capacity of state elites to push against the interest of dominant classes. Weyland thus continues an old but important insight that is worth reiterating, namely, that democratization does not necessarily translate into enhanced political power for the poor; the latter requires deliberate recrafting of political parties and of the state apparatus.

To sum up, these are just some examples of the type of research that has sought to demonstrate how states structure the society and politics of developing countries. While seemingly disparate—both by geographical and by topical focus—this research shares a theoretical unity insofar as the causal arrows here generally point from the state to society. Moreover, within more specific problem areas, such as corporatism, ethnic and race patterns, or welfare provision, researchers consciously build on each other's findings, debating and modifying existing propositions in the manner of "normal science." Some of this research would clearly benefit in the future by pursuing problems across regions. Nevertheless, the cumulative findings are already impressive. For example, we know by now that patterns of political inclusion or exclusion—whether along class or ethnic lines—were often structured by states themselves and often originated much earlier. Only comparative and historical research is likely to unearth such patterns and their causes and consequences. As another example, we also know that deliberate political attempts within the developing world to reduce inequalities or alleviate poverty have often been not very successful, hampered by political fragmentation, especially the absence of well-institutionalized left-of-center ruling parties. What role can states then play in redistributing a developing country's already scarce resources? While

29. For one direct challenge, see Mallick 1993. For a recent attempt to extend the argument see Harris 2001. For other modifications and extensions, see Ranade 1991, Kuhonta forthcoming, and Rani Tudor forthcoming. On related subjects, see Herring 1983, Echeverri-Gent 1993, and Heller 1999.

30. A study of related interest that finds elements of the Brazilian state more efficacious is Tendler 1997.

further research on this question is clearly important—especially in the context of countries with good economic growth and serious inequality—the discouraging record on this front has also contributed to a shift of focus away from the state as an agent of redistributions to the state as an agent of economic growth.

STATE AND ECONOMIC GROWTH

While sharing statist assumptions, the political economy literature on the state's role in promoting or hindering economic growth has developed in dialogue with the literature on economic development, especially the more policy-oriented literature. During the 1950s and the 1960s economic development was a robust subfield of economics that underlined the market imperfections prevailing in the developing world and often emphasized the positive role that the state could play in overcoming these imperfections.³¹ Involved in their own disciplinary debates, political scientists unfortunately—and ironically—did not contribute to this literature; the positive role that states may play in promoting economic development thus remained undertheorized, and the conditions that may support such interventions poorly understood. A new generation of economists from the 1970s onward abandoned the old assumptions and prescriptions of development economics and increasingly came to embrace a neoclassical worldview that emphasized the proper functioning of self-regulating markets as the source of rapid growth and development. The recent statist literature that I now discuss has generally developed in reaction to this pro-market orthodoxy, shared by some scholars and practitioners of development.

The critique of development economics developed by neoclassical economists from 1970s onward began mainly as a critique of import substitution trading regimes and eventually by the 1990s became a full-blown critique of state intervention for development. It was initially proposed that tariffs (or quotas) and overvalued exchange rates that were often at the core of import substitution development strategies hurt economic growth because they misallocated resources, discouraged exports, and by the same token, limited much-needed imported technology. A more neutral trading regime, following the logic of comparative advantage, would instead lead to labor-intensive exports and higher growth rates. Large amounts of empirical materials were brought to bear on these arguments, especially the relatively high economic growth rates achieved by export-promoting East Asian countries on the one hand and the economic performance of import-substituting economies, sluggish in South Asia and Africa and crisis prone in Latin America on the other hand. The debt crisis of the 1980s not only gave urgency to this critique but also weakened the bargaining posi-

31. See, for example, Hirschman 1981.

tion of many developing countries vis-à-vis the developed countries to whom the debt was often owed. The external pressure to liberalize debt-ridden economies further focused attention on a variety of inefficiencies that could be attributed to interventionist states. These included bloated public sectors, government controls on economic activities, the urban bias of national pricing regimes, and numerous public subsidies; the resulting “Washington consensus” thus offered to all developing countries a more liberal model of development in place of the older model that depended heavily on *dirigism*.³²

The statist response to this neoliberal critique involved a wide variety of social scientists and took several forms, especially a reinterpretation of East Asian success but also broader comparative analyses and quantitative studies. The basic message of this new research was that it was not the degree but the quality of state intervention that was of essence for understanding developmental outcomes; state intervention was not only compatible with but responsible for rapid economic growth in some parts of the developing world. A pioneering contribution in this context was Chalmers Johnson’s formulation (1982) of the concept of the developmental state with reference to the state’s positive role in Japan’s rapid economic growth over the twentieth century. In a brilliant book that started out as a history of Japan’s Ministry of Trade and Industry (MITI), Johnson ended up accomplishing nothing less than laying bare the political mechanisms that propelled the rapid industrialization of Japan.

In his interpretation of the Japanese model of development, Johnson (1982, conclusion) stressed the following: the high priority assigned by leaders to economic development as a national goal; a political system in which bureaucracy was dominant and staffed by a fairly small group of society’s “best and the brightest”; a variety of “market-conforming” interventions by these bureaucrats that sought to mold the incentives of private producers; and finally, state-business cooperation, especially following World War II, that ensured that both state and business were able to pursue their core goals. Johnson was not very sure, and rightly so, as to whether this statist model of rapid economic growth—which differed from both laissez-faire capitalism and state socialism—could be readily emulated by other developing countries. What he was sure about, however, was that elements of this Japanese model were being practiced in other parts of rapidly growing East Asia, especially in South Korea and Taiwan, and that its success threw up an important intellectual challenge to certain Anglo-American liberal orthodoxies of how countries do and ought to develop economically (C. Johnson 1987, 1999).

32. See Williamson 1998. On import substitution versus export promotion, see Bhagwati 1986. On East Asia versus Latin America, see Sachs 1985. On urban bias, see Lipton 1977 and Bates 1981. On synthetic policy implications, see World Bank 1991.

This provocative formulation was not likely to remain unchallenged and was not.³³ For our purposes, however, what is important to note is that Johnson's study marked the beginning of a whole slew of studies—some of which were influenced by Johnson and others not—that similarly challenged the antistatist conclusions of neoliberal scholars. As one might expect, East Asian materials provided important case studies. The rapid economic growth of South Korea, for example, was interpreted as state driven in a number of important studies. The economists Il Sakong and Leeroy Jones (1980) provided an early overview that stressed the state's role in such areas as prioritizing economic development, supporting private entrepreneurs, and undertaking both indirect and direct interventions that were clearly associated with improved economic performance. Jung-en-Woo (1991) demonstrated how the state's control of finance in South Korea came to be used as a critical element of industrial policy that led to rapid industrialization. And the economist Alice Amsden (1989) provided the most ambitious revisionist interpretation that not only argued that the South Korean state deliberately got prices wrong so as to grow rapidly but also sought to provide an alternate theory of late development that emphasized the state's role in facilitating learning by doing within firms.

Empirical materials from Taiwan provided the core of a broad-based study of East Asian development by Robert Wade (1990). In this influential and widely read book, Wade demonstrates with meticulous care how a variety of such state interventions as selective protectionism, technology promotion, and deliberate export promotion came to be associated with Taiwan's rapid economic growth. He also generalizes from these specific materials to suggest the conditions under which states can lead rather than follow markets to higher rates and levels of production. If states in the rapid growth of East Asia constructively altered market incentives, managed conflict, reduced risks, and gave entrepreneurs direction, some economists are now also seeking to interpret these actions in terms of economic theory (Chang 1994). And yet other scholars have wondered to what extent these statist insights apply to such other Southeast Asian countries as Malaysia or Thailand (Felker 1998, Jomo et al. 1997).

If the economic success of East Asia has attracted a wide variety of statist analyses, the poor economic performance of numerous sub-Saharan African countries has also attracted similar analytical attention. Instead of arguing that poor economic performance is mainly a result of politically motivated distorted price regimes, scholars such as Richard Sandbrook (1985) argued persuasively that the problem lay deeper in poorly functioning states. Irrespective of the policy regime, state interventions in sub-Saharan Africa have been quickly personalized by Africa's neopatrimonial states, leading mainly to enrichment of ruling elites and their loyal follow-

33. For one of the numerous challenges by Japanese specialists, see Calder 1993. For a review of related debates, see C. Johnson 1999.

ers. Detailed country case studies have further unearthed the functioning of such neopatrimonial states that, instead of being developmental, readily turn predatory.³⁴ Crawford Young's majestic study (1994) traced the roots of such predatory states to the colonial experience, and Jeffrey Herbst (2000) proposed that exceptional demographic circumstances of sub-Saharan Africa have contributed to poor state formation. And if near-experimental evidence were needed in support of such arguments, it has been provided by widespread failure of structural adjustment policies in Africa. Pursued mostly at the behest of external donors, this new pro-market policy regime has failed to generate higher economic growth in most African countries. Whether in regard to individual countries or the continent as a whole, analysts studying this failure have concluded that deeper problems of ineffective states have made it difficult to pursue any developmental policy regime consistently.³⁵

Besides subjecting the more extreme cases of developmental success and failure to statist analyses, a variety of more-mixed cases are also continuing to attract similar reinterpretations. While many Latin American elites as well as scholars of Latin America embraced neoliberalism in the aftermath of the debt crisis of the 1980s, not every one did. Barbara Stallings (1995), for example, provided a nuanced analysis of Latin America's post-1980 economic woes in terms of the limited capacity of states in the region to take advantage of the globalization of finance. Albert Fishlow (1990) also argued that the inability of many of the region's states to tax their populations contributes to low domestic savings rates, making them highly dependent on foreign investment for growth; as long as such investment is forthcoming the region grows, but when, for one reason or another, such investment dries up, growth declines, creating economic and political crises. Others have sought to reinterpret the high-growth experience of such significant countries as Brazil (from 1930 to 1980) in statist terms (Krieckhaus 2000), and yet others have juxtaposed the nature of states in Brazil and Mexico against East Asia with the aim of delineating how relative state capacity can be explained in terms of the characteristics of the bureaucracy (Schneider 1999). Outside of Latin America also, such mixed cases of Turkey and India have attracted statist analyses that wonder why these countries have not done as well as East Asia and provide answers in terms of the character and choices made by the state.³⁶

Finally, more genuinely comparative, cross-regional studies have also sought to highlight the state's economic role in developing countries. An early such study was Stephan Haggard's *Pathways from the Periphery*

34. See, for example, Callaghy 1984.

35. See, for example, Lewis 1994 and Van de Walle 2001. For possible exceptions like Ghana, see Herbst 1993.

36. On Turkey, see Onis 1998. On India, see Herring 1999.

(1990). Haggard accepted the conclusion of many economists that export promotion was superior to import substitution as a development strategy but sought to underline that even export promotion strategies of East Asia were state driven, helping explain East Asia's superior performance vis-à-vis Latin America. More recently, John Waterbury (1993) undertook a comparative analysis of Mexico, Turkey, India, and Egypt around the issue of why states in such diverse countries opted for import substitution strategies, especially focused on the leading role of public enterprises. While Waterbury concludes that public enterprises in all his cases have not performed as their creators had hoped they would, he also suggests sympathetically that, given the interests and ideologies of developing country leaders, as well as their nascent private sectors following World War II, import substitution strategies may well have been a necessary stage in the evolution of state-market relations within these countries.

The most important recent work in this comparative genre is clearly Peter Evans's *Embedded Autonomy* (1995). This is a broad-based comparative analysis of the state's role in promoting industry—especially computer-related industries—in such countries as Korea, Brazil, and India. Evans seeks to explain why some states are more successful than others in industry promotion. His argument is both parsimonious and supported by detailed empirical materials. He argues that states that are most successful—developmental states—display the characteristic of embedded autonomy. The autonomy of bureaucracies lies in their being highly professional, that is, both skilled and not readily manipulated by a variety of personalistic interests. At the same time, however, these bureaucratized states are also well embedded in the social structure, especially insofar as cooperation between state and business elites is well institutionalized. State autonomy enables state elites to define and pursue developmental goals, and state embeddedness assures that this autonomous power is used in a manner consistent with the goals and interests of business. State-business alliances of this nature may have some unsavory characteristics but have proven to be highly instrumental in promoting rapid industrialization and economic growth in the developing world.

To sum up, this single country and comparative case study statist literature on economic growth has chipped away at the orthodoxy that getting prices right is the best way to promote economic growth in the developing world. Some quantitative work has also raised doubt about the empirical basis on which claims relating an open trade orientation to rapid economic growth rest.³⁷ The point here is not that this body of research is beyond criticism itself. The statist literature on economic growth can be criticized for having a weak underlying theory, for focusing too much on bureaucratic and not on political variables, and for not asking why some regions of the developing world have ended up with developmental states, and others

37. See, for example, Rodriguez and Rodrik 2001.

with predatory states.³⁸ Such criticisms are par for the course in any ongoing research endeavor; one set of contributions opens up paths for others to challenge, modify, and so push further the frontiers of knowledge.

More important, this statist body of research has already had considerable influence, even beyond the academy. For example, growing evidence that East Asian economic success was state driven, along with pressure from Japan to take account of this success, led the World Bank to commission its own study, *East Asian Miracle* (1993). While the Bank stopped well short of endorsing the role of state intervention and of industrial policy—and its selective use of evidence was suitably and widely criticized (Fishlow et al. 1994; Amsden 1994)—the study nevertheless went some distance in acknowledging the state's constructive role in East Asia. Similarly, the Bank in 1997 used its influential *World Development Report* to focus attention on the state's role in development. Once again, the bank's understanding of what the state ought to do in the process of development remains quite different than that entertained by many statist scholars. Nevertheless, by underlining the positive role that the state can play, the bank as an important signaling institution in policy and ideological debates on development signaled that its sharply pro-market and antistatist position was undergoing some modification.³⁹

■ | Conclusion

This review of research on comparative political development that is informed by a state-society perspective has focused on some problem-oriented research programs. More specifically, I have discussed research that analyzes the origins and dynamics of revolutionary, authoritarian, and democratic states in the developing world on the one hand and how states mold the politics and economics of these countries on the other hand. Given the limitations of space and of the genre of a review essay that must touch on an entire subfield, the discussion has often been highly condensed and much has been left out. Numerous single country studies of merit have not been mentioned. I especially regret not discussing research on failed states, rentier states, and structural adjustment.⁴⁰ While good research exists on all of these issues, either it tends to be too policy oriented

38. I address some of these issues in much greater detail in my forthcoming book, *In Search of Development*.

39. Whether the financial crisis in East Asia in the late 1990s will fundamentally alter this new position remains to be seen. For one statist analysis of the financial crisis, see Wade 2000.

40. On the rentier state, see Bellin 1994, Chaudhry 1994, and Beblawi and Luciani 1987. On structural adjustment, see Haggard and Kaufman 1995. On failed states, especially in Africa, see Reno 1998 and Zartman 1995.

or the studies do not always build on each other, thus creating identifiable research programs that involve a substantial number of theoretically interested scholars. I also regret that the merits and shortcomings of individual contributions discussed here were not pursued in greater detail. Prior to concluding, therefore, I would like to take note of some general criticisms of the style of research discussed here, set them aside, and then suggest as an insider where some of this neo-Weberian research on developing countries should head in the future.

One broad criticism of state-society research, especially of the research that focuses more on state structures and actions, comes from scholars who are sensitive to local and cultural heterogeneity of developing country societies. Joel Migdal (2001), for example, chastises this body of research for assuming that states are all-knowing and all-powerful and for neglecting the variety of nonstate structures of power in society that foil state designs. James Scott (1998) has also recently and eloquently documented state-created developmental disasters, urging more respect for the local, both in developing practice and in our analytical lenses. I take these to be friendly and useful criticisms that ought to remind all state-oriented scholars not to mistake their analytical foci for some normative love of the state, as well as to balance their statist concerns in a more state-society direction.⁴¹ A more extreme culturalist critique, however, that state and society are not useful distinctions because the boundaries between the two are horribly difficult to delineate mistakes analytical and empirical issues (Mitchell 1991). Of course, boundaries between states and society are diffuse in the real world, but one needs some prior analytical distinction to even make sense of that claim. The Weberian distinctions of state and society, that is, of arenas of legitimate coercion and of association, have served comparative scholars very well for quite some time; until and unless someone comes up with more powerful lenses, they remain useful distinctions for building a variety of theoretically informed empirical research.

A still harsher set of criticisms may be attributed to rational-choice scholars. Some of these are in the form of provocative polemics,⁴² others are implicit in reviews of the subfield (Geddes 1991), and yet others can be heard in numerous professional conversations. Cumulatively they seem to suggest that state-society scholarship is atheoretical, methodologically flawed because it rests on too few cases, not rooted in microreasoning, and not social-scientific enough because knowledge within it does not accumulate. It would take me too far afield to consider such criticisms; for the present only a few brief responses must suffice.

First, the polemical claim that much of comparative politics scholar-

41. A collection edited by Migdal, Kohli, and Shue (1994) made precisely this argument in detail.

42. See, for example, Bates 1997a. As a response, see in the same issue, Johnson 1997.

ship, including scholarship in comparative political development, is dominated by atheoretical area studies can only be sustained by neglecting the entire body of scholarship discussed here. All the studies discussed simultaneously contain specific empirical accounts and theoretical claims that are of broader significance. Relatedly, the criticism that knowledge in comparative political development does not accumulate is also not sustainable in light of the research reviewed. In each and every one of the five research programs discussed, scholars have built on the work of each other, challenging and modifying existing claims, and so leading to newer findings and insights.

The deeper issues at stake in the disagreements between rational-choice and state-society scholars concern the level and type of theory building that is appropriate for the study of comparative political development. Rational-choice scholars believe that a general theory of all politics, including the politics of developing countries, is possible and desirable and ought to be built on microeconomic reasoning. State-society scholars are deeply skeptical of this starting position. They believe instead in middle-range theory that is partly rooted in sociological assumptions and reasoning. Moreover, theory for state-society scholars is a means to understanding real-world problems of significance, rather than an end in itself. They are not content to wait till all the micropieces have been put together in a macro-jigsaw puzzle that makes the whole and the parts of the world mutually intelligible. As a matter of fact, many of them simply do not believe that this will ever happen. Given these fundamental disagreements, the best one can hope for is an agreement to disagree.

None of these comments should be interpreted as if state-society research reviewed here is impervious to criticisms. Nearly every single contribution discussed could be subjected to a more-detailed scrutiny. That, however, was not the purpose of this essay. The main purpose instead was to chart the recent intellectual development in the subfield. I hope it is clear to readers (especially readers who are not fundamentally hostile to this type of scholarship) that the subfield is a vibrant one. State-society scholarship in comparative political development tends to share some broad neo-Weberian theoretical assumptions and often pursues research problems of real-world significance, and knowledge within it tends to accumulate within problem-oriented research programs. Every field, however, has its blind spots and continuous effort ought to be made to improve on its shortcomings. Outside criticisms of the subfield are telling us (who work within the subfield) to avoid unnecessary reification of our analytical categories on the one hand and to be more theoretically self-conscious and rigorous in analyzing and documenting our claims on the other hand. These criticisms ought to be taken to heart.

Even when viewed from the inside, state-society research on developing countries ought to move in some new directions in the future. I will make two such suggestions as final concluding comments. First, state and

society are really nation-level concepts and thus create some blind spots to global influences. Scholars of developing countries are often well aware of international influences on national development trends; the literature on dependency even sought to make a paradigm of sorts out of such awareness. However, while rightly discarding the more economic aspects of the dependency perspective, state-society scholars may have gone too far in embracing nation-level explanations. Whether reinterpreting colonial pasts or newer developments under conditions of increasing globalization, state-society scholars will need to create more open models in the future. Even outcomes like democracy, for example, cannot increasingly be debated mainly in terms of the impact of national economic and political determinants. International forces both stimulate democratization in the developing world and constrain its operation by limiting policy choices open to governments. The same is likely to be true of a variety of other political outcomes that grab scholarly attention.

Finally, far too much of state-society research has been structured around the regime typology of democracy and authoritarianism. Whatever the roots of this near-obsession—the role of liberal ideology cannot be underestimated—it does create some important blind spots. Authoritarianism is a residual category, too amorphous to be very useful analytically without further disaggregation. As more and more developing countries democratize, varieties of democracy will also need to be understood. While some such research is already underway, the concluding point that I wish to emphasize is at a deeper analytical level: a focus on democracy and its absence creates a bias toward understanding the distribution of power at the expense of how power may be aggregated and used constructively. Power, like wealth, has to exist before it can be distributed. In many developing countries, the need to create coherent and enduring state authority is more pressing than the issue of how to check and distribute this power. In other developing countries, where such modern states exist, both democracies and dictatorships are often powerless to pursue socioeconomic goals. Why? Research on such questions will be better structured around such typologies as developmental and predatory states rather than around democracy and authoritarianism. An important research task of the future will be to continue to theorize and to study empirically which types of national authority structures are capable of defining and pursuing developmental goals, and how readily such authority structures mesh with a democracy.

JEFFRY FRIEDEN AND LISA L. MARTIN

International Political Economy: Global and Domestic Interactions

International Political Economy (IPE) is a relatively new subdiscipline within political science. The first textbook in the area was published in 1977 (Spero 1977), and courses were not routinely offered until the middle 1980s. Even today, the boundaries of IPE are not always clear—the overlap with political economy research in comparative and American politics is especially great. We take the field to include all work for which international economic factors are an important cause or consequence. This ranges from the domestic politics of trade and exchange rate policy, through the politics of World Trade Organization (WTO) dispute settlements, to the impact of international flows of goods or capital on national political systems.

Despite imprecision about the definition of IPE, in the past twenty years the field has approached consensus on theories, methods, analytical frameworks, and important questions. This is not to say that scholars in IPE all agree with one another. However, disagreement today generally takes the form of contention among productive, theoretically and empirically motivated claims, rather than the paradigmatic clashes that earlier characterized IPE. Scholars typically see alternative approaches as complementary or applying under different, specifiable conditions. Other disagreements might be debates about the relative weights that we should attribute to alternative explanatory variables. These types of scholarly differences are the hallmark of a mature field of research that has moved from sweeping attempts at self-definition to formulating refutable analytical claims and evaluating their fit with empirical regularities.

This essay presents what we believe to be the consensus among political scientists with regard to the analysis of the politics of international economic relations. The review we present is not intended to be exhaustive. We do not, for example, attempt to include the work of scholars who challenge the positivist approach that is assumed here. We believe that this survey does reflect the principal focuses of North American scholarship, although it is not reflective of much European scholarship. Most publica-

tions in the principal U.S. journals of the profession and the subdiscipline, and most North American graduate training and research, are within the range of the theoretical and empirical topics and approaches presented here.

The most challenging questions in IPE have to do with the interaction of domestic and international factors as they affect economic policies and outcomes. Modeling interactive effects is complex, but there have been exciting research efforts in IPE along these lines. We begin with a survey of some of the more-promising efforts in this direction. It quickly becomes evident that many of the advances made in the study of domestic-international interaction would have been impossible without firm foundations in the analysis of domestic and of international factors in and of themselves. Indeed, recent work on the international-domestic research frontier builds heavily on progress made over the last couple of decades in the analysis of the domestic and international levels; and these analyses provide the essential building blocks for the current and future study of interaction.

The second section therefore turns to a discussion of the domestic politics of foreign economic policymaking. Here scholars typically begin with a picture of the principal economic interests at stake and their forms of organization, then investigate how these interests transit through institutions of national political economies that determine patterns of delegation and aggregation.

We follow with a discussion of the international level of analysis. This starts with strategic interaction among national states as they relate to one another in international economic affairs. It continues with a more-detailed analysis of the role that international institutions play in this interaction, with institutions as both cause and consequence of variation in these international economic relationships.

Throughout the paper, we call attention to empirical puzzles and outcomes that have received extensive attention in IPE, as well as some that have not received as much attention as we think they deserve. The research frontier for IPE is thus defined in terms of empirical puzzles as well as a theoretical agenda, although we do not cover more than a sample of the empirical work that has been done over past decades. One relatively well developed empirical subject area, from which we draw many of our illustrative examples, is international trade. Monetary relations have received somewhat less attention but are now the subject of substantial research efforts. Other areas, such as foreign direct investment, have been the subject of some scholarship in the past, but are now relatively neglected and are prominent candidates for modern analysis. Many of these substantive topics are relevant to ongoing debates about the causes and consequences of globalization for national and international political economies.

We also identify strands of research that are organized around general

concepts, such as international cooperation or stability, rather than specific issue areas. Scholars have long been interested in such general questions as the sources of interstate conflict or collaboration. We do, however, note that some of this sort of scholarship on international interaction would benefit from the use of more-nuanced and measurable outcomes than cooperation. We also note that, despite a generally accepted theoretical architecture for their analysis, there has been strikingly little detailed empirical work on international economic institutions.

■ | Domestic-International Interaction

For decades, a principal challenge to students of international politics generally, and international political economy specifically, has been the need to take into account both the domestic political economy of foreign economic policy and the role of strategic interaction among nation-states and nonstate actors. While all scholars recognized the domestic and international levels as necessary building blocks of a more systematic and integrated analysis of international relations, the difficulties of this integration were just as evident. Recent models of domestic-international interaction have pushed IPE research forward along these lines in important ways, although of course much remains to be done. In this section, we present and discuss some of the frameworks that have been developed to guide the study of domestic-international interaction in IPE.

The core of the domestic-international connection is the impact of domestic institutions and interests on international interaction, and vice versa. Our ultimate goal is a simultaneous understanding of this mutual causation, recognizing feedback effects at both levels—a general equilibrium model, rather than a partial equilibrium one in which one level is held fixed while the other varies. Of course, endogenizing two such complicated levels is extremely complex, and progress has been made in small steps at best. Nonetheless, there have been some promising efforts at integration. Taken as a body of scholarship, they suggest the troubling yet exciting possibility that what we think we have learned from studies lodged solely at the domestic or international level may have to be revisited when we take interactive effects more seriously.

Like all of IPE, the interaction of domestic and international conditions can be analyzed in terms of three factors: interests, institutions, and information. The interests of economic and political actors are a driving force behind economic policy decisions, and scholars attempt to specify these interests with some precision. How interests are translated into outcomes depends on the strategic environment, especially institutions and information. Interests are aggregated through institutions, which also delegate responsibility for decision making to particular actors. The structure of

information similarly influences bargaining and policy choice. All three of these factors—interests, institutions, and information—are important at the international and domestic levels, and domestic-international interaction can often be examined by looking in a more-focused way at these factors. For example, we might ask how domestic institutions affect available international information or how international institutions affect domestic interests. This more-disaggregated approach to the relationship between the national and the global helps us formulate arguments amenable to empirical evaluation.

One approach to domestic-international interaction looks directly at how the international economy affects domestic interests, institutions, and information in ways that then feed back to national policies.¹ The international economy might affect foreign national economic policymaking through two related channels. The first runs directly from the global economy to the preferences of national socioeconomic and political actors. In this variant, international economic trends directly affect the interests of domestic groups, leading them to move toward new policy preferences or to change their domestic political behavior. For example, the expansion of world trade can have a powerful impact on firms' or industries' trade policy preferences. New export opportunities can lead previously protectionist firms to turn toward free trade, as some argue was the case for U.S. manufacturers after World War II; alternatively, the opening of new export markets can lead free-trade firms to redouble their lobbying efforts. In a Stolper-Samuelson framework (for example, Rogowski 1989), the expansion of world trade strengthens owners of nationally abundant factors, such as labor in poor countries and capital in rich countries. Similarly, the state of international capital markets can have a big impact on the preferred policies of groups in potential borrowing countries: the prospects of access to thriving global financial markets can lead firms and sectors to champion national trade, monetary, or exchange rate policies they might not otherwise support. Again, all these involve a common causal mechanism: change in the international economic environment affects the policy preferences and behavior of domestic groups, and thus has an impact on national policymaking and foreign economic policies.

In addition to affecting domestic interests, the international economy might also affect domestic institutions, as by making a previously feasible policy difficult to sustain. For example, national capital controls are relatively easy to impose and enforce when the world's capital markets are dormant or barely active, as was the case until the middle 1970s. However, the explosion of international financial activity in the 1980s and 1990s made it

1. The essays in Keohane and Milner (1996) contain some recent examples; the tradition is closely related to early work on interdependence (for example, Cooper 1968; Keohane and Nye 1977), and also to dependency-influenced arguments about the effects of international markets on development.

extremely difficult for national governments, especially in the more financially developed industrial world, to sustain controls on cross-border investment. While this process reflected previous policy choice by some major governments, it can be taken as the exogenous result of economic and technological trends for most countries, and it dramatically altered the set of policies feasible for governments to contemplate (Goodman and Pauly 1993; D. Andrews 1994). For example, Eichengreen (1996) argues that movement toward Economic and Monetary Union (EMU) in Europe was made inevitable by the increasing difficulties European nations had in sustaining capital controls that allowed them to run independent monetary policies, and more generally that high capital mobility has forced most countries to choose between irrevocably fixed and freely floating exchange rates, eliminating the possibility of defending intermediate exchange rate systems.

One variant of analysis of the impact of the international economy on domestic institutions is about the effects of globalization on the prospects for the social-democratic welfare state and similar social policies. For example, Rodrik (1997) has argued that economic integration has reduced the ability of governments to tax capital, thus limiting the scope for government policies to deal with the social dislocations that globalization itself creates. Others believe that these effects are less limiting, still allowing for different national economic policy paths (Garrett 1998b). All these arguments share a common causal approach connecting the international and domestic levels: globalization, understood as developments in international economic integration, alters the choices available to national governments; this in turn affects national policy (and, one could continue, international outcomes).

While these perspectives look at the impact of international factors on domestic interests and institutions, they can be turned around to look at how the structure of international economic institutions alters the information available to and the policy incentives for some domestic actors. It has been argued, for example, that this is the secret of the effectiveness of WTO provisions allowing one country to retaliate for WTO-illegal policies by another country by excluding some of the violator's exports. This will lead exporters in the violator to lobby *domestically* against national trade policies that might lead to such retaliation, even though, in the absence of the prospects for retaliation, they would be indifferent (Goldstein and Martin 2000). For example, if country A obtains WTO permission to sanction country B for its barriers to A's clothing exports, and country A then puts a tariff on country B's grain exports to A, this will give B's grain farmers an incentive to lobby *within their own domestic political economy* for a reduction in B's barriers to clothing imports. In fact, some scholars argue that one important effect of international institutions is to change domestic informational and institutional conditions so as to permit such linkages among domestic policies, mobilizing exporters against their own government's

trade protection. This line of thinking has been applied especially to the highly structured nature of interstate bargaining within the European Union, where cross-issue linkages are rife and often draw contending domestic interests into the political fray (L. Martin 2001). Here, too, the chain of causation goes from international institutions to domestic institutions, information, and interests, then to national foreign economic policies.

In all these approaches, international factors affect national policy by way of their direct effect on the domestic political economy. This effect may take place by restricting the set of feasible policies, by constraining domestic institutions, by altering domestic information, or by changing the preferred policies and behavior of domestic actors.

Another approach, not necessarily exclusive from the foregoing, posits that national governments stand between the domestic and international levels, intermediating between them in ways not reducible to one or the other level and in ways that bring interaction between the levels to the fore. A powerful metaphor for this view of national governments as mediating the domestic-international interaction is that of two-level games (Putnam 1988). Initial work on this problem had a very simple characterization of the relevant domestic institutions and interests. First, the central domestic institutional actor was assumed to be the “chief of government” (COG) or head negotiator, operating at both the domestic and international levels. Second, domestic interests were summarized in terms of the “win set,” the set of all international deals that would be preferred to the status quo by domestic society. In fact, much research in the two-level games tradition concentrates on the determinants of the win set. Third, initial work assumed that the COG was a disinterested representative of the interests of his or her constituency. This assumption has often been dropped in work building on the metaphor, as the COG is allowed to have independent interests. Adding another set of interests leads to the identification of the “acceptability set,” those international agreements that the COG finds preferable to the status quo. Any deal reached and implemented must lie within both the win set and the acceptability set.

The two-level game framework provides tools for thinking about domestic-international interaction. It also hints at more fully interactive analyses, inherent in what Robert Putnam called “reverberation” and what Jack Snyder called “synergy” (1993a). Synergy represents new possibilities that open up as the result of interaction between the domestic and international levels and might occur when negotiators are able to use international negotiations to create previously unattainable outcomes, thus enlarging their domestic win sets. Yet treatments of two-level games have not yet made much progress toward full interaction between the two levels.

Most work in this tradition has focused on how domestic interests, institutions, and information influence negotiation and cooperation on the international level. One promising avenue is to look at the impact of legis-

latures on the ability of governments to commit to international agreements (L. Martin 2000). In democracies, legislatures have the ability to block or frustrate the implementation of international commitments even if they do not require formal legislative approval. Therefore, agreements negotiated without legislative participation may lack credibility. Agreements gain credibility when the legislature has been involved in a structured, institutionalized manner in the negotiating process. One major reason has to do with considerations of uncertainty, as legislative participation reveals information to both its own government and others about which deals will be implemented. Evidence drawn from the United States and the EU demonstrates that the credibility of commitments rises with institutionalized legislative participation.

Other productive applications of the two-level games approach include work on trade bargaining that formalizes ideas about win sets and ratification, and treats uncertainty explicitly (Milner and Rosendorff 1997; Milner 1997). These models identify a president with preferences over the degree of trade liberalization and a legislature that must ratify any international agreement. They assume two countries; for simplicity, the domestic politics of the “foreign” state are not treated in as much detail as those of the “home” state. On the international level, a Nash bargaining solution specifies the outcome of negotiations. Analytical interest thus turns primarily to the effect of the ratification requirement in the home state. Here, two factors get the most attention: the interests of the legislature, particularly how its ideal point differs from that of the president, and the degree of uncertainty associated with international negotiations.

These models give rise to a rich set of conjectures about the conditions under which agreements will be reached and the distribution of benefits within these agreements. One of the important contributions of this work has been to identify the conditions under which the Schelling conjecture holds. This is the idea, present also in the two-level games metaphor, that negotiators can often benefit from having their hands tied. If a legislature must ratify an international agreement and if the legislature will only approve a narrow range of agreements, the negotiator can use this fact as bargaining leverage. Therefore, negotiators might see advantages in being bound by a legislature. Formal approaches show that this logic holds in some but not all circumstances. For example, when the legislature is too hawkish—it has little interest in trade liberalization, in contrast to the president—ratification requirements will undermine the prospects for any international deal.

Domestic informational conditions, in these models, have an important impact on international interaction. The work assumes that the legislature may not know precisely the content of a negotiated agreement, perhaps because legislators may not know just how any agreement will influence economic outcomes in their districts. The effect of such uncertainty on the prospects for international cooperation is complex.

“Endorsers” can come into the picture, experts or interest groups who send signals about whether they find the agreement acceptable. When such endorsers exist, uncertainty does not impede international cooperation; in fact, the prospects for cooperation may be better under these conditions than in the complete information game. Mansfield, Milner, and Rosendorff (2000) extend this framework to consider interaction between democracies and autocracies, where autocracies do not require legislative ratification. They find—perhaps consistent with the Schelling conjecture—that pairs of democracies have more success in lowering barriers to trade than pairs that match a democracy with an autocracy. Statistical work bears out this proposition.

An alternative approach to the formal analysis of how the informational setting affects domestic-international interaction considers domestic politics the source of uncertainty about international agreements (Downs and Rocke 1995). For example, when two countries sign a trade agreement, they are uncertain about the exact economic implications for domestic groups and about which domestic groups will be mobilized to apply political pressure in the face of unwelcome economic consequences. This implies that deeper cooperation—that which provides greater mutual benefits but also involves greater unilateral risks—requires more flexibility, or “imperfection,” in agreements. Tightly binding agreements that do not allow states temporary breathing room in the face of unexpectedly high domestic costs will be unstable. Some room for maneuver, such as less-than-perfect enforcement of commitments, allows states to cooperate over the long term, even if they sometimes deviate temporarily from the strictures of the agreement.

Work on domestic-international interaction has made substantial progress over the past fifteen years. The two-level games project stimulated such work and provided a language for discussing the nature of interaction, and further work has developed the approach in very promising ways. Yet there is much to be done. At this point, the analysis of domestic-international interaction requires heroic assumptions and simplifications, such as to reduce domestic institutions to an executive and a legislature or to reduce domestic interests to a median voter. Future work will need to allow for more nuance and development, incorporating such other domestic institutions as political parties, courts, and central banks and a more-sophisticated treatment of domestic interests.² Models of domestic-international interaction will also need to address more issue areas than international trade, where they have been developed most successfully, perhaps because of the strong preexisting microfoundations. Despite the

2. Some progress along these lines has been made in the analysis-of-trade literature, where Grossman and Helpman (1995) provide microfoundations for both economic and political interests; politicians are assumed to be competing over campaign contributions from special interests.

continuing weaknesses and large agenda for the future, the analysis of domestic-international interaction has made substantial advances and holds great promise. One reason this progress has been possible is that scholars were able to build on well-developed domestic and international analyses, specifically of the domestic politics of foreign economic policy and of international strategic interaction. Without these prior bodies of scholarship, whatever the weakness of a focus restricted to the solely domestic or global level, research progress on the *interrelationship* of the national and the international would have been impossible. It is to these two bodies of research that we now turn.

■ | The Domestic Political Economy of Foreign Economic Policy

Understanding how domestic and international factors interact to determine the politics of international economics requires a firm foundation in theories both of domestic political economy and of international interaction. Most of the work in IPE has been directed toward providing these two sets of building blocks. The first large area of IPE research examines national policies toward the international economy. This sometimes comes close to the investigation of purely domestic factors about a policy that just happens to involve foreigners. More commonly, the international connection itself plays an important part in the making of foreign economic policy. In any case our understanding of the domestic politics of international economic policy requires careful analysis of the economic interests at stake and of how they work their way through domestic political institutions. Substantial work on both these aspects of the problem has greatly enhanced our understanding. This is especially true of trade policies, which we use heavily as an example.

In this domestic analysis, we identify three analytical steps. First, scholars specify the economic interests at stake. Second, they characterize the organization of these interests. Third, they investigate how these interests are mediated through political institutions. In carrying out this analysis, we can also differentiate between broad public policies and mass interests—such issues as inflation, growth, and economic reform—on the one hand and narrower policies and special interests—such issues as trade protection, industrial subsidies, and sectoral regulations—on the other.

ECONOMIC INTERESTS

Most IPE scholarship on foreign economic policymaking begins with an explicit or implicit model in which politicians confront a combination of pressures from concentrated interests and the broad public. This frame-

work and its emphasis on the demand side coming from self-interested socioeconomic actors has roots in three perspectives: the Stigler-Peltzman approach to the political economy of regulation; the analysis of rent-seeking, or directly unproductive behavior; and the analytical Marxism that applies neoclassical tools to traditionally Marxist concerns. In all these frameworks, governments weigh benefits to special interests against costs to the general public, as well as clashes among special interest groups, and different levels of concern over broad public goods. Variation in foreign economic policy is due, at least as a first cut, to differences in the character and intensity of the interest-group and public pressures, and to differences in the incentives of governments to respond to these pressures. Analysis of domestic interests can be structured along two dimensions: specifying the content of groups whose interests are at stake and specifying the organization of these interests.

An important starting point for domestic IPE is to ascertain the interests, or policy preferences, of groups both broad and narrow. This can be done inductively, by observation, interviews, surveys, and other empirical techniques to map the interests of the relevant populations. There has in fact been a great flowering of work along these lines recently, especially making sophisticated use of surveys of public opinion in the United States and western Europe. Scholars have used public opinion surveys to explore both interest-group and mass-public preferences about European integration in general (Dalton and Eichenberg 1993; Gabel 1998), about EMU in particular (Gabel 2001), and about trade policy (Scheve and Slaughter 2001a, b) in imaginative and instructive ways. These empirical investigations of individual, group, and mass public policy preferences are an important source of information.

Scholars have also deduced policy preferences on the basis of prior theories of how characteristics of particular groups will lead them to desire particular policies. One example of how this approach has been used with great effect is the study of trade policy. Here political scientists have had the raw material of trade theoretic conclusions from economists interested in the differential impact of trade policies, who have developed three models of the distributional effects of trade liberalization and protection. The first, the Stolper-Samuelson theorem, predicts that factors of production that are scarce in a country will benefit from trade protection, so that labor in capital-rich (developed) countries and capital in labor-rich (developing) countries should be protectionist. The Ricardo-Viner, or specific-factors, approach emphasizes comparative costs and asserts that factors of production specific to import-competing (exporting) industries will be protectionist (free trade). More recent developments look at scale economies, typically to argue that larger firms should prefer more-open trade.³

3. Alt and Gilligan 1994 surveys the first two; Milner and Yoffie 1989, Richardson 1990, and Chase forthcoming survey the latter, which is less developed.

There are other arenas in which theory helps us understand the expected interests of socioeconomic actors. Where economists have explored the distributional effects of foreign economic policies, political scientists can deduce the policy preferences of the affected interests. Sometimes there is only a short step from existing work in economics to these deductions, as in the analysis of immigration; at other times, it requires substantial extension and imagination, as in the analysis of foreign direct investment, currency policy, or financial regulation. And there are many issue areas in which there is little or no preexisting work to serve as a starting point for deriving groups' interests. Here scholars of IPE have developed original arguments about the impact, for example, of financial structure on currency policy preferences (Henning 1994) or of portfolio diversification on trade policy preferences (Schonhardt-Bailey 1991).

Just as scholars have deduced group interests from the distributional impact of targeted economic policies, they have also searched for theoretically grounded ways to specify mass interests over such broad areas as macroeconomic policy and the provision of public goods. One useful baseline is the simple idea that the public prefers those policies most likely to raise national income. This was in fact the unstated presumption of early studies of trade policy (notably Schattschneider 1935), based on the economic argument that free trade is the most efficient policy for any nation. In other areas, we can draw on efficiency and social welfare arguments to designate the purportedly most desirable policy—low inflation, effective countercyclical currency policy, social insurance against exogenous terms of trade shocks—and posit, at least as a first approximation, that these are the public will. Another possibility is to focus not on society as a whole but on the median voter and attempt to establish the expected policy preferences of such pivotal actors.⁴ This is standard in studies of income redistribution, where it is common to assume that the net expected benefits of redistributive policies on the median voter will determine their electoral viability.⁵ The generally applicable point is that if the median voter—more generally, the politically pivotal portion of the public—can, roughly speaking, be identified, it may also be possible to identify the policy preferences of this segment of the population.

The content of the foreign economic policy preferences of concentrated interests, mass publics, and pivotal actors is important, but politics involves a series of exchanges and exchange implies that socioeconomic and political actors are willing to trade off policy dimensions: firms that

4. While there are instances in which the median voter's interests are those which maximize social welfare, this is not always (and perhaps not usually) the case. The median voter, in other words, is not necessarily a representative agent of society; it is simply his or her pivotal position in the relevant political space that makes him or her crucial.

5. Meltzer and Richard 1981 is the classic source.

prefer both free immigration and free trade may compromise by forgoing the former to put together an effective coalition for the latter. Public opinion may be hostile to devaluation but even more hostile to the expenditure reductions and tax increases necessary to defend the exchange rate. For this reason, we need to know the relative intensities of actors' policy preferences, the weights groups and individuals place on their various policy goals.

The basic determinant of preference intensities is the size of the stakes. This may be a direct consequence of the amount of money transferred directly or indirectly or of the policy rents otherwise created. As with policy preferences themselves, scholars have also suggested some general characteristics of actors that affect their preference intensities. One such characteristic is the specificity of the actor's assets—physical or human capital, for example—to a particular use. The idea, drawn loosely from transaction-cost-based analysis, is that the more specific the asset the greater the actor's incentive to defend its value in its current use (Frieden 1991b; Alt and Alesina 1996). Firms with very specific investments will be more concerned to protect themselves from foreign competition; workers with specific skills will be more adamant about demanding social insurance to protect them against unemployment.

Although it is important to have a clear sense of the narrow and broad interests at stake, scholars also need a sense of the ways in which these interests are organized. One simple and powerful starting point is to distinguish between concentrated and diffuse interests, especially as most scholars have believed that concentrated interests are likely to dominate diffuse interests. This was the view, for example, of Schattschneider's pioneering study (1935) of the Smoot-Hawley tariff. Schattschneider assumed that concentrated protectionist industries would dominate the diffuse free-trade interests of disorganized consumers. The explanatory power of this general proposition has been confirmed in many contexts, as scholars find that mass publics or electorates often have little impact on policies whose effects are difficult to discern or technically complex.⁶

But simply distinguishing between concentrated and diffuse interests is not enough. For one thing, there are frequently conflicts of interest *among* concentrated interests. Many studies of trade policy, for example, have shown that antiprotectionist exporters, multinational corporations, and consuming firms can effectively counteract the influence of those who would like to restrict imports (Milner 1988; Destler and Odell 1987). Concentrated interests may also have intense views on general public policies.

6. The insight has been explored in much more detail since the publication of Olson 1965, which focused attention on such problems of collective action. In the realm of IPE, Gourevitch (1986), following on his influential 1977 article, found this bias in favor of concentrated groups to hold across many countries and time periods.

For example, while the simple concentrated-diffuse dichotomy might regard macroeconomic stabilization as a general interest policy of diffuse concern and opposition to stabilization as largely emanating from special interests, there are often concentrated groups with intense interests in broad stabilization policies. This might be the case of the financial sector, typically seen as strongly supportive of anti-inflationary policies despite their diffuse impact. Finally, despite the fact that policy is often biased in favor of special interests, there is no question that diffuse interests often have a powerful impact on policy. After all, politicians in democracies must be elected; most authoritarian regimes need support from at least portions of the mass public. And there is little doubt that mass publics do in fact, at times, have strong desires related to international economic policy, especially when it affects such prominent issues as inflation, unemployment, and taxation. Even where mass publics are not organized around such policy issues, there may well be incentives for political entrepreneurs to address these concerns. In other words, it is almost certainly untrue that debates over foreign economic policy always pit concentrated interests against the public or that the mass electorate is always bested by concentrated interests.⁷

In this context, it is important to note the ways in which groups, and even broad segments of the public, are organized, for this pattern of organization can have a powerful impact on policy. This is especially the case because individuals and firms often have crosscutting material interests, and similar economic agents can be organized into very different collectivities across countries; even within a single country, over time the organization of interests can change dramatically. An autoworker has at one and the same time economic interests associated with his industry, with a range of similarly constituted exporting or import-competing sectors, with the working class generally, with her or his region, and with her or his status as consumer. In addition, she or he may have strong partisan, ethnic, or other ideological commitments. It is perfectly plausible that in one setting, autoworkers will cohere and identify primarily with their industry, while in other settings they may divide in many different ways. Perhaps the most striking example of these differences is the tendency of workers in Japan to be organized along firm lines, in the United States along industrial lines, and in much of Europe along class lines.

Variation in the organization of economic interests has important implications for the making of policy. An obvious example is the difference

7. Gowa (1988) argued for such a position as applied to international trade and monetary policy, specifically that the former implicated specific interests much more than the latter and would therefore be more politicized. But the highly politicized nature of European monetary integration and currency problems in the developing world would seem to indicate the shortcomings of an approach based on such assumptions, perhaps because they misidentify the specific interests or underestimate the importance of mass publics.

between labor or management interests that are organized on sectoral as opposed to class lines. Where sectoral forms of association prevail, there is likely to be much more pressure for particularistic policies: trade protection, export subsidies, controls on or subsidies to foreign investment. Where class politics prevails, policies are less likely to focus on industry-specific benefits and more likely to involve concerns of broader interest to labor or capital as a whole. Scholars have long noted that northern European-style social democracies, with their densely organized and centralized labor movements, have tended to eschew trade protection in favor of class-based social insurance schemes, while countries with less of a tradition of class political organization (such as the United States and Japan) have tended to engage in more sectoral protection.⁸ While this and other such relationships have been investigated with regard to the organization of labor, they also hold for the organization of business interests, which vary in similarly important ways among countries.

Scholars studying the domestic politics of foreign economic policy, then, pay considerable attention to the policy preferences of interest groups and of the broad public, to the intensity of these policy preferences, and to the organizational forms of these special and broad interests. This has been true in the analysis of international trade, financial regulation, currency policy, and foreign investment. The analysis of trade is probably the best developed of these literatures. We now have quite a nuanced understanding of which economic actors are most likely to demand protection or trade liberalization and under which circumstances. We have a preliminary sense of similar issues in international monetary policy, such as which actors are most likely to prefer fixed or flexible exchange rates. In areas such as financial regulation and foreign direct investment, our knowledge is more rudimentary, with some intriguing puzzles and preliminary analyses but only relatively sketchy theoretical and empirical work. The analysis of the content and organization of domestic interests promises to continue to make advances as it is extended to these underexplored empirical areas.

POLITICAL INSTITUTIONS

The interests, preference intensity, and organization of socioeconomic actors is only a starting point for the analysis of domestic constraints on foreign economic policy. These interests are mediated through domestic political institutions in ways that can fundamentally affect outcomes. This is especially true of the organization of the political system as it responds to political pressures.

8. Katzenstein 1985b is the classic statement. Some might argue that the small size of these countries dictated the form of interest aggregation. Whatever the direction of causation, there appears to be a strong correlation between the two.

Institutions perform two general functions: aggregation and delegation. Some institutions aggregate interests in ways that affect the ability of groups to organize and the weight they will have in the political process. Other institutions delegate decision-making authority to particular actors, similarly changing the weight of various interests in the political process. Still other forms of legislative and bureaucratic organization are best understood as delegating authority from one group of actors to another. In one way or another, electoral, legislative, and bureaucratic institutions serve to mediate the pressures brought to bear by organized interests and the general public and to transform them in ways that directly affect policymaking.

Electoral institutions matter to the making of foreign economic policy because they affect the transmission of societal interests to politicians. One impact is on the need for politicians to respond to policy-relevant concerns of the broad electorate: the more the electoral system reflects the preferences of the mass public (or median voter), the more policy has also to reflect these preferences. Some have hypothesized that systems of proportional representation, or more generally larger electoral districts, tend to reduce the impact of special interests and increase the electoral importance of the median voter. This would imply that such systems would be more likely to incline toward freer trade, and fewer sectoral benefits and subsidies, than first-past-the-post systems in which local or regional interests may be more powerfully represented.⁹

Different electoral systems may also affect the influence of special interest groups in ways other than local or regional particularism, such as when politicians rely heavily on private campaign contributions. Where election funding is entirely public, the scope for a direct contributions-based channel for the influence of interest groups is presumably more limited than in systems in which private contributions are very important. And as national electoral systems—or simply electoral environments—evolve over time, this can affect policy. For example, it has been argued that the increasing importance of urban middle-class voters in Japan facilitated the deregulation of the country's international financial institutions and put pressure on the government to allow the yen to appreciate (Rosenbluth 1993).

In addition to differences among countries' electoral systems, there are also differences in the electoral bases of different branches of government in a single country. This is not usually the case in unitary parliamentary systems, but in federalist systems or those with a division of powers, it is common for different levels or branches of government to be elected in different ways. Because it is uncommon for subnational units of a federal system to have a major role in foreign economic policy, the more important

9. Rogowski 1987b, McGillivray 1997, and Alt and Gilligan 1994 all present variants of these arguments, for which the evidence remains mixed.

differences in electoral institutions are among branches of government, such as different electoral institutions for two chambers of the legislature or for the legislature and the executive. Just as variation in district magnitude or proportionality may have a systematic impact on representation in comparative perspective, so will they affect the political incentives of, say, the upper and lower houses of the Japanese Diet or the U.S. Congress and president. The most common argument along these lines is indeed, as above, that smaller districts are more likely to reflect special interests, while upper chambers and executives are more concerned with broader or even national public opinion. This should tend to make the upper chambers and executives less favorable to trade protection, sectoral or regional subsidies and incentives, and other particularistic policies. The overall impact of these differences on policy depends, of course, on the legislative institutions by which policy is made and implemented.

Legislative organization, then, is a second important set of political institutions that affect foreign economic policymaking. The importance of agenda control, veto points, and other interactions among policymaking institutions is well established in the broader political science literature, and direct consideration of these factors has been drawn into analyses of the making of foreign economic policy as well. Where there are clear electoral differences among chambers or branches, it is important to determine the relative influence of the different units on policy. A constitutional structure, for example, that gives trade policy authority to the more localist legislature will produce different outcomes from one that delegates all trade policymaking to the executive. The areas in which different branches or chambers dominate; the constitutional status of international agreements and treaties; the order in which laws are designed, amended, vetoed, and approved—all these will affect policy.

The most-developed work on the impact of legislative institutions on foreign economic policy has to do with U.S. trade policy. One strand of scholarship asserts that Congress will be systematically more protectionist than the president, given the executive's national constituency. The disaster of the 1930 Smoot-Hawley tariff led policymakers to redesign trade policy institutions so that the president could, within carefully specified constraints, negotiate reciprocal trade agreements with other countries. This evolved into a system in which the executive essentially proposes trade policy to a legislature that can only make limited changes or none at all. This change in the institutional structure of U.S. trade policymaking is often given credit for the general liberalizing trend in U.S. trade policy (Haggard 1988; Bailey, Goldstein, and Weingast 1997).¹⁰ More-differentiated analyses have extrapolated from the expected impact of this institutional structure to draw inferences about the effects of partisan control of Congress and the presidency on the outcome of trade policy.

10. But see Irwin and Kroszner 1999 and Hiscox 1999 for contending views.

Specifically, Lohmann and O'Halloran (1994) have argued that divided U.S. governments are likely to give rise to more protectionist policies than unified governments.

Even in unitary parliamentary systems, legislative institutions can be important. In the general political science literature, it has been argued that divided government in the U.S. system is analogous to minority government in parliamentary systems (Laver and Shepsle 1991). In similar ways, scholars have looked at the role of parties in parliamentary systems. One such approach focuses on the degree of party discipline, on the principle that a disciplined national party, like the U.S. president, is concerned about a national constituency. This leads to expectations that electoral and legislative institutions that lead to strong party discipline will typically deliver policies less in thrall to special interests, such as freer trade (McGillivray 1997). Another approach looks at the incentives to individual parties in multiparty governments, arguing that these differ from those faced by single-party majority governments. Where the government is made up of a coalition, it is difficult for voters to assign blame or credit to a particular party for government policy. This reduces the incentives for opportunistic partisan manipulation of foreign economic policy. One implication is that countries that typically have multiparty governments are more likely to adopt fixed exchange rates that prohibit the manipulation of currency values for partisan electoral purposes, or to put it differently, that single-party governments are more likely to choose adjustable exchange rates so that they can manipulate them for partisan electoral purposes (Bernhard and Leblang 1999).

The third sort of domestic political institutional variable commonly considered by IPE scholarship is *bureaucratic institutions*, especially patterns of delegation to bureaucratic and other agencies. The previously mentioned work on U.S. trade policy is about delegation from one elected body (Congress) to another (the president). In recent years, this delegation has become even more explicit, with the frequent use (or attempted use) of fast-track bargaining authority by the executive. Trade policy in the United States also involves a number of independent agencies, such as the International Trade Commission and the U.S. trade representative, with the potential authority to deal with trade policy issues ranging from allegations of dumping to other instances of unfair trade. The recent upsurge of interest in standards that relate to trade—environmental, labor, health and safety—has also led to the creation of delegated institutional mechanisms to deal with these issues, such as the independent agencies established under the auspices of the North Atlantic Free Trade Association (NAFTA). In the European Union, trade policymaking is delegated to the European Commission and, within this, to industry-specific groups that include representatives from many member states. Trade policy is not unique. International financial policy is typically handled by independent agencies with special

responsibilities for national banking systems: central banks, national regulators, finance ministries.

The impact of delegation on the making of foreign economic policy is complex and controversial. Some scholars see independent agencies as particularly likely to be captured by special interests. Most scholars, however, regard independent bureaucratic entities from an explicit agent theoretic perspective in which the agency responds to the policy needs of politicians, albeit with some room for maneuver (slack). Often independent agencies are regarded as providing politicians a protective cushion from day-to-day or particularistic demands, while also ensuring their accountability to the public. The interplay of independence and accountability depends strongly on the issue area and on other characteristics of the political economy.

One well-developed literature is that on central bank independence, which typically begins with the assertion that independence can mitigate the time inconsistency of monetary policy.¹¹ Politicians can thus best respond to the policy preferences of the mass public (median voter) for low inflation by insulating the central bank from the temptation to alter monetary policy, within the boundaries of ultimate accountability to the political authorities. Central bank independence has also been drawn into analyses of international monetary policies. For example, it has been argued that governments in countries with independent central banks are less likely to engage in electorally motivated manipulations of exchange rates. A related argument has been made that governments that can commit credibly to low inflation with an independent central bank are less likely to need to fix their exchange rate in order to gain anti-inflationary credibility, so that central bank independence and fixed currencies are policy substitutes (Clark and Reichert 1998; Clark and Hallerberg 2000; Broz forthcoming). In all these cases, the relationships between political leaders and bureaucratic institutions have been drawn into the analysis of foreign economic policies.

As this discussion illustrates, there is a clear logic to the approach commonly used by scholars in the analysis of the domestic politics of foreign economic policy. In any given policy arena, we first specify the interests at play, drawing on observations of expressions of policy preferences or on established bodies of theory or on both. It is useful to distinguish between special and mass interests and between policies of primarily narrow or sectoral incidence and policies that broadly affect public welfare. We then look at the ways in which these interests are organized. Finally, we examine political institutions that determine the impact of organized and mass interests on policy through both aggregation and delegation. Elec-

11. Classic presentations can be found in Grilli, Masciandaro, and Tabellini 1991 and in Alesina and Summers 1993.

toral institutions affect the relationship among voters, interest groups, and politicians. Legislative institutions influence the ways in which politicians bargain toward policy outcomes. Bureaucratic institutions condition the ways in which policy is made and implemented, depending on the degree of autonomy delegated to bureaucratic agencies. Decades of scholarship in IPE demonstrate that this consensual method can provide convincing and ultimately revealing results.

This concatenation of methods has been applied successfully to the making of national policies toward international trade and finance, exchange rates, foreign investment, and other foreign economic policies. The literature on trade, particularly on the level of protection offered to various sectors, is especially well developed. That on international finance and exchange rates is moving forward rapidly, for example with detailed studies of the choice of exchange rate regimes and policy responses to market pressures on currencies. Foreign investment, both patterns of international investment flows and patterns of national policies toward foreign investment (regulation, control, international agreement), is a topic deserving of more and more-sophisticated study. In any case, the advances of this realm of IPE research have facilitated the systematic incorporation of domestic factors into a more integrated analysis of domestic-international interaction in the world's political economy.

■ | International Interaction

Along with the domestic politics of foreign economic policy, the second major building block of the larger IPE edifice is the analysis of strategic interaction at the international level. Scholars have developed theoretical and empirical approaches to the ways in which states relate to one another as they devise their international economic policies, and to the institutional forms that these international economic relations take. In this section we outline approaches to studying international interaction, illustrated with examples drawn from trade and monetary policies. In keeping with most U.S. research, the emphasis is on relations among states, rather than between states and such nonstate actors as multinational corporations or nongovernmental organizations. Many of the tools of analysis discussed here could, nonetheless, easily be adapted to these purposes.

STRATEGIC INTERACTION AMONG STATES

Scholars of IPE have long understood that the interactions of states in the international system influence economic policies and outcomes in crucial ways. For tractability, most arguments about strategic interaction take the characteristics of states as given, typically informed by the work on domes-

tic political economy discussed above. Treating states as units—although not identical units—scholars ask how the constraints and opportunities offered by the international system and the processes of interaction with other states influence decisions and outcomes.¹²

The analysis of international interaction centrally involves three elements: the identification of state interests; the specification of the strategic setting; and the attention to the role of uncertainty, beliefs, and ideas in explaining policy choice.¹³ Scholars typically strive to begin with a good understanding of state interests, the outcomes states desire. These interests are then placed within a strategic context that is largely determined by system characteristics. One important aspect of the strategic setting is the uncertainty on a particular issue (see Morrow 1999b), which makes state beliefs and how they change over time a central part of the analysis. While national interests are important to the analysis of international interaction, scholars are typically interested in identifying *patterns* of state interests and *variations* of interests across states and issues, rather than concentrating on individual states and actors within states. In other words, the goal is to come up with *general* patterns of state interests rather than particularistic, nationally specific ones. One approach is to build on the domestic analyses described above but to focus on commonalities among groups of countries. For example, patterns of interests can be characterized by specifying the degree of common interest among actors. Some studies of monetary cooperation during the interwar period suggest that changing perceptions of the degree of common interest were the primary determinant of international cooperation (Oye 1986a).¹⁴ Even when a high degree of common interest exists, however, conflict about the distribution of gains from cooperation may intervene, even to the extent of delaying cooperation indefinitely (Fearon 1998a). In some settings, states may have no incentive to deviate from a cooperative outcome; in others, they may wish to renege if they can do so without punishment.

When states have a high degree of common interest, our attention is drawn to those factors that encourage or discourage realizing those common interests. These might be characteristics of the states themselves—for example, whether they have institutional structures that encourage living up to their commitments—or aspects of the international environment—for example, the costs of monitoring others' behavior. When the degree of common interest is low, in contrast, our attention turns to factors that influence outcomes in the presence of high levels of conflict. We might ask

12. For exposition of a framework similar to that outlined here, see Lake and Powell 1999a.

13. Information is, technically speaking, an aspect of the strategic setting. However, we isolate it as a third factor because of its importance.

14. Although other studies find more explanatory power in domestic economic interests and institutions; see Simmons 1994.

about first-mover advantages, the opportunity costs of using military force or holding out for a better deal, or the wherewithal of states to engage in coercive diplomacy.

A second approach is to look at how the position of nation-states in the international system affects their interests. That is, the focus is more outside in than inside out, asking how the international political or economic system shapes state goals. A simple starting point is to assume that all states want to maximize their economic welfare (although some combination of security and economic well-being is a common assumption as well). However, what policies allow a state to do so are heavily dependent on its position in the international political economy. Governments of large countries are more likely to be protectionist than governments of small countries, for example; the latter are likely to value exchange rate stability more highly than the latter.

Power differentials are often posited to affect state interests. Thus, a hegemonic state is said to have different interests in such issues as institutionalization, liberalization, and international cooperation than a weak state. For example, monetary coordination in Europe appears to be directly shaped by patterns of internationally determined interests. European countries most effectively coordinate their exchange rate and monetary policies when the United States acts in a manner that creates instability and negative externalities (Henning 1998). Thus, regional integration efforts in Europe have been most successful in the absence of hegemonic leadership (B. Cohen 1997). The long tradition of work in IPE that focuses on how patterns of interests depend on the distribution of power in the international system has developed from the initial presumption that a highly concentrated distribution of power led to preferences that facilitate cooperation (Kindleberger 1975; Krasner 1976). A second generation asked about the possibility of interests conducive to cooperation among small groups (Snidal 1985) and how the issues at stake influenced patterns of interests (Conybeare 1984; Gowa 1989). Recent work is even more careful about how the distribution of power influences interests, distinguishing among different types of players in the international system (Lake 1984) and considering the interaction of security and economic interests (Mansfield 1994). In one way or another, scholars interested in analyzing interstate interaction must begin with a clear (if simplified) picture of state interests, just as in domestic analysis.

Where the analysis of international interaction goes beyond domestic political economy is in its attention to the international political and economic strategic setting. By *strategic setting*, we mean the structure of interaction, the form of the game. How many states are involved in any particular negotiation or other mechanism of policy choice? Bilateral interactions create different constraints and opportunities than those involving a few states, which in turn differ from those involving large numbers of states. Can members of a "club" exclude outsiders, or is participation in

the process of interaction open to all who wish to join? Are states interacting in a highly institutionalized environment or one with few rules constraining the nature of the interaction? These questions may lead us to examine implicit or explicit voting rules, such as whether unanimity is required or whether some version of majority rule prevails. For example, studies of debt restructuring suggest that relationships among lenders have a great deal of structure, creating a hierarchical pattern of interaction with the IMF and a few large banks at the top (Lipson 1986). This structure allows banks to overcome collective action problems to respond to debt crises. The international environment also influences the order in which issues will be considered or which issues will be considered or which issues will be linked to one another. Some other important aspects of the strategic environment include questions such as how much weight actors put on future relative to immediate outcomes, that is, the relevant discount factors. Studies of trade wars suggest that a long shadow of the future mitigates pressures for conflict (Conybeare 1986). We also care about the prospects for enforcing agreements. Roughly accurate answers to all these questions are necessary in the attempt to specify how international interaction matters for IPE.

One aspect of the strategic setting that is especially important is the nature of uncertainty. Strategies in an environment of complete information differ tremendously from those in which states are unsure of others' preferences or of the relationship between policies adopted and outcomes achieved. To simplify somewhat, two types of uncertainty are important for IPE: uncertainty about the preferences of others and uncertainty about causal relationships.¹⁵

States may only have estimates of the preferences of others. For example, they may not know the value another government attaches to the aggregate welfare of citizens as opposed to the particularistic welfare of special interests. They may not know how willing others are to sacrifice economic benefits for security concerns. Rationalist approaches assume that beliefs are updated in a systematic manner through the process of interaction, as information is revealed by the choices of other states and by the observed outcomes resulting from these choices.¹⁶ States first develop strategies based on a best guess about others' preferences, taking into account that others will also make their best guesses, and perhaps bluff or otherwise misrepresent their true interests. As interaction progresses, states observe the choices and signals of others and update their beliefs (typically using Bayes' rule). They also attempt to manipulate information, some-

15. A related typology of beliefs can be found in Goldstein and Keohane 1993b; see especially Goldstein and Keohane 1993a.

16. Sociological approaches, which have been more common in the study of international security than in IPE and in European scholarship, posit other dynamics of belief change.

times having an incentive to obfuscate, at other times having an incentive to try to reveal their true preferences.

One of the more important tools in the analysis of beliefs and uncertainty is game theoretic investigation of signaling and reputation (see Morrow 1999b; Morrow 1994b; Kreps and Wilson 1982). In most settings, signals sent by states are most effective in changing the beliefs of others if these signals are costly. Otherwise, intended recipients are likely to dismiss signals as cheap talk. Analyses of some international monetary regimes sees them in this light, as commitment to a fixed exchange rate or a currency union involves a political cost that sends a signal about the intentions of governments (Giavazzi and Pagano 1988). However, in some circumstances even cheap talk can be an effective signal (Crawford and Sobel 1982). This occurs when states believe that they are likely to have a high degree of common interest and that they will be better off if they coordinate their actions rather than making independent choices. Alternative models of monetary regimes that see them as solving coordination problems rather than as commitment devices draw on this logic, identifying particular exchange rate systems as focal points (Frieden 1993; Broz 1997).

A second type of uncertainty in IPE is about the relationship between policies and outcomes, or causal relationships.¹⁷ Some refer to this type of uncertainty as lack of precise knowledge about “the state of the world” (Goldstein and Keohane 1993a, 10). Examples of such uncertainty in IPE proliferate. Will fixed or flexible exchange rates improve investment flows and macroeconomic stability? What will be the costs of adjustment after conclusion of a trade-liberalizing agreement?

Where states are uncertain about the impact of their policies, prior beliefs again are central. Judith Goldstein’s analysis of U.S. trade policy illustrates how the institutionalization of particular causal beliefs has structured the policymaking environment (Goldstein 1993). Uncertainty about the relationship between economic policies and outcomes, often called model uncertainty, can make efforts at monetary coordination counterproductive (Frankel and Rockett 1988; Iida 1990, 1999). While some authors refer to this type of uncertainty as states “not knowing their own interests,” it is more fruitful to assume that states do know which outcomes they prefer, such as rapid economic growth, but are simply uncertain about which policies will get them closest to their goals.

In this setting, expert knowledge and learning are important. One common strategy in the face of causal uncertainty is to delegate some policymaking to experts, those with better knowledge about the relationship between policies and outcomes (Krehbiel 1991). This logic provides a way to ask about the influence of “epistemic communities” (Haas 1992) or “supranational entrepreneurs” (Moravacsik 1999). Another strategy is to

17. For theoretical discussions of causal knowledge, see Gilligan and Krehbiel 1989; Krehbiel 1991.

learn from observing the experience of others. Learning of this sort may account, for example, for diffusion of liberal economic policies in the last fifty years (Simmons and Elkins 2000).

The analysis of international interaction thus requires identifying the patterns of state interests, the strategic environment, and the nature of uncertainty and beliefs. With these factors in hand, analysts can go on to specify the strategies states will use. The interaction of state strategies, in turn, gives rise to equilibria, outcomes from which no state has an incentive to deviate. The goal of the analysis of international interaction is to identify equilibria, the circumstances under which they exist, and their characteristics in terms of aggregate welfare, the distribution of benefits and costs, and stability. Of course, completely and accurately taking all these steps is a mammoth undertaking, and one for which technology often fails us. Instead, most analysts isolate aspects of the problem, holding others constant or relegating them to the background, as in the examples summarized in this section.

The framework for understanding the impact of international interaction on IPE is well developed. It has been extended and applied in varied empirical contexts. However, substantial work remains to be done in this area to get a more precise sense of how international interaction matters for IPE. Greater attention to strategic interaction could inform empirical studies of the choice between trade competition and cooperation, including the choice to join and abide by trade agreements, the political problems associated with sovereign debt and its repayment, the logic behind currency unions and other exchange rate regimes, and the sources and implications of increased flows of foreign direct investment. One specific area in which such strategic interaction at the international level has assumed center stage is the study of international institutions and organizations, to which we now turn.

INTERNATIONAL INSTITUTIONS

Information, uncertainty, and beliefs are important explanatory variables in the analysis of international interaction. Some of the work summarized above refers indirectly to issues of uncertainty, for example, in asking whether policymakers share the same model of monetary policy or in emphasizing transaction costs. However, few of these studies directly address the degree of uncertainty or the role of beliefs in explaining patterns of cooperation, while uncertainty is central to the study of domestic-international interaction. A body of literature that has concentrated directly on informational issues is that on international institutions. The rationale for the existence and influence of institutions at the international level is driven almost entirely by informational considerations. In a world of complete information, according to most current arguments, states would not demand institutions and institutions would have no impact on

outcomes. However, once we consider the myriad uncertainties that states confront, a potentially powerful role for institutions emerges.

The turn toward taking international institutions seriously can be traced to a collective project on international regimes (Krasner 1983) and to Robert Keohane's analysis of how institutions could facilitate the maintenance of patterns of cooperation "after hegemony" (1984). As Keohane's title suggests, much of the early work on institutions developed in reaction to the predictions of early hegemonic stability theory that international developed in reaction to the predictions of early hegemonic stability theory that international economic cooperation should plummet as U.S. power declined in the 1970s. However, in many areas of the international economy, cooperation appeared to be stable or to increase. At the same time, the international economy had become more highly institutionalized, and organizations such as the General Agreement on Tariffs and Trade (GATT) and the International Monetary Fund (IMF) had gained prominence. Perhaps these two trends were related, and international institutions accounted for the persistence of cooperation.

The modern analysis of international institutions begins with simple assumptions. As a first cut, states are treated as unitary actors; domestic politics have barely begun to be integrated into the models. These states are assumed to confront generic collective action problems. They may take the form of Prisoner's Dilemma-type games, where cooperation would benefit all, but each has an incentive to renege if it could do so undetected or without punishment. They may take the form of coordination or bargaining problems, where the difficulty is to choose a particular equilibrium in a situation in which the states disagree on which outcome they prefer (Martin 1992b). It is also possible for these problems to coexist; states may face a bargaining problem followed by an enforcement problem. In a world of no transaction costs, states could solve these dilemmas to reach Pareto-superior outcomes. However, actual problems of incomplete information, such as costs of bargaining and monitoring, can prevent their resolution, leaving all states worse off. International institutions can perform information provision functions that allow states to overcome collective action problems and therefore have an impact on patterns of behavior, even if these institutions do not regulate, enforce, or otherwise take on the characteristics associated with strong institutions on the domestic level.

This insight has led to a large body of theoretical and empirical literature on international institutions, both in IPE and international security (see Martin and Simmons 1998 for a review). Much of the empirical work has been plausibility probes—tests to see whether cases exist in which institutions perform the functions identified, and are associated with higher levels of international cooperation. Much of the early empirical work on institutions took the form of single case studies, and thus suffered from selection bias and difficulties in determining the causal impact of institutions. The latter is especially troubling in the study of institutions, since

they are obviously endogenous: states choose and create institutions when they see a demand for them. Endogeneity makes the process of isolating the causal influence of institutions on outcomes difficult. One way in which this problem can be mitigated is to examine a large number of cases and to control for alternative explanations for both cooperative outcomes and the involvement of institutions. This has been done in the study of economic sanctions, for example, and institutions are found to have a substantively and statistically significant impact on levels of cooperation (Martin 1992a).

While theories of international institutions have grown more sophisticated over time, empirical work on the effect of institutions in IPE has been limited. It has been more evident in security issues (Duffield 1995; Wallander 1999) and environmental affairs (Keohane and Levy 1996). Recent years have not seen many focused studies of specific economic institutions. One obvious exception is the literature on the European Union. However, little of this literature adopts a particularly institutionalist perspective on the EU (despite the term *institutionalists* being tossed into some theoretical labels) or takes the factors identified as central to institutionalist theories seriously.¹⁸ Application of institutionalist theories to economic institutions thus appears to offer another promising, highly productive avenue for existing research. Basic theoretical precepts are in place, but they need to be adapted to the complexities and context of specific institutions and issues.¹⁹ The scope for careful empirical work is enormous. For example, Beth Simmons's study of the legalization of international monetary affairs combines the theoretical insights of institutionalism with careful empirical work to show that concerns of commitment and reputation help explain states' decisions to create and comply with the rules of the IMF (Simmons 2000).

One of the few sustained studies of a particular economic institution looks at the International Coffee Agreement (ICA) (Bates 1997). As with most international agreements relating to the regulation of the production of commodities, the ICA can be understood as a cartel. However, the history of the coffee agreement shows that the actions of intermediaries—the roasters—and major consumers—the United States—have a significant effect on the ability of producers to limit supplies and so keep prices high. The ability to maintain the cartel, and U.S. willingness to support it, depends on security and other political factors as much as economic considerations. From an institutionalist perspective, one of the most-interesting findings is that specific agreements reached within the ICA, for example, about the distribution of production quotas, were typically implemented

18. For one exception, see Andrew Moravcsik's work (1999) on the role of supranational entrepreneurs in the EU.

19. Economists have also begun to elaborate theories of international economic institutions. See Bagwell and Staiger 1999; Maggi 1999.

and had measurable effects on the price of coffee, indicating a causal impact of institutions on cooperation. One could imagine similar analyses being carried out on other commodity arrangements, integrating more systematically the variables identified by institutionalist theories.

Multilateral trade organizations have received more empirical attention than most economic institutions. Much of this attention has been focused on the dispute resolution mechanisms of GATT and WTO. The analytical framework for studying international institutions draws our attention to their ability to provide information about standards and state behavior. These functions enhance the value of having a reputation for living up to international agreements and allow decentralized enforcement of cooperative agreements to take place. The framework naturally leads us to focus on dispute resolution characteristics of institutions. Empirically, this work has been particularly interesting in the GATT and WTO context, as dispute resolution has been highly formalized (Goldstein and Martin 2000). Institutional analyses of GATT dispute resolution ask about the conditions under which states turn to GATT to resolve disputes (Busch 2000) and the factors that determine the outcome of GATT disputes (E. Reinhardt 1999). Outside GATT, James McCall Smith has explored the variation in formality of dispute settlement provisions in regional trade agreements (J. Smith 2000). Given the trend of other work on international cooperation discussed above, it is perhaps not surprising that empirical studies of GATT dispute resolution emphasize the importance of domestic factors, such as trade dependence and democracy, in determining outcomes. While our analytical framework begins with the simplifying assumption that states can be treated as unitary actors, it is inevitable that analysts begin to integrate domestic politics in a systematic manner into this framework. This move brings us back to our starting point, theories that concentrate on the interaction between domestic and international politics. But the development of such attempts to integrate domestic and international factors can now build on well-developed approaches to international strategic interaction.

In some ways, the empirical research agenda for international factors resembles that discussed above for domestic sources of policy. Work on trade issues is the most advanced, matched closely by that on monetary issues. However, empirical work at the international level differs in that much of it has been focused on broad concepts such as cooperation or stability. While useful, this focus has sometimes distracted from in-depth attention to specific issue areas and particular institutions. The potential for future research seems particularly promising, as we explore the functioning of specific economic institutions in much more detail and compare institutionalized interaction to interaction without the structuring effects of institutions. Doing so will, in part, require a turn away from broad synthesizing concepts to a focus on more concrete variation in state behavior,

such as patterns of trade and investment flows and specific policy decisions. It will also allow us to feed this work back to the challenging attempt to integrate domestic and international analyses.

■ Conclusions

The study of international political economy has made great advances in twenty-five years. After a period of internecine paradigmatic conflict, most scholars in the field have accepted a general, positivist approach to investigating issues in the politics of international economics. The subdiscipline has moved strongly in a direction in which new work builds self-consciously on, rather than firing broadsides against, existing work and in which abstract theoretical work complements empirical scholarship.

The study of IPE requires an understanding of how domestic and international factors interact, and this issue represents the current research frontier. However, understanding interaction requires solid building blocks on both the domestic and international levels. Thus, the investigation of an issue in IPE typically involves a series of interrelated and mutually complementary considerations. Scholars identify the socioeconomic interests at stake, how groups are organized, and how organized and general interests express themselves in the context of domestic electoral, legislative, and bureaucratic institutions. On this basis, other work focuses on the interaction of states at the international level, emphasizing especially the impact of the informational environment and the strategic setting on relations among states. The creation and operation of international institutions is a particularly important component of these international interactions.

While the general method of analysis is well established and widely accepted, this hardly means that IPE has exhausted its potential. In fact, it is surprising how narrow the range of analytical and empirical problems that existing scholarship has tackled in earnest is. Trade policy in the United States, the EMU in Europe, and a few international institutions have been studied in some detail; but almost every other area of IPE is wide open for investigation. It may be that a great deal of theoretical, analytical, and methodological brush needed to be cleared before scholars could give these issues as much attention as they warrant. If so, the academic study of IPE has virtually limitless opportunities to demonstrate the effectiveness and appropriateness of its theoretical and empirical tools.

We have identified both theoretical and empirical areas in need of further research. Theoretically, we see the most important frontier as the integration of the domestic and international levels of analysis. Some promising frameworks have been developed, but this work is in its infancy. We also see scope for applying existing frameworks to a wide variety of understudied empirical issues. Probably the best-studied area of IPE is trade,

followed closely by monetary issues. Financial issues, including international investment of various types, receive little attention in the current literature. Likewise, theoretical work on international institutions has far outstripped the quantity and quality of empirical work. As international organizations such as the WTO, IMF, and potential investment agreements are the focus of sustained political attention and at the forefront of debates about globalization, applying the analytical tools of IPE more seriously to these institutions is likely to be both important and fruitful.

JAMES E. ALT

Comparative Political Economy: Credibility, Accountability, and Institutions

Widespread fiscal difficulties in the late 1980s and the debate over the future of European fiscal and monetary policy in the 1990s produced a near consensus among scholars and practitioners that the design of political institutions matters for economic policy outcomes. Many believed that an independent central bank would produce credible low-inflation monetary policy (for instance, Grilli et al. 1991; Alesina and Summers 1993). This belief informed the design of the European Central Bank and institutional reform in several countries including Italy, New Zealand, and the United Kingdom. Others suggested that “transparent” fiscal institutions would enhance accountability and, if implemented, bring about desirable effects on policy outcomes like those required as conditions of participation in the European Monetary Union (von Hagen and Harden 1994; Alesina, Roubini, and Cohen 1997; Alesina and Perotti 1999). Concern for fiscal discipline also lay behind the extended campaign to enact a balanced budget constitutional amendment in the United States. The movement did not succeed but did encourage ongoing discussion about whether and how such laws affected policy (Poterba 1994; Alt and Lowry 1994; Lowry and Alt 2001), whether they could be statutory as well as constitutional (Inman 1999), and whether they increased economic volatility (Bayoumi and Eichengreen 1995; W. Crain 2000).

All these suggested institutional innovations in fiscal and monetary policy grew out of increasingly elaborate political-economic models of politicians responsible for economic outcomes’ being accountable to and seeking reelection from an electorate that votes retrospectively. Out of these contributions comes an increased understanding of how markets and voters exercise their power to control politicians who hope to be seen as competent in carrying out policy. Principal-agent models that analyze the institutional design of incentives to act in certain ways and to reveal or conceal action and information dominate this literature. Lupia and McCubbins’ model (1998) of delegation and voter information; Lohmann’s analysis (1999) of information transmission in a complex economy;

Fearon's treatment (1999) of competence, moral hazard, and adverse selection in retrospective voting; and Ferejohn's explanation (1999) of the relationship between transparency and accountability all exemplify the new sophistication.

Frequently, the situations these newer models analyze involve the control of politicians by many actors (for instance, voters or market participants) engaged in strategic interaction. The literature is constantly dealing with multiplicity: multiple principals and agents, decentralized unions, governments of many parties. How many are they? How centralized or fragmented? How coordinated are their actions? Under what conditions of centralization, coordination, fragmentation, or numbers do we observe institutional effects on policy outcomes? Naturally enough, as a consequence, the results we review—estimates of systematic effects of institutional variations on outcomes—are also increasingly conditional and interactive.

Multiple actors responding to enforcement incentives can face coordination problems, in ways charted by Kreps (1990a). He shows how strategic communication in appropriate institutional settings can create “convergent expectations” that ameliorate the problem of coordinating enforcement strategies. In one way or another, this intuition grounds much of the modern, institutional political economy of monetary and fiscal policy. The first section provides an introduction, putting the more-specialized and policy-focused literatures in context. The second and third sections narrow the focus and describe respectively how, within institutional political economy, literatures have grown up around the key concepts of credibility in monetary policy and centralization and transparency in fiscal policy.¹ Both literatures reveal the interaction of economic theory and political analysis. This review deemphasizes the normative aspects of the literature and concentrates on the positive questions of how the institutions work and why, and what is the current state of empirical estimates of their effects.

In general, the results demonstrate the importance of relating policy outcomes to not only economic fundamentals but also the ideological or redistributive goals of political parties and interest groups and the institutional context in which policy is made. The final section sums up, relating transparency and credibility and highlighting how literatures on monetary and fiscal policy institutions have remained separate and how they might be brought together.

■ | The Political Economy of Institutions

Institutions affect behavior primarily by providing actors with greater or lesser degrees of certainty about the present and future behavior of other actors . . .

1. Even with that scope, this paper omits important material analyzing the extent of trade and the composition of the economy.

enforcement mechanisms for agreements, penalties for defection, and the like. The key point is that they affect individual action by altering [add: opportunities institutional actors have to affect] the expectations an actor has about the actions that others are likely to take in response to or simultaneously with his own actions. . . . From [a cultural] perspective, institutions provide moral or cognitive templates for interpretation or action. (Hall and Taylor 1996, 939)

Twenty-five years ago, political economy did not rate its own chapter in a multivolume *Handbook of Political Science* (Greenstein and Polsby 1975). The explosive growth of research since then now necessitates separate treatments that distinguish international from domestic political economy (as in this volume), as well as formal or rationalist from more historical approaches (as in the quotation above). Despite these distinctions, a frequent point of agreement is that these approaches view institutions as enabling communication, cooperation, or coordination in situations that involve multilateral enforcement of bargains. The coordinating function may not be viewed in exactly the same way, of course. To the rationalist, institutions provide information about others' incentives and menu of actions, while to the historian, they provide precedent and norms. But this is to a large extent the same thing described in two different ways.

The piece of political economy research reviewed below analyzes how the credibility of monetary institutions and transparency of fiscal institutions affect policy and outcomes like the inflation rate and scale or indebtedness of government. The central concept, institutions, refers to rules, procedures, norms, or conventions designed self-consciously to determine "who has the power to do what when." Political economy focuses on how political and economic institutions constrain, direct, and reflect individual behavior. It explains collective outcomes like production, resource allocation, and public policy in a unified way, stressing the political context in which market phenomena take place and using an economic approach, constrained maximizing and strategic behavior by self-interested agents. In sum, it emphasizes economic behavior in the political process and political behavior in the marketplace.

Institutions help individuals deal with certain fundamental problems of exchange, collective choice, and collective action. If there were no non-simultaneous exchange, ex post opportunism would not be a concern. If information were fully and freely available, specialization and delegation would not produce agency costs. If nothing were ever chosen by vote, there would be no problem of cyclical instability. If there were no social dilemmas, problems of communication, cooperation, or coordination would seem less important. Instead, in the presence of these problems, institutions increase predictability, reduce uncertainty, or induce stability in human interactions.

A, perhaps the, comparative advantage of political science lies in studying self-consciously designed collective choices. Political scientists

treat as a fact that choices are made in institutional contexts. That context, at least partly of human design, limits, constrains, channels, and determines *what* is available to choose, *how* choices from this feasible set are made, and *who* gets to make these choices. Relative to economics, a contribution of political science is to accord equal status to normative and positive questions, focusing *relatively* less on optimal arrangements and more on figuring out how things actually work.

Within the field, there is a sharp division between those who model institutions as rules, procedures, and choice *mechanisms* taken as preexisting and those who model institutions as rules, procedures, and choice mechanisms taken as equilibria in some underlying social game. Two contributions that take institutions as preexisting have been particularly influential: structure-induced equilibrium (see Shepsle and Weingast 1987)² and the Romer-Rosenthal setter model (H. Rosenthal 1990).³ Others, like Schotter (1981) and Greif (1997), define institutions as equilibria in an underlying social game, emphasizing the *self-enforcing* characteristic and *coordinating* function of institutions. Models of such institutions focus on the conditions regarding common knowledge, shared information, and enforcement strategies that must be satisfied (Calvert 1995b).⁴

Suppose there were benefits to both rulers and ruled from private investments promoted by bargains limiting the wealth that the state can appropriate from citizens but that differences in timing and rates at which benefits accrue raise the risk of renegeing or opportunism.⁵ Suppose also that there is no independent third party with the power to compel the agreed performance.⁶ Then such bargains will not be realized. Institutions

2. According to the structure-induced equilibrium approach, vetoes, gate-keeping, or agenda power arise in jurisdictions, specific domains of control like divisions in firms or specialized committees in legislatures, perhaps backed by further rules requiring self-restraint by other actors.

3. The setter model has more structure. It involves a proposer who offers a proposal in a defined sequence of moves, which results in the proposal's being subject to a take it or leave it choice, and a status quo, which is the reversion point if the choice is to leave it. This little sketch of political action turns out, like supply and demand curves, to help analysts make valuable predictions in institutionally complex situations.

4. The historical-institutional analysis of institutions governing exchange in the absence of a centralized legal system includes the game theoretic analysis of agency relations among medieval Mediterranean traders (Greif 1997) and the modern conception of political democracy as a commitment device (Masten 2000).

5. In the same way, politicians can gain from deals with each other, as when legislators trade votes in pursuit of the majorities necessary to enact legislation of benefit to their constituents, or parties bargain to form coalitions based on shared policy commitments.

6. Even if there were, it would be powerful enough to abrogate commitments and transgress the interests of others.

as equilibria can solve such *cooperation* problems by being self-enforcing, that is, by giving agents incentives to abide by the institutional rules. Such self-enforcing political bargains promote commitment, the assurance that when the time comes others will uphold their end of the bargain and encourage reluctant investors or legislators alike to take the risks and bear the costs necessary to achieve joint gains (North and Weingast 1989). Exactly such an institution is central to the analysis of credibility and monetary policy in the following section.

But suppose the self-enforcement of political bargains requires the threat of multilateral or collective action, whether violent, like some protests, or peaceful, like market responses. Suppose further that an individual can impose only a small sanction. Then deterrence of opportunism involves many participants, and would-be collective enforcers must settle on the definition of an infraction and find a way of communicating the occurrence of an infraction and the appropriate response to it (Masten 2000). These would-be enforcers face a problem of *coordination*, which institutions ameliorate by fostering focal points.

Kreps (1990a) shows how the coordination of beliefs offers an equilibrium strategy for dealing with unforeseen contingencies in repeated interactions. As Masten (2000) notes, “constitutions, legislative rules, and other widely shared norms or expectations about the allocation of decision authority may serve this coordination function.” Hardin (1989) proposes that *written* constitutions hasten the process of convergence to shared conventions of behavior among individuals. He argues that “[contracts] are generally backed by external sanctions; constitutions are more nearly backed by . . . the difficulty of reCOORDINATING on an alternative arrangement.” While the literature is not as developed or clearly focused as in the case of monetary policy, the section after next makes central the coordinating role of institutions in fiscal policy.

■ | Monetary Policy: Central Banks, Labor Markets, and Parties

Twenty or even fifteen years ago, this review would have focused on the literature of whether monetary policy could be manipulated for electoral reasons, a literature that started from the stark question of whether Federal Reserve chairman Arthur Burns deliberately helped Nixon gain reelection. A quarter century after that event, Alesina, Roubini, and Cohen (1997) summed up a massive literature by saying, “Electoral monetary cycles occur frequently, but not always, and their size is small.” Instead, for the past two decades, political economy research on monetary policy has been dominated by the time consistency problem. As Kydland and Prescott (1977) showed, policymakers with discretion to set monetary policy are

tempted to expand the money supply in response to adverse shocks but the expectation of this temptation (typically among labor market participants) produces a positive inflation bias that could be avoided with a simple monetary rule. Barro and Gordon (1983) point out that unless policymakers are *credibly* committed to the rule, the basic problem remains.⁷ The challenge for monetary policymakers is thus twofold: to establish institutions that resolve the time-consistency problem inherent in discretionary monetary policy and to find ways policymakers can commit credibly to those institutions.

Barro and Gordon (1983), Rogoff (1985), and others viewed the appointment of a relatively conservative, legally independent central banker as the best feasible solution to these problems, one which promises lower inflation at the same expected level of output, at the price of greater real fluctuation in the economy.⁸ Keech (1995, ch. 8) provides an excellent review of a long list of measurement issues (like legal independence versus behavioral independence, the importance of appointment powers, the use of rules, and the relative priority of stabilization goals in practice) left in the background here.⁹ Even so, examining the conjecture that there is an institution, the independent central bank, that “solves” the credibility problem (that is, gives policy that is *ex post* optimal and consistent with rational expectations) grounds more than a decade of serious comparative empirical work, much of it contributed by political scientists.

Early tests of the proposition, frequently by economists, showed that for a panel of Organization of Economic Cooperation and Development (OECD) countries, higher central bank independence did produce better inflation outcomes (Grilli et al. 1991; Cukierman et al. 1992; Alesina and Summers 1993). If anything, these empirical results were more positive

7. Without commitment, the monetary authority has incentives (from private information about its preferred inflation target) not to follow the “best” rule even if it announced it. Hence the rule is not necessarily credible even if announced, and announcing a noncredible rule is “pointless” (Persson and Tabellini 2000). The study of credibility, by contrast, is almost nonexistent in fiscal policy. Though credibility does occasionally appear in studies of debt and time-consistent capital taxation (Persson and Tabellini 2000, ch. 12) and fiscal stabilization and reform (Perotti 1999) it is not a major theme.

8. Retrospective mass voting cannot discipline the monetary authority. Appointing one whose preferences are for low inflation, so strong as to leave little doubt about intentions, and with the power to carry them out, provides a second-best solution under discretion. But is such an institution credible? That is, does the policymaker have incentives to behave in the appropriate way? Persson and Tabellini (2000, ch. 17) discuss the benefits of contracting for an inflation target.

9. Also neglected here is the comparison with central bank independence of alternative commitment technologies like fixed exchange rates (Clark forthcoming). Brief reviews of contractual and other approaches to the central bankers’ political incentives can be found in Alesina, Roubini, and Cohen 1997, ch. 8, and Persson and Tabellini 2000, ch. 17.

about central bank independence than some theorists had claimed. Central bank independence was a free lunch, producing lower inflation (often by 2 percentage points or more) without even an increase in output variability. However, recent work has been less sanguine. For example, Campillo and Miron (1997) find that the inflation-fighting benefits of central bank independence are not robust to the inclusion of controls for debt, openness, political instability, or past experience with inflation, all of which might be expected to influence the demand for and ability to provide lower inflation. Generally, central bank independence has not been found to explain inflation performance outside the OECD countries (Cukierman et al. 1992; Campillo and Miron 1997; see also Maxfield 1997).

Nevertheless, there have been recent advances in pinning down the nominal and real effects of central bank independence. To a large extent, the progress has come from relaxing restrictive assumptions of earlier models by incorporating (ultimately strategic) interactions among an expanded set of institutional actors besides central banks. Labor market agents (and in particular the character of labor market organization) and partisan governments and legislatures play a prominent role in this literature. There is general agreement, for instance, that an independent central bank does reduce inflation but at some cost to employment; how much varies with labor market centralization in ways described below. Central bank independence should also moderate partisan cycles in inflation and growth, but compelling empirical evidence is elusive, for reasons that become clear below.

The simplest way to incorporate other agents is to consider how the central bank reacts to them, without making the interaction fully strategic. Franzese (1999) argues that as long as inflation results from some convex combination of the preferences of the bank and government, as Lohmann's description (1992) of a partially independent central bank would imply, the anti-inflationary benefit of central bank independence depends on all factors that might lead the government to inflate absent an independent bank. Therefore, if Left governments are expected to inflate more than Right governments, central bank independence should reduce inflation more given Left governance. Likewise, if greater union coverage puts upward pressure on wages or if coordinated wage bargaining produces wage restraint, central bank independence should be more beneficial where there is higher unionization but less helpful in lowering inflation where unions are well coordinated. Overall, Franzese shows for a panel of OECD countries that the impact of central bank independence is conditional on the inflationary pressure facing the bank, in the process finding much more precise estimates of the (conditional) anti-inflation benefit of central bank independence (especially in the face of shocks from abroad) than the preceding literature. Yet if central banks react to unions, and unions anticipate their reactions, a fully strategic model is needed to understand monetary policy.

UNIONS, EMPLOYERS, AND MONETARY POLICY

Fortunately, many recent papers consider how large unions might influence the setting of monetary policy by independent central banks. In Barro and Gordon's model, labor market actors have no wage-setting power and seek only to anticipate the level of inflation set by the monetary authority. But if labor market agents are not simply price takers, central banks must consider the behavior of wage-setting unions and employers when setting monetary policy. One important literature explores this proposition by combining insights gleaned from the study of corporatism with models of the behavior of independent central banks.

Students of corporatism have long argued that the structure and density of labor unions helps determine whether wage demands are militant or restrained. In the most influential study of corporatist institutions, Calmfors and Driffill (1988) argue that increasing the centralization of wage bargaining has two countervailing effects. First, unions' power to demand higher real wages increases, as labor from different unions becomes less substitutable (a competition effect). Second, as unions grow to encompass more consumers, the unions internalize the inflationary effects of real wage increases more (a strategic effect). Calmfors and Driffill argue that wage bargaining at the sectoral level is likely to unleash real wage pressure without the moderation that full encompassment would bring. Real wages, and thus inflation and unemployment, follow a hump-shaped curve over centralization of wage bargaining—either low- or high-centralization produces good outcomes, while moderate centralization can be disastrous.

Others have made parallel efforts to incorporate Calmfors and Driffill's insights into models of strategic interaction between wage-setting unions and inflation-setting central banks. There is broad agreement on the strategic problems. Labor unions want to coordinate wage restraint and central banks want credible inflation prevention. However, the newer models make qualitatively different predictions about inflation and unemployment, largely because they make different assumptions about the goals and organization of labor, its substitutability across industries and unions, and the elasticity of demand for labor.

The place to begin, according to Iversen (1998a, b, 1999), is with the wage setters. They must balance the desire for higher real wages with their fears of or dislike for (within-union and economywide) unemployment and (within-union) wage inequality. At the same time, they anticipate the central bank's reaction, which balances inflation against unemployment. Following Calmfors and Driffill, as the centralization of wage bargaining increases, so does a union's real wage setting power and also the inflation-cost of its own wages. Thus, if the central bank simply accommodates union wage demands (as Iversen supposes it will if the central bank lacks independence), the Calmfors-Driffill hump shape will result for both inflation and unemployment. However, with a nonaccommodating central

bank (high central bank independence), the hump is inverted for unemployment and flattened (and lowered) for inflation. For low levels of centralization, inflation is lower, due to the independent central banks' solving the time consistency problem. Unemployment is unchanged, since money remains neutral (has no real effects) where unions are price takers.

For moderately centralized labor markets, Iversen argues, higher central bank independence leads to dramatic improvement in both inflation and unemployment, since the bank's credible threat discourages wage demands, solving the collective action problem posed by Calmfors and Driffill. Yet by contrast, for highly centralized economies, Iversen argues that high central bank independence may backfire. Encompassing unions in such economies face pressure from the median wage earner to maintain wage equality but find it difficult to achieve this by imposing wage compression (a *relative* lowering of wages) on their most productive workers. Employers are tempted to raise the wages of productive workers or sectors outside the bargaining agreement (a process known as wage drift). To reduce the effects of wage drift, unions may demand higher nominal wages across the board, accepting higher inflation as the price for wage equality. When conservative, independent central banks refuse to accommodate these demands, the consequence is higher unemployment.

Cukierman and Lippi (1999) also seek to incorporate the Calmfors and Driffill framework into a model of monetary policymaking. Specifically, Cukierman and Lippi assume that unions do not care about economywide unemployment or within-union wage inequality, that unions organize by craft rather than industry, and that labor is perfectly substitutable across industries but not unions. Accordingly, they show that while under most conditions central bank independence will lower inflation, the main impact of higher central bank independence on unemployment is to raise the curve Calmfors and Driffill described, producing worse outcomes generally and shifting its peak toward a higher level of centralization. So by changing assumptions, Cukierman and Lippi generate different predictions from Iversen. Organization by craft is common enough to justify their alternative treatment, but the assumption about substitutability is harder to accept.

Hall and Franzese (1998) take a different tack from both Iversen and Cukierman and Lippi. They argue that it is not centralization but *coordination* among bargaining units that matters for the strategic interaction of central banks and unions.¹⁰ Effective coordination can achieve the same positive effects on inflation and unemployment as centralization by forcing the lead union to consider the inflationary consequences its demands would have if they were replicated throughout the economy. Where coordination is present, an independent central bank can further improve

10. They do not analyze the rationale (in the sense of the first section of this essay) for an institution that provides this coordination, however.

economic outcomes by credibly threatening to punish inflationary wage demands. However, absent labor market coordination, high central bank independence may raise unemployment. The central bank's threats to lower the money supply in response to wage demands are not credible, since in decentralized economies, any given union can discount the possibility that its actions will trigger monetary policy decisions. Unable to credibly threaten decentralized unions, a conservative central bank may preemptively tighten the money supply to undermine the ability of unions to demand higher wages in the first place—suggesting a potential real effect of central bank independence (Franzese, forthcoming).

Theoretical disagreements persist over the role of central bank independence in decentralized or uncoordinated labor markets and in highly centralized economies. Nevertheless, there is growing consensus that in moderately centralized or coordinated labor markets (which, after all, was for Calmfors and Driffill the worst case scenario), high central bank independence should produce lower inflation and lower unemployment. Despite the variety of specifications and measures (Iversen, Hall and Franzese, and Cukierman and Lippi all use different measures of labor market centralization or coordination, as well as different codings of central bank independence, with something to be said for each in every case), all the empirical work confirms the benefits of central bank independence (given moderate centralization or coordination of wage bargaining) for both inflation and unemployment. Where unions possess the ability to coordinate their wage demands but the temptation to defect for higher wages exists, conservative, autonomous central banks have proven a credible means of ensuring wage restraint.

Beyond this point, however, there remains considerable disagreement. In decentralized economies, Iversen does not find either real or nominal effects of central bank independence. Hall and Franzese find that central bank independence in those cases lowers inflation but raises unemployment. Cukierman and Lippi find that both unemployment and inflation are worse. There is no obvious explanation for this. At the same time, reassuringly for those who hope to see empirical evidence cumulate, in economies with highly centralized labor markets, most authors find modestly improved inflation under high central bank independence. But where Hall and Franzese, and Cukierman and Lippi observe a modest reduction in unemployment (for the latter, contrary to expectation), Iversen finds dramatically higher unemployment. While Iversen's measure of labor market centralization is less subjective and finer grained, a handful of outlier cases are probably crucial to the result for high centralization of wage bargaining. The damaging effects of public sector unions on wage restraint (Garrett and Way 1999) further complicate analysis of the high-centralization case. This may also underpin the relative failure of the high central bank independence–high centralization of wage bargaining combination (Franzese forthcoming). So while, broadly speaking, a lot of

progress has been made in the last few years, divergent understandings and measures of central bank independence and centralization of wage bargaining still need to be reconciled in future research.

PARTIES AND MONETARY POLICY

So far the review has proceeded without mentioning political parties, as if the effects of parties were perfectly captured by either central bank independence or labor market centralization. However, reducing the problem of monetary policy to the interaction between unions and central banks leaves out partisan governments in at least three ways. Parties could have an independent role in the *institutional design* stage, by appointing central bankers and establishing the cost of overriding the bank. Parties could affect the monetary policy *decision* stage, by overriding central bank decisions.¹¹ Finally, governing parties could have independent effects in inflation and employment in *negotiations* with labor market agents, by making credible policy commitments in exchange for wage restraint.

INSTITUTIONAL DESIGN

Bernhard (1998) has argued that democracies with ideologically diverse coalition governments are more likely to adopt independent central banks, since backbenchers who do not share the ministers' policy preferences can use the central bank as an independent source of information to keep tabs on the cabinet's economic policies.¹² Bernhard supports his argument with panel data showing a tendency toward higher central bank independence in countries with center-left governments (intracoalition diversity) and federalism (regional diversity). While there has been little comparative work on the parties' role in the appointment process, some evidence from the United States shows that more conservative parties appoint more conservative central bankers (Chapell, Havrilesky, and McGregor 1993). At the least, the continuing partisan variation in appointments during a period of consistently high legal independence of the Federal Reserve has to raise concern about whether the comparative politics empirical work discussed above confounds central bank independence with central bank conser-

11. There is an extensive literature on how partisan preferences and institutions interact in a setter model framework in the setting of U.S. monetary policy. The most recent estimates (Morris 2000) are that each incremental point of "liberalism" (measured by ADA scores) of the "pivot" lowers the real federal funds rate by about seven basis points. That means roughly that the difference in interest rates attributable to say, the difference today between a moderate congressional Republican and Democrat, would be about two-tenths of a percentage point. Morris 2000 contains a complete bibliography of the relevant literature.

12. This is one point where the monetary policy literature borders on transparency, since the role of the independent central bank is to improve the monitoring of policy settings.

vatism. Put another way, analysis of the partisan origins of central bank appointments could be a worthwhile study for students of comparative monetary politics.

MONETARY POLICY DECISIONS

Independent central banks strike many observers as an obvious potential remedy for both opportunistic and partisan political cycles (Alesina, Roubini, and Cohen 1997; Waller 1992). Clark and Reichert (1998) provide some evidence that central bank independence does indeed moderate opportunistic behavior, but evidence for moderation of partisan cycles remains elusive. Lohmann (1998b) argues that decentralized appointment procedures for central bankers (as in the U.S. Federal Reserve Board, which combines executive appointments with privately selected board presidents) may be the best way to avoid opportunistic cycles, since centrally appointed bankers may behave opportunistically to improve their party's chance of reelection.

PARTISAN INTERVENTION IN LABOR NEGOTIATION

Lange and Garrett (1985) argue that partisan governments and labor market arrangements are complements. They believe the Right is better suited to a decentralized labor market while the Left does better in centralized labor markets, since it can credibly promise policy side payments for wage restraint. Right parties, and, presumably, conservative central bankers, may be unable to make such commitments, since neither depends on the votes of labor union members.

While Lange and Garrett initially found support for complementarity among parties and labor market institutions, in more recent work, Garrett (1998b) finds less evidence that a combination of Left governance and encompassing unions produces good economic outcomes. He finds no association between Left-encompassment and inflation and obtains significant results for growth and unemployment only for countries highly open to the international economy. Unfortunately, his results do not yet incorporate the more recent strategic analysis of interactions between the labor market and central banks, but this could certainly be combined with Lange and Garrett's earlier insights about parties.

OTHER INSTITUTIONS

Beyond unions and parties, a growing body of work argues that central banks will be more credibly independent where power is more divided, whether through separation of powers or through federalism. Lohmann (1998a) claims that German federalism may lie behind the legendary independence of the Bundesbank, since the Bundesrat allows the interests of the Lander to check the national government, protecting the central bank's legally independent status. Bernhard (1998) provides general empirical

support for this proposition in a panel of OECD countries. However, while the finding of lower inflation rates in federal systems seems robust, Wibbels and Rodden (2001) show that the difference could result from decentralization of tax powers and the impact of borrowing costs, without reference to central bank independence.

Moser (1999a) generalizes the veto players argument, showing that where a central bank can be overridden only by the combined vetoes of separate policymakers (e.g., an executive and a legislature), central bank independence is more credible. It is also potentially more conservative, since the central bank can choose to accommodate the more conservative policymaker when necessary. And indeed, Moser finds that central bank independence more strongly reduces inflation in those OECD countries where bicameralism provides two potential vetoes on any change in central bank institutions. Still, while these early tests for association among separation of powers, federalism, central bank independence, and inflation give encouraging results, there is fairly high collinearity of these institutional variables across cases, not to mention the conspicuous presence of all of them in the United States and Germany. All this suggests that the analysis of their importance to monetary politics is still at an early stage.

■ | **Fiscal Scale and Balance: Fundamentals, Institutions, Parties**

Both debt and spending levels have increased substantially across OECD countries since the mid-1970s. In large part as a response to the wide variation in fiscal policy settings and outcomes over the past thirty years, a vast literature, which has grown substantially over the past decade, examines fiscal policy decisions and outcomes. This literature has focused predominantly on *fiscal scale* (levels of spending and revenue) and *fiscal balance* (deficits, debt).¹³ More than in the case of fiscal balance, the literature on fiscal scale has concentrated on the effects of institutions and politics, rather than explaining variation in the levels of taxing and spending (or indebtedness, in the case of balance) per se. Nevertheless, there are common elements in the explanations of both fiscal scale and balance. In particular, three types of factors affect fiscal policy outcomes: *fundamentals*, including economic variables and demographics; *institutions*, fiscal, political, and electoral; and *partisan politics*, the ideology of the incumbent.

Twenty years ago, a survey of explanations would have included each factor. Income growth and decline (Barro 1979), the level of income (e.g., Alt and Chrystal 1983) and its distribution (Meltzer and Richard 1981),

13. Keech 1995, ch. 7, provides an excellent discussion of a wide range of topics relevant to the analysis of fiscal policy.

the trade dependence of the economy (Cameron 1978), as well as the age structure of the population, exemplify the first. The number and geographical nature of legislators' constituencies (Weingast, Shepsle, and Johnsen 1981) is an example of the second, as is the hypothesis that some tax structures and electoral systems promote fiscal illusion (Buchanan and Wagner 1977). Some (Cameron 1978; Alt and Chrystal 1983) conjectured about parties and debt or the size of government. However, many of the conjectures seemed to have hit an empirical dead end by the early 1980s, and few contained any theoretical elements that could explain dynamics, growth, or change over time.

Since then, two approaches have dominated theoretical research on the fiscal effects of institutions. One examines policy as the outcome of a game involving *multiple principals and multiple agents*. What is central is both the multiplicity and also the agency problem, with opportunistic politicians who bargain over policy and rational voters who hold them accountable through retrospective voting (Persson and Tabellini 2000). In these models, institutions affect outcomes through two channels. First, the rules governing legislative bargaining and the nature of elections affect party competition directly. Through this competition the rules also affect fiscal policy. A fundamental question is, do the principals manage to play the agents off against each other or vice versa? Using this approach allows Persson and Tabellini to derive conjectures about conditions under which more decentralization can mean less corruption, when there will be a bigger electoral cycle and less transparency, when proportional systems produce more welfare—social security spending, and, if well-placed checks and balances allow voters to induce politicians to discipline each other, when presidential systems would produce a smaller size of total government spending.

The other approach treats (fiscal) institutions as a response to the *common pool resource* problem.¹⁴ Its core intuition is that politicians spend more on their constituencies to the extent that they do not internalize the full costs of their spending and taxing decisions. Multiplicity matters again in this model. Competition between claimants on the budget generates a spending bias because each of n claimants internalizes only $1/n$ of the cost of financing an additional unit of spending (von Hagen and Harden 1995). More decision participants, more heterogeneity among them, more spillovers, all exacerbate the problem. The intuition is that reducing the number of decision makers or centralizing the budgetary process lowers spending.

This section first reviews fiscal scale and then fiscal balance. It examines the fiscal performance of both OECD countries and the U.S. states. Unsurprisingly, largely because of substantial institutional differences, the

14. This approach goes back to Weingast, Shepsle, and Johnsen 1981 and even earlier.

literatures on OECD countries and the U.S. states have grown independently, with separate concepts and explanations. Disappointingly, little can be said with great confidence about the determinants of fiscal scale and balance. Fundamentals matter but leave much unexplained. In terms of the residual variation, effects of politics and institutions are not yet well understood. The effect (particularly) on fiscal balance of centralization and transparency of fiscal institutions is clear, but endogeneity remains a concern. Do transparent institutions produce good performance, or does good performance lead to institutions that give voters a better look?

FISCAL SCALE¹⁵

FUNDAMENTALS

A long tradition links spending levels to fundamentals. According to Wagner's law, public spending rises with per capita income (Mueller 1989). Franzese (1998a) provides recent supporting empirical evidence for this. Other economic variables like growth or unemployment also affect spending. In general, positive economic events reduce spending in the short term while negative events like an increase in unemployment trigger either discretionary or automatic spending increases. Economic variables explain much of the variation in spending.

The relationship between external trade and the level of spending is less clear. Cameron (1978) identified a positive relationship between trade and spending. In subsequent empirical work, Garrett (forthcoming) reports that although open economies have higher levels of public spending, recent increases in trade have led to slower growth in public spending. Garrett and Mitchell (2001) find a negative relationship between openness and spending, a finding partly confirmed by Rodrik (1998) in a smaller sample of countries. Rodrik also claims that measures of external risk (for instance, volatility in the terms of trade) have a larger effect than trade itself. He finds that public spending increases as external risk increases. So increases in globalization may have slight contractionary effects on public spending.

Public spending also should respond to demographic variables, most notably characteristics of the income distribution and of the voting population. According to Meltzer and Richard (1981), there should be a positive relationship between transfer spending and both the skew of the income distribution, since with a right-skewed income distribution typical of industrial societies, the median voter would have an income less than the mean. Building on this, Franzese (1998a) shows that if an increase in the distance between the income of the actual median voter and average income in-

15. The voluminous, earlier public choice literature on the growth and size of government is not discussed here. See D. Mueller 1989 for a useful summary.

creases transfers, then increased voter participation also has a positive effect on the scale of government (see also Hicks and Swank 1992).

Indeed, there is plenty of survey evidence that lower-income individuals prefer more redistribution. Hence, it is a little paradoxical that more unequal societies do not on the whole redistribute more than more egalitarian ones. There could be measurement problems here, but Moene and Wallerstein (2001) offer a partial resolution by distinguishing between transfers as welfare and transfers as insurance for the temporarily unemployed. This result is more consistent with Rodrik's (above). Iversen and Soskice (2001) offer more support, showing how job insurance policy preferences depend on individuals' views of their immobility, or the difficulty they would have securing an equally good job if they were to become unemployed.

EFFECTS OF BUDGET INSTITUTIONS

There are scattered empirical findings, reflecting the fact that empirical research on the effects of political and fiscal institutions has been directed more toward explaining variation in fiscal balance than scale. Von Hagen (1992) provides some evidence that decentralized budget processes lead to higher levels of spending. Similarly, Kontopoulos and Perotti (1999) find a strong relationship between the number of spending ministers and spending growth. In terms of the U.S. evidence, Rueben (1995) shows that tax limitation laws are associated with slower growth in both spending and revenue. Balanced budget laws also seem to reduce spending growth (Poterba 1996; Bohn and Inman 1996). In general, models that predict a spending bias make stronger predictions than when they are used in the case of deficits.

In a recent paper, Ferejohn (1999) provides a theoretical analysis of transparency. He argues that increasing transparency facilitates voter control and monitoring of elected representatives, inducing greater executive effort which, in turn, increases voter confidence and the willingness of voters to spend resources through the public budgets, leading to an increase in the scale of government. Budget process transparency means commitment to nonarbitrary language (for example, generally accepted accounting principles), independent auditing and verification, and more information and more justification in fewer documents. Using a variety of measures published by the National Association of State Budget Offices (1995, 1999) and the National Conference of State Legislatures (1998), Alt, Lassen, and Skilling (2002) show that the expected links between transparency, executive job performance, and fiscal scale appear in the U.S. states.

EFFECTS OF POLITICAL AND ELECTORAL INSTITUTIONS

In an imaginative application of common-pool theory, Baqir (1999), following Weingast, Shepsle, and Johnsen (1981), shows that a greater num-

ber of districts (seats) on city councils predicts higher spending, even after controls for ethnic heterogeneity, other sorts of dispersion and spillovers, and electoral system characteristics. For similar reasons, political institutions that fragment decision making across branches of government (for example, divided government or minority government) are thought to increase spending, because each agent internalizes less of the costs of their spending decisions. However, there is little direct empirical evidence on this point, and, as documented in the U.S. states, divided government may in fact make attaining desired spending levels more difficult.

Other electoral institutions also seem to matter. For example, Persson and Tabellini (1999) predict that proportional systems of voting lead to higher spending. Milesi-Ferretti et al. (preliminary work, 1999) and Kontopoulos (1997) provide supportive evidence for OECD countries and Stein, Talvi, and Grisanti (1999) provide related evidence in Latin American countries. Franzese (1998a) finds a positive relationship between the frequency of the electoral cycle and transfer spending. Further, Pampel and Williamson (1989) link spending growth to electoral competition.

PARTISAN POLITICS

Here, the intuition linking the ideological complexion of the government to fiscal scale or the size of government is clear. Left governments are more likely than Right governments to want or to target a high level of spending. Moreover, in addition to examining straight ideological complexion, the existence of social democracy, Christian democracy, and corporatist institutions (for instance, union power or collective bargaining) is predicted to lead to increased spending.

Clear results exist with respect to the U.S. states. Alt and Lowry (1994) and Besley and Case (1995) find that Democrats have a higher target spending level. Alt and Lowry (2000) confirm this. Nevertheless, in OECD countries, although early cross-sectional studies (Cameron 1978; Roubini and Sachs 1989a, b) found a positive relationship between Left governments and the fiscal scale, recent work is empirically less conclusive, according to a recent survey by Blais, Blake, and Dion (1993, 1996). For example, in a well-known paper, Hicks and Swank (1992) find that Left governments spend more, but their results are not robust. When Beck and Katz (1995) reestimate using more rigorous methods, they find coefficients that are not significant. Franzese (1998a) confirms that the partisan effect is not significant although it moves in the expected direction. Similarly, Garrett (forthcoming) does not find a significant partisan effect, although he notes that countries with more frequent Left governments are associated with higher spending. Perhaps in spite of work showing clearly that parties have different preferences, as Garrett (1998b) notes, "government spending is driven far more by the business cycle and demography than by political conditions."

Blais, Blake, and Dion conclude that "the partisan composition of gov-

ernment makes a difference, though a small one" (1993, 57). More pertinently, they find that, on average over all results, if a government of the Left has been in control for ten years, spending will be 5 percentage points higher than if a government of the Right had been in control. Amazingly enough, that is almost exactly the same steady-state magnitude of difference found by Alt and Lowry (2000) for the difference between Democratic and Republican parties in the U.S. states. So there is some empirical convergence here, even if confidence in individual results is not strong.

FISCAL BALANCE

Apart from fundamentals, even before the 1980s, theoretical models showed how political and institutional factors like fiscal illusion and geographically dispersed interests could affect fiscal balance (surpluses and deficits). An excellent review of newer developments (Alesina, Roubini, and Cohen 1997) adds four types, including models of debt as a strategic variable (Alesina and Tabellini 1990; Tabellini and Alesina 1990; Persson and Svensson 1989), intergenerational redistribution (Tabellini 1991), distributional conflict (Alesina and Drazen 1991), and fiscal institutions (von Hagen 1992). Nevertheless, the empirical literature lagged behind these theoretical developments, and even after ten years of empirical research, some of these theoretical models still lack empirical support. Indeed, until Franzese 1998b there were few direct tests of these models.

Rather, since Roubini and Sachs 1989a, b, the empirical literature focused more on measuring the effects of (exogenous) political institutions on fiscal performance. A stronger consensus has emerged that institutions have a real effect on fiscal balance. Three classes of institutions in particular seem to be important: fiscal institutions (the presence of a centralized budgetary process, balanced budget laws), political institutions (coalition government, divided government), and electoral institutions (electoral system, political stability).¹⁶

FUNDAMENTALS

The normative benchmark for explaining variation in the fiscal balance is the tax-smoothing model (Barro 1979; Lucas and Stokey 1983), where deficits result from temporary variations in the economic environment. Franzese (1998b) lists the economic controls that should be included in a test of the model—unemployment, real growth, interest rates, the terms of trade, and the openness of the economy—and finds that these variables all

16. Despite some very strong prior beliefs, the consensus seems to be that there is no systematic effect of the size of government on measures of economic performance (Slemrod 1995). A large macroeconomic literature examines the effect of fiscal balance on economic growth or performance, surveyed by Bernheim (1989) and Barro (1989).

have a substantively large, and statistically significant, effect on fiscal balance in the expected way. Increases in growth and the terms of trade improve fiscal balance, while increased unemployment, interest rates, and openness worsen the fiscal balance.¹⁷ Overall, Franzese (1998b) estimates that fundamental factors explain about 43 percent of the variation in debt accumulation in postwar OECD countries. Since this leaves a good deal of variation in debt accumulation unexplained, models incorporating political and institutional factors have been developed to explain the remaining variation.

BUDGET PROCESS INSTITUTIONS

The spending bias from fragmentation or the common-pool problem does not easily generalize into a deficit bias (Alesina and Perotti 1996; Kontopoulos and Perotti 1999) since the increased spending can be financed by taxation (Weingast, Shepsle, and Johnsen 1981). There is one dynamic common-pool resource model in which fragmented budgetary decision making leads to a deficit bias (Velasco 1997), but the results are sensitive to functional form and general statements cannot always be made (Kontopoulos and Perotti 1999). Like others following Roubini and Sachs, Kontopoulos and Perotti appeal to the intuition that governments with fragmented decision-making structures will face greater problems reacting to negative shocks, but concerns remain with this conceptual framework.¹⁸

Nevertheless, Poterba and von Hagen (1999) conclude that findings that the budgetary decision-making process has a large and significant effect on fiscal performance are robust. Fiscal institutions respond to the deficit and spending bias caused by the common-pool resource problem (von Hagen and Harden 1995; Velasco 1997). Establishing a centralized budgetary decision-making process (for example, creating a dominant role for the minister of finance or establishing negotiated fiscal targets) is an appropriate response to this problem, as it better internalizes the common-pool costs associated with multiple ministries. Evidence in von Hagen 1992, 1998, von Hagen and Harden 1994, 1995, Hallerberg and von Hagen 1997, and de Haan et al. 1999 strongly supports this position for

17. High interest rates produce higher costs of servicing debt, dominating the effect of low interest rates in producing a good time to borrow. Debt can also smooth out longer-term changes like that associated with an aging of the population. However, such demographic variables do not seem to have much explanatory power for variation in debt accumulation.

18. The relationship of fiscal policy and output volatility has also been examined. One stylized fact to emerge is that many fiscal policy settings are pro-cyclical, so fiscal policy can lead to more rather than less volatile output. This pro-cyclicality is frequently associated with weak or fragmented political and fiscal institutions. This is particularly true in developing countries (Gavin and Perotti 1997; Talvi and Vegh 2000) but is also the case in OECD countries (Lane 1999; Fiorito 1997).

OECD countries. Stein, Talvi, and Grisanti 1999 and Alesina and Perotti 1999 do the same for Latin American countries. Kontopoulos and Perotti 1999 shows that fragmented decision making caused by a high number of spending ministers is associated with higher spending and deficits in OECD countries (with each extra minister producing perhaps an extra 1/6 percentage point of growth).¹⁹

In the United States there is more concern with the effect of balanced budget laws. Bohn and Inman (1996) show that tight balanced budget rules reduce deficits, and maybe also spending growth. Poterba (1994) and Alt and Lowry (1994) show that there is a more rapid fiscal tightening in response to an unexpected deficit in states where there are tight rules. Poterba (1996) surveys this literature and concludes that the empirical evidence shows that “tightly drawn anti-deficit rules, especially when coupled with limits on government borrowing, reduce state deficits and affect spending and borrowing levels as well,” so states with tighter requirements generate superior fiscal performance. Data on relative state bond yields shows that balanced budget laws affect the risk premium charged on debt issued by these states. Lowry and Alt (2001) show how a balanced budget law found in about half the U.S. states improves the ability of market investors to learn about policy from noisy signals. However, these laws may also increase output volatility (Bayoumi and Eichengreen 1995; Crain 2000).

In a similar way, the transparency of institutions affects the cost of monitoring and assessing performance and thus the potential for agency problems. It is widely believed that fiscal transparency has a large and positive effect on fiscal balance. Indeed, Alesina, Roubini, and Cohen assert that “hierarchical-transparent procedures have positive effects on fiscal discipline” (1997, 240). Alesina and Perotti claim that “a lack of transparency makes fiscal discipline and expenditure control harder to achieve” (1996, 403) and Poterba and von Hagen write that “higher levels of transparency are associated with lower budget deficits” (1999, 2). According to the IMF, “transparency in government operations is widely regarded as an important precondition for macroeconomic fiscal sustainability, good governance, and overall fiscal rectitude” (Kopits and Craig 1998, 1). Indeed, both the IMF and the OECD are currently developing codes of best practice for fiscal transparency.

However, the case connecting institutional transparency and fiscal

19. Fiscal rules have also been examined recently, largely motivated by the provisions in the Maastricht Treaty. There is little evidence that these rules have a substantial effect in OECD countries (Kopits and Symansky 1998), though the consensus is that fiscal rules will lead to more volatile output (Bayoumi and Eichengreen 1995). Centralized budgetary processes also seem to lead to better consumption smoothing in the economy, and reduce the pro-cyclicality of fiscal policy (Arreaza et al. 1999).

policy outcomes is far from compelling, more often asserted rather than empirically established. Von Hagen (1992) and de Haan et al. (1999) show a very weak link between some measures of fiscal transparency in European countries and fiscal performance. Alt, Lassen, and Skilling (2001) show that OECD countries with more transparent budget processes accumulate less debt over time. Alesina and Perotti (1999) argue that transparency has a positive effect on fiscal performance in Latin American countries but use very indirect measures of transparency. However, many remain convinced of the importance of fiscal transparency, justifying further analysis.

Even less analytical attention has been paid to why particular (centralized, transparent) fiscal institutions are established than to why countries have independent central banks, creating problems in estimating the effects of fiscal institutions (Poterba 1996; Alesina and Perotti 1996). These institutions do emerge endogenously in accounts of budgetary reform in the United States (Kiewiet and McCubbins 1991; Stewart 1989). Recent attempts to understand and explain systematically the origins of variation in fiscal institutions (Hallerberg and von Hagen 1997; Strauch and von Hagen 1999) leave much to be done. Indeed, Poterba and von Hagen identify the endogeneity of budget rules as “the single most important issue for further work” (1999, 12).

POLITICAL AND ELECTORAL INSTITUTIONS

Tsebelis (1999) models how more veto players lead to greater policy inertia or stability. Others propose that fragmented decision making leads to worse fiscal performance rather than inertia. Thus, Persson and Tabellini surveying the literature, claim that “debt and deficits appear to be correlated with specific political and institutional features; debts have typically been accumulated by countries ruled by coalition governments and/or unstable governments” (2000, 345). Of course, if stability or inertia means not responding to shocks, resulting in debt accumulation, these approaches are not at all inconsistent.

Indeed, Roubini and Sachs (1989a, b) found that coalition and minority governments generated worse fiscal performance, accumulating more debt. However, this finding has been challenged subsequently. Edin and Ohlsson (1991) found that poor fiscal performance was associated with minority governments rather than coalition governments. De Haan and Sturm (1994, 1997) and Borrelli and Royed (1995) find that neither of these results is robust. More recently, Kontopoulos and Perotti (1999) find a weak relationship between the number of coalition parties and fiscal performance. In bad economic times, having more parties in the governing coalition slows the government’s response to shocks (see also Alesina and Perotti 1996). Alesina, Perotti, and Tavares (1998) find that coalition and minority governments are equally likely to tighten fiscal policy,

although these governments seem to find fiscal tightening difficult to sustain.²⁰

In U.S. states, Alt and Lowry (2000) provide evidence that divided government makes it more difficult for each party to adjust to its desired level of spending. Alt and Lowry (1994) and Poterba (1996) find that U.S. states with divided government take longer to respond to budget imbalances due to economic shocks. McCubbins (1991) attributes U.S. federal deficits in the 1980s to divided government, but Alt and Stewart (1990) do not identify a consistent relationship between divided government and fiscal balance in the United States over the last 130 years. Despite the wealth of findings, a reasonable person would not yet be convinced that a systematic relationship exists between fragmented political institutions and fiscal performance.

Electoral process characteristics also influence fiscal balance. (Ignore for now the connections between the electoral system and coalition or divided government.) Polarized political parties facing electoral uncertainty were identified as an important determinant of fiscal balance: the more likely a party was to be replaced by one with very different priorities, the more incentive it had to tie the hands of its successor by increasing borrowing (Alesina and Tabellini 1990; Tabellini and Alesina 1990). However, despite the continued popularity of the theory, this model has found little empirical support (Franzese 1998b).

However, some evidence associates political instability with worse fiscal performance. Grilli et al. (1991), Franzese (1998b), Roubini and Sachs (1989a), and de Haan and Sturm (1994) report that debt is positively related to the number of changes of government and the frequency of elections in OECD countries. Further, Alesina, Roubini, and Cohen (1997) report some evidence of a small systematic fiscal loosening prior to elections. Edwards and Tabellini (1991) and Roubini (1991) show that more frequent government turnover is associated with slightly worsened fiscal performance in developing countries. Kontopoulos (1997) also finds that elections that are more competitive generate higher spending levels and deficits. This result appears stronger in countries with more proportional electoral systems.

PARTISAN POLITICS

There is no clear theoretical prediction about the relationship between partisan policy preferences and fiscal balance. Lowry, Alt, and Ferree

20. Examining the macroeconomic effects of fiscal tightening, the key finding is that many recent fiscal adjustments have had an expansionary effect on the economy, particularly those implemented at high debt levels, those which focus the adjustment on spending cuts, and those which occur in bad times (Giavazzi and Pagano 1990, 1996; McDermott and Westcott 1996; Alesina, Perotti, and Tavares 1998; Alesina and Ardagna 1999; Perotti 1999).

(1998) show that U.S. voters have clear expectations that different parties will provide different fiscal scale but no expectation of partisan differences in fiscal balance. Alesina, Roubini, and Cohen (1997) find no relationship between the ideological complexion of the incumbent and fiscal performance. Franzese (1998b) finds very small partisan effects on balance. Intuitively, Left parties might accumulate debt, as they want to spend more and may be more reluctant to tighten fiscal policy. However, from even casual observation, Belgium, Italy, and Japan, dominated by Right parties, have the three highest debt levels in the OECD.²¹ Most empirical studies contain some measure of ideological complexion of government, and no systematic effect has been detected.²² While the consensus seems to be that partisan effects do not explain patterns of debt accumulation, there is some suggestion that polarization has a negative effect on fiscal performance, and this may be more important than the ideological complexion of the government per se.

■ | Summary

What is the state of the discipline? First of all, we're in a new world. Standard textbooks in macroeconomics will include formal models of politics and political variables in macromodels. Examples include the extensive treatment of institutions and parties in Persson and Tabellini (2000) and interest group behavior in Grossman and Helpman (forthcoming) among others. Not only that but there is also the simple fact that the empirical work on monetary and fiscal policy to date—much of it done by political scientists—shows that things political scientists know and care a lot about, like parties and institutions, really matter for policy outcomes.

Monetary institutions are designed to resolve intertemporal coordination problems (time inconsistency) with minimal distributive implications. Because monetary policy is less distributive in nature than fiscal policy, there may be fewer disagreements among groups on the optimal policy outcome.²³ Therefore, central bank independence (depoliticizing the process) seems like an appealing way to solve the intertemporal coordination problem. But that is only a part of the question of why monetary institutions take the form they do.

21. An exception is the model of Persson and Svensson (1989), where parties differ over the scale of government, in which left-wing parties should run a surplus, right-wing parties run a deficit. Direct tests of this model have not found empirical support.

22. Results are not robust and in fact vary according to the countries included and the sample period.

23. Still, partisan differences do exist with respect to the trade-off between unemployment and inflation.

The banker's concern for preserving a reputation as a means to attain credibility is a driving force in the model of central bank behavior. But is there more credibility in some settings than others? Why? Are there alternative commitment devices that would give as much credibility? With what other costs and benefits? Opportunistic policy settings could also be avoided by contracting, as well as by making policy decisions subject to multiple veto players. The latter, though, could lead to more uncertainty about the inflation level, so inflationary expectations might not come down as quickly, and so raise the costs of disinflation. Also, multiple veto players does not resolve the issue of who *proposes* the optimal rate, solved in the case of central bank independence by *assuming* the central banker is a conservative.

In the case of fiscal institutions, two types of coordination concern us. One, between members of the governing party or coalition, enables them to balance the budget across the cycle and reduces the spending bias (intra-temporal coordination). The other is between competing political parties that alternate in office through time (inter-temporal coordination). Absent coordination, more debt accumulates and spending growth is higher and more volatile.

Credibility is less able to solve these fiscal coordination problems. A brief thought experiment reveals why. In ways analogous to a central bank's announcing an inflation target, state governors (often by law) announce a target deficit when filing the budget. At that point, private actors form an expectation of what, given their information at that time, they think the deficit will be. The bank's credibility is measured by how close inflation expectations are to the announcement (Faust and Svensson 2000), and governors are more credible the less the difference between their announcement and private expectations of fiscal balance at the time the announcement is made. In monetary policy, labor supply and thus income growth depend on these expectations. In fiscal policy, a balanced budget law allows markets and governors to coordinate on and communicate a standard for responsible conduct, lowering interest rates on state debt (Lowry and Alt 2001). This may be the only quantity in an economic model that depends on the credibility of the governor's announcement, so reputation and credibility are smaller issues here.²⁴

So more transparency seems like a good thing to have, and indeed it probably usually is. In general, transparency better allows agents to understand whether deviations from expectations are the result of opportunism or stochastic shocks, a central concern in models of accountability. But where should we look for transparency? Some place it at the level of the political system, arguing that democracy is by its nature more transparent

24. This is probably also why no one finds that laws requiring the *announcement* of a balanced budget have any effect. Instead the outcome is what is observed and judged by markets and voters.

than autocracy. Others look at the regime level, as if transparency were a variable characteristic of a political system. Transparency might reside in a system's propensity to generate complex, multiparty governments or the extent to which it diffuses power across branches of government, or government and opposition. These things of course also vary over time within individual systems. And transparency can be found in the characteristics of individual institutions like budget processes, where the commitment to nonarbitrary language, availability of independent verification, and presence of more information and justification in (other things equal) fewer documents increase the ability to detect what is going on. This is not far-fetched: the OECD and IMF cross-nationally and various organizations within countries now routinely survey and publish such information.

As a concluding note, while credibility and transparency seem like complements, in fact increasing transparency too far undoes credibility, that is, reduces the extent to which the concern to maintain a reputation can give policymakers incentives to behave in socially desirable ways. The logic of this is that while more transparency, like the desire for credibility, can induce greater effort from an agent, at the limit knowing too perfectly what an agent's preferences are undoes the agent's ability to induce a response (Faust and Svensson 2000). In some partisan models of political economy, voters are *sure* of the redistributive motives of politicians (Alesina and Rosenthal 1995), so policymakers who redistribute *ex post* cannot achieve best credible outcomes (Besley and Pande 1998). Trade-offs like these will inform attempts to further endogenize monetary and fiscal institutions—an important direction of research in the next few years.

International Conflict: Assessing the Democratic Peace and Offense-Defense Theory

International relations saw four main movements during the decade of the 1990s. First, game theory attracted attention as a way to study the micro-foundations of the field (cf. Powell, this volume). Second, interest in statistical methods revived in both international political economy and conflict. Third, the constructivist movement challenged the questions, approaches, and results of the mainstream. Fourth, the end of the cold war led to new problems for study and a shift in focus away from great power rivalry. The first three of these trends created methodological ferment, which fortunately bubbled forth with less acrimony than in past decades (cf. Knorr and Rosenau 1969 for a view of the 1960s). However, the field has not reconciled this ferment into a commonly accepted approach for using these methods. This essay reviews progress in three areas: game theoretic micro-foundations, the democratic peace, and offense-defense theory. Each of these areas has predominantly used one of three methods: formal models, large-*n* statistical studies, and case studies, respectively. This essay goes beyond reviewing each of these areas to discuss how the three methods can be combined fruitfully, which none of these areas has fully done to date.

Because this essay reviews only three literatures, albeit three central literatures in conflict in the last decade, it cannot do justice to the diversity of topics and approaches studied. I refer interested readers to reviews on the following topics: the causes of war (Levy 1998), deterrence (Huth 1999), alliances (Morrow 2000), arms races (Glaser 2000a), civil-military relations (Feaver 1999), internal war (David 1997), and psychological approaches (Rosati 2000).

This essay complements David Laitin's review in this volume of comparative politics, which focuses on the contributions of formal models, large-*n* studies, and case studies to that field. I begin with the microfoundations of conflict as developed in game theoretic models. The democratic peace has been driven by large-*n* studies, and case studies have been central to the study of the effects of the offense-defense balance. I review each of those literatures in order. I conclude with some general comments on

how the three methods complement one another and the proper roles for each in the study of international conflict. I use the three literatures to illustrate the concluding argument.

■ | Bargaining and the Microfoundations of Conflict

Systemic theories attempt to explain international conflict as a consequence of the properties of the international system. Microfoundations address the actor-level processes that underlie such systemic theories. During the 1990s, noncooperative game theory took ideas about how images enter into bargaining first stated by Jervis (1970) to study the microfoundations of international conflict (e.g., Fearon 1995b; Morrow 1989; Powell 1999).

This analysis focuses on the origin of crises and their escalation to war. Crises force states to bargain over issues, such as division of territory. They arise because a state makes demands for changes in the status quo. Events outside the control of any government, border skirmishes for instance, can provide an opportunity for a state to press demands, but the key is that a state chooses to use a random event in this way. Crises are purposive acts by states seeking to change the status quo.¹ Implicit in such demands is the possibility that the crisis might escalate to war if the initiator of the crisis does not receive sufficient concessions. War occurs when one side uses violence and the other resists violently; the bargaining over the issues in dispute has failed. War then requires choices by both sides to fight. Further, either side could avoid war by offering sufficient concessions to convince the other to settle for peaceful change in the status quo. Unlike some arguments where a state seeks war and would not accept any settlement (e.g., Lebow 1981, 23–40), this view asserts there always exists some settlement that a side would accept short of war. Further, the focus on the origin and escalation of crises matches the most-used data collections on international conflict, the Correlates of War and the International Conflict Behavior projects.

This approach does not address why states have conflicts of interest. It assumes that such conflicts exist and then examines when those conflicts of interest lead to crises and war. This assumption of conflict of interest is common across other current approaches to international conflict. Much earlier literatures tried to explain the basis of conflicts of interest in individuals (cf. Waltz 1959 for a critique of such arguments), but those approaches have generally been dropped.² Constructivists hold out the hope of explaining how conflicts of interest arise and can disappear, but their ar-

1. Huth (1996) studies when territorial conflicts burst into crises and when they are resolved by negotiations.

2. Mercer 1995 is a recent attempt to explain why groups have conflicts of interest drawing on psychology.

guments explain why actors can manage conflicts rather than why conflicts of interest disappear (see Wendt 1999 on cultures of anarchy). After all, conflicts of interest are common in domestic politics as well as international politics; violent conflict is not.

The central question in this work is why the parties fight when there are peaceful settlements that both would prefer to conflict. Conflict is costly in material terms for nations and personal terms for leaders (Buono de Mesquita and Siverson 1995). The issues in a dispute can generally be resolved through a number of possible settlements. The contending sides disagree about the desirability of these possible settlements. Imagine a conflict over a piece of territory. Each side in the dispute would like to control all the territory, and each views controlling more territory as better. There are many ways to divide the territory, and given that war is costly for both sides, there are probably many divisions of the territory that both would prefer to war. Why then do states go to war?

To avoid war, the contending states need to agree on a specific settlement. These negotiations create a dilemma for both sides. Each side would like to use the threat of conflict to convince the other to agree to a deal more favorable to its interests. If a state knew exactly the best deal it could extract from the other short of war, it would make that demand on the threat of going to war. The other would accept the deal rather than fight. However, the other side's minimal acceptable deal depends on the value it places on fighting a war; a value that combines how important the stakes are to it, its judgment about its chances on the battlefield, and how it assesses the likely costs of a war. All of these are known only to that state; they are, in the language of game theory, its private information. The state making the demand cannot know any of these three, although it certainly has some beliefs about each. It cannot be certain which demand will extract the best possible settlement for itself, and asking for too much leads to war. The dilemma rests between asking for too little and demanding too much.

The dilemma of uncertainty extends further to how the target responds to a demand. A state making a threat has the incentive to exaggerate its threats. The side receiving a demand must judge the credibility of the threat. Because the recipient tries to judge how willing the threatening state is to go to war, the latter may be able to benefit by exaggerating its resolve to fight. If successful, the target of its threat may grant more than is necessary to conciliate the threatener. However, the target understands the threatener's incentive to overstate its willingness to fight when making a demand. It should discount the signals of resolve that the threatener makes with its demand, reducing the credibility of the threat in its eyes and the chance that it will grant sufficient concessions to dissuade the threatening state from war. The dilemma produced by private information about resolve leads to excessive demands and overly aggressive actions and to the tendency to downplay the seriousness of the other side's demands and demonstrations of resolve. Both of these effects increase the chance that

the sides will fail to reach an agreement to settle their differences short of war.

Costly signals help to transmit a party's resolve to the other side. When one side is uncertain about the other side's resolve, the former may be convinced if the latter takes an action that only resolute types are willing to take. If the irresolute types are unwilling to take that action, then observing the action is sufficient for the observer to conclude that the acting state is resolute. The costs of sending the signal prevent the irresolute types from taking the same action as the resolute types.

Costly signals, however, cannot solve the problem of uncertainty in all cases. First, there cannot be perfect signals of resolve. Imagine that there was an action that always convinced the receiver that the sender was resolute and so the receiver should grant the demands of the sender to avoid war. Then every type of sender would like to send that signal, regardless of its resolve, because it would gain its ends peacefully. However, the receiver should not believe that the sender is resolute after receiving the perfect signal precisely because the sender would always want to send it regardless of its resolve. Perfect signals of resolve are contradictory by their very nature. Second, although war is costly, the costs of conflict cannot be the sole source of signaling costs. Otherwise, actions in a crisis would be costly only when they triggered war. But then why would any party ever choose to go to war? War would be costly, and there would still be the prospect of a peaceful settlement short of war. Some costs outside of just war must exist in order that at least one state prefers going to war instead of continuing to bargain. Fearon (1994a) solves this problem with audience costs that build throughout the crisis, with a domestic audience's imposing a cost on the party that backs down to avoid war. A side might then choose to fight to avoid having to pay audience costs even though it saw settlements it preferred to war.³

War could occur because there is no settlement that both sides prefer to war. Fearon (1995b) discusses two reasons why this could occur, commitment problems and indivisibility of issues. A commitment problem occurs when one side has an incentive to renege in the future on any agreement.⁴ The other side is unwilling to settle short of war because it knows the first side will break the deal in the future. Indivisibility of issues could prevent the existence of a division of the stakes that both sides would prefer to war.

I see these arguments as reasons why conflicts cannot be resolved rather than why they begin. We know that a peaceful status quo outcome

3. The audience cost argument clarifies earlier lines of argument about reputation by Snyder and Diesing (1977). Alastair Smith (1998) explains why domestic audiences would be willing to impose costs on their leader after the crisis.

4. R. Harrison Wagner has promoted the idea of commitment problems as a source of war; see Wagner 1994.

exists before a crisis. If either of these problems cause war in a situation, such a stable status quo cannot exist by definition. Otherwise, both sides could avoid war by agreeing on the status quo *ex ante*. These causes of conflict are important when a crisis arises from a shock which eliminates the possibility of returning to the status quo. For example, the dissolution of Yugoslavia into separate republics foreclosed the possibility of returning to the status quo. Majority groups could not commit to protecting ethnic minorities, leading minorities to see war as preferable to any peaceful settlement in the same state as the majority (Fearon 1994c).

Commitment and indivisibility of issues can explain why some violent conflicts are extremely difficult to end. Once the status quo is no longer possible, the sides may not be able to find a solution under which both are willing to live in peace. I see both of these problems as central to the explanation of protracted conflict, particularly civil wars (Walter 1997).

These arguments on the microfoundations of international conflict provide four central lessons for studying war. First, war is not predictable in the sense that the outcome of individual cases can be known in advance (Gartzke 1999). Private information compounded by incentives to misrepresent that information and to disregard signals about it is the primary reason for war. The elements of private information about resolve that drive an individual dispute to war cannot be determined by others prior to the onset of war; they are subjective judgments about the likely outcome on the battlefield and how important the stakes are to an actor. The best predictions we can hope for are probabilities of war and lesser conflict.

Second, we may be able to predict variation in the likelihood of war across sets of dyads or international systems. The probability of war in a crisis may vary systematically because the availability of costly signals varies across dyads. Those dyads with more costly signals or a richer menu of such signals may have a lower probability of war than other dyads (Morrow 1999a).

Third, the factors that make war more likely in a dyad also make a crisis less likely (Fearon 1994b). One of the costs of initiating a crisis is the chance that it will escalate to war. If the parties can anticipate the magnitude of that chance, then coercive diplomacy becomes less attractive as the risk of war rises.

Fourth, *ex post* measures of resolve, such as actions taken in a crisis, should predict the outcome of a crisis better than *ex ante* measures, such as the balance of capabilities before the crisis (Fearon 1994b). Actions taken in a crisis include the effects of private information as revealed by those actions during the crisis, while *precrisis* acts reflect only the observable factors and judgments about the other side's private information.

Although these microfoundations focus on the origin and escalation of crises, their principles can be applied to security policies before crises break out. A state's perception of its security depends on what motivations it believes that other states have. Discerning the motivations of other states

through foreign policy short of crises is the problem of uncertainty central to the microfoundations of conflict. Arming and seeking allies not only increase a state's capabilities; they also signal its willingness to use force to defend or change the status quo, depending on the state's goals.⁵ The difficulty of determining whether preparation for war is aggressive or defensive is central to the security dilemma, the fount of offense-defense theory.

The development of game theoretic microfoundations has primarily proceeded through formal modeling. The empirical tests of these arguments are large-*n* studies because the hypotheses concern variation across many cases; case studies are less suitable because unobservable factors drive the outcome of individual cases. Some examples of large-*n* tests are Bueno de Mesquita and Lalman 1992, Fearon 1994b, and Eyerman and Hart 1996.

■ | The Democratic Peace

The democratic peace is the observation that pairs of democracies are much less likely to go to war than dyads that include at least one other type of system (Rummel 1983; Doyle 1986b, Bremer 1992; Maoz and Russett 1993; Russett 1993). After extensive debate as to the truth of this empirical regularity, there appears to be consensus on this proposition. However, such consensus does not exist on the explanation of this regularity. This section examines first how the field arrived at a consensus that the regularity exists and then the debate on the significance and meaning of the democratic peace.⁶

The idea that democracy is conducive to peace goes back at least as far as Kant (1795). Showing that democratic dyads are more peaceful than other types of dyads requires clear definitions of war and democracy and the idea to look at democratic dyads. The proposition that democracies are more peaceful than other types of states does not appear to be true (however, see Ray 1995; Rousseau et al. 1996; Benoit 1996). Even if democracies are less likely to engage in war than other types of states, the effect of a democratic dyad is much greater than simply doubling the pacifying effect of democracy. The puzzle is why democratic dyads are unusually peaceful when democracies are not.

Before examining the theories proposed to explain this regularity, I discuss its establishment as stylized fact. Early efforts (Small and Singer 1976)

5. Glaser 1992, 1997, Kydd 1997a, and Morrow 1993, 1994a, 2000 are attempts to use arguments from game theoretic microfoundations to analyze security policy.

6. The literature on the democratic peace is huge, and there is no way I can do justice to all the contributions for and against that proposition in my brief review here. Ray 1998 is an excellent review essay on the topic and can lead the reader through the literature in detail.

were inconclusive largely because the lack of a clear definition of democracy made a full large-*n* study on dyads that went to war and dyads that did not extremely difficult. The first general effort (Rummel 1983) covered the period 1976 to 1980 because of its coding of democracy was limited to that period, opening up that study to the criticism that it could not be extended beyond that limited sample. Doyle's work (1986b) relied on his own coding of liberal republics (following Kant) rather than democracy. The precise definition of democracy is critical because there are a number of borderline cases, such as the War of 1812 and the Spanish-American War (cf. Ray 1995). The strength of the relationship varies with how these cases are coded. Not surprisingly, those who wish to discredit the democratic peace tend to include those cases as wars between democracies, while the proponents exclude them.

These coding arguments (including another about exactly which states went to war with each other in general wars) also played the role of focusing the discussion on which characteristics of democracy lead to the peaceful nature of pairs of democracies. This evidence could be quite valuable in distinguishing among possible explanations of the regularity.

There were questions whether the relationship was a statistical artifact. Spiro (1994) pointed out that individual years of a particular dyad were not statistically independent of one another and examined the data year by year in an attempt to show the relationship existed only because the proponents compounded essentially the same observations over and over. However, this test pushed the interdependence argument too far; alternative tests (Maoz 1998; Beck and Tucker 1996) showed that the relationship still held even when interdependence of dyadic observations over time was considered in the analysis.

Green et al. (2001) argued that the democratic peace is spurious because the statistical analysis lumps together many dyads that have other unobserved characteristics than joint democracy. They show that the statistical effect of a democratic dyad on the initiation of disputes becomes statistically insignificant when each dyad is allowed to have its own baseline rate of conflict.⁷ However, the democratic peace contends that democratic dyads as a group are essentially different from other dyads; Green et al. fail to test whether the set of base rates of conflict for democratic dyads are different from other dyads. They argue that you should not assume that democratic dyads operate the same as other dyads; yet this is precisely the empirical claim of the democratic peace. Democratic dyads are different.

Explaining the regularity required more than establishing that it existed. Early in this literature, two broad explanations—the norms argument and the constraint argument—were proposed. The norms argument contended that leaders in democracies were used to dealing with their oppo-

7. For the statistically knowledgeable, Green et al. (2001) run a fixed effects model which includes a constant for every dyad over time.

nents in ways that recognized the legitimacy of their positions and demands through cooperative solutions, such as compromise and open electoral campaigning. When democracies faced one another, their leaders extended their common norms of limited, rule-bound competition to find peaceful solutions to their international disputes, such as through the use of conflict resolution techniques (Dixon 1994). On the other hand, democracies understand that nondemocratic opponents will not abide by such norms, and so democracies must rely on the tactics of power politics in their disputes with nondemocracies (Maoz and Russett 1993).

The constraints explanation begins with the observation that democratic leaders must gain the ascent of others throughout the government in order to go to war. Between democracies, the dual effect of these constraints is such that war is unlikely. Between a democracy and an autocracy, the autocracy may attempt to use the constrained nature of the democracy to take advantage of it in a crisis, making both sides more belligerent and war more likely (Morgan and Campbell 1991). Further, the public character of a democracy makes it easier for others to perceive how the constraints will affect its behavior in a crisis (Bueno de Mesquita and Lalman 1992).

These initial arguments suffer from several problems as explanations of the democratic peace. First, the two arguments are almost two sides of the same coin rather than competing explanations. Leaders in the norms argument learn the ways of limited competition because they operate within a system that places constraints on what they can do. Those constraints operate only because the politicians within a democratic system agree to abide by the constitutional rules of the system. Systems do differ in the constraints they place on their leaders and in the extent to which they rely on normative rather than formal constraints on power (think of the differences in the democracies of Great Britain and the United States). Some have tried to use these differences to support one or the other argument (notably Maoz and Russett 1993), but essentially, these two arguments may not be separable.

Second, neither argument develops its microfoundations in detail (the exception here is Bueno de Mesquita and Lalman 1992). The norms argument asserts that crisis bargaining between democracies will be different than “normal” power politics. However, that assertion hardly moves beyond the original observation that democratic dyads are much less likely to go to war than other dyads. Further, the norms argument does not seem to account for the “autocratic peace”—the observation that autocratic dyads are less likely to go to war than autocratic-democratic dyads (Beck and Tucker 1998).⁸ If democracies do not fight one another but are as likely to go to war generally as other types of states, then the chance of war in a mixed dyad must be higher than in an autocratic dyad.

8. Honor among tyrants, perhaps?

Case studies might be able to unravel this problem, except that neither the norms argument nor the constraints argument is well-enough developed to support the detailed predictions that could be tested with case studies. Maoz (1998, 10–13) is correct to take Layne's (1994) case studies to task as a contradiction of the normative argument. Layne's failure to find that democracy did not appear as a factor in the decisions of states in crises is irrelevant. The norms argument contends only that the pattern of negotiations and calculations is different in democratic dyads than others. Owen's case studies (1994) supporting the democratic peace as a liberal peace approach a tautology; he defines liberals as those who do not want to fight other states that are liberal, and then if the liberals make enough noise, the liberal democracy does not go to war. It is hard to see how this argument advances hypotheses beyond those in the original empirical regularity. Again, the norms and constraint arguments do not provide the range of testable hypotheses that a case study could profitably test.

One explanation of the democratic peace is that there is nothing to explain. Farber and Gowa (1995; Gowa 1999) argue that the democratic peace is limited to the post-World War II period and that shared interests among democracies during that period accounts for their lack of conflict. In short, there is a regularity that needs no novel explanation. They present evidence that when the period from 1815 to 1980 is broken into the pre-World War I, interwar, and postwar eras, the rate of militarized disputes and wars between democracies is statistically significantly lower than the rate in other dyads only during the postwar period. They then argue that the common interests induced by bipolarity account for the relative peace among the democracies in the cold war era.

Their argument is less than convincing. First, the lack of statistical significance requires the separation of the pre-1914 and interwar periods. When these two are combined, the escalation rate of disputes to war in democratic dyads—the central concern of the democratic peace—is significantly less than for other dyads (also see Maoz 1998, 31–38; Thompson and Tucker 1997). Second, the microfoundations that I described earlier in this paper suggest that less chance of escalation may lead to a higher rate of disputes because the risk of war is less. The rate of militarized disputes in democratic dyads may be irrelevant to the democratic peace. Third, the interest argument poses a chicken-or-egg problem; do states have shared interests because of their common regimes, or common regimes because of their shared interests? For example, would Italy have remained in the Western bloc if it had gone Communist in the late 1940s? Fourth, if shared interests produced by bipolarity sustained the peace in the democratic world, why did they fail to do so in the Communist world where the Soviet Union invaded its ally Hungary in 1956, fought with China in the late 1960s, and intervened militarily in its other allies repeatedly (Ray 1998, 38)?

Fifth and finally, Farber and Gowa (1995) never provide an indicator of common interests that would allow us to see if democracy has an independent effect after controlling for interests. The advocates of the democratic peace did the hard work of arriving at a consensus of how to measure democracy for empirical tests; the proponents of interests now need to develop indicators of interests and show empirical results that those indicators are consistent with their arguments about how interests operate. On this point, Green et al. (2001) argue that their baseline rates of conflict in a dyad reflect shared interests, but that argument assumes that state interests change less often than government type. They present no evidence for their claim.

Three approaches have attempted to develop explanations of the democratic peace and test them against evidence. The first examines possible causes of peace related to democracy that supplement the democratic peace. O Neal and Russett (1997) contend that the democratic peace should be thought of as the liberal peace, where trade plays a large role in creating peace among nations. They examine the cold war years and find that joint democracy and higher trade flows are related to a lower frequency of disputes.

This approach is promising, yet it suffers from several important limitations. First, as noted above for the argument that interests cause the democratic peace, microfoundations suggest that lowering the risk of war should raise the chance of a dispute. Evidence that the rate of disputes is lower in democratic dyads may or may not be consistent with a democratic peace. Second, the microfoundations of the argument that higher trade flows help to produce peace are unclear (Morrow 1999a). The common argument that states with healthy trading relations will avoid war with each other out of the fear of losing the trade misses the point that what makes one state less willing to fight, and so more willing to make concessions, should raise the demands of the other side. I am not claiming that trade has no effect on how conflict prone a dyad is, only that the theory behind that argument is incomplete at this time. Third, it may be that we observe that dyads with large trade flows are also peaceful because the anticipation of peaceful relations encourages trade (Pollins 1989; Reuveny and Kang 1996). Further research could answer these questions and advance the standing of this explanation for the democratic peace.

The second approach turns to the microfoundations of conflict to develop a theory of why democratic dyads should be more peaceful and then tests the implications. Building on a suggestion in Fearon's discussion (1994a) of audience costs; Schultz (1998, 1999, 2001) examines how the role of a lawful opposition increases the ability of a democracy to signal its resolve during a crisis. The opposition presumably has a better understanding of the government's resolve than the opposing state does. It also has the incentive to exploit the crisis for its own political gain should the

crisis end in an undesirable outcome—war or major concessions—for the state. Then the actions of the opposition serve as a signal for the resolve of the government. When the government is highly resolved, the opposition will support the government because there is little chance of political gain and a great chance of partisan loss from opposing the government's stance. When the government is weakly resolved but pursues a strong stand in the crisis anyway, the opposition should oppose the government's actions in the crisis in the hope of gaining politically. The prospect of this opposition disciplines the government, leading it to settle those crises quickly. In a democratic dyad, this improved signaling by both parties makes a peaceful settlement of a dispute much more likely than other types of dyads.

Schultz (1999, 2001) tests this argument using both large-*n* tests and case studies. The theory provides detailed predictions both of what statistical patterns we should expect in the data and what actions and responses we should expect in the case studies. The theory provides the hypotheses that structure both types of empirical work, and the combination of both reinforces our confidence that the theory is supported by the evidence.

A third approach to explaining the democratic peace looks at broader patterns of behavior by democracies in conflicts (Bueno de Mesquita et al. 1999). Democracies are more likely to win the war they fight than autocracies are (Lake 1992); in wars between democracies and autocracies, the democracies initiate the war more often than the autocracies (Bennett and Stam 1998); and democracies that initiate wars suffer lower costs than autocracies that initiate wars (Siverson 1995). Bueno de Mesquita et al. (1999) observe that democratic leaders are less secure in office than autocratic leaders and that losing wars or fighting costly wars increases the risk of replacement to leaders (Bueno de Mesquita and Siverson 1995). They propose a model of removal from office tied to crisis and possible war to examine how decisions in a crisis vary with the institutions that select and remove leaders from office. Democratic leaders select when they go to war more carefully than autocratic leaders, requiring a greater chance of winning before they are willing to fight. Further, democratic leaders commit more resources to the war effort than other leaders (Reiter and Stam 1998). That model explains all these regularities and proposes additional regularities supported by the evidence, such as the willingness of democratic leaders to engage in low-cost conflict. Here the theory ties together a set of observed regularities into a consistent whole and proposes novel regularities.

These three candidate explanations of the democratic peace are not mutually exclusive. All three could be operating as part of the complete explanation. Certainly the signaling properties of an opposition and the selection and effort effects of more tenuous leader tenure seem closely related and compatible within a more general theory. Both rely on the same microfoundations. Searching for more hypotheses that can be tested

against both large-*n* data sets and case studies should be the next direction in efforts to explain the democratic peace.

■ | Offense-Defense Theory

Offense-defense theory derives from the idea that war and conflict are more likely when it is relatively easy to conquer territory (Jervis 1978). When the offense is advantaged, states will be more aggressive in their designs because the prospect of conquest is easier and they will be more militaristic in their security strategies because the threat of being conquered is greater. The broadest statement can be found in Van Evera (1999) where the offense-defense balance is claimed to make just about every dimension of security affairs, from alliances to arms races to diplomacy, more belligerent and difficult to resolve short of war.

Technology and military organization are the two main sources of the offense-defense balance. Although there is some disagreement about exactly how weaponry and organization combine to create advantages for the offense or defense, mobility is thought to favor the offense, while firepower favors the defense. Sometimes, geography and the force-to-space ratio is included in the offense-defense balance. These additions move the concept from being a characteristic of the international system to one of dyads. A state's probability of victory in a war depends on many factors in addition to the offense-defense balance. The balance of capabilities and support from third parties, at a minimum, also affect which side wins a war (Glaser and Kaufmann 1998; Lynn-Jones 1995).

The idea that ease of conquest leads to war is attractive in the abstract. It moves beyond structural realism to produce a theory where the security dilemma is a variable to be explained, rather than a constant. The argument can be extended to explain the tightness of alliances (Christensen and Snyder 1990), the likelihood of competition versus cooperation in international politics (Glaser 1992, 1994–95), and nuclear strategy (Glaser 1990). Formal models can reflect the offense-defense balance and show that increasing offensive advantages produce higher levels of armaments (Powell 1993).⁹ Offense-defense theory can lead to novel hypotheses and added theoretical sophistication beyond structural realism.

Specifying the offense-defense balance as a concept has proven difficult. Levy (1984) pointed out that the concept was used to mean at least four different things. Efforts centered on refining the concept. The most careful effort to date is Glaser and Kaufmann (1998) who begin by assessing the margin of force each side needs to win a war in the dyad as an at-

9. However, also see O'Neill 1990 where increasing the advantage to the offense either decreases the chance of war or has no effect on it.

tacker and as a defender, holding the other side's forces fixed. The product of these margins defines the offense-defense balance that a state faces against another particular state; is it easier for the state in question to build offense or defense to defeat the other in a war?¹⁰

The measurement of the offense-defense balance has proven problematic. At times, some proponents of the theory appear to judge perceptions of the balance solely by whether an actor thought the attacker would win a war (Christensen 1997, 74, 78–80, 91–92), even though the theory clearly recognizes that more than the balance determines which side wins a war. Glaser and Kaufmann's proposed measurement (1998) of the balance depends on net assessments. Net assessments implicitly assume that analysts can judge which side will win a war from a given military balance. However, if the outcomes of war were predictable, then the sides would not have to fight; the loser would surrender the stakes to avoid the costs of war (Wagner 2000). Instead, war is inherently unpredictable, and different analysts may not arrive at net assessments, and hence judgments of the balance, that are even close to one another.¹¹

Another problem for measurement of the balance is that states can influence it through the militaries they choose to construct. Van Evera (1999) says the offense was advantaged during the period from 1792 to 1815 and the defense was advantaged from 1815 on.¹² Neither technology nor the possibilities for military organization changed when the Napoleonic Wars ended in 1815. All the ways Napoleon and revolutionary France before him created mass armies capable of offensive campaigns were as available after Waterloo as they were before. However, the monarchs of Europe chose to build small, professional armies instead of the large, citizen armies they built from 1813 to 1815 to defeat Napoleon. This difference resulted from an implicit agreement among monarchs that the suppression of liberal revolution was more important than building a powerful army for interstate warfare. As Best (1986, 204–6) argues, militaries during the Restoration were professional because monarchs thought their

10. This directional balance will differ for the states in a dyad whenever geography or technology creates an advantage for one side (Glaser and Kaufmann 1998, 58).

11. The use of net assessments may arise from the extensive study of nuclear strategy in strategic studies. Net assessment is much easier to perform for a strategic nuclear war, and it is more likely to achieve some consensus on an assessment. The factors that produce risk are much less likely to have any effect on the outcome of a nuclear war. Indeed, it may be the certainty of judging that strategic nuclear war will be a disaster that provides its deterrent power.

12. Van Evera (1999) takes the endogeneity of the offense-defense balance very far, claiming that the balance is a cause of itself as states adopt more offensive means as the offensive becomes advantaged. Glaser and Kaufmann (1998) reject this full endogeneity, I think correctly for the purpose of their theory, because, otherwise, something else determines that balance, and we cannot think of the offense-defense balance as a primitive concept.

most likely targets were their own populations. Is the offense-defense balance a fundamental concept if it can be changed so dramatically through national policy (Shimshoni 1990–91)?

The two World Wars are central to the development of the theory. World War I is commonly thought of as a war where the defense was dominant because of the roles of the machine gun and artillery. World War II is seen as an era where the offense was dominant as the introduction of the tank and ground support aircraft led to mobile warfare, making it possible to conquer large amounts of territory quickly. However, most leaders before each of those wars perceived the balance in the opposite way; they thought that attacking armies would sweep to victory in 1914 and that the war in 1939 would lead to another stalemate in the trenches (J. L. Snyder 1984). The difference between the actual and perceived balances could pose major problems for testing the theory. Proponents may be able to shift whether the actual or perceived balance is critical across cases to eliminate cases that contradict the theory.

The paradigmatic cases of the World Wars pose a puzzle for the theory. Offensive dominance is claimed to make war more likely, yet the perceived dominance of defense helped to lead to World War II. Offense-defense theorists argue that perceived dominance of the offense before World War I led to tight alliances and overly aggressive war plans that caused the Austro-Serbian crisis after the assassination of the archduke to escalate into a general war (J. L. Snyder 1984; Van Evera 1984). In contrast, the British and French perception that the defense was stronger in the 1930s discouraged them from taking the steps that might have deterred Hitler from attacking Poland. Indeed, Hitler drew back from the brink on August 26, 1939, when he first realized the possibility that Britain and France would go to war if he attacked Poland. Here the dominance of defense helped to trigger the German-Polish war that spread to a general war in Europe. Hitler's belief that Britain and France would not go to war for Poland increased his willingness to attack Poland. Indeed, his belief was correct; Britain and France did nothing to aid Poland against the German onslaught (Christensen and Snyder 1990; Van Evera 1999, 151–56). Together, the two World Wars suggest that either offense dominance or defense dominance can play a key role in the outbreak of general war.

Offense-defense theorists respond to this observation in two ways, both about why perceived defense dominance should not be thought of as a cause of World War II. First, Hitler believed that the offense was advantaged, and so was willing to go to war even if Britain and France were likely to go to war over Poland. Second, Hitler's quest for domination in Europe was the central cause of World War II independent of the offense-defense balance. Even if France and Britain had not gone to war after the German invasion of Poland, Germany would have gone to war with them later in any case.

In response, one could argue counterfactually that World War I would

have occurred even if state leaders held the belief that the defense was advantaged (cf. Trachtenberg 1991, 63, 71). Austria would have gone to war with Serbia in 1914, anticipating that a major power can defeat a minor power regardless of the offense-defense balance. Russia would then face the same decision between war and watching Serbia be crushed by Austria that it faced in 1914. Had Russia intervened, the Great Powers would then have faced the same chain of intervention decisions as in the actual July crisis. Would Germany allow Austria to fight Russia alone? If it intervened, would France stay on the sidelines in the war between Russia and Serbia on one side and Austria and Germany on the other? If France did remain neutral at the outbreak of war, could the Germans trust the French to stay on the sidelines? The one decision for war that we could all agree would not have happened immediately would be the British intervention into the war. The German invasion of Belgium was key in rallying the cabinet in favor of intervention, and Belgium would not have been invaded in a defensive German war plan (Ferguson 1999, 158–66).

It is difficult to see how such lines of historical and counterfactual argument can resolve the status of offense-defense theory. Trachtenberg (1991) provides detailed historical evidence that preemption played little role in the 1914 crisis contrary to the received story from offense-defense theory. The counterfactual arguments depend closely on the theory held; if one believes that offense-defense theory is true, then the judgment that World War I would not have occurred if the defense was broadly believed to be advantaged is straightforward. However, other theories lead to different counterfactual conclusions. We need to look elsewhere to test the validity of offense-defense theory.

The examination of additional cases to test the theory requires an assessment of the balance. Measures that are easier to collect than the net assessment procedure that Glaser and Kaufmann propose would assist in examining additional cases. Morrow (1993) used war plans to assess the perceived offense-defense balance as offense dominant during the 1860s. War plans should reflect leaders' perceptions of whether the offense is advantaged. There are, of course, some difficulties in using war plans as a general indicator of the perceived balance: (1) detailed war plans become necessary only with the onset of mass mobilization in the mid-nineteenth century, (2) war plans are typically offensive in nature, perhaps to gain first-strike advantages, and (3) war plans reflect strategic considerations beyond the offense-defense balance, such as power asymmetries, state goals, and alliances (Sagan 1986).

Such indicators of the offense-defense balance would be useful to conduct large-*n* tests of the hypotheses of the theory. The theory purports to be general across time and space, and it would be useful to know whether the set of cases to which the theory applies is limited. Such limits could help us identify underlying but unidentified conditions that are necessary for the theory. However, most empirical examinations of the theory are limited

to the two World Wars or else make judgments about the offense-defense balance that are not reproducible (Quester 1977; Van Evera 1999).¹³

An example may clarify the advantages of testing the theory on cases beyond the usual set. Christensen and Snyder (1990) extend the logic of offense-defense theory to account for the tightness of alliances under multipolarity. Their argument is that perceived offense dominance leads to tight alliances and the rapid spread of small conflicts into general war, while perceived advantages for the defense lead to loose alliances that undermine deterrence of powers seeking to dominate the system. World War I corresponds to the first case, and World War II to the second. Morrow (1993) points out that the 1860s contradict this argument. Offense was believed to be advantaged by national leaders, yet France and Austria failed to coordinate their policies to stop the unification of Germany under Prussia. Christensen (1997) responded by modifying the theory so that it only holds when all sides perceive a general threat to the status quo; this he claims did not exist during the 1860s. This modification of the theory seems reasonable and consistent with its arguments. However, the three cases now examined require support either for novel predictions in the details of the case or in cases not yet used in the construction of the revised theory. Without such novel tests of the revised theory, the theory has no empirical support independent of the cases used in its construction.

A careful examination of the logic of inference may clarify the issue here. Table 1a depicts how the two cases considered by Christensen and Snyder (1990) fit their argument. The argument claims that alliances will be overly tight when the offense is advantaged and fail to be tight enough to deter a state seeking hegemony when the defense is advantaged. Each of the World Wars is placed in the relevant cell of Table 1a, and the evidence conforms to their hypothesis; all cases should fall on the main diagonal of the table. Table 1b presents Morrow's addition (1993) of the 1860s. It is a discordant case according to the original hypothesis. Table 1c presents the revised theory, which constructs a more elaborate hypothesis by differentiating those cases, such as the 1860s, where states did not believe that any state sought hegemony. Again, the hypothesized pattern of cases on the diagonal returns in the upper portion of Table 1c as the revised theory predicts.

The lack of additional cases undermines our confidence in the generality of the hypothesis. The period 1688 to 1714 when a large coalition formed to stop the expansion of France under Louis XIV would be a useful case to consider. Defense was advantaged during this period; it was an era of siege warfare (Van Evera 1999, 171). There was no question that France posed a threat to the system in the eyes of other states. According to the theory then, we should find loose alliances that failed to deter Louis XIV. Two general wars did occur during this period; however, according to one general history,

13. A notable exception is Hopf 1991.

TABLE 1A. CASES TESTING CHRISTENSEN AND SNYDER'S ARGUMENT: FIRST CUT

PERCEIVED ADVANTAGE	ALLIANCE BEHAVIOR	
	CHAIN GANGING	BUCK-PASSING
OFFENSE ADVANTAGE	World War I	
DEFENSE ADVANTAGE		World War II

TABLE 1B. CASES TESTING CHRISTENSEN AND SNYDER'S ARGUMENT: ADDING THE 1860s

PERCEIVED ADVANTAGE	ALLIANCE BEHAVIOR	
	CHAIN GANGING	BUCK-PASSING
OFFENSE ADVANTAGE	World War I	The 1860s
DEFENSE ADVANTAGE		World War II

She [France] faced a fairly solid coalition of states who had all been bullied by Louis over the last two decades and who were to manage to stick together through this war and that of the Spanish Succession until the ascendancy of France was destroyed. (McKay and Scott 1983, 45)

Closer examination of these wars might show that the coalition was not strong or that the perceived advantage was with the offense. It could be that the Nine Years' War from 1688 to 1697 fits the theory because Louis XIV did not realize that the coalition would form to oppose him, although France anticipated that the War of Spanish Succession would be a general war.

Examination of such additional cases would assist the development of the theory in two ways. One, proponents would be forced to provide clearer operational definitions of the independent and dependent variables, which would enhance the commonality of understanding of these concepts. Although I have focused on the measurement of the offense-defense balance, there are also questions about whether the theory seeks to explain all wars or only the largest, most significant wars (cf. Van Evera 1999, 1 for the latter claim). Two, the theory would be tested on novel evi-

TABLE 1C. CASES TESTING CHRISTENSEN AND SNYDER'S ARGUMENT: CHRISTENSEN'S REVISION

When States Perceive a State Seeking Hegemony

ALLIANCE BEHAVIOR

PERCEIVED ADVANTAGE	CHAIN GANGING	BUCK-PASSING
OFFENSE ADVANTAGE	World War I	
DEFENSE ADVANTAGE		World War II

When States Do Not Perceive a State Seeking Hegemony

ALLIANCE BEHAVIOR

PERCEIVED ADVANTAGE	CHAIN GANGING	BUCK-PASSING
OFFENSE ADVANTAGE		The 1860s
DEFENSE ADVANTAGE		

dence, cases not used in the development of the theory to date. Should the theory be revised again in response to these cases, the proponents would need to seek out unexamined cases or novel hypotheses to demonstrate that the newly revised theory explained something beyond the evidence used in its construction.

Fearon (1995a) provides a piece of general evidence that is a puzzle for offense-defense theory. The period from 1648 to 1789 is generally considered to be dominated by defense, while offense was favored from 1815 to 1913. The theory concludes that war should be more common in the latter period than the former. The opposite is true, however. War and great power war are much more common from 1648 to 1789 than from 1815 to 1913 according to Levy's data (1983). A revised version of the theory might be able to account for this pattern, but a comparison of a large number of cases gives rise to this puzzle.

A revision of the theory could begin by looking at some unstated premises of the theory. Fearon (1995a) points out that offensive advantages increase the variance of war outcomes—a higher chance of all-out victory or crushing defeat. If state leaders are risk-averse, then increasing the variance of outcomes makes war less attractive to them, holding the mean outcome constant. Implicitly then, offense-defense theory focuses on the

unusual leaders who are not risk-averse and so do not lower their value for war as the variance of outcomes rises. This assumption may be reasonable given the expressed desire to explain large wars if we believe that large wars are triggered by unusual risk takers like Hitler.

If this argument is correct, offense-defense theory would not attempt to explain the large number of wars that are fought for limited ends. In many wars, neither side seeks to conquer the other. For instance, Ethiopia and Eritrea went to war over some border territory. The threat of total defeat led Eritrea to settle their dispute on grounds favorable to Ethiopia, but Ethiopia did not pursue its military advantage to conquer Eritrea.

Defensive advantages should make limited wars more likely. Such wars often occur in territorial disputes where one state seizes the territory in dispute to present the other side with a *fait accompli* (Huth 1996). Because such preemptive moves are not opposed by substantial force initially, the offense-defense balance is irrelevant for the success of the initial blow. On the other hand, defensive advantages increase the ability to hold the seized territory afterward; this forces the target to choose between accepting the *fait accompli* and fighting a long and bloody war as Ethiopia and Eritrea did. *Fait accompli* is an attractive tactic under such conditions.

Such limited wars may trigger general wars or turn into long, bloody conflicts on their own. The rapid expansion of World War I overshadows its origin as a war between Austria and Serbia; World War II began with the German-Polish War. The Iraq-Iran War was a war over limited territorial ends that proved difficult to end even after massive loss of life. General wars are not generally sought by either side; wars begun with limited intentions can trigger them.

Work connecting game theoretic microfoundations and offense-defense theory is in its infancy. Glaser (1997, 178–81, 183–85; also see Kydd 1997a) argues that offensive advantages complicate signaling of intentions, although he does not show how such advantages lead all types to adopt military policies that send the same signal. The ability to distinguish offensive and defensive capabilities, rather than just offense dominance, is central to his argument about spirals of mistrust. States could infer the motivation behind a rival's military buildup if offensive and defensive weapons are different. It could be argued that offensive advantages reduce the range of bargains that both sides would accept short of war, and so increase the difficulty of reaching a settlement short of war. Offensive advantages also could create a commitment problem where the greater attractiveness of attacking leads neither side to trust the other to honor a deal in the future. How the offense-defense balance affects the efficacy of costly signals should be explored further.

The expectations of state leaders about the likely course of fighting clearly affect their choices about whether and when to go to war. Offense-defense theory correctly emphasizes the role of such expectations in the

initiation of war. Further, proponents of that theory can present detailed cases where they claim the offense-defense balance operates as the theory claims it does. However, strategic responses to expectations about the likely course of a war may not be as straightforward as the theory assumes. Offensive advantages may make war less likely in some situations, while defensive advantages increase the risk of war in others. Reaching a consensus on the general pattern here requires the examination of a wider range of cases, through novel cases as well as large-*n* comparisons.

■ | Three Methods and the Three Literatures

Game theoretic microfoundations, the democratic peace, and the offense-defense balance provide a comparison of the state of methodology for the study of international conflict as each line has drawn heavily on one of formal models, large-*n* studies, and case studies. Microfoundations as a research program uses models to elaborate arguments about conflict. The democratic peace as a research program has been primarily empirical using large-*n* studies. Offense-defense theory has been driven by case studies. All would benefit from greater integration of all three methods. This section presents my views of how formal models, large-*n* studies, and case studies should be integrated and illustrates my argument with the three research programs.

Science develops a common understanding of how and why the phenomena in question occurs; methodology addresses how shared scientific understanding can be created. Research methods seek to render research results open for skeptical questioning with the object of reaching consensus as to whether observations accord with theory. Methodology is not about how one arrives at insights about politics; it is about how one demonstrates scientifically to others that one's insights are true. This is not to say that reaching a shared agreement is easy, only that methods try to make such agreement possible.

Formal models, large-*n* studies, and case studies each have a distinctive contribution to the creation of a shared scientific understanding of international conflict. Formal models provide a way to open up the logic of a theory to close examination. Elaboration of a theory is critical to determine what conclusions follow from the theory, and so what hypotheses test the theory. Both large-*n* and case studies can be used to test the hypotheses of a theory, although each has particular advantages. A common view is that large-*n* studies deal more effectively with questions of external validity—whether we can generalize the results from the cases studied to others—while case studies are stronger on internal validity—whether the causal process is operating in the cases studied (Campbell and Stanley 1966). I

agree with this view but wish to expand on the complementary role of large-*n* and case studies and the proper form of each.¹⁴

Large-*n* studies provide us with confidence that a general pattern predicted by a theory appears across a number of cases. Such patterns can be novel facts in the sense of being unknown before their documentation. The democratic peace exemplifies a novel pattern found through statistical analysis. The range of patterns that we can examine, however, is limited to variables that can be quantified across a large number of cases. Small and Singer's (1976) inability to find the democratic peace rests in part on the lack of a clear definition of democracy when they did their research. Although regimes involved in each war could be coded without difficulty, the regimes of the large number of dyads that remained at peace in a given year could not be coded without great effort. Consequently, it was difficult to determine whether the lack of wars between democracies resulted only from the rarity of war and democracy historically. Measuring variables typically involves a compromise between the accuracy of a measure and its ease of collection across a large number of cases. Often we must settle for indicators which we believe are correlated with a variable even though they include substantial measurement error. For example, war plans could be used as a measure of the offense-defense balance because they depend in part on that balance as perceived by the governments making those plans. Still, many factors other than the offense-defense balance enter into a state's war plans, and war plans include error in their measurement of that balance.

Case studies, in contrast, allow the collection of more precise information about variables in hypotheses. By focusing on a single case, a researcher has the ability to collect and assess information in much greater depth than is possible across a large number of cases. The focus on a single case also allows us to test hypotheses that could not be tested in a large-*n* study because of the lack of information on many other cases. The range of hypotheses drawn from one theory that can be tested by detailed study of a case is the source of the belief that case studies assess the causal process better than large-*n* studies. Still, case studies present issues in the measurement of variables. Case studies are often presented as narrative rather than explicit tests of hypotheses; this presentation can obscure what values the variables take on the case according to the researcher. This obscurity hinders the ability of readers to judge those values against the case material and determine whether the case supports or contradicts the hypotheses. Judgments of the perceived offense-defense balance from statements about which side was likely to win the war is an example of this problem. Explicit judgments about variables also aids the effort to test the same hypotheses

14. Huth 1988 provides an excellent example of the complementary role of large-*n* and case studies.

on different cases. Researchers can transfer the principles behind the judgments to assess the variables on other cases.

All empirical work faces the question of whether the hypotheses follow from the theory being tested. In international relations (and political science more generally), our theories rarely make definite predictions about individual cases or classes of events. They may represent only part of the full causation of individual cases or may apply only to a limited range of cases, or the underlying concepts may be unobservable and so empirical assessments of them may be only partial and prone to error. Then any test of a single event is rarely decisive for or against any theory. The proponents of a theory contradicted by a single test or the opponents of a theory supported by such a test can appeal to other plausible causes to account for the observation in that test. Further, such critical tests are often difficult in our field because we share general knowledge of the historical record that we use for testing. Novel facts are hard to find in our field, although archival work or the search for statistical patterns may uncover them.

Research on game theoretic microfoundations may be able to assist with this issue. The unpredictability of individual events is a central conclusion of such work, and it forces the search elsewhere for testable hypotheses. With this point in mind, I turn to the implications of microfoundations for empirical work with either large- n or case studies.

First and foremost, it is critical to examine variation across and within cases, rather than to try to predict the outcome of individual cases. Private information plays a large role in determining the outcome of a crisis, and private information is not knowable to others in full by its very nature. Our theoretical expectations then cannot extend to the prediction of individual cases. We may be able to predict variation across sets of cases though. These theoretical expectations can confront evidence in a test. Some sets of dyads may be more likely to go to war than others.

The unpredictability of individual cases favors large- n analyses where probabilities of conflict can be estimated more reliably than from a small number of cases. These studies attempt to identify which sorts of cases are more likely to undergo a crisis and escalate to war. The statistics of such studies are complicated by the role of private information. Decisions to initiate and escalate disputes cannot be seen as independent (Reed 2000), and what we can determine about private information from actions in a crisis are limited by how we measure those actions (Smith 1999). Large- n studies also face the well-known problems of constructing indicators of concepts that can be collected across a large number of cases and of defining the relevant scope of cases that are believed to be comparable.

The unpredictability of individual cases does not rule out the use of case studies, but the focus of case studies should be shifted to examining variation within cases. If private information does drive behavior in crises, its values are fixed within an individual crisis, and its specific value should

produce a consistent pattern of behavior. The testable prediction is that certain sets of behavior should be found together in individual crises. Case studies allow us to delve more deeply into detailed patterns of behavior in a crisis.¹⁵ This focus on patterns of behavior differs from the typical focus on explaining the events of the case.

This search for patterns, however, is possible only if the theory being tested is capable of making predictions about detailed patterns of behavior in a crisis. Some theories, such as Waltz's version (1979) of neorealism, explicitly state that they cannot predict actions in a crisis. Others make such predictions based on implicit theories of how the judgments of states can be understood from the internal acts of a government, such as memoranda. Such implicit theories should be made explicit so they are open to scrutiny. Because case studies seek to exploit the detail of the case to increase both the effective observations available for a test and the range of hypotheses tested (Campbell 1975; King et al. 1994, 217–28), it is critical to have clear and explicit expectations about the hypotheses to be tested in advance of conducting the study. Otherwise, the theory can be adapted to fit the details of the case, rather than the details testing the theory.

Second, the indeterminacy of individual crises limits what we can expect from our theories of conflict (Gartzke 1999). Theories that purport to predict when wars will occur hold out the prospect of solving the problem of war if we can eliminate the conditions that such theories identify as the complete causes of a war. Unfortunately, such theories cannot exist given the microfoundations elaborated above. The occurrence of war is necessarily unpredictable both to the parties involved and to us as observers.¹⁶

Theories with probabilistic predictions can still be used for policy analysis. Beyond predicting the risk of war in a situation, the chances of war could be manipulated using the theory as a guide. However, the risk of war could not be eliminated, and reductions in the risk of war would be accompanied by undesirable countereffects. For instance, the advent of nuclear weapons may have reduced the chance of war while raising the consequences of war should it occur. The total expected risk, probability times consequences, might not have changed greatly, but its form had. Such changes in the form of the risk might be welcomed by all parties even if the total expected risk had not changed.

Third, case studies may allow us to delve into *ex post* information about the private information in cases more thoroughly than large-*n* studies can. This added detail contains both benefits and dangers. One danger is the belief that such studies can explain the onset of conflict because the fit of a theory to the data is better when *ex post* information is used. The

15. The Behavioral Correlates of War data set, collected by Russell Leng, could also be used for such studies.

16. Kuran (1991) provides a similar argument about the unpredictability of revolutions.

better fit may also lead some to believe that case studies necessarily provide better tests than large-*n* studies. Instead, each type of test has a different role that complements the other. Ex post information about both sides' resolve may allow further tests on the pattern of behavior to be expected in a particular conflict. Still, events in a crisis do not reveal the complete value of private information, and so we should not overestimate what we can learn about it from the events of a crisis (Smith [1999] presents statistical methods for dealing with this issue in large-*n* studies).

The two empirical research programs reviewed here reflect the micro-foundational point that war is predictable only in a probabilistic sense across dyads and seek to explain variation in the probability of war across dyads. The two research programs have taken different approaches to dealing with the issues raised in testing a probabilistic theory. Tests of the democratic peace have compared large numbers of cases to look for the variation in probability; tests of offense-defense theory have searched for many different behaviors that should be found together in particular cases according to the theory.

Both could benefit from use of the other empirical approach. The democratic peace is now moving toward several candidate explanations with microfoundations that could be examined with cases to support detailed predictions from those explanations. Offense-defense theory should be moving toward addressing a wider range of cases in the hope of pinning down the concepts empirically and theoretically as well as testing the theory beyond the cases from which it was drawn.

Of the two research programs, the democratic peace has done a better job of creating a consensus about its terms and the evidence for its claims. There are shared definitions of democracy and war that allow for comparisons of different research projects in the program. Offense-defense theory has built consensus among its proponents primarily about its application to and explanation of particular cases. Unfortunately, those understandings may not translate to other cases yet to be studied.

Offense-defense theory has paid greater attention to theory than the democratic peace. The early efforts to explain the democratic peace rarely advanced to the stage where the candidate theories made novel predictions beyond the existing empirical results. Offense-defense theory, on the other hand, has clear expectations for the cases, provided that the concepts of that theory could be assessed empirically.

The democratic peace has faced more challenges that its empirical regularities could have alternative explanations. The empirical cases of offense-defense theory have not faced that challenge often but should. Offense-defense theory predicts that aggressive behaviors—such as arms races, fait accompli tactics, tight alliances, and military secrecy—should be found together. But so do other explanations, such as those about the underlying motivations of states. If states can shift the offense-defense balance greatly by how they build their militaries, the argument that underlying

motivations cause both these aggressive behaviors and the offense-defense balance could be compelling. Again, moving away from the explanation of individual cases could help resolve this issue. Within an individual case, it is easy to find alternative explanations for particular actions; across a large number of cases, it is hard to find any one of those alternative explanations supported frequently.

Science is about the creation of a shared understanding of why events occur across the scientific community. Scientific understanding requires a logically sound theory that is supported by empirical tests of the observable consequences of theory. Such understanding takes a great deal of work, both theoretical and empirical, to create. I only hope the next decade's research into international conflict will be as fruitful as the last decade's.

*The Enduring Relevance of the Realist Tradition*¹

Writing in the 1983 edition of this volume, Robert Keohane declared that “‘Political Realism’ has constituted the principal tradition for the analysis of international relations in Europe and its offshoots in the New World.” Noting that attacks on realism were a frequent occurrence throughout its lengthy reign, Keohane also observed that “the very focus of these critiques seem only to reconfirm the centrality of Realist thinking in the international political thought of the West” (1983, 503).²

Realist theories are still widely criticized, but the realist tradition has yet to be supplanted by an alternative perspective with similar range or explanatory power. Although critics were quick to announce the obsolescence of realist theory in the wake of the cold war (Kegley 1993, 1995; Kratochwil 1993; Rosecrance and Stein 1993; Lebow 1994; Koslowski and Kratochwil 1994; Vasquez 1997), the real world has paid scant attention to these academic obituaries. Major powers remain acutely sensitive to the distribution of power, are wary of developments that might leave them vulnerable, and still strive to enhance their positions at the expense of potential rivals. Although states do cooperate in a variety of ways, they continue to guard their autonomy jealously and find extensive collaboration difficult to sustain. Military force remains a depressingly constant feature of political life, and events such as the 1991 Persian Gulf War, the Rwandan genocide, the Bosnian conflict, and the simmering rivalry between India and

1. I thank Stephen Brooks, Mlada Bukovansky, Dale Copeland, Michael Desch, Colin Elman, Miriam Elman, Markus Fischer, Charles Glaser, Keir Lieber, Sean Lynn-Jones, Michael Mastanduno, John Mearsheimer, Steven Miller, John Ruggie, Jack Snyder, Randall Schweller, Josh Spero, Allan Stam, Marc Trachtenberg, William Wohlforth, Christine Wohlforth, and the editors for valuable comments on earlier drafts of this essay.

2. This view was recently echoed by Michael Mastanduno: “Realism is now both the dominant paradigm in the study of international relations and the most challenged” (1999, 139).

Pakistan remind us that insecurity and the struggle for survival are still important elements of the human condition.³

Thus, the realist tradition remains the single most important approach for understanding international politics. It identifies and explains the central *problematique* in the field of international relations and sheds considerable light on a diverse array of important international phenomena.⁴ As a result, no serious scholar can safely disregard its arguments and implications. Even prominent critics of realist theory acknowledge its central place in the discipline, and many of its arguments are echoed by scholars who are not normally regarded as “realists.”

The realist tradition has a distinguished lineage, and it includes the works of Thucydides, Niccolo Machiavelli, Thomas Hobbes, Friedrich Meinecke, E. H. Carr, Hans Morgenthau, and others. In the field of international relations, Kenneth Waltz's *Theory of International Politics* (1979) has been the central point of departure for over two decades. Accordingly, this chapter examines the main developments in realist thought since *Theory of International Politics* was published. Realism remains a creative and relevant research enterprise, and scholars working in the realist tradition continue to make important advances. Their efforts in recent years have helped refine the basic logic of the theory, broadened its range of application, and explored its implications for the future. Debates within the realist tradition and between realism and its intellectual rivals have spurred theoretical progress throughout the field of international relations, and realist theories continue to provide important insights into the basic nature of international politics.

The remainder of this essay is organized as follows. The first section defines the realist tradition and proposes two criteria for judging its theoretical progress. The next section examines competing versions of realist theory, focusing on the recent debates between the neorealist, defensive, offensive, and “neo-classical” variants. The third section explores how different realist theories have illuminated a number of central issues in the field. The last section turns to the current research agenda: what questions remain unresolved, and what lines of inquiry are likely to yield new theoretical progress? The essay concludes with further reflections on the place of realist theories within international relations.

3. These conflicts include both internal and international wars, and the realist tradition has much to say about both subjects.

4. For example, realist theories offer cogent explanations for the rarity of regional hegemony, the sensitivity of states to shifts in the balance of power, the formation of spheres of influence, the barriers to extensive cooperation between states, and the tendency of states to imitate each other. Realist theory does not provide the only explanation(s) for these (and other) phenomena, but it does provide a set of arguments that one cannot easily dismiss.

■ | What is the Realist Tradition?

Scholars often refer to different theoretical traditions as “isms” (e.g., *Marxism*, *Keynesianism*, *behaviorism*, *liberalism*, etc.) and then use these labels to identify (or pigeonhole) individual scholars. Although such labels provide a convenient shorthand, this tendency has unfortunate consequences and ought to be resisted. Although social scientists often have strong personal attachments to particular worldviews, a social science theory is not a religious movement like Catholicism, Protestantism, or Buddhism. Rather, a theory is simply an intellectual tool that we use to help make sense of the world around us. Scholars do not sign a loyalty oath when they work within a particular theoretical tradition, and no methodological laws are broken when a scholar draws on more than one theoretical tradition when seeking to explain some particular phenomena.⁵ Although such paradigmatic distinctions can be useful, treating them as orthodoxies that require individual loyalty and using them to label individual scholars fosters unnecessary discord between rival traditions and does little to enhance our collective understanding of world politics. In general, our attention should focus on the theories themselves, rather than on the individual scholars who create or employ them.

In this essay I have tried (albeit with only partial success) to avoid the labels of “realism” and “realists” and relied instead on the term *realist tradition*. The theories emerging from the realist tradition provide a set of tools that can be employed by anyone who finds them useful, but scholars who use realist theories may also employ other theories when working on problems where realist theory is incomplete or inappropriate.

There is no single realist theory, of course, just as there is no single theory of social networks, evolution, or neoclassical economics. Rather, the realist tradition encompasses a family of related arguments sharing certain common assumptions and premises.⁶ Realist theories seek to explain politics as it *really* is, as opposed to normative theories that offer prescriptions for how politics *ought* to be. The realist tradition also tends to emphasize the continuity of historical experience and is skeptical of efforts to transcend the competitive nature of political life.

Applied to international politics, theories in the realist family share the view that the international system is anarchic (meaning that there is no central authority that can govern world affairs or protect different states from each other). Realist theories have focused primarily on states, be-

5. We can also distinguish between efforts to refine or develop a particular theory (i.e., by clarifying its logic or extending its explanatory range) and efforts to use one or more theories to explain a specific historical process or recurring phenomena.

6. For recent efforts to define the realist paradigm, see Frankel 1996; Keohane 1983; Walt 1992, 1997a, 1998; James 1995; Gilpin 1986, 1996; Grieco 1990, 1997; Brooks 1997; Van Evera 1999; and Donnelly 2000. Alternative conceptions include Kegley 1995 and Legro and Moravcsik 1999.

cause states have been the most prominent social groups in recent world history, but realist theories have also been applied to a variety of nonstate actors as well. Realist theories generally assume that states seek to survive and that they pursue their ends in a more or less rational manner, although different theorists vary in their strength of their commitment to this premise.

The central conclusion of all realist theories—what might be termed the realist *problematique*—is that *the existence of several states in anarchy renders the security of each one problematic and encourages them to compete with each other for power or security*. Realist theories see the insecurity of states (or groups) as *the* central problem in international relations, and they portray international politics as a self-help system where states must provide security for themselves because no one else will.⁷ Thus, the realist tradition places power at the center of political life: it sees the acquisition and management of power as the main issue that political actors face. It also takes a fairly pessimistic view of the human condition, emphasizing the recurring elements of tragedy rather than the hard-won instances of progress.

This conception of international politics is widely accepted, even among scholars who do not usually think of themselves as realists. For example, much of the recent formal work in international relations assumes that states are independent actors in anarchy, generally seeking to maximize their chances for survival subject to various exogenous constraints (see Powell 1990, 1999; Lake and Powell 1999b; Fearon 1994a, 1995b; Kydd 1997a, b). The extensive literature on power transitions also shares many assumptions with the realist tradition, although scholars working in this subfield disagree on whether their theories are realist (DiCicco and Levy 1999). Similarly, the rationalist approach to international institutions (sometimes termed neoliberal institutionalism) adopts most of the core premises of realist theory and has yet to escape its intellectual orbit (see Keohane 1984, 14, 67; Mearsheimer 1994–95, 430–32; Keohane and Martin 2002; Waltz 2000a, 24–25). Even scholars working in different research traditions sometimes acknowledge the historical association between anarchy and competition, while maintaining that other factors (e.g., democracy, norms, shared understandings and discursive practices, etc.) can help states overcome these incentives (Wendt 1999; Ruggie 1996, 1998). By identifying the core problem of international politics—the insecurity and competition induced by the existence of independent states in anarchy—

7. Legro and Moravcsik offer a different conception of realism's core premises, but few scholars working in the realist tradition would endorse it. In their version of realist theory, states have "fixed and uniformly conflictual preferences." This view thus treats a key *conclusion* of realist theory (i.e., the occurrence of conflict) as an assumption (1999, 13–16). In fact, one of the important advances in contemporary realist theory has been to show that conflict can arise even if state preferences are not initially at odds.

the realist tradition has set the terms of debate even for those thinkers who do not accept its generally pessimistic conclusions.

In short, the basic elements of the realist tradition are akin to the theory of universal gravitation. On earth, gravity is a force that heavier-than-air objects must overcome in order to fly. One can think of several ways to overcome this force (aircraft, balloons, rockets, etc.), but the fact that gravity can be countered does not mean it has ceased to exist or that knowledge of its role is not essential for designing a workable flying machine. As we shall see, a number of realist theories suggest ways that states can try to mitigate the incentives for competition, but virtually all of them recognize how hard it is to eliminate them completely.

CRITERIA FOR EVALUATION

The utility of a research tradition may be judged by two basic criteria. The first criterion is *explanatory power*: do theories drawn from this research tradition tell us useful things about political events in the real world? Does the theory help us understand phenomena that are both widely prevalent and important? Political science is an empirical enterprise, and the ultimate value of any analytical approach lies in its capacity to explain important aspects of the real world of politics.⁸

A second criterion is *internal fertility*: is a given research tradition able to refine its theoretical claims, while expanding (or in some cases, bounding) the range of phenomena to which they apply?⁹ Is it able to answer critical objections and explain specific anomalies without losing its theoretical

8. Stephen Van Evera argues that explanatory power can be judged by the percentage of variance explained by the independent variable(s), the range of topics covered by the theory, and the prevalence of the phenomena being explained (1997, ch. 1).

9. Social scientists often invoke Imre Lakatos's "methodology of scientific research programmes" (1970), when assessing different research traditions, but they rarely recognize its limitations. Lakatos argues that the key criterion for choosing theories is "excess empirical content," but he never explains how one measures this elusive trait and historians of science have found it difficult to apply his approach to the real world of scientific practice. Lakatos also warns that research traditions "degenerate" when scholars employ ad hoc amendments to account for anomalies (while failing to anticipate "new facts"), but working scientists routinely (and correctly) embrace ad hoc assumptions in the conduct of normal science. Moreover, accusations that a research program is degenerating are difficult to establish because a single progressive advance can redeem a lengthy series of failures. To show that a research tradition is degenerating, therefore, one would have to show that *none* of it contains new insights. Given the many problems with Lakatos's criteria, it is not surprising that his schema is often used as a cudgel with which to bash whatever research tradition a particular critic happens to dislike (see Laudan 1977; Suppe 1977; McCloskey 1994; Walt 1997a). It is equally unsurprising that advocates of different theoretical traditions generally believe that their own enterprise meets Lakatos's criteria (Elman and Elman 2002).

coherence? Are theories based on the research tradition able to illuminate new topics or provide novel insights into past events? Do theories drawn from the particular tradition help identify the flaws in alternative approaches and suggest ways that they might be repaired?

Judged by these criteria, the realist tradition remains an important and lively arena of scholarly inquiry. To see why this is so, let us now consider some of the most important recent developments.

■ | The Varieties of Contemporary Realist Theory

THE NEOREALIST FOUNDATION

Contemporary realist theory begins with Kenneth Waltz's landmark *Theory of International Politics*. In this book, Waltz sought to place realist thought on a firmer social scientific foundation (Waltz 1979, 1991). Drawing on the philosophy of science, neoclassical microeconomics, and systems theory, Waltz developed an extremely parsimonious theory of the international system that sought to explain enduring regularities in international behavior across wide ranges of time and space. Waltz conceived of the international system as an anarchic order composed of states which sought "at a minimum, to survive" and argued that this condition created a "self-help" system in which competition was rife, cooperation difficult, and balances of power tended to form. This positional picture also suggested that bipolar structures would be less war prone than multipolar orders and that low levels of interdependence were more conducive to peace.

Waltz's most fundamental contribution was his emphasis on the international system as an active and autonomous causal force. For earlier realist thinkers, such as E. H. Carr (1946) or Hans J. Morgenthau (1946, 1948), international anarchy was a permissive condition that allowed human aggressiveness to express itself. Conflict occurred because humans craved power or because certain states had revisionist goals, and because there was no central authority to stop them from pursuing these ambitions.

For Waltz, however, the anarchic international system was an active force that "shaped and shoved" the states that made up the system.¹⁰ The condition of anarchy forced all states to worry about their security and compelled them to take active measures to obtain it. Because systemic effects were present, one could not explain international outcomes by looking solely at the characteristics of states or even their underlying preferences. Rather, Waltz argued that "state behavior varies more with differences of power than with difference in ideology, in internal structure of

10. In Waltz's words: "Each state arrives at policies and decides on actions according to its own internal processes, but its decisions are shaped by the very presence of other states as well as by interactions with them" (1979, 65; 1986, 343).

property relations or in governmental form” (1986, 329). Being placed within an anarchic order encouraged states with different internal characteristics to act in similar ways; by the same logic, states with similar characteristics would behave quite differently if they were placed in a different external environment. These systemic pressures encouraged national leaders to emulate successful behavior by other states, and states that failed to compete effectively were more likely to be eliminated from the system. For Waltz, the pressures of competition in an anarchic realm explained why states tended to balance power, why efforts to increase one’s own power might be self-defeating, and why bipolar systems were more stable than multipolar ones.

By locating the sources of insecurity in the characteristics of the system rather than in human nature, Waltz’s conception encouraged efforts to mitigate the competitive pressures he identified. If conflict were ultimately due to human nature (as Morgenthau believed), then efforts to create a more peaceful world were doomed to fail. But if conflict arose from the circumstances in which states were placed, then *in theory* one could identify especially dangerous conditions and fashion appropriate policies to mitigate them. One could be “realistic” in appraising the obstacles while recognizing that there was more room for improvement than earlier realists had recognized.¹¹

Theory of International Politics sparked an enormous reaction, much of it critical. Because anarchy was a constant and changes in polarity occurred only rarely, critics argued neorealist theory was unable to explain international change (Ruggie 1983; Buzan, Jones, and Little 1993) and could provide only general predictions about state behavior (Keohane 1983). Other commentators complained that theory was historically inaccurate (Schroeder 1994a) or accused Waltz of helping to legitimate a dangerous discourse of power politics, thereby contributing to the very problems the theory purported to explain (Ashley 1984). And a number of writers observed that neorealism did not provide detailed policy guidance, because its core variable—polarity—was difficult (if not impossible) for states to manipulate (Van Evera 1999).

Finally, a number of scholars noted that Waltz’s theory was underspecified. As Randall Schweller has emphasized, if we accept Waltz’s assumptions that the system is anarchic and all states merely seek to survive, then there is no need for them to worry about each other and no reason for conflict to arise. In order for competition to occur, the theory must allow for

11. Morgenthau, Carr, and other earlier realist scholars did offer various prescriptions for peace. Indeed, as Marc Trachtenberg (2001) observes, many scholars working in the realist tradition see the world as highly competitive yet advocate policies of moderation. This irony reflects their sense that in a dangerous world, excessive ambition and ideological zeal can place even powerful states at risk. By freeing realist thought from a pessimistic focus on human nature, however, neorealism offered a more optimistic basis for action.

the possibility that at least one state will try to alter the status quo. Yet in part because he systematically excluded unit-level elements, Waltz's neorealist theory did not explain how this might occur (Schweller 1996; see also Milner 1991; Wendt 1999).¹² Similarly, some of Waltz's other predictions (such as the propensity for states to balance power or the peaceful nature of bipolar systems) were not strictly deducible from his premises in the absence of additional information about the preferences of states and the structure of their interactions (Wagner 1993; Powell 1999).

These various critiques share a common theme, namely, that Waltz's purely structural theory was too parsimonious, unable to account for a number of important issues, and prone to indeterminate (or incorrect) predictions. Not surprisingly, therefore, much of the subsequent work within the realist tradition has sought to build on Waltz's important insights while enriching his spare depiction of the international system.

Recent realist research is also less parsimonious because it has placed greater emphasis on empirical testing. Like Carr and Morgenthau, Waltz illustrated his arguments with apt examples but did not test them with systematic empirical evidence. By contrast, most of the subsequent research in the realist tradition has relied on a broader base of quantitative or qualitative evidence. This increased attention to empirical testing is consistent with realism's long-standing emphasis on studying politics as it really is and has helped subsequent scholars enrich Waltz's stark conception.

DEFENSIVE REALISM

A central development in contemporary realist theory has been the emergence and refinement of defensive realism (Jervis 1978; Van Evera 1984, 1985, 1999; Walt 1989; Snyder 1991; Glaser 1994–95; Lynn-Jones 1995; Taliaferro 2000–01). Defensive realism accepts the basic idea that international anarchy forces states to worry about their security as well as Waltz's claim that states generally balance against especially strong or aggressive powers.

Defensive realism then makes an important revision to Waltz's purely structural theory. Waltz defined different international systems solely in terms of the gross distribution of power (i.e., bipolar or multipolar) and omitted other aspects of the physical and military environment of states. By contrast, defensive realists focus on the "fine-grained structure of power" (Van Evera 1999) and emphasize how geography and technology combine

12. As discussed below, Schweller's critique is not as damning as it appears. History offers no example of a great power that was a pure security seeker, so it makes sense for great powers to act as if other states had at least some revisionist aims. More importantly, a world of pure security-seeking states would still be prone to competition, either because each state could not be sure that the other states were benign or because none could guarantee not to become aggressive in the future (Jervis 1976, 62; Copeland 2000b, 199–200; Mearsheimer 2001).

to affect the security of states. In particular, defensive realism relies on the crucial concept of the *offense-defense balance*, generally defined as the relative ease or difficulty of conquest (Quester 1977; Jervis 1978; Van Evera 1984, 1999; Glaser and Kaufmann 1998). Acknowledging that anarchy encourages states to worry about security; defensive realists nonetheless assert that the offense-defense balance determines the *intensity* of security competition between states. When technology, geography, and so on, make conquest easy, states are less secure, cooperation is more difficult, and competition and war will be more intense, frequent, and extensive.

This amendment leads to two additional theoretical claims. First, defensive realism argues that most states can enhance their security by adopting defensive military postures, especially when the offense-defense balance favors the defending side. Under these conditions, a defensive military posture allows states to protect their own territory without threatening others and to do so in the most cost-effective way. Moreover, adopting a defensive posture enables status quo states to signal their peaceful inclinations (Jervis 1978; Glaser 1994–95; Kydd 1997a, b). Thus, defensive realism suggests that states can make probabilistic judgments about other states' intentions and that status quo powers can use this knowledge to dampen the competitive pressures inherent in anarchy.

Second, in part because they believe that states are strongly inclined to balance aggressive powers, defensive realists argue that expansion is usually difficult and rarely profitable. Nationalism and the emergence of postindustrial "information" economies make it difficult to exploit conquered territories (especially in the modern era): this means that the gains from conquest are not cumulative and usually subject to diminishing returns (Van Evera 1990–91; Kaysen 1990; Brooks 1999). Expansion may yield strategic benefits under certain conditions, but defensive realism believes such circumstances will be rare. Thus, defensive realists generally endorse policies designed to preserve the status quo and see ambitious attempts to expand as self-defeating anomalies that are inconsistent with rational statecraft and probably the product of some sort of domestic political pathology.¹³ These claims are buttressed by ambitious attempts to test the theory through careful empirical studies (Snyder 1991; Van Evera 1999).

Defensive realism has not gone unchallenged, of course. Critics charge that the concept of the offense-defense balance incorporates virtually any factor that could conceivably affect the outcome of a war, raising doubts about whether it is possible to design military postures that will have the predicted effects (Levy 1984; Shimshoni 1990–91; Lynn-Jones 1995; Betts 1999; Lieber 2000). By attacking a core concept in defensive

13. These two claims—that status quo states can signal benign intentions and the relative difficulty of conquest—reinforce each other. If conquest is hard, then states do not need to assume the worst about other states' intentions and can rely on less than perfect evidence about them.

realism, these critiques also cast doubt on its key prescriptions. If states cannot measure the offense-defense balance or distinguish between offensive and defensive capabilities, then security-seeking states cannot escape the security dilemma and cannot signal their peaceful intentions in a convincing manner. Other scholars have challenged the claim that expansionist behavior is generally unprofitable, either by suggesting that the gains from conquest are greater than defensive realists maintain or by emphasizing the barriers that impede the formation of strong balancing coalitions (Liberman 1996a; Mearsheimer 2001; Kaufman 1992). And as defensive realists have sometimes conceded, relatively few great powers seem to have acted in the manner the theory recommends. Although this is not a fatal shortcoming, it does not increase confidence in the explanatory power of their arguments.¹⁴

OFFENSIVE REALISM

The strongest challenge to the defensive variant of realist theory has come from other scholars within the realist tradition. This group—sometimes termed offensive realists—has offered both a sharp critique of the defensive realist position and proposed a more rigorously systemic account of the competitive nature of international politics.

Offensive realism challenges its defensive cousin on several grounds.¹⁵ Waltz and the defensive realists begin by assuming that all states seek to survive. But as noted, it is not clear why conflict will occur unless at least one of the states in the system has some other reason to overturn the status quo. Defensive realism reinforces this status quo bias by arguing that conquest is usually difficult; this implies that rational states will rarely be inclined to expand.

To overcome this puzzle, offensive realism emphasizes the inability of states to gauge one another's intentions with 100 percent confidence. Even if states could judge current intentions accurately, they cannot be sure that intentions will not become more aggressive in the future. Because states cannot know what others might do, they have a powerful incentive to increase their own power so as to be ready for a challenge if it does emerge. Moreover, states are probably aware that other states face the same problem; this means that other states will be tempted to increase their own power as well. Thus, states are driven to compete for power because they

14. Defensive realists would respond by noting that those states that ignored the theory's implications (e.g., Wilhelmine and Nazi Germany, Imperial Japan, and possibly the former Soviet Union) tended to suffer especially grave foreign policy reverses.

15. The summary in these paragraphs is based on Mearsheimer 2001, but elements of these arguments can also be found in Spykman 1942, Wight 1979, Gilpin 1981; Schweller 1994; Labs 1997; Zakaria 1998; and Copeland 2000a.

are uncertain about the intentions of others and because they know that others may be driven to compete for power by similar fears. Even a world made up of status quo powers might still exhibit high levels of competitive behavior, simply because none of these powers could be certain what other states would do in the future.

Several implications follow. First, where Waltz and the defensive realists argue that states should not try to maximize power (for fear of provoking a hostile coalition), offensive realists suggest that all major powers are constantly looking for opportunities to improve their relative power position. Second, defensive realists see balancing as the preferred tendency for most states, while offensive realism predicts that states will normally choose to pass the buck rather than shoulder the burdens of balancing themselves. Given a choice, great powers try to get other states to do the fighting and dying necessary to prevent a hegemon from emerging. Balancing is the fall-back option when buckpassing fails, but great powers will prefer to let other states fight while they remain safely on the sidelines. Third, the temptation to pass the buck explains why efforts to balance an aggressor are often inefficient; this is one reason why offensive realists believe that opportunities to expand are more common than defensive realists do. Fourth, offensive realists also believe that the gains from expansion are usually cumulative; for them, conquest often pays. Taken together, these claims imply that great powers will compete more vigorously, because all face similar temptations and all recognize the danger of falling behind. Even states that might otherwise be content with the status quo will be inclined to try to alter it in their own favor, because if they don't, some other state is likely to try to seize the moment and shift it against them.

Thus, offensive realism sees anarchy as an even more powerful disposing force than Waltz's neorealist theory did. Where Waltz argued that the fear that others will balance discourages attempts to maximize power (1979, 127), offensive realists argue that power maximization is precisely what the system encourages. Most importantly, the desire to acquire more power does not occur because humans are driven by a lust for power (as Morgenthau suggested) or because certain regimes are driven by nonsecurity motives such as an expansionist ideology. Rather, states strive to increase their power even if their only goal is to preserve their independence, because the stronger a state is, the more likely it is to survive the vicissitudes of international life.

These incentives do not imply that the great powers are constantly at war, however. Rather, offensive realism sees great powers as opportunistic aggressors; it implies that states look for opportunities to increase their power at acceptable cost and risk (Mearsheimer 1994–95; 2001; Labs 1997; Zakaria 1998). Indeed, the system encourages states to be *prudent* expansionists; because all great powers are in competition with each other, they must try to gain more power only when circumstances are favorable.

Given this conception of the international environment and its conse-

quences, offensive realism predicts that security competition (and the danger of war) will be greatest in multipolar systems, because states will be more inclined to pass the buck and efforts to expand are less likely to face rapid and effective opposition.¹⁶ Opportunities for expansion will also increase when power is unevenly distributed among the major powers, because this allows the strongest state(s) to gain more power at the expense of the weaker states. Thus, security competition will be most intense when the system is multipolar and the distribution of power is heavily skewed. Indeed, when a major power becomes strong enough to make a plausible bid for regional hegemony—as Napoleonic France, Wilhelmine and Nazi Germany, and Imperial Japan did—offensive realism predicts the outbreak of a hegemonic war.¹⁷ Although each of these attempts ultimately failed, supporters of the theory argue that each of these aggressors had a plausible “theory of victory” and came close to success. Unlike their defensive counterparts, therefore, offensive realists do not need to invoke domestic variables in order to explain even highly expansionist behavior (Copeland 1996a, 2000a; Mearsheimer 2001).

Although advocates of offensive realism have also marshaled considerable historical evidence to support their case, it is hardly immune from criticism. First, the claim that states will try to increase their power whenever circumstances are favorable is difficult to falsify; if competition is intense and great powers are actively seeking to expand, then offensive realists will see this as supporting the theory. But if competition is mild, then they are likely to argue that conditions for expansion were not attractive. The theory is not tautological, but a proper test requires an independent measure of the costs and risks of expansion.¹⁸

Second, as its advocates admit, the theory faces a number of historical anomalies, including Germany’s failure to launch a hegemonic war in 1905 or the U.S. failure to exploit its economic superiority and nuclear monopoly during the early cold war.¹⁹ More generally, offensive realism has

16. Multipolarity also increases the risk of entrapment whereby states are dragged into a war by the fear that their allies will be defeated or will defect if they do not come to their aid (Snyder 1984; Christensen and Snyder 1990).

17. According to Dale Copeland (1996a, 2000a), whose work combines elements of both offensive and defensive realism, such a conflict is especially likely when the leading state is strong enough to make a plausible bid for hegemony and also believes that its relative power will decline sharply in the future. Copeland also argues that preventive wars are more likely in bipolar systems, at least under certain conditions.

18. For a preliminary attempt to provide such a test, see Mearsheimer 2001, ch. 9.

19. Russia’s defeat in the Russo-Japanese war gave Germany a window of opportunity, but Germany’s leaders chose not to exploit it. Similarly, although U.S. policymakers debated the possibility of preventive war during the early cold war, they ultimately declined to use the U.S. nuclear advantage to deny the Soviets an analogous capability or to extract other concessions (Trachtenberg 1991, ch. 3).

trouble explaining why the leading power would ever permit another state to catch up (at the very least, it should work very hard to prevent it). Proponents of the theory tend to portray the international system as a realm where intense competition is endemic, but great powers sometimes try to moderate the level of competition and may succeed in doing so temporarily (Trachtenberg 2001; Schroeder 1994b). In general, in fact, offensive realism understates the risks of relentless competition and underestimates the possibility that states could increase their security by agreeing (either formally or tacitly) not to compete in certain ways. Although offensive realism allows for such arrangements so long as they leave the balance of power unchanged, the theory cannot account for such behavior and suggests it will be quite rare.

Despite these limitations, offensive realism offers a powerful systemic account of key aspects of international behavior, including the sensitivity of states to relative position or the widespread tendency for war aims to escalate. It also avoids defensive realism's ambiguity on whether expansion is desirable and its reliance on contested concepts like the offense-defense balance.

RECONCILING OFFENSIVE AND DEFENSIVE REALISM?

Offensive and defensive realism are rival theories within the same broad family. They emphasize different assumptions, employ different causal logics and reach different conclusions. Offensive realism assumes states cannot discern intentions with any confidence (thereby encouraging worst-case planning); defensive realism suggests that they can (and do) under a fairly wide range of conditions (Brooks 1997). Offensive realism argues that the system forces all states to compete for power; defensive realism believes these structural imperatives are much weaker and traces aggressive behavior to various domestic pathologies operating within an anarchic setting. Thus, offensive realism is more pessimistic about the prospects for moderating international competition, while defensive realism believes that proper strategic policies can reduce (though not eliminate) the insecurity inherent in anarchy.

Despite these clear differences, the gap between these two strands of realist theory may not be as large as it first appears. Defensive realists recognize that states can sometimes increase their security by expanding, and they invoke unit-level variables only to explain exaggerated security fears or consistently self-defeating aggression. Similarly, offensive realists admit that *excessive* expansion can leave a state worse off. Both defensive and offensive realists recognize that the efficiency of balancing behavior can vary, and agree that inefficient balancing can make opportunistic aggression more attractive. Offensive realists reject the concept of an offense-defense balance, but John Mearsheimer (who has produced the most careful and detailed explication of offensive realism to date) uses a key element of this

concept—geography—to explain why no state can aspire to global hegemony.²⁰ These similarities reveal that there is more overlap between the two strands of thought than either group has recognized.

Indeed, the differences between offensive and defensive realists may depend less on conceptual disagreements than on differing assessments of empirical conditions, which in turn shape the interpretation of specific historical cases. As already noted, defensive realists generally believe that balancing tendencies, defensive military advantages, and the power of nationalist resistance combine to make conquest difficult and generally unprofitable. As a result, they see Germany's decision for war in 1914 or Japan's policy of expansion in the 1930s as a departure from realist behavior that must be explained in some other way (Snyder 1991; Van Evera 1999; Kydd 1997a). By contrast, advocates of offensive realism believe that conquest is often profitable and argue that military and diplomatic conditions before World Wars I and II facilitated the German and Japanese attempts to establish regional hegemony. Thus, the question of which theory is more accurate depends not on the deductive rigor of the two alternatives but on which theory best fits the historical record.

NEOCLASSICAL REALIST THEORY

Some critics of neorealism have suggested that Waltz's quest for parsimony and scientific rigor sacrificed the practical wisdom found in the work of Thucydides, E. H. Carr, and Hans J. Morgenthau (Ashley 1984). In this spirit, a number of scholars have proposed a new synthesis that some commentators have termed neoclassical realism (Rose 1998). Like other realist theories, the neoclassical variant sees the system as anarchic, emphasizes the importance of relative power, and highlights the importance of security in the face of rival centers of power. But where Waltz insisted that his theory was not a theory of foreign policy, the neoclassical strand of realist theory pays less attention to explaining the properties of the system as a whole and focuses primarily on explaining specific foreign policy decisions.

As Rose (1998) has noted, neoclassical realists believe "there is no immediate or perfect transmission belt linking material capabilities to foreign policy behavior." Neoclassical realists acknowledge the role of relative power, but they believe that power is impossible to measure precisely and frequently misunderstood. Accordingly, any attempt to analyze foreign policy must also consider how power was perceived by the decision makers.

20. Mearsheimer stresses the difficulty of projecting power over large bodies of water. This constraint—which is a form of defensive advantage—explains why states can establish hegemony within a region (as the United States has done in the Western hemisphere) but not over the entire world. In theory, technological developments might eventually overcome the "stopping power of water" and permit one state to establish global hegemony. Defensive realists would regard this as a shift in the offense-defense balance (Mearsheimer 2001, ch. 4).

As the “neoclassical” label implies, this perspective is in some ways a return to the earlier views of Thucydides, Machiavelli, Carr, and Morgenthau, where anarchy is a permissive condition rather than an independent causal force. Because there is no overarching authority to control what states do, unit-level forces within states can affect the choices they make. Power is the underlying variable in the neoclassical version of realist theory, but the causal logic of the theory places domestic politics as an intervening variable between the distribution of power and foreign policy behavior. At the same time, neoclassical realists also believe there are clear limits to these domestic effects. Over time, shifts in the balance of power will constrain state behavior, and leaders or regimes whose strategic vision is clouded will pay a price for their lack of clear sight.

Thus, neoclassical realism seeks to combine the main elements of structural theory with an empirically grounded sensitivity to the calculations that real-world decision makers face. This perspective has produced a number of important historical analyses that combine a concern for shifting balances of power with particular attention to the ways that national leaders interpreted and reacted to it. Thus, Randall Schweller (1998) has offered a novel explanation for World War II, arguing that a tripolar distribution of power facilitated Hitler’s revisionist aims, and Fareed Zakaria (1998) argues that the U.S. rise to world power was delayed (though not prevented) by the absence of a strong state apparatus. William Wohlforth (1993) suggests that disagreements about the true balance of power shaped U.S. and U.S.S.R. conduct throughout the cold war, and Thomas J. Christensen (1996) argues that Sino-U.S. hostility in the early cold war was rooted in anarchy but exacerbated by each side’s efforts to mobilize domestic support. Similarly, Daniel Byman and Kenneth Pollack (2001) have recently called for renewed attention to the ambitions and personalities of particular leaders in order to explain why certain states chose highly aggressive foreign policies.

Because neoclassical realism eschews a monocausal focus on either domestic or systemic variables, it is especially well suited to the construction of historical narratives.²¹ Unfortunately, this open-minded eclecticism is also its chief limitation. Neoclassical realism tends to incorporate domestic variables in an ad hoc manner, and its proponents have yet to identify when these variables will exert greater or lesser effects. Furthermore, neoclassical realism relies almost entirely on theoretical arguments developed by others and has yet to offer a distinct set of explanatory hypotheses of its own. Thus, where neorealism sacrificed precision in order to gain parsimony and generality, neoclassical realism has given up generality and predictive power in an attempt to gain descriptive accuracy and policy relevance.

21. Several neoclassical works have been criticized on purely historical grounds; see Lynn-Jones 1998.

■ | The Vitality of Contemporary Realist Theory

In addition to various efforts to refine or restate the core premises of realist theory, much of the recent work in the realist tradition seeks to explain particular empirical puzzles and to engage in critical debate with alternative approaches. Some of these efforts have also led to important theoretical refinements, and these works reaffirm the continued vitality of realist thought.

ALLIANCE THEORY

Given the central role that alliances play in the operation of a balance of power system (Morgenthau 1959), it is not surprising that recent realist research has devoted considerable attention to exploring the dynamics by which alliances are formed and their effects on subsequent state behavior. Building on Waltz's claim that great powers tended to balance against the strongest state or coalition rather than "bandwagon" with it, subsequent research argued that states were in fact more likely to balance against *threats* (Walt 1987, 1988). Threats were conceived as a function of power, geographic proximity, offensive capabilities, and perceived intentions, and this revision helped explain a number of empirical anomalies in Waltz's theory.²² This line of research also explained why especially weak or isolated states might be somewhat more inclined to "bandwagon" than the major powers were. Balance of threat theory has been challenged by several critics (Kaufman 1992; Labs 1992; Brand 1994; Barnett and Levy 1991; Vasquez 1997), but it also received support from a number of empirical studies (Garnham 1991; Priess 1996; Mastanduno 1999; Walt 1992).

Perhaps the most ambitious extension of realist theory in the realm of alliances is Glenn Snyder's *Alliance Politics* (1997), which adds a series of systemic variables to Waltz's structural theory in order to explain different patterns of alliance behavior.²³ The resulting theory is not as parsimonious as Waltz's, but what is gained is a more detailed set of predictions than a purely structural theory can provide. Other research on alliances has ex-

22. For instance, balance of threat theory explained the relative lack of balancing against the United States both during and after the cold war, and the size of the coalition that defeated Iraq during the 1991 Gulf War (Walt 1987; Garnham 1991).

23. Specifically, Snyder adds relationships (patterns of alignment, interest, capability and interdependence), interactions (the processes by which relationships are translated into outcomes), and structural modifiers (military technology and institutions) to define a variety of international systems whose dynamics he then deduces. In essence, Snyder takes variables Waltz consigned to the unit-level and restores them to the systemic level, arguing that they are qualities of the system as a whole rather than merely properties of the units. His work is clearly realist in orientation, as revealed by his statement that "strategic interests are largely derived from the structure of the international system and the alignments that form within it" (1998, 23).

explored the relative importance of internal versus external threats (David 1991), the tendency for revisionist states to prefer “bandwagoning” to balancing (Schweller 1994), the twin dangers of abandonment and entrapment (G. Snyder 1984), the impact of perceptions and military technology on alliance cohesion (Christensen and Snyder 1990; Christensen 1997), the stability of different alliance patterns (Mueller 1995), and the future of contemporary alliance commitments (Mearsheimer 1990; Layne 1993; Hellman and Wolf 1993; Walt 1997b, 1998, 1998–99).

REALIST THEORIES OF INTERNATIONAL POLITICAL ECONOMY

Realist theory has also made major contributions to the study of international political economy. In general, realist scholarship in this area emphasizes the central role that power plays in shaping the calculations of states, and it highlights how the possibility of conflict and war can shape state behavior in a variety of nonmilitary realms (Kirshner 1999). As a result, recent realist scholarship has provided a useful counterpoint to theories that neglect or bracket these elements.

An obvious contribution was Joseph Grieco’s seminal work on relative gains and international cooperation (Grieco 1988, 1990; Gowa 1986). Responding to the liberal argument that international institutions could enhance the prospects for cooperation by providing information and reducing the risk of cheating, Grieco showed that institutionalist theory had overlooked the central role of power. In particular, liberal institutionalism had underestimated the obstacles to cooperation by ignoring the issue of how the gains were distributed. Because the fear of war and the desire for national autonomy makes states sensitive to the balance of power, they will be wary of cooperative arrangements that might leave others significantly better off over time. The point was not that cooperation was impossible (especially when security concerns were low), only that institutionalist theory had portrayed it as easier than it really was.

Grieco’s work launched a vigorous debate, and a careful reading shows that most of his critics ultimately confirmed or conceded his basic point.²⁴ As the leading institutionalist, Robert Keohane, later acknowledged, Grieco “made a significant contribution by focusing attention on the issue

24. For example, Duncan Snidal used a formal model to show that the importance of relative gains declined as the number of powers increased, but his results depend on the assumption of equal returns to scale from any act of cooperation. As he notes, “unequal gains diminish the incentives of one side to cooperate . . . for both absolute and relative gains reasons.” In this model, in short, relative gains do not impede cooperation because there were by definition no gaps in gains. Snidal justified this assumption by saying that any gaps that did arise could be corrected through side payments, but this point (which was also noted by Grieco) essentially means that states can cooperate if they can cooperate in dividing up the gains. Compare Snidal 1991, 717, n. 29; and Grieco 1990, 233–34.

of relative gains, a subject that had been underemphasized, especially by liberal or neo-liberal commentators on the world economy" (1993, 283).²⁵ Having conceded the theoretical issue at hand, the central question is largely empirical: how often (and how extensively) has a concern for relative gains led states to forgo mutually beneficial agreements?²⁶ Although the realist position received additional support from several empirical studies (Krasner 1991; Mastanduno 1991; Matthews 1996; Nayar 1995), these efforts should not be regarded as definitive.²⁷

Scholars working within the realist tradition have offered a host of other studies within the subfield of political economy. In *Allies, Adversaries, and International Trade* (1994), Joanne Gowa shows how the bipolar distribution of power during the cold war encouraged the expansion of economic interdependence among the OECD states (see also Mastanduno 1999, 1991). Institutions and values played important supporting roles but were less important than the shared objective of opposing the Soviet Union. More recently, Lloyd Gruber's *Ruling the World: Power Politics and the Rise of Supranational Institutions* (1999) showed how powerful states can force weaker powers to accept second-best outcomes by threatening to "go it alone." Gruber's analysis (which was backed by an exemplary set of case studies) thus challenged the widespread belief that

Similarly, Robert Powell (1991) has constructed a formal model showing that relative gains do not impede cooperation when the costs of warfare discourage the use of force. This result is consistent with Grieco's claim that cooperation was easier when a state's sensitivity to relative gains was low (i.e., because it was relatively secure). Because Powell assumes states have identical preferences, his formulation is more consistent with realist theory than Grieco's. But the underlying principle—that the fear of war makes states sensitive to relative gains and discourages cooperation—is identical.

25. Keohane qualified this concession by arguing that Snidal's and Powell's work had significantly diluted the force of Grieco's critique. But as discussed in the previous footnote, Powell's and Snidal's analyses are consistent with Grieco's central theoretical claim.

26. As Jervis (1999) notes: "the greatest deficiency in the absolute/relative gains literature is that it has remained largely at the level of theory and prescription, with much less attention paid to when decision makers *do* in fact exhibit relative gains concerns" (47, n. 14; see also Liberman 1996b).

27. Efforts to test competing liberal and realist theories of cooperation face serious methodological and interpretive hurdles. Liberal theorists can point to cooperation among the OECD countries as evidence that institutions have facilitated mutually beneficial arrangements; realists respond by noting that the conditions they regard as conducive to cooperation were also present. More generally, both liberal and realist theories identify obstacles to cooperation and recognize that states will sometimes fail to make mutually beneficial deals, but realists believe that the obstacles are larger and that the number and scope of actual agreements will be smaller. But as Kirshner notes, "distinguishing between these subsets in practice is extremely difficult, to the point that it raises serious questions regarding the tractability of such endeavors" (1999, 83; see also Downs, Roche, and Barsoom 1996).

international institutions are inherently fair and Pareto improved and corrected the tendency for scholars of political economy to downplay or disregard the central role of power.

Realist theory has also returned to the familiar question of economic interdependence. These works include efforts to explain the limited effectiveness of economic sanctions (Pape 1997), as well as several related studies showing how expectations about the future affect the relationship between economic interdependence and international conflict. Although the specific variables in these models are different, the underlying logic of these works is quite similar. Specifically, both the potential impact of economic sanctions and the pacifying effects of international interdependence depend on each side's expectations about the future benefits of continued economic interchange. Sanctions exert the greatest coercive impact when they threaten a valuable long-term relationship, precisely because they threaten an anticipated stream of long-term benefits. By contrast, threatening or imposing sanctions has little effect when the relationship is modest or believed to be evanescent (Drezner 1999–2000, 1999, 1997). By a similar logic, the expectation that if war is avoided, high levels of trade will continue increases the expected costs of conflict and thus provides an additional incentive to remain at peace. If states fear that trade may be disrupted (whether by war or by some other cause), however, then interdependence is a source of vulnerability and states are more likely to use force to gain control of the resources they need (Copeland 1996b, 1999–2000; Liberman 1999–2000). In both theories, in short, expectations of the future drive present-day policy decisions.

Scholarship in the realist tradition continues to remind us that international economic relations shape, but are also shaped by, the political structure of international relations. From this perspective, the growth of international trade, communications, and currency transactions (often lumped under the heading of globalization) has been underwritten by U.S. military power and backed by an extensive array of alliance commitments and regulatory arrangements (Gilpin 1976; Waltz 2000b; Kapstein 1994).²⁸ The realist perspective also provides a different account of the spread of neoliberal practices in Latin America, eastern Europe, and elsewhere. Instead of seeing this process simply as the triumph of economic efficiency over elite rent seeking, a realist account would also emphasize the desire to emulate U.S. practices in the wake of its cold war victory and the use of U.S. power to encourage these decisions (Sterling-Folker 1997; Wade 1992).

28. As Joseph Nye (1995, 90–91) has commented, “It has become fashionable to say that the world after the Cold War has moved beyond the age of power politics to the age of geoeconomics. Such clichés reflect narrow analysis. Politics and economics are connected. International economic systems rest upon international political order.” Nye’s views are especially interesting insofar as his scholarly work is liberal in orientation and frequently critical of realist theories.

NEW INSIGHTS ON NEW ISSUES

One sign of the continued fertility of realist theory has been its ability to illuminate new topics that were not addressed in earlier realist scholarship. Shortly after *Theory of International Politics* was published, for example, Barry Posen (1984) used a combination of realist theory and organization theory in a prizewinning account of the sources of military doctrine. Scholars working in the realist tradition have also developed theories to explain important elements of domestic politics. For example, Avery Goldstein (1991) used Waltz's neorealist theory to explain coalition formation in the Chinese Communist Party, and Michael Desch (1996, 1999) argued that more dangerous international environments led to the formation of stronger state bureaucracies and more tranquil civil-military relations. Other scholars have developed realist theories explaining the relationship between revolution and war (Walt 1996; Conge 1996), and the spread of military innovation (Posen 1993a; Resende-Santos 1996; Goldman and Andres 1999).

Realist theory has also made a central contribution to the burgeoning literature on ethnic conflict. In particular, Barry Posen's seminal article "The Security Dilemma and Ethnic Conflict" (1993b) showed that key elements of realist theory could explain why certain multiethnic societies were especially prone to conflict in the event that the central government collapsed. Posen's basic insight has been echoed by a number of subsequent writers (Lake and Rothchild 1996; Fearon 1998b; Rose 2000), and expanded or modified in several other works (Kaufmann 1996, 1998). Although these works vary in a number of respects, each takes as its starting point the idea that the collapse of central authority within a state can place different ethnic groups in a "state of nature" analogous to the anarchic international system. These conditions do not make ethnic war inevitable (for the same reasons that states are not always engaged in war), but they do make it more likely. When violence does break out, moreover, efforts to end the fighting are likely to fail in the absence of external guarantees that can ameliorate the insecurity the contending parties are likely to face (Walter 1997).

REALIST THEORY AND INTERNATIONAL HISTORY

Realist theory has also informed a number of important interpretations of past historic events. Markus Fischer (1992) has argued that relations among the heterogeneous political units of feudal Europe exhibited the competitive dynamics that realist theory predicts (i.e., alliances, security competition, war, etc.) despite the ideology of Christian universalism that infused the period. Fischer's work thus challenged the constructivist claim that a transformation in norms, identities, or the structure of property rights can overcome the competitive tendencies of an anarchic political order. It

also suggested that realist theory could be fruitfully applied to the premodern era (see also Hall and Kratochwil 1993; Fischer 1993).

Scholars working in the realist tradition have offered powerful new interpretations of the Concert of Europe. The Concert is often invoked as a case that shows how shared identities and institutionalized norms can dampen the level of great power competition (Schroeder 1994b), but Korina Kagan (1997–98) and Matthew Rendell (2000), in separate treatments, present a powerful challenge to this view. Although there was no great power war between 1815 and 1850, Kagan and Rendell argue that shared identities and common norms did little to encourage restrained behavior. Instead, as Kagan observes, “the great powers played the game very hard indeed, and not according to Concert rules. Rather than mutual consideration, mutual mistrust and exploitation were the name of the game. The chief source of restraint was countervailing force” (1997–98, p. 54). By showing that realist theory can account for an episode long regarded as a paradigmatic case of great power cooperation, these analyses both reaffirm the value of the realist tradition and cast doubt on several prominent alternatives.

Realist theory has also shaped a number of recent studies of the cold war. In separate works, historians Melvyn Leffler (1992) and Marc Trachtenberg (1999) showed how security considerations—and especially a concern for relative power—drove U.S. (and European) calculations during the early years of the cold war. Similarly, Mark Sheetz’s study of NATO’s early history presents a striking confirmation of realist logic and a challenge to the claim that international institutions can help states overcome significant security fears. Although the United States repeatedly tried to create arrangements (such as the European Defense Community) that would enable it to withdraw its troops, these efforts failed because “regimes and institutions were not able to overcome the logic of anarchy or ameliorate the security dilemma in the absence of American power” (1999, 6–7). Realist theory receives further support in recent studies of U.S. policy toward the former Soviet bloc, which demonstrate that the United States was far more offensively minded than previously believed (Mitrovich 2000; Grose 2000).

Finally, several recent works suggest that the realist tradition may tell us more about the end of the cold war than any of its theoretical rivals. In particular, the work of William Wohlforth and his collaborators (Wohlforth 1994–95; Schweller and Wohlforth 2000; Brooks and Wohlforth 2000–01) shows that the cold war ended because Soviet power was eroding rapidly and because Soviet leaders saw few alternatives to capitulation.²⁹ Far from disconfirming realist theory, in fact, the end of the cold war is consistent with its emphasis on material power and its view of the international sys-

29. For a series of Soviet statements acknowledging that they could no longer afford to compete with the West, see Brooks and Wohlforth, 2000–01, 46–47.

tem as inherently competitive. Among other things, the United States did not respond to the Soviet Union's repeated concessions by reducing the pressure or offering compensating concessions in return. Instead, as realist theory predicts, the United States steadily increased its demands as the Soviet Union's position grew weaker, and proceeded to exploit its victory when the Soviet Union finally collapsed.³⁰

These examples demonstrate that realist theory continues to illuminate important historical events and processes. It is hardly the only way to think about the past (even for those who are solely interested in foreign policy), and some international developments are inconsistent with realist premises and require an alternative perspective (for an important example, see Kaufmann and Pape 1999). Yet the studies discussed in this section remind us that abandoning the realist tradition would deprive us of a powerful tool for grasping much of international history, thereby impoverishing the interpretation of the past and distorting our understanding of the present.

THE POST-COLD WAR WORLD

The realist tradition has long been concerned with contemporary events, and a number of scholars have used realist theory to analyze contemporary world politics and to forecast its future course. Although most of these writers reject the optimism that characterized the immediate aftermath of the cold war, their predictions have been quite diverse. According to John Mearsheimer, the lack of a great power rival is eroding the U.S. commitment to Europe and will eventually rekindle security competition within Europe itself (Mearsheimer 1990, 2001). Other writers agree that NATO is likely to erode now that there is no external threat to hold it together (Walt 1997b, 1998–99), but some believe that the separate European states are more likely to draw together in order to balance the United States (Waltz 2000a). In separate treatments, Waltz and Christopher Layne predicted that a combination of “imperial overstretch” and external balancing would undermine the preeminent position of the United States and hasten a return to multipolarity (Waltz 1993; Layne 1993). This view has been countered by William Wohlforth (1990), who argues that U.S. preponderance is now so large as to deter a hegemonic challenge or even a serious effort to balance it (see also Joffe 1995). Wohlforth's article is an important theoretical advance, insofar as it applies Waltz's structural approach to a new configuration of power (unipolarity) and suggests that bandwagoning rather than balancing is the expected behavior in such a realm. Aaron Friedberg and Denny Roy have used realist theory to predict emerging se-

30. When Mikhail Gorbachev complained that “U.S. policy is one of extorting more and more concessions,” U.S. secretary of state George Shultz replied, “I’m weeping for you” (quoted in Wohlforth 1994–95, 121).

curity competition in Asia (Friedberg 1993–94; Roy 1994), and several other scholars have used realist arguments to explain why Japan may be abandoning its antimilitarist political culture and shifting to a policy of “reluctant realism” (Twomey 2000). Michael Mastanduno has provided a similar appraisal for the United States, arguing that its foreign policies since 1991 are largely consistent with realist balance of threat theory (Mastanduno 1999).

These views share the common belief that much of contemporary world politics is still characterized by a concern for security and power. The precise forecasts differ, however, because different authors invoke different realist theories and employ different parameter values when analyzing contemporary events. Thus, Mearsheimer anticipates renewed competition in Europe because he believes that European integration will not overcome the durable power of nationalism and that the absence of a hegemonic rival in Europe has removed the need for U.S. protection and will soon produce a U.S. withdrawal. Waltz disagrees, largely because he thinks Europe is more likely to unify in opposition to U.S. dominance. Both anticipate renewed competition, but they disagree about the identity of the units that will be competing. Similarly, Wohlforth’s optimism rests not only on a theoretical refinement of existing structural arguments but also on a careful assessment of global power trends. He recognizes the *potential* for renewed competition should the United States withdraw but believes that this is precisely why the United States should (and will) remain engaged (see also Art 1996a).³¹

CRITIQUES OF ALTERNATIVE APPROACHES

Just as E. H. Carr once offered a trenchant critique of earlier idealist approaches to international relations, contemporary scholars have used realist theory to challenge a number of alternative perspectives. These efforts include important critiques of institutionalist theory (Grieco 1990; Krasner 1991; Mearsheimer 1994–95; Schweller and Priess 1997; Waltz 2000a) and the recent wave of cultural approaches in security studies (Desch 1998). Not surprisingly, scholars working in the realist tradition have also challenged the so-called democratic peace hypothesis on a variety of grounds. In particular, realist critics have suggested that the absence of war between democratic states is a statistical artifact, valid only for the post-1945 period, or better explained by the distribution of power or some other

31. To an extent, this debate rests on different assessments of the effects of U.S. engagement on the overall probability that the United States would have to fight a major war. Disengagement might increase the overall danger of war but makes it less likely that the United States would be immediately involved. Engagement lowers the overall risk of war but makes U.S. involvement virtually certain if war does come.

factor (Spiro 1994; Layne 1994; Rousseau and Mueller 1995; Elman 1997; Farber and Gowa 1995; Gowa 1999).³²

These various exchanges were often quite sharp, in part because some participants appeared to view the competition between rival approaches in purely zero-sum terms.³³ Reasonable people can differ in their assessments of these debates, but each of these critiques raised important questions about these different nonrealist approaches and forced advocates of these perspectives either to refine their theories, qualify their claims, or search for additional empirical support.³⁴ Just as criticisms of different realist theories have helped their proponents rethink and refine their positions, these realist critiques helped the field as a whole become more rigorous and sophisticated.

SUMMARY

This survey of recent realist scholarship suggests that it meets the two criteria for theoretical progress identified earlier. First, realist theory continues to shed useful light on a diverse array of important contemporary phenomena. It is not the only useful way to think about alliance relations, ethnic conflict, international cooperation, international political economy, security competition, and so on, but our understanding of these (and other) phenomena would surely be impoverished (if not grossly distorted) were the insights of the realist tradition neglected.

Second, realist theory remains theoretically fertile. The family of realist theories has become more diverse, and as the debate between offensive and defensive realism shows, debates within realism have clarified competing causal logics and identified key sources of disagreement. Realist theory has also moved into new subject areas and offered a host of new hypotheses on more familiar subjects. Realist theory has also answered the charge that it was better at identifying the sources of conflict than suggesting sol-

32. Donald P. Green, Soo Yeon Kim, and David H. Yoon have recently offered an important methodological critique of much of the democratic peace literature, see Green, Kim, and Yoon 2001 (and the various responses to them).

33. Thus, Theo Farrell (1999, 168) likened realism to a sinking ship, and Bruce Russett predicted that “the theoretical edifice of realism will collapse” if the democratic peace hypothesis is confirmed (1995, 164). More reasonably, Ze’ev Maoz pointed out that the democratic peace research program had also generated “considerable empirical support for nation- and dyadic-level propositions derived from realist perspectives” and suggested that “the realist and liberal perspectives on world politics may complement each other quite well” (1997, 193).

34. In a recent survey, Keohane and Martin (2002) acknowledge Mearsheimer’s charge that institutions were largely epiphenomenal, and thus have little *independent* effect on state behavior. In particular, they admit that “institutional theory has not yet responded very well to this fundamental challenge.” See also Maoz 1997, 192–93.

utions, and scholars working within this tradition have offered several practical remedies based on their theoretical insights.

These achievements also show that recent claims that realism is degenerating are mistaken (Vasquez 1997; Legro and Moravcsik 1999). This charge rests on the observation that different realists have advanced competing arguments on a variety of subjects and that some scholars have combined structural and unit-level arguments when analyzing particular international phenomena. With respect to the first point, condemning a research tradition because it contains competing hypotheses would require us to toss out virtually every body of theory in the social sciences (Walt 1997a, 932). More importantly, disagreements between different realist theories are an important source of theoretical progress, just as they are for other approaches to international relations. When different realist theories generate different predictions, therefore, it is more properly seen as a sign of theoretical vigor than evidence of degeneration.

Similarly, the fact that some scholars invoke unit-level variables in conjunction with realist theory is hardly persuasive evidence of degeneration, especially when realist theory helps explain why other unit-level variables are needed. In his version of defensive realism, for example, Jack Snyder (1991) uses domestic variables to explain *excessive* expansion (i.e., expansion where the costs exceed the benefits). Snyder explicitly acknowledges that states in anarchy can sometimes increase their security by expanding and that the constant concern for security empowers those who may have other reasons for advocating expansionist policies (1991, 8). Snyder uses domestic politics to explain deviations (i.e., *overexpansion*) from this realist baseline. It is a theory of domestic politics, to be sure, but realist theory identifies both why a unit-level explanation is needed and why certain groups (e.g., the military) enjoy privileged positions within policymaking circles.³⁵ Finally, realist logic also explains why these states suffered from their failed attempts to expand (i.e., they were defeated by defensive coalitions brought into being by their expansionist behavior).

My own work on alliance formation emphasizes the role of power (and especially military power) in a manner consistent with realist logic. If the power of other states were not a major concern, national leaders would not bother to form alliances at all. States look for allies in order to balance the power of other states, and that search intensifies when other states appear to have malign intentions. Balance of threat theory also predicts that domestic variables (e.g., ideology) will have larger effects when structural

35. Both Snyder (1991) and Stephen Van Evera (1984) emphasize the role of military organizations in shaping foreign policy behavior, but a pure theory of domestic politics would have trouble explaining why states would create military organizations in the first place. By emphasizing the need for states to protect themselves in an anarchic setting, however, the realist tradition can account for this nearly universal tendency quite easily.

elements (i.e., the distribution of power, the ease of conquest, etc.) are muted or indeterminate. Thus, a state facing two neighbors of equal capability will look to domestic factors when gauging which is more dangerous, and a state that is objectively very secure will be more heavily influenced by its own domestic constraints and freer to indulge ideological or other preferences. To say that these arguments are not “realist” is peculiar, at the very least.³⁶

Indeed, as Jennifer Sterling-Folker has argued, realist theories are especially well suited to the incorporation of unit-level variables in a causally consistent manner. At the most fundamental level, realist theories focus on the impact of the external environment on state behavior. *How* states try to respond to these environmental pressures will also be affected by domestic factors, however, and which level dominates is ultimately an empirical issue. In the real world, governments try to reconcile external *and* internal pressures, and there is no degeneration when scholars invoke both types of explanation (Sterling-Folker 1997).

The bottom line is that realist theory is alive and well. It remains relevant, rigorous, and theoretically fecund. Not every realist work survives the crucible of academic scrutiny (which is as it should be), but scholarship grounded in realist theory continues to provide important insights into key elements of world politics.

Like any theoretical tradition, of course, the realist family contains gaps, disagreements, and unresolved puzzles. Although one can never be sure how a theoretical enterprise will evolve in the future, the next section identifies several especially promising areas for future research.

■ | A Research Agenda for the Realist Tradition

CONCEIVING AND MEASURING POWER

The concept of power is central to realist theory, yet there is still little agreement on how it should be conceived and measured. We still lack a firm conceptual foundation on which to base valid measures of national power, although several recent works have made considerable progress on this problem (cf. Wohlforth 1993, 1999; Copeland 2000a; Mearsheimer 2001; Schweller 1998). Identifying appropriate criteria for measuring national power is especially difficult because the relevant components have undoubtedly changed over time. Population, GNP, military spending, and coal and steel production may have been valid indicators in the past, but

36. Other examples of scholars combining realist and nonrealist variables in a causally consistent manner include Posen 1984, Christensen and Snyder 1990, and Snyder 1998. Waltz also recognized that structural theories could be supplemented by theories of domestic politics (see Waltz 1986, 331).

what indicators can capture the declining role of smokestack industries or the growing importance of information technology (Brooks 1999; for a preliminary effort, see Tellis et al. 2000)?

More valid measures of power would aid efforts to test different predictions derived from realist theory and would also help resolve the debate between offensive and defensive realists on the profitability of conquest. Similarly, although defensive realists argue for greater attention to the “fine-grained structure of power” (and especially on the so-called offense-defense balance), they have yet to provide a simple and compelling way to measure it (for initial attempts, see Glaser and Kaufmann 1998; Van Evera 1999; Lynn-Jones 1995).

Finally, realist theory generally implies that power should be broadly (though not perfectly) fungible across issue areas. If this is the case, then we should observe great powers using their diverse capabilities (e.g., military power) to extract concessions in otherwise unrelated areas. Although there have been a few intriguing efforts along these lines (e.g., Spiro 1998; Art 1996b; Stein 1980), the topic of fungibility of power and the possibility of issue linkage remain underexplored by realists and nonrealists alike.

THE IMPACT OF INFORMATION

As offensive realists have recently clarified, the pessimism that pervades much of realist theory reflects the uncertainty that pervades most of international political life. States are insecure in part because there is no central authority to protect them from each other, because they do not know how the balance of power will evolve, and because they cannot be sure what other states are going to do.

It follows that realist theory ought to be interested in factors or developments that affect the quality of information available to states. Thus, recent research on revolutions suggested that these events increased the risk of war in part by reducing each side’s ability to calculate the balance of power and to estimate the likelihood of revolutionary contagion (Walt 1996). And as noted earlier, several defensive realists have argued that states can signal benign intentions by making specific military policy decisions (i.e., adopting a defensive military posture), thereby reducing an opponent’s uncertainty and leaving both sides more secure than before (Glaser 1994–95; Kydd 1997a, b). Similarly, the rationalist approach to international institutions argues that institutions facilitate cooperation by providing information to states, thereby reducing the fear that others will cheat (Keohane 1984).³⁷ Other scholars suggest that states can use past be-

37. Thus, the rationalist approach to institutions is a subset of the realist family rather than a distinct alternative. Its contribution to our understanding of world politics depends on whether the effects of institutions are large or small. As noted earlier (n. 27), however, measuring the independent impact of institutions is not easy.

havior or domestic characteristics to make probabilistic judgments about a state's future conduct (Fearon 1994a; Shultz 1999), although the reliability of these inferences remains contested (Mercer 1996; Copeland 1997; Huth 1997; Hopf 1994).

TESTING CAUSAL MECHANISMS

Social science in the United States places a premium on theoretical novelty and undervalues careful empirical testing. This tendency is unfortunate, as it encourages faddishness and divorces scholars even further from the real world. Moreover, debates within the realist family and between supporters of realist theory and those of its various rivals should not be resolved by asking who can muster the flashiest abstract argument; rather, we should ask which explanation best fits the facts. Determining which theory (or approach) is most useful is an empirical question, and rendering such judgements usually requires careful historical evaluation of the specific causal mechanisms identified in each theory.

Thus, an important part of the research agenda should be an effort to conduct more systematic and fine-grained tests of key realist predictions. One obvious way to do this is to investigate whether the motivations of key decision makers are consistent with the expectations of different theories. It matters what states do, but it also matters *why* they do it.

To take an obvious example, offensive and defensive realism offer contrasting predictions about the goals of national leaders and the policies they are likely to favor. Offensive realists predict that leaders will be constantly concerned about the balance of power and actively searching for opportunities to improve their position. They should also be relatively disinterested in gauging other states' intentions and inclined to assume the worst because they cannot be sure that others are not (or will not become) malevolent. By the same logic, they will expend little effort signaling their own commitment to the status quo (save as an effort to hoodwink rivals). By contrast, defensive realists predict that great powers will often look for ways to stabilize the status quo and will devote considerable effort to distinguishing status quo and revisionist powers. An obvious way to test these competing theories, therefore, is to examine the conduct of great powers and especially the perceptions and motivations of key leaders. Earlier realist scholarship has made a start on some of these questions but can hardly be considered definitive.

Much the same prescription applies to the realm of political economy. Prior work has clarified the conceptual disputes between realist and nonrealist approaches to interdependence, cooperation, and institutions; what is needed now are more careful efforts to evaluate these claims empirically. One especially promising strategy is to compare competing predictions about the motivations of key actors, rather than simply counting the number of institutionalized agreements or trying to devise unbiased measures

of cooperation. When states refrain from cooperating, for example, is it because they were worried that others would cheat or concerned that others might reap disproportionate gains? When states negotiate new cooperative arrangements, do the terms reveal a sensitivity to the distribution of gains or is the bulk of the effort focused on developing appropriate verification measures? If realist theory is correct to highlight the problem of relative gains, then we should be able to find clear cases where states refrained from pursuing mutually beneficial deals because one (or both) parties were worried about the effects of the arrangement on the long-term balance of power.³⁸ If scholars working in the realist tradition hope to bolster their critiques of various institutionalist arguments, showing that states have acted in a realist manner *and for realist reasons* would obviously strengthen their case.³⁹

MAPPING THE POST-COLD WAR WORLD

As discussed earlier in this essay, a number of scholars have used realist theory to describe the current era of world politics and to predict its future course. Efforts to update these forecasts—and to identify why some predictions were not borne out—should be part of the future research agenda. Is Mearsheimer correct in anticipating a gradual U.S. retrenchment and the reemergence of great power security competition? If not, why not? Will other major powers combine to check the threat of U.S. preponderance—as Waltz has predicted—or is Wohlforth correct in predicting that the unipolar world of U.S. preponderance will prevent significant great power conflict for a generation or more? Will the rise of China trigger balancing behavior in Asia and an intensifying Sino-U.S. rivalry (as most realist theories would predict), or will economic interdependence, democratization, and expanded membership in international institutions integrate China into the society of nations and prevent another familiar cycle of great power competition?

Efforts to map contemporary world politics should also address the phenomenon of globalization. The rapid expansion of international trade, finance, and communications, along with the increased prominence of various nongovernmental organizations, have led some observers to foresee a new international order in which states are eventually supplanted by some new form of global governance (see Ohmae 1995; Mathews 1997; Reinicke 1998). At the very least, apostles of globalization believe that states will be increasingly bound by the impersonal power of the market-

38. The obvious example here is CoCom, which limited Western economic cooperation with the former Soviet bloc (Mastanduno 1992). For contrasting empirical assessments, see Mathews 1996 and Liberman 1996b.

39. For other predictions about the post-cold war international political economy, see Kirshner 1999.

place and will find their freedom of action constrained to a far greater extent (Friedman 1999; Strange 1996).

Realist theory casts a different light on these claims. First, the current situation is not unprecedented: although it does have certain novel features (such as the size of global currency transactions and the plummeting cost of communications), current levels of interdependence are roughly equal to the first decade of the twentieth century. The term *globalization* is also something of a misnomer, given that the phenomenon is largely confined to the advanced industrial countries of Europe, North America, and Asia and has had relatively little impact elsewhere.⁴⁰

Second, realist theory suggests that the real question is not whether globalization will vanquish the state (which shows no signs of withering away anyway) but rather how it will affect the relative power of different states. Just as some countries raced ahead following the industrial revolution in the nineteenth century, some contemporary states will compete more effectively in global markets and exploit new information technologies more readily than others. Instead of creating a more homogeneous world, in short, globalization is more likely to exacerbate global inequalities and alter the hierarchy of power among states. The realist tradition also warns that such a trend could be dangerous, because large shifts in power usually foster insecurity, competition, and a heightened risk of war (Copeland 2000a; Gilpin 1981).

Finally, realist theory has long emphasized the interrelationship of politics and economics; close economic ties normally depend on stable political relations (Gilpin 1976, 2000; Buzan 1984). For the past fifty years, the United States has provided a political order that has nurtured the growth of world trade and made it possible to create novel institutions like the European Union. If that political stability were to evaporate, however, the globalization process could easily unravel. When considering the post-cold war order, in short, the realist tradition offers a valuable corrective to the belief that economics is trumping politics (Waltz 2000b).⁴¹

BUILDING A BETTER MICROFOUNDATION

The microfoundations of the realist tradition are not as firm as one might wish. The treatment of states as unitary rational actors is both widely accepted and problematic, and the claim that states (or leaders) place a premium on survival can be challenged as well. Thus, one obvious way to broaden and strengthen realist theory would be to develop more persuasive microfoundations for its generally gloomy expectations.

40. See Hirst and Thompson 1999, Waltz 2000b, and Gilpin 2000, 24–25.

41. Even an ardent globalizer like Thomas Friedman understands that the current order rests on a particular structure of political and military power. In his pithy phrase, “without America on duty, there will be no America Online” (1999, 376).

One possibility is evolutionary biology, as recently argued by Bradley Thayer (2000). According to Thayer, an evolutionary perspective can explain why humans establish hierarchies of dominance, are readily indoctrinated, and quickly draw distinctions between members of their own group and outsiders. He suggests that these insights can explain competition and war without invoking anarchy as an independent causal force. This line of inquiry is in its early stages and should be viewed with some skepticism, but it could provide a set of underlying assumptions that would buttress the realist worldview.

Jonathan Mercer (1995) proposes a different answer to the same problem, arguing that social identity theory explains the tendency for individual humans to form strong group loyalties, and thus to favor measures that will favor their own group over others. Although the jury is still out, these perspectives offer firmer grounds for the realist claim that states can be thought of as unitary, self-interested actors.

REALISM AND DOMESTIC POLITICS

The realist tradition has focused primarily on international politics, but it is in fact a broader approach to political life that is potentially relevant to other questions. Realist theory has made important contributions to the study of ethnic conflict *within* states, and some scholars have also made analogies to other quasi-anarchic realms (such as gang warfare in inner cities). In the future, scholars should look for other arenas that realist theory has tended to neglect.

One topic—arguably neglected since Thomas Hobbes—is the origins of the state system itself. Ashley Tellis (1995–96) points out that realist theory simply assumes the existence of states-as-actors without explaining why states arose in the first place. Although Tellis does not provide such a theory himself, his comments remind us that Hobbes and other early realists saw the creation of the state as a solution to the incessant warfare that confronted individuals in the (anarchic) state of nature. A realist approach to state formation would emphasize the imposition of sovereign authority in order to mobilize power and create security for ruler and ruled alike, as opposed to approaches that regard the state as a voluntary contract between sovereign and subject or between free and equal citizens. Similarly, Michael Desch's recent work on the international sources of domestic cohesion and civil-military cooperation suggests that external constraints may play a larger role in domestic politics than previously recognized (Desch 1996, 1999).

FOREIGN POLICY

Realist theory can also be used to develop more sophisticated theories of foreign policy. Although Waltz maintains that neorealism is a theory of in-

ternational politics rather than a theory of foreign policy (cf. Elman 1996c; Waltz 1996), few scholars seem to have been persuaded by this argument (Fearon 1998c; Mearsheimer 2001). If the insecurity of anarchy constrains the states in the system, then surely it affects some of their subsequent behavior. There seems to be little reason for realist theory to confine itself to explaining the systemic properties of particular orders, and most scholars working in the realist tradition (including Waltz) have used their theoretical understanding to make foreign policy recommendations (e.g., Walt 1989; Layne 1997; Art 1998–99).

INTERNATIONAL LAW

A final possibility is international law. Until recently, scholarship linking international relations and international law has generally been dominated by liberal or constructivist approaches. This tendency is unsurprising, given that realist theory tends to downplay the role of norms and rules while these rival traditions place a high premium on them. But there is still a place for a realist approach to this subject, highlighting the dominant role of major powers, their ability to evade legal constraints when they feel it is necessary, and the tendency for customary law to reflect the interests of states rather than any exogenous set of normative standards.⁴²

■ | Conclusion

International relations is an eclectic and contentious discipline. As I have argued elsewhere, its intellectual and methodological diversity is a desirable quality, because global political processes are unlikely to be captured by a single intellectual or methodological approach (Walt 1998, 1999). Moreover, the coexistence of different research traditions encourages a fruitful competition between them, forcing each approach to confront anomalies, refine its explanatory logic, and conduct more robust empirical tests.

Unfortunately, the clash of competing ideas can also lead to intolerance and intellectual imperialism.⁴³ Given the many contributions that

42. For initial attempts along the lines suggested here, see Goldsmith and Posner 1999, 2000 and Alvarez 2001.

43. It is tempting to apply realist theory to the academic world, which can be viewed as a quasi-anarchic realm in which scholars compete for power, prestige, and other scarce resources (e.g., grants, awards, positions for their students, etc.). As in international politics, scholars often form alliances to advance these interests and may try to increase their own power by attacking other approaches. Thus the structure of academic politics explains why scholars who reject the substantive conclusions of realist theory often behave in a remarkably realist fashion.

the realist tradition has made (and continues to make) to our understanding of world politics, it is still striking to observe the hostility it provokes. As Robert Gilpin gloomily observed a few years ago, “nobody loves a political realist,” and especially not in the liberal United States (Gilpin 1996; Shimko 1992; Jervis 1998). This hostility is evident in the harsh rhetoric that critics of realist theory sometimes employ, as well as the speed with which they declared a venerable research tradition dead in the immediate aftermath of the cold war.⁴⁴

More recent events confirm that realism remains strikingly relevant. The United States and its allies have fought three wars in the past ten years, at least one of them (the 1990–91 Persian Gulf War) motivated by balance of power considerations. Bill Clinton may have claimed that the “cynical calculus of power politics” was obsolete, but his policies as president demonstrated an abiding appreciation for the realities of power. Clinton soon discovered that invoking the United Nations was not a viable substitute for a foreign policy, engaged in humanitarian intervention and peacekeeping operations with great reluctance, and promoted narrow U.S. interests in bilateral trade negotiations. The United States took advantage of Russia’s weakness to expand NATO up to the Russian border, pressured the post-Soviet republics into giving up their nuclear arsenals, and reaffirmed its security ties in Asia in preparation for a future competition with China. Clinton also showed an ample willingness to use force (against Iraq, Sudan, Afghanistan, and Serbia) even in the absence of authorization from the U.N. Security Council. The United States also rejected a global ban on land mines and the creation of an international criminal court, for fear that these agreements would place unacceptable constraints on U.S. freedom of action.

The new administration of George W. Bush shows no sign of reversing these tendencies, and key Bush advisers have openly proclaimed their commitment to realist principles. Bush’s decision to abandon the 1972 Anti-Ballistic Missile Treaty illustrates the tendency for great powers to seek superiority over future rivals, and the forceful U.S. response to the terrorist attacks of September 11, 2001, reminds us that great powers pay scant attention to diplomatic niceties when their own security is threatened.⁴⁵ Realist theory tells us that powerful states are likely to act in this way (especially when they face few immediate constraints), and it also cor-

44. Manifestations of intolerance include John Vasquez’s explicit attempt to discourage continued funding for realist research (1997, 899), Bahman Fozouni’s suggestion that scholars examine “why for the past several decades the discipline of international politics remained mesmerized by a false theory” (1995, 508), and Stanley Hoffmann’s declaration that “realism is utter nonsense today” (Friedman 1993). See also Farrell 1999 and Ashley 1984.

45. Among other things, U.S. leaders have minimized allied participation in order to keep a free hand to wage war as they wished, declared that the United States does not need authorization from the United Nations in order to use force, and used a combination of threats and bribes to gain support from countries such as Pakistan.

rectly predicts that other states (including some close U.S. allies) will be alarmed by U.S. unilateralism and become eager to find ways to limit it.

No matter where one looks, in fact, one sees additional signs of realism's enduring relevance. In Africa, anarchy and insecurity combine to trigger reciprocal massacres in Rwanda and Burundi. In the Middle East, the peace process between Israel and the Palestinians founders over each side's unwillingness to accommodate the other's security concerns.⁴⁶ In South Asia, India and Pakistan acquire nuclear weapons, partly due to fear of China (in India's case) and partly out of fear of each other. Japan is shedding its pacifist veneer, and already maintains a borderline nuclear capability as a hedge against the removal of the U.S. security umbrella (Green 2001; Mack 1997; Harrison 1996). Even in Europe, where the construction of a "pluralist security community" is most advanced, influential elites warn of a "renationalization" of foreign policy should the United States withdraw (Art 1996a; Bertram 1995). Thus, in an era where some commentators believe power politics is irrelevant, the United States continues to keep roughly 200,000 troops in Europe and Asia, largely to prevent the outbreak of renewed security competition there.

These developments suggest that efforts to excommunicate realist theory are doomed to fail. Barring a fundamental transformation in the basic nature of world politics, the realist *problematique* will continue to influence the behavior of states. Major powers will continue to compete, the shadow of organized violence will continue to loom large in their calculations, and competition between and within states will sometimes spill over into open warfare. Although efforts to promote cooperation through the development of shared norms and institutions will be made and will sometimes succeed, these efforts are unlikely to overcome the fundamental problems identified by the realist perspective. As a result, students and scholars will always be drawn back to the realist tradition, if only to understand the full nature of the challenge that humankind faces.

Realist theory is far from perfect, of course, and this inventory of recent accomplishments cannot obscure its limitations. Many realist theories are not as precise as one would like, the realist family does not address a number of important topics (such as the growing global commitment to human rights), and it cannot account for many aspects of international change. The core concept of power is not well conceptualized, key hypotheses are untested, and variations within the realist family have proliferated faster than they have been resolved. Given these problems, it would be fair to conclude that the realist tradition is the *worst* approach to the study of world politics—except for all the others.

46. From a realist perspective, the central problem between Israel and the Palestinians is simple. A viable Palestinian state could be a potential long-term threat to Israel's security; this means that an agreement that would be acceptable to the Palestinians is hard for Israel to accept, and vice versa.